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presented by Payton Jeffrey Jones

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Date: February 17, 2021

The Neurotic Treadmill: Decreasing Adversity, Increasing Vulnerability?

A dissertation presented

by

Payton Jeffrey Jones

to

The Department of Psychology

in partial fulfillment of the requirements

for the degree of

Doctor of Philosophy

in the subject of

Psychology

Harvard University

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The Neurotic Treadmill: Decreasing Adversity, Increasing Vulnerability?

Abstract

In the last few decades, violence has markedly decreased within the United States. This decrease applies to almost all distressing and dangerous life events including interpersonal violence, rape, and sexual assault. Despite this, rates of posttraumatic stress disorder (PTSD) have not decreased. In addition, rates of PTSD following trauma are higher in developed countries such as Canada compared to less developed countries such as South Africa. What might explain this counterintuitive pattern? One possibility is that there exists a societal treadmill effect for trauma. Specifically, as the frequency of adverse events decreases, definitions and negative expectations regarding trauma may expand and increase, generating greater vulnerability to trauma. This model is tested in a series of experiments.

In Paper 1, I examine whether manipulating the frequency or displayed range of serious traumas in an experiment will result in expanded or contracted definitions of trauma.

In Paper 2, I examine whether definitions of trauma affect individuals' anxiety and intrusive memories after watching a distressing film.

In Paper 3, I turn to examine whether providing trigger warnings to previously traumatized individuals is iatrogenic, adversely impacting vulnerability by increasing expectations of harm.

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"Members of the generation that survived the horrors of…the Nazi death factories, such as Treblinka…are unlikely to be much affected by some of the relatively mild stressors that seemingly incite PTSD today. That is, if what counts as a traumatic stressor depends on the context of one's environment, then the massive decline in violence today results in a massive broadening in the kind of things capable of producing posttraumatic psychopathology. The relatively greater comfort, safety, health, and well-being of the 21st-century world may have rendered us less resilient to stressors far less psychologically toxic."

Richard J. McNally

The ontology of posttraumatic stress disorder: natural kind, social construction, or causal system? Clinical Psychology: Science and Practice (2012a), 19, 220-228

"In this frame of mind it occurred to me to put the question directly to myself: "Suppose that all your objects in life were realized; that all the changes in institutions and opinions which you are looking forward to, could be completely effected at this very instant: would this be a great joy and happiness to you?" And an irrepressible self-consciousness distinctly answered, "No!" At this my heart sank within me: the whole foundation on which my life was constructed fell down."

John Stuart Mill

The Autobiography of John Stuart Mill (1873)

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Chapter 1 —

General Introduction

As the modern world has developed, have humans truly become happier? Despite the pessimism of some skeptics (e.g., Gray, 2016), the answer is almost undoubtedly yes (Pinker, 2018, pp. 262-290). Happiness increased between 1981 and 2007 in forty-five out of fifty-two measured countries in the World Values Survey (Inglehart, Foa, Peterson, & Welzel, 2008). But although progress increases happiness, it does so at a rate much less than we tend to expect. A nearly ubiquitous human experience is the acquisition of a new object, article of clothing, or promotion in status which initially brings great joy and pleasure. Yet almost equally ubiquitous is the experience of quickly returning close to our baseline level of happiness, barely noticing the new acquisition after a few weeks. This quick habituation to altered life circumstances has been dubbed "the hedonic treadmill" (Brickman & Campbell, 1971).

In its initial form, the theory of the hedonic treadmill was greatly overstated. Individuals do habituate to many types of experiences and return close to "hedonic set points," but those set points of happiness vary from person to person and can change under some conditions (see Diener, Lucas, & Scollon, 2006; Pinker, 2018, pp. 262-290). The hedonic treadmill may not be as hopeless as "one step forward, one step back," and may be more accurately stated as "one step forward, one half-step back." That is, we partially habituate to life improvements, though their long-term net effect is still non-zero and positive. As problems in the world are ameliorated, new and unique challenges often rise to take their place. Yes, money does increase happiness – but not as much as we tend to expect (Dunn, Gilbert, & Wilson, 2011). Increases in income lead to less-than-proportional increases in life evaluation and emotional well-being[1] (Kahneman & Deaton, 2010; Stevenson & Wolfers, 2008). The basic concept of hedonic habituation passes a

[1] Income increases happiness as a factor of one's *percentage* increase in income (i.e., the log of income). The relationship is also log-linear for the Cantril ladder, in which participants are asked to rate their current life from "the worst possible life for you" to "the best possible life for you."

gut check: a windfall of $1000 is never unwelcome, but means much less to a billionaire than a pauper. Progress does not imply a journey devoid of new challenges. Solving a larger problem often results in smaller yet non-negligible side effects.

The Neurotic Treadmill: The Case of PTSD

Parallel to the question of human happiness is that of human misery—as the world has progressed, have we become less anxious, depressed, or emotionally impaired? By many metrics, the world has become a much friendlier and safer place. Yet it is possible that a parallel neurotic treadmill effect may apply, resulting in less-than-expected decreases in human misery.

Note that the existence of a hedonic (happiness) treadmill does not automatically imply the existence of a neurotic (misery) treadmill. Although intuitive models of emotion often place happiness and misery on two ends of a spectrum, the reality of emotion is more complex. Models that separate well-being and ill-being as two separate dimensions tend to provide a better explanation of the data than models that use a single dimension (Rubin & Talarico, 2009). Personality researchers likewise separate between extraversion (linked to experiencing more positive emotion, but not less negative emotion) and neuroticism (linked to experiencing more negative emotion, but not less positive emotion; Rusting & Larsen, 1997). Indeed, separate physiological and neural processes are involved in producing positive and negative emotions (Lang & Bradley, 2010). The idea of a neurotic treadmill is therefore distinct from earlier research on well-being.

To investigate the idea of a neurotic treadmill, it is convenient to examine a form of misery very closely tied to specific negative events: posttraumatic stress disorder (PTSD). In this chapter, I propose the foundations of a neurotic treadmill theory for PTSD, discuss the history and science of PTSD, and discuss changes in the PTSD concept over time. In the following

chapter, I take a deeper dive into the epidemiology of PTSD to evaluate whether such a neurotic treadmill effect is plausible.

PTSD is a disorder of nonrecovery following trauma. PTSD can be conceptualized as meeting at least three criteria—an individual endures an (1) *adverse event*, that adverse event causes an (2) *acute stress reaction* including emotional distress and functional impairment, and the individual (3) *fails to recover* from this acute stress reaction in a normative timeframe. An acute reaction to an adverse event is not enough for a diagnosis of PTSD – the reaction must be lasting, typically for at least 1 month (APA, 2013). Some adverse events cause intense acute distress, anxiety, and functional impairment in most individuals; however, this is not indicative of psychopathology unless this reaction endures for more than a few weeks. After enduring an acute stress reaction, most individuals will quickly recover to baseline functioning, while a few will not. Importantly, not all adverse events will cause an acute stress reaction and reactions to the same event will vary on an individual basis.

It is possible that a treadmill effect influences what types of events can induce acute or chronic stress reactions. For every step taken forward in terms of reducing the frequency of adverse, potentially traumatic events, a step (or more likely, a half-step) might be taken backwards in terms of vulnerability to these events, causing PTSD rates to stay close to where they started. Specifically, I hypothesize that there is a relationship between the *frequency of adverse events* and personal vulnerability in the form of *definitions and expectations regarding trauma* (see Figure 1.1). This treadmill model suggests that as the frequency of adverse events decreases, definitions and negative expectations regarding trauma expand and increase. Another way to conceptualize this treadmill effect is to think in terms of *relative* distress depending on prior experience. Imagine that we could develop an objective 1-100 scale of how "adverse" an

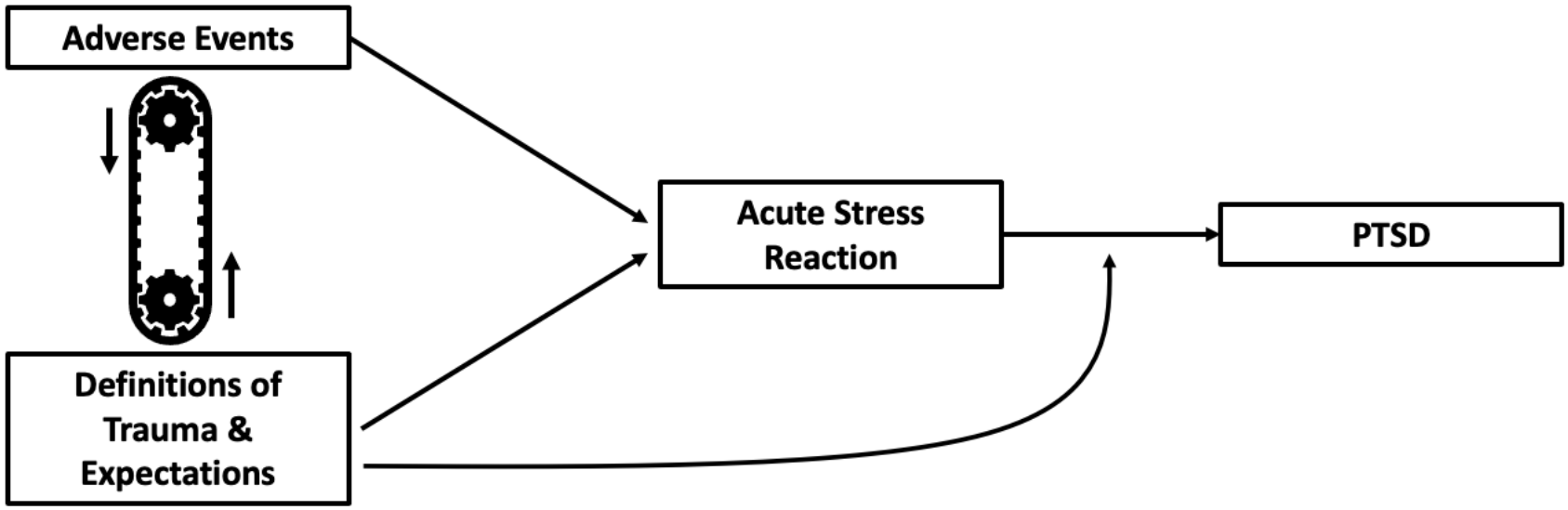
event is. For an individual who has experienced many events ranging from 0-60, and therefore has certain definitions and expectations regarding trauma, it may take an event rated at 95 to generate an acute stress reaction. However, for an individual who has only experienced events ranging from 0-20, and therefore has different definitions and expectations regarding trauma, an event rated at 45 may be enough to create a highly distressing experience.

The treadmill model predicts that there is a reciprocal relationship between adverse events and individuals' personal definitions and expectations of trauma. As adverse events become rarer and less severe, individuals' definitions and expectations of trauma expand. Expanded personal definitions increase the likelihood of acute stress reactions for any given adverse event. Thus, the model predicts only small declines in the rates of acute stress reactions and PTSD as adverse events decrease. It should also be noted that only a subset of acute stress reactions will result in nonrecovery and PTSD. It is possible that expectations regarding trauma may also contribute to whether acute stress reactions translate into long-term harm.

It should be noted that expanded personal definitions and expectations that result from an adaptation effect could also have relevant second-order effects that indirectly influence reactions to events. For instance, widespread expansions in personal definitions of trauma could influence societal norms and moral evaluations surrounding PTSD. These norms and evaluations could then have subsequent consequences: for instance, they could influence the manner in which one incorporates an event into their life story, or how central to their life they regard the event to be. Such norms and moral evaluations could also feed back into definitions and expectations of trauma. We can therefore separate between two potential mechanisms explaining how expanded personal definitions of trauma might affect reactions to events: directly, via immediate and long-term appraisals of events and their outcomes, and indirectly, by first affecting societal and moral

expectations, which in turn affect outcomes or feed back into personal concepts. To better examine each part of this model, we must first understand the basic history, etiology, and clinical presentation of PTSD.

Figure 1.1. A Treadmill Model for PTSD



Posttraumatic Stress Disorder (PTSD): An Introduction and History

PTSD is characterized by a re-experiencing of traumatic events, flashbacks, hypervigilance and startle reactions, dysphoria or numbness, and a variety of other symptoms (APA, 2013). PTSD is highly heterogeneous, with more than 600,000 unique ways to qualify for the disorder (although many of these combinations are clinically implausible, e.g., someone who has traumatic nightmares, but no intrusive recollections while awake; Galatzer-Levy & Bryant, 2013). Males are more likely to experience traumatic events but are less likely to develop PTSD after a trauma (Tolin & Foa, 2006). This effect is partly due to the category of sexual violence, which is primarily experienced by women and causes PTSD at higher rates than other types of trauma. However, women remain more vulnerable to PTSD even when controlling for the trauma type (Tolin & Foa, 2006). The effect of certain sex hormones on memory may help explain this vulnerability (Cheung, Chervonsky, Felmingham, & Bryant, 2013; Felmingham, Fong, & Bryant, 2012).

PTSD was initially added to the DSM-III in 1980 because of discussions surrounding "post-Vietnam war syndrome" (McNally, 2003a; APA, 1980). Individuals returning from the Vietnam War experienced symptoms such as flashbacks and intrusive memories, prompting psychiatrists to examine the phenomenon more closely. These individuals were grouped with individuals who were traumatized by rape, natural disasters, and the Holocaust to form the category of PTSD, which dealt with nonrecovery from traumatic events in general (McNally, 2003a).

In its initial diagnostic conceptualization, PTSD concerned reactions to extremely terrifying and presumably rare events. It was assumed that PTSD applied only to the infrequent instances in which individuals were exposed to extreme terror or horror. More recently, however,

PTSD has applied to a much broader range of trauma (McNally, 2011). The diagnostic criteria for a potentially traumatic event (Criterion A of the DSM PTSD diagnosis) have expanded over time. These changes, which have been the sources of various controversies, will be covered in greater depth in a later section.

Outside of the realm of standardized diagnostic epidemiology, expansions are even more dramatic. Researchers have studied "PTSD"[2] resulting from being bullied at work (Matthiesen & Einarsen, 2004) or delivering a heathy baby without complications (Olde, van der Hart, Kleber, & van Son, 2006). These technically-incorrect expansions are not mere fringe examples – together, these two articles have been cited more than 1000 times. This broader definition of trauma might lead to interesting results. Under this conceptualization, most individuals would experience adverse events that could potentially qualify as trauma, but only a few would develop PTSD (Resick et al., 2013). This paints a very different picture of PTSD than the earlier version: if many people experience trauma, but few develop PTSD, the most relevant causal factor in PTSD would be vulnerability to trauma rather than the trauma itself. Indeed, McNally (2011) notes that as the definition of trauma has expanded, the focus of PTSD has been shifted from the index event to individual vulnerability factors.

There are several vulnerability factors which predispose individuals to developing PTSD. Individuals with normal or lower intelligence are at higher risk for developing PTSD compared to individuals of higher intelligence (Breslau, Lucia, & Alvarado, 2006; Macklin et al., 1998; McNally, 2006). Individuals who have higher rates of *pre*-traumatic stress reactions (i.e., they

[2] Such examples should not technically be called PTSD as the index events do not qualify under Criterion A. Regardless, the researchers make assertions confirming supposed PTSD status. Matthiesen & Einarsen (p. 348) state "the present study indicates that psychiatric distress and PTSD may be widespread among victims of bullying at work." Olde et al. (p. 13) assert that "quantitative studies found empirical evidence for…PTSD in women who had given birth regularly to a child."

experience intrusive thoughts or other symptoms in reference to events they anticipate, such as military deployment), have higher rates of actual PTSD once they are actually exposed to adverse events (Berntsen & Rubin, 2015).

Individuals who have overgeneral memory recall are also at greater risk for developing PTSD following trauma (Bryant, Sutherland, & Guthrie, 2007). Individuals with overgeneral memory have difficulty recalling specific autobiographical episodes from their life when prompted to do so. For example, when given a cue word such as "loyal" or "guilty" and asked to recall a memory, individuals characterized by overgeneral memory tend to offer broader, more general categories of events rather than specific stories (McNally, Lasko, Macklin, & Pitman, 1995). This may be because difficulty recalling specific memories is linked to problem-solving (Evans, Williams, O'Loughlin, & Howells, 1992): to cope with problems today it may be helpful to recall specific instances for which certain solutions worked and certain solutions did not (McNally, 2003a). Overgeneral memory is correlated with intelligence, but predicts PTSD over and above what can be predicted by intelligence and event severity (Kleim & Ehlers, 2008). Overgeneral memory is not specific to PTSD and is also associated with depression and difficulty recovering from depression (Sumner, Griffith, & Mineka, 2010; Brittlebank, Scott, Mark, Williams, & Ferrier, 1993). Overgeneral memory has been linked to cognitive avoidance—it may represent an avoidant style of thinking about one's painful past that increases risk for depression or PTSD (Brewin, Watson, McCarthy, Hyman, & Dayson, 1998). Alternatively, it may be that cognitive demands associated with avoidance or rumination interfere with specific memory retrieval processes (Moore & Zoellner, 2007). Trauma exposure alone is insufficient to produce overgeneral memory, meaning that overgeneral memory is best

viewed as a vulnerability for psychopathology rather than a consequence of adverse events (Moore & Zoellner, 2007).

Individuals with reduced hippocampal volume are at greater risk for developing PTSD. (Gilbertson et al., 2002). Identical twin siblings of PTSD patients who were not exposed to combat trauma have similarly small hippocampi, suggesting that this is a genetic vulnerability rather than a result of environmental influences on the brain (Gilbertson et al., 2002). Also at risk are individuals who display neurological 'soft signs' – subtle abnormalities of language, coordination, or perception that cannot be localized to specific neural deficits or lesions (Gurvitz et al., 2000). Twin studies again reveal that this likely reflects pre-existing vulnerabilities rather than being a consequence of trauma (Gurvitz et al., 2006). The vulnerabilities listed above refer to the chance of developing PTSD given that a trauma has occurred—that is, the *conditional* probability of developing PTSD given trauma exposure. Some individuals are at greater risk of *experiencing* trauma in the first place: individuals of the male sex, with higher impulsivity scores, and greater extraversion and neuroticism are at increased risk of encountering traumatic stressors (Tolin & Foa, 2006; Netto et al., 2016; Breslau, Davis, & Andreski, 1995).

Some theorists suggest that prior trauma exposure might increase risk for PTSD when experiencing another trauma: that is, multiple traumas may exhibit a cumulative effect on PTSD symptoms. Meta-analyses confirm that participants' reports of trauma exposure prior to the index event do indeed correlate with PTSD diagnostic status (r = 0.11-0.17, Brewin, Andrews, & Valentine, 2000; Oser et al., 2003). However, one potential issue with these meta-analyses is that they only consider studies in which events prior to the index event are assessed retrospectively. Retrospective reports of trauma can be influenced by participants' current emotional and mental

state and have substantial reliability problems (Hardt & Rutter, 2004; Raphael & Cloitre, 1994; Schraedley, Turner, & Gotlib, 2002; but see also Brewin, Andrews, & Gotlib, 1993).

Breslau and colleagues (2008) conducted a 10-year *prospective* study assessing cumulative risk for PTSD in 990 individuals. Their results suggest that the cumulative effect of traumas only holds true in a prospective fashion if the individual developed PTSD following the first trauma. That is, PTSD *worsened* in a cumulative fashion, but it did not appear to *develop* in a cumulative fashion. In other words, risk for developing PTSD was highest with the first trauma (of a given type & severity). For subsequent traumas of similar type & severity, there was still a risk of developing PTSD, but the risk was somewhat lowered for later traumas compared to earlier traumas. Although this is the only major prospective study addressing the issue, we should remain cautious in interpreting the results of any single study. Additional prospective studies (and meta-analyses of these studies) would help clarify the issue.

What might explain Breslau's results? One possible explanation is latent inhibition. Latent inhibition describes a phenomenon in which animals and humans exposed to a stimulus without pairing to a reinforcement or punishment show reduced learning when that stimulus is paired with a reinforcement or punishment later (Lubow, 1973). For example, individuals who have a long history of non-painful dental treatment are less likely develop a dental phobia if they experience a painful procedure (compared to individuals who experience a painful procedure in their first dental visit; Davey, 1989). Individuals who do not develop PTSD after a first trauma may be partially inoculated, rather than made more vulnerable, to future traumas. Adverse events may therefore predict psychopathology in a U-shaped curve, with *some* exposure to adverse events being advantageous compared to either *no* exposure or *high* exposure (Seery, Holman, & Silver, 2006).

Most individuals who develop PTSD experience a great deal of distress during the index trauma. However, this is not true of all individuals. For example, some individuals who are molested in childhood do not realize what is happening, and thus their experience is confusing, frightening, disgusting, but not terrifying (Clancy & McNally, 2005). But when these individuals realize what happened later in adolescence or adulthood, they can develop PTSD (Clancy & McNally, 2005; McNally & Geraerts, 2009). These cases suggest that patients' current conceptualization of the traumatic event – its meaning for the person – is essential, whereas the actual in-the-moment experience of the trauma may matter less. This point is further emphasized by the existence of individuals who report traumatic symptoms from being abducted by space aliens (Clancy, McNally, Schacter, Lenzenweger, & Pitman, 2002). While hearing audiotapes of their "abduction encounters", the psychophysiological responses of the alleged alien abductees was greater than these responses of combat veterans with PTSD who listen to audiotaped narratives of their traumas (McNally et al., 2004). The importance of subjective conceptualization is also emphasized by individuals who develop PTSD-like symptoms after having false memories induced by recovered-memory therapists (McNally, 2005). In other words, the existence of a traumatic memory is important to the etiology of PTSD, but that traumatic memory need not be formed by an event that was traumatic in the moment it was experienced – it need not even be a veridical experience (Rubin, Berntsen, & Bohni, 2008).

The cognitive model of PTSD (Ehlers & Clark, 2000) similarly emphasizes the importance of the subjective appraisal of adverse events. Individuals who express mental defeat or mental confusion when appraising an adverse event are more likely to develop PTSD and less likely to recover (Beierl, Böllinghaus, Clark, Glucksman, & Ehlers, 2019; Dunmore, Clark, & Ehlers, 1999). The cognitive model also predicts that events are more likely to cause emotional

disruptions when they violate important beliefs about the world, such as an expectation that the world will be safe or fair. This could be one potential mechanism of a neurotic treadmill: in a generally safe context, individuals might have stronger expectations that they will be safe and will be treated fairly and justly (i.e., a weak expectation that they will experience trauma). If they then experience a serious adverse event, this may violate their core beliefs to a greater extent than it would otherwise. Some evidence suggests that being able to anticipate a trauma can be protective. The animal literature has long documented that predictable stressors are less distressing than unpredictable ones (Grupe & Nitschke, 2013; Mineka & Kihlstrom, 1978). Among political prisoners, those who were activists (and thus had undergone some mental preparation for being captured and tortured) were less likely to develop PTSD even though they were subjected to harsher torture (Başoğlu, Mineka, Paker, Aker, Livanou, & Gök, 1997). A context in which one expects to be safe may set the stage for greater confusion and mental defeat when serious adverse events occur.

Berntsen and Rubin's (2007) centrality of events model for PTSD sheds additional light on the topic of appraisal. The more a person views a traumatic memory as central to their life narrative, the more likely they are to develop PTSD. The centrality of events model suggests that integrating a traumatic event into one's life story may be a poor decision, especially if it causes the individual to center their life story around the trauma. Therapeutically, this theory suggests that PTSD should be treated by gently reducing the importance of the trauma to the individual's life story, allowing the individual to see outside the "lens" of trauma.

Multiple studies have demonstrated that event centrality is correlated with aggravated PTSD symptoms (Berntsen & Rubin, 2006; Brown et., 2010; Gehrt, Berntsen, Hoyle, & Rubin, 2018; Robinaugh & McNally, 2011). More importantly, evidence suggests this isn't merely a

correlation, but a directional causation. Event centrality fulfills the requirement of temporal precedence: event centrality predicts later increases in PTSD symptoms, but PTSD symptoms do not predict later increases in event centrality (Boals & Ruggero, 2016). Experimental evidence is also supportive. A therapy specifically tailored to decrease event centrality succeeded in reducing centrality compared to treatment-as-usual, which translated into greater decreases in PTSD symptoms (i.e., changes in centrality mediated treatment outcomes; Boals & Murrell, 2016).

PTSD Concepts and Referent

The official definition of PTSD as defined by the Diagnostic and Statistical Manual of Mental Disorders has changed over time. We must distinguish between the diagnostic definition of PTSD and individuals' personal understanding of PTSD; we hereafter refer to the former as the *diagnostic concept* of PTSD and the latter as a *personal concept* of PTSD. These two concepts are not necessarily independent. Individuals may certainly modify their personal concept of PTSD upon learning about the more official diagnostic concept, and manualized changes in the diagnostic concept may be influenced by certain experts' personal concepts of PTSD.

Although the diagnostic concept of PTSD has changed over time, it is unclear whether the actual clinical phenomenon – that is, the *referent* of the PTSD diagnostic concept – has been similarly altered. One possibility is that the concept of PTSD has become broader merely as a function of diagnosticians. Distress in the wake of adverse events may have remained constant over time, while clinical definitions have evolved. This would lead us to conclude that the PTSD suffered by a torture victim would be very different from the PTSD suffered by someone who learned about the sudden unexpected death of a close friend. The reason that the latter qualifies

for PTSD in the 21st century may be merely a function of changing diagnostic standards; the actual symptoms following the event may have been identical in 1975 and 2019, but only formally acknowledged in 2019. Yet it is also possible that the syndrome experienced by the two individuals shares a concept *and* a referent – because of differing vulnerabilities in each individual, a similar clinical phenomenon could emerge from two very different events. One of the possible vulnerability factors may be an individual's personal concept of PTSD.

Both types of PTSD *concepts* (the ideas people have in their heads about PTSD, including both personal and diagnostic) are distinct from the PTSD *referent* (the real-world phenomenon that is being referred to). Many philosophical referents don't seem to care much about their concepts. That is, whether we call a tree an alder or an oak, it doesn't matter much to the tree – it is an *indifferent kind* (Hacking, 1999, pp.100-124). Modifying the concept of an indifferent kind might affect how humans interact with it, but it doesn't affect the referent directly. Hacking (1999) posits that not all philosophical referents share this indifference. Some are *interactive kinds*: the referent interacts directly with its concept. An example is a 'police officer'. The meaning of the concept 'police officer' is critical to the behavior and function of real-world police officers (the referents of the concept).

In a similar manner, PTSD may be an interactive kind, especially when considering the *personal* concept of PTSD. The personal concept of PTSD and trauma (or lack thereof) that exists in the mind of the trauma survivor might influence the course and presentation of PTSD symptoms. As an example, we can imagine a person who holds a very fatalistic view of PTSD. This person believes that PTSD is an inevitably disabling condition from which a trauma survivor can never recover. Such a person might be especially unlikely to actively work towards their own recovery or seek treatment, thus influencing the course of their symptoms.

The idea of PTSD as an interactive kind is especially plausible given what we know about the importance of cognitive appraisals of trauma. Negative appraisals about a traumatic event are among the most important predictors of PTSD in the short and long term (Beierl, Böllinghaus, Clark, Glucksman, & Ehlers, 2019, Dunmore, Clark, & Ehlers, 1999). Someone who views PTSD as common and permanent may be more likely to have negative appraisals of an adverse event that happens to them. It therefore seems likely that the personal concept of PTSD interacts at least partially with the PTSD referent.

Diagnostic Concept Bracket Creep

The diagnostic concept of PTSD has changed substantially over the past decades. Why did this shift occur? The DSM-III (APA, 1980) and DSM-III-R (APA, 1987) concept of PTSD assumed that only extremely rare and extreme stressors outside the realm of normal experience could produce the symptoms characteristic of the disorder. Diagnosis was confined to canonical stressors such as combat, rape, torture, and natural disasters.

When the DSM-IV committee met to formulate the PTSD construct, empirical research complicated the previous diagnostic definition (McNally, 2015). One consideration was that evidence suggested that most individuals suffering from canonical stressors did not develop PTSD, which contradicted previous assumptions about the nature of traumatic stress (Breslau, Davis, Andreski, & Peterson, 1991). More importantly, cases were observed in which individuals who had not experienced canonical DSM-III stressors nevertheless met the symptomatic profile of PTSD (Dohrenwend, 2010). DSM-IV PTSD committee members therefore expanded the stressor criterion to include a much wider variety of potential traumatic experiences. Had the committee not broadened the concept of trauma, individuals who otherwise qualified for the diagnosis would have been unable to receive reimbursable treatment for their suffering. DSM-5

scaled back this expansion but remained broader than DSM-III. Many researchers have expressed concern that the expanding definition of PTSD may undermine the integrity of the psychobiological concept of PTSD (Bracha & Hayashi, 2008; Elhai, Kashdan, & Frueh, 2005; McNally, 2003b; McNally, 2009).

Personal Conceptual Bracket Creep

In addition to diagnostic concept changes, individuals may also modify their own personal definitions of trauma. The increasing salience of the word trauma within the English language is documented by Haslam & McGrath (2020), who analyze linguistic trends in how the word "trauma" is used. Their analysis of the Google Books corpus reveals that usage of "trauma" and "psychological trauma" rose steeply and linearly from 1970 to 2008. Interestingly, this contradicts the notion that changes in the trauma concept were primarily driven by the introduction of the PTSD diagnosis in 1980. The increase does not seem to be specific to "trauma" but instead exemplifies broader changes relating to the importance of harm-based morality. Those who hold broad concepts of trauma also tend to hold broad concepts of other harm-based concepts, including bullying, prejudice, and abuse (McGrath et al., 2019). With this in mind, it seems likely that the creeping concept of trauma in the general population represents a general creep in harm-related concepts, as proposed by Haslam (2016a).

A rise in the usage of "trauma" is also found in the Corpus of Contemporary American English (COCA; Davies, 2010), and a corpus of psychology journal article abstracts also indicates an increase in the semantic breadth of the term (Vylomova, Murphy, & Haslam, 2019). The word "trauma" appears most expansive the farther one strays from the academic bubble of rigorous scientific psychiatry. As noted earlier, even highly-cited psychology research articles have used PTSD to refer to reactions to non-Criterion-A events. The Google Books corpus

shows an exponential growth of the terms “historical trauma”, “intergenerational trauma”, “collective trauma”, and “cultural trauma” that accelerates beginning in the early 1990s (Haslam & McGrath, 2020). It is easy to find very broadened examples of “trauma” online, where some writers posit that reading the news or browsing social media can be a form of trauma (Jacobs, 2018; Lees, 2018).

Some researchers have asserted that definitions of trauma and violence are expanding among recent generations of students, noting an increase in associated protective policies on college campuses such as creating safe spaces and disinviting potentially distressing speakers (Lukianoff & Haidt, 2018). Yet strong conclusions in this area are premature, as they are limited to specific demographic groups and may not generalize to the global population.

What drives changes in personal concepts of trauma? Haslam (2016a; 2016b) makes a strong case that changes in personal concepts of trauma are part of a larger trend in harm-related concepts (see also McGrath et al., 2019; Vylomova et al., 2019). Haslam and colleagues (2020) recently outlined several plausible causes of harm-related concept creep. First, they point to a growing sensitivity to the moral dimension of harm in Western cultures. Words relating to the moral foundation of “harm” (e.g., “care”, “compassion”, and “safety”; Haidt & Graham, 2007) have risen steeply in relative frequency since the early 1980s; no other moral foundation has increased to the same degree (“purity” increased somewhat during this period, and “fairness”, “ingroup”, and “authority” remained mostly stable; Haslam et al., 2020). The rise in the “harm” dimension parallels increases in the use of “trauma” (Wheeler, McGrath, & Haslam, 2019; Haslam & McGrath, 2020). Second, they point to objective changes in social conditions, such as declines in the rate of violence, mirroring the primary model presented in this dissertation. The mechanisms of this potential adaptation effect are the subject of later discussion. Third, they

point to deliberate actors who might seek the expansion of harm-related concepts to serve their own goals or incentives. Expanding the breadth of harm-related concepts can be effective in increasing the perceived importance of a social problem (e.g., Jenness, 1995; Haidt, 2016). Deliberately driving concept creep may also be effective in expanding social or legal responses to harm (Sunstein, 2018). It should be noted that this may potentially have both positive and negative consequences (Cikara, 2016; Haslam, 2016b).

Clearly, directly experiencing a traumatic event or having a traumatic event occur to a friend or family member may also influence an individual's perception of trauma. Psychological characteristics such as neuroticism, trait anxiety, or anxiety sensitivity may also play a role.

As mentioned earlier, perhaps the general degree of life adversity in one's own life or one's social group impacts personal concepts of trauma. As violence and other types of serious adversity decrease, perhaps an adaptation effect drives expansions in personal trauma concepts. One recent insight in this area comes from research on *prevalence-induced concept change* (Levari, Gilbert, Wilson, Sievers, Amodio, & Wheatley, 2018). Prevalence-induced concept change means that when instances of a concept become less prevalent, individuals broaden their interpretation of the concept, changing the context in which future instances are evaluated.

Levari and colleagues (2018) first tested this model by showing participants dots that ranged on the objective color spectrum from purple to blue. The participants were tasked with deciding whether each dot was purple or blue over 1000 trials. Participants were instructed to be as accurate as possible. Unbeknownst to the participants, the frequency of blue dots steadily decreased as the trials went on. As a result, participants shifted their threshold for deciding whether a dot was purple or blue (see Figure 1.2). Specifically, as blue dots decreased, participants experienced bracket creep in their definition of "blue" and were more lenient with

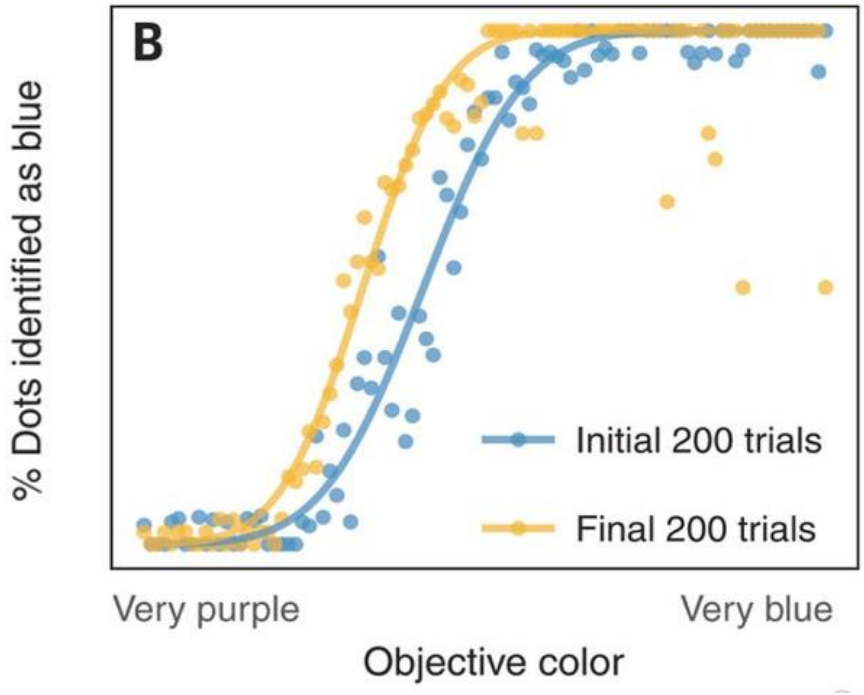
classifying blue dots. Levari and colleagues (2018) replicated this experiment in several modified conditions. The experiment replicated when explicitly informing participants that blue dots would decrease in prevalence, when changing the prevalence suddenly rather than gradually, and even when participants were incentivized to remain consistent with their ratings. Moreover, the same findings applied when they used stimuli measuring the threateningness of faces or the ethicality of short descriptions of proposals for psychology studies.

Prevalence-induced concept change is related to broader work in human decision making under uncertainty including signal detection theory (SDT; see Macmillan, 2002). To illustrate the main concepts of SDT, we can imagine a radar technician tasked with identifying enemy submarines from a blurry radar display. When the technician sounds the alarm, the decision may be either a *hit* (there really was a submarine) or a *false positive* (there was no submarine). When the technician decides not to sound the alarm, this inaction may be either a *true negative* or a *miss*. SDT predicts changes in decision-making depending on the frequency of submarines, the clarity of the radar signal, and the consequences for the various outcomes. For example, if the frequency of submarines drops to zero, yet the technician is still expected to sound the alarm at least once weekly, we might expect the technician to decrease the threshold for submarine detection (in SDT terminology, β). This shift is similar to concept expansion in prevalence-induced concept change.

That said, there are some key differences between prevalence-induced concept change and SDT. In SDT, individuals must distinguish a true signal in the midst of noise. In contrast, prevalence-induced concept change does not require a ground truth. For example, Levari and his colleagues cannot exactly define when participants are right or wrong about the threshold between purple and blue dots because the threshold for color is ultimately a socially constructed

one (the reality of color is a spectrum, not a set of categories). Instead of speaking in terms of sensitivity and bias in the detection of a signal, they speak instead in terms of a socially constructed threshold shifting under certain conditions. Moreover, prevalence-induced concept change appears to be sufficiently robust as to occur in conditions not predicted by SDT. When decreasing the prevalence of blue dots, SDT predicts a shifting threshold only if the participants feel obligated to continue detecting some minimum number of blue dots. Yet Levari et al. found that the threshold shifted even when even when participants were incentivized not to shift their threshold and when they were told in advance that the blue dots would become less frequent.

Figure 1.2. Prevalence-Induced Concept Change in the Color Blue



Note. Adapted from Levari et al., 2018

Globally, the occurrence of serious violent events has become less common. Prevalence-induced concept change offers one potential mechanism for why personal definitions of trauma might expand as a result. But why does that matter? It has been noted earlier that trauma survivor's concept of trauma is extremely important—an individual's pretraumatic stress reactions and the centrality they place on traumatic events are important vulnerability factors which predict PTSD. Indeed, an individual's very categorization of a stressful event as a "trauma" renders it likely that the individual places the event on a pedestal of importance and life centrality compared to events which are not personally classified as traumas. In other words, if two individuals experience the same event, but only one individual considers that event to be a form of trauma, the centrality of events model predicts that this individual is likely at higher risk of developing PTSD merely by nature of their *definition of trauma* (see Figure 1.1).

Notably, an individual's classification of a certain event as a "trauma" may share a bi-directional relationship with stress reactions to the event. For example, an individual may not categorize a motor vehicle accident as a "trauma" a priori, but after experiencing a surprisingly distressing accident, the individual may re-conceptualize the event as a trauma. This re-conceptualization is likely to impact symptoms moving forward independent of the a priori definition (e.g., perhaps increasing the likelihood that acute stress will translate to PTSD). Personal definitions of trauma may impact emotional reactions, but the individual noticing their own emotional reactions may also lead them to reformulate their definitions. This may constitute a reciprocal, circular process that maintains an individual's level of distress following a stressful event. This need not be an explicit cognitive process: expectations and appraisals could happen at an implicit level, and the same effects of expanded conceptualizations could still apply.

To summarize, there may exist a neurotic treadmill effect whereby decreases in adversity lead to less-than-proportional decreases in human misery. PTSD is an ideal test case because it is a well-defined type of misery tied to specific adverse events. Although there are many potential mechanisms that could drive a PTSD treadmill effect, one promising avenue of investigation relates to definitions and expectations of trauma and PTSD. Personal concepts could be plausibly influenced by changes to the level of adversity in the population. Research on prevalence-induced concept change provides one promising avenue for researching this type of adaptation effect. Definitions and expectations about trauma and PTSD are also potentially important in the etiology of PTSD; the cognitive model of PTSD and the centrality of events model shed light on specific mechanisms (e.g., cognitive appraisals). But given the epidemiological data on PTSD, is a treadmill effect for PTSD even plausible? Before experimentally investigating these ideas, I will first review the epidemiology of violence and PTSD to provide a preliminary answer.

Chapter 2—

Cause for Concern in the Epidemiology of Violence and PTSD

In *The Better Angels of Our Nature*, Pinker (2011) documents how violence and adversity have decreased globally, including within the United States. This substantial decrease applies to many distressing and dangerous life events (hereafter *adverse events*), including domestic violence, rape, and assault. In the most recent epidemiological survey of the United States using clinical interviews, interpersonal violence accounted for 49.7%[3] of PTSD cases (Goldstein et al., 2016). We might expect that a decrease in such adverse experiences might have led to a decrease in the number of individuals who are emotionally traumatized. Conversely, if a neurotic treadmill effect exists, we might expect that decreases in the prevalence of adverse events only marginally affect the overall rate of PTSD.

In this chapter, I aim to give a general overview of the relevant epidemiology on major adversity (especially interpersonal violence) and PTSD. First, I will examine the frequency of various adverse events within the United States. I will examine both retrospective self-reported rates of adverse experiences (Criterion A trauma) from epidemiological studies, as well as more objective reports collected by reporting agencies using more consistent yardsticks. As we shall see, there are major discrepancies between the two types of reports that are relevant to all future sections.

Next, I will examine the *overall rate* of PTSD: the lifetime prevalence rates from epidemiological surveys using nationally representative samples (hereafter, *overall rate*). The overall rate is a point estimate of the percentage of persons living within the United States who qualify for a diagnosis of PTSD at some point during their lifetimes. This statistic collapses all types of trauma. Limitations of this metric are discussed, and it is juxtaposed with objective rates of adverse experiences.

[3] Includes physical assault, sexual assault/harassment, kidnapping, mugging, or witnessing these same events occur to someone else. Does not include military/terrorist violence, serious injuries, or seeing a dead body or body parts.

Then, I will examine the PTSD prevalence rates conditional upon exposure to at least one specific Criterion A trauma in one's lifetime (hereafter, *conditional rate*). Because the assessment of Criterion A traumas has changed in important ways over the years and across differing studies, I examine this statistic for individual Criterion A events rather than as a whole (e.g., the conditional rate of PTSD given exposure to military combat). This statistic represents a type of vulnerability to traumatic events. Unfortunately, it is highly unreliable due to issues with retrospective self-reports of events and inconsistencies in measurement.

Next, I will examine the PTSD prevalence rates that are attributable to certain kinds of events that participants described as their "worst event" (hereafter, *attributable rate*). This statistic is somewhat similar to the conditional rate, in that it is able to be broken down by specific types of trauma. However, it only considers participants' worst event, and does not consider other traumas they might have experienced. Due to the way it is calculated, it is somewhat easier to make consistent across the varying assessment strategies used in major epidemiological surveys (i.e., the attributable rates across each category always sum to the overall rate). The attributable rate also opens the door for incorporating objective measures of exposure to assess vulnerability.

Then, I will switch tracks from the United States to briefly examine global patterns in the overall PTSD rate. I will juxtapose the PTSD rate with various cross-national metrics that serve as proxies for adverse experiences. This analysis shows patterns that are very counterintuitive. Unfortunately, there are very strong limitations to these data, with uncontrolled confounds that may affect the data quality (primarily, cultural confounds that may affect reporting). These confounds will be discussed. As will become evident from these sections, the epidemiology of PTSD is highly complex and confounded due to the changing nature of assessment. Thus, I

conclude with a brief sanity check: are the counterintuitive patterns in PTSD rates plausible considering the patterns in anxiety disorders as estimated by the more consistent Global Burden of Disease study?

Table 2.1. A Guide to Terms Used in this Chapter 2

Term	Description	Probability Notation
Overall rate	Lifetime prevalence of PTSD	p(PTSD)
Conditional rate	Lifetime prevalence of PTSD among individuals who have experienced at least one Criterion A event of a given type	p(PTSD | Trauma)
Attributable rate	Lifetime prevalence of PTSD that is attributable to a specific type of trauma (i.e., worst event).	p(PTSD ^ Trauma)

A Note on Epidemiological Data

Over the course of the next sections, I will present data on epidemiological rates of PTSD within the United States and across the world. Overall, the data suggests that PTSD does not consistently track with rates of violence and adversity. In some cases, PTSD rates seem to be *lower* in locations and times when adversity is rife. Because this pattern is quite surprising, questions about the validity of the data might spring to mind. It is rational to apply greater scrutiny to claims that are especially surprising and contradict our intuitive psychology.

The use of epidemiological surveys (as opposed to treatment seeking rates, total number of diagnoses given, or simple polling) can alleviate some, but not all, of our doubts. Epidemiological studies (1) use representative samples of the total population, (2) ask about specific symptoms and frequencies to arrive at a diagnostic status rather than relying on self-diagnosis, and (3) use semi-structured interviews and trained interviewers to help identify cases of inappropriate self-report, ensuring participants fully understand the questions.

First, epidemiological studies select representative samples of the total population of interest. In practical terms, this means that the ethnic, racial, gender, education level, and region of the sample are carefully selected in order to mirror the makeup of the target population (in this case, the general population of the United States of America or another target nation). To appreciate the importance of a representative sample, it is useful to consider an *unrepresentative* sample. Imagine, for instance, that our data reflected PTSD rates among individuals seeking care at hospitals and counseling centers. Since levels of stigma around mental illness strongly affect who seeks treatment, increasing rates of PTSD over time in this unrepresentative sample might reflect the fact that more people are willing to seek care. It might also be biased towards the upper classes with better insurance policies or be biased by changes in healthcare systems over

time. Luckily, we can examine epidemiological studies that are designed to be nationally representative, partially alleviating these types of validity concerns. It should be noted that epidemiological data do not completely alleviate these problems. Even in a representative anonymous survey in which participants, stigma *still* might prevent participants from disclosing certain types of experiences.

Second, epidemiological studies of mental illness do not ask participants to diagnose themselves. Instead, they ask specific, carefully constructed questions about events and symptoms the participants might have experienced. As in example, we can consider the NESARC-III study, for which the researchers used the NIAAA Alcohol Use Disorder and Associated Disabilities Interview Schedule-5 (AUDADIS-5; Grant et al., 2015). Rather than asking participants if they have experienced a "traumatic event" (which might be biased towards participants' own perceptions of what is "traumatic"), the AUDADIS-5 specifically asks participants to report on 19 specific events they might have experienced and 13 they might have witnessed, learned about, or may have been repeated exposed to. After specifically interrogating details of the event to ensure it matched their standardized criterion of a potentially traumatic event, the researchers ask about specific symptoms in reference to the event. As an example, consider the following question:

"AFTER (that/that worst) event happened…

> [Did you] have any physical reactions when something reminded you of (that/that worst) event, like breaking out in a sweat, breathing fast, or feeling your heart pounding? Again, this could happen when someone reminded you of the event OR in a situation that reminded you of it, OR around the same time of year it happened"

This question (N12Q5G) is used to assess a single symptom of PTSD, namely "intense or prolonged psychological distress or marked physiological reactions in response to internal or external cues that symbolize or resemble an aspect of the traumatic event(s)." To receive a PTSD diagnosis in an epidemiological survey, participants would respond to such a question (including follow-ups from the interviewer) for each symptom of PTSD, answering affirmatively to sufficient symptoms to qualify for the diagnosis.

Asking about specific symptoms (rather than asking participants to self-diagnose) helps researchers control for potential sources of error. For instance, it is likely that the term "posttraumatic stress disorder" or "PTSD" have become more widely known in the United States over the past few decades. If researchers relied on self-diagnosis to report rates of PTSD, they might see increases in the PTSD rates simply due to increased familiarity with the disorder. Similar confounds might arise from reduced stigma surrounding PTSD (though stigma against mental illness is not uniformly in decline; Schomerus & Angermeyer, 2017). By asking about specific symptoms, epidemiological rates of PTSD are partially (but not entirely) protected from this problem. Individuals may still be reluctant to disclose specific symptoms that they feel are embarrassing or socially unacceptable, but a dimensional view (endorsing specific symptoms) typically provokes less fear of stigma than endorsing a mental health diagnosis (e.g., Corrigan, 2017).

Third, epidemiological studies typically use semi-structured interviews with trained interviewers. This helps ensure that participants are answering the questions in the precise way that the researchers intend them to answer. That is, it helps ensure that the participants fully understand the questions and answer accordingly. This is especially important for PTSD, where some common symptoms can be easily misunderstood. For instance, individuals often

misunderstand the term "flashback", which refers to a dissociative reaction in which the individual feels (and sometimes acts) as if the traumatic event is actually recurring. Flashbacks are a very rare symptom; a common misunderstanding of flashbacks is that they refer to times in which individuals are suddenly reminded of the traumatic event; having thoughts or images of the event appear in their mind (this is a separate symptom typically referred to as "intrusive memories"). Trained interviewers using semi-structured interviews can avoid such confusion by asking carefully constructed standardized questions and by following up on questions when there are any doubts about the answer. Trained interviewers quickly become aware of common pitfalls and misunderstandings, clarifying when appropriate. If researchers used self-diagnosis or even written self-report of specific symptoms, we might have additional doubts about rates of PTSD over time. For instance, it might be possible that cultural misunderstandings of certain symptoms become more common over time. With semi-structured interviews in epidemiological studies, this validity concern is appropriately addressed.

The Prevalence of Relevant Adverse Events in the United States

How has the prevalence of adverse events changed in the United States in the years between 1981-2013, the years for which we have epidemiological data on PTSD? As a first look, we can examine the lifetime prevalence of specific trauma types from the major epidemiological studies on PTSD (the same studies we will use in later sections, minus the ECA, which did not assess trauma prevalence). This is perhaps the most direct way to address the issue. Unfortunately, it comes with serious limitations.

Assessments of lifetime trauma are *retrospective*, meaning that the interviewers ask the participants to reflect back on their whole life to recall whether they experienced certain types of events. Retrospective reports of trauma are notoriously unreliable. In one longitudinal study,

researchers gathered retrospective reports of lifetime trauma from the same participants using a semi-structured diagnostic interview in 1993 (participants aged 34-35) and again in 1999 (participants aged 40-41; Hardt & Rutter, 2004). Overall, 63.9% of participants showed inconsistencies in reporting, with 33.1% reporting new traumas in 1999 that occurred prior to 1993, and 40.2% of participants reporting a trauma in 1993 that they failed to report in 1999. This general inconsistency is further exacerbated by the fact that current mental states affect the reporting of retrospective events; in particular, negative emotional states in the present influence the recall of negative events in the past (Raphael & Cloitre, 1994; Schraedley, Turner, & Gotlib, 2002; but see also Brewin, Andrews, & Gotlib, 1993).

The lifetime prevalence of specific adverse events is also problematic because one individual can experience multiple adverse events of the same type. For some types of adverse events, this is the norm rather than the exception. Thus, it might be possible for the overall number of adverse events to decrease or increase in a population without a corresponding change in the lifetime prevalence of those same events. Conversely, the lifetime prevalence could decrease even if the overall number of adverse events remained constant (reflecting an increase in variability, where some unlucky individuals experience multiple traumas, but an increasing proportion of individuals experiences exactly zero traumas). Stated another way, the most relevant statistic is the number of adverse events, not the number of persons who have experienced 1+ (but potentially many more) adverse events.

Another issue is that the trauma assessment differs significantly across each epidemiological study of PTSD. The problem is quite dramatic when it comes to calculating overall exposure to trauma. In the ECA, there were 7 categories of trauma. These categories expanded rapidly across each subsequent study, with 34 total categories in the NESARC-III.

This expansion included breaking up existing categories as well as adding completely new ones. As one extreme example, NESARC-II included the event "Respondent indirectly experienced a terror attack (e.g., 9/11 on radio or TV)", to which 86% of participants endorsed exposure. Exposure to this event alone was higher than the aggregated exposure to all events in all other studies. This makes the overall lifetime exposure rate to trauma essentially nonsensical when comparing the NESARC-II to other studies; similar examples make comparison meaningless in almost every case. Luckily, trauma assessment differences affect the overall rate of PTSD less dramatically, as newly added categories rarely account for a large proportion of PTSD cases. For example, watching 9/11 on TV or radio event only accounted for 4% of PTSD cases in NESARC-II (nudging the total lifetime rate from 6.1% to 6.4%).

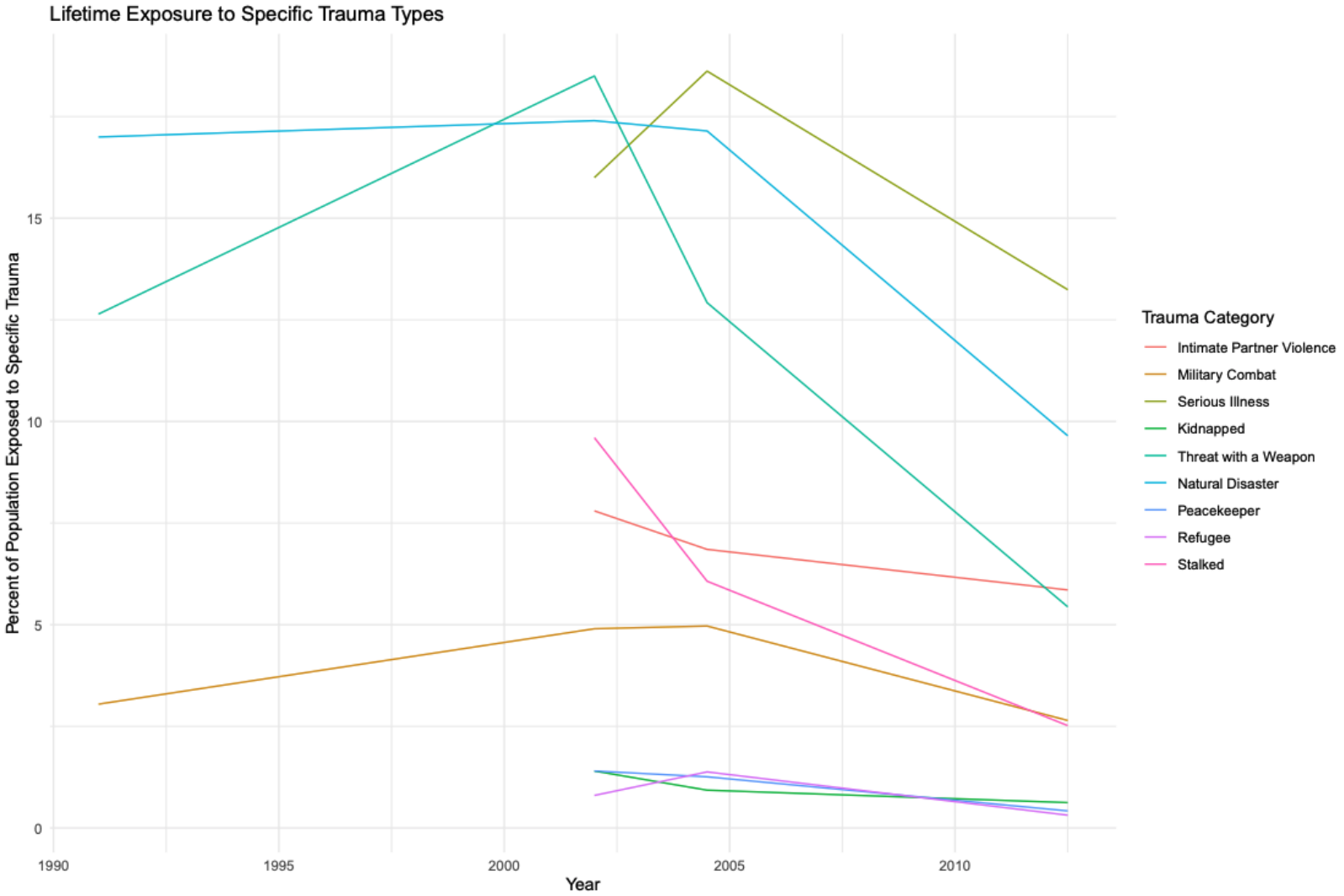
Perhaps we can aggregate exposure by the specific domain of trauma? For some statistics, it might be possible to collapse multiple types of trauma into a broader category by taking the sum or average. However, this does not work for the prevalence: the average prevalence of "witnessing someone else's serious injury" and "witnessing someone else's serious illness" does not equal the overall prevalence of "witnessing someone else's serious injury or illness"[4]. Instead, our only option is to look only at the few categories that were kept consistent over the years. There are only three categories that match in all four available studies (natural disaster, active military combat, and being mugged or threatened with a weapon), and a handful more that match in three of the four studies.

With these limitations in mind, the event exposure rates are shown in Figure 2.1. The resultant figure is somewhat chaotic, but appears to indicate an overall decrease for most types of

[4] Neither does the sum; individuals endorsing both events would be counted twice. This might be possible to calculate with the original data, but only summary data are available. Notably, taking the sum is viable when examining the attributable rate, as "worst events" are mutually exclusive.

events between 2002-2013. This decrease seems most consistent between the final two measurement points, which represent the NESARC-II and NESARC-III, which is notable because these two studies were closest in terms of the categories they used, increasing confidence that this is a real effect.

Figure 2.1. Self-Reported Lifetime Exposure to Specific Adverse Events in Major Epidemiological Studies



Note. This figure displays the few event categories that were kept largely consistent across studies. Corresponding descriptions for each category can be found in the Appendix.

As mentioned earlier, the lifetime exposure to specific traumatic events that is collected in epidemiological studies of PTSD is difficult to interpret. It hides the role of multiple events happening to a single person, is biased through retrospection, and its assessment is incongruous across each study. Let us now turn to more objective reports of adverse events in the United States during the same time frame.

I will focus primarily on interpersonal violence and serious crime, events that have the advantage of (1) being included in Criterion A since the inception of the diagnostic PTSD concept, (2) being available via consistent, high-fidelity longitudinal reports and (3) being highly relevant to PTSD (though clearly not covering the entire scope of potentially traumatic events). First, I will focus on homicide. The second-hand effects of homicide account for only a small proportion of PTSD. However, homicide is useful because it is a highly objective metric that typically tracks with other forms of violence. In the words of Pinker (2011, p. 62), "Homicide is the crime of choice for measurers of violence because regardless of how the people of a distant culture conceptualize crime, a dead body is hard to define away, and it always arouses curiosity about who or what produced it. Records of homicide are therefore a more reliable index of violence than records of robbery, rape, or assault, and they usually (though not always) correlate with them." Although homicide does not completely encompass all adverse events capable of producing PTSD, it is perhaps the best single-variable proxy.

The Federal Bureau of Investigation (FBI) compiles and releases national data on homicide and other major crimes as part of their Uniform Crime Reporting (UCR) program. Important to our aims, the primary aim of these data is to track crime *over time,* and hence consistency of measurement across years is one of the UCR's most important priorities. The FBI data on homicide is presented in Figure 2.2. The overall trend is a clear decrease, though there is

a small increase between the early 1980s and early 1990s. The FBI data on other major crimes are presented in Figure 2.3. These are slightly more mixed, with some crimes such as aggravated assault showing a strong initial increase, followed by an equally steep decline. All metrics show an overall decrease.

Figure 2.2. Homicide Rates Between 1981-2013 in the FBI Data

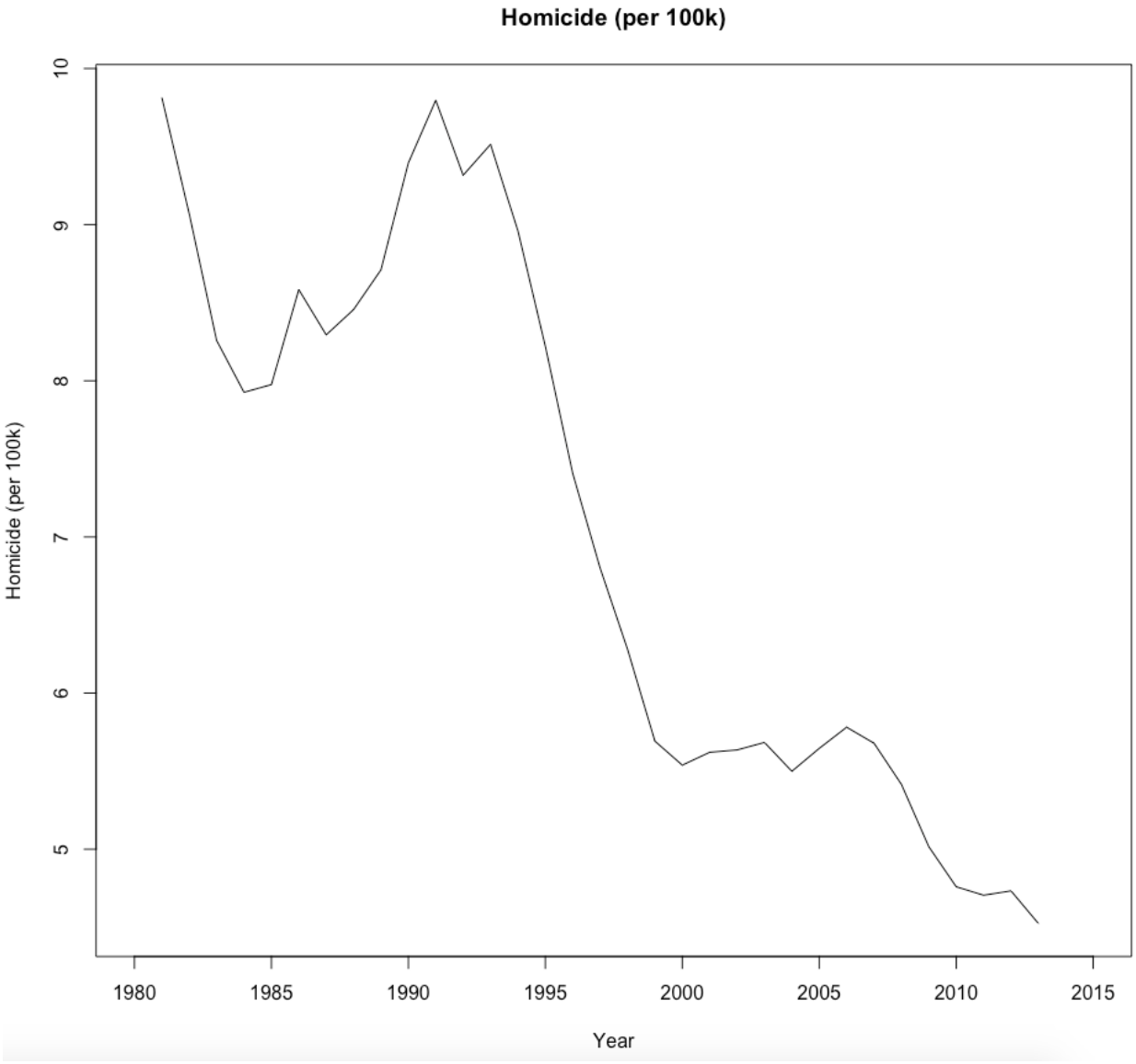
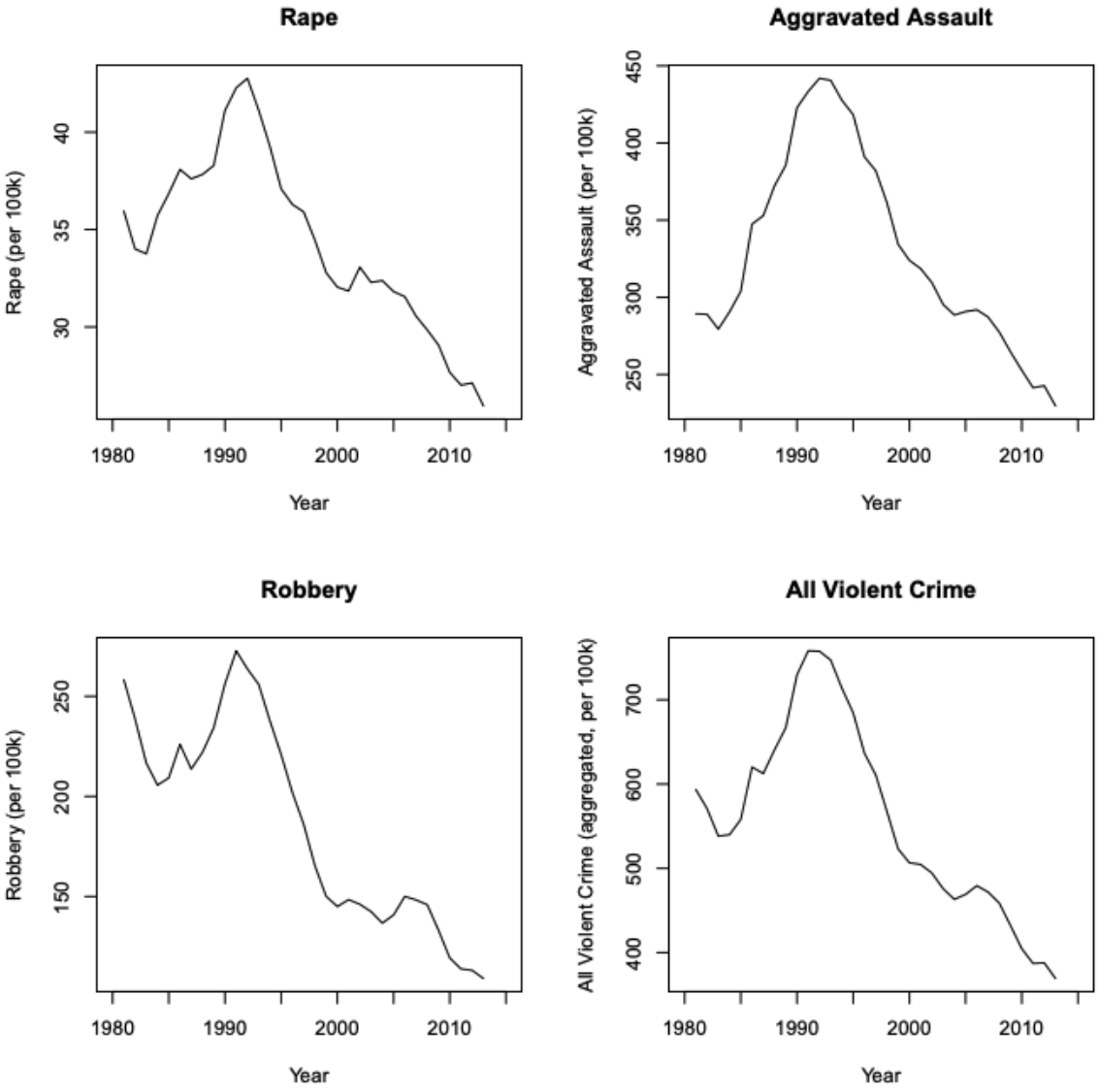


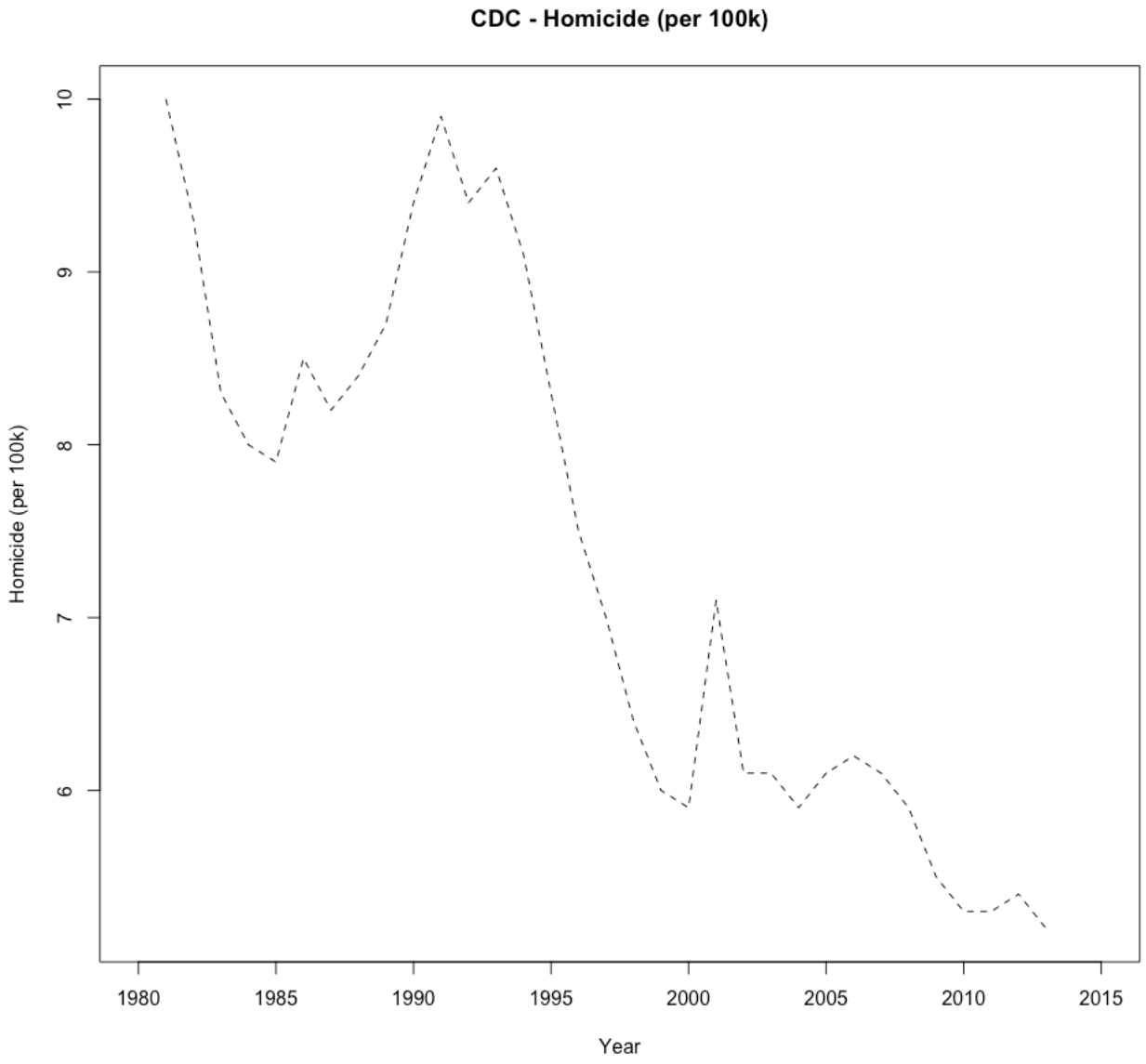
Figure 2.3. Other Major Crimes Data from the FBI Uniform Crime Reporting Program



Note: The vertical axes are defined by the minimum and maximum of each series. This is ideal for illustrating relative increases or decreases, but it notably obscures the base rates of each event.

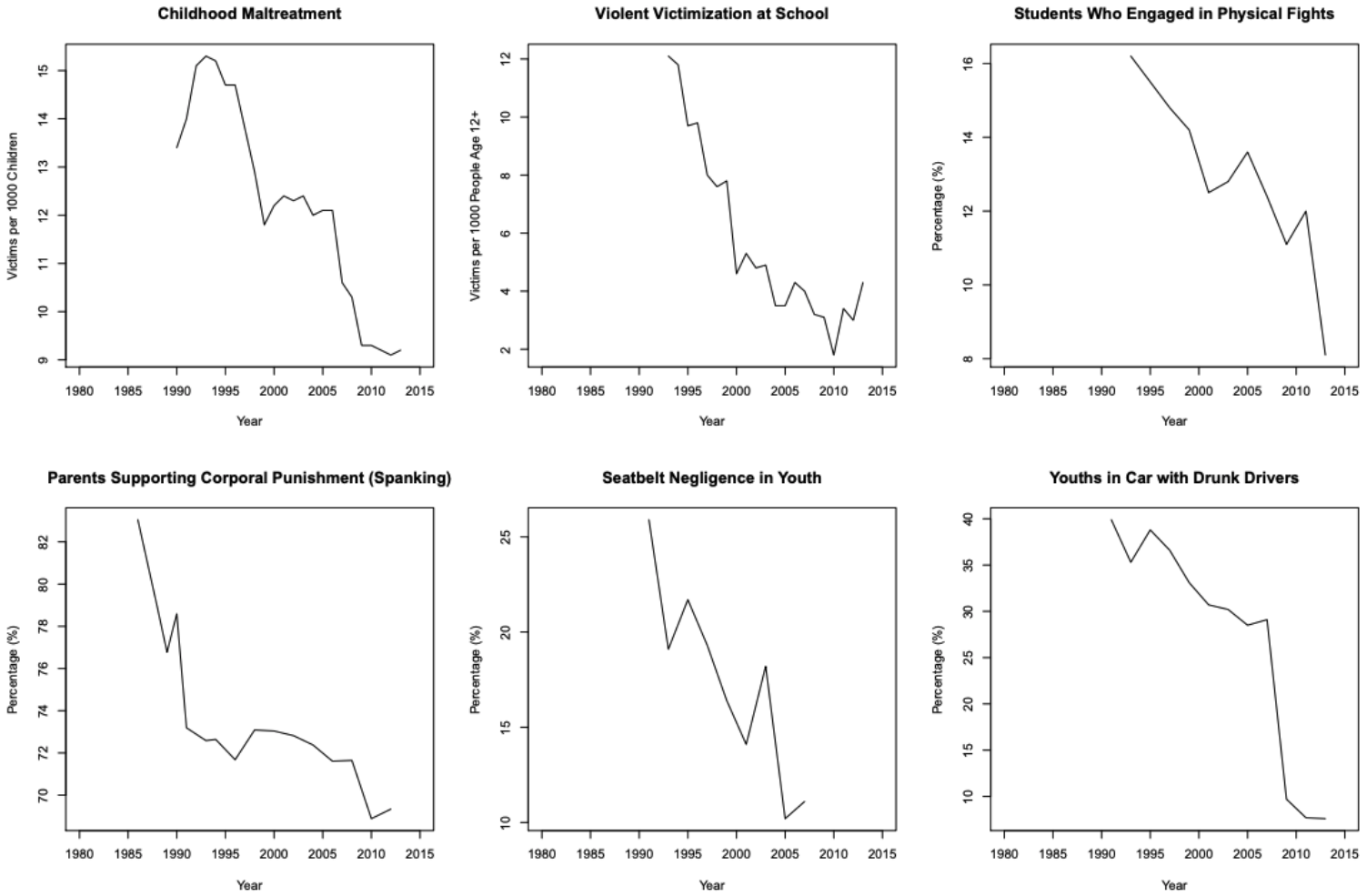
To increase our confidence in the reliability of these data, we can compare to other data sources. Figure 2.4 displays the homicide rate for the same period as collected by the Center for Disease Control (CDC) Causes of Death report. Rather than relying on aggregated police reports, as in the FBI data, the CDC relies on aggregations of death certificates. The same pattern is evident in these data; indeed, it is difficult to distinguish the two graphs, though they are in fact distinct.

Figure 2.4. Homicide Rates Between 1981-2013 in the CDC Data



The FBI Crime reports are the most reliable source of objective adverse events that affect adults in the US. However, adverse events affecting youth are also relevant. Various agencies including the US Bureau of Justice Statistics, the General Social Survey, the US Department of Health and Human Services, and the CDC keep tallies of adverse events that happen to youth. Figure 2.5 displays several of these types of reports.

Figure 2.5. Adverse Events Affecting Youths and Related Statistics



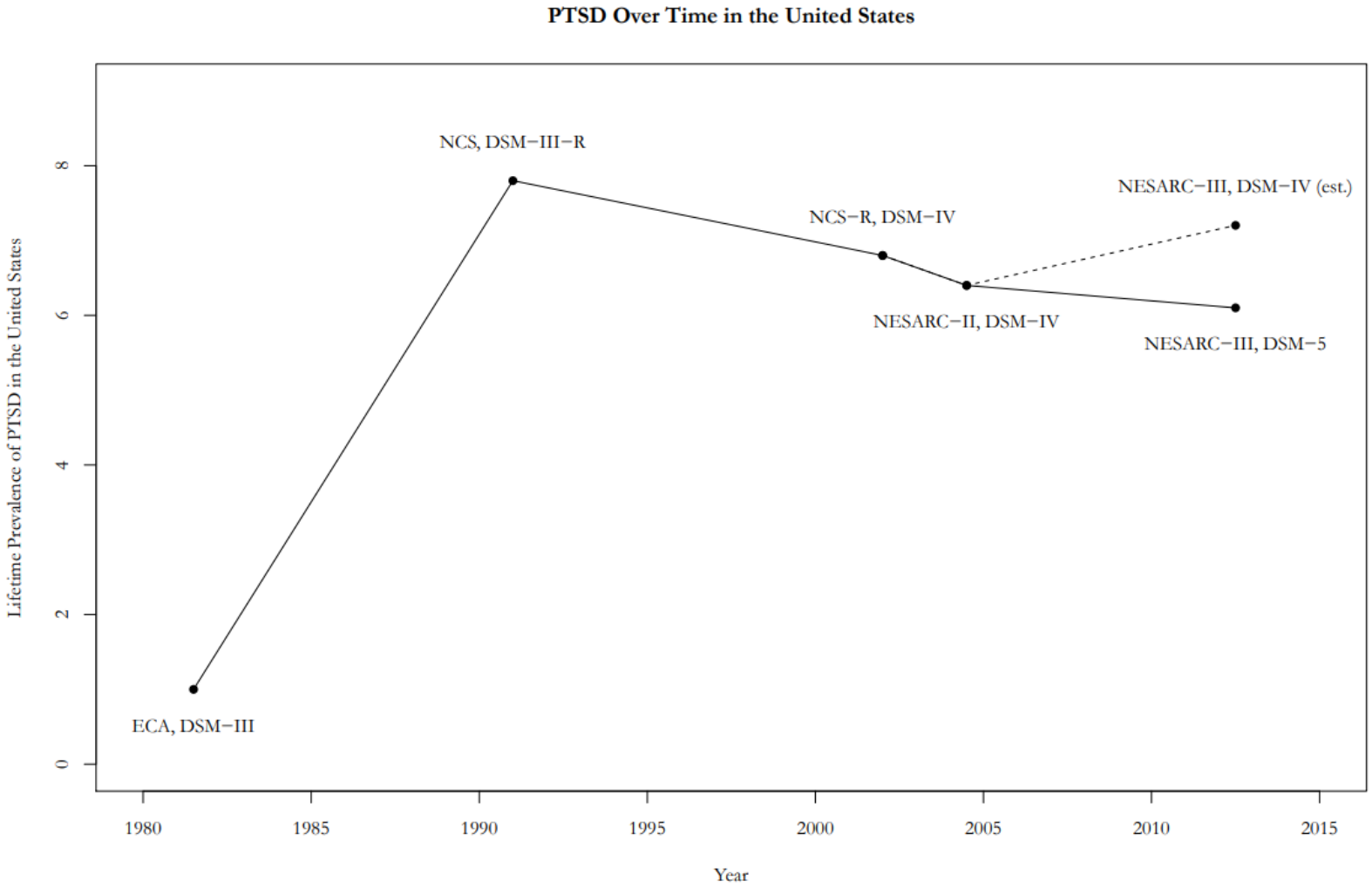
Sources: Childhood maltreatment & victimization – US Department of Health and Human Services, Administration for Children and Families. Violent victimization at school – Our World in Data (Bureau of Justice Statistics). High school students engaging in physical fights on school grounds – Centers for Disease Control and Prevention (CDC). Parents who "Strongly agree" or "Somewhat agree" that spanking should be used as a form of discipline – Our World in Data (General Social Surveys). High school students who reported "never" or "rarely" wearing a seatbelt – CDC. High school students who rode with a driver who had been drinking – CDC.

The Overall 12-Month Prevalence of PTSD in the United States

The most straightforward epidemiological metric to consider within the United States is the overall prevalence of PTSD. Many of the same caveats mentioned earlier also apply here: assessments are not perfectly consistent across studies, especially in terms of what is counted as a potentially traumatic event (Criterion A). Indeed, the various data points span four separate versions of the DSM.

Figure 2.6 displays the relevant data points on PTSD prevalence from available epidemiological studies that (1) used large, nationally representative samples, (2) assigned PTSD diagnoses according to DSM criteria, and (3) used highly validated semi-structured interviews conducted by a trained interviewer. The data indicate that PTSD prevalence estimate increased between 1981-1992 (spanning the transition from DSM-III and DSM-III-R), and then remained mostly stable from 1992 to 2013 through the DSM-IV and DSM-5 eras.

Figure 2.6. PTSD Over Time in the United States



ECA = Ecological Catchment Area Survey (Helzer et al., 1987), NCS/NCS-R = National Comorbidity Survey (& Replication; Kessler et al., 1995; Kessler et al., 2005), NESARC-II/III = National Epidemiological Survey on Alcohol and Related Conditions (Pietrzak et al., 2004; Goldstein et al., 2012). The DSM-IV prevalence for NESARC-III is estimated based on diagnostic comparison data of DSM-IV versus DSM-5 symptomatology reported in Kilpatrick et al., 2013.

Although these data indicate an overall increase, if we exclude the ECA and the DSM-IV estimate for the NESARC-III, can we begin to imagine that maybe PTSD might be in a decline that matches our expectations? There are two reasons why such a conclusion would be flawed. First, there is no principled reason to exclude the ECA data. It is true that each successive edition of the DSM included diagnostic changes (which I later discuss in detail), but there is no strong reason to expect that the DSM-III in particular would be a special outlier, especially compared to the DSM-III-R. The only empirical study to compare the DSM-III to DSM-III-R criteria indicated that the DSM-III-R criteria were overall *stricter* (Schwarz & Kowalski, 1991; though see later discussion).

Second, it would be dubious to favor the NESARC-III DSM-5 estimate over the DSM-IV estimate, especially considering that the previous two estimates used the DSM-IV. Although data on earlier editions is limited, we have much better quality data regarding diagnostic differences between the DSM-5 and DSM-IV. The DSM-5 to DSM-IV conversion rate here was generated from a large national survey matched to US Census demographics that simultaneously evaluated both sets of DSM criteria (N = 2953; Kilpatrick et al., 2013). This provides a trustworthy source for converting between the two sets of criteria. To preserve consistency across time, the NESARC-III DSM-IV estimate should be preferred.

All things considered, how well does the pattern in Figure 2.6 match the decline of violence we examined earlier? Not particularly well. From 1981-1991, PTSD rates rose. Some objective markers of violence increased during that time, whereas others formed a U-shape. From the early nineties onward, however, the rate of homicide, rape, aggravated assault, and robbery declined. This contrasts with the rate of PTSD prevalence, which has remained stable

since the nineties. Even if we charitably massage the data such that PTSD shows a decline, its decline does not match the slope of the decline of violence.

One potential issue is that the lifetime prevalence of PTSD should be expected to lag somewhat behind the violent crime rate. Perhaps the PTSD rate will plummet in the next few years, matching the decline in the crime rate but lagged due to the measurement interval.

However, this pattern seems unlikely given that PTSD is overrepresented among more recent cohorts, even after controlling for the typical age of onset of PTSD. If anything, we should expect the PTSD rate to rise in the future rather than suddenly plummet to match declining rates of violence. Using cohort as a statistical predictor[5], the NCS-R reported that the odds that Gen Xers (those born 1962-1973, age 18-29 at the time) will suffer from PTSD in their lifetimes was 15% higher than Baby Boomers (1941-1961, age 30-44 in 1991), 30% higher than those in the Silent Generation (1932-1946, age 45-59 in 1991), and 500% higher than those in the early Silent Generation or Greatest Generation (prior to 1932, age 60 and above). Indeed, the lifetime PTSD rate among Gen Xers was 6.3%, nearly reaching the combined rate of 6.8% (with still more than 50 years of their lives remaining on average). More recent results are not encouraging. Data on Millennials from the NESARC-III are consistent with the increasing pattern of PTSD across generations. NESARC-III Millennials (1983-1995) had a 7.8% lifetime converted DSM-IV PTSD rate (6.6% DSM-5), 24% higher than the Gen Xer rate when the Gen Xers were the same age.

[5] This analysis does not simply compare the prevalence of PTSD at different cohorts, but is based on a discrete-time survival analysis to attempt to control for age based on retrospective reports of age of onset. The NCS-R report states "The method used to estimate lifetime risk was based on the assumption of constant conditional risk of first onset in a given year of life among people who differ in age at interview. This assumption is almost certainly incorrect in light of evidence for significant inter-cohort differences in lifetime prevalence. Because the lifetime estimated prevalence was higher in more recent cohorts, lifetime risk in younger cohorts will be *underestimated*." (emphasis added, pg. 599)

Is this effect due to age, period, or cohort? Although PTSD does tend to onset early, *higher* rates in younger cohorts cannot possibly be accounted for by age alone. Because we are examining the lifetime prevalence, even with an age effect PTSD rates should increase monotonically across the lifetime (assuming we can roughly trust retrospective reports). Age of onset was also controlled for in the NCS-R lifetime risk analysis. This leaves period and cohort effects. A period effect would indicate that individuals have become increasingly vulnerable over time. A cohort effect would indicate that there are differences between the cohorts, but no differences within cohorts over time (i.e., Baby Boomers would not be at higher risk now than they were in the 1990s). Unfortunately, without consistent longitudinal data, it is difficult to distinguish these two types of effects. Our small set of diverse epidemiological studies is not fit for the task.

Does Diagnostic Inflation Explain the Epidemiology Gap?

Unfortunately, the epidemiological data in the United States suffer from a serious flaw. There have been major changes in diagnostic criteria and conceptualization of the disorder during this time period, and thus the epidemiological surveys differed in the exact criteria they used to define a case of PTSD (e.g., Breslau & Kessler, 2001). Although we can infer DSM-IV rates for three large epidemiological surveys, the time period covered by these studies is relatively narrow (2001-2013). A lack of stable measurement of PTSD prevalence rates in the United States is a threat to the validity of any interpretations we make. Specifically, it raises the possibility that PTSD rates are explained not by a neurotic treadmill, but by the threshold for diagnosis becoming more lenient over time. Many scholars have raised concerns about broadenings in the diagnostic criteria for PTSD since DSM-III. Can diagnostic inflation account for changes in PTSD prevalence?

Each new release of the DSM has reliably provoked arguments among scientists, practitioners, and other mental health professionals regarding changes to diagnostic criteria. Many criticisms have centered on expansions to diagnostic categories that make the criteria more lenient and easier to meet. Some scientists worry that widening criteria may lead to inappropriately pathologizing normal human distress. A poignant example is the removal of the bereavement exclusion for depression in DSM-5, which provoked substantial controversy (Zachar, First, & Kendler, 2015). PTSD has been the subject of similar controversies regarding bracket expansions, which we will cover later in this chapter. Changes in DSM versions have included diagnostic tightening as well as loosening. For example, the DSM-5 famously tightened criteria for bipolar disorder to prevent overdiagnosis among youths.

Have successive versions of the DSM led to an overall inflation in diagnostic breadth? Fabiano and Haslam (2020) tackled this question using a unique meta-analytic strategy. They considered 123 studies in which a single sample was simultaneously diagnosed using two or more separate versions of the DSM (e.g., a PTSD study in which individuals were diagnosed using both DSM-IV and DSM-5 criteria). This allowed them to empirically compare the breadth of diagnosis in each version: by holding the sample constant, they ensured that any changes in diagnosis are due to changes in the DSM criteria. They quantified the change as a risk ratio from the newer DSM version to the older DSM version: a risk ratio of 1.50 would signify that diagnosis is 50% more likely in a successive DSM version. Overall, they came to the somewhat surprising conclusion that successive versions of the DSM have *not* resulted in overall inflation. The average risk ratio across all effects was 1.00 (95% CI = 0.93-1.08).

This overall null effect does not mean that specific changes in DSM criteria have not resulted in expansions. For example, in some cases an expansion occurred between DSM-III and

III-R but was reversed in DSM-IV or 5. Some specific disorders have also expanded or contracted overall. For example, Fabiano and Haslam (2020) identify ADHD as an example of overall inflation. Surprisingly, their statistics place PTSD as an example of overall *deflation*, with the DSM-5 being 37% *stricter* than the DSM-III (risk ratio = 0.63). This effect is mostly accounted for by a large contraction from DSM-III to DSM-III-R (risk ratio = 0.55). There are reasons to distrust this meta-statistic, however. First, their literature search identified only a single study empirically comparing DSM-III to III-R and DSM-III-R to IV. Second, and more importantly, this study and many others they meta-analyze compare rates of DSM diagnoses *among trauma survivors*, not rates in the general population (e.g., diagnoses among survivors of a school shooting, diagnoses among military personnel[6]). Thus, these statistics miss some of the most controversial and important diagnostic inflations in PTSD that might affect the epidemiological studies: changes to Criterion A. Criterion A defines what counts or does not count as a "trauma"; by focusing on a specific trauma, their risk ratio largely misses expansions resulting from new types of trauma being added to the pool of PTSD diagnoses.

Let us temporarily put aside this statistical analysis and turn instead to a criterion-based, theoretical examination of DSM expansions and contractions. Considering each successive version, do conceptual expansions or contractions correspond to the changes we see in the epidemiological rates of PTSD? From DSM-III to DSM-III-R, the criteria expanded to (explicitly) include vicarious exposure (e.g., "in some cases the trauma may be learning about a serious threat…e.g., that one's child has been kidnapped, tortured, or killed", APA, 1987, pp. 248). DSM-III differentiated between acute and chronic PTSD (defined by at least 6 months duration); DSM-III-R removed this distinction and shifted the required duration of symptoms to

[6] Of the 11 PTSD studies identified in the Appendix, 9 used samples of trauma survivors. Only one study drew from the general population (one other used a treatment-seeking sample).

one month. On the other hand, DSM-III-R also made the diagnosis explicitly stricter by adding the specification that traumas should be "outside the range of usual human experience[7]" (APA, 1987, p. 250). The single empirical study comparing the DSM-III and DSM-III-R criteria suggested that DSM-III-R was substantially stricter overall (although limited to a non-representative sample of individuals exposed to a school shooting, limiting generalizability; Schwarz & Kowalski, 1991). Meanwhile in our epidemiological rates, PTSD prevalence rates jumped from 1% in the National Catchment Area Survey (using DSM-III; Helzer et al., 1987) to 7.8% in the original National Comorbidity Survey (using DSM-III-R; Kessler et al., 1995). Given the theoretically mixed expansions and contractions in criteria from DSM-III to III-R (and empirical evidence contradicting the idea that DSM-III-R criteria were broader than DSM-III; Fabiano & Haslam, 2020), the dramatic jump from between these two estimates is puzzling. One guess is that idiosyncrasies in the ECA led to an underestimate, and idiosyncrasies in the NCS led to an overestimate. Interestingly, if this were true, it would make our Figure 2.6 look like a smoothly increasing curve over time.

Next, the diagnostic conceptualization expanded rather dramatically in breadth in the DSM-IV (APA, 1994). The idea that trauma must be “outside the normal range of human experience” was removed, and Criterion A expanded considerably (e.g., “events experienced by others that are learned about include, but are not limited to, violent personal assault, serious accident, or serious injury experienced by a family member or close friend; learning about the sudden, unexpected death of a family member or close friend; or learning that one’s child has a life-threatening disease”, APA, 1994, p. 424). Yet the rate of PTSD actually slightly declined to 6.8% in the 2001-2003 National Comorbidity Survey Replication (using DSM-IV; Kessler et al.,

[7] In DSM-III the phrase “outside the range of such common experiences” is used in the narrative description of the disorder, but not within the criteria themselves (APA, 1980, p. 236)

2005) and 6.4% in the 2004-2005 National Epidemiologic Survey on Alcohol and Related Conditions Wave 2 (NESARC-II; using DSM-IV, Pietrzak, Goldstein, Southwick, & Grant, 2011).

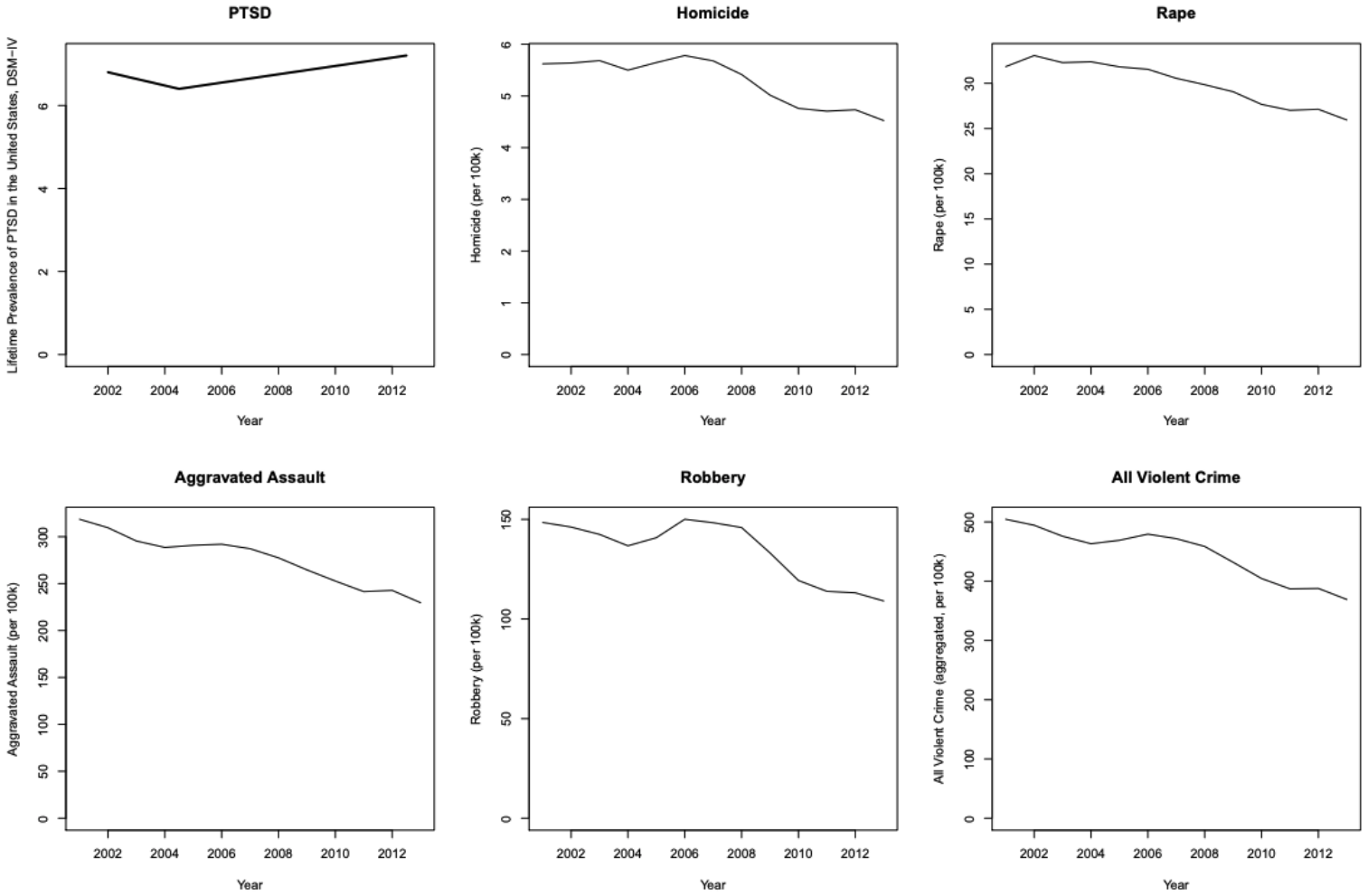
Finally, the diagnostic criteria were tightened in the DSM-5 (Pai, Suris, & North, 2017). The types of events qualifying as trauma narrowed: medically based trauma was restricted to sudden catastrophes not due natural causes (e.g., anaphylactic shock; excludes terminal cancer and heart attacks) and indirect traumas were limited to those occurring to family members, close associates, or experienced repeatedly as part of professional responsibilities (e.g., forensic child abuse investigators). The 2012-2013 NESARC-III indicated fairly stable rates of PTSD at 6.1% (using DSM-5; Goldstein et al., 2016). A nationally representative online epidemiological study by Kilpatrick and colleagues (2013) is illustrative: using DSM-IV criteria they estimate a PTSD prevalence of 9.8% and using DSM-5 criteria on the same sample the estimate a prevalence of 8.3%. Fabiano & Haslam's (2020) meta-analysis of diagnostic comparisons also suggests that DSM-IV and DSM-5 rates of PTSD are generally comparable (risk ratio = 0.97). Diagnostic differences may have influenced prevalence estimates, but the changes do not closely correspond with the PTSD rate.

Diagnostic changes certainly matter. Yet the changes in PTSD rates when we jump from version to version are not predictable given the diagnostic changes, and diagnostic changes over the years have involved both tightening and loosening the criteria. Overall, diagnostic changes do not provide a satisfying explanation for a lack of decline in PTSD rates.

If an examination of the diagnostic changes has caused the reader to lose all faith in the validity of the epidemiological trends since 1985, we can at the very least rely on the range of data spanning from 2001-2013. The NCS-R and NESARC-II used identical DSM-IV criteria.

The NESARC-III used DSM-5 criteria. Luckily for us, Kilpatrick and colleagues' (2013) large, nationally representative survey allows us to convert the NESARC-III DSM-5 PTSD rate to the inferred DSM-IV rate, and we can do so with a high degree of confidence. Figure 2.7 juxtaposes this restricted range of data for which we can infer DSM-IV diagnoses of PTSD with the FBI rates of violence. Although this trustworthy slice of data is narrow, it confirms that diagnostic changes across DSM version are not the whole story.

Figure 2.7. DSM-IV PTSD and Violent Crime in the United States, 2001-2013



Note. NESARC-III DSM-IV rates are converted following Kilpatrick et al., 2013. Vertical axes range from zero to the maximum series value. For reference, PTSD increased by 6%, homicide declined by 20%, rape declined by 23%, aggravated assault declined by 39%, robbery declined by 36%, and all violent crime declined by 37%.

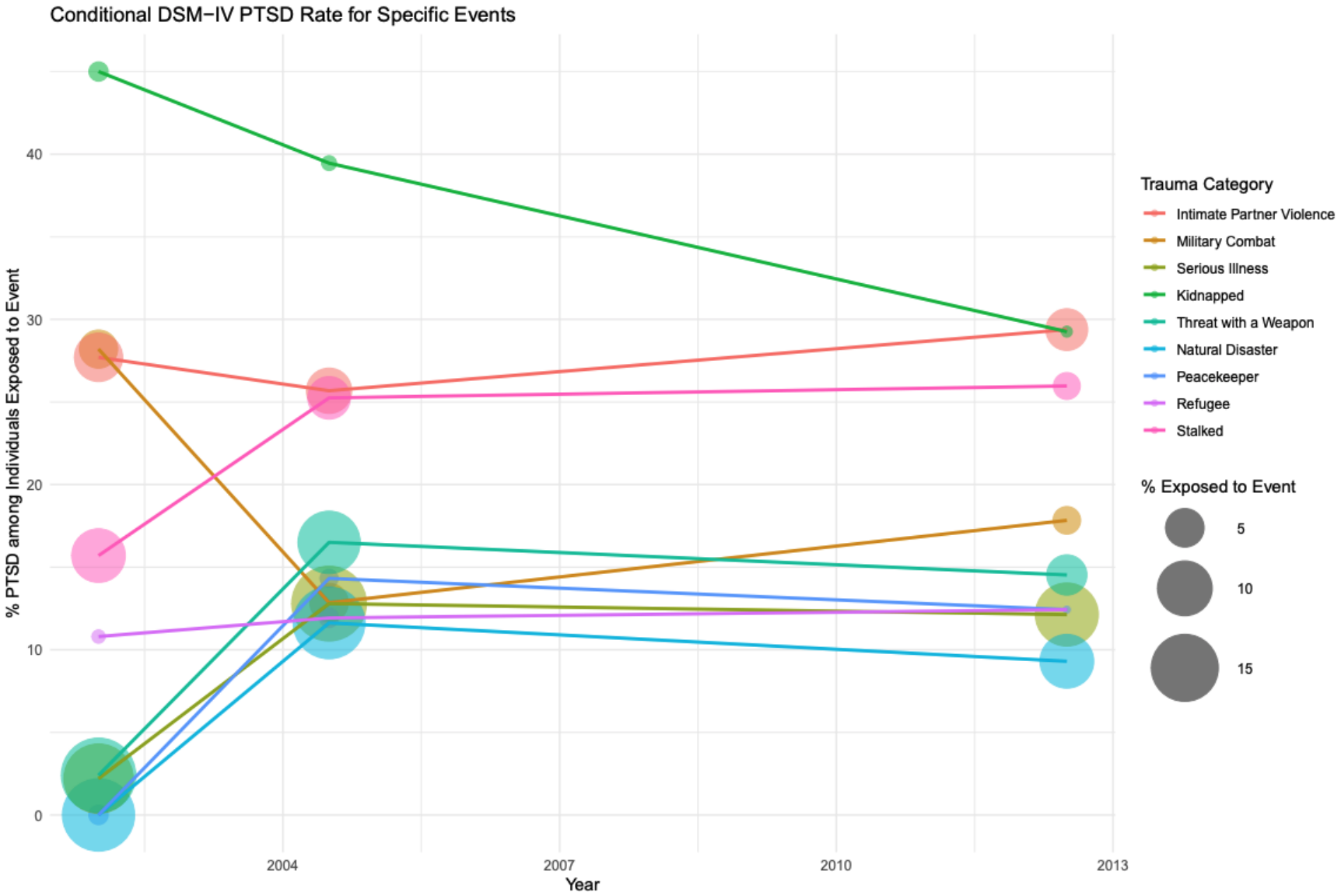
The Conditional Rate of PTSD Given Exposure to Specific Events

In addition to the overall rate, we can examine the *conditional* rate of PTSD (the rate of PTSD among those who experienced a specific event). The conditional rate is highly relevant to the idea of a neurotic treadmill effect because it reflects a type of vulnerability to PTSD. If there is a neurotic treadmill effect, then we would expect vulnerability to increase over time. The conditional rate most frequently reported in epidemiological studies of PTSD is the rate of lifetime PTSD among individuals who have experienced *at least one trauma* (PTSD | Event).

The global conditional PTSD rate would be the lifetime rate of PTSD among individuals who have experienced at least one trauma of *any type*. Unfortunately, due to the drastic changes in the types of events that count as trauma under Criterion A over time, this statistic is meaningless when compared across multiple surveys. To understand this, let's imagine an extreme hypothetical. Imagine that we had a Strict Criterion A that counted only rape, physical assault, sexual assault, combat, and very serious accidents as traumas. Imagine this study showed a lifetime PTSD rate of 8%, and a conditional PTSD rate of 25%. Now imagine that we had instead an All-Inclusive Criterion A that counted *every possible negative event* as trauma. Even with this drastic change in Criterion A, the overall rate may not change much, because rape, physical assault, sexual assault, combat, and very serious accidents account for the lion's share of PTSD symptomatology. Perhaps the overall rate would move to 10%. However, the conditional rate would change drastically – because everyone in our sample has experienced some negative event, the conditional rate would also be 10% (drastically lower than the earlier 25%). Expansions in Criterion A are still a problem for the overall PTSD rate, but the problem is much more drastic when considering the conditional rate.

Instead, we can examine the conditional rate for specific types of events. By tracking the conditional rates across events that have been tracked consistently across epidemiological studies, we can gain some insight into whether vulnerability has changed over time. We can use the same set of consistent events that we examined earlier when considering exposure. This is shown in Figure 2.8 (limited to DSM-IV rates for interpretability). The conditional prevalence increased for seven categories and decreased for two. As with the graphs displaying trauma exposure, this information is still very limited. It is strongly biased due to the retrospective nature of reports, and though we have selected events that are relatively consistent, there are still differences in measurement over time.

Figure 2.8. Conditional DSM-IV PTSD Rate for Specific Events.



This conditional PTSD rate is useful, but it is limited because it groups together those who experienced multiple traumas with those who only experienced one trauma. Even better would be the rate of PTSD per experienced event (PTSD | *Each* Event) over time: this would be a more direct proxy of vulnerability to certain events over time, as it would control for the overall *amount* of exposure to adverse events. Unfortunately, assessing PTSD per event is an extremely challenging epidemiological endeavor. To get a true PTSD per event rate, each participant in the sample would need to recall the discrete number of events that happened them across every single type of potentially traumatic event. Many common traumas such as domestic abuse are often highly recurrent and hence difficult to discretize into a specific count of events. Retrospective reports show biases in the consistency of reporting based on the type of event, whether the event was directly or indirectly experienced, and the life epoch in which the event occurred (Krinsley, Gallagher, Weathers, Kutter, & Kaloupek, 2003). Although epidemiologists have come up with clever techniques to address the problem, such as using random events (e.g., Breslau et al., 1998; Liu et al., 2017), these techniques have not been applied consistently enough on a national scale to give us reliable estimates of PTSD per event over time.

The Prevalence of PTSD Attributable to Specific Types of Trauma

We may gain further insight by examining the index traumas ("worst events") linked to each PTSD case in the available epidemiological datasets in the United States. Data on index traumas is available for all of the epidemiological datasets we examined earlier except NESARC-II. One downside is that each study categorized trauma types in different ways, complicating our task. Nevertheless, we can get a sense of the trend by aggregating specific trauma types into broad categories that can be made to be consistent across studies. A table summarizing this aggregation process can be found in Appendix A. This type of aggregation is

inappropriate for the conditional rate (where the addition of new categories biases the rate through multiplication), but it is less problematic for the attributable rate (where the addition of new categories may still bias the rate slightly, but through addition).

Figure 2.9 presents the rates of PTSD caused by specific domains of trauma in available epidemiological studies. Each rate represents the number of people who qualified for a diagnosis of PTSD *and* reported an event which falls in the given domain (in logical notation, our rate represents PTSD ∧ Event). Thus, the figure parallels the earlier trend we see in the total rate, with the lowest combined rate of PTSD cases in the ECA study.

Importantly, this statistic aggregates vulnerability to trauma with the prevalence of trauma. Thus, it should be interpreted in context with the prevalence of adverse events presented in earlier sections. For example, if we saw a sharp increase in the attributable rate of PTSD due to natural disaster, this could indicate either an increase in natural disasters, an increase in vulnerability to natural disasters, or both. But if a sharp increase in natural disasters is implausible, we would suspect that the increase is primarily related to vulnerability.

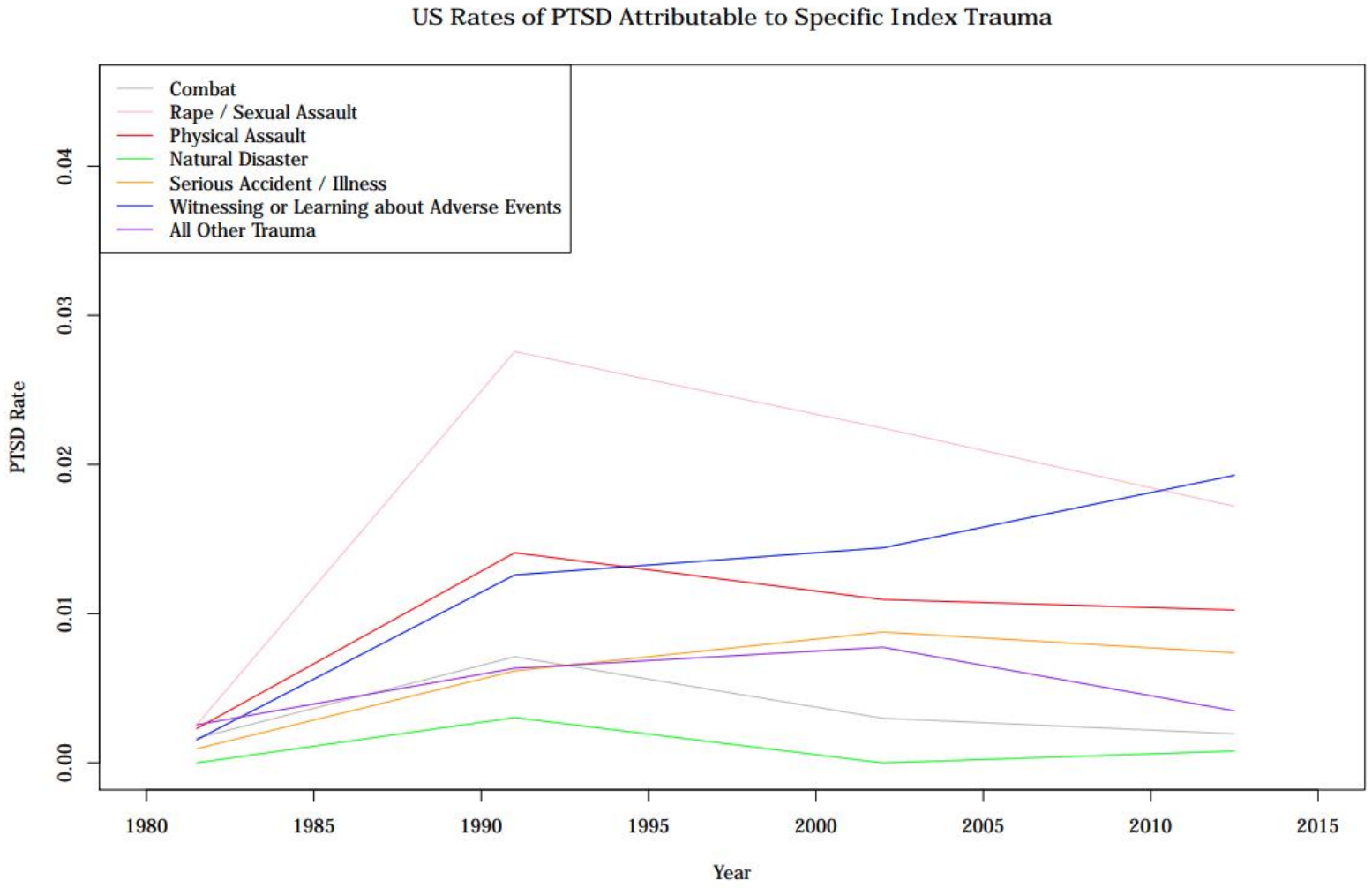
Insight about the specifics of the PTSD rate can be gained by examining the specific domains of trauma. Most prominently, we see a near-linear increase in PTSD resulting from witnessing or learning about the trauma of others. It is notable that not only do we see an influx of PTSD from "witnesses" over the years, but the epidemiological specificity of measurement also changed to accommodate this influx. In the ECA, this category was represented by a single index event ("Seeing someone hurt or die"). The NCS expanded this to two ("You witnessed someone being badly injured or killed"; "You suffered a great shock because one of the events on this list happened to someone close to you"). By the time the NESARC-III was conducted, there were 15 separate index traumas involving witnessing or learning about an event. This

increase in specificity regarding index traumas was not universal across categories: "natural disaster" and "military combat" remained a single index event across all surveys[8].

Important to our earlier question, the burden of PTSD attributable to interpersonal violence (the type of trauma we can be most confident has become rarer in the United States) has not fallen. The burden of PTSD linked to physical and sexual assault has increased over time. Considering the fact that many of the events falling under "witnessing or learning about adverse events" also involve interpersonal violence, that increase may even be steeper if we were able to separate the categories in a more detailed fashion.

[8] With the exception of the NESARC-III, which had category for prisoner of war status in addition to military combat.

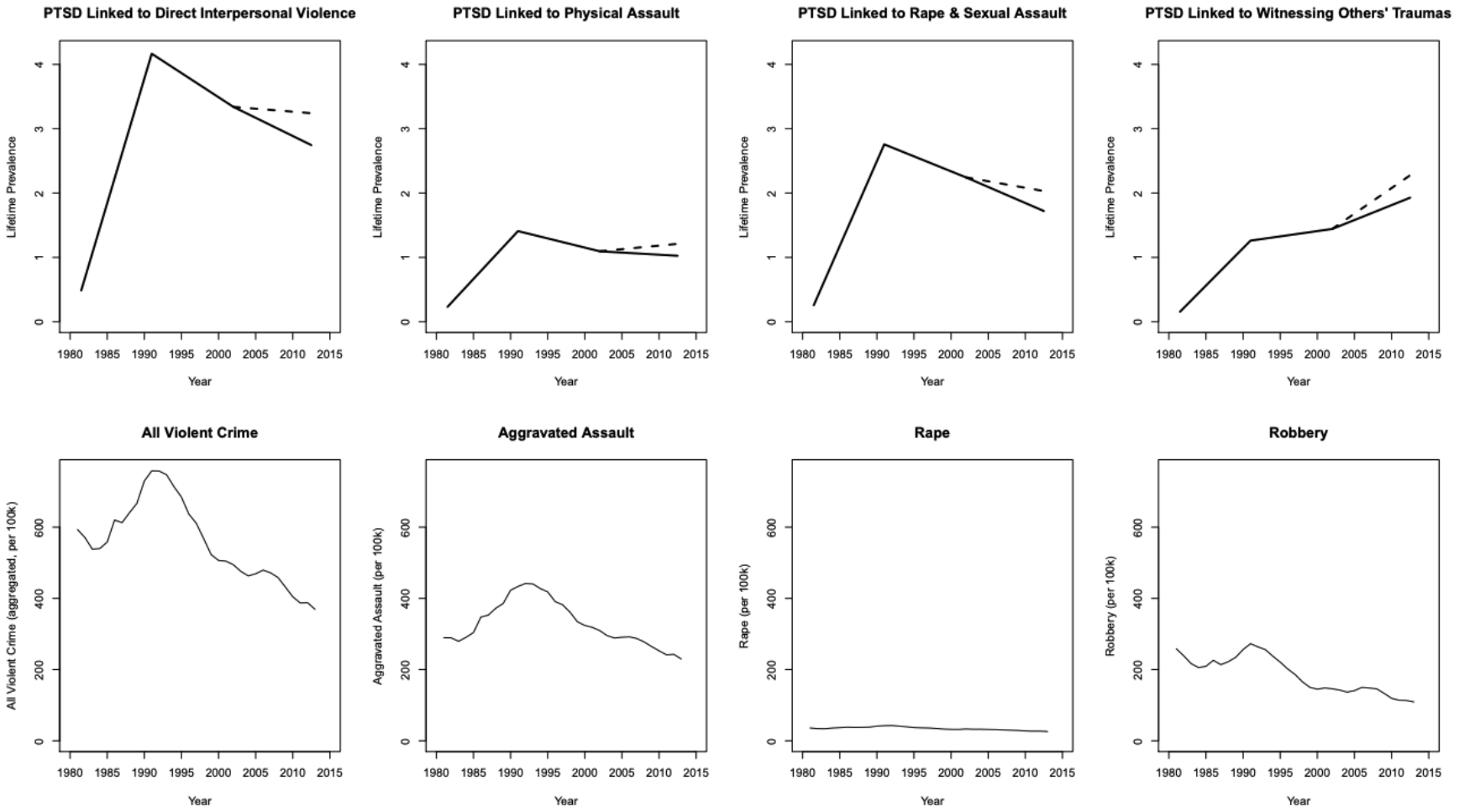
Figure 2.9. United States PTSD Rates by Specific Index Trauma



Note. Rape and sexual assault were not measured as independent index traumas in the ECA; the pictured rate represents the upper bound created by the category "All Other Trauma". Where data were presented separately by gender, the population mean was estimated via averaging

Now that we have broken down the PTSD rate by trauma type, we can also reexamine the FBI data. Figure 2.10 shows the rates of PTSD linked to interpersonal violence (rape, sexual assault, and physical assault) and witnessing trauma and juxtaposes this with the FBI crime rates. Here, the vertical axes are set to a minimum of zero and a fixed maximum. This helps better represent the different base rates of events and attributable rates and compare across statistics of different types, though it compacts the shape of the trends with low base rates (for instance, rape actually declined 28% during this period, though the trend is hard to see). Although the rates of PTSD linked to directly experiencing interpersonal violence have not kept up with the sharper decreases in interpersonal violent crime, they do seem to have declined somewhat from their peak in the NCS dataset. Witnessing or learning about trauma is the clear exception, as it shows a monotonic increase. In the NESARC-III "witnessing or learning about" category, indirect interpersonal violence (e.g., witnessing sexual abuse) accounted for 30.0% of PTSD, non-violent events accounted for 22.5% (e.g., witnessing a serious illness), and ambiguous events accounted for the remaining 47.5% (e.g., witnessing a serious or life threatening injury). Unfortunately, previous epidemiological studies do not provide the same level of detail regarding indirect traumas.

Figure 2.10. DSM-IV PTSD Rates Attributable to Specific Trauma Types and FBI Crime Data.

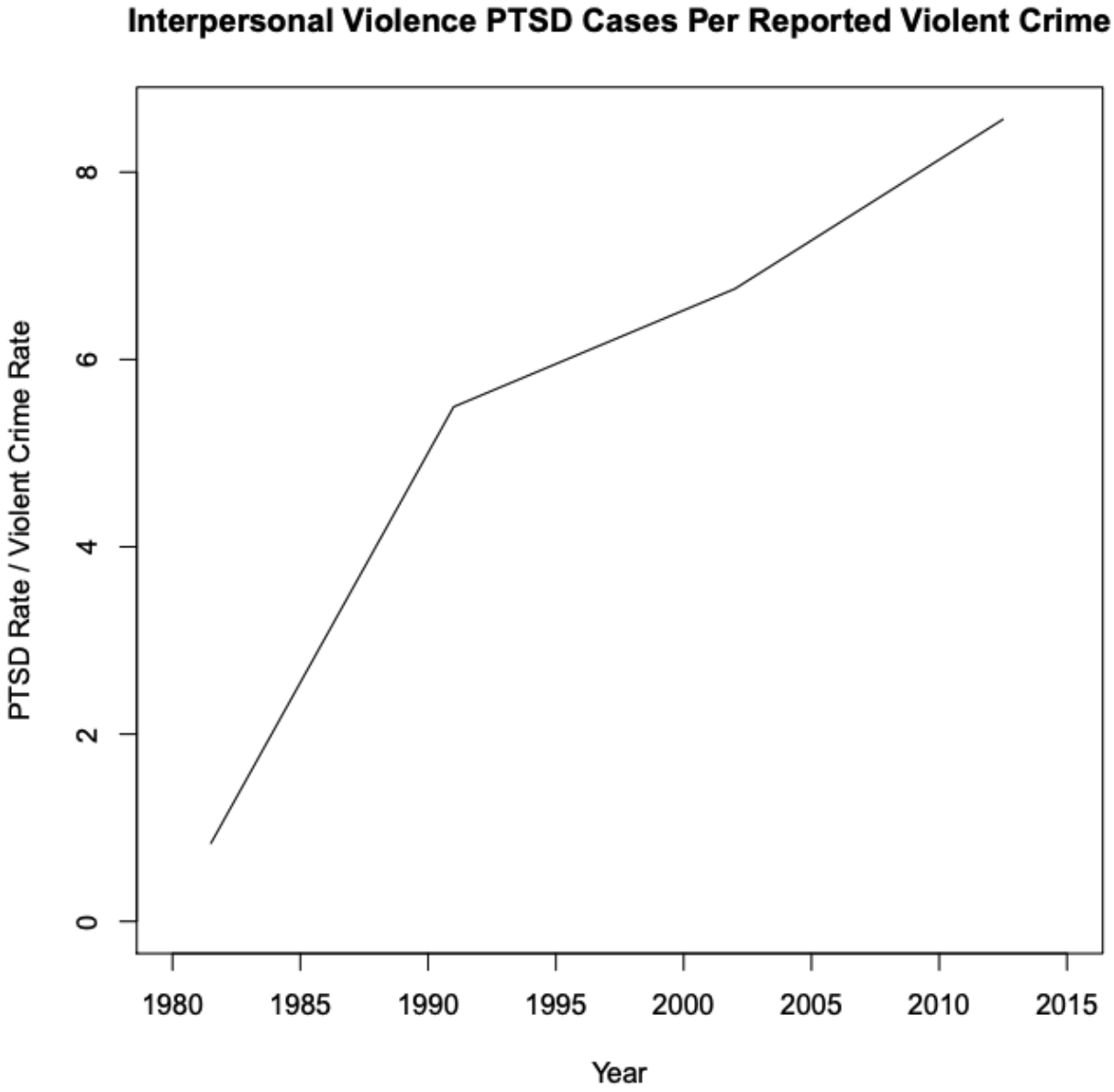


Note: The left three graphs in the top row exclusively include PTSD linked to experiencing the event oneself. Witnessing interpersonal violence is aggregated with witnessing or learning about all types of events (e.g., includes witnessing accidents).

Now that we have broken down the PTSD rate attributable to interpersonal violence, we can divide this rate by the number of violent crimes in the same year (PTSD ^ Direct Interpersonal Violence / Violent Crime; the top left graph of Figure 2.10 divided by the bottom left graph). This results in a proxy for vulnerability, as it is a ratio between the frequency of the outcome and the frequency of the index events. Although this method is imprecise and should be considered as a rough guess about vulnerability, it is perhaps the best we can do given the limitations of our data. This is shown in Figure 2.11.

In this section, I analyzed trends in interpersonal violence and PTSD within the United States over time. To summarize, interpersonal violence has largely declined during the relevant time frame. This trend is clearer and more pronounced in standardized third-party measurements of specific events (e.g., homicides via death certificates, police reports of assault), and is present but less clear in self-reports of exposure to violence from epidemiological studies. It is much harder to make any firm conclusion regarding PTSD rates within the United States over time. The epidemiological measurements of PTSD have many confounding problems, including shifting diagnostic criteria, methodological variations between studies conducted by separate organizations, retrospective reporting biases, and the lagged nature of lifetime prevalence. However, whereas there is ambiguity in whether PTSD rates have risen or merely remained stable, it would be difficult to argue that PTSD rates have substantially *declined*. If we can trust these data, we should be concerned why PTSD rates (and especially PTSD rates linked to interpersonal violence) are not falling in tandem with related index events. If we cannot trust these data, we should be even more concerned. If we cannot reliably measure PTSD, much of what we assume to know about the disorder may be affected by the same measurement confounds.

Figure 2.11. PTSD Cases (Attributable to Interpersonal Violence) per Violent Crime



Note: Violent crime data are from the FBI UCR report. The DSM-IV correction is used for the NESARC-III. NESARC-II PTSD rates attributable to interpersonal violence are unavailable and are therefore omitted.

An International Neurotic Treadmill: The Vulnerability Paradox and Beyond

Rather than comparing a single nation over time, an alternate approach is to compare many nations within the same time window using more consistent diagnostic criteria. Because nations vary in their levels of wealth, poverty, inequality, violence, technology, adoption of modern Western norms, and other variables, cross-sectional international data can add a second lens through which to examine PTSD rates. First I will examine the best data available, and then move to a consideration of the strong cultural confounds that influence this analysis.

Dückers and colleagues (2016) conducted exactly such an evaluation of cross-national PTSD rates for nationally representative epidemiological surveys conducted between 2001-2007. They selected epidemiological studies that used the Composite International Diagnostic Interview (CIDI; World Health Organization, 1994) and DSM-IV criteria. The CIDI was the primary PTSD measure in the World Mental Health surveys and is a structured diagnostic interview that has been cross-culturally validated. By examining only studies using the CIDI and DSM-IV criteria, they bypass concerns about shifting diagnostic standards present in the United States time series epidemiological data. Their reliance on carefully sampled epidemiological studies using symptom-by-symptom questionnaires also allows them to (partially) mitigate potential confounds such as mental health awareness or willingness to seek treatment.

Indeed, Dückers' analysis points to a surprising pattern that parallels the time-series data in the US. Countries with high vulnerability indices (e.g., high income inequality, political corruption), such as Mexico and South Africa, consistently have lower rates of PTSD following trauma compared to countries with lower vulnerability indices, such as Canada and the United States. In other words, factors of adversity at the level of the group seems to have an inverse

relationship with individual vulnerability following exposure to traumatic stressors. They dub this trend the *vulnerability paradox*.

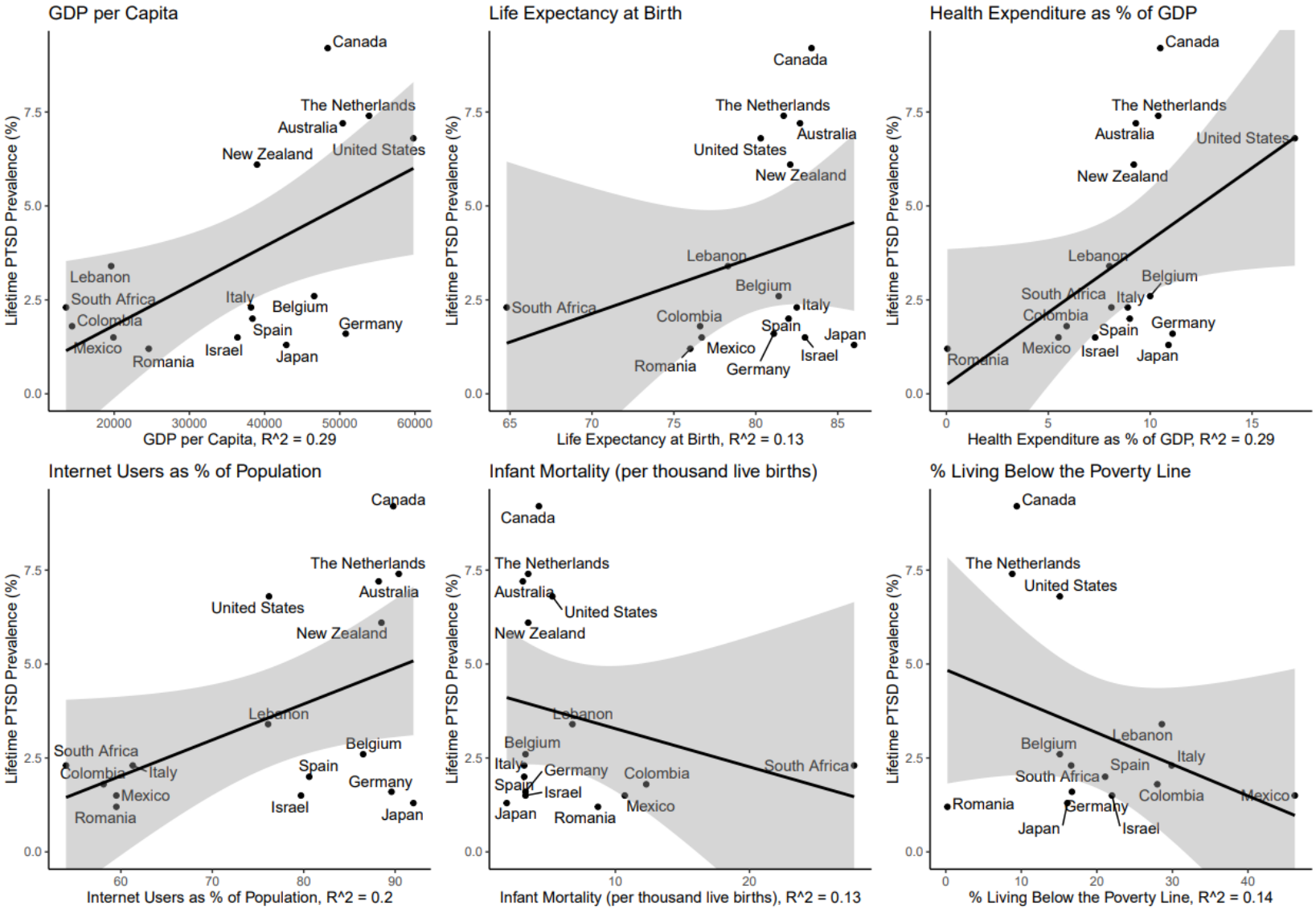
McNally (2018) points out that *paradox* is hardly an appropriate descriptor for the pattern: "vulnerability" at the ecological level of nations should not be conflated with individual vulnerability to PTSD. Nevertheless, the pattern is surprising. Why would less-developed nations, whose populations face a much larger degree of adversity, have *lower* rates of PTSD following trauma? What might explain the apparent group trends? One possibility is a neurotic treadmill effect; in the backdrop of a more advantaged society, the same stressors may be perceived as being more severe (McNally, 2018). As mentioned before, cultural differences in willingness to report specific symptoms are also an important confound.

Dückers used a unique 'national vulnerability index'. One possibility is that the paradox is specific to this metric and does not generalize to other measures. To robustly verify this apparent cross-national neurotic treadmill effect, I examined cross-national rates of PTSD (both overall rates and conditional rates) compared to various additional nation-level metrics.

Figure 2.12 displays relevant statistics drawn from the CIA World Factbook compared with overall epidemiological rates of PTSD (Central Intelligence Agency, 2020; Dückers et al., 2016). As shown in the top left, nations with a higher per capita gross domestic product are more likely to have higher overall rates of PTSD. Gross domestic product is not necessarily an appropriate proxy for individual wealth; it can also merely indicate the presence of a few exceptionally wealthy individuals. However, the pattern remains when examining the percentage of individuals living below the poverty line (bottom-right): the more individuals in poverty, the lower the PTSD rate. Higher life expectancy tracks with higher rates of PTSD (top-middle), as does higher percentage of GDP spent on healthcare (top-right), and lower rates of infant

mortality (bottom-middle). National PTSD rates also track with the percentage of the national population who use the internet, which may serve as a useful proxy for certain cultural variables (bottom-left).

Figure 2.12. Cross-National PTSD Rates and Statistics from the CIA World Factbook



The World Factbook statistics from Figure 2.12 are compared to the *overall* rate of PTSD in a given nation. This overall rate does not consider the fact that exposure to traumatic events differs across nations. Luckily, most of the epidemiological studies aggregated by Dückers and colleagues reported the CIDI assessment of lifetime trauma exposure assessed using a detailed list including combat or war experience, natural disaster, physical or sexual abuse, physical abuse as a child, motor vehicle accident, unexpected death or life-threatening illness of a loved one, or witnessing a potentially traumatic event (Dückers et al., 2016). What happens if we compare the same national statistics to the *conditional* rate of PTSD: that is, the rate of lifetime PTSD among those who have experienced a serious trauma? The results are shown in Figure 2.13. In short, the trends remain.

Indeed, the effect originally reported by Dückers and colleagues involved the conditional rate. This inspired at least one critique (Vermetten, Stein, & McFarlane, 2016). The critics point out that other causes may bias rates of self-reported trauma exposure, and that the rates of self-reported trauma exposure in developed nations may be implausibly high[9]. For instance, consider that the lifetime exposure rate to Criterion A trauma reported in South Africa was 73.8%, whereas the lifetime exposure rate in the Netherlands was 80.7%. This points to a strong possibility of cross-cultural confounds in reporting trauma exposure. The critics further suggest that "other sources may legitimately allow comparison of prevalence estimates: for example, the death rate from motor vehicle accidents in South Africa is 25.1 per 100,000 compared with 3.4 in The Netherlands, and there were 35.7 v. 8.9 murders per 100,000 in South Africa v. The Netherlands." The critics are likely correct that objective reports such as vehicle accident rates or

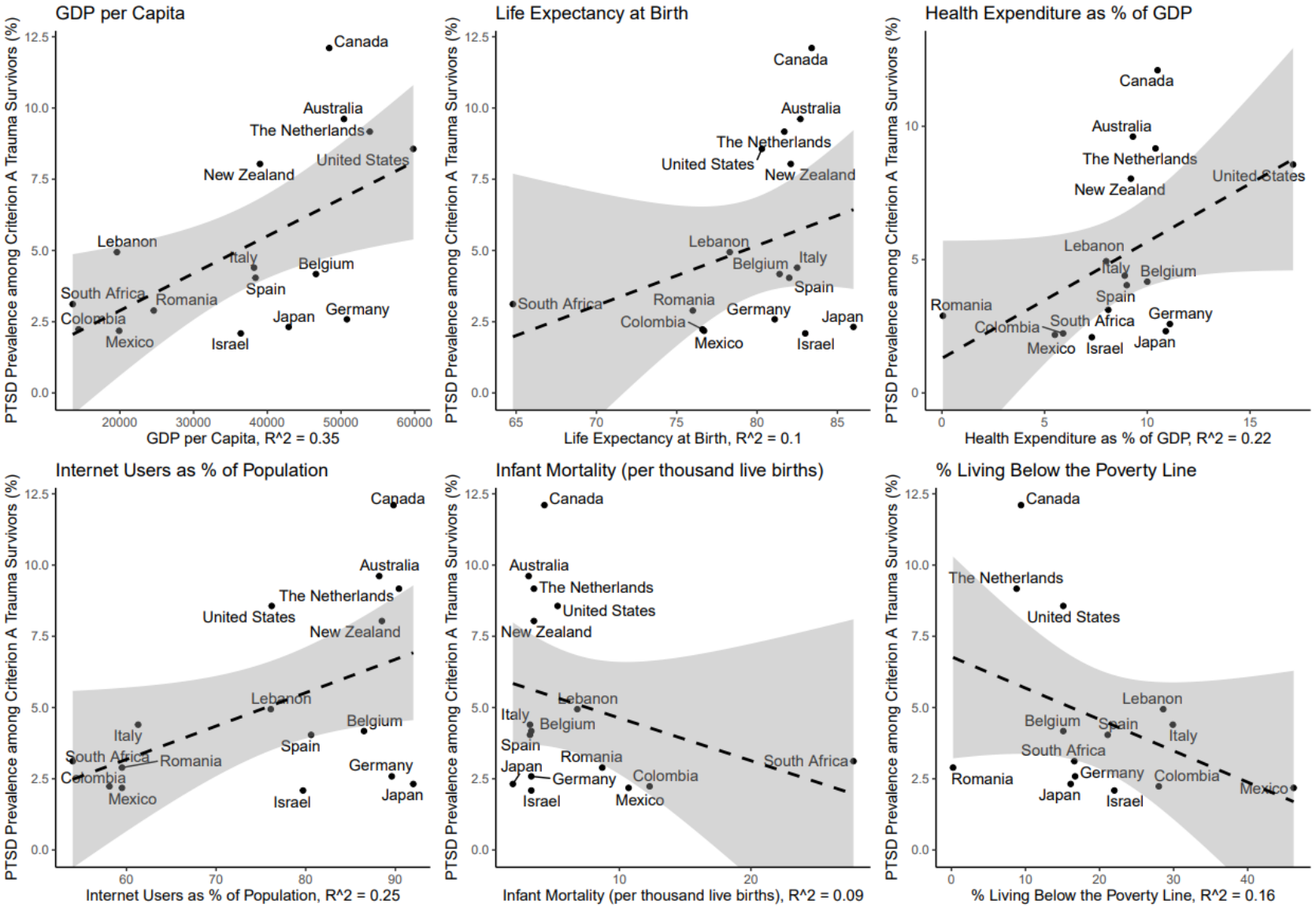
[9] As a side note, another reason this may occur is that the trauma exposure rate in the population only requires a single event per individual. Thus, multiple events in a single individual do not increase the rate. Thus, two countries might have equivalent trauma exposure rates even though one has a higher incidence of traumatic events overall.

homicide rates are a better indicator of total adverse events experienced by the population, particularly when strong confounds are suspected to affect self-reports.

But if such rates were used for correction, this would not cause Dückers' counterintuitive effect to disappear, but rather to *skyrocket*. The overall rate of PTSD (prior to any correction and unconditional upon exposure to trauma) is 3 times higher in the Netherlands compared to South Africa. If we use the murder rate statistics as a proxy for trauma exposure, the inferred conditional rate of PTSD would be *13 times higher in the Netherlands than in South Africa*; if we use motor vehicle accidents, *24 times higher*. Hence, corrections to issues with self-reporting of trauma are likely to strengthen Dückers' paradox, not weaken it.

Considering the likely cultural issues with the reporting of trauma, we should also suspect cultural issues with the reporting of the PTSD symptoms themselves. Although the epidemiological studies are careful to ask about specific experiences rather than endorsement of diagnoses or past treatment, individuals in some cultures may still be hesitant to endorse symptom patterns that they interpret as showing emotional weakness. With PTSD symptoms, there are no objective analogues to which we can resort. Most PTSD symptoms are inherently defined by the subjective experience of the sufferer (e.g., “intense or prolonged emotional distress at exposure to internal or external cues”, “feelings of detachment or estrangement from others”), and are therefore always measured in a subjective manner.

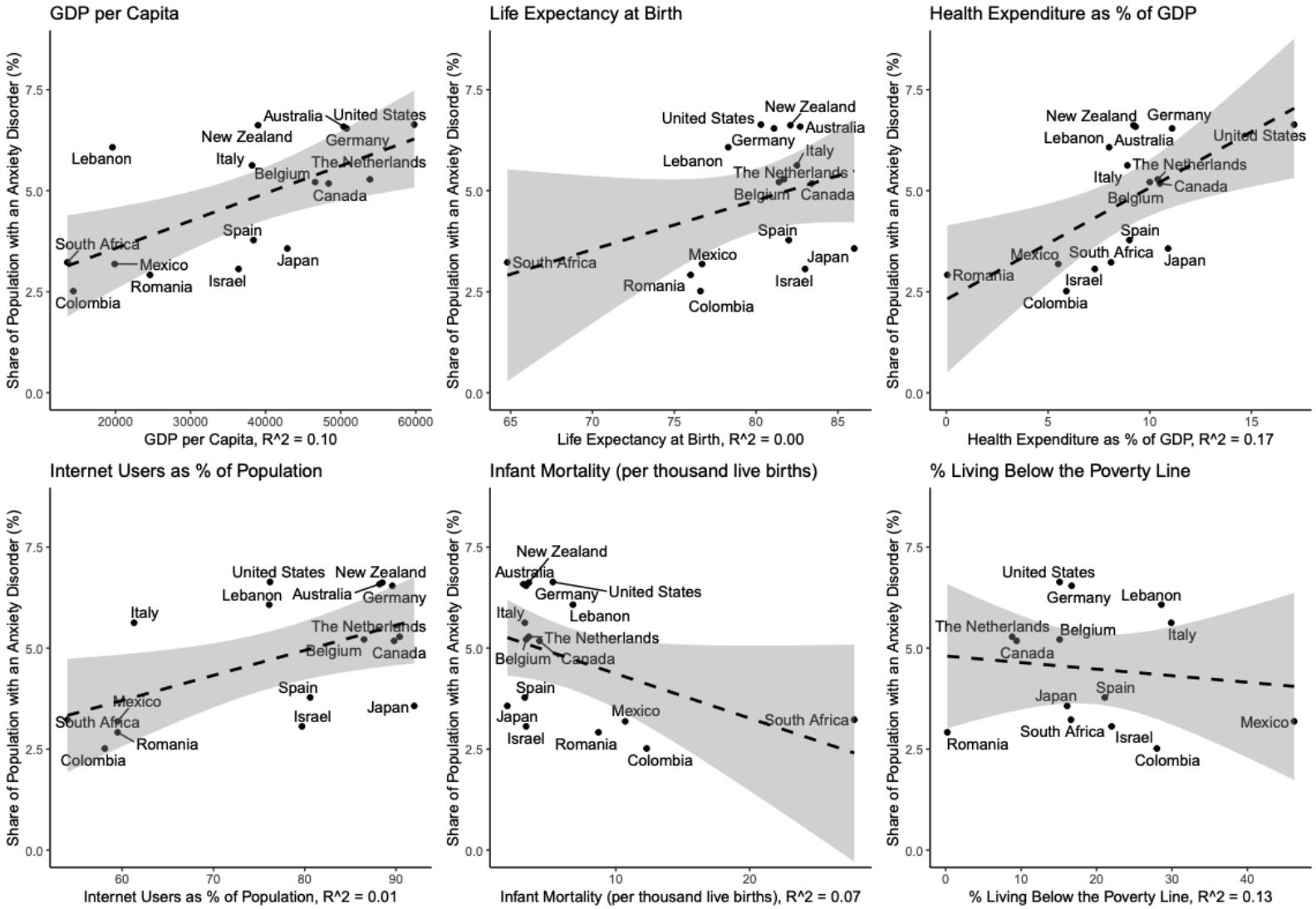
Figure 2.13. Cross-National Rates of PTSD among Trauma Survivors and Statistics from the CIA World Factbook



Cross-National and Longitudinal Rates of Anxiety Disorders: A Sanity Check

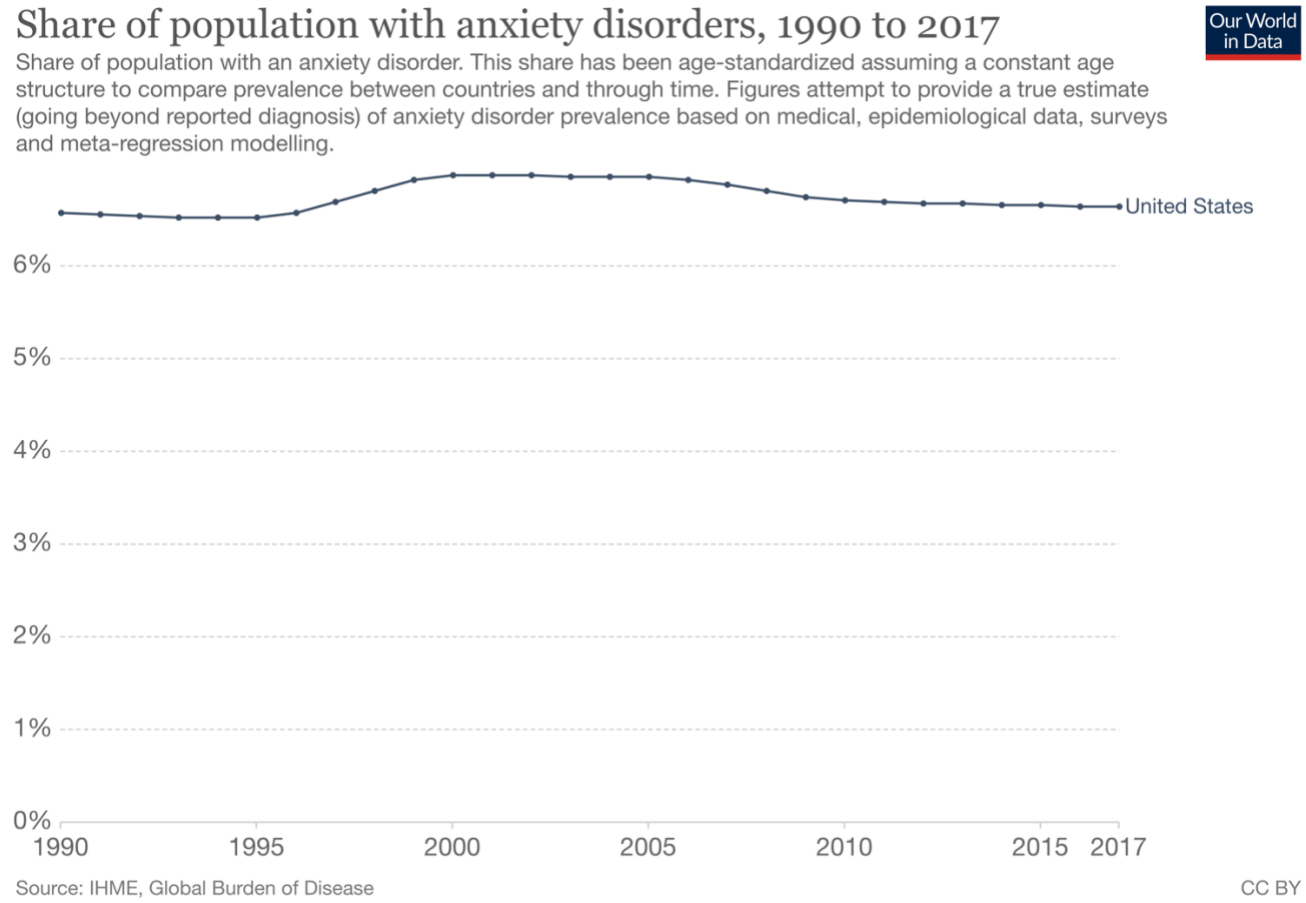
Although we have exhausted the availability of epidemiological studies on PTSD specifically, we can do a sanity check by comparing to global rates of anxiety disorders defined more broadly. In this case, we have the advantage of the Global Burden of Disease Studies, which provides a more constant yardstick over time and has the advantage of being standardized by a central organizational structure. The Global Burden of Disease metrics on anxiety disorders are juxtaposed with the CIA's metrics in Figure 2.14. The Global Burden of Disease metrics are also continuously tracked over time. Using these metrics, we can confirm in Figure 2.15 that anxiety disorders have not markedly declined within the United States, or indeed within any of our selected countries, since 1990, visible in Figure 2.16.

Figure 2.14. Global Burden of Disease Anxiety Disorders and CIA Metrics



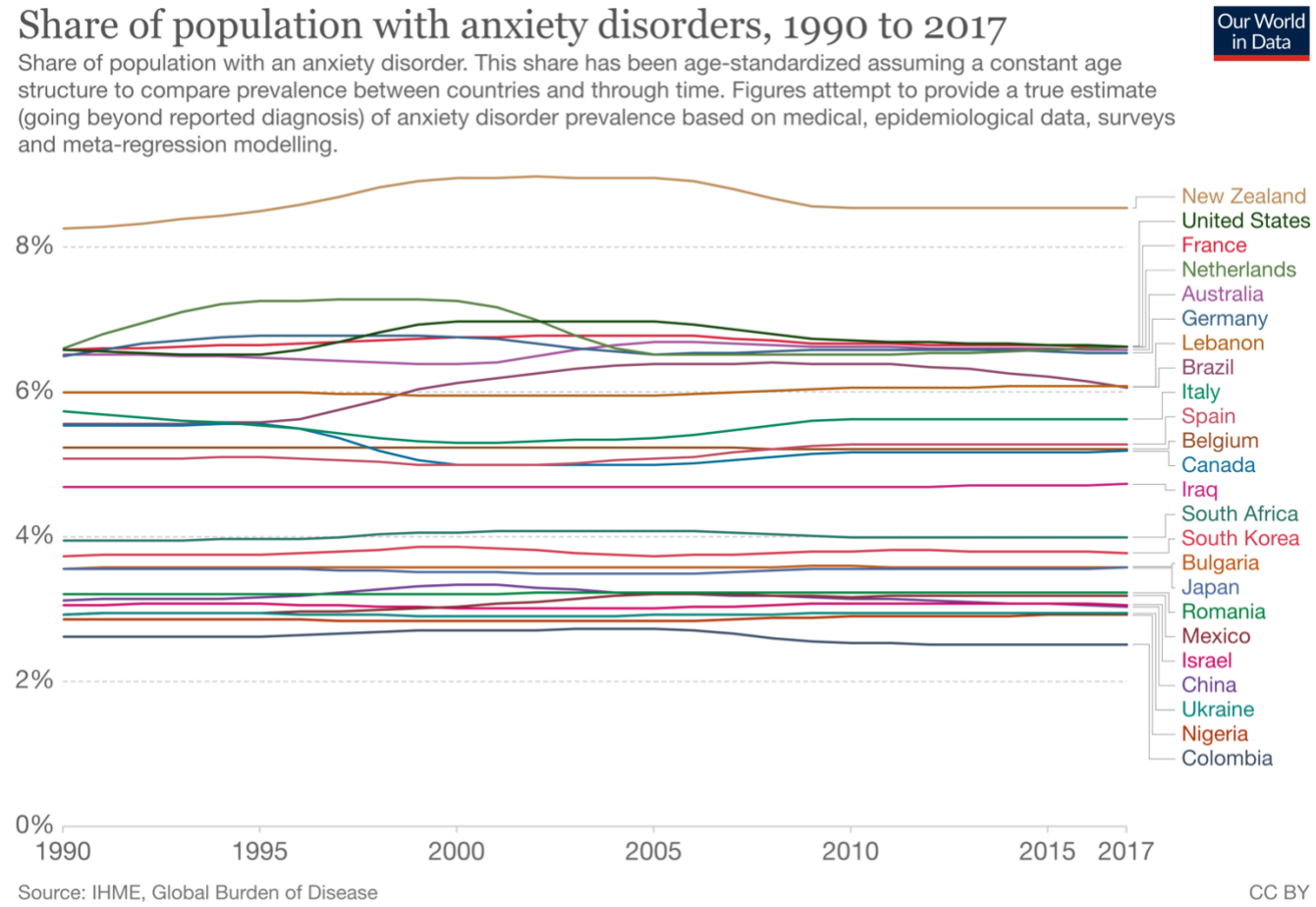
Note. Uses Global Burden of Disease 2017 data.

Figure 2.15. Global Burden of Disease Anxiety Disorders Over Time – United States.



Note. To view these data interactively, use https://ourworldindata.org/grapher/share-with-anxiety-disorders

Figure 2.16. Global Burden of Disease Anxiety Disorders Over Time – International.



Note. To view these data interactively, use https://ourworldindata.org/grapher/share-with-anxiety-disorders

In summary, the international trends in PTSD and anxiety suggest a counterintuitive pattern. Vulnerability to PTSD (and anxiety) is lowest in areas where adversity is relatively intense, and highest in areas where adversity is relatively mild. There is a strong possibility that cultural confounds affect these estimates, such as cross-national differences in willingness to anonymously report specific symptoms of distress. Unfortunately, if cultural confounds are solely responsible for such large effect sizes (e.g., five times greater PTSD rate in Australia than Israel, six times greater in Canada than Mexico), then there is likely little we can learn from the epidemiology of PTSD. That is, if we accept that confounds in PTSD symptom reporting are powerful and pervasive even when measured using structured interviews, this is a major problem not only for the current analyses, but for most studies on PTSD.

Aims

Human well-being exhibits a hedonic treadmill effect. It's possible that a parallel neurotic treadmill effect also exists. For PTSD, a form of ill-being tied to specific types of adversity, a neurotic treadmill seems plausible from a theoretical perspective. A decrease in serious adversity could lead to an expansion in personal concepts of trauma and PTSD, which could in turn lead to increases in personal vulnerability. It also seems plausible from an epidemiological perspective, given that rates of PTSD do not apparently decline in tandem with violence.

The following three chapters aim to test key aspects of this theory. Paper 1 builds upon previous work in signal detection theory and prevalence-induced concept change to examine the conditions in which individuals expand or contract their personal definitions of trauma. Participants sequentially classified descriptions of events (e.g., "broke a leg in a bicycle accident") as either 'trauma' or 'not trauma'. In the first experiment of Paper 1, I gradually decreased the prevalence of serious events to examine whether participants would

correspondingly expand their definitions of trauma. In the second experiment of Paper 1, participants viewed a restricted range of events (either only nonserious or only serious events) to test whether the implicit range of events affects the trauma concept.

In Paper 2, I examine whether personal concepts of trauma truly matter when it comes to experiencing a stressful event. Individuals were randomly assigned to either a 'broad concept' or 'narrow concept' condition that aimed to alter their concept of trauma. Participants subsequently reported their own genuine view of trauma. They were then subjected to a stressful event (watching a disturbing film clip). This experiment aimed to test whether broader trauma concepts would result in increased distress in response to a stressful event.

In Paper 3, I turn to a more applied setting relevant to cultural norms surrounding harm. I tested whether providing trigger warnings to previously traumatized individuals is iatrogenic. Trigger warnings alert trauma survivors to potential harms; the intention is to allow trauma survivors to appropriately brace themselves to face distressing content. However, it is possible that trigger warnings backfire by implicitly signaling that trauma cues are dangerous and typically result in considerable anxiety, altering personal concepts about harm. I test whether trigger warnings affect anxiety when reading literature passages and whether they impact how individuals view their own trauma.

Chapter 3 —

Paper 1: Exposure to Descriptions of Traumatic Events

Narrows One's Concept of Trauma

Background

Trauma centers treat injuries resulting from sudden physical insults to the body. Psychological trauma is a metaphorical extension of the medical term applied to emotional harm, formalized in the appearance of posttraumatic stress disorder (PTSD) in the third edition of the Diagnostic and Statistical Manual of Mental Disorders (DSM-III; American Psychiatric Association [APA], 1980). The diagnosis emerged in part due to discussions surrounding "post-Vietnam war syndrome" (Shatan, 1972). The psychological complications of war veterans were grouped with those of individuals who were traumatized by rape, natural disasters, or events such as the Holocaust to form the category of PTSD (McNally, 2003a). The original formulation presumed that PTSD could only arise following exposure to terrifying, presumably rare events falling outside the boundary of ordinary experience. Yet the concept of trauma has increasingly expanded to embrace a wider range of stressful events (McNally, 2016). For example, individuals have reported PTSD symptoms resulting from being bullied at work (Matthiesen & Einarsen, 2004) or giving birth to a healthy baby after an uncomplicated delivery (Olde, van der Hart, Kleber, & Van Son, 2006).

This raises an important question for diagnosis, treatment, and related policies: what *is* trauma? From a diagnostic standpoint, the current edition of the DSM (DSM-5) maintains the relatively strict definition of "exposure to actual or threatened death, serious injury, or sexual violence" (APA, 2013, p. 271). Yet among laypersons, 'trauma' often has a much broader meaning. Media outlets have applied it to include experiencing microaggressions (Williams, 2015), reading the news (Jacobs, 2018), or learning secondhand about "difficult or disturbing stories" (Lees, 2018, para. 3).

Expansions in the usage of the term 'trauma' over time may indicate "conceptual bracket creep in the definition of trauma" (McNally, 2003b, p. 231). The term 'creep' is here used to denote the expansion of a semantic boundary over time. Haslam (2016a) suggests that a wide variety of harm-related concepts (such as bullying, abuse, and prejudice) have similarly creeped. Psychologists vary in their views on the benefits and costs of expanding the definition of trauma and other harm-related concepts. On one hand, such expansions may reflect a development in deeper empathy towards individuals who suffer from various types of negative events (Haslam, 2016a). In addition, expansions in the concept of trauma may reduce uncertainty about the (un)acceptability of certain behaviors, empowering victims and third-party allies to take more decisive action (Cikara, 2016).

On the other hand, some scholars worry that expansions in the concept of trauma dilute the meaning of the term (Haslam, 2016a; McNally, 2016). In the words of the historian of military psychiatry, Ben Shephard, "Any unit of classification that simultaneously encompasses the experience of surviving Auschwitz and that of being told rude jokes at work must, by any reasonable lay standard, be a nonsense, a patent absurdity" (Shephard, 2004, p. 57).

From a sociocultural standpoint, Haslam (2016a) worried that diluting the public's perception of the trauma concept might "[risk] reducing the range of people who see themselves as capable of moral agency," therefore increasing "a tendency for more and more people to see themselves as victims who are defined by their suffering, vulnerability, and innocence, and who have diminished agency to overcome their plight." From a clinical standpoint, McNally (2009) wondered whether it might alter the etiological understanding of PTSD, "[undermining] the very rationale for having a diagnosis of PTSD in the first place." Confusions about the PTSD concept

could potentially result in the misapplication of trauma treatments and the overmedicalization of normal stress, undercutting natural resilience.

Regardless of whether harm-related concept creep is helpful or unhelpful, little is understood about how and why it occurs. One possibility is that concept expansions occur as a result of altered frequencies of exposure. For example, if overt violence occurs at a low frequency, individuals may shift their concept boundaries to encompass additional examples (e.g., classifying hateful speech as violence). Indeed, in a series of experiments, Levari and his colleagues (2018) found that shifting the prevalence of displayed categories reliably alters conceptual boundaries across several sets of stimuli and conditions. For example, when participants were shown decreasing amounts of threatening faces (relative to non-threatening faces), they expanded the range of faces they classified as threatening. The researchers dub this effect "prevalence-induced concept change," referring to the semantic shift that occurs due to changes in the relative prevalence of instances of certain categories. Prevalence-induced concept change is proposed as a broad within-person mechanism that could apply to the expansion or contraction of any kind of category (not just harm-related concepts). Harm-related concepts specifically (violence, trauma, abuse) are of special interest because interpersonal violence and related forms of harm have decreased over time, in some cases quite drastically (Pinker, 2011).

Another possibility is that harm-related concept expansions may occur as a result of an altered range of reference. Parducci (1965) argued that when making decisions, humans are sensitive to both the relative *frequency* of different types of stimuli, as well as the absolute *range* of stimuli. For example, when deciding how serious of an injury a sprained ankle is, a person's judgment may be influenced by their perception of how common it is relative to other injuries like broken bones and lacerations (the *frequency*), but also by the most and least severe injuries

that come to mind (the *range*). Recent research has suggested that this is a particularly computationally efficient way for the brain to make subjective evaluations (Bhui & Gershman, 2018), which may explain why it has been documented in domains as diverse as judgments of loudness (Jesteadt, Luce, & Green, 1977) and product prices (Niedrich et al., 2009).

It's important to distinguish between range and frequency as pieces of a cognitive puzzle existing in the minds of individuals (as mechanisms), and the ranges and frequencies of different stimuli that individuals might encounter in real life or in an experiment (as environments). In an experimental setting, fully disentangling the cognitive mechanisms of range and frequency is difficult, but is not our aim. Instead, we aim to determine which types of environmental manipulations (if any) result in the expansion of the trauma concept. In the case of potentially traumatic events, two distinct types of experience might be influential: (1) experiencing events directly oneself, or (2) hearing about, witnessing, or otherwise learning about events. In the lab, of course, we must rely exclusively on the latter to manipulate perceived frequencies and implicit frames of reference. This provides an imperfect but meaningful analogue to real-world declines in violence.

We attempt to test the influence of displayed stimuli (descriptions of events) as they relate to binary judgments of events as either 'trauma' or 'not trauma.' In the first study, we conducted a preregistered extension of Levari and colleagues' (2018) experiments. Specifically, we asked participants to classify events as either trauma or not trauma while manipulating the relative frequency of serious and nonserious events. In the second study, we repeated a similar experiment, but manipulated the total range of event seriousness while maintaining the relative frequencies as a constant.

Experiment 1

Method

Stimuli generation & norming. We generated 600 descriptions of events covering the entire spectrum from "not at all traumatic" to "extremely traumatic." The descriptions ranged in length from 2-16 words and varied widely in their thematic and emotional content (e.g., "walked up a flight of stairs," "killed a child pedestrian while driving").

To obtain initial objective ratings of the stimuli, we conducted a pretest. We randomly divided the stimuli into six equal sets of 110 items. In each set, 98 items were unique to the set, whereas the other 12 items appeared in all sets, serving as a consistency check. We presented each set of descriptions in random order to participants recruited from Amazon Mechanical Turk (MTurk) (n_{total} = 250, $n_{set} \approx 42$). Participants were asked to rate each description on a 7-point Likert scale from "Not at all traumatic" to "Extremely traumatic." Interrater reliability on the consistent set of 12 items was good when assessed across each of 250 participants as separate judges (ICC_1 = 0.70; Shrout & Fleiss, 1979), and excellent when considering the average value across each of the six sets (ICC_1 = 0.99). Inclusion and exclusion criteria for pilot raters were the same as for the main experiments, as detailed below. Demographic information of raters is included in the supplemental materials (osf.io/3e2us). Pilot raters were ineligible to participate in the experiments.

Procedure. We preregistered Experiment 1 on the Open Science Framework (osf.io/tw92r). Participants were recruited from MTurk. They were allowed to participate in the study if they were adult United States residents and had an MTurk approval rate of 95% or greater. Participants first completed a CAPTCHA and US residency screener (e.g., "What emergency number is most common in the United States?") and were immediately excluded if

they failed either task. As preregistered, we recruited participants until a total of 300 had completed the study (which required passing this initial screener).

Participants were first given basic instructions regarding the survey[10]. They were then shown each event in sequence in a standardized window and were instructed to press one of two keys to indicate either "trauma" or "not trauma" for each description. They viewed each item for a minimum of 1.5 seconds before clicking and were instructed to take a break every 30 items (< 5 seconds). Each participant viewed a total of 300 items. Attention checks were interspersed throughout this task (i.e., "please press the p key on your keyboard"). After rating all items for their condition, participants completed a demographics and psychiatric history questionnaire and a human participant verifier (writing three sentences about the past weekend). They were then shown a debriefing form explaining the purpose of the experiment. As preregistered, participants were excluded from the analysis if they incorrectly answered attention checks or failed the human participant verifier during the experiment. A total of 24 participants were excluded, leaving a total of 276 participants.

Using the rating data from the pretest, we sorted items into categories depending on their mean rating on the 7-point Likert scale in the pretest: nonserious events (mean = 1-2), ambiguous events (mean = 3-4), serious events (mean = 5-7). We intentionally selected items that had acceptably low standard deviations in the pretest ($sd < 1.6$) to avoid selecting items that were inconsistently interpreted by different participants. We then selected 342 descriptions of events that corresponded to nonserious events (142 descriptions, e.g., "walked up a flight of stairs"), ambiguous events (100 descriptions, e.g., "broke an ankle while running") or serious events (100

[10] "Some events that happen in life may be considered to be trauma. In this survey, you will read a series of [X] descriptions of events that may occur in a person's life. For each description, you will be asked to decide whether the event itself is "trauma" or "not trauma". There are no right or wrong answers."

descriptions, e.g., "killed a child pedestrian while driving") to be used in the main study. The exact number of items in each category was determined by a calculation that ensured we would have sufficient unique stimuli in each category given our experimental design.

We will refer to the probability that participants were shown an item from the "serious" category in each block as the signal prevalence. For participants in the control ("stable") condition (n = 138), participants were shown serious events with a signal prevalence of 33.3% throughout the experiment. For participants in the experimental ("decreasing") condition (n = 138), we modified the signal prevalence over time. The signal prevalence was set at 33.3% for the first 100 trials, 25% for the next 50 trials, 16.6% for the next 50 trials, 8.3% for the next 50 trials, and 4.12% for the last 50 trials[11]. The decrease in the probability of serious events was balanced by an increase in probability of nonserious events, whereas the frequency of ambiguous events remained constant.

Analysis. To analyze the data, we used the lme4 package (R Core Team, 2019; Bates, Machler, Bolker, & Walker, 2015). We fit a binomial generalized linear mixed effects model to the data, estimated via bound optimization by quadratic approximation (BOBYQA; Powell, 2009). In each experiment, the dependent variable was the participants' identification of a stimulus as "trauma" or "not trauma." Random effects were added for the (a) intercepts for participants and (b) slopes for trial number if they improved model fit as determined by a χ^2 test.

Based on the results of Levari et al (2018), we predicted that individuals in the stable condition would remain consistent in their ratings over time, whereas individuals in the decreasing condition would become more lenient in their concept of trauma (i.e., have a higher likelihood of rating nonserious or ambiguous events as trauma in later trials). The predictor

[11] Due to an error in the item selection algorithm, the last 25 trials showed items at an incorrect signal prevalence. These final 25 trials were therefore removed in all reported analyses for Experiment 1.

variables in our binomial generalized linear mixed effects model were experimental condition, pilot ratings of descriptions (i.e., "objective seriousness" of each description), trial number, and their interactions. We expected a significant three-way interaction of the predictor variables, indicating that individuals in the decreasing condition would classify nonserious or ambiguous items as "trauma" at a higher likelihood compared to individuals in the stable condition, but only at later trials.

Results

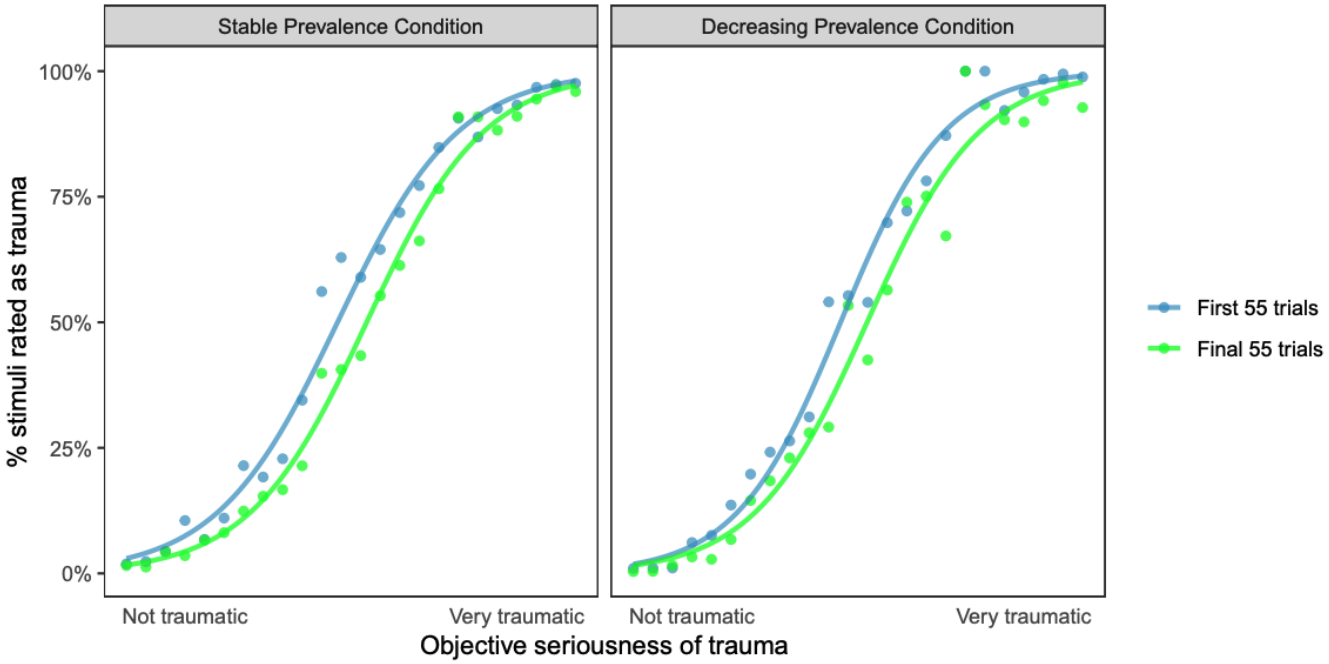
In terms of demographic characteristics, our participants were predominantly male (56%), Caucasian/White (81%), non-Hispanic (96%), not religious (56%), trauma-naïve (72%), with no history of diagnosed mental illness (85%). A table displaying full demographic information is provided in the supplemental materials.

A model including both random effects for the intercepts of participants and slopes for trial number showed superior fit compared to models excluding either of these effects, as determined by a χ^2 test ($p < 0.001$, $N = 276$). Our prediction of a three-way interaction between condition, objective seriousness of each item, and trial number was not supported ($\beta = -0.21$, $z = -0.35$, $p = 0.73$, *model dispersion* = 0.63). Instead, participants became stricter in their threshold of assigning the descriptor of "trauma" over time across *both* conditions. This effect is visualized in Figure 3.1. Over increasing trial numbers, the threshold shifted to the right (increasingly strict) in both conditions. Because the three-way interaction was not supported, we subsequently tested for two-way interactions, as preregistered. In the model without the three-way interaction, there was no two-way interaction between trial number and condition ($\beta = 0.10$, $z = 0.53$, $p = 0.59$), indicating that any shifting of the thresholds across trial number did not differ by condition. We found significant two-way interactions between trial number and objective seriousness, and

between condition and objective seriousness, neither relevant to our hypotheses (β = 1.20, -1.69, z = 3.67, -8.97, ps < 0.001). The first interaction indicated that across conditions, the slope was slightly steeper at earlier trial numbers. The second interaction indicated that across time points, the slope of the stable condition was less steep than the slope in the decreasing condition. This interaction was especially unexpected because the experimental manipulation was applied gradually, so any effect involving the conditions would presumably interact with trial number (i.e., we expect differences in later trial numbers when the conditions are distinct, but not in earlier ones when the conditions are identical). Because these two-way effects are unexpected and inexplicable, we are hesitant to provide a more substantive interpretation. To ensure that this two-way interaction model was appropriate, we tested for differences in fit between it and the three-way interaction model. There was no significant difference in fit between this model and the model including the three-way interaction (p = 0.73).

In an exploratory model including only main effects, we indeed observed a main effect of trial number (β = -0.81, z = 8.78, p < 0.001), which corresponds to the shift towards the right depicted in the graph. As noted earlier, the effect of trial number was not moderated by condition when two-way interactions were included. We were surprised to see movement of participants' rating threshold within the stable condition, as this effect was absent in all seven of Levari et al.'s (2018) experiments. Tables with complete details on each model are available in the supplemental materials (osf.io/3e2us). Overall, these results did not provide support for prevalence-induced concept change in the rating of descriptions of traumatic events. Instead, they seemed to indicate the presence of another, unexpected effect of narrowing across both conditions.

Figure 3.1. No Effect of Prevalence, but an Effect of Trial Number on Trauma Ratings



Each dot represents an event description, with lines drawn to indicate the threshold at which participants on average made the binary split between "not trauma" or "trauma" based on objective seriousness. Participants in both conditions rated descriptions more strictly in later trials compared to earlier ones.

Discussion

In Experiment 1, we found an unexpected effect. Regardless of the prevalence of serious traumas changed over time, participants became increasingly strict in what they classified as a trauma. After rating the first several items, participants became stricter, assigning the label of "trauma" only to relatively severe events. This effect appeared strongest for the first few trials, with a gradual but smaller shift continuing in later trials (see supplemental figure S1).

What might explain this effect? One possibility concerns the initial *range* in participants' working concepts of trauma, what Parducci called an "implicit frame of reference." Some of our more serious trauma items included "was tortured as a prisoner of war" and "was raped by a close friend." When our sample of relatively young American participants initially considered the term trauma, such events may not readily be brought to mind. In other words, it is possible that participants saw descriptions in the experiment that were more severe than they expected to see. We hypothesize that their initial implicit range encompassed low-to-moderate seriousness; as the experiment progressed, their implicit range expanded to encompass the full range of low-to-high seriousness. Ironically, this hypothesis relies on the idea that the participants experienced concept creep prior to entering the study – that is, they started with a relatively broad definition of trauma (then adjusted that definition when reminded of more serious events).

We therefore tested whether the narrowing effect we observed in Study 1 was attributable to participants' frame of reference. We thus devised a new experiment with experimental conditions that directly addressed the range of events shown to participants.

Experiment 2

Methods

Our participant recruitment and data collection methods were the same as in Experiment 1, but we modified the conditions to test our hypothesis about the range of events. After exclusion, 267 participants remained (33 participants were excluded). In the first condition (hereafter "nonserious range" condition; n = 135), participants were shown only items decisively judged to be nontraumatic in the stimuli norming pretest (range of mean: 1-3) [12]. To illustrate, the least serious event in the nonserious range was "walked up a flight of stairs" and the most serious was "was not hired after a job interview." In the second condition (hereafter "serious range" condition; n = 132), participants were shown only items decisively judged to be in the range of serious traumatic events (range of mean: 5-7). The least serious event in the serious range was "received chemotherapy" and the most serious was "was raped by a family member." Participants viewed 90 items for which the relative frequencies within each condition remained constant throughout (no frequency manipulation). If our hypothesis concerning the effect of the range of events was correct, we expected to see significantly stricter ratings in the high-range condition compared to the low-range condition.

To analyze the data, we used a binomial generalized linear mixed effects model. Our dependent variable was participants' binary rating of each stimulus as "trauma" or "not trauma." The predictor variables were experimental condition, pilot ratings of descriptions (i.e., "objective seriousness" of each description), and trial number. In this case, we did not model a three-way interaction, as we did not expect the objective seriousness of trauma to interact with the trial

[12] To provide at least some diversity in event seriousness and mitigate demand characteristics, we slightly expanded the range in the nonserious category (defined as 1-2 in Experiment 1). For reference, the events “was sick with the common cold” and “overslept and arrived late to work” were tied for the most serious events in this category in Experiment 1.

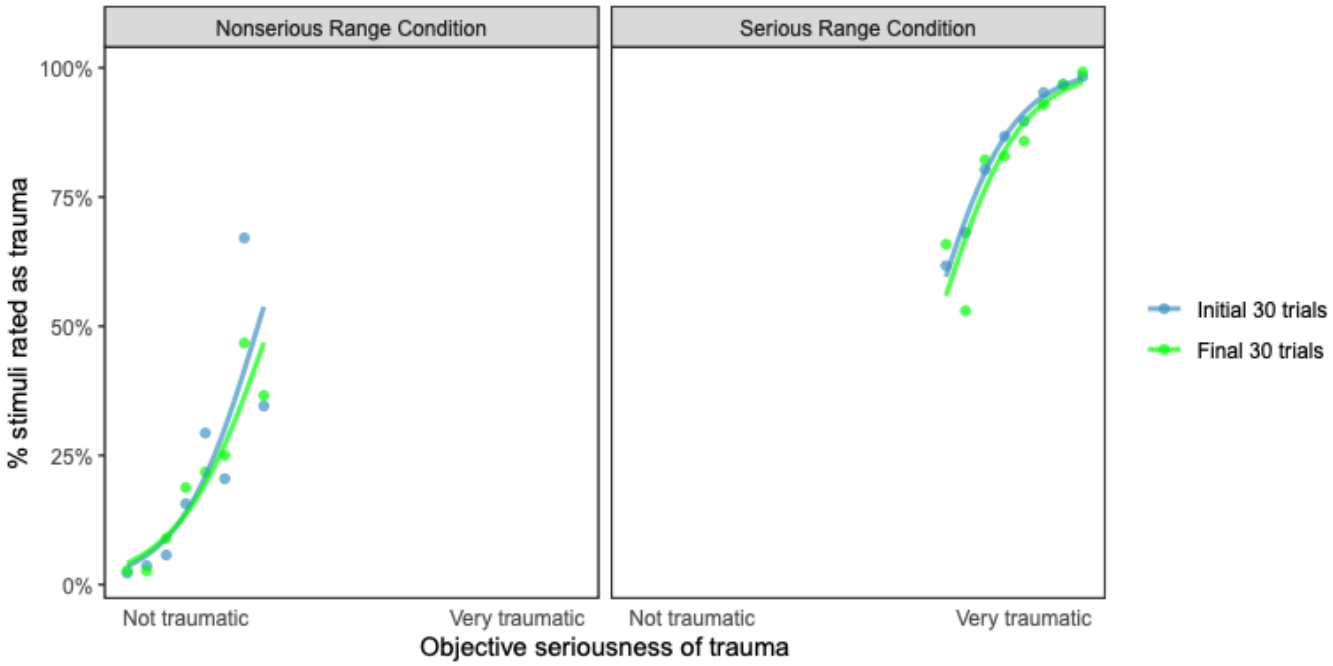
number and the condition. Instead, we hypothesized a significant two-way interaction of the condition and trial number, indicating that individuals in the serious range condition would classify items as "trauma" at a lower likelihood compared to individuals in the low-range condition, but primarily at later trials. Our model included the main effects and the hypothesized interaction term.

Results

Participants in Experiment 2 were predominantly male (58%), Caucasian/White (75%), non-Hispanic (90%), not religious (58%), trauma-naïve (64%), with no history of diagnosed mental illness (82%). A table displaying complete demographic information is provided in the supplemental materials.

Our prediction of a significant two-way interaction of the variables was not supported (β = 0.22, z = 0.84, p = 0.40, N = 267, *model dispersion* = 0.68). Instead, our prediction that individuals in the high-range condition would classify items as "trauma" at a lower likelihood was supported, but this effect was not moderated by trial number (i.e., the effect manifested almost immediately). This is reflected by the main effect of condition on participant ratings, which remained significant regardless of whether the nonsignificant interaction term was in the model (β = 5.58. 5.66, z = 17.79, 19.243, ps < 0.001; see Figure 3.2).

Figure 3.2. Range Restrictions in Ratings of Trauma

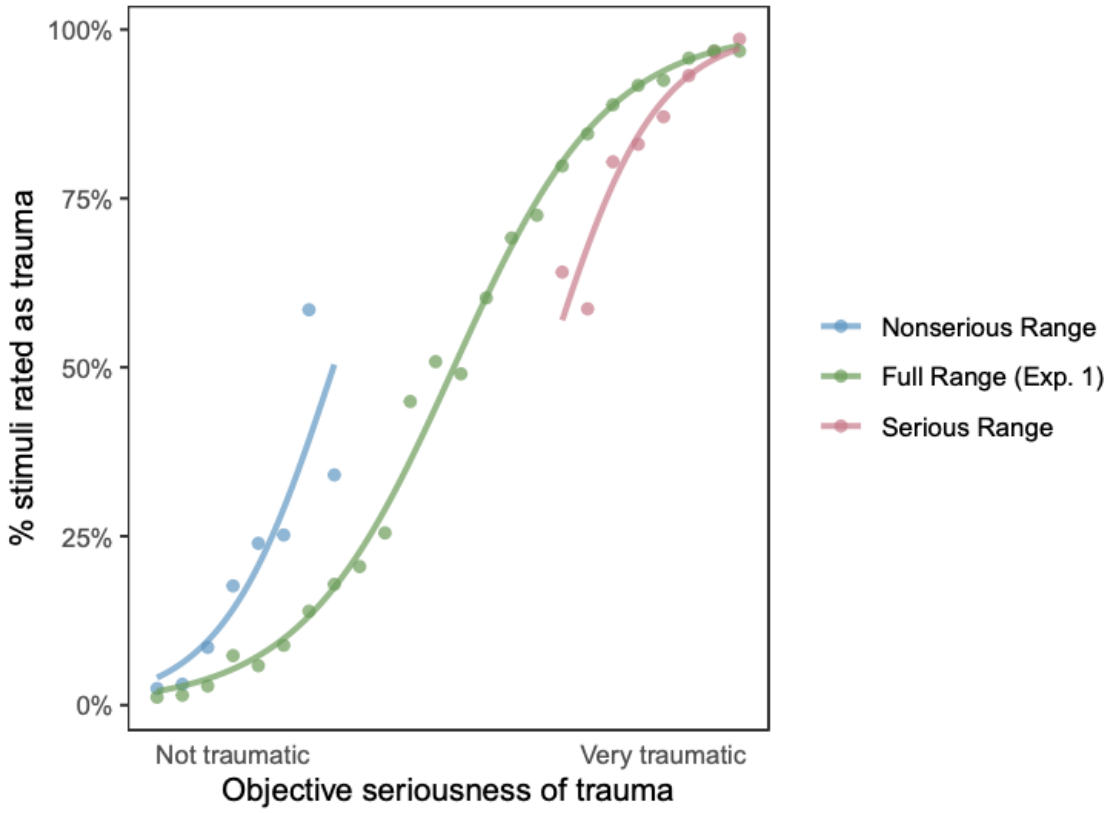


Each dot represents an event description, with lines drawn to indicate the threshold at which participants on average made the binary split between "not trauma" or "trauma" based on objective seriousness. Controlling for the objective seriousness of the descriptions, participants who saw only serious events were much stricter than expected compared to participants who saw only nonserious events.

To interpret the results, it is useful to consider them in relation to the Experiment 1 data. In the stable condition of Experiment 1, participants were shown events from the full range in random order. Because there was no manipulation of frequency, this condition is conceptually identical to the two Experiment 2 conditions. We can therefore use these data to visualize a hypothetical “full range” condition. Keep in mind that this visualization is intended for explanatory purposes, not for inference.

If the range hypothesis were correct, we would expect participants in the "nonserious range" condition to be the most lenient, followed by participants in the proxy "full range" condition, with individuals in the "serious range" condition behaving the most strictly. Indeed, this pattern applies, clearly visible in Figure 3.3. Individuals in the nonserious range condition have the most leftward curve, indicating that even mild items were frequently classified as traumatic. In contrast, individuals in the serious range condition had the curve farthest to the right, indicating that even serious descriptions were often classified as nontraumatic. The proxy full range condition falls between these two. Recall that when separated by time, the early trials of the full range condition produced a curve farther to the left, whereas the later trials produced a curve farther to the right.

Figure 3.3. The Influence of Range on Trauma Ratings



Each dot represents an event description, with lines drawn to indicate the threshold at which participants on average made the binary split between "not trauma" or "trauma" based on objective seriousness. Participants had the strictest threshold (e.g., only very serious events are "trauma") when they saw only serious events, and the most lenient threshold (e.g., even some minor events are "trauma") when they saw only nonserious events.

General Discussion

Concepts can expand over time to include events hitherto deemed to fall outside their original boundaries. In our first experiment, we sought to determine whether the concept of trauma would expand as the most serious instances of trauma became increasingly rare. We did not find evidence of prevalence-induced concept change for trauma. Instead, we found an unexpected effect whereby individuals became stricter in their trauma ratings over time, regardless of altered frequencies.

Perhaps people underweight the role of frequency when evaluating potentially dangerous events. For example, many people dread shark attacks despite their rarity (likely an adaptive trait). Thus, exposure to serious events may affect trauma concepts even when exceedingly rare. We hypothesized that exposure to our most extreme examples broadened participants' implicit range of events.

We tested this range hypothesis in our second experiment. Indeed, we found that altering the range of events shown to participants influenced their ratings of trauma. Participants who saw only nonserious events were lenient in classifying events as trauma compared to those who saw serious events. A shift in the working range of events thus provides one plausible explanation for the narrowing effect we observed in the first experiment.

What implications does this have for the concept creep of "trauma"? It seems that the frame of implicit reference may play a greater role than frequencies of events within the frame of reference. This suggests that the perceived *absence* of certain threats (genocide does not happen in Boston) may play a special role beyond the perceived *rarity* of certain threats (assault happens rarely in Boston). That is, harm-related concepts may take especially large leaps forward when the most extreme events are eliminated from the public consciousness. Pinker (2011) provides a

dramatic coverage of such examples in his first chapter, reminding us of truly horrific events that were once common but have since disappeared from our concerns: being broken on the wheel, forced to fight to the death for others' entertainment, crucified, burned at the stake, and a litany of other terrible fates. More recently, perhaps young people in developed nations can safely forget about the threats of being paralyzed by polio, captured and tortured by their government, extorted by the mafia, or drafted to the front lines of an interstate war. That is not to say that the public ever becomes *unaware* of such events, but that they cease to be relevant concerns that quickly spring to mind when considering the concept of trauma.

Our results suggest that frames of reference shape individual judgements about the breadth of the trauma concept. This may explain seemingly paradoxical trends in the prevalence of trauma and PTSD. Epidemiological studies often indicate very high self-reported rates of exposure to trauma in first-world countries, despite relatively low rates of violence and disaster in those same countries. In one remarkable example, the lifetime rate of exposure to trauma in Canada was recorded as slightly higher than the rate in South Africa (76% vs. 74%; Dückers, Alisic, & Brewin, 2016)[13].

Furthermore, countries with high vulnerability indices (e.g., high income inequality, political corruption), such as Mexico and South Africa, have *lower* rates of PTSD following trauma compared to countries with lower vulnerability indices, such as Canada and the United States (Dückers et al., 2016). In the United States, rates of violent crimes such as rape steadily declined between the early 1990s and 2010s (Pinker, 2011). Yet during the same timeframe, rates of PTSD have remained relatively stable (Kessler, Sonnega, Bromet, Hughes, & Nelson,

[13] An alternative explanation is that trauma is more evenly distributed in Canada, with individuals in South Africa experiencing a much greater number of traumas per person, but with both countries having a similar number of individuals never experiencing trauma.

1995; Kessler, Berglund, Demler, Jin, Merikangas, & Walters, 2005; Kilpatrick, Resnick, Milanak, Miller, Keyes, & Friedman, 2013).

Our results help explain why the same stressors may be perceived as being more traumatic in an advantaged society. This is not only an epidemiological issue, but a clinical one. Negatively appraising an adverse event forms the basis for developing PTSD in the cognitive model (Ehlers & Clark, 2000). Negative appraisals of an event incrementally predict long-term PTSD rates above and beyond other risk factors (e.g., Bryant & Guthrie, 2005; Ponnamperuma & Nicolson, 2016). The individual who classifies an event as a ‘trauma’ may be more likely to see the event through the lens of permanent harm, importance to life narrative, and risk for PTSD. This type of appraisal may promote rumination about the event and increase vulnerability (Berntsen & Rubin, 2007).

Trauma concepts are also relevant to therapists. A therapist who works in the criminal justice system may be less likely to categorize a given event as a "Criterion A Trauma" compared to a therapist who works primarily with the worried-well. Indeed, manipulations of immediate context have previously been shown to alter standardized ratings of psychopathology, a problem insufficiently addressed in clinical practice (Wedell, Parducci, & Lane, 1990). In the broader political and educational landscape, our results suggest that reminders of very severe events may curb categorization of relatively minor events as 'trauma'.

Our research has several limitations. Although we attempted to reduce demand characteristics (e.g., by emphasizing in the instructions that there were no right or wrong answers), we cannot be certain that demand characteristics were fully absent. For instance, a participant who saw primarily nonserious events might have rated some events as ‘trauma’ that

they didn't truly believe fit the category merely to please the researcher[14]. Importantly, our experiments cannot fully disentangle range from frequency in the mechanistic sense of range-frequency theory. Instead, they provide information on specific manipulations. Results may be affected by hidden incentives or demand characteristics. We instructed participants that there were no right or wrong answers. However, it's possible that participants felt obligated to place some minimum number of events in each category. If this were the case, the basic principles of signal detection theory (SDT) might explain a shift in the threshold.

Although manipulating the range seemed to have the most prominent effect in our experiments, it remains unclear whether there exist conditions under which manipulation of the frequency would affect ratings of trauma. It is possible that changes in frequency do affect rating thresholds, but only over a much longer time period.

Conversely, it is possible that the range effects we obtained in this short timeframe are not durable. Indeed, the fact that ratings can be so easily manipulated suggests that immediate context is relatively powerful, and thus any interventions that influence trauma concepts may be overridden in future contexts. This study only provides information about exposure to *descriptions* of events, not direct exposure to events. It seems likely that direct exposure to events might influence trauma concepts more drastically or more durably. Future studies might explore the effect of exposure to events through audio, video, or virtual reality.

The concept of psychological trauma has expanded rapidly in the socio-political arena as well as in psychiatric diagnosis. We investigated whether a paradigm assessing prevalence-induced concept change could help explain this concept creep. Our results indicated that unlike

[14] Notably, in Levari's (2018) studies, participants' ratings shifted even when participants were told ahead of time that the prevalence would change and even when they were paid to remain consistent in their ratings over time. This somewhat reduces concerns that the effects of stimuli range/frequency are primarily due to demand characteristics.

stimuli tested in previous experiments, such as ratings of color, threateningness, or ethicality (Levari et al., 2018), the categorization of trauma was not significantly affected by changes in prevalence. Instead, when individuals were shown the full range of events, they became stricter over time in their willingness to categorize events as trauma. A second experiment revealed that manipulating the range of events altered trauma ratings: individuals shown mostly benign events were lenient in categorizing events as trauma, whereas individuals shown mostly serious events were more restrained. Thus, expansions in the concept of trauma may occur primarily in frames of reference in which very serious events are absent.

Chapter 4 —

Paper 2: Does Broadening One's Concept of Trauma Undermine Resilience?

Background

The word 'trauma' is often used by physicians to describe physical injuries resulting from a sudden insult to the body (e.g., head trauma). Its usage in psychiatry, however, refers to injuries of an emotional variety. This usage of trauma denotes events capable of producing intense acute distress that may persist for many years, exemplified by posttraumatic stress disorder (PTSD). Initially, only extremely terrifying and rare events were presumed capable of producing PTSD. Since that time, the range of events deemed capable of producing PTSD has substantially expanded (McNally, 2011). For example, scholars have reported PTSD resulting from being bullied at work (Matthiesen & Einarsen, 2004) or giving birth to a healthy baby with no complications (Olde, van der Hart, Kleber, & van Son, 2006). This "conceptual bracket creep in the definition of trauma" (McNally, 2003a, p. 281) has also extended more generally to other types of harm (e.g., bullying, aggression; Haslam, 2016a).

Diagnostic Concept Bracket Creep

The definition of trauma as embodied in the DSM's Criterion A for PTSD has significantly expanded. When committees met to update the criteria for PTSD for later versions of the DSM, empirical research had complicated the DSM-III diagnostic definition (McNally, 2015). Evidence suggested that most individuals suffering from canonical stressors do not develop PTSD, and those who did develop PTSD often had pre-existing risk factors (Breslau, Davis, Andreski, & Peterson, 1991). More importantly, cases were observed in which individuals who had not experienced canonical DSM-III stressors nevertheless met the symptomatic profile of PTSD (Dohrenwend, 2010). DSM-III-R explicitly defined trauma in Criterion A, broadening it to include vicarious exposure (e.g., witnessing another person being harmed; APA, 1987). DSM-IV committee members further expanded the stressor criterion to include a much wider

variety of potential traumatic experiences. If the committees did not broaden the concept of trauma, individuals who otherwise qualified for the diagnosis would have been unable to receive reimbursable treatment for their suffering. DSM-5 scaled back this expansion in comparison to DSM-IV but remained expanded compared to DSM-III. Many researchers have since expressed concern that the expanding definition of PTSD may undermine the integrity of the psychobiological concept of PTSD (e.g., Bracha & Hayashi, 2008; McNally, 2003b).

Personal Concept Bracket Creep

In addition to diagnostic concept changes, individuals may expand their personal definitions of trauma. Although research in this area is comparatively lacking, there are anecdotal suggestions that at least some conceptualizations of traumatic distress have expanded far beyond even the broadened version in the DSM (e.g., speech as violence, Feldman-Barrett, 2017). This trend seems especially evident on the American college campus where calls for protective policies such as trigger warnings, safe spaces, and disinvitations of potentially distressing speakers have increased (Lukianoff & Haidt, 2018). These evolving views substantiate Haslam's (2016) concern that our concept of psychological harm continues to expand dramatically. Yet strong conclusions in this area are premature, as they are limited to specific demographic groups and may not generalize widely.

What influences the breadth of one's personal concept of trauma? Schroeder and colleagues (2017) found that parents who scored higher on victim sensitivity and entitlement had broader concepts of 'bullying'. Expanding this line of research, McGrath and colleagues (2019) evaluated personal concepts of harm, including bullying, abuse, prejudice, and trauma (each of which were closely related). Those with broader concepts of harm were more likely to endorse liberal political attitudes, greater empathic concern, and sensitivity to injustice towards others.

Consistent with Schroeder et al. (2017), those with broader concepts also reported greater entitlement and personal vulnerability. Surprisingly, findings on age were mixed, with one sample showing a link between broader concepts and younger age, whereas the other found no such link (McGrath et al., 2019). Other psychological characteristics such as neuroticism, trait anxiety, or anxiety sensitivity may also play a role.

Perhaps personal concepts of trauma are impacted by general exposure to life adversity either directly or through one's social group. That is, a mildly stressful event in the context of a relatively stress-free life might cause more acute distress than the same event in the context of a challenging life (McNally, 2016b). One recent insight in this area comes from research on *prevalence-induced concept change* (Levari, Gilbert, Wilson, Sievers, Amodio, & Wheatley, 2018). Prevalence-induced concept change means that when instances of a concept become less common, individuals broaden their interpretation of the concept, changing the context in which future instances are evaluated.

One recent investigation found no evidence for prevalence-induced concept change in the concept of trauma over the course of a short experiment (Jones, Levari, Bellet, & McNally, 2020). However, the authors found a *range*-induced concept change – that is, individuals who read brief descriptions of exclusively nonserious events ("walked up a flight of stairs," "was not hired after a job interview") broadened their conceptual brackets, whereas individuals who saw exclusively serious events ("received chemotherapy,", "was raped by a family member") narrowed their conceptual brackets. In summary, it appears that personal trauma concepts are malleable and depend on context.

Does the Personal Concept of Trauma Matter?

Although concepts of trauma have likely changed over time, it is unclear whether the actual clinical phenomena surrounding trauma – that is, the emotional and psychological *consequences* of a given event – have been similarly altered.

One possibility is that the concept of trauma has changed, but the emotional consequences following stressful events have remained stable. That is, distress in the wake of any given event may have remained constant over time, but we merely refer to the same emotional reactions by using different words. For example, an individual bullied in the 1970s might have had a comparable emotional experience to an individual bullied in the 2010s, the only difference being that the latter might be labelled 'trauma' by the individual and his/her contemporaries. Yet there is a second possibility: as personal concepts of trauma have changed over time, so have the average emotional consequences of a given event. That is, an average individual bullied in the 1970s may have experienced a very different emotional experience than an average individual bullied in the 2010s.

In other words, emotional reactions to an event may partly depend on how the event is understood by the person experiencing it. For example, consider the case of childhood sexual abuse. Many children who are sexually abused do not understand what is happening, and thus experience their molestation as confusing, but not horrifying (Clancy, 2005). However, when recalling these experiences years later through the eyes of an adult, they can experience intense betrayal, shock, and symptoms of delayed onset PTSD (McNally, 2012b). That is, the emotional sequalae of this event depend heavily on the victim's beliefs and understanding of the world. This opens the possibility that events that could not have caused PTSD in the past can cause PTSD today. As McNally (2012a) noted, "Vicarious trauma provides especially dramatic examples. Witnessing the torture and execution of human beings was long a form of

entertainment throughout the world. In ancient Rome, amphitheaters featured Christians, criminals, and others tied to stakes as hungry lions devoured them alive to the delight of thousands of cheering fans (pp. 223-224)." Today, witnessing such events would almost certainly result in PTSD for a nontrivial proportion of individuals.

Humans constantly adapt their classifications and categorizations of the world, but humans' concepts do not necessarily affect the *referent* of the concepts. The philosopher Ian Hacking thus makes a useful distinction between 'indifferent kinds' and 'interactive kinds' of categorizations (Hacking, 1999, pp. 100-124). For instance, a tree is an indifferent kind—regardless of whether humans call the tree an alder or an oak, the tree does not respond to its classification. In contrast, a 'police officer' is an interactive kind—humans' collective categorization of an individual as a police officer directly influences how the police officer acts as well as how others act around the police officer.

PTSD may be an interactive kind insofar as the trauma survivor's personal concept of trauma at least partially influences the course of symptoms. The ‘interactive kind’ hypothesis would imply that the conceptual bracket of trauma interacts with one’s short-term and long-term emotional responses to a stressful event. One potential pathway is via appraisals of negative events. Indeed, trauma survivors who report negative appraisals about a traumatic event (e.g., mental defeat, mental confusion) are more likely to develop PTSD and to experience it more persistently (Beierl, Böllinghaus, Clark, Glucksman, & Ehlers, 2019; Dunmore, Clark, & Ehlers, 1999). Why does appraisal matter? In the short term, appraising an event as catastrophic might amplify distress, solidifying the encoding of the trauma memory and its perceived importance. In the long term, people who believe that a stressor is likely to cause a chronic and relapsing

emotional condition from which they will never recover may be especially unlikely to take steps enabling them to confront and overcome their distress.

In this study, we investigated whether personal concepts of trauma are related to stress vulnerability. We conducted a randomized experiment in which individuals were trained to have either narrow or broad beliefs about trauma. Participants then watched a stressful film clip (i.e., trauma film paradigm; James et al., 2016) and responded to various self-report measures. We contacted participants several days later to collect follow-up reports of event-related symptoms (e.g., intrusive memories of the clip). This experiment helps test several important questions. First, we test whether broader concepts of trauma predict poorer stress reactions, including negative emotions and event-related symptoms. Second, we test the extent to which personal concepts of trauma are malleable based on a brief manipulation. Finally, we test whether our brief manipulation of personal trauma concepts has a causal effect on negative outcomes.

Method

Participants

Participants were recruited from Amazon Mechanical Turk and were invited to participate in a two-part study separated by a 48-hour (minimum) window. Participants were excluded from the data analysis if they failed an English language verifier, if they failed more than one attention check across both parts of the study, if they reported having seen the film clip before, or if they voluntarily reported that their data should not be used for any reason (see Supplemental Materials for details). Accordingly, 309 participants provided valid data for Part 1 and 293 participants did so for both parts.

Procedure

We randomized participants to one of two groups. The Narrow Group received psychoeducational materials and exercises meant to induce the belief that the definition of trauma is limited to exceptionally severe events, whereas the Broad Group received a paired series of psychoeducational materials and exercises meant to induce the belief that the definition of trauma extends to virtually any event that may cause emotional distress.

After the manipulation, participants watched a film clip from the movie *The Last King of Scotland* that depicts a mutilated corpse (Cladderwood, Bryer, Steel, & Macdonald, 2006). Participants rated their emotions in response to the film and answered basic verification questions to ensure they had watched it in its entirety.

Participants then completed a manipulation check (Trauma Breadth Scale) and other scales (see Measures). This scale measures the extent to which participants construe trauma broadly. Participants reported demographic information and completed an English verifier question. Finally, after assuring participants that their replies would not imperil their compensation, we asked participants to report if there was any reason their responses might be considered invalid and we asked them for any feedback about the survey that the researcher should know.

In Part 2, participants completed the Impact of Events Scale (IES-R; Weiss, 2007; Horowitz, Wilner, & Alvarez, 1979) in reference to watching the film clip and repeated the Words Can Harm Scale and Trauma Breadth Scale. They were also asked to report if they considered watching the film clip in Part 1 of the study to be a trauma. All procedures were approved by the Harvard University Institutional Review Board.

Measures

Experimental Manipulation

Part 1: Psychoeducation. Participants read a single page of psychoeducational material. They were required to remain on the screen for at least eight seconds and were told to read carefully, as they might be tested on the information later. Participants were given vignettes intended to induce either a narrow or broad belief about trauma (see Supplemental Materials).

Part 2: Guessing Task. Participants were asked to guess the prevalence of PTSD for several different events (e.g., War combat experience has a ___% chance of causing PTSD). Participants were then given feedback on the "correct" answer. In the narrow condition, the feedback was based on actual rates of PTSD (Liu et al., 2017; e.g., war combat experience = 1.9%). In the broad condition participants were asked about less severe events (e.g., being fired from a job) and were given inflated rates of PTSD (e.g., being fired from a job = 55%).

Part 3: Sorting Task. Participants viewed a series of brief descriptions of stressful events (e.g., "being shoved," "being a victim of sexual assault," "witnessing violence on TV"). They were asked to sort the events into one of two categories: "Not Trauma" or "Trauma." They were only allowed to proceed once they had the answer "correct" according to the information provided in the psychoeducation section.

Film and Emotions

Film Clip. The 7-minute film clip included selected scenes from the film *The Last King of Scotland* (Claderwood et al., 2006) including images of the mutilated corpse of a pregnant woman. The same clip was used in a previous trauma film study (Marks & Zoellner, 2014). Participants were instructed to enter full screen mode and to watch it in its entirety.

Emotions. After watching the clip, participants rated their emotions (Fearful, Anxious, Depressed, Sad, Happy, Horrified, Helpless, Irritable, Ashamed, Guilty) on a slider scale from 0 to 100. Emotions (except Happy) were aggregated as Negative Emotions for analysis.

Self-Report Scales

Words Can Harm Scale (WCHS-10) (Bellet, Jones, & McNally, 2018; Jones, Bellet, & McNally, 2020)**.** The WCHS is a 10-item scale with sliders (0-100; Strongly disagree – Strongly agree) measuring the extent to which participants believe that words can cause long-lasting emotional harm. Examples of items include "I should be careful about what I say, as it could permanently damage someone's emotional health" and "Even a simple phrase can be emotionally traumatizing for someone vulnerable." The WCHS-10 was administered at both time points.

Perceived Posttraumatic Vulnerability Scale – Self and Other (PPVS-S; PPVS-O) (Bellet et al., 2018; Jones et al., 2020). The PPVS-S is a 19-item scale measuring the extent to which participants believe they would be vulnerable to an imagined future trauma ("A stranger threatens to take your life and tries to kill you, but you survive the incident"). Participants are asked to rate their projected PTSD-like responses to the event (e.g., "I would never be the same as I was before the event," "I would have difficulty sleeping") on a slider (0-100; Strongly disagree – Strongly agree). The PPVS-O is identical to the PPVS-S but asks participants to first imagine "a specific individual who would be considered an 'average' person," and imagine the event happening to that person, rather than to themselves. The PPVS-S and PPVS-O were highly correlated in our sample (r = 0.93).

Trauma Screener and Life Events Checklist (LEC-5). Participants were given a Criterion A screener (Yes/No). They were also given the LEC-5 and asked to identify their most stressful or traumatic event.

Trauma Breadth Scale (TBS-5) / Manipulation Check. We developed a scale to serve as a manipulation check for this study. The scale was designed to measure participants "Narrow" vs. "Broad" beliefs about trauma: that is, the degree to which they endorse the view that any

event can be a trauma. The items were rated on a 7-point Likert scale (Strongly disagree – Strongly agree). Example items include "Even minor, everyday events can be traumatic" and "Even if people act in a well-intentioned way, they could traumatize someone vulnerable accidentally."

To avoid demand characteristics, we prefaced this scale with the following text: "For the next few items, we are interested in your *own genuine opinions*. Again, please do not respond how you think the researcher might want you to respond. Instead, provide your *own honest opinions*." Participants viewed this text for five seconds before they were able to proceed. This text was repeated at each page break during the TBS-5.

The full version of the scale included 10 items. We removed one item because multiple participants reported that it was confusing. We further performed exploratory factor analyses on the scale based on data from the first time point aiming to characterize the scale properties and to the items converging on a single factor. Based on this analysis, we removed four additional items. The final five-item version of the scale had good reliability at the first time point and excellent reliability at the second time point (Cronbach's *alpha* = 0.89, 0.91) and demonstrated acceptable properties in single factor confirmatory factor analyses at both time points (*CFI* = 0.98, 0.99; *RMSEA* = 0.08, 0.09; *SRMR* = 0.02, 0.02). We use the standardized sum scores from the five-item version of the scale in all future analyses.

Impact of Event Scale – Revised (IES-R) (Weiss, 2007). The IES-R is a 22-item self-report scale that measures PTSD-like symptoms in response to a specific anchor event. In our case, we asked participants to answer in reference to "the film clip you viewed in Part 1 of the study." Items are rated on a 5-point Likert scale ("Not at all" – "Extremely").

Other. In Part 2 of the study, we asked participants "In your own view, was watching the film clip in Part 1 of the study a trauma?" (Yes/No). Four attention checks (e.g., "Please select 'Strongly Agree'") were interspersed throughout the survey.

Research Questions and Analyses

Our analyses were guided by a series of research questions. First, we were interested in whether beliefs about trauma affected outcomes. Thus, we tested several questions related to the TBS-5, and to participants' binary rating of whether they considered the film clip a trauma.

Q1: Did individuals with a broader view of trauma (i.e., higher scores on the TBS-5) experience more intense negative emotions in response to the film clip?

Q2: Were individuals with a broader view of trauma (i.e., higher scores on the TBS-5) more likely to rate viewing the film clip as a trauma several days following viewing the clip?

Q3: Did individuals with a broader view of trauma (i.e., higher scores on the TBS-5) experience more severe symptoms on the IES-R and its intrusions subscale?

Q4: If individuals viewed the film clip as a trauma, did they experience more severe symptoms on the IES-R and its intrusions subscale?

Next, we were interested in the causal effects of our experimental manipulation. That is, was it sufficient to cause changes in participants reported beliefs about trauma on the TBS-5 or on their rating of the film as trauma? If so, did the experimental manipulation have a causal effect on negative emotions and experienced symptoms?

Q5: Did our experimental manipulation impact beliefs about the definition of trauma, as measured by scores on the TBS-5, in the expected direction?

Q6: Did our experimental manipulation impact whether participants viewed the film clip as a trauma?

Q7: Did our experimental manipulation impact the extent to which participants experienced negative emotions in response to the film clip?

Q8: Did our experimental manipulation impact the extent to which participants experienced intrusion symptoms or general symptoms as measured by the IES-R?

We analyzed all data by using linear regressions in R. For binary response variables, we used logistic regression. Responses on the IES-R were heavily skewed, with most responses close to zero. Thus, for analyses with the IES or IES subscales as the DV, we used negative binomial regression. Analyses were conducted with all available valid participants. This means that analyses relevant to the first time point had a slightly higher sample size (n = 309) than analyses that included data from the second time point (n = 293). In analyses relevant to the first time point, we controlled for the following variables: Age, Political Orientation, Gender, and Previous Psychiatric Diagnoses (Y/N) because they were potentially relevant to the dependent variables (i.e., TBS-5, negative emotionality, rating the clip as trauma, IES-R). In analyses relevant to the second time point, we additionally controlled for the amount of time elapsed between viewing the film clip and completing Part 2 of the study. We report effect sizes as Cohen's f^2, which is a ratio of the unique contribution of the variable of interest to the model r^2 to the overall r^2 of the model (subtracted from 1). For generalized linear models, we computed a pseudo-r^2 based on the ratio of residual to null deviance values. Cohen's f^2 values of 0.02, 0.15, and 0.35 represent the thresholds for small, medium, and large effect sizes, respectively.

Results

Sample Characteristics

Our sample included 309 participants. Of these participants, a majority identified as male (n = 175) and the remainder as female (n = 134). Participants identified their ethnicity as

Hispanic (n = 20) or Not Hispanic (n = 289) and their race as Caucasian (n = 244), Black/African American (n = 30), Hispanic (n = 10), Asian/Pacific Islander (n = 10), Multi-racial or multiple races selected (n = 14), or Other (n = 1). They had a median age of 37 years old with a standard deviation of 11.5 years. Most participants identified as Not Religious (n = 155). Participants leaned slightly liberal in political orientation (*mean* = 2.75; 1 = *very liberal* to 5 = *very conservative*). A minority of participants reported experiencing a Criterion A traumatic event in their lifetime (n = 85) or having been diagnosed with a psychiatric or psychological problem (n = 50). There were 16 individuals who did not complete Part 2 of the study, leaving us with 293 participants who completed follow-up measures.

Beliefs about Trauma

Q1: Did individuals with a broader view of trauma (i.e., higher scores on the TBS-5) experience more intense negative emotions in response to the film clip? Yes. After controlling for covariates, TBS-5 scores predicted greater composite negative emotions (f^2 = 0.09, $p < 0.001$). More liberal political orientation was also predictive of greater negative emotions (f^2 = 0.03, p = 0.002).

Q2: Were individuals with a broader view of trauma (i.e., higher scores on the TBS-5) more likely to rate viewing the film clip as a trauma several days following viewing the clip? Yes. In a logistic regression, those with higher TBS-5 scores were more likely to rate the film clip as a trauma (f^2 = 0.03, p = 0.002).

Q3: Did individuals with a broader view of trauma (i.e., higher scores on the TBS-5) experience more severe symptoms on the IES-R and its intrusions subscale? Yes and no. In a negative binomial regression, those with higher TBS-5 scores were slightly more likely to experience symptoms in general on the full IES-R (f^2 = 0.012, p = 0.048), but were not more

likely to experience intrusion symptoms as measured by the Intrusions subscale (f^2 = 0.007, p = 0.151). More conservative political orientation predicted both general symptoms and intrusions (f^2 = 0.01, 0.02, p = 0.047, 0.012), and younger age predicted more intrusion symptoms (f^2 = 0.03, p = 0.010).

Q4: If individuals viewed the film clip as a trauma, did they experience more severe symptoms on the IES-R and its intrusions subscale? Yes. In a negative binomial regression, those who viewed the film clip as a trauma experienced increased symptoms on the full IES-R (f^2 = 0.09, p < 0.001). Those who viewed the film clip as a trauma also experienced greater intrusion symptoms on the IES-R (f^2 = 0.11, p < 0.001). Those of younger age and more conservative political orientation also experienced greater intrusion symptoms (f^2 = 0.02, 0.02, p = 0.019, 0.024).

Experimental Manipulation

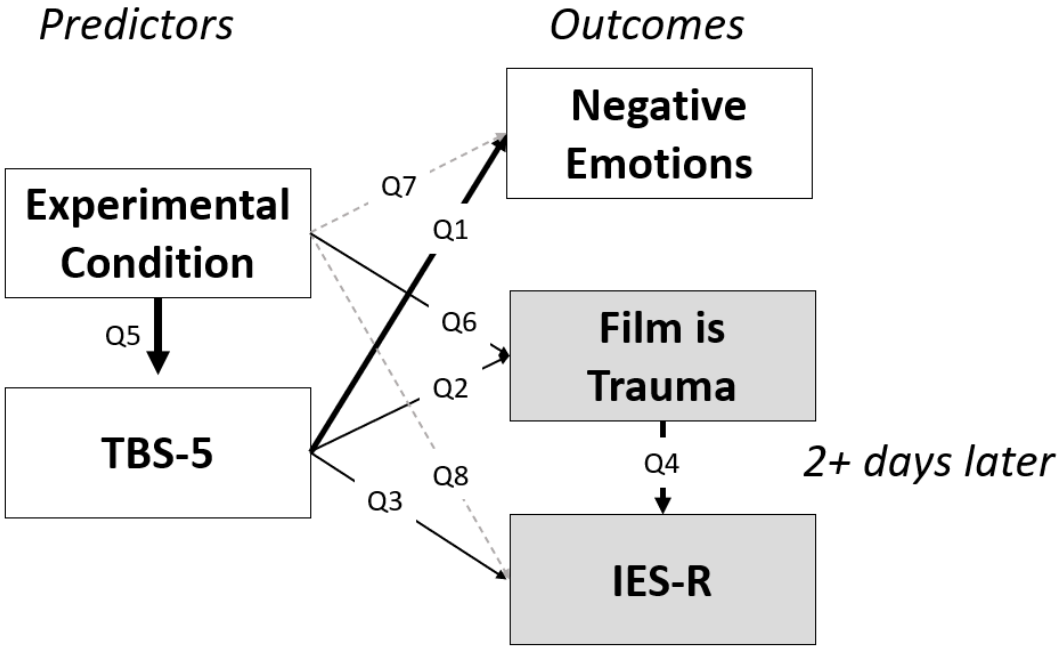
Q5: Did our experimental manipulation impact beliefs about the definition of trauma, as measured by scores on the TBS-5, in the expected direction? Yes. Controlling for relevant covariates, we found that those in the Narrow Condition scored lower on the TBS-5 compared to those in the Broad Condition with a medium-large effect size (f^2 = 0.28, p < 0.001). This effect endured at the second time point, showing that our intervention had lasting effects (f^2 = 0.28, p < 0.001). At both time points, more liberal political orientation also predicted TBS-5 scores (f^2 = 0.04, 0.04, p < 0.001).

Q6: Did our experimental manipulation impact whether participants viewed the film clip as a trauma? Yes. Those in the Narrow Condition were less likely to view the film clip as trauma (f^2 = 0.02, p = 0.020).

Q7: Did our experimental manipulation impact the extent to which participants experienced negative emotions in response to the film clip? No. There was no significant difference between conditions ($f^2 = 0.01$, $p = 0.109$). Those with more conservative political orientation experienced slightly more negative emotions ($f^2 = 0.02$, $p = 0.027$).

Q8: Did our experimental manipulation impact the extent to which participants experienced symptoms on the IES-R and its intrusions subscale? No. There was no significant difference between the conditions for either the full IES-R ($f^2 < 0.01$, $p = 0.335$) or intrusion symptoms ($f^2 = 0.01$, $p = 0.212$). Those of younger age ($f^2 = 0.03$, $p = 0.010$) and more conservative political orientation ($f^2 = 0.02$, $p = 0.012$) experienced greater intrusion symptoms. A summary of the results of all eight research questions appears in Figure 4.1. Overall, results consistently support the idea that beliefs about trauma (i.e., the TBS-5) predict reactions to a stressful film clip. Evidence regarding the results of the experimental condition are more mixed.

Figure 4.1. Summary of Research Questions



Note. *IES-R = Impact of Events Scale – Revised; TBS-5 = Trauma Breadth Scale*
Solid lines indicate significant results, dashed lines indicate nonsignificant results (*alpha* = 0.05). Lines widths are weighted according to effect sizes ($f^2 < 0.07$, *1 pt*, $0.07 < f^2 < 0.15$, *3 pt*, $0.16 < f^2$, *4.5 pt).* Measurements that took place at the second time point are colored grey. For Q3, a significant effect was found for the IES-R total but not for the Intrusions subscale specifically.

Exploratory Analyses

We were interested in correlations between broad beliefs about trauma (TBS-5) and other variables. A summary of associations between the TBS-5 and other scales appears in Table 4.1. The TBS-5 was strongly associated with the belief that words can cause long-lasting emotional damage (i.e., WCHS). It was moderately associated with participants' sense of vulnerability to PTSD symptoms following a hypothetical trauma, both in terms of their own perceived vulnerability, and the vulnerability of others. This is consistent with the findings of McGrath and colleagues (2019), who found that those with broader concepts of harm felt more personally vulnerable and endorsed greater sensitivity to injustices done to others. Broad beliefs were associated with more liberal political orientation. Although the magnitude of the correlation between broad beliefs and age was similar size to that of political orientation, it was nonsignificant in this case. This is also consistent with McGrath et al. (2019), who found inconsistent correlations between harm-related concepts and age.

Table 4.1

Correlations between Trauma Breadth Scale and Selected Covariates

Variable	TBS-5	TBS-5 (follow-up)
1. TBS-5		
2. TBS-5 (follow-up)	.83**	
3. Gender	.04	.09
4. Religiosity	.00	.00
5. Political Orientation (Right)	-.13*	-.12*
6. Age	-.10	-.08
7. Caucasian	.09	.11
9. WCHS	.70**	.69**
10. PPVS-S	.38**	.38**
11. PPVS-O	.32**	.31**
12. Previous Trauma	-.05	-.04
13. Previous Psychiatric Diagnosis	-.04	-.05

Note. TBS-5 = Trauma Breadth Scale, WCHS = Words Can Harm Scale, PPVS-S = Perceived Posttraumatic Vulnerability Scale – Self, PPVS-O = Perceived Posttraumatic Vulnerability Scale – Other, Caucasian = Caucasian/White race compared to all other identified races (collapsed due to sample size constraints). Correlations with binary variables are point-biserial correlations.
* indicates $p < .05$. ** indicates $p < .01$.

Discussion

Is trauma limited to only a small subset of extreme events such as rape and warfare? Or can almost anything cause long-lasting emotional damage? We measured participants' beliefs about this issue and had them watch a disturbing film clip. We found that the more individuals viewed trauma as a broad concept applying to many different events, the more likely they were to experience negative emotional outcomes after watching the film. They reported more negative emotions immediately after the film, had more intrusion symptoms in the days following, and were more likely to report that watching the film was itself a trauma. Moreover, we found that their belief was at least somewhat malleable – after a short series of psychoeducational tasks, participants could be induced to report a somewhat broader or narrower belief about traumatic events.

Importantly, however, the belief induction in our study was insufficient to significantly alter two of the three outcomes. Those induced to have a broader belief (versus a narrow belief) did not experience significantly greater negative emotions nor did they report significantly greater symptoms on the IES-R or its Intrusions subscale. They were, on the other hand, more likely to rate watching the film as a trauma. The lack of experimental effects on emotional outcomes must qualify any conclusions about trauma concepts. Although our data show that beliefs about trauma predict important emotional outcomes, conclusions about causality remain unconfirmed. Indeed, another variable (e.g., trait anxiety) may drive both beliefs about trauma and sensitivity to stressors. Accordingly, broadening or narrowing a person’s definition of trauma may not necessarily have any effect on experienced emotions or PTSD symptoms.

That said, it is difficult to parse whether a causal effect is truly absent, or whether our study was simply inadequate to capture a causal relationship. On one hand, our intervention had

a medium-to-large effect size that persisted for several days. On the other hand, it seems unlikely that five-to-ten minutes of psychoeducational tasks would cause deeply internalized shifts in beliefs about trauma and emotional vulnerability. The measured effect on beliefs may be surface-level and ephemeral, or perhaps even the result of demand characteristics (although we did put measures in place to prevent demand characteristics). Perhaps a larger study with more potent interventions over a longer period might detect causal effects of beliefs about trauma.

Our results are highly relevant to debates about conceptual bracket creep in the definition of trauma and PTSD. First, our study suggests that beliefs about trauma and PTSD can be altered by relatively small psychological interventions. Thus, it is highly plausible that bracket creep could result from intentional or unintentional social signaling about trauma. Although researchers have pointed to decreases in violence (e.g., McNally, 2003a, pp. 279-281; Pinker, 2011) as a likely explanation for bracket creep in the past few decades (Haslam, 2016a; Jones et al., 2020), our results suggest that trauma brackets could also be shifted (at least temporarily) in the absence of such objective societal changes. For instance, changing formal diagnostic standards regarding PTSD and then informing the public about those standards could plausibly shift beliefs regarding what types of events can cause long-lasting emotional harm. More subtly, protective policies like trigger warnings and safe spaces might signal that minor adversities are potentially traumatogenic (Lukianoff & Haidt, 2018). Indeed, some recent studies have indicated that trigger warnings increase perceptions of personal vulnerability (Bellet et al., 2018) and reinforce the perceived importance of traumatic events (Jones et al., 2020), although other studies have found no such effects (Bellet et al., 2020).

Second, our results are largely consistent with previous researchers' words of caution about bracket creep, namely that "by identifying increasingly mild events and experiences as

harmful, concept creep may make people vulnerable and fragile, prone to catastrophize everyday life" (McGrath et al., 2019, p. 79). Indeed, among the top quartile scorers on the TBS-5, 56% reported that viewing a film clip from a popular Hollywood movie was a trauma.[15] Broader trauma concepts were highly correlated with the belief that words can cause long-lasting emotional harm (r = 0.70) and moderately correlated with the subjective perception that one is vulnerable to PTSD following trauma (r = 0.38). More importantly, broader trauma beliefs were associated with objective vulnerability; those with broader trauma concepts experienced more negative emotions and PTSD symptoms in response to the film clip.

We were surprised to find that although more liberal political orientation was related to higher scores on the TBS-5, more conservative political orientation was associated with greater negative emotions and intrusions symptom following the film clip. We suspect this may be a factor of the specific film clip used, as past studies have noted greater negative reactions among conservatives for stimuli primarily focused on core disgust domains (e.g., mutilated body parts, dog feces; Elad-Strenger, Proch, & Kessler, 2019; Smith, Oxley, Hibbing, Alford, & Hibbing, 2011). It could also result from traditional conservative views regarding women as a protected class or greater sensitivity to the death of the fetus. Extending this study to diverse stressful events would provide useful clarification.

This study had several important limitations. Our online sample was modest in size, mostly Caucasian, and entirely English-speaking, thereby limiting the generalizability of the results. The study used a single video clip for all participants, and it is possible that results might differ depending on the exact content of the stimulus used. Although some results were experimental, many were correlational, opening possibilities for spurious relationships

[15] Compared to 28% in the bottom quartile

attributable to unmeasured variables. Although the measures demonstrated good reliability, some of them are newer scales with limited tests of validity; one scale was developed in the context of this study specifically. Both direct replications of the current study and conceptual replications that vary the stimuli and measurement tools would be informative.

In conclusion, individuals' personal trauma concept varies in breadth; some reserve the term for only the most severe stressors, whereas others apply it broadly. This variation is predictive of how individuals respond to a stressful experience, namely, watching a disturbing film clip: those with broader beliefs suffer greater emotional consequences. We further found that beliefs about trauma can be influenced through psychoeducational content, but we did not find that this psychoeducational content significantly influenced emotional outcomes. Understanding shifting concepts of trauma is essential to sociocultural debates regarding harm, violence, and PTSD. Hacking argued that changes in concepts directly alter human experience by "looping" back into how individuals understand themselves (Hacking, 1999). If vulnerability to PTSD expands as adversity declines, then public health efforts that simply decrease adverse events may fail to reduce rates of PTSD (McNally, 2016b). Indeed, if activists are serious about reducing rates of PTSD, there exists a serious need to grapple with the fact that PTSD rates in the United States have not fallen in tandem with decreases in violence over the same time period (Helzer, Robins, & McEvoy, 1987; Kessler, Sonnega, Bromet, Hughes, & Nelson, 1995; Kessler, Berglund, Demler, Jin, Merikangas, & Walters, 2005; Kilpatrick, Resnick, Milanak, Miller, Keyes, & Friedman, 2013; Pinker, 2011). If Hacking's hypothesis has merit for the trauma concept, investigating the causes and consequences of conceptual bracket creep should be a primary aim in the study of trauma-related disorders.

Chapter 5 —

Paper 3: Helping or Harming?

The Effect of Trigger Warnings on Individuals with Trauma Histories

Background

Giving a trigger warning means providing prior notification about forthcoming content that may be emotionally disturbing (Boysen, 2017). In this sense, trigger warnings are similar to PG-13 or "viewer discretion advised" warnings that are common across many different forms of media. Trigger warnings are distinct in that they originated as a measure of protection specifically for survivors of trauma. For those with posttraumatic stress disorder (PTSD), viewing reminders of trauma can spark painful reexperiencing symptoms (e.g., flashbacks; American Psychiatric Association [APA], 2013). Trigger warnings originated in online discussion groups for survivors of sexual trauma, where individuals would warn readers before discussing their experiences. Since their inception, trigger warnings have expanded far beyond the boundaries of specialized online communities. Trigger warnings are now used in educational settings, social media, entertainment, and other venues. In addition to their expansion in setting, they have also expanded in scope beyond sexual violence (Wilson, 2015).

Trigger warnings have sparked considerable debate in higher education. Proponents of trigger warnings emphasize their importance in creating an inclusive atmosphere for disadvantaged groups on campus (e.g., Karasek, 2016). They argue that trigger warnings provide agency to engage or not to engage and that they allow trauma survivors to adequately prepare to engage with difficult material. Critics suggest that trigger warnings imperil free speech, academic freedom, and effective teaching, preventing students from engaging with challenging material (e.g., Ellison, 2016). Other critics have suggested that trigger warnings foster unreasonable expectations about the world, hampering natural resilience among young people (e.g., Lukianoff & Haidt, 2015). Further, trigger warnings could also be problematic for trauma survivors in particular (McNally, 2016a). Those who view trauma as a core part of their identity

have worse symptoms (Berntsen & Rubin, 2006; Brown, Antonius, Kramer, Root, & Hirst, 2010; Robinaugh & McNally, 2011). Therefore, trigger warnings might iatrogenically reinforce the importance of past traumatic events for the very people they were originally designed to help.

The arguments surrounding trigger warnings are often complex. Before diving into this complexity, a much more basic question should be answered: do trigger warnings actually work? That is, do they help trauma survivors emotionally prepare to engage with difficult material? From the vantage point of clinical science, trigger warnings are a type of community-based clinical intervention intended to foster emotional well-being among trauma survivors. Yet due to their grassroots origin in a non-clinical setting, trigger warnings have expanded for years without the rigorous scientific evaluation that normally accompanies such interventions.

Bellet, Jones, and McNally (2018) were among the first to experimentally test the effect of trigger warnings. In a crowd-sourced sample of individuals who had not experienced past trauma, they found that trigger warnings given before literature passages had no significant effect on anxiety. Further, they found that trigger warnings undermined participants' sense of their resilience to potential future traumatic events, and their sense of the resilience of others. They also reported a moderation effect – among individuals who believed that words were emotionally harmful, trigger warnings acutely increased anxiety reactions.

Since this original study, the scientific literature has quickly expanded. Bellet et al. (2019) conducted a preregistered replication of the same protocol of Bellet et al. (2018) with undergraduate college students. Their results suggest that trigger warnings created a trivially small, yet genuine increase in anxiety. However, they found strong evidence that the previously observed effects on projected vulnerability and the moderation effect from Bellet et al. (2018) did not replicate among college students. In the most comprehensive set of studies to date,

Sanson, Strange, and Garry (2019) concluded that trigger warnings had trivially small effects overall. Across six studies of varying sample characteristics, they found that negative affect and intrusive memories were similar regardless of whether individuals received trigger warnings. Bridgland, Green, Oulton, and Takarangi (2019) similarly found that trigger warnings had trivially small effects on arousal levels when participants viewed photos. Importantly, however, their results differentiated *anticipatory* anxiety from *response* anxiety. Anticipatory anxiety refers to levels of anxiety after viewing the trigger warning but before viewing the stimulus, whereas response anxiety refers to anxiety after viewing the stimulus. Although trigger warnings appeared to have a trivial effect on *response* anxiety, they reliably increased *anticipatory* anxiety. Relatedly, Bruce (2017) found that trigger warnings produced greater physiological markers of anticipatory anxiety compared to PG-13 warnings or no warnings. Gainsburg and Earl (2018) found that trigger warnings increased negative anticipatory affect, but slightly decreased negative response affect. Articles evaluating the effect of trigger warnings on anxiety or negative affect are summarized in Table 5.1.

Some trigger warning advocates suggest that although trigger warnings may not help individuals cope with triggering content, they may help individuals *avoid* the content altogether. Although avoidance reduces anxiety in the short run (Hofmann & Hay, 2018), it maintains or worsens PTSD in the long run (e.g., Brewin & Holmes, 2003; Dunmore, Clark & Ehlers, 1999; Foa & Kozak, 1986). There are some instances in which a small degree of avoidance may be helpful (Hofmann & Hay, 2018), but the scope of such situations is limited (e.g., acts of partial avoidance to increase the acceptability of exposure treatments; Deacon et al., 2010; Levy & Radomsky, 2014). These situational exceptions should not be interpreted to mean that avoidance should be used as a primary coping mechanism or a long-term strategy. Graduated, prolonged

exposure to trauma cues is beneficial to long-term well-being, especially in a controlled treatment setting (e.g., Powers, Halpern, Ferenschak, Gillihan, & Foa, 2010), although the principle of fear extinction via exposure applies much more broadly than in controlled psychotherapy (e.g., Milad, Rauch, Pitman, Quirk, 2006; Myers & Davis, 2007). Indeed, considering current theories of anxiety and learning, a *lack* of exposure to trauma cues (e.g., successful & pervasive avoidance) is likely to be much more harmful for trauma survivors in the long-term. In one study of more than 300 female assault survivors, 8.1% of patients on a wait list experienced reliable worsening of PTSD symptoms compared to 0% reliable worsening among those receiving prolonged exposure treatment (Jayawikreme et al., 2014).

Regardless of the considerable body of literature contraindicating the counsel of avoidance for trauma survivors, it remains unclear whether individuals provided with trigger warnings use them to avoid triggering content. Gainsburg and Earl (2018) found that participants were marginally more likely to avoid film clips with trigger warnings ($p = 0.06$). In contrast, Kimble (2019) found that individuals very rarely avoided material due to trigger warnings. In this study, we primarily focus on the issue of whether trigger warnings help trauma survivors emotionally cope with (rather than avoid) triggering content. However, we measured whether participants dropped out of the study after seeing a trigger warning (i.e., avoided) as a secondary outcome.

Table 1

Effect of Trigger Warnings on Anticipatory and Response Anxiety

Authors (*Year*)	*n*	Source	Trauma Exposure	Stimuli	Outcome	Anticipatory Anxiety *(d)*	Response Anxiety *(d)*
Bellet, Jones, & McNally (*2018*)	270	Crowd-sourced	No	Literature passages	Self-reported anxiety		0.06 [-0.18, 0.30]
Gainsburg & Earl (*2018*)[1]	276; 979	Crowd-sourced	Mixed	Essay	Negative affect (SAM)	0.75** [0.58, 0.92]; 0.26** [0.10, 0.42]	-0.17* [-0.33, -0.01]
Bellet, Jones, Meyersburg, Brenneman, Morehead, & McNally (*2019*)	462	Students	No	Literature passages	Self-reported anxiety		0.20* [0.02, 0.38]
Sanson, Strange, & Garry (*2019*)[2]	1880	Students / Crowd-sourced	Mixed	Story / Film clip	Negative affect (PANAS)		0.02 [-0.08, 0.13]
Bridgland, Green, Oulton, & Takarangi (*2019*)[2]	1600	Crowd-sourced	Mixed	Photos	State anxiety (STAI)	1.36** [0.99, 1.74]	0.07 [-0.03, 0.16]
Authors (*Current Study*)	451	Crowd-sourced	Yes	Literature passages	Self-reported anxiety		0.08 [-0.11, 0.26]

Note: Positive *Cohen's d* type effect sizes indicate an increase in anxiety. SAM = Self-Assessment Manikin, PANAS = Positive and Negative Affect Scale, STAI = State Trait Anxiety Inventory. Cells are left blank for studies that did not measure anticipatory anxiety.

*p < 0.05, **p < 0.01

[1]Anticipatory anxiety effects are from Studies 2 & 3 respectively (*n* = 276, 979) and based on reported *t*-values; response anxiety is from Study 3. Confidence intervals are estimated based on incomplete information.

[2]Results are internal meta-analyses across all experiments. For Bridgland et al. (2019) response anxiety, we meta-analyzed the effects reported in Table 5

The encouraging growth of studies has begun to converge on the consensus that trigger warnings are not typically helpful in reducing anxiety. This finding has been consistent across various types of trigger warnings and types of potentially triggering content. For instance, Sanson et al. (2019) find similar effects regardless of whether trigger warnings mention potential emotional reactions (e.g., "You might find this content disturbing") or whether they only mention content (e.g., "The following story contains violence and death"). Similar effects are found with literature passages, stories, photos, and film clips. The literature also suggests several different types of harm potentially caused by trigger warnings (e.g., anticipatory anxiety, perception of vulnerability), but with occasionally mixed or contradictory results.

There remain several important limitations to this area of research. First, none of the studies has exclusively focused on the primary intended target of trigger warnings – survivors of trauma. Although some of the studies have included trauma survivor subgroups (e.g., Sanson et al., 2019), this has not been the main focus of any study. If trigger warnings are designed to promote the well-being of trauma survivors, this is an important limitation. Second, there remain several unanswered questions relevant to trauma survivors, especially those who are experiencing symptoms of PTSD. For example, the severity of PTSD symptoms might moderate trigger warnings' effects (e.g., trigger warnings may be helpful to those with severe PTSD symptoms, but not to those with milder symptoms). Trigger warnings may also harm trauma survivors by reinforcing the belief that their trauma is central to their identity. Third, many studies used different operationalizations and stimuli. On one hand the consistency of results across diverse studies suggests that the findings may be robust to varying format of the warnings operationalizations. On the other hand, more direct replications are also essential and provide protections against potential biases (e.g., publication bias, selective reporting).

In the current study, we tested the effect of trigger warnings in a large sample of trauma survivors recruited from Amazon Mechanical Turk, a crowd-sourcing platform. This preregistered study includes a direct replication of the experiment in Bellet et al. (2018) and extends the paradigm to address questions specific to trauma survivors. Further exploratory analyses examined vulnerable subpopulations (e.g., those who had received a diagnosis of PTSD), as well as tests of validity for measures used in the original study.

Method

Participants

The preregistration for the study design and analysis plan are available on the Open Science Framework (https://osf.io/gdxtr/). Any deviations or exploratory analyses that were not specified in the preregistration are marked as such within this manuscript. Participants were recruited online from Amazon Mechanical Turk. We prespecified a sequential data collection procedure with a stopping rule based on Bayes Factors. However, the evidential criteria for our stopping rule were not met at any intermediate step, so we recruited participants until the specified ceiling of 600 participants had completed the study. Participants were excluded from the study if they incorrectly answered an attention check or if they failed an English fluency verifier (see Supplemental Materials for details). This left a final sample of 451 participants.

Procedure

This study was a randomized controlled experiment with one control group (no trigger warnings) and one experimental group (trigger warnings for distressing passages). After providing informed consent, participants were asked to complete a CAPTCHA and answer three questions to verify US residency (e.g., "What is the most common emergency number in the United States? [0-0-0 / 9-1-1 / 0-0-0-9-5 / 9-9-9]"). Participants failing these items were

immediately barred from completing the study. Remaining participants then completed a single-item question[16] that screened for the presence of trauma according to PTSD diagnostic Criterion A in the Diagnostic and Statistical Manual of Mental Disorders (DSM-5; American Psychiatric Association, 2013). Participants endorsing this screener later completed the Life Events Checklist for DSM-5 (LEC-5) to more specifically assess the presence of a qualifying traumatic event. Participants who had not experienced such a trauma were excluded from the study.

Participants then read literature passages typical of a high school or college English class. Passages were standardized by length, and participants were shown the passages for a minimum of 20 seconds before they were allowed to proceed to the next screen. The passages were previously rated on the degree to which they provoked anxiety in a pilot study (Bellet et al., 2018). Depending on their content, passages are hereafter classified as either *neutral* (no disturbing content, e.g., a character description from Melville's *Moby-Dick*), *mildly distressing* (general themes of violence or harm with no graphic details, e.g., a description of a battle from Bradley's *Flags of our Fathers*), or *markedly distressing* (graphic scenes of violence, injury, or death, e.g., the murder scene from Dostoevsky's *Crime and Punishment*). After each passage, participants rated their emotional state by using slider bars ranging from 0 (not at all) to 100 (very much) on seven emotions: *sad, happy, afraid, anxious, angry, content*, and *disgusted*. The responses to the *anxious* rating were used as the primary outcome measure.

Participants in both conditions first read three mildly distressing passages in random order to establish a baseline. Next, participants read a series of five neutral passages and five

[16] "Some people experience extremely distressing events that are outside the range of common experience. Examples of such an event include being in a life-threatening situation such as military combat, a natural disaster (floods, earthquakes), a man-made disaster (being in a car accident where someone was seriously injured or killed), being raped, being violently assaulted, or being tortured. At any time in your life, have you directly experienced any of these kinds of events?"

markedly distressing passages intermixed in random order. In the experimental condition, markedly distressing passages were preceded by a trigger warning (*TRIGGER WARNING: The passage you are about to read contains disturbing content and may trigger an anxiety response, especially in those who have a history of trauma*). In the control condition, passages were preceded by a screen that indicated they were about to view the next passage, which was acknowledged by clicking a radio button. After these 10 passages, participants read three more mildly distressing passages appearing in random order that served to test for any sensitization effects.

After reading all passages, participants completed the questionnaires detailed below. Participants also answered questions about demographic information and psychiatric history, completed an English fluency verifier, and answered validity-related questions that did not impact payment (e.g., "What do you think was the purpose of this study?", "Is there any reason you think that your data should not be used (this will not impact payment)?"). At the end of the study, they received a debriefing form explaining the purpose of the study in detail.

Measures

Self-Reported Emotion. After each passage, participants rated their emotions on a sliding scale from 0 to 100 ("Please use the slider bars to show how much you are experiencing each emotion on a scale from 0 (not at all) to 100 (very much)"). We were primarily interested in ratings of anxiety, but to reduce demand characteristics, we included a broad range of emotions: sad, happy, afraid, anxious, angry, content, and disgusted.

Centrality of Event Scale (CES). The CES is a 7-item questionnaire that measures the extent to which participants view the memory of their worst event as a reference point for personal identity and the attribution of meaning to other experiences in their life (Berntsen &

Rubin, 2006). Items (e.g., "I feel that this event has become a central part of my life story") are rated on a 5-point Likert scale (1 = *totally disagree*, 5 = *totally agree*). The CES has been linked to higher levels of PTSD symptoms (e.g., Berntsen & Rubin, 2006; Brown, Antonius, Kramer, Root, & Hirst, 2010; Robinaugh & McNally, 2011). The CES has also been linked to perceived posttraumatic growth, or the sense that life has been enhanced (e.g., rendered more meaningful) as a result of a traumatic event (Boals & Schuettler, 2011; Groleau, Calhoun, Cann, & Tedeschi, 2013). That said, posttraumatic growth is a somewhat controversial measure that should not be thought of as a straightforward measure of psychological health (Jayawickreme & Blackie, 2014); indeed, some studies indicate that it is positively correlated with more severe PTSD (Dekel, Ein-Dor, & Solomon, 2012; Kleim & Ehlers, 2009). The CES displayed excellent internal consistency in the current study (α = 0.94).

Perceived Posttraumatic Vulnerability Scale – Self (PPVS-S). This 19-item questionnaire measures participants projections of their own emotional impairment and posttraumatic symptoms if they were to hypothetically experience a trauma in the future (Bellet et al., 2018). Participants are asked to imagine being exposed to an attempt on their life, and then indicate their agreement with the effects of that experience (e.g., "I would not be able to work a job, or take care of myself") on a 100-point scale (1 = *disagree*, 100 = *agree*). The PPVS-S displayed excellent internal consistency in the current study (α = 0.95).

Perceived Posttraumatic Vulnerability Scale – Other (PPVS-O). This 19-item questionnaire measures participants' belief that if an "average" person were to experience a trauma, they would experience persistent and debilitating emotional harm (Bellet et al., 2018). Participants are asked to imagine an average person being exposed to an attempt on his or her life, and then indicate their agreement with the effects of that experience (e.g., "he/she would

have nightmares of the event") on a 100-point scale (1 = *disagree*, 100 = *agree*). The PPVS-O displayed excellent internal consistency in the current study ($\alpha = 0.95$).

Life Events Checklist for DSM-5 (LEC-5). The LEC-5 is a self-report instrument that identifies specific traumatic events that have occurred in one's lifetime (Weathers, Blake, et al., 2013). The LEC-5 contains 16 events known to potentially result in PTSD or distress (e.g., "life threatening illness or injury") and an additional option for "any other very stressful event or experience". In our study, participants were initially screened by a question assessing the presence of a Criterion A trauma. Later in the study, they were provided with the LEC-5 and asked to choose the event description that best matched their most stressful or traumatic event. The LEC-5 includes the highly ambiguous item “Any other very stressful event or experience”, and therefore was not appropriate for automated screening at the beginning of the study. Thus, the initial screener was used to identify the presence of a qualifying traumatic event, whereas the LEC-5 was used to gather more specific details for participants who had completed the study. Participants’ answers to the LEC-5 are reported in the results section.

PTSD Checklist for DSM-5 (PCL-5). The PCL-5 is a 20-item questionnaire that assesses the presence and severity of PTSD symptoms in the past month (Weathers, Litz, et al., 2013). When answering the PCL-5, participants in our study were instructed to answer the questions keeping in mind their worst event as selected on the LEC-5. Items on the PCL-5 correspond closely to DSM-5 criteria for PTSD (e.g., "In the past month, how much were you bothered by repeated, disturbing, and unwanted memories of the stressful experience"). The PCL-5 is often used to monitor symptoms over time, to screen for PTSD, or assist in making a provisional diagnosis of PTSD. For exploratory analyses involving the PCL-5, we used the cutoff score of 33 recommended by the United States Department of Veterans Affairs (Weathers, Litz, et al., 2013),

and based on research (Bovin et al., 2017; Wortmann et al., 2017). The PCL-5 displayed excellent internal consistency in the current study (α = 0.96).

Words Can Harm Scale (WCHS). The WCHS is a 10-item scale that measures the degree to which participants believe that words can cause serious and lasting emotional harm (Bellet et al., 2018). Participants rated their agreement with each statement (e.g., "Even a simple phrase can be emotionally traumatizing for someone vulnerable") on a 100-point scale (1 = *disagree*, 100 = *agree*). The WCHS displayed excellent internal consistency in the current study (α = 0.92).

Trigger Warnings Attitudes Assessment (TWAA). We administered three items to assess participants' prior exposure to and attitudes about trigger warnings. First, we provided participants with a definition of trigger warnings (i.e., "A trigger warning is a statement given prior to presented material that allows the viewer to prepare for or avoid distress that it may cause, particularly if the viewer has clinical mental health issues"). Participants were then asked to give a binary rating of whether they believe that trigger warnings should be given prior to potentially distressing material (TWAA-1). If the participants selected "yes", they were shown a checklist asking why they think trigger warnings should be used (e.g., "Trigger warnings help to protect vulnerable populations…") including an "Other" option with the ability to write in a response (TWAA-2). Participants were then asked to rate their agreement with the statement "I have personally seen many trigger warnings used before" on a 7-point Likert scale (1 = *strongly disagree*, 7 = *strongly agree;* TWAA-3). Only TWAA-3 was used for the primary analysis in the present study, as specified in the preregistration. The other items are to be used in future studies addressing attitudes about trigger warnings.

Trauma-Matching Passages. We asked participants if any of the literary passages they read during the study reminded them of their worst event. If participants answered yes, we provided them with a checklist of passages, and asked them to identify which ones reminded them of their worst event. These passages were marked as "trauma-matching" passages.

***Demographics Questionnaire*.** We asked participants to report their gender, race, ethnicity, religiosity, political orientation, and age. Religiosity and political orientation were assessed with a 5-point Likert scale (1 = *not religious*, 5 = *extremely religious*; 1 = *very liberal*, 5 = *very conservative*). We also asked participants to report whether they are currently a full-time undergraduate student.

Psychiatric History. At the beginning of the study, all participants were given a screener assessing for the presence of a Criterion A traumatic event. Participants were only included in the study if they indicated the presence of a Criterion A event. Near the end of the study, we asked participants whether they had “ever been diagnosed with a psychiatric or psychological problem.” If participants answer yes, we asked them to choose all diagnoses that apply from a list including PTSD and “Other” (to allow for a free response of any disorders not listed).

Behavioral Avoidance (Dropout). We measured behavioral avoidance by assessing whether participants dropped out of the study after seeing a trigger warning or at any other point (e.g., dropout post-randomization). Dropout was measured prior to applying inclusion/exclusion criteria because participants who dropped out did not complete all necessary measures (e.g., attention checks) prior to dropout.

***Attention Checks*.** To ensure that participants were reading the passages closely, we included four attention checks on their content (e.g., "What was the last passage about? A description of a girl / a pirate ship / how cars are manufactured"). We also included three

attention checks within the Likert-type response scales (e.g., "If you're actually reading this question, please select the number 3 as your response. Thank you for reading all the questions carefully"). These attention checks were used as exclusion criteria in addition to the English fluency verifier (see Supplemental Materials) and a validity item ("Is there any reason you think that your data should not be used (this will not impact payment)?").

Analyses

All analyses were conducted in R (R Core Team, 2019). Code for the analyses is available in the supplemental materials. We used Bayes Factors (BFs) as our inference criteria. BFs give relative evidence between two competing hypotheses. For all tests, we used a preregistered minimum BF value of 1/3 or 3 as a criterion for "substantial evidence" relative to the null or alternative hypothesis, respectively.

Preregistered Replication Tests. We preregistered five replication tests, each related to a previous effect observed in Bellet et al. (2018). These analyses initially used linear regressions with trigger warning condition (trigger warnings versus no trigger warning) as the primary independent variable. As indicated in the preregistration, we first examined whether demographic or psychiatric history differed by condition. If this were the case, we added those variables as covariates in regression analyses. Our two competing hypotheses relevant for BFs were that (1) the observed effect was equal to zero ($t_{obs} = 0$) or that (2) the observed effect was equal to the effect in the previous study by Bellet et al. ($t_{obs} = t_{orig}$). Replication BFs were computed following the *t*-value comparison procedure described by Verhagen and Wagenmakers (2014). Following this procedure, we tested the replication of the effect of trigger warnings on (1) participants' perceptions of their own posttraumatic vulnerability via the PPVS-S, (2) participants' perceptions on others' posttraumatic vulnerability via the PPVS-O, (3) immediate

anxiety response following markedly distressing passages, (4) subsequent anxiety response to mildly distressing passages presented without a trigger warning, and (5) an interaction effect between trigger warning condition and the WCHS on immediate anxiety response (including a simple slopes analysis if the interaction was significant).

Trauma Survivor-Specific Preregistered Tests. We preregistered several additional tests to answer specific questions about trauma survivors. For these tests, our two competing hypotheses were (1) the observed effect was equal to zero ($t_{obs} = 0$) or that (2) the observed effect was not equal to zero ($t_{obs} \neq 0$). Specifically, this is done by comparing a linear model which includes the parameter of interest (e.g., condition) against a linear model without that parameter (e.g., intercept only model) using the *lmBF* function in the *BayesFactor* package (Morey & Rouder, 2018). First, we tested whether trigger warnings affected participants' ratings of trauma centrality on the CES. Second, we tested whether PTSD severity scores on the PCL moderated any of the previous tests (e.g., effect on PPVS-S, PPVS-O, etc.). Third, we tested whether participants' self-reported prior exposure to trigger warnings (see TWAA) moderated any of the previous tests.

Exploratory Tests. Based on critiques we received of our past work, we were interested in whether the effect of trigger warnings differed in specific subgroups of our sample. It may be that trigger warnings are not helpful for trauma survivors broadly but are indeed helpful for those who have severe PTSD symptoms or have been diagnosed with PTSD. Accordingly, we tested the effect of trigger warnings among the subgroup of individuals who (1) met a clinical cutoff for a probable PTSD diagnosis based on their PCL scores or (2) reported a past diagnosis of PTSD. Another possibility is that trigger warnings are only helpful when the content of the passage matches the traumatic experience of the survivor (i.e., the passage actually triggers remembrance

of the trauma). Therefore, we asked participants to identify "trauma-matching" passages, allowing for a direct test of this hypothesis. We selected only the individuals who specified trauma-matching passages and selected only the responses in reference to those specific passages. We then tested whether trigger warnings prior to these trauma-matching passages affected anxiety. In addition to testing trauma matching, we also tested whether the effect of trigger warnings on anxiety was moderated by the type of trauma reported by participants.

Behavioral Avoidance (Dropout). We assessed dropout by the counting the number of participants who were randomized to an experimental condition but did not finish the study. We calculated the percentage of total dropouts using for the denominator the number of post-randomized participants prior to applying inclusion/exclusion criteria.

Scale Convergent and Discriminant Validity. Some of the scales used in this study (PPVS-S, PPVS-O, WCHS, TWAA) were created by the authors of this manuscript. In addition to computing reliabilities, which are listed above, we sought to provide tests of convergent validity. Specifically, based on the hypothetical constructs measured, we predicted that the PPVS-S and PPVS-O would be strongly positively related to one another, positively related to PCL scores, and positively related to the CES. We also wanted to ensure that these measures would show discriminant validity, meaning that they did not relate to theoretically unrelated measures. Thus, we predicted that they would have a weak to null relationship to religiosity. We predicted that the WCHS would be positively related to the CES, PPVS-S, and TWAA-1, but have a weak to null relationship to religiosity. We predicted that TWAA-3 (prior exposure to trigger warnings, the only TWAA item used for this study's outcomes) would be related to younger age and more liberal political orientation. These validity analyses were included due to

external suggestion after the preprint was made available, and thus were not preregistered, but were prespecified prior to examining inter-scale relationships.

Results

Sample Characteristics

Our sample contained a majority of self-identified females (*n* = 239, 53%) with a significant minority of males (*n* = 208, 46%) and a small number of participants who specified another gender (*n* = 4, 1%). Participants had a mean age of 37 (*SD* = 11.2 years), and identified their race as Caucasian (*n* = 336, 75%), Black/African American (*n* = 39, 9%), Asian/Pacific Islander (*n* = 23, 5%), Hispanic (*n* = 23, 5%), Native American/Alaska Native (*n* = 5, 1%), or multi-racial/selected multiple categories (*n* = 25, 6%). A substantial minority of participants identified their ethnicity as Hispanic (*n* = 41, 9%). Participants identified as not religious (*n* = 201, 45%), somewhat religious (*n* = 72, 16%), moderately religious (*n* = 79, 18%), very religious (*n* = 67, 15%), or extremely religious (*n* = 32, 7%). A minority of participants identified themselves as full-time undergraduate students (n = 44, 10%). Participants were skewed slightly toward liberal political orientation (*mean* = 2.64; 1 = *very liberal* to 5 = *very conservative*). Participants reported a wide diversity of traumatic experiences on the LEC-5. All 16 categories were represented, with the largest categories being natural disaster (*n* = 95, 21%[17]), transportation accident (*n* = 79, 18%), sexual assault (*n* = 78, 17%), and physical assault (*n* = 47, 10%). Only a small minority of participants selected the ambiguous category "Any other very stressful event or experience" (*n* = 13, 3%).

Preregistered Replication Tests

[17] Due to the initial screening process, all participants in this study endorsed exposure to at least 1 Criterion A event

The results of the replication tests appear in Figure 5.1. Overall, replication tests either favored the null hypothesis or gave ambiguous evidence. In the original study by Bellet et al. (2018), a significant effect was found by trigger warning condition on perceived vulnerability to self (PPVS-S) and perceived vulnerability of others (PPVS-O). Neither of these significant effects replicated in our sample, with substantial evidence favoring the null hypothesis for an effect on perceived vulnerability of others (PPVS-O). A significant interaction effect was also found in the original experiment, such that participants' belief that words can harm (WCHS) moderated the effect of trigger warnings on immediate increases in anxiety. This interaction effect did not replicate in our sample, with substantial evidence favoring the null hypothesis. For immediate increases in anxiety or sensitization to anxiety (which were nonsignificant in the original study), we found ambiguous evidence and substantial evidence favoring the null hypothesis, respectively.

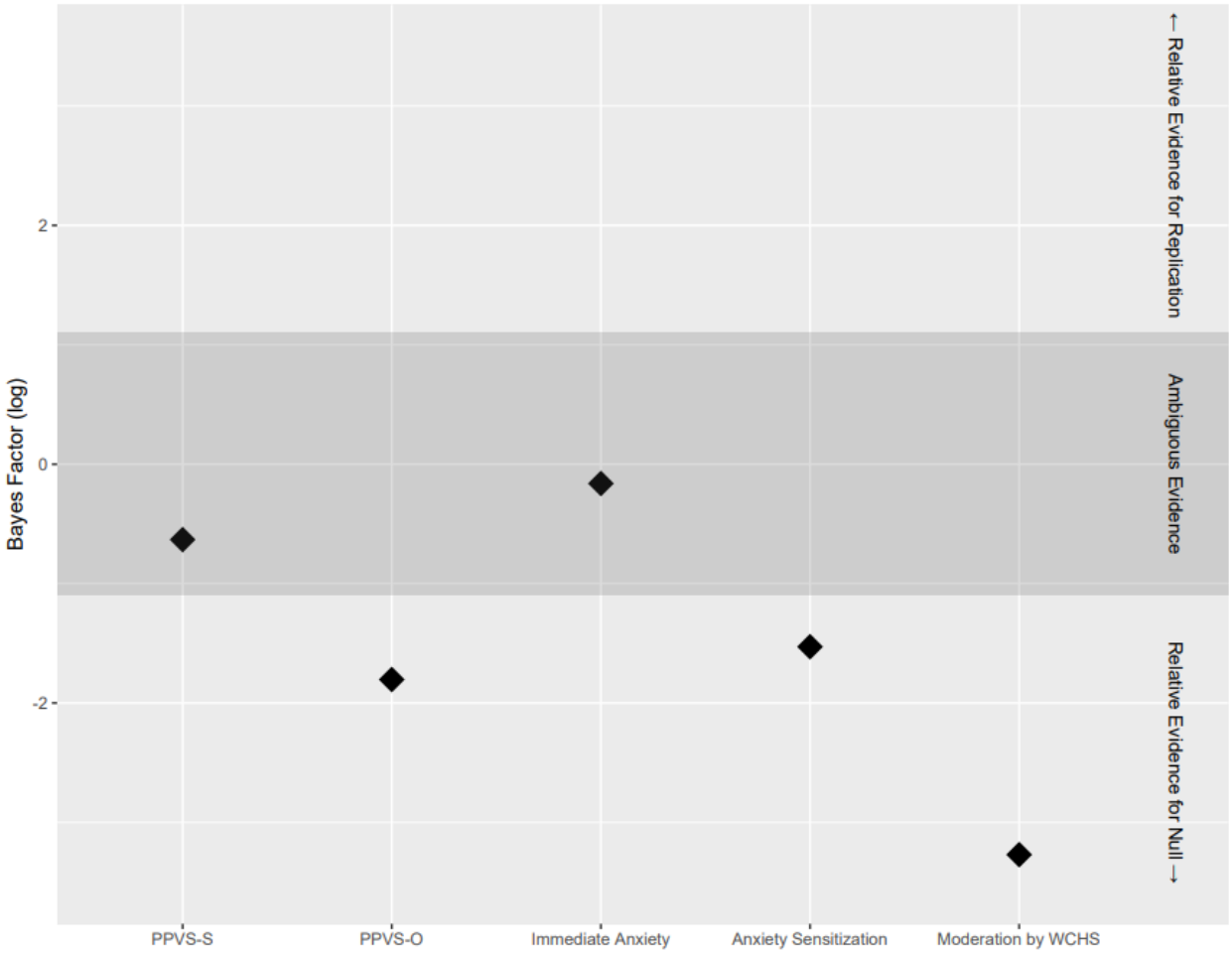


Figure 5.1. Replication Tests

Bayes Factors (log) are presented representing relative evidence for either the null hypothesis ($t_{obs} = 0$) or relative evidence for a hypothesis of equivalence with the effect from the original study ($t_{obs} = t_{orig}$). Overall, evidence was either ambiguous or favored the null hypothesis.

Trauma-Specific Preregistered Tests

First, we tested whether trigger warnings affected participants' ratings of trauma centrality on the CES. We found substantial evidence that trigger warnings increased the degree to which participants viewed their worst event as central to their life narrative (*BF* = 3.26, *d* = 0.25 [0.07, 0.44]).

Second, we tested whether PTSD severity scores on the PCL moderated any of the previous tests (e.g., effect on PPVS-S, PPVS-O, etc.). We found substantial evidence favoring the null hypothesis for a moderation effect on trauma centrality (*BF* = 0.11, $\Delta r^2 < 0.01$) and on perceived vulnerability (self or other; *BFs* = 0.10, 0.13, $\Delta r^2 < 0.01$, 0.01). We found ambiguous evidence for a moderation effect of PTSD severity on anxiety sensitization (BF = 0.86, $\Delta r^2 = 0.01$). We found substantial evidence that PTSD severity moderates immediate anxiety reactions (BF = 3.14, $\Delta r^2 = 0.01$). That is, individuals who scored higher on the PCL had *increased* anxiety when they were given trigger warnings.

Third, we tested whether participants' self-reported prior exposure to trigger warnings (see TWAA) moderated any of the previous tests. We found substantial evidence favoring the null hypothesis for a moderation effect on trauma centrality (*BF* = 0.27, $\Delta r^2 < 0.00$), perceived vulnerability (self or other; *BFs* = 0.19, 0.22, $\Delta r^2 < 0.00$), and anxiety sensitization (*BF* = 0.21, $\Delta r^2 < 0.00$). We found ambiguous evidence for a moderation effect on immediate anxiety reaction (*BF* = 2.17, $\Delta r^2 = 0.01$).

Exploratory Tests

Critics of recent trigger warning research have suggested the plausible hypothesis that whereas trigger warnings may not be helpful for college students generally (e.g., Bellet et al., 2019) or even for trauma survivors generally, they may be helpful for more specific

subpopulations. For instance, it is possible that trigger warnings are only helpful for (1) individuals with clinical-level PTSD symptoms or (2) individuals who have received a diagnosis of PTSD. Furthermore, it may be that trigger warnings are only helpful when (3) the content of the literature passage directly matches the content of their trauma (i.e., it triggers a remembrance of the trauma). We tested each of these hypotheses in exploratory tests. The results of these tests are presented in Figure 5.2.

Full Sample. When comparing the null hypothesis ($t_{obs} = 0$) to an open alternative hypothesis ($t_{obs} \neq 0$) , the full sample showed substantial evidence favoring the null hypothesis (*BF* = 0.14, *d* = 0.08 [-0.11, 0.26], *n* = 451). In other words, trigger warnings did not appear to affect immediate anxiety reactions in our full sample.

Clinical Cutoff. When examining only individuals who met the cutoff of 33 on the PCL for a probable diagnosis of PTSD recommended by the United States Department of Veterans Affairs (Weathers et al., 2013), we found substantial evidence favoring the alternative hypothesis (*BF* = 3.86, *d* = 0.43 [0.10, 0.76], *n* = 150). Among these individuals, trigger warnings increased immediate anxiety reactions. This is consistent with our preregistered test suggesting that PTSD severity scores moderated the effect of trigger warnings on anxiety reactions.

Self-Reported Diagnosis of PTSD. For individuals who self-reported receiving a past diagnosis of PTSD, we found substantial evidence favoring the null hypothesis (*BF* = 0.32, *d* = -0.17 [-0.76, 0.42], *n* = 53). That is, trigger warnings did not affect anxiety reactions for individuals who reported a diagnosis of PTSD.

Matching Trauma Passages. We asked individuals whether the passages reminded them of their worst event. If they answered "yes," we asked them to use a checklist to identify specifically which passages reminded them of their worst event. Examining only the individuals

who reported passages that reminded of them of their worst event, and examining only the relevant passages, we found ambiguous evidence (BF = 0.88, d = 0.33 [-0.02, 0.68], n = 133) for an effect of trigger warnings on anxiety. The effect was in the direction of increasing anxiety. That is, individuals who saw trigger warnings for relevant passages had trivially increased anxiety, suggesting that trigger warnings did not reduce anxiety reactions when passages matched past traumatic experiences.

Trauma Type. We used the LEC-5 to assess the type of trauma that best characterized each individual's worst event. Using the 16 categories from the LEC-5, we tested whether the type of trauma impacted the effect of trigger warnings. We found substantial evidence favoring the null hypothesis (BF < 0.001, Δr^2 = 0.02, n = 451). However, some of the 16 categories had very few observations, limiting the statistical validity of the test. Therefore, we tested for the influence of trauma type by condensing the LEC-5 categories into 5 broad groups: sexual violence (n = 107), other interpersonal violence (n = 74), accidental injury or illness (n = 146), natural or other disaster (n = 107), and other (n = 17). Using these categories, we again found substantial evidence favoring the null hypothesis (BF = 0.004, Δr^2 < 0.01, n = 451). That is, the type of trauma did not moderate the effect of trigger warnings.

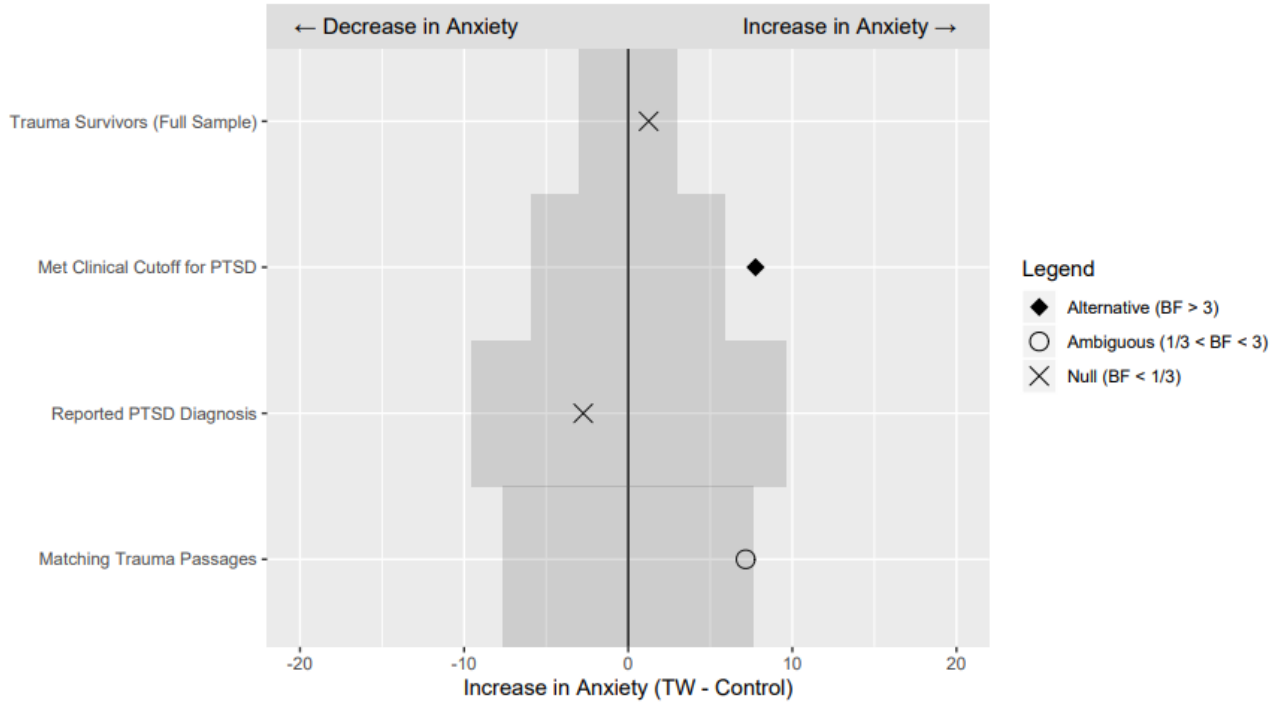


Figure 5.2. Trigger Warnings' Effect on Anxiety in Vulnerable Groups

Mean difference in anxiety change between the trigger warnings condition and the control condition across subgroups. Shapes of points on the figure correspond to a Bayesian comparison of the null hypothesis ($t_{obs} = 0$) and an alternative hypothesis ($t_{obs} \neq 0$). The shaded region corresponds to the boundaries of frequentist critical regions ($p < 0.05$, two-sided).

Behavioral Avoidance (Dropout)

In the trigger warnings condition, one individual (0.3% of the unscreened sample, n = 304) dropped out of the study. One individual also dropped out in the control condition (0.3%, n = 303). This suggests that individuals did not use trigger warnings to avoid potential trauma cues. The number of overall dropouts regardless of condition was very small. This is notable given that 33% of our sample met the clinical cutoff for PTSD symptoms and 29% reported that at least one literature passage reminded them of their worst event.

Other Emotions

Although self-reported anxiety was our primary outcome measure, we collected self-reports of various emotions to reduce demand characteristics ([anxious], afraid, angry, disgusted, sad, content, and happy). We analyzed these secondary emotions to see if trigger warnings had an immediate effect on any of them. We found Bayes Factors favoring the null hypothesis for anxious, afraid, sad, content, and happy (*BFs* = 0.14, 0.25, 0.25, 0.29, 0.23). For angry and disgusted, we found ambiguous evidence (*BFs* = 0.92, 0.99). For both angry and disgusted, the direction of this effect was such that trigger warnings trivially increased anger and disgust. The results of this analysis appear in a supplemental figure.

Scale Convergent and Discriminant Validity

As predicted, the PPVS-S and PPVS-O were strongly positively related to one another (r = 0.70), and were each positively related to the PCL (r = 0.59, 0.40) and the CES (r = 0.40, 0.28) but not to religiosity (r = -0.11, -0.04). The WCHS was positively related to the CES (r = 0.29), PPVS-S (r = 0.48), and TWAA-1 (r = 0.46), but not to religiosity (r = -0.02). Prior exposure to trigger warnings as measured by the TWAA-3 was only weakly related to younger

age (r = -0.17) and very weakly related to more liberal political orientation (r = -0.07). A full correlation matrix of these variables appears in the supplemental materials.

Discussion

Past research has indicated that trigger warnings are unhelpful in reducing anxiety. The results of this study are consistent with that conclusion. This study was the first to focus on how trigger warnings function in a sample of people who had survived Criterion A trauma as defined by the DSM-5 (APA, 2013). Trigger warnings did not reduce anxiety for this sample broadly. Trigger warnings also did not reduce anxiety among those who met a clinical cutoff for PTSD symptoms, reported a diagnosis of PTSD, or those who reported that the stimuli matched the content of their past trauma. Trigger warnings showed trivially small effects on response anxiety overall. When effects did emerge, they tended towards small *increases* in anxiety rather than decreases.

Bellet et al. (2018) previously found that trigger warnings increased individuals' projections of their own vulnerability to future trauma, as well as the vulnerability of others. Our results suggested substantial evidence that these effects did not replicate. Bellet et al. (2018) also reported that individuals who endorsed the belief that words are emotionally harmful showed greater anxiety in response to trigger warnings compared to individuals who did not endorse that belief. Again, we found substantial evidence that this effect did not replicate. One possibility is that these effects were unique to the trigger-warning naïve (trauma-naïve), crowd-sourced, older sample used by Bellet et al. (2018). However, given that these effects originally had a small effect size and did not replicate in larger samples of college students (Bellet et al., 2019) or trauma survivors (present study), the original results may have been a false positive.

We found substantial evidence that giving trigger warnings to trauma survivors caused them to view trauma as more central to their life narrative. This effect is a reason for worry. Some trigger warnings explicitly suggest that trauma survivors are uniquely vulnerable (e.g., " …especially in those with a history of trauma"). Even when trigger warnings only mention content, the implicit message that trauma survivors are vulnerable remains (why else provide a warning?). These messages may reinforce the notion that trauma is invariably a watershed event that causes permanent psychological change. In reality, a majority of trauma survivors are resilient, experiencing little if any lasting psychological changes due to their experience (Bonanno, 2004; Bonanno & Mancini, 2008). Aggregated across various types of trauma, only 4% of potentially traumatic events result in PTSD (Liu et al., 2017)[18]. However, trauma survivors who view their traumatic experience as central to their life have elevated PTSD symptoms (Berntsen & Rubin, 2006; Brown et., 2010; Robinaugh & McNally, 2011). Trauma centrality prospectively predicts elevated PTSD symptoms, whereas the reverse is not true (Boals & Ruggero, 2016). Decreases in trauma centrality mediated therapy outcomes (Boals & Murrell, 2016). This suggests that increasing trauma centrality is directly countertherapeutic. In other words, trigger warnings may harm survivors by increasing trauma centrality.

We tested whether the severity of PTSD symptoms in our sample moderated any of our tested hypotheses. In most cases, we found either evidence for no moderation or ambiguous evidence. However, we did find substantial evidence that PTSD symptoms moderated the effect of trigger warnings on response anxiety. For individuals who had more severe PTSD, trigger warnings increased anxiety. This effect is ironic in the sense that trigger warnings may be most

[18]Note that this rate is per event, not per person. Many individuals experience multiple traumatic events. The rate of PTSD varies depending on the type of traumatic event (e.g., interpersonal violence versus natural disaster; Liu et al., 2017).

harmful for the individuals they were designed to protect. We found no evidence that individuals' prior exposure to trigger warnings moderated any of the previous effects.

A limitation of past research was that trigger warnings were primarily tested among individuals who were trauma-naïve or in mixed samples. That is, the possibility remained that despite being unhelpful for most who view them, trigger warnings may have been helpful for trauma survivors or individuals with PTSD. In this study, we find no evidence supporting this possibility. Trigger warnings were not helpful for trauma survivors. For individuals who met a clinical cutoff for severity of PTSD symptoms, trigger warnings slightly *increased* anxiety. Trigger warnings were not helpful for individuals who self-reported a diagnosis of PTSD. Perhaps most convincingly, trigger warnings were not helpful even when they warned about content that closely matched survivors' traumas. That is, when considering only the passages which participants reported as reminding them of past trauma, trigger warnings were still unhelpful.

Although the research base on trigger warnings has grown quickly, several constraints on generality regarding trigger warnings still remain. For example, experiments thus far have tested trigger warnings prior to short-term stimuli, such as literature passages, film clips, and photos. Studies have only tested a limited range of negative psychological outcomes (e.g., self-report anxiety, negative affect, intrusive memory). Our study provides important information about individuals who have suffered from trauma, many of whom met the clinical threshold for PTSD symptoms. However, it does not provide information about individuals diagnosed with PTSD via clinical interview. It is unclear whether our findings (especially concerning evidence of potential harms) would apply to extended classroom discussions or other situations of greater temporal duration. In addition, whether the potentially negative effects of trigger warnings found thus far

have more than short-term adverse effects remains uncertain. Nevertheless, these potential constraints on generality do not imply that trigger warnings are *helpful*. Rather, they imply that potential moderators remain untested.

Public arguments regarding trigger warnings have been politically charged, complex, and data-poor. Recent research on trigger warnings can importantly inform or perhaps even settle some of these debates. The research suggests that trigger warnings are unhelpful for trauma survivors, college students, trauma-naïve individuals, and mixed groups of participants (Bellet et al., 2018; Bellet et al., 2019, Bridgland et al., 2019; Sanson et al., 2019). Given this consistent conclusion, we find no evidence-based reason for educators, administrators, or clinicians to use trigger warnings.

Whether trigger warnings are explicitly harmful is less clear. We found evidence that trigger warnings increase the narrative centrality of trauma among survivors, which is countertherapeutic (Boals & Murrell, 2016). We also found that trigger warnings increase anxiety for those with more severe symptoms of PTSD. Although these effects were preregistered and found in a large sample, the size of the effects were small and have not yet been rigorously tested across multiple studies. However, such knowledge is unnecessary to adjudicate whether to use trigger warnings – if there is no good reason to deploy them in the first place, we need not require strong evidence of harm before abandoning them. Trigger warnings should serve as an important caution to both clinical and nonclinical professionals who use interventions aimed to improve well-being among trauma survivors. Such practices should be thoroughly vetted via appropriate scientific techniques before they are adopted. Using unvetted interventions is irresponsible to victims of trauma.

Chapter 6—

General Discussion

The hedonic treadmill describes how changes in life circumstances lead to less-than-expected (though greater than zero) impacts on human well-being. Humans habituate quickly to most changes, returning close to baseline happiness. Yet well-being and ill-being are not simply two sides of the same coin (Rubin & Talarico, 2009). Does human misery follow a similar pattern in a parallel neurotic treadmill? We can examine this question as it relates to PTSD, a form of misery closely tied to specific negative events. From a theoretical standpoint, the idea of a neurotic treadmill for PTSD is plausible. First, falling rates of violence may impact personal expectations and definitions of trauma and PTSD. Subsequently, those changes in personal expectations and definitions of trauma and PTSD may impact PTSD etiology (i.e., increase vulnerability) through short-term and long-term appraisals of events. Second-order effects such as moral evaluations of trauma and PTSD may also come into play.

Is the neurotic treadmill plausible from an epidemiological perspective? Rates of PTSD do not appear to track with rates of dangerous and distressing life events such as interpersonal violence, rape, and sexual assault. This asynchrony seems to exist both temporally within the United States (rates of violence have decreased within the United States, but PTSD rates have not) and across nations (nations with the highest rates of violence do not necessarily have the highest rates of PTSD). Although there are many alternative explanations that might explain the effects, they are at least consistent with the possibility of a neurotic treadmill.

This dissertation investigates the possibility that as violence in the world decreases, vulnerabilities to PTSD increase. For every step taken forward in reducing rates of adverse events, a half-step is taken backwards in terms of vulnerabilities to these events. The precise nature of this vulnerability was the object of exploration in each dissertation chapter. Specifically, I investigated whether broadening of personal definitions of trauma (i.e., viewing

milder and milder events as potential traumas) could (1) plausibly occur as a function of decreased direct or indirect exposure to serious events and (2) plausibly increase vulnerability to event-related distress. I then investigated whether (3) institutional policies designed to increase emotional safety (a potential second-order sociocultural mechanism) could increase vulnerability.

Summary of Main Findings

Key findings from each of the three papers provide insight into a potential neurotic treadmill effect. In Paper 1, I found that gradually reducing the prevalence of serious adverse events (but still displaying a small proportion of those events) did not result in expanding concepts of trauma over the course of a short experiment. Instead, participants in both conditions seemed to become *stricter* about the definition of trauma as they rated more and more events. In a second experiment in Paper 1, this result became clearer. When participants were shown only serious events, they quickly became much stricter about the definition of trauma. When participants were shown only nonserious events, they became broader about the definition of trauma. We thus hypothesize that in the first experiment, participants became stricter because they were exposed to more serious events than those that would typically come to mind. In summary, over the course of a short experiment I found no effect of decreasing the prevalence of serious events, but I did find an effect of shifting the total range of events.

In Paper 2, I investigated whether broad or narrow beliefs about the definition of trauma impact emotional reactions to stressful events. I found that individuals with broader definitions of trauma experienced more negative emotions after viewing a stressful film clip and were more likely to report viewing the clip as a personal trauma. Those who viewed the film clip as a personal trauma also reported increased event-related distress several days following. The

experimental portion of the study, in contrast, did not provide support for a causal effect. That is, my experimental manipulation had an effect on trauma beliefs, but did not result in increased negative emotions or event-related distress. Therefore, broad trauma beliefs predict vulnerability, but it is difficult to tell whether they cause vulnerability or whether both beliefs and vulnerability are due to some other factor.

In Paper 3, I investigated beliefs about trauma and vulnerability in an applied context. That is, is it possible that institutional policies designed to promote emotional safety actually increase vulnerability? I investigated the effect of providing trigger warnings to trauma survivors prior to reading potentially disturbing literature passages. I find that overall, trigger warnings given to trauma survivors do not prevent distress and in some cases very slightly increase distress. Those who received trigger warnings reported just as much (or more) anxiety in response to reading potentially disturbing literature passages compared to those who did not receive trigger warnings. This remained true for those who self-reported a past PTSD diagnosis or qualified for probable PTSD based on a symptom measure. It was also true when the passages' content matched survivors' worst event (i.e., when the passage actually triggered reminders of the event). Relevant to beliefs about trauma, I found that providing trigger warnings increased the degree to which participants saw their worst event as centrally defining of their life narrative. In contrast, providing trigger warnings had no impact on perceptions of personal vulnerability and did not increase anxiety for later passages given without a warning. Overall, trigger warnings seem mostly inert, with small hints of potential harms.

Future Directions

In summary, the evidence suggests that expansions in personal definitions of trauma are a plausible mechanism for increasing vulnerability. This area merits additional explanation.

However, taking the evidence in sum, it seems that expansions in personal definitions of trauma are unlikely to provide a *complete* explanation for a neurotic treadmill effect. Although Papers 1 and 2 revealed effects that are consistent with a treadmill for personal definitions of trauma, the effect sizes ranged from small to moderate and there was limited support for causality.

In Paper 1, limiting participants' exposure to only nonserious events did broaden personal definitions of trauma as measured by trauma ratings. However, limiting exposure to *only* nonserious events does not have a realistic practical analogue. Even in the safest of societies, individuals inevitably learn about or indirectly experience serious adverse events. Perhaps the effect might extend to a situation in which individuals are occasionally exposed to serious events, but the punctuated exposures are greatly spread out across time. If the narrowing effect of being reminded of serious events has a short temporal duration, individuals in such a situation could still spend most of their time with relatively broad personal definitions of trauma. That is, completely limiting exposure to only nonserious events *during one's lifetime* does not have a practical analogue, but limiting exposure to nonserious events *during most days, weeks, or months* might have one. This possibility merits investigation in future studies.

The experiments in Paper 1 also fail to distinguish between different types of exposures (e.g., learning about an event on the news, learning about an event happening to an acquaintance, witnessing an event, or experiencing it oneself). One might even argue that in the modern world, indirect exposure has even *increased* through the ready availability of news media. An auxiliary hypothesis might suggest that indirect exposures have little effect on trauma concepts compared to more direct exposures (e.g., experiencing a serious adverse event personally, or a close friend or family member experiencing it). Nevertheless, the Paper 1 experiments themselves use indirect exposures (descriptions of events), and the data suggest that even indirect exposures

should narrow trauma concepts. In summary, the evidence from Paper 1 does not cleanly support a strong version of the hypothesis that gradual reductions in exposure to serious events can fully explain modern expansions in the trauma concept.

Similarly, Paper 2 suggests limitations to the idea that broadened trauma concepts are fully responsible for increases in vulnerability to adverse events. A correlational effect indicated that broadened trauma concepts predicted emotional vulnerability. However, a causal effect was not supported: although a brief intervention was able to shift trauma concepts to a moderate degree, the intervention did not result in significant changes in emotional vulnerability. It is possible that there is a true causal effect that was simply not observed due to inadequacies of the experiment (i.e., a false negative). The correlational effect suggests the need for future research to explain the precise nature of the link between broadened trauma concepts and vulnerability if a direct causal link is not present. However, even though we cannot completely rule out a false negative, the nonsignificant point estimate for a causal effect gives us valuable information. For example, the cross-national PTSD-vulnerability relationship in epidemiological data is large ($\Delta R^2 = 0.42$; Dückers et al., 2016, interaction model compared to exposure only) and significant with a sample size of 16, whereas the Paper 2 experimental effect was small ($f^2 \leq 0.01$) and nonsignificant with a sample size of 293. The absence of a comparably large finding in a single study should not rule out the mechanism, but it should temper optimism that such a mechanism can fully explain the larger effect of discrepant rates of PTSD and violence. Paper 2 indicates that trauma concepts are relevant to emotional vulnerability, but causality may be more complex than initially imagined, and there are likely other factors at play.

Likewise, evidence regarding an increase in vulnerability resulting from protective social policies (in this case, trigger warnings) remains limited. In Paper 3, trigger warnings caused a

slight increase in the extent to which trauma was viewed as central to one's life. They did not increase perceptions of personal vulnerability. Although this is only a very preliminary glimpse into policies aimed at emotional safety, it seems unlikely that policies with a relatively limited range of implementation are sufficient to explain such a broad trend in rates of event-related distress. Once again, this is not to say that protective policies and culture are irrelevant, but simply that the current evidence is insufficient to suggest they are the unitary culprit.

If the mechanisms tested in this dissertation cannot fully explain the discrepant rates of violence and event-related distress, what can? It seems we are quite far from any satisfying answer, but there are numerous directions for future research. I briefly outline some areas for potential future exploration.

Increased Awareness of Mental Illness and Changes in Stigma

It is possible that the 'true' rates of PTSD in the past (and in less-developed countries) are obscured because of lack of awareness and stigma, distorting epidemiological measurement. That is, measurements of PTSD incidence and prevalence may underestimate the ground truth in societies where individuals feel less comfortable disclosing symptoms of mental illness. The issue is somewhat complex because stigma has not uniformly decreased in the modern world, and some forms of stigma have even increased (Schomerus & Angermeyer, 2017).

That said, at least some forms of mental health stigma are apparently decreasing, especially when it comes to willingness to seek care. There is good reason to believe that the 21st century 'mental health crisis' writ large is least partially attributable to increases in awareness or decreased stigma against receiving help, which may increase treatment seeking. This is an especially important point because demand for mental health services have risen much more sharply than empirical rates of diagnosed disorders. Using the United States as an example,

although rates of depression in epidemiological studies seem to have increased very slightly (Institute for Health Metrics and Evaluation [IMHE], 2018), rates of treatment seeking have increased much more dramatically (Hockenberry, Joski, Yarbrough, & Druss, 2019; Center for Collegiate Mental Health [CCMH], 2020). The case of college counseling centers provides an illuminating example: depression severity among treatment seekers increased by 13.8% from 2010-2019, whereas the overall number of treatment seekers per institution increased by 49.7% (CCMH, 2020).

This concern applies slightly less to epidemiological studies of PTSD. Such studies typically use structured clinical interviews that ask specific questions about symptoms. This helps avoid the issue of measurement error due to lack of mental health awareness and partially reduces concerns about underestimation (Corrigan, 2017). That is not to say that it is not a major concern: mental health symptoms may certainly still be underreported in certain cultures. Yet the problem is likely less dramatic in epidemiological settings compared to examinations of convenience samples of treatment-seekers or rates of self-diagnoses.

It is still possible that stigma and awareness affect even epidemiological PTSD incidence and prevalence estimates through various covert mechanisms. Although diagnoses typically carry more stigma than specific symptoms, individuals may still be reluctant to disclose certain symptoms they perceive as conveying emotional weakness. Increased treatment seeking could also indirectly affect epidemiological studies. For example, if someone seeks treatment for PTSD, they may be more familiar with the typical symptoms of PTSD and how those symptoms are typically measured in clinical interviews. They may recognize PTSD symptoms that they did not formerly realize they exhibited (for example, recognizing avoidance behaviors or feeling

emotionally numb). If they are later included in an epidemiological study, they may thus answer affirmatively more often than those who have PTSD but have less knowledge about the disorder.

These concerns are relevant to studies on PTSD rates, especially when those rates are based on treatment-seeking samples, when sampling biases are likely, or when diagnoses arise from self-report rather than structured interviews or questionnaires. While this area merits further exploration, it is also unlikely to provide a thoroughly satisfying explanation for the epidemiological data. If these concerns are sufficient to explain the large statistical effects in PTSD rates within and across countries, this would indicate a major need to revisit the validity of PTSD diagnostic measurement and would threaten the conclusions of a vast array of PTSD studies.

Social and Monetary (Dis)incentives

A highly controversial but relevant hypothesis regards potential incentives and disincentives for (1) malingering, (2) unintentionally reporting factitious or exaggerated symptoms of PTSD due to social or moral incentives, (3) characterizing symptoms of other mental disorders (e.g., depression) as PTSD, and (4) engaging or not engaging in behaviors relevant to PTSD development and maintenance. When it comes to symptom reporting, it is important to distinguish between those who are fully aware they do not suffer from PTSD symptoms but nevertheless report symptoms (i.e., malingering), and those who exaggerate the intensity of symptoms without any conscious awareness of doing so (i.e., factitious PTSD). Because PTSD is uniquely tied to a specific external event, there may also be incentives to characterize one's distress as PTSD rather than another emotional disorder.

Legal and Monetary Incentives. Research on malingering and factitious PTSD primarily centers around legal and monetary contexts in which a diagnosis of PTSD would

influence outcomes. In these contexts, malingering appears disturbingly common, estimated to occur in at least 20-30% in personal injury cases and in at least 20% of compensation-seeking combat veterans (Taylor, Frueh, & Asmundson, 2007). Symptom exaggeration appears to be an especially problematic issue in the Veterans Affairs (VA) system, where PTSD status and severity is closely tied to service-connected disability compensation. Among veterans seeking treatment, one study found that 77% showed at least some signs of symptom exaggeration on a standardized forensic interview, with 53% exhibiting clear symptom exaggeration according to standardized criteria (Freeman, Powell, & Kimbrell, 2008). A systematic investigation by the VA Office of the Inspector General (VAOIG) found that among veterans who received less than 100% compensation, the modal patient reported gradually getting worse over time until they reached the 100% compensation mark, whereupon mental health visits plummeted by 82% (VAOIG, 2005; McNally & Frueh, 2013). Many veterans face dire financial situations as well as significant psychological adjustments when returning from war; seeking help for PTSD & associated compensation (even if PTSD is not the precise issue at hand) is way in which veterans can meet both needs (McNally & Frueh, 2013).

Detecting symptom exaggeration is non-trivial: Fox & Vincent (2020) note that "the symptoms of PTSD can be believably feigned regardless of the veracity of their existence, and regardless of depth of psychological knowledge, direct coaching, or advance practice." In one study, 98.9% of naïve participants could meet the criteria for PTSD on a self-report measure without coaching (Lees-Haley & Dunn, 1994). Fortunately, it appears that outright falsification is less of a concern in epidemiological studies, where there is not a clear monetary or legal incentive to lie (Dohrenwend et al., 20016; McNally & Frueh, 2012).

Reputational and Moral Incentives. Exaggerated symptoms following trauma are not necessarily limited to contexts with a clear monetary incentive. It is possible that smaller and more nuanced incentives might also give rise to symptom exaggeration. In a globalized world where it is easy to feel lost in the crowd, expressing distress is one way to gain sympathy, compassion, and attention from loved ones and others. Indeed, there are various reasons (aside from monetary incentives) that one might exaggerate PTSD symptoms in particular: to garner sympathy, to stir up feelings of condemnation towards a perpetrator or group of perpetrators, to gain a higher moral or social status, or to shield oneself from criticism. If one sees the world as divided into victims and oppressors (Lukianoff & Haidt, 2018), from a moral standpoint it is surely better to be in the former group.

The moral and reputational advantage of victimhood is central to a sociological thesis recently forwarded by Campbell & Manning (2018). Many sociologists have studied *honor cultures,* in which individuals' reputations for physical strength, aggression, and bravery are critical to their moral worthiness. Those unwilling to readily defend their honor are derided as weak or spineless. Honor cultures contrast with *dignity cultures*, which emphasize that all people have an inherent moral worth that cannot be taken away from them. Individuals in dignity cultures are expected to develop a quiet self-assuredness of their own value; aggressive reactions to verbal slights are typically viewed as shameful because they indicate a lack of self-confidence or self-restraint.

Campbell & Manning (2018) outline a third type of culture that they believe has recently arrived on the scene: *victimhood culture*. Whereas a dignity culture encourages individuals to ignore minor slights, victimhood culture takes them very seriously, especially when there is a power asymmetry between the offender and the offended. In contrast to honor cultures,

victimhood cultures frown upon direct retaliation to slights. Instead, individuals must appeal to a third party, such as a government or organization, which in turn punishes the offender. Victims and their allies are seen as morally respectable, and those with unearned and unacknowledged privileges are seen as repugnant.

The paradox of a victimhood culture is that there is no direct route to proving one's respectability. In an honor culture, one gains respectability through demonstrations of bravery and mettle. In a dignity culture, one gains respectability through developing and demonstrating self-control. Respectability in a victimhood culture, though, depends not on your own virtues, but on someone else's maltreatment towards you (or your shared identity with others who are maltreated). Increasing one's respectability is difficult and achieved primarily by portraying oneself as a member of a marginalized group or demonstrating one's allyship with such groups. Therefore, within such victimhood cultures there are strong social incentives to acquire and maintain the identity of a trauma survivor (among other identities).

Although Campbell & Manning's thesis seems highly applicable, it is important to remember that victimhood cultures are both new and rare. Western, developed nations (where we see unexpectedly high rates of PTSD) are primarily dominated by dignity cultures. Full-blown victimhood cultures are argued to exist primarily in contemporary college and university campuses, although some elements of these cultures may have permeated more broadly. While enhancing one's victimhood may be a path to increasing respectability in certain subpopulations, that is hardly the case when we take a broader look.

Characterizing Symptoms of Other Disorders as PTSD. PTSD symptoms are also unique in that they identify a specific cause of distress. Rather than conceptualize one's distress as a nebulous, cryptogenic anxiety or depression, some individuals may unintentionally prefer to

conceptualize their symptoms as the result of a trauma. This re-conceptualization of distress is not only more likely to garner sympathy and support, but also may help individuals to find meaning and purpose in their pain. Yet there is little research investigating symptom exaggeration of PTSD in these contexts. What little research does exist suggests that this issue needs a great deal more attention.

Disincentives and Voluntary Behavior. It should be noted that there are also clear *disincentive* structures that may inhibit symptom reporting. Reporting symptoms of PTSD may attract compassion from some, but stigma from others. As stigma against PTSD decreases, the balance of incentives and disincentives shifts. For those with PTSD who may have otherwise underreported symptoms or declined treatment, this is a very positive change. However, this change equally affects those with subthreshold PTSD or no PTSD: decreases in disincentives to report are also expected to result in corresponding increases in factitious symptoms. This is an area that merits increased attention.

In cases of traumatic stress, a person's voluntary behavior directly alters the average course of recovery. An example of a helpful voluntary behavior might include seeking social support, whereas less-helpful behaviors include avoidance of trauma cues, social avoidance, voluntary unemployment or reduced employment, and substance-use. Because these behaviors are all susceptible to reinforcement structures, it is possible that they could be influenced by potential incentives that could therefore speed up or slow down the average course of recovery in the general population. Incentives towards helpful behaviors and disincentives towards harmful behaviors could nudge the rate of PTSD.

Importantly, it may not be that unproductive behaviors such as avoidance or reduced employment are necessarily *incentivized* in modern developed nations, but that they were much

more heavily *disincentivized* in the past (and in developing nations). That is, an individual in 1980 may have been heavily disincentivized from taking an extended leave of absence from work due to PTSD symptoms. The steeper cost of unemployment may have prompted individuals towards behaviors that were ultimately therapeutic (though painful). This does not necessarily mean that harsher incentive structures are "better"; they may have reduced the overall prevalence of PTSD at the expense of incredible suffering among those with the most severe form of the syndrome (e.g., those unable to work regardless of incentive structures).

One limitation of this hypothesis is that seeking evidence-based care for posttraumatic stress is almost certainly more incentivized now than it was in the past and in highly developed compared to less-developed nations. Empirically supported treatments for PTSD are effective. That said, empirically supported treatment would not affect the *lifetime* rate of PTSD (barring retrospective reporting biases), and it is also quite rare (Harvey & Gumport, 2015; Kazdin & Blase, 2011). Overall, incentive structures have been explored very little in the empirical literature and merit a much closer look. Although there is a paucity of current evidence, incentive structures remain a plausible explanation for epidemiological discrepancies.

Conclusion

Epidemiological data on stressor-induced psychopathology is surprising because it does not appear to correspond closely to our dominant etiological models. That is, based on current etiological models, we should assume that general decreases in exposure to serious adverse events should lead to a corresponding decrease in PTSD prevalence in the population. This is not the case within the United States over time or across countries. In some cases, the reverse appears to be true. Assuming the epidemiology is not completely backwards, it follows that our etiological models are missing key aspects. Specifically, there may be vulnerabilities that

increase as a function of decreasing adversity in a neurotic treadmill. Across three papers, this dissertation found some evidence that expanding beliefs about trauma and harm may play a role in increasing vulnerability to stressors. However, the relatively small effects seem unlikely to fully explain the missing link. If psychiatrists and clinical psychologists aim to reduce rates of stressor-related mental illness in the general population, our current course of action may be inadequate and possibly even counterproductive. Rates of PTSD are highest in the countries that have worked the hardest to treat it. Issues of expanding definitions of trauma and harm, mental health awareness and stigma, potential (dis)incentive structures, and diverse other potential forms of expanding vulnerabilities should be closely investigated in future research.

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Appendix A. Grouping Screened Events Across Major Epidemiological Studies – Conditional Rates

Name in Table	NCS	NCS-R	NESARC-II	NESARC-III
Intimate Partner Violence	NA	Beaten up by spouse or romantic partner	Being beaten up by intimate partner	Victimization by intimate partner violence
Military Combat	Combat	Combat experience	Military combat	Active military combat
Serious Illness	NA	Life-threatening illness	Own serious or life-threatening illness	Serious or life-threatening illness
Kidnapped	NA	Kidnapped	Kidnapped or held hostage	Kidnapping or being held hostage
Threat with a Weapon	Threat with weapon	Mugged or threatened with a weapon	Mugged, held up, or threatened with a weapon	Being mugged, held up, threatened with weapon
Natural Disaster	Natural disaster	Natural disaster	Natural disaster	Natural disaster
Peacekeeper	NA	Relief worker in war zone	Peacekeeper	Service as peacekeeper, relief worker
Refugee	NA	Refugee	Refugee	Refugee status
Stalked	NA	Stalked	Stalked	Stalking

Appendix B. Grouping Worst Events Across Major Epidemiological Studies – Attributable Rates

Name in Table	ECA	NCS	NCS-R	NESARC-III
Natural Disaster	Natural disaster	Natural disaster	Natural disaster	Natural disaster
Combat	Combat	Combat	Combat experience	Active military combat Prisoner of war status
Rape / Sexual Assault	(Not reported)	Rape Molestation	Raped Sexually assaulted Stalked	Sexual abuse before age 18 Sexual assault in adulthood
Physical Assault	Physical attack	Physical attack Threat with weapon Physical abuse	Kidnapped Beaten up by caregiver Beaten up by spouse or romantic partner Beaten up by someone else Mugged or threatened with a weapon Purposely injured, tortured, or killed someone	Physical abuse before age 18 Victimization by intimate partner violence Physical assault by someone else Kidnapping or being held hostage Being mugged, held up, threatened with weapon
Serious Accident / Illness	Serious accident Threat or close call	Accident	Toxic chemical exposure Automobile accident Other life threatening accident Man-made disaster Life-threatening illness Accidentally caused serious injury or death	Serious or life-threatening injury Serious or life-threatening illness Injury in a terrorist attack

Witnessing or Learning About Adverse Events	Seeing someone hurt or die	Witness Shock (trauma to others)	Unexpected death of loved one Child with serious illness Traumatic event to loved one Witnessed death/dead body, or saw someone seriously hurt Saw atrocities Witnessed physical fight at home	Someone else's serious or life-threatening injury Someone else's serious or life-threatening illness Someone else's injury in a terrorist attack Someone else exposed to natural disaster Someone else's sexual abuse before age 18 Someone else's sexual assault in adulthood Someone else's physical abuse before age 18 Someone else's victimization by intimate partner violence Someone else's physical assault other than intimate partner violence Someone else being kidnapped or held hostage Someone else being stalked Someone else being mugged, held up, threatened with weapon Seeing a dead body or body parts Another traumatic event to someone else
All Other Trauma	Other trauma	Neglect Other trauma	Some other event Private event	Service as peacekeeper, relief worker Being a civilian in a war zone, place of terror

	Relief worker in war zone	Refugee status
	Civilian in war zone	Incarceration in juvenile detention or jail
	Civilian in region of terror	Another traumatic event
	Refugee	