

Traumatic Formations and Psychiatric Codifications:  
A Rhetorical History of Post-traumatic Stress Disorder

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ABSTRACT

Since it was first included in the American Psychiatric Association's (APA) *Diagnostic and Statistical Manual of Mental Disorders* (DSM) in 1980, post-traumatic stress disorder (PTSD) has become a medical and cultural phenomenon. Moreover, it has led to the belief that PTSD is a universal aspect of human experience. *Traumatic Formations and Psychiatric Codifications: A Rhetorical History of Post-traumatic Stress Disorder* challenges this view by examining the rhetorical processes by which PTSD and its predecessor diagnoses were codified. Using critical techniques taken from rhetorical studies, Science and Technology in Society studies, and historiography, this dissertation examines the social, medical, and institutional formations that created the need for psychological trauma to be codified as an actionable psychiatric diagnosis at four specific historical moments, beginning in Victorian England and culminating with the official codification of PTSD in 1980. By attending to the rhetorical processes of codifying unique post-traumatic illnesses over the course of 150 years, this dissertation argues that post-traumatic illnesses are better understood as dynamic entities that respond to specific social problems. Furthermore, it finds that the diagnoses themselves must conform to the constraints of their day as determined by the institutions (government, military, or disciplinary) that call upon psychiatric medicine to intervene in social problems.

*Traumatic Formations* presents four historical case studies: railway spine in Victorian England, shell shock in World War I, post-Vietnam syndrome in the 1970s, and PTSD in 1980. After introducing the project in the first chapter, Chapter 2 examines how British legal courts in the late nineteenth century called upon physicians to determine whether train accident survivors were entitled to monetary compensation for their psychological injuries. To make psychological trauma legible to legal courts, British physicians codified railway spine as a psychological effect of a physical injury, thus connecting victims' mental problems to the accidents they survived. Chapter 3 analyzes how the shell shock epidemic in World War I ushered in a shift in theoretical understandings of psychological trauma. When psychiatrists located near the frontlines of combat demonstrated that soldiers did not need to be exposed to exploding munitions to manifest the symptoms associated with shell shock, medical professionals and the British military came to understand shell shock as a psychological problem rather than a physical malady. Chapter 4 examines how a small group of antiwar psychiatrists advocated for military veterans who had trouble readjusting to civilian life after fighting in the Vietnam War. They persuaded the American public, the federal government, and mental health clinicians that the veterans' adjustment problems were the result of a new psychological illness called post-Vietnam syndrome. Chapter 5 analyzes how post-Vietnam syndrome became PTSD. In the process of convincing the APA to include PTSD in the 1980 edition of the DSM, many of the unique features of post-Vietnam syndrome were compromised so that the PTSD diagnosis could be applied to people who were traumatized by events other than war.

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GENERAL AUDIENCE ABSTRACT

*Traumatic Formations and Psychiatric Codifications: A Rhetorical History of PTSD* takes a historical case study approach to understanding post-traumatic stress disorder (PTSD) as a medical and cultural phenomenon. Working from the rhetorical tradition, as well as intersecting traditions of historiography, medical and scientific rhetorics, and Science and Technology in Society studies, it investigates how disciplinary medical science intervenes in social problems and codifies them as post-traumatic diagnoses in order to meet the needs of other institutions, like legal courts, the military, and the American Psychiatric Association. This project begins in Victorian England with railway spine, then examines shell shock in World War I, post-Vietnam syndrome in the 1970s, and culminates in a case study of PTSD in 1980. For each case study, I conduct a rhetorical analysis of a large corpus of documents, including archival medical sources, government documents, and cultural texts. By analyzing post-traumatic diagnoses over the course of 150 years, I demonstrate that psychiatric diagnoses are rhetorical repositories of historical social problems and institutional goals.

DEDICATION

For Ben,

STANDBAST

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## Chapter 1: Introduction

### **Introduction**

On August 14, 2014, military psychiatrist Charles Hoge and his colleagues at the Walter Reed Army Institute of Research published a startling report in *Lancet Psychiatry* demonstrating that the recently revised diagnostic criteria for post-traumatic stress disorder (PTSD) excluded thirty percent of military personnel previously diagnosed with the disorder. Equally astonishing, thirty percent of soldiers who were previously ineligible for PTSD now qualified for the diagnosis (Hoge et al., 2014, p. 5-6). The problem arose, ironically, from the American Psychiatric Association's (APA) "efforts ... directed toward the goal of enhancing the clinical usefulness" of the fifth edition of its *Diagnostic and Statistical Manual of Mental Disorders* (2013a), commonly referred to as DSM-5 (APA, 2013, p. 5). Over the course of twelve years, thirteen work groups evaluated the efficacy of DSM-5's predecessor, DSM-IV-TR (2000), with field trials, expert reviews, and summits until the psychiatrists reached a consensus on the contents of DSM-5. As a result, the work groups added new diagnoses, removed redundant ones, and revised the definitions of others (p. 7). PTSD was among those significantly revised.

The new codification of PTSD included a new, expanded definition of trauma and three additional symptoms, raising the total from seventeen symptoms in DSM-IV-TR to twenty symptoms in DSM-5. Many of the criteria that had remained unchanged since their initial inclusion in DSM-IV in 1990 were substantially reworded. For example, Criterion A, the definition of a traumatic event, in DSM-IV-TR stated:

- A. The person has been exposed to a traumatic event in which both of the following were present:

- (1) the person experienced, witnessed, or was confronted with an event or events that involved actual or threatened death or serious injury, or a threat to the physical integrity of self or others
- (2) the person's response involved intense fear, helplessness, or horror. (APA, 2000, p. 467)

Thirteen years later, the APA removed “fear, helplessness, and horror” from Criterion A and expanded the definition to reflect two new types of traumatization: indirect traumatization and traumatization through repetitive events. The current definition of post-traumatic stress disorder in DSM-5 is:

- A. Exposure to actual or threatened death, serious injury, or sexual violence in one (or more) of the following ways:
  1. Directly experiencing traumatic event(s).
  2. Witnessing, in person, the event(s) as it occurred to others.
  3. Learning that the traumatic event(s) occurred to a close family member or close friend. In cases of actual or threatened death of a family member or friend, the event(s) must have been violent or accidental.
  4. Experiencing repeated or extreme exposure to aversive details of the traumatic event(s) (e. g. first responders collecting human remains; police officers repeatedly exposed to details of child abuse). (APA, 2013a, p. 271)

The APA provided little by means of justification for changing the codification of PTSD in DSM-5. A “fact sheet” available on the APA’s website states, “Compared to DSM-IV, the diagnostic criteria for DSM-5 draw a clearer line when detailing what constitutes a traumatic event. Sexual assault is specifically included, for example, as is a recurring exposure that could apply to police officers or first responders” (APA, 2013b, p. 1).

While sexual assault was mentioned in the original definition of PTSD in 1980, recurring trauma represents a new way for a person to be traumatized. Prior to 2013, a person repeatedly exposed to traumatic events would not have qualified for a PTSD diagnosis



because repeated exposure did not entail “actual or threatened death or serious injury” as was delineated in DSM-IV-TR.

Fortunately, a change to the diagnostic instruments used to diagnose PTSD remedied the problem uncovered in the *Lancet Psychiatry* article. However, Hoge and colleagues’ (2014) revelation of the discrepancy in PTSD’s diagnostic criteria presents a conundrum in the social discourses of PTSD. In the time since PTSD was officially included in DSM-III in 1980 (under a vastly different description than it has today), the diagnosis not only became a cultural phenomenon but also became understood as “timeless diagnosis, the culmination of a lineage that is seen to run from the past to the present in an interrupted yet ultimately continuous way” (Leys, 2000, p. 3).

Young (1995) observed that the creation of the PTSD category in 1980 ushered in a series of attempts to locate the signs of psychological trauma in historical literary works (pp. 3-4). For example, researchers across disciplines have found evidence of PTSD in the 1666 *Diary of Samuel Pepis* (Daly, 1983), in Shakespeare’s *King Henry IV, Part One* (Trimble, 1985), and the *Epic of Gilgamesh* (Parry-Jones and Parry-Jones, 1994). Shay (1994, 2002), a psychiatrist who worked for the Department of Veterans Affairs hospital in Boston, MA, drew strong comparisons between the psychological sufferings of the military veterans he treated and the ancient Greek soldiers in Homer’s epic poems *The Iliad* and *The Odyssey*. The practice of detecting PTSD in the ancient past has endured well into the twenty-first century. Historians have found evidence that Roman legionnaires manifested post-traumatic symptoms (Melchior, 2011). Others have read the signs of PTSD even earlier in history at the Battle of Marathon (Regel & Joseph, 2010) and in Mesopotamia (Abdul-Hamid & Hughes, 2014). These forays into psychiatric

anachronism are often justified by such claims as “people have always known that exposure to overwhelming terror can lead to troubling memories, arousal, and avoidance” (van der Kolk, McFarlane, & Weisaeth, 1996, p. 47) or that PTSD is a “condition that went unacknowledged for millennia” (Morris, 2015, p. 1; see Herman [1992] for a similar claim).

I have set out to juxtapose two rhetorical practices in this introduction, both pertinent to discussions of psychological trauma. The first is the practice of defining. Noting that the act of defining has “ethical and normative ramifications,” Edward Schiappa (2003) writes that, “Definitions put into practice a special sort of social knowledge—a shared understanding among people about themselves, the objects of their world, and how they ought to use language” (p. 3). For a person to be diagnosed with PTSD he or she must meet the APA’s definition of a person with PTSD. For a person to be diagnosed with PTSD in 1980, when the APA first introduced the diagnosis into its DSM-III, he or she would have had to meet its Criterion A by experiencing “a recognizable stressor that would evoke significant symptoms of distress in almost everyone” (APA, 1980, p. 238). Between 1980 and 2013, the definition of PTSD and what constitutes trauma has changed three times. As Schiappa rightly points out, there are ramifications attached to a PTSD diagnosis. In the case of the military personnel mentioned in Hoge and colleague’s study, a PTSD diagnosis carries with it both positive and negative life changing consequences. A traumatized soldier is granted access to special medical and psychiatric care, and under certain circumstances, he may be awarded an early retirement and monetary compensation, if his traumatization is connected to his military service. On the other hand, a pilot diagnosed with PTSD may

have her flight status revoked, thus ending her career or forcing an unforeseen or unwanted change in her duty status and role in the military. The definition of PTSD also helps the military to estimate its personnel needs and the U. S. Congress to plan the Department of Veterans Affairs annual budget. What counts as trauma and traumatization has significant personal, medical, and social consequences.

Historicizing, creating a history or arguing from history, is the second rhetorical process. Ian Hacking (2007) observes that collapsing contexts to draw a convenient link between the past the present is “a certain kind of rhetoric.” That is, “When we maintain that many people of long ago and in different places are of the kind that interests us, it makes our kind seem more genuine” (p. 299). Historicizing the PTSD diagnosis by locating it in a remote (or recent but unrelated) past is to engage in Whig history, in which the present is transplanted into the past to make present and future inevitable (Latour, 2000). In one sense, it is factually incorrect to state that PTSD existed any time before 1980 because PTSD was not a recognized condition until that year. Unlike other kinds of facts, Bruno Latour (2000) writes, “scientific facts seem, once we wander away from the local conditions of production in the past as well as in the future, to free themselves from their spatiotemporal envelope” (p. 250). As Hoge and colleague’s case has elucidated, moving PTSD outside its “spatiotemporal envelope”—or context, as we tend to call it in rhetorical studies—has consequences for both the diagnosis and the person being diagnosed. PTSD in 2017 is attached to beliefs, rights, remedies, laws, attitudes, and other discourses in the public, medicine, and government that were nonexistent in 1980 and utterly unimaginable in ancient Mesopotamia. Historicizing PTSD not only misrepresents the sociotemporal sufferings of people from other times and

cultures, but also denies the suffering of people who have been affected by events they experience in our world today.

Without context, trauma, as a concept, is static. But as the short history of PTSD I've sketched out demonstrates, the definition of trauma and the diagnosis that describes it are anything but static. The APA acknowledged that its diagnoses are dynamic categories in the introduction to the DSM-III (and in each edition thereafter). Robert Spitzer, who oversaw the compilation of the DSM-III, writes in the introduction, "It should be understood ... that for most of the categories the diagnostic criteria are based on clinical judgement, and have not been fully validated by data about such important correlates as clinical course, outcome, family history, and treatment response. Undoubtedly, with further study the criteria for many of the categories will be revised" (APA, 1980, p. 8). With this brief, but crucial, disclaimer, Spitzer acknowledges that the construction of mental health diagnoses is a rhetorical process based explicitly on judgement and implicitly on the consensus of the judges. The APA reserves its right to change diagnoses based on new circumstances, such as new data, so that the diagnoses fit the knowledge and context of their times.

The questions of why and how the formal definitions of mental health diagnoses change is important to the field of rhetorical studies and to humanistic inquiries into medicine because these changes index moments of transition in the state of medical knowledge and in the context in which that knowledge is created. As I've mentioned, there are significant personal, social, and institutional (governmental) stakes attached to these changes. These changes, I argue, are rhetorical in nature because 1) these changes represent psychiatric medicine calibrating its knowledge to the contexts in which it

operates; and 2) because the formally codified diagnoses it creates are the products of deliberative action. PTSD and post-traumatic illnesses, the term I use to reference codifications of psychological stress that existed prior to PTSD, make for an excellent extended study of these processes because psychological trauma is the product of events in the world. Therefore, any attempt for medical researchers to codify a post-traumatic illness is an attempt to create medical knowledge that describes and intervenes in the world.

In this dissertation, I follow the long tradition of rhetorical scholarship that takes a historical approach to answer questions that cut across time and geography to trace changes in knowledge, practice, and consequences. As Kathleen J. Turner (1998) writes, “Historical research provides an understanding of rhetoric as a process rather than as simply a product; it creates an appreciation of both the commonalities among and the distinctiveness of rhetorical situations and responses” (p. 2). To examine the historical rhetorical processes through which the codifications of post-traumatic illnesses have changed as well as the implications of those changes, this dissertation seeks to answer these central questions:

1. What rhetorical situations created the exigences for the codification of post-traumatic illnesses?
2. How did medical practitioners interpret these exigences and operate within the constraints of the institutions that called upon them to codify diagnoses?
3. And what were the lasting rhetorical effects and material consequences of those codifications?

In the following sections, I situate this study in the context of the relevant the literature of rhetorical studies of medicine and in humanistic studies of PTSD. Then I review the methods I use to answer the research questions.

## **Literature Review**

This dissertation adds to existing literature in medical rhetoric and the medical humanities by examining the historical processes by which post-traumatic illnesses were codified over the past 150 years. In the following two sections I address the literature that is most directly applicable to my topic. I begin by reviewing monographs written by medical anthropologists that take PTSD as their subject. Similar to my purpose of examining the traumatic formations that necessitated the codification of post-traumatic illnesses, these ethnographic studies connect the PTSD diagnosis to its immediate social circumstances to elucidate the effects of psychological trauma on communities, medical practitioners, and government policy. I then review a set of monographs written by medical rhetoricians, historians, and philosophers that take a historical perspective to examine how change occurs in social beliefs about health and medical practice.

### *Medical Anthropology Studies of PTSD*

In the last twenty years, medical anthropologists have published book-length studies on PTSD in military and clinical contexts. While each of these studies is valuable in its own right, a theme common to them is that the reality of the disorder or its diagnostic technologies is obscured and needs to be made evident through the anthropologist's cultural immersion and intimate proximity to traumatized persons and

those who treat them. To a certain extent, each of the studies is inherently rhetorical, focusing on topics like identification, affect, and procedure and documentation; however, none of these studies examines the social, medical, and institutional processes that have contributed to different codifications of post-traumatic illnesses, including PTSD.

The ur-text of anthropological inquiry into PTSD is Allan Young's (1995) *Harmony of Illusions*. Young's medical ethnography of PTSD denies the disorder's cultural and historical ubiquity. Instead, he provides an account demonstrating that PTSD "is glued together by the practices, technologies, and narratives with which it is diagnosed, studied, treated, and represented by various interests" (5). Building on the anthropological work of Abram Kardiner, who published the first US study of war neurosis in 1941, he examines the science behind pathogenic memory, one of the key symptoms of PTSD. Ultimately, his VA hospital fieldwork leads him to the conclusion that the diagnosis of PTSD is based on little scientific examination of a patient. Rather, the patient's ability to narrate his or her trauma and its effect on his or her daily life is the foundation of the diagnosis. More importantly, the final diagnosis rests in the attending physician's careful construction and presentation of the patient's narrative and ethos as a troubled person.

Young's book has been highly influential, but ultimately seems dated because it deals with the flaws of the 1980 edition of the APA's *Diagnostic and Statistical Manual of Mental Disorders* (DSM-III) as well as the ways that clinicians are ill-equipped to make accurate psychiatric diagnoses. Young concludes that the VA's ineptitude combined with a social agenda to make right by Vietnam vets fuel the proliferation of the PTSD diagnosis in VA hospitals. Also of note is his conclusion, which breaks with his

sharp focus on PTSD to ruminate on the troubles endemic to DSM-III's attempt to shift from an analytic model of psychiatry to biological and neurochemical understandings of mental illness – issues that persist today. Young ultimately endorses a shift to a more scientific understanding of mental illness that is grounded in biology, a position that has gained a tremendous amount of support from the neuroscience and biopsychiatry communities in the past three decades. The two following anthropological studies have added a great deal of local knowledge about PTSD with their extensive site studies, but did not endeavor to make claims as grand or important as Young's.

Erin Finley's (2007) *Fields of Combat: Understanding PTSD among Veterans of Iraq and Afghanistan* reflects the shift in cultural attitudes toward PTSD and military personnel with PTSD. Unlike Young, who casts PTSD as a dramatization of American regret for the Vietnam war, Finley focuses intently on the VA hospital system's inability to treat veterans returning from the Global War on Terror. She acknowledges, but does not offer evidence, that “the diagnosis was officially adopted because it was thought that the suffering of Vietnam veterans should be recognized, not because any evidence existed at that time to suggest that PTSD represented a wholly new disorder” (118), and turns to a series of historic VA scandals to suggest that the VA must do a better job of holding up its end of the benefits-for-service model of veteran compensation.

From her fieldwork in a Texas VA clinic and congressional forums in San Antonio, Finley creates a sympathetic portrait of disabled veterans fighting for the service-related benefits they believe the US government owes them. The typical VA treatment regimens leave veterans with a lifelong mental illness diagnosis, a reality that is fueled by the politics and economics of the government medical system. Yet, Finley finds



solace in the VA's changing of the guard. As older clinicians retire, their replacements bring new theoretical orientations to mental illness in the form of evidence-based paradigms that are aimed at the patient's full recovery from PTSD. The group therapy sessions of the 1970s persist, but mental health workers are increasingly moving veterans out of their comfort zones and into intensive treatment programs like prolonged exposure therapy so that they can recondition themselves to function as whole beings in the world. The new paradigm for PTSD clashes with both military and VA culture, resulting in a paradox in the benefits-for-service model of VA healthcare. Finley's claims question the understanding of PTSD as a lifetime disability. As a result, she wonders how should the VA remunerate veterans? Bringing to bear the realization that a person's disability status is always in flux and coupling it with new research findings on PTSD, Finley makes a case study of a law suit filed against the VA because a veteran's PTSD status was downgraded, resulting in fewer benefits for him. The veterans in Finley's study admit that compensation for service-connected injuries was a factor in their decision to seek treatment.

Unlike other accounts of PTSD in the military, Kenneth MacLeish's (2013) *Making War at Fort Hood* opts to understand PTSD within an affective framework. During his four-year fieldwork at Fort Hood, he encounters soldiers who describe their PTSD as a disorder of a vulnerable body. Two lines of thought central to this claim are that PTSD makes a single object out of a whole range of bodily experiences, which, in turn, are treated as a single entity by VA mental health workers who have not experienced the way that war affects the body. (MacLeish argues that combat PTSD is markedly different than PTSD from sexual assault or natural disaster. Thus, it requires

treatments specific to its causation.) Secondly, he portrays PTSD as a catch-22. PTSD is a part of soldiering because its symptoms are actually conducive to surviving battle. Therefore, PTSD pathologizes the work that soldiers do. Since the informants in his fieldwork are keenly aware of this conundrum, they find ways to avoid mental health care so that they can go about the business of soldiering because only those soldiers who are separating from the Army want PTSD symptoms on their health records for the purpose of attaining VA benefits. MacLeish concludes that the Army's rhetorical gestures toward PTSD are meaningless, as they do not account for either the soldier's lived experience or the iatrogenic effects of mental health screenings. He further concludes that soldiers are focused on their bodies, not their minds; therefore they need treatment for their bodies and not their emotions. The better way to treat PTSD, he avers, is to recreate the military mental health care system so that PTSD treatment does not deny soldiers leave time with their families, limit their opportunities for military career advancement, and permanently mark their medical records.

### *Medical Rhetoric and the Medical Humanities*

Rhetoricians and medical humanists have developed a variety of strategies to analyze medical discourse and health phenomena *in situ*. Historian Charles E. Rosenberg acknowledges that disease is a far more elusive and complex entity than reductive biological models can account for. "Disease," he writes, "is at once a biological event, a generation-specific repertoire of verbal constructs reflecting medicine's intellectual and institutional history, an occasion of and potential legitimation for public policy, an aspect of social role and individual—intrapsychic—identity, a sanction for cultural values, and a

structure element in doctor and patient interactions” (Rosenberg, 1991, p. xiii). The multivalent role of disease in society and medicine imbues disease as a concept with multiple meanings and locates it in numerous private, public, and institutional contexts that are both constituted by and constitutive of disease. Rosenberg developed the concept of disease “frames” to tame the complexity of disease as a concept by “describing the fashioning of explanatory and classificatory schemes of particular diseases” (p. xv). For instance, biology often serves as a conceptual framework for public health programs on disease. Yet, for Rosenberg, the key framing of disease is the doctor-patient interaction, the milieu in which the disease is named, the patient is diagnosed, and the prognosis declared. The same disease also frames those it afflicts socially, by structuring their interactions and shaping ideas that circulate about the afflicted. Social responses to disease and those who suffer from them are often negotiated in complex institutional interactions. Courts, as I will discuss below, often determine whether a person does or does not fit within a certain disease framework; an important verdict can often entitle a person to monetary benefits or determine whether a person is intellectually capable of undergoing a criminal trial.

Framing a situation is an integral aspect of rhetorical inquiry and analysis. Judy Segal (2005) adapted the concept of *kairos*, meaning timeliness or fitness to situation, to the history of medicine to locate the moments when medical discourse shifts. Moments of change bear special significance in rhetorical study because changes in medical discourse indicate instances when medicine becomes differently persuasive (Segal, 2005, p. 27). As discourse tends to change in relation to its circumstances, discourse can also index the factors that influence change. Segal’s *kairology* of medicine borrows from Ian Hacking’s

(1998) concept of ecological niches, a set of conditions in which medical phenomena (transient mental illnesses, in his case) can thrive. Hacking's niches require a number of vectors, but he emphasizes four. An illness is such by (1) fitting into a taxonomy of illness (medical vector), (2) being situated between cultural elements (the cultural polarity vector), (3) being observable as an illness (observability vector), and (4) providing a release that is not elsewhere available in its cultural setting (Hacking, 1998; Segal 2005).

Amy Koerber (2013) profitably employs kairology in *Breast or Bottle?: Contemporary Controversies in Infant Feeding Policy and Practice* to demonstrate how shifts in immunology theory had to occur to create the circumstances in which the observable immune benefits of human milk could be theorized and rhetorically mobilized. Her kairology traced how the body-as-complex-system metaphor operated at the intersection of rhetoric, science, and medicine to validate extant knowledge about the immune properties of human milk, but only after immunologists produced a corpus of knowledge that fit within the systems metaphor.

Koerber's (2013) study brings the complexity of the nexus formed by rhetoric, medicine, science, and society into high relief. In particular, she demonstrates that change is not the product of a uniform momentum, as kairology would have it. Change happens at different times in different, often stochastic, ways because rhetors, institutions, knowledges, and practices tend to contradict, contravene, and counteract each other as much as they ally to build momentum within and across sites. To return to Rosenberg's frames, fitness to situation is a variegated concept that relies on the position of a node in a network at a given moment. Kairos as understood as *rhetorical timing* is a valuable

analytic concept, but in projects like Koerber's and mine, it stretches the meaning of opportune moment into a timeframe that may be too large to label as a "moment."

Where Koerber's (2013) study examines shifts in medical authority and cultural practices, Jenell Johnson's (2014) *American Lobotomy* explores "how the meanings of lobotomy emerged, accrued, and transformed as they circulated between medicine and public culture" (16-17). Although her book is organized chronologically, she focuses on moments of "intertextual resonance" where documents and other rhetorical artifacts created shifts in thinking about psychosurgery in America. In particular, she traces how lobotomy became an accepted medical practice and then fell out of favor, becoming instead a standard horror trope in popular culture. She then examines how the negative image of lobotomy creates problems for contemporary psychiatry and neuroscience. By examining a variety of texts, Johnson not only follows the scientific shifts in thinking about lobotomy, but also demonstrates the ascendancy of lobotomy in popular culture, effectively showing that the discredited medical practice lives on today in ways that impede the uptake of scientific innovation, discriminate against people with mental illness, and entertain fans of horror and the paranormal. Johnson brings to medical rhetoric a methodology that centers upon aggregating texts ranging from scientific papers and medical history to horror films and haunted asylum tours to "theorize why a marvel emerged at a particular place and time; hunt for the origins of its polysemic tendrils in cultural narratives, social structures, and political events; and track how it is transformed" when it circulates through and between different publics (13).

Kimberly K. Emmons's (2014) *Black Dogs and Blue Words: Depression and Gender in the Age of Self-Care* adopts a more systematic methodology than do Koerber

and Johnson. She situates her study of depression discourses in the scientific and public literature on mental health, including news media stories and popular press books that were produced between 1995 and 2005, the decade she cites as “a moment in the history of depression when biological psychiatry offered the most productive models of identification and treatment of the illness” (8). After analyzing her corpus for rhetorical features, including “definitions, metaphors, stories and stock characters, and genres” (10), she triangulates her findings by conducting two focus groups with women about their experiences with and understandings of depression. She concludes her analysis of the gender bias in depression discourses with a call for women to resist and counter depression’s hegemonic rhetoric. While more than a decade passed between Emmons’s research and the publication of *Black Dogs*, her study demonstrates how a rhetorician of medicine can identify a cultural phenomenon, map its specific effects, and offer counter-rhetorics to ameliorate a bias in medicine.

Elena Conis’s (2015) *Vaccine Nation* provides a necessary contrast between rhetorical histories of medicine and medical history. As a medical historian, Conis begins her study of American vaccination policies, practices, and beliefs by asking why it is that most Americans accept universal vaccination when nearly fifty years ago vaccination practices were radically different and the number of vaccines was considerably fewer. Her study creates a linear picture of vaccination beginning with the Kennedy administration and progressing through to today. Along the way, she cites changes in social attitudes toward the role of children, views on women, and the growing influence of pharmaceutical companies on federal policy as agents that cast disease according to the prevailing cultural beliefs of the day. That is, beliefs about health during the Cold

War differ from beliefs during the Information Age, which in turn influence vaccine practice. There is much that studies of medical rhetoric can take from Conis's work, which accounts for micro, meso, and macro level sociopolitical changes while demonstrating that vaccine has remained a constant concern from the Great Society to the Affordable Care Act. Unlike Koerber, Johnson, and Emmons, who organize their studies around rhetorical events, Conis traces vaccines through eras that are delineated by the regular changes of presidential elections because she is examining, in part, the effects of federal vaccine policy. However, these epochs each bring with them new beliefs, arguments, situations, and patterns that document social change. In many ways, Conis's work is highly rhetorical; however, she never explicitly leverages rhetoric as an analytical lens.

What I am interested in is the creation of medical phenomena through the interplay of social context, medical knowledge, of institutional governance, a process that takes time as it fades in and out of medical interest. With each episode of renewed interest, physicians create new knowledge about psychological trauma to address a specific set of circumstances. Therefore, PTSD in 2017 is unlike PTSD in, say, 2012 or 1980. I show here that the codified thing called PTSD is an amalgam of *historic formations* that resulted from plates pressing together at a fault line for over a century.

The concept of formations arises in Celest M. Condit's (1999) *The Meanings of the Gene* and in the later writings of Ian Hacking (1995; 1998; 2000; 2007), who revises his ecological niches idea in favor of a more robust analytical framework for medical inquiry. Condit (1999) sampled magazines, newspapers, government documents, and television news coverage pertaining to eugenics and genetics from 1900 to 1995, to

identify “rhetorical formations” (p. 14). The term rhetorical formation designates “the relatively co-occurrent sets of discourse—metaphors, narrative, values, and so on—that the data analysis and critical readings have located in *each time period*” (p. 14, emphasis added). Although rhetorical formations can be located in discourse, she argues they are analytically distinct from Michel Foucault’s discursive formations which presume that an “underlying episteme ... uniformly permeates all talk and all practices” within a historical timeframe (p. 14). As a result, Condit’s methodology uncovers the various rhetorics that circulate at and over time, offering a means of tracing how, where, and when rhetorical formations change, such as times when one metaphor is challenged by another and is ultimately usurped by it.

Ian Hacking uses a methodology similar to the rhetorical historians discussed in this section in his writings on mental illness with one notable exception. Hacking (1998) is interested in mental health and social phenomena that seem to manifest unexpectedly, run a course that effects some change in society, and then disappear. *Mad Travelers: Reflections on the Reality of Transient Mental Illness*, traces the emergence of fugue (a type of compulsive walking disorder) in 1887 though to 1907 when the diagnosis is forgotten. Hacking deals solely with the first fuguer, but notes that after he is diagnosed, instances of the illness became strikingly more common. Hacking began this line of research in his *Rewriting the Soul: Multiple Personality and the Sciences*, where he first describes his “looping effect of human kinds” (1995, p. 21). Of the looping effect, he writes, “People classified in a certain way tend to conform to or grow into the ways that they are described; but they also evolve in their own ways, so that the classifications and descriptions have to be constantly revised” (p. 21). His prime example of this feedback



effect is multiple personality (disorder), which came about in the mid-1900s, became a cultural phenomenon in the 1970s and 1980s, and then vanished for various reasons in medicine and society.

After jettisoning “human kinds” as a problematic concept and opting instead for “kinds of people,” Hacking’s *Historical Ontology* (2002) incorporates aspects of Michel Foucault’s *Archeology of Knowledge* and “What Is Enlightenment?” (vectors of truth, power, and ethics) into his looping effect to formulate a theory of historical ontology. “Historical ontology,” he writes, “is about the ways in which the possibilities for choice, and for being, arise in history. It is not to be practiced in terms of grand abstractions, but in terms of the explicit *formations* in which we can constitute ourselves, *formations* whose trajectories can be plotted as clearly ... or, at one remove, that can be traced more obscurely by larger organizing concepts such as objectivity or even facts” (p. 23 emphasis added). The purpose of such a theory is to understand how categories of people come into existence, change the people they categorize, and, in turn, are changed by the people who categorize them. I would argue that the categories can change institutions and society, too.

In “Kinds of People: Moving Targets,” Hacking (2007) identifies ten “engines of discovery” that propel the process of “making up people” as moving targets. The first seven are engines of discovery that create an imperative for locating and creating new kinds of people. They are counting, quantifying, norming, correlating, medicalizing, biologizing, and geneticizing—all of which create scientific knowledge about certain types of people. The remaining three are normalization, bureaucratization, and resistance by the known people to their knowers. These engines play across the five domains of his

ontology: (a) classification, (b) people, (c) institutions, (d) knowledge, and (e) experts. He explains, “there are (e) *experts* or professionals who generate or legitimate the (d) knowledge, judge its validity, and use it in their practice. They work within (c) institutions that guarantee their legitimacy, authenticity, and status as experts. They study, try to help, or advise on the control, of the (b) people who are (a) classified as a given kind” (Hacking, 2007, p. 297). Hacking’s heuristics of historical ontology offer a means of tracing change in medicine, other institutions, and society through locations where rhetorics are produced.

## **Methods**

This dissertation is a historical study of the development the PTSD diagnosis. In it, I specifically examine the rhetorical processes by which PTSD and its predecessor diagnoses were codified as psychiatric knowledge. My first task in embarking on this research was to identify the moments when post-traumatic illnesses were codified and put into practice. These moments of change would help me to following the trajectory that my project would trace. I turned to the rich literature on the history of psychiatry to learn the disciplinary history of the field and its many perspectives. Henri Ellenberger’s (1970) tome *The Discovery of the Unconscious* provided me with a strong sense of how dynamic psychiatry emerged in the eighteenth-century and developed in following centuries. I then triangulated Ellenberger’s (1970) study with George Rosen’s (1969) *Madness in Society*, Michel Foucault’s (2006) *History of Madness*, Roy Porter’s (2002) *Madness: A brief History*, Edward Shorter’s (1998) *A History of Psychiatry*, and Tom Burn’s (2014) *Our Necessary Shadow*. There is tremendous diversity in the historical approaches and theoretical findings in these texts, but all agreed on two points relevant to my project. The

first point is that there is evidence in the historical record that the thing that we now call mental illness has existed in one form or another in virtually all human cultures across time. The second, and more germane point, is that the prehistory of psychiatry begins in the middle ages with the incarceration of “mad” men and women, but, as Shorter (1998) writes, “Before the end of the eighteenth century, there was no such thing as psychiatry” (p. 1). At this point, I refined my focus to diagnoses created after 1750. Turning to the secondary literature that focused on the history of trauma from 1750 to the present, I found that most widely referenced study of trauma was Michael R. Trimble’s (1981) *Post-traumatic neurosis: From railway spine to whiplash*. Trimble (1981) and numerous sources citing his work convincingly argue that railway spine was indeed the first codified post-traumatic illness. Others certainly had been in circulation before this time, but railway spine marked the beginning of a concentrated research program in psychiatry to understand the causes, courses, and treatments of post-traumatic illnesses.

I should note here that military historians and the US Department of Veterans Affairs have cited earlier conditions that existed before railway spine as predecessors of PTSD. I disagree with them. Indeed, conditions like nostalgia, which I discuss in Chapter 3, are mental illnesses that soldiers experienced during war; however, they are not post-traumatic illnesses because they are not pathological, psychological, or functional responses to a specific event or series of events. Although military history has been indispensable to this project, I defer to historians of psychiatry whose specific object is to trace the history and development of that discipline.

Taking railway spine as the inaugural post-traumatic illness, I then read the secondary literature on PTSD to identify the post-traumatic illnesses that were codified

between 1853, when railway spine was first mentioned and the official entry of PTSD into the DSM-III in 1980. This search resulted in the following candidate disorders: shell shock during WWI, war neurosis during WWII, and Post-Vietnam Syndrome. I then examined the chronological spread of the diagnoses. Between railway spine and shell shock, a half century elapsed; approximately twenty-five years separated shell shock and war neurosis. War neurosis, known as “gross stress reaction” in DSM-I, stayed in circulation until the appearance of DSM-II in 1968. Post-Vietnam Syndrome appeared in 1972, immediately followed by PTSD in 1980. To keep the spacing between diagnoses consistent, I selected railway spine (1853), shell shock (1914), Post-Vietnam Syndrome (1972) and PTSD (1980) as my case studies for this dissertation. My hypothesis was that greater space between the diagnoses would ensure that the context in which the diagnosis was codified and the medical thinking used in that process would be different. Fortunately, my hypothesis proved to be correct.

The method I used to select my case studies resonates with two established methods of rhetorical historiography. The first of these methods is Judy Z. Segal’s (2005) “kairology,” which examines change in “rhetorically tilted medical history” (p. 23). I have already offered a critique of this method in my discussion of Koerber (2013). Despite the theoretical shortcomings of kairology I have identified, my rhetorical history of PTSD is similar in principle because it traces the development of a post-traumatic illness through the moments when they were codified, signifying a change in sociomedical thought and practice. The second method is pan-history, which Debra Hawhee and Christa J. Olson (2013) define as the practice of researching and writing histories “whose temporal scope extends well beyond the span of individual generations”

or “leap across geographic space, tracking important activities, terms, movements, or practices as they travel with trade, with global expansion, or with religious zealotry” (p. 90). I would add to that tentative list *medical knowledge and practice*, which I see as another force that vectors out across four dimensions. Pan-history combines a “time-slicing” approach to selecting case studies, like the process I have already outlined for my own case studies. The approach complements kairology because it focuses on creating time-slices that demonstrate the durability and context of the concept under examination. Where pan-history differs is that it requires the researcher to compile multiple archives for the time slices and to examine how the “archives move” (p. 101). That is, finding the rhetorical activity in the form of “movements, motives, and motifs” that are embodied in the research. In reading the history of psychological trauma, I observed that three types of discourse moved in the codification of post-traumatic illnesses: social documents, medical documents, and governmental or institutional documents. Each of these played roles in the codification process. Therefore, for each case study, I compiled social texts, including journalistic accounts and fiction and non-fiction writings; medical texts, including pamphlets, journal articles, and books; and government and institutional documents, including Congressional testimony and correspondence. With these archives compiled, I used a theoretical apparatus that combined Condit’s and Hacking’s notions of formations. I analyzed the archives for the rhetorics that were used to bring each of the diagnoses into being. In each case, I would look for the traumatic situation (the exigence or social phenomenon) that anchored the traumatic formation, the discourses that were used to justify the codification of a post-traumatic illness and constrained how that illness was codified.

At this point, I recognized that a search for formations alone did not adequately address the reasons why post-traumatic illnesses were codified in specific ways at specific moments in history. In each of my case studies, there were significantly different reasons why the post-traumatic illness was codified, which produced markedly different rhetorics. To remain faithful to the archives I collected, I had to adopt a different theoretical lens for each codification of post-traumatic illness. As a result, within the rhetorical formation of railway spine, I identified a phenomenon that I call medico-legal rhetoric. I analyzed shell shock through the rhetorical lens of *megethos*, the rhetoric of magnitude. For Post-Vietnam Syndrome and PTSD, I identified two distinct types of constitutive rhetorical practice. I review each of these analytical tools in their respective chapters. In sum, each traumatic situation had a different exigence, which rhetors responded to with different rhetorics. In this way, I could attend to psychological trauma as a concept that moved across time as well as the way that was codified in vastly different contexts.

## **Chapter Overviews**

Each case study in this dissertation is treated in its own chapter. Together they constitute a pan-historical rhetorical study that approaches PTSD as a medical and cultural phenomenon. These chapters challenge the notion that PTSD is an inevitable diagnosis that describes a mental illness that has always existed across time and cultures. By examining the rhetorical processes of codifying unique post-traumatic illnesses over the course of 150 years, this dissertation argues that post-traumatic illnesses are better understood as dynamic entities that respond to specific social problems. The diagnoses

themselves must conform to the constraints of their day as determined by the institutions (government, military, or disciplinary) that call upon psychiatric medicine to intervene in the social problem.

Chapter 2 presents the first codified post-traumatic illness, railway spine. After the first passenger railway opened in England in 1830, train accidents became common occurrences. Many of the survivors sued the railway companies for compensation for their injuries. British courts were adept at trying cases that involved bodily harm, but were insufficiently prepared to adjudicate cases in which passengers sued for post-traumatic symptoms, including functional paralysis and diminished mental acumen. The courts called upon surgeons, who had advanced knowledge of neurology, to testify about the validity of passengers' invisible illnesses. Medical opinion was bitterly divided on the matter. This chapter analyzes the debate among British surgeons and physicians that occurred in pages of medical journals and in monographs. The debate centered on whether railway spine was caused by physical injury or by intense fear. The study finds that doctors espousing a somatic view of railway spine adopted a medico-legal rhetoric to make their codification of railway spine legible to the courts. By adopting the rhetorical practices of the courts, doctors who endorsed a somatic codification of railway spine persuasively argued that train accident survivors who manifested the symptoms of psychological trauma were indeed deserving of monetary compensation because the cause of their symptoms were traceable to bodily harm. Although opinion about the nature of railway spine remained divided among physicians, British and later American courts found the somatic codification of railway spine useful until the end of the nineteenth century.

Decades after British physicians codified psychological trauma as the product of a physical wound to the victims' bodies, British military psychiatrists studying the causes of shell shock on the battlefields of WWI challenged somatic conceptions of psychological trauma. In Chapter 3, I examine the context in which shell shock emerged as a phenomenon that had the potential to undermine the Allied war effort. Shell shock overwhelmed British military doctors, who were not only materially unprepared to tend to a deluge of traumatized soldiers, but also theoretically ill-equipped to understand the phenomenon itself. Traumatized soldiers were often treated as cowards or malingerers and faced harsh penalties in courts martial. London-based neurologists originally codified shell shock as a physical wound to the body caused by a soldier's proximity to an exploding artillery shell. However, psychological psychiatrists stationed close to the frontlines contested this codification, arguing instead that shell shock was the product of extreme fear and privation.

I analyze the traumatic situation of shell shock through the lens of *megethos*, the Greek word for magnitude, to examine how shell shock shifted medical opinion and practice. The concept of *megethos* has circulated in rhetorical theory from at least the time of Aristotle. Over millennia, it has accrued a variety of meanings and functions, including the ability to help rhetors determine what is most important. Using the magnitude of the war of the backdrop, I demonstrate it how psychologically-oriented psychiatrists persuaded the Royal Army to accept their codification of shell shock and to create facilities to tend to shell-shocked soldiers near the battlefield rather than in hospitals back in England. Lastly, I explain how psychological rhetorics of shell shock disrupted military doctrine on cowardice and malingering.



Forward psychiatry, an army's ability to triage the psychologically wounded during combat, was a major innovation for the British and American militaries in WWI. The U. S. military refined these practices in World War II, the Korean War, and the Vietnam War. While the U. S. military became adept at treating soldiers who broke down in combat, a new problem arose when Vietnam soldiers returned home and manifested psychological problems. However, a nexus of circumstances prevented soldiers from receiving treatment for these problems. Media reports of atrocities perpetrated by U. S. soldiers fueled the American public's opposition to the war and disdain for the soldiers who fought it. Additionally, Vietnam veterans did not manifest symptoms of psychological distress until they separated from the military, making it difficult to connect their maladies with the war. Since their mental problems could not be directly connected to the war, they were not eligible for treatment in Department of Veterans Affairs Hospitals. To compound matters, there was no diagnosis in the APA's DSM-II that accurately described the symptoms veterans experienced because the APA removed combat-related disorders from DSM-II in 1968. However, in the early 1970s, a small group of activist psychoanalysts joined the antiwar group Vietnam Veterans Against the War to help heal traumatized veterans.

In Chapter 4, I examine the rhetorics used by the activist psychoanalysts to reconstitute the identity of the Vietnam veteran in the public, governmental, and clinical spheres. They argued that the veterans suffered from Post-Vietnam Syndrome (PVS), a cluster of symptoms that formed a mental health diagnosis that was not officially recognized by the APA. The activists articulated that the veterans themselves were survivors of an atrocity, and deserving of support from the nation that sent them to war.

In Chapter 5, I analyze the actions and discourses the psychoanalysts used to convince the APA to include PVS in DSM-III, so that veterans' combat-related mental health problems would be formally recognized by psychiatric medicine. In the process of calibrating their discourse to accommodate the APA's institutional regulations, PVS was recodified as post-traumatic stress disorder and added to DSM-III in 1980.

Chapter 6 concludes the dissertation, discusses its implications, and offers future directions for this research.

## Chapter 2: Railway Spine

### **Introduction: Railway Travel, Medicine, and the Law**

In the nineteenth century, the introduction of passenger railway travel in England, the United States, and continental Europe ushered in a new era of medical thought and practice. Beyond the common health concerns about life in this new industrial age, like the noise of the train's machinery and the smoke locomotives spewed, physicians on both sides of the Atlantic Ocean contemplated the effects that the mechanical vibrations and jolts from the new mode of transportation would have on the human body and mind. Contradictory medical opinions about rail travel abounded in the 1830s. In 1835, Bavaria's *Obermedizinalkollegium* posited that the passengers would develop brain damage, but two years later, a medical opinion reprinted in the pages of the *American Railroad Journal* offered an entirely different view, "The vibratory, or rather oscillatory motion communicated to the human frame, is very different from the jolting and swinging motions of the stage-coach, and is productive of more salutary effects. It equalizes the circulation, promote digestion, and tranquilizes the nerves" (qtd in Schivelbusch, 1986, p. 114). Medical and public opinions often wildly vacillated to condemn the health hazards of the past, which technological progress was steadily diminishing, and forecasting even greater detriments to public health in the future. Even decades after railways had been in operation in Great Britain, members of the traveling public still espoused their concerns despite their enthusiasm for the convenience of mechanized travel, with one such commentator writing, "there exists so profound a popular belief ... that railway traveling in itself, and with the occurrence of any

noticeable accident, produces mild concussions, unfelt by the strong and fat, but most dangerous to the nervous and the thin” (Littell’s Living Age, 1866, p. 554).

The railroad was a powerful symbol of industrial change that also served as a repository for the social anxieties that are concomitant with the permanent mechanized disruption of everyday life. These concerns manifested in a variety of anti-railroad discourses in Great Britain, ranging from concerns about trains blighting both vistas of the countryside and the crops that grew there, to fears of increased mobility for members of lower social classes, who would undoubtedly deteriorate the moral character of geographies previous inaccessible to them (Harrington, 1994). Though early concerns about the social and health effects of railway travel abated over time, railway accidents posed unique new problems for the practice of medicine.

On the day the Liverpool Manchester Railway Company—the first passenger train in the world—opened to the public in 1830, its famous steam engine, *Stephenson’s Rocket*, struck and killed The Right Honourable William Huskisson (Malleon, 2002; Camps, 1866). The death of a member of Parliament did little to dissuade railway companies from laying tracks and opening lines that crisscrossed the country. As the number of railways grew, so did the number of railway fatalities and injuries. In 1864, railroads in the United Kingdom were responsible for 36 deaths and 700 injuries; the previous year, 35 were killed and 401 were injured (Camps, 1866). In the United States, one in every 117 railway employees was killed in 1889, and one in 12 was injured (Caplan, 1998).

New York physician Edmund Arnold (1862) indicted railways companies for treating death as a matter of course. With train lines connecting established cities via

remote geographies in the east and pushing out into the undeveloped west, Arnold argued that railway companies must arrange to tend to the medical needs of passengers who fall victim to the risks of riding the rails. He wrote, “Let not this or that railway company point to statistics to show the special safety of their line; the breaking of a rail, the springing of a wheel, the falling of a piece of rock on the track, an apparently trifling act of carelessness on the part of a railway servant, and the best regulations are rendered to no avail” (Arnold, 1862, p. 12). He concluded his argument for training railway staff in the healing arts and employing medical practitioners to be stationed along the lines with a plea to public health: “Neither can any member of the traveling community, by care, prudence, or precaution of his own insure himself against being numbered amongst the victims of the next great catastrophe by which the public mind shall be startled” (p. 12).

Arnold’s use of the term “public mind” can be read as a powerful double metaphor indicative of its time. The traveling public, both in the United States and in Europe, was indeed concerned with the potential risks of traveling by train. Newspaper articles often reported in ghastly detail the deaths resulting from railway accidents and the legal proceedings that followed. The “Accidents and Occurrences” section of the November 26, 1859 edition of *The Examiner*, an English paper, reports the findings of a legal inquiry into the death of Mary Brown, a single, 84-year old woman traveling on the North-London Railway. The jury found that the “train being set into motion without sufficient care being taken by the company’s servants in seeing after the security of the passengers before starting” resulted in Brown falling through a defective carriage window of the moving train (*Examiner*, 1859, p. 763). The article states, “The body of the deceased was shockingly mutilated, the back and abdomen were extensively cut by

the steps of the carriages striking her. Both ribs were broken, the skull fractured, the spine dislocated, and she had sustained other injuries” (p. 763). The brief news items is devoid of any sensational or condemnatory rhetoric against the railway company. Rather it adds to the somber death toll that began with the birth of the passenger railway. It also places railway companies and medicine in the courtroom, a setting that became increasingly and uncomfortably familiar in nineteenth century industrialized nations.

The courtroom is also the venue where the second “public mind” of Arnold’s metaphor was ferociously debated by physicians attempting to account for a new pathological condition called “railway spine.” The September 14, 1861 edition of *The Lancet* outlines the factors that contributed to the creation of railway spine as a medical diagnosis: “The development of railway travelling [sic] has brought out quite a new subject of medical inquiry. The injuries to the human frame resulting from the various and numerous accidents to which railway trains are liable, have already furnished the material for many costly legal disputes, and not a few medical conflicts” (p. 255).

One of the most polarizing factors in medical conflicts about railway injuries was that doctors were often pitted against each other in legal cases. In the aftermath of a train accident, lawyers for plaintiffs seeking monetary damages would require their patients to undergo a medical examination to substantiate their claim for remuneration. Railways, too, employed surgeons to examine plaintiffs. In court, both sides of the case would present medical evidence, offer a prognosis for the patient, and often stipulate the amount of remuneration to which plaintiffs were entitled. In clear cases of bodily trauma, medical experts tended to concur. However, cases arose in which the plaintiff left the accident site under his or her own power only to develop symptoms later. More befuddling to medical

opinion, there were plaintiffs who developed severe somatic and psychic symptoms after the crash, but who had no traces of bodily harm. In these cases, medical opinion varied widely and experts disagreed vehemently with each other.

Surgeons, who specialized in diagnosing and healing bodily traumas, were faced with patients who walked away from railway accidents unscathed only to develop paralytic and psychic symptoms hours, days, or months later. These victims of a condition that came to be known as “railway spine,” a slang term for an established medical diagnosis called “concussion of the spine,” inspired sweeping (and contentious) conceptual changes in medicine as physicians attempted to describe the underlying mechanism of this malady. Since a large proportion of passengers who developed psychological and neurological symptoms in the aftermath of railway accidents sought legal damages from railway companies, railway spine was considered a medico-legal condition, and doctors would treat each case as if were to go to trial. As such, medical science became entangled with the law, forcing the practice of medicine to comply to new norms and to consider new types of evidence as indicative of disease. In its new medico-legal context, surgeons were pressed to develop a diagnosis that could establish the linkage between the traumatic event of the train crash and the neuropsychological symptoms that manifested in unscathed victims after they left the wreckage.

Although juries in these cases tended to be sympathetic to plaintiffs, with nearly 70% of cases being found in the victim’s favor (Caplan, 1998), medicine itself was at an impasse. The public nature of the conflicting medical testimony concerned physicians, who were actively working to make medicine a reliable, scientific institution (Starr, 1992). Furthermore, doctors could not seem to agree on the mechanism that caused the

mysterious new malady of railway spine. A variety of theories were advanced, but none could provide sufficient scientific evidence to persuade medical practitioners of their legitimacy. Concurrently, theories and practices were developed to meet the less stringent criteria for evidence in the courts, but physicians remained polarized for decades.

To compound these conflicts in medical opinion, railway companies in the United Kingdom lobbied parliament to set a cap on the damages that could be recovered by injured passengers (Skey). This bid was unsuccessful, though, and perhaps worked against their favor. In 1864 the United Kingdom passed an amendment to the Campbell Act of 1846, which stated that railway companies were liable for their passengers' safety (Schivelbusch, 1986). Germany passed similar legislation in 1870. And the US followed suit, with historian Barbara Young Welke (2001) noting that by 1920 the vast majority of courts had held railway and streetcar companies liable for passenger injuries. These new legal structures, in addition to extant pressures from the courts and internal pressures from disciplinary medicine to maintain its emergent scientific professionalism, forged a rhetorical formation that would change the role of medicine, as well as its practices and research program.

In the remainder of this chapter, I situate the railway spine diagnosis in its historical context, then employ the concepts of historical ontology and rhetorical formations to elucidate the rhetorical processes by which railway spine became a codified medical diagnosis. This process involves new kinds of people being made and new knowledges being generated to account for the effects of railway accidents. In particular, I focus on how medicine as a profession expanded its role as engine of scientific knowledge to become a bureaucratic force in legal settings. By doing so, physicians and



surgeons tending to train accident survivors became preoccupied with generating knowledge about railway injuries for the courts. As a result, and paradoxically, railway spine ceased to be a viable category for medical inquiry, and an emergent medical paradigm replaced the medico-legal one, at least within medicine itself. Yet railway spine remained useful to the courts as a powerful discourse for determining facts in injury compensation cases long after it physicians abandoned it.

### **The Medico-legal Rhetoric of Railway Spine**

Beginning in the late 1850s, a sporadic dialogue took place in the pages of *The Lancet*, one of Britain's most acclaimed medical journals. This dialogue addressed the concomitant problems of physicians offering medical testimony in railway compensation cases and the nature of the recent phenomenon of invisible injuries. As time passed, the rhetoric of surgeons writing in *The Lancet* shifted from doubt and condemnation to that of empirical science. By the mid-1860s, surgeons attempted to amalgamate written documentation of as many cases of concussion of the spine as possible to better understand the nature of the malady and its symptoms. Also during this time, the focus shifted from protecting railway companies from those intent on attaining settlements for feigned injuries to ensuring that victims of railway crashes were adequately compensated. In this section, I trace the rhetorical shift in the pages of *The Lancet* from its origin in 1857 to 1866, the date when the first book on railway spine was published and a new medical debate over railway spine erupted.

In a lecture on “concussion of the spine” delivered before St. Bartholomew's Hospital and reprinted in the January 10, 1857, edition of *The Lancet*, surgeon Frederic

C. Skey lamented the recent phenomenon of doctors providing medical testimony in courts for railway injury compensation cases. In his estimation, the legal bar, a place that required the art of rhetoric, was an ill-suited venue for the thorough presentation of medical facts—facts which he deemed should be the sole determinants of a case’s outcome. He informed the doctors assembled before him, “It is very difficult to give due weight, in a legal court of justice, to important and striking facts in medicine, on the recognition of which a true decision may entirely hang” (Skey, 1857, p. 27). The surgeon continued, “We cannot infuse medical feeling into the legal mind. The habits—the very atmosphere—of a court are against us. It may be perfectly true that ‘doctors differ,’ but doctors differ only in their respective modes of reaching the common goal of all doctors—viz., truth. The lawyer fights for victory” (p. 27). “Forensic power” in concert with “legal learning, eloquence, tact, [and] knowledge of human character” are the “weapons” lawyers wield “to make the worse appear the better reason” and the “weaker cause to be upheld” (p. 27). Lawyers were simply unable to “comprehend” medicine, its underlying methodologies, and criteria for evidence. If a doctor, “[p]rompt[s] a counsel learned in the law with all the care due to any complicated medical injury, ... he will fail to elicit from his witnesses even the most striking features of his case; and so long as questions hinging on medical opinions are determined in courts of law the evil will prevail” (p. 27).

To address the problem of medical cases being tried by lawyers and presumably to uphold *the good*, Skey advocated for “a court in which a knowledge of medical science should be rendered indispensable, and of men who should undergo a sound medico-legal education, which will enable them to cope with questions which blend medical and legal

acquisition” (p. 27). Initially it is unclear whether Skey intended for the noun “men” to refer to lawyers or doctors or both, but within the context of his lecture, he expressed doubts as to whether lawyers were capable of attaining the medical knowledge necessary to elicit medically sound evidence from witnesses. He asks, “how is it possible that this knowledge can be even attempted to be acquired [by lawyers]?” (p. 27). Thus it becomes clear in the remainder of the lecture that the men to which he refers are doctors, who should exert their scientific authority in their interactions with the courts as a means to tame the rhetorical powers of lawyers who could possibly win compensation for plaintiffs whose injuries—real, imagined, or feigned—did not medically warrant it.

Although Skey’s lecture asserts that medicine’s role in the court is to temper the rhetoric of lawyers with scientific facts, it is the surgeon himself who deploys a powerful rhetorical figure into the medico-legal context of concussion of the spine, the malingering opportunist. Skey recounts his involvement in the legal proceedings of a lawsuit against a railway company by a 40-year-old man who was struck by a baggage cart on the station’s platform. After the man was helped to his feet, he left to attend a party at a friend’s home. On the following day, the plaintiff “having seen no medical man whatsoever” wrote a letter to the railway company demanding remuneration “for the violence done him by their servant, stating that he was seriously injured, that his nerves were shaken, and that he had ‘concussion of the brain and spine’ from which it was probable he would ‘never recover’” (Skey, 1857, p. 28). The man then left town for a vacation during which time he rowed a boat off a turbulent sea coast and went snipe shooting. Ten days before his case went to trial, Skey, accompanied by other physicians, visited the plaintiff at the behest of the railway company. The man confirmed his leisure activities, but stated that

“his intellectual functions were impaired, and that he could not undertake the duties of his profession” (p. 28). After a physical examination demonstrated that the plaintiff had back pain and an irregular walking gait, Skey left the house convinced that the case in question could not be concussion of the spine because the man’s symptoms did not immediately appear after the injury.

To support his diagnosis, Skey offers his preferred codification of spinal concussion:

A man sustains a blow, and is thrown to the ground. On attempting to rise, he finds his muscles no longer obedient to his will. If he rises unaided, he totters in his gait, and struggles onwards for a few paces and falls. What is yet a more common occurrence is, that he makes no attempt to rise. His limbs, or more probably his legs only, are the seat of paralysis, partial or complete. The muscles of his abdomen may be involved, and commonly his bladder fails in its function as an *expulsor*, rarely or never as a mere *reservoir*, and his urine must be drawn off daily with the catheter. From this condition he rarely recovers under a period of from one to three months. (Skey, 1857, p. 28)

Since the plaintiff failed to adhere to the symptoms of spinal concussion, as Skey delineates them, he asks his audience whether the man was suffering from hysteria, “or nothing at all” (p. 28). Despite Skey’s insinuation that the man’s injuries were a work of “imaginative power,” he recovered £2,000 in damages from the railway company.

The malingering opportunist was a familiar figure in nineteenth century medicine, but one that was typically associated with military service. A September 14, 1861, article in *The Lancet* extends Skey’s concerns over physicians playing the fool to trickery: “The difficulties proverbially attached to the exposure of the tricks of military malingerers are as nothing compared to with the task of determining the reality of some of the injuries to health, physical or mental, which those interested in recovering ‘substantial’ damages

assign to railway collisions” (*The Lancet*, 1861, p. 255). Railway travel had, in comparison to military service, democratized trauma and the average citizen’s means of exacting tolls on institutions that had caused them harm. If the malingering figure were to wear the many faces of the British public, as opposed to the rather uniform face of the Royal Army and Navy, medicine and its new ally the court could easily be overwhelmed by falsehoods.

As a conduit for truth (of both the scientific and legal varieties), disciplinary medicine was forced into a position where it had to develop its own methods to discover the reality of railway injuries. The author of the 1861 article in *The Lancet* writes, “Medical practitioners ought never to raise a false suggestion, or to support an untrue proposition, with a view to elimination of the truth which it is their single and constant object to discover” (p. 255). Skey’s rigid paradigm of comparing a patient to his own subjective definition of a disease was no longer sufficient for eliciting the truth of injuries sustained by railway compensation plaintiffs. Instead, the article implores physicians to exercise “care” and to proceed as science does, “by a rigid observation of facts” (p. 255).

The anonymous author also raises the stakes for the medical profession and emphasizes its role in maintaining the legal rights of the British people. Although the number of doctors employed in court cases was on the rise, medical theory and practice could not keep pace with the injuries caused by railway travel. “[N]otoriously not over-scrupulous” railway companies subverted both the truth of medicine and the power of the courts by settling claims in advance of trials. They also petitioned Parliament for a “life-tariff,” which would set a maximum penalty railways would pay to those injured or killed while riding trains. In voicing his opinion against people’s lives being treated as

operational costs (they “could be as easily calculated as the wear and tear of rolling-stock” [p. 255]), the author argued that doctors and the courts should take on railway suits on a case-by-case basis. “It is impossible,” he wrote, “with any approach to justice, to distribute the cases of injury under the heads of a scale. The actual damages sustained by a shock or other personal injury vary as infinitely in kind and degree as do the constitutions, ages, social stations, and professions of men” (p. 255). Courts and doctors should be chastened not to offer opinions until all the facts are known because a set compensation is a “flagrant violation of the rights of juries, which are the coequal with the rights of the people” (p. 255). In addition to discovering truth, medicine was now charged with preserving justice.

A hypothesis that was previously rejected by Skey entered into the author’s charge for medicine to discover truth and uphold justice. “Shock” could “damage” a person (p. 255). The concepts of “shock” as a bodily phenomenon and as a psychological experience had been in circulation for well over one hundred years (Morris 1868), but neither concept had been thoroughly theorized, as most physicians seemed to presume it was tacit knowledge among colleagues. The author’s use of shock here is important to the concept of railway spine for two reasons. Firstly, it indicates that something outside the boundaries of established medical knowledge underlay the mechanism of concussion of the spine caused by railway accidents. Secondly, it serves as a call to readers of *The Lancet* to posit their own theories based on intensive empirical observation. While Skey explained the late onset of concussion symptoms as “hysterical” (meaning a psychological condition), the author of the 1861 article entertains the possibility that there is a delayed onset for nervous (meaning of the nervous system) symptoms

following railway collision: “a whole train of nervous symptoms ... gradually developed themselves, reaching their acme perhaps, only after many months” (p. 255). Such observation proved difficult given the invisible nature of railway spine — it was nearly impossible to gather objective facts about a person’s nervous system while he or she was alive. In lieu of objective scientific facts, doctors turned to a different technique of gathering data about nervous cases to persuade their colleagues about the existence of railway spine. They relied on their own ethos as men of science who could glean facts and tell the truth and began to compile their case studies of spinal concussion. A catalogue of spinal concussion cases would provide outside evidence in railway compensation cases, and quantifiable data on spinal concussion following railway accidents.

Four years later, John E. Erichsen, whose name would become synonymous with railway spine, published a case study in *The Lancet* that cast doubt upon the current medical codification of spinal concussion. The suggestively titled article, “Concussion (?) of the Spine from a Fall from a Scaffold,” recounts the circumstances of a 30 to 40-year-old patient called W. R. who fell 30 feet from a scaffold onto the flat of his back. After he was carried to University College Hospital, it was determined that he had no broken bones and no visible signs of injury. “There was no palsy of the bladder sphincter; and when he lay in bed he could move his legs fairly, but on attempting to get up he was quite unable to stand” (Erichsen, 1865, p. 288). After three-months rehabilitation, he was able to move with the help of a bystander, but never recovered his ability to walk on his own due to partial palsy (paralysis) of the legs.

In the discussion following the case study, Erichsen noted that “we [physicians] have a special motive for recording this case. It is, unfortunately, not uncommon for a railway passenger to receive a shock to the spine which leaves no mark of injury to the tissues, and yet, by the *patient’s own account*, disables him as this man is disabled” (p. 255 emphasis added). To further bolster the authenticity of W.R.’s case, Erichsen noted that the “poor fellow’s impairment is genuine” because “[h]e has no claim for compensation like that which often throw such suspicion upon the feeble movements of a passenger who has been injured in a railway collision” (p. 255). Having established that W.R. had no reason to exaggerate his symptoms, the surgeon then highlighted that the patient had an “obscure injury of the [spinal] cord” but retained his excretory functions. Again comparing W.R. to railway claimants, he posited, “It is quite certain that were this man the plaintiff in an action against a railway company the fact of the bladder and sphincter functions being perfectly performed, and the entire absence of palsy sensation, might tend to throw great doubt over the genuineness of the partial palsy of motion with which his is affected” (p. 255).

Erichsen’s case is an argument borne of careful observation that systematically dismantled the rigid notion of spinal concussion offered by Skey. Where Skey wielded the presumed certainty of medical science to make a legal implication that a man is feigning spinal concussion for compensation, Erichsen cast doubt on the certainty of medical science to bolster the reliability of injured passengers. W.R.’s case demonstrated that subjective symptoms can indeed be reliable evidence of injury both for doctors and the courts. Erichsen used the formation of medicine, law, and railway accidents to



increase both medical and legal knowledge of the symptoms that could manifest after train collisions.

Taking legal interest in medicine as his exigence, Erichsen subverted the current medico-legal context of spinal concussion and used it to advance his own theory of an underlying mechanism. He advanced the hypothesis that the accident produced a lesion or molecular degeneration of the spinal cord itself, one that could not be observed by any outward signs of trauma but that impaired nervous function in the lower extremities. Furthermore, he took the opportunity to call out another shortcoming of medical practice, the lack of post-clinical care: “It is one of the weak points in our hospital system that we cannot follow up these cases and learn the length of time which elapses before an injury is remedied, or whether it is ever repaired” (p. 255-256). Since an N of 1 is hardly sufficient evidence to prove a hypothesis, in medicine or in court, Erichsen petitioned the readership of *The Lancet* to send him “accurate, but concise, records bearing upon this point” (p. 256). The idea of a catalogue of railway spine data resounded with *The Lancet’s* readership, implying that they, too, desired more information on spinal concussion. Erichsen’s charge was taken up by neurologist Thomas Buzzard who diligently and studiously compiled cases from the field. Erichsen would return to the spinal concussion discussion in 1866, when he published the first and most highly divisive book on spinal concussion, *On Railway and Other Injuries of the Nervous System*.

## **Cataloguing Railway Spine: Empiricism and the Engines of Creation**

Erichsen's open solicitation of railway spine case studies and their follow-ups received at least one response. The month after his short communication was published in *The Lancet*, Thomas Buzzard published a letter to the editor of *The Lancet* reinforcing the need for physicians to heed Erichsen's call to compile cases. Buzzard had attended to three survivors of a "severe railway accident" in 1861 who had received compensation for their injuries in court. With four years having passed between his last visit with the patients, Buzzard set out to cold call his former charges. A forty-year old pregnant woman who was pitched over a seating divider in a second-class carriage during the accident still suffered from back pain and throbbing headaches during his visit. A twenty-three year-old man whose second-class carriage derailed and slid down an embankment still exhibited the signs of nervousness, including fears of crowds and tremors when he awoke. The last of the patients, a twenty-one year old woman who experienced nightmares after the accident, died a year after the accident from phthisis, but it was unclear to Buzzard whether her progressive wasting was related to her child's death from measles or the train accident itself.

A rhetoric of medical surveillance permeates Buzzard's letter to the editor, as he makes the case for the utility of the practice of popping in on former patients unannounced. His surprise visits served to confirm that each of his living former patients continued to suffer from functional and psychological symptoms four years after their train accidents. This finding was important to Buzzard's colleagues because their ability to diagnose railway spine surpassed their capacity to render accurate prognoses for patients. Such a dearth of information played into the accusations of malingering related

to railway spine that were perpetuated by doubting physicians, like Skey, as well as railway companys' legal councils. It appears that Buzzard was cognizant of these discrepancies, although he never outright mentions the latter. Instead, he concludes his letter with a short paragraph that marks the overlap of the medical, legal, and corporate spheres created by railway spine:

It is only right to add that in my interview with the first two patients I was unable, after very careful observation, to perceive any traces of intemperance in either of them. It is evident that when persons in a humble position have suddenly received considerable sums of money, this is a point which must ever be kept in view, especially where nervous symptoms are detailed. I feel convinced, from what I observed in my perfectly unexpected visit, that this source of fallacy may be excluded in the present instance. (Buzzard, 1865 p. 443)

Buzzard names the rhetorical commonplace (topoi) that marks skeptical discourse on railway spine. His claim that he “was unable, after very careful observation, to perceive any traces of intemperance” marries surveillance with classification in the study of railway spine. Not only can a person's moral status be directly observed by the clinical gaze, but the data gathered from that observation can be used for purposes outside the doctor-patient interaction, purposes that are tangential at best to diagnostic medical knowledge. His brief concluding paragraph acknowledges that doctors who wish to pursue a line of inquiry about railway accident victims must do so with the approbation of the other institutions that hold a stake in the railway spine diagnosis.

By following up with a patient (over a period of years), medical doctors could create a body of knowledge about railway accident victims that could assist them in categorizing post-accident disorders and rendering better prognoses. They also had the follow-up visit, a crude, yet in Buzzard's eyes, effective technology of discerning whether their diagnoses and prognoses were accurate. Presuming patients were

insufficiently quick-witted to remember and (re)fabricate their post-accident ailments, the unannounced follow-up visit was an objective method of validating medical opinions. Since these opinions would inform the size of the monetary settlements accident victims received from railway companies as compensation, the follow-up also served to verify if the correct sum was awarded.

From October 1865 to May 1867, Buzzard published in the *Lancet* three open letters and a four-part serialized article, “On Cases of Injury from Railway Accidents; Their Influence Upon the Nervous System, and Results.” In his first open letter, dated October 14, 1865, Buzzard offered physicians a method of following up with their former railway accident patients as a means of gathering data about their progress from their initial diagnoses. Buzzard informed *The Lancet’s* readership in his second letter, a brief transmission published on January 6, 1866, that he would be publishing a series of papers in the journal “to record briefly the prominent symptoms ensuing upon shock to the nervous system from railway accidents and other analogous violence, with the condition of the patient observed several months or years afterwards” (Buzzard 1866a p. 23). Up to this point, he noted, medical literature on “concussion of the head, or of the spine” has documented the “immediate effects” of injury (p. 23), but longitudinal information necessary for treatment and prognosis was available. To fill this gap in medical knowledge, the neurologist requested that his readers submit to him case notes and follow up information on their railway spine cases.

In his publications, Buzzard demonstrated a heightened awareness of the discourses about malingering associated with railway spine as well as the ways those discourses were marshaled to discredit the patient seeking compensation and the railway

spine diagnosis itself. When addressing *The Lancet's* readership on January 6, 1866, Buzzard made a special petition for doctors to send him details of “cases in which recovery very rapidly followed upon the patients’ receiving compensation” (p. 23). Understanding that railway spine compensation was a professionally charged topic, Buzzard ensured anonymity to those who submitted cases that resulted in court awards, going so far as to omit any patient details that could be traced back to the attending physician, like the geography of the train accident, the amount of the compensation, or any other remarkable details (Buzzard 1866a, Buzzard 1866b).

Despite his promises to ensure professional anonymity, Buzzard revealed in a third letter, dated February 17, 1866, that although he had received cases from “numerous correspondents” there was a conspicuous absence of cases involving speedy recovery after compensation. The supposed reticence of his fellow physicians to share these types of cases created an imbalance in his taxonomy. He wrote that the cases he received could be broadly classified under five headings:

1. In which serious symptoms of organic lesion have speedily supervened, and caused a fatal termination.
2. In which but slight movement has followed, and the patient has remained for years after the accident incapacitated.
3. In which the elapse of some days, months, or years has resulted in perfect recovery.
4. In which, at a more or less remote period, epilepsy, paralysis, or insanity has occurred.
5. In which, upon compensation being paid, the subjective phenomena have rapidly and entirely subsided. (Buzzard 1866c, p. 186)

The last class of cases proved to be the most difficult for Buzzard to aggregate. He wrote, “Medical men have naturally an objection to commit themselves to an expression of opinion which in some cases will amount to a charge of deceit against all alleged sufferers” (p. 186). Buzzard acknowledged that positively diagnosing an accident

survivor who manufactured the symptoms of the disease can be a source of personal and professional embarrassment for a physician. But the surgeon maintained that cases of the fifth type were medically significant and should not be mistaken as indications that physicians have been duped. He writes, “Our business as medical men is with the scientific, not with the moral aspects of the case, and these, I believe, can be recorded without any chance of placing the informant [the doctor who submits the case] in a position of discomfort or insecurity” (p. 186).

Buzzard’s most significant contribution to the study of railway spine was the connection he forged between injury, compensation, and prognosis. His attempt to taxonomize railway spine cases demonstrated his sympathies for physicians who were put in the position to offer diagnoses and prognoses that had legal bearing on a patient’s health and livelihood. Buzzard’s letters also modeled professionalism, demonstrating to readers an objective orientation toward survivors of railway accidents.

In his final letter of the series, dated May 25, 1867, Buzzard broke with the conventional medical positions on two of the most consternating aspects of railway spine. The first was that subjective symptoms – those ailments whose origins cannot be empirically observed on the body but are reported by the ill– are, in fact, actually disabling. He provided several cases in which subjective symptoms affected patients for the duration of their lives. Buzzard’s aim was to prove to his peers that subjective complaints should be included in medical testimony before the law. Even more radically, he returned to the issue of patients recovering fully after winning compensation claims for their injuries. He denied that recovery “is decisive proof that the alleged symptoms were factitious,” arguing “the settlement of the claim at issue leaves the patient for the

first time in a condition favourable for recovery” (Buzzard 1867, p. 623). Buzzard steadfastly defended patients, proclaiming, “It is a fact that mental anxiety alone is often sufficient to give rise to these phenomena [the subjective symptoms of railway spine]” (p. 623). He hedged this bold assertion of medical fact slightly when he forecast, “I shall have to quote examples of this” in a future installment of his series. The examples were never to come, though. The May 25, 1867 installment of his long essay would not fulfill its promise of “To be continued.” Whether his assertions were too bold for *The Lancet* is impossible to tell. The sympathetic surgeon lost his platform.

Buzzard’s efforts to categorize railway spine cases ultimately ended in failure. His failure to source information from his professional colleagues suggests that medical professionals may have been reticent to share cases where they were bested by opportunists manufacturing the symptoms of an invisible disorder. Or it may have been a sign that the malingering figure was a comparative rarity in the patient population. In either instance, Buzzard’s attempt to create a working document to guide physicians in the prognosis of railway spine cases revealed that the present was to soon a timeframe from which to assess patients.

### **John E. Erichsen and the Spheres of Railway Spine Deliberation**

In 1866, John E. Erichsen published *On Railway and Other Injuries of the Nervous System*, a slim volume based on a series of lectures he delivered to medical students attending University College Hospital (London) in spring of the same year. The book’s brief preface sets out the surgeon’s purpose, stating:

My object has been to describe certain forms of Injury of the Nervous System that commonly result from Accidents on Railways, to which I have reason to believe

the mind of the Profession has not been directed with that amount of attention which their frequency and the important questions involved in them, appear to demand. (Erichsen 1866, n.p.)

Like many physicians of the day, Erichsen attended to a number of victims of railway accidents and was dissatisfied by the lack of consistency with which his colleagues handled these patients. The concerns Erichsen introduced in the opening pages of his book demonstrated that he was acutely aware of the deleterious effects of railway spine cases on the medical profession and its prestige in the legal and public spheres. The surgeon attested that it is common medical knowledge that “ordinary accidents of civil life” “often of a trivial character” (p. 2) can instigate a progressively negative train of health symptoms that can lead to death. However, in the case of a “violent shock of a railway collision” “evidence of outward and physical injury” may not be immediately present, but the injury can cause organic lesions on the spine and “functional disarrangements entailed by them” (pp. 2-3). Erichsen’s task was to develop a method of reliably diagnosing the patients who developed functional disarrangements after surviving railway accidents.

To accomplish his task, Erichsen employed a rhetoric of disciplinary reform. That is, he instructed his primary audience of medical students and secondary audience of lawyers and lay people on the methods by which doctors could address and surmount controversy within their own ranks. Indeed, professional reform is a common theme of Erichson’s educational writings overall. He opened his 1874 textbook *On Hospitalism and the Causes of Death after Operations* by stating that medical art outstrips the capacity of medical science. Surgeons too focused on performing an artful surgery often lose their patients to infection from poor sanitation. He argued that shifting surgeons’



perspectives to understand surgery as an entire process rather than focusing on the discrete operation itself would improve survival rates and advance knowledge within the discipline.

Erichsen understood medicine as a dynamic profession consisting of individual practitioners who ply their craft by acting upon prior knowledge. For him, this understanding of the profession highlighted the distinction between art and science. In addressing the issue of railway spine to his audience of medical students, his most potent tactic was to portray practitioners of medicine as scientists rather than artists. The scientist worked with the knowledge created by colleagues and seeks to advance that knowledge in some regimented way. The artist, though, privileged his own experience and acted accordingly upon it. Not only were doctors split by art and science, they were cast in opposition to each other by the medico-legal nature of railway accidents. Erichsen argued that the invisible injuries caused by railway accidents divided members of the profession into two camps in the courtroom, those who testified on behalf of patients seeking compensation following accidents and those who testified on behalf of railway companies. The surgeon was bemused that two members of the same profession could reach contrary opinions so frequently that “a very undue amount of blame has been cast on members of the medical profession” (p. 19). In short, the highly public court proceedings of injured passengers taking action against railway companies were made more sensational by dueling medical opinions. The spectacle, as Erichsen frames it, exacted a toll on medicine’s standing in society.

In “The Personal, Technical, and Public Spheres of Argument: A Speculative Inquiry into the Art of Public Deliberation,” G. Thomas Goodnight (2012) offered

insights into controversies that cross realms of expertise. He used the term “spheres of argument” to “denote branches of activity—the grounds upon which arguments are built and the authorities to which arguers appeal” (Goodnight, 2012, p. 200). The sphere in which a controversy emerges dictates the standards of arguments pertaining to that controversy. It is apparent that a disagreement between friends has different grounds for argument than a professional dispute between doctors about the mechanism of a disease. In Goodnight’s formulation of argument spheres, the public realm, where law and policy are created and enforced, transcended personal and technical spheres. Yet, the public sphere may rely upon arguments from the technical and personal spheres to settle disputes. Although his article is an inquiry into the state of deliberative rhetoric in contemporary society, the principles he sets out are useful in understanding the relationship of medicine to the law and society in mid-nineteenth-century England.

Controversies pertaining to rail travel originated in both personal and public spheres. Travelers conducting personal business were injured in train accidents. The injury in and of itself remained a personal matter with the railway company until the traveler took legal action against the company. Once in court—the domain of the public sphere, only technical sphere arguments from physicians can validate the claim made by the traveler against the railway company. When the technical sphere is embroiled in controversy over the nature of the injury, as was the case with railway spine, then the courts must act in the public’s interest by “circumscrib[ing] the practice of technical argument” (Goodnight, p. 204). To prevent the courts from usurping the grounds for medical deliberation and knowledge creation, Erichsen devised a savvy rhetorical solution intended to manufacture consensus among physicians in technical arguments

pertaining to railway spine. He cast the controversies accompanying a railway spine diagnosis as an intraprofessional problem, effectively eliminating the influence of extraprofessional interests on the diagnosis of the disease. Reframing the matter as a conflict of professional opinion, Erichsen isolated the issue to the stuff that professional opinion is made of, namely personal experience (art) and disciplinary history (science). He created a dichotomy between the two, privileging the collective knowledge of medical practice over the knowledge that a physician created through his or her own subjective experiences with patients.

### **Railway Spine in History and Metaphor**

Considering that mid-nineteenth century British medical science relied heavily upon empirical observation as an engine of knowledge creation, Erichsen had to argue for an alternative means of objectively diagnosing railway spine. He accomplished this in two ways. First, he removed the railway from railway spine, arguing instead that the invisible illness was actually a varietal of a centuries old neurological condition called spinal concussion. Erichsen was not alone in his use of the term spinal concussion, but he was the only commenter of the day to use it as a rhetorical appeal to history. Skey (1857) and others in the field used the term as a matter of course. Erichsen's rhetorical innovation is that he changed terminology to divorce the symptoms from train accidents and to show that people had suffered from similar symptoms in the past. Secondly, he used compelling metaphors to help advance his theory of the mechanism that caused minor bodily injuries to result in major psychological symptoms.

In his lectures, Erichsen wove a medical history founded on the principles of English empiricism to persuade his audience that the recent phenomenon of railway spine was indeed an updated manifestation of a well-documented medical malady. He began with the obvious: that railway accidents are capable of producing shocks to the body that “ordinary accidents” ostensibly could not. He wrote,

The rapidity of the movement, the momentum of the person injured, the suddenness of its arrest, the helplessness of the sufferers, and the natural perturbation of the mind that must disturb the bravest, are all circumstances that of a necessity greatly increase the severity of the resulting injury to the nervous system, and that justly causes these cases to be considered as somewhat exception from ordinary accidents. (Erichsen, 1866, p. 22).

Erichsen noted that the term “railway spine” was coined in response to the ostensibly unique connection between railway accidents and psychosomatic suffering of injured travelers. The connection, though, was tenuous because the exact same condition was described by surgeons in England and France “a quarter of a century or more before the first railway was opened, and that they [the symptoms] were then generally recognized by surgeons as arising from the common accidents of civil life” (p. 23). What gives “railway spine” the appearance of a unique medical condition is its frightful etiology, the frequency of train accidents and the sheer number of victims they yield. A single train accident could injure many more people than most “common accidents of civil life.” Volume, combined with the frequency of accidents as well as their severity equated to a public health threat that caused concern in both the personal and public spheres.

Erichsen’s history lesson began with a case of “a palsy occasioned by a fall, attended with uncommon symptoms” related by Dr. Maty in the third volume of “Medical Observations and Inquiries” in 1766, a full century before his lecture. In full

bravado, Erichsen explained, “I feel that I need no apology for giving you an abstract of it here, although, as it occurred between sixty and seventy years before the first railway was opened in this country, it might first appear to have less relation to railway accidents than it really has, for it is identical in its course and symptoms with many of them” (Erichsen, 1866, p. 24). Erichsen summarized Maty’s lengthy account, which I will convey here in brief. While traveling to rejoin his regiment, the Count de Lordat, a high-ranking French military officer, was injured when his horse-drawn carriage tumbled down a steep embankment. Although he wrenched his neck, hit his head on the carriage’s ceiling, and bruised his left shoulder, arm, and hand, the Count emerged from the wreckage and walked several miles to the next town. Six months after the accident, after rejoining his regiment and conducting a military campaign, the Count developed a stutter and weakness in his left arm. After another military campaign, he was obliged to separate from the army because he developed involuntary convulsions all over his body. No treatments improved the Count’s condition; he continued to waste away. Nearly four years after his accident, the count died.

With the Count’s medical history laid out before his audience, Erichsen averred, “This case is of the utmost interest and importance; and though it occurred more than a century back, and was published exactly one hundred years ago, it presents in so marked a manner the ordinary features of a case of ‘concussion of the spine,’ arising from injury, that it may almost be considered a typical case of one of those accidents” (p. 25). To provide further evidence that railway spine was indeed the same ailment as concussion of the spine, the surgeon detailed the major ways that the two disorders were one in the same:

- 1<sup>st</sup>. That there was no evidence of blow upon the spine—merely a twist of the neck in the fall.
- 2d. That no immediate inconvenience was felt, except from the bruise on the shoulder and hand.
- 3d. That the patient was able to walk a considerable distance, and to continue his journey after the occurrence of the accident.
- 4<sup>th</sup>. The symptoms of paralysis did not manifest themselves for several months after the injury.
- 5<sup>th</sup>. They were at first confined to the left arm and to the parts of speech.
- 6<sup>th</sup>. They very slowly but progressively increased, extending to the left leg and slightly to the right arm.
- 7<sup>th</sup>. This extension of paralysis was very gradual, occupying two or three years. The sphincter were [sic] not affected, and the urine was healthy.
- 8<sup>th</sup>. The general health gradually but slowly gave way, and death at last ensued, after a lapse of four years, by a gradual decay of the powers of life.
- 9<sup>th</sup>. After death, evidences of disease were found in the membranes of the cord itself. The narrator of the case stating that the membranes were primarily, and the cord secondarily, affected. (Erichsen, 1866, p. 26)

With the addition of a few more cases to buttress his comparison, Erichsen removed all doubt of the reality of railway spine by changing both the terms and the timeframe for the invisible disorder. Through careful comparison, Erichsen's disciplinary rhetoric provided railway spine with a history to legitimize the contemporary problem. Nineteenth-century victims of train collisions joined the category of people who became mysteriously ill from apparently minor accidents. Additionally, medical inquiry into these kinds of patients, such as Buzzard's patient categories, no longer fell under the auspices of railway spine. Rather, it was a continuation of extant research on the established category of spinal concussion. Most importantly, Erichsen's historical rhetoric uncoupled contemporary railway spine from the medico-legal sphere of deliberation. As victims with spinal concussion, railway accident survivors suffered from a malady that was untainted by potential claims to compensation. Instead of being the birthplace of railway spine, in Erichsen's analysis the court became a venue that intruded upon medicine and

its historic research into spinal concussion, as well as its documented body of knowledge on the condition.

Erichsen's ontology of spinal concussion as a pre-Industrial Revolution injury demonstrated to his audiences that railway spine was indeed an object of medical knowledge before its association with compensation claims and legal proceedings. Yet, the cases he selected did little to provide scientific evidence of the mechanism that caused the symptoms associated with spinal concussion. That is, how did the jolt to the spine manifest itself as the symptoms of spinal concussion? He argued that in some, but not all, cases of spinal concussion, autopsy revealed inflammation of the spinal cord. Since there was no method of examining the spinal cord pre-mortem in 1866, Erichsen had to explain how a stage coach accident or train derailment could result in some passengers developing a particular set of symptoms that could be objectively identified as spinal concussion. To explain the phenomenon of spinal concussion, Erichsen selected two apt and familiar metaphors to account for the mechanism behind spinal concussion.

Since spinal concussion was the product of a shock to the body, Erichsen had to account for the difference between the shock a person sustains from a broken limb and a shock to the spine that is "followed by the train of evil consequences" (p. 73). He argued that the violence necessary to fracture or dislocate a limb often leaves the brain and spine unaffected. Conversely, a minor jar could damage delicate nerve structures. He substantiated this claim by comparing the body to a watch, a familiar technology in the nineteenth century. A body sustaining a blow is the same as a watch falling to the ground. He explained, "A watchmaker once told me that if the glass was broken, the works were rarely damaged; if the glass escapes unbroken, the jar of the fall will usually be found to

have stopped the movement,” hence breaking the watch (p. 73). It wasn’t a perfect metaphor: the “works” of the human body are more complex than the gears that move the hands of a chronometer. And, of course, the parts of a watch could be repaired or replaced. However, the comparison of the watch to the body was apt for reasons extending beyond his audience’s familiarity with dropped watches. A dropped watch with an intact lens is indistinguishable from a watch that was wound down from disuse. The two could only be differentiated upon winding because one is a mechanism without any mechanical energy, while the other regardless of winding will not operate. The reason it could retain energy or convert energy into movement was a matter for an expert to decide.

Erichsen’s hypothesized that the slights of jars to the spine could mysteriously sap the body’s nervous energy. After sustaining a blow from a fall, a pocket watch could either be broken or be made to look broken. Recall that it was Skey’s (1857) contention, in which he was by no means alone, that sufferers of railway spine were either exaggerating or manufacturing nervous symptoms in hopes of personal gain. Here Erichsen exchanged his rather practical metaphor of the dropped watch for a more wondrous metaphor that hinged upon scientific ignorance instead of scientific knowledge.

Fahnestock’s (1986) study of scientists’ accommodation of scientific knowledge to public audiences found that scientists writing for nonexpert audiences tended to employ wonder and applicability as rhetorical appeals. The appeal to application emphasizes the utility and benefits of a scientific concept for a particular audience. On the other hand, the appeal to wonder celebrates the awe that can accompany a scientific



phenomenon. Erichsen here capitalized on the awe science can inspire as well as the futility of scientists to explain its awesome displays. To bolster his comparison of human bones to glass and the central nervous system to the escapement of a pocket watch, he then compared the human body to a magnet. “We do not know how it is that when a magnet is struck a heavy blow with a hammer, the magnetic force is jarred, shaken, or concussed out of the horse-shoe” (p. 73). With the metaphor of the magnet, Erichsen invoked all of the facts of science that remain unexplained. Not only is the body like a piece of iron, it also contains a mysterious vivacious force: “So, if the spine is badly jarred, shaken, or concussed by a blow or shock of any kind communicated to the body, we find that the nervous force is to a certain extent shaken out of the man, and that he has in some way lost nervous power” (p. 73). Therefore, Erichsen surmises, the shock caused a change at the molecular level to the structure of the spinal cord. As for the unfortunate recipient of the blow, Erichsen noted “*that there has never been an interval of complete restoration to health*” of any patient who suffered from spinal concussion (p. 83, emphasis in original).

In these two evocative metaphors, Erichsen set out the vague etiology, mechanism of action, and prognosis that explained every possible case of spinal concussion. His metaphors, rather than his science, drove home the point that the most innocuous of blows could set into motion a series of symptoms that eventually lead to death. The event and an explanation of how it affected the individual were of the utmost importance for Erichsen because the symptoms accompanying spinal concussion were numerous and irregular from patient to patient. In fact, assessing the symptoms themselves would most

likely not result in a diagnosis of spinal concussion lest the physician know that the patient sustained a jar, shock, or blow.

Many of his contemporaries in medicine took issue with Erichsen's book for myriad reasons, but on both sides of the Atlantic Ocean, lawyers representing survivors of railway accidents and the general public seized upon Erichsen's explanation of railway spine.

### **The Role of the Physician in Erichsen's Medico-legal Rhetoric**

*On Railway and Other Injuries of the Nervous System* (1866) stands apart from other medical writings on railway spine because Erichsen made two explicit rhetorical moves that his colleagues almost explicitly avoid. The first move is that he explicitly condemns railway spine settlements that he believed were insufficient compensation for accident survivors. Secondly, since railway spine was a diagnosis of chief importance in legal proceedings, Erichsen encouraged uniform and thorough examinations of each patient, as if each case were to be tried by law. In making these two moves in his lectures and subsequently in his book, Erichsen countered discourses associating railway spine with malingering by portraying the accident survivor as a victim deserving of a settlement that would replace his or her previous income until death.

### *William Camps and Miss -----*

In 1866, the same year *On Railway* was published, William Camps, a physician and surgeon who previously published books on epilepsy and hysteria, reprinted "a paper quite recently read at one of our metropolitan medical societies" as *Railway Accidents or*

*Collisions; Their Effects, Immediate and Remote*. Unfortunately, the eighteen-page pamphlet's vague publication information makes it impossible to know if Camps' text predates Erichsen's, but there is nothing in the text that would indicate that the pamphlet was intended to be a response to Erichsen. The bulk of Camps' paper rehearsed claims and opinions on railway spine that were already circulating in the medical press. For example, echoes of Buzzard's call for a taxonomy of railway spine can be heard in Camps' statement, "I am of the opinion that a carefully collected, and well-arranged record of these cases of injuries from railway accidents or collisions, and more especially, if they can possibly be followed out, or kept under medical observation for some length of time" (Camps, 1866, p. 6). Beyond offering his assent to contemporary conversations on railway medicine, his paper is novel insofar as it sets out a rational populist argument for medicine to improve its knowledge of railway spine. In particular, Camps offered an argument appealing to the public's health, stating that railway spine could be "worked out to some extent in regard to the science of preventive medicine" (p.8). He wrote, "the travelling, and more especially the shareholding public, should be taught by the medical profession, that a railway accident or collision is no trifling matter to the health, the life, and the limbs, of Her Majesty's subjects; who when sick or injured, from any causes whatever, fall under our professional care and superintendence" (p. 8). Camps demurred from extending his criticism to those parties who doubt the existence of railway spine, opting instead to group the three classes of people with stakes in railway spine (railway company shareholders, accident victims, and doctors) under the big tent of the "great public at large" (p. 8). Railway accidents were a national scourge, and Camps wanted to make railway spine a national issue.

As compensation for injury was one of the more divisive issues related to spinal concussion, Camps broke ranks with other commentators who either associated compensation with opportunism, like Skey, or avoided the subject altogether, like Buzzard. Instead, Camps endorsed a type of compensation that was awarded on a case-by-case basis. For example, he cited one patient who received £7,000 in damages (a very large settlement for its day). Camps wrote that the sum was not at all too high, “considering the position in life of the unfortunate sufferer” (p. 14). In another example, he endorsed a settlement of £1,200 for a “young lady residing at Brixton with her father, who is a retired officer in the East India Company’s Service” (p. 14). Although the young lady’s doctors determined that she was unlikely to recover from a “permanent and progressive disease of the spine, produced by the accident” (p. 14), Camps made no comment about her receiving a fraction of the previous claimant’s settlement. By only vaguely addressing the issue of compensation, Camps missed the opportunity to better define the relationship between medicine and the law, maintaining the status quo of medicine taking a servile position in matters of litigation.

On the contrary, Erichsen’s medico-legal rhetoric suggested that medical experts should have a role in determining the compensation a claimant receives. He demonstrated this in the case of 28-year-old woman whom he identified only as Miss ----. On June 9, 1865, a portion of the bridge Miss ----’s train was crossing collapsed, and the train plummeted into a stream. For two and a half hours she was buried beneath the wreck, “another lady, a fellow-passenger [sic], who had been killed, being stretched across her” (Erichsen, 1866, p. 65). Nearly eleven months later, when Erichsen visited her, Miss ---- had almost completely recovered from the twisted neck she sustained in the accident, but

her left arm was paralyzed. She brought legal action against the railway company, but Erichsen wrote “as she had sustained no pecuniary loss by the accident, she was only awarded the wretched ‘compensation’ of £1,350” (p. 68). In comparison to the other cases Erichsen conveyed in *On Railway*, the surgeon was most indignant when discussing this case, which he concluded with the observation: “Mental sufferings, bodily pain, and disability, and complete annihilation of the prospects of a life, weigh lightly in the scales of justice, which have only made to kick the beam by the burden of the actual money loss entailed by the accident ” (p. 68). For the first time in the medical literature on railway spine, a physician suggested that a patient should be compensated for having experienced a traumatic event.

In *The Empire of Trauma*, Fassin and Rechtman (2009) postulate that the concept of trauma shares its genealogical origins with the legal condition of victimhood. In their frank and accurate assessment of late nineteenth century psychiatric medicine, they identify financial compensation as the driving force behind advances in medical knowledge of trauma. What set trauma apart from other neuropsychological disorders was that “unlike all other forms of mental illness—where the etiological agent, although it might vary over time and with different theories, is never an external party that can be prosecuted—trauma neurosis offered grounds for suggesting a right to compensation, given the nature (albeit undefined) of its causal agent” (p. 35). For a physician or surgeon to assert that a patient’s neuropsychological injuries are the fault of a train accident, he would need to be prepared to comport himself to the practices of the emerging insurance industry (p. 35) and to battle the railway surgeons who offered expert (and usually counter) testimony in court. Since *On Railway* is to some extent the product

of Erichsen's own courtroom battles to force railway companies to compensate the injured, including those who presented no physical signs of injury, it seems reasonable that he would equate medical testimony with advocating for patients.

Erichsen's text demonstrated his beliefs that malingerers can be easily discovered before court appearances through routine and standardized examination. Furthermore, a rhetoric of patient advocacy permeated Erichsen's other writings, especially those on hospital reform (Erichsen 1874). His commentary on Miss ---'s financial recovery was not beyond the scope of physician in Erichsen's medico-legal rhetoric. Rather it reinforces the tacit understanding that a patient with a claim against a railroad company was once already a victim of that company, and it was the doctor's duty to legally avert a second victimization. Trauma, as Fassin and Rechtman (2009) observe, is an argument for compensation. Erichsen used the historical record to demonstrate the reality of railway spine. Accordingly, he instructed his audience on the ways in which a doctor should comport himself while assessing the patient and defending those assessments in a legal setting. Lastly, and most tellingly, he argued that medicine, when working in conjunction with the law, must take on the role of social advocate for the compensation-seeking victim.

Erichsen's medico-legal rhetoric was, of course, overly optimistic. Responses to *On Railway* demonstrate that mostly lawyers and general public took Erichsen's side. His medical colleagues bickered incessantly over his theory of spinal concussion for decades. None were more critical of his work, or more eager to profess a rival history of spinal concussion than the surgeons railway companies employed.

## Responses to Erichsen

The reception of *On Railway* accentuated the boundaries between medicine, the law, and the public. Reviews of the book in legal journals and the popular press extolled the virtues of Erichsen's tract. A review in Britain's *Quarterly Journal of Jurisprudence* (1866) praised the "utmost practical skill and judgment [that] marks every page" of Erichsen's book (p. 344). The unnamed reviewer recommended the book for curious rail travelers and barristers interested in "the trials and disputed points in medico-legal and arbitration cases so frequently brought before public tribunals" (p. 343). Newspapers on both sides of the Atlantic Ocean adroitly condensed Erichsen's main points into a few column inches, enough to distill the symptoms and the immanent fate of the passenger who falls victim to the "new disease." Few American reviews offered more than Erichsen's name, omitting the book and its title entirely. As in the case of *The New-Orleans Times* (1866), many papers seized upon the enduring image of the man who "escaped from a railway smash with no apparent injury" to find that "he is not the man he was" and has "lost bodily energy, mental capacity, [and] business aptitude" (p. 10). For the next five decades, newspaper articles on railway spine would appear sporadically. The December 28, 1912 *Duluth News Tribune* published a court report under the title "Woman Made Old Gets \$16,960." The Supreme Court of Illinois awarded Mrs. Mary Shaw the sum after medical experts deemed that her health was "wrecked" after the Rock Island train she was riding derailed in 1910. Medical experts presented three key pieces of evidence to support the diagnosis. Mrs. Shaw's "jet black hair" turned gray, her "face became ashen and she had all the appearances of aged woman," and she lost 108 lbs., wasting from 212 lbs. at the time of the accident down to 104 lbs. at the time of the trial.

Public and legal discourse on railway spine may have remained unchanged in the half century leading up to World War I, but many of Erichsen's contemporaries were quick to voice their disapproval in the pages of medical journals and books of their own. In assessing the primary sources on railway spine, it appears that *On Railway* shifted the publication venue for medical knowledge and debate on railway spine from discipline-specific journals to books. While the books retained the conceit that they were written for physicians, it is tempting to speculate that the authors of books written in response to Erichsen's hoped to attain the same notoriety. It would not be until some twenty years later that railway surgeon Herbert Page would shift the conversation about spinal concussion into a new area of medical research. In the interim, though, it appears that Erichsen provided closure to the legal and public spheres while exacerbating tensions within the medical profession. Indeed, some commentators of the day cast medicine as a practice caught in a tug of war between the public trust and the individual patient. John Charles Hall, senior physician to Sheffield Public Hospital, aligned medicine's interests with those of the railway companies. He writes, "a physician forgets what is due to himself and the public, when he undertakes to become an *advocate* instead of a *witness*" (Hall, 1867, p. 7 emphasis in original). Physicians who encountered potential cases of railway spine had to negotiate the rhetorical formation in which the knowledge of the disease circulated while maintaining the guise that they were balancing the interests of all the railways' stakeholders.

Where surgeons like Camps (1866) perpetuated the belief that all of the queen's subjects were responsible, to some extent, for the respective fates of the railway companies and the victims seeking compensation from them, an entire subdiscipline of



medicine, known as railway surgery emerged. This specialty area was comprised of physicians and surgeons who adamantly defended their employers and saw themselves as defenders of the public interest against malingerers, opportunists, and the doctors who championed their causes. The most outspoken of the railway surgeons in the late nineteenth century was Herbert W. Page, surgeon to the London and North-West Railway (Trimble, 1981).

### **Herbert Page's Contributions to Railway Spine**

Histories of railway spine have seized upon the ostensibly contentious exchanges that took place in medical texts and journals from the 1850s to the 1900s. Without a doubt, personal squabbles sufficed for professional dialogue in some publications of the day. In retelling the story of railway spine, John Erichsen and Herbert Page have been cast as mortal enemies dueling with irreconcilable ideas of the effects of physical trauma on the psyche. Trimble (1981) positioned Page as “An Alternative” to Erichsen in the second chapter of his history of post-traumatic neurosis. His book coincided with the APA's adoption of PTSD as a diagnostic entity in its DSM-III. Trimble's work was the first to trace the arch of post-traumatic psychological disorders to a time before the publication of Sigmund Freud's (1895) *Studies on Hysteria*.

Trimble positioned Page's (1885) *Injuries of the Spine and Spinal Cord without Apparent Mechanical Lesion* as “essentially a rebuttal of the ideas expressed by Erichsen” (Trimble 1981, p. 21). Trimble (1981) accurately recognized that Page relentlessly rebuked the key concepts of spinal concussion as Erichsen conceived it.

Foremost among these contentions was that it was extremely difficult for an accident to damage the spinal cord without harming the spine itself.

Yet, Page had to explain the mechanism by which a passenger could survive an accident unscathed only to later develop the symptoms of railway spine. Recall that Erichsen attributed the constitutional change in the patient to undetectable molecular disturbances of the spine that depleted the patient's nervous energy. Page's explanation hinged upon the idea that fear itself could shock the nervous system into causing the body to behave as if it had sustained a physical injury. He demonstrated how the process works with the case of a railway employee who survived a near-death accident unscathed:

How largely fright may of itself conduce to the condition recognized as shock is well shown by a case communicated to us by a surgeon of large experience, who, summoned to a railway station to see and conduct to the hospital a railway servant who had had his foot, as was supposed, run over on the line, found him in a state of collapse, and in greatest alarm as to the injury to his limb. Upon examination it was discovered that the only damage was the dexterous removal of the heel of his boot by the wheel of a passing engine. And medical literature abounds with cases where the gravest disturbances of function, and even death or the annihilation of function, have been produced by fright and by fright alone. (Page 1885, p. 162)

Like Erichsen, who struggled to explain how railway accidents were the similar to and yet different from accidents of everyday civil life, Page theorized that the element of fear played a significant or "in many cases the only" (p. 162) role in the etiology of post-traumatic symptoms.

Rather than substantiate his case for a psychical model of trauma by delving into the medical archives, Page built his case on contemporary surgical works. He cited at length John Furneaux Jordan's (1880) *Surgical inquiries: Including the Hastings Essay on Shock, the Treatment of Surgical Inflammations, and Clinical Lectures*:

The principal feature in railway injuries, ... is the combination of the psychical and corporeal elements in the causation of shock, in such a manner that the former or psychical element is always present in its most intense and violent form. The incidents of a railway accident contribute to form a combination of the most terrible circumstances which it is possible for the mind to conceive. The vastness of the destructive forces, the magnitude of the results, the imminent danger to the lives of numbers of human beings, and the hopelessness of escape from the danger, give rise to emotions which in themselves are quite sufficient to produce shock, or even death itself. .... All that the most powerful impression on the nervous system can effect, is effected in a railway accident, and this quite irrespectively of the extent or importance of the bodily injury. (qtd in Page, 1885, p. 163)

Page's approach to theorizing the underlying mechanism in railway spine took into account the rhetorical formation of the diagnosis itself. The question of how fear and the injuries it produced could be compensated arose in medico-legal circles. Although a railway surgeon himself, Page attempted to incorporate medical perspectives from other specialties and other countries into his theories. In 1881, he won Harvard University's Boyleston Prize for the essay "Injuries to the Back without Apparent Mechanical Lesions, in their Surgical and Medico-Legal Aspects," which served as the basis of his first book (Caplan 1998, p. 17). In his second book, *Railway Injuries with Special Reference to Those of the Back and Nervous System*, Page mentioned the research of American and German neurologists and dedicates the text to J. M. Charcot.

While Page's work explicitly took aim at Erichsen's concussion of the spine, it also opened up railway spine to the theories and treatments for other psychical ailments that were being practiced around the world. As historian Eric Caplan (1998) observed, participants in cases involving the medico-legal aspects of railway accidents could pick and choose from competing theories of railway spine. He quotes American neurologist Charles Dana who surveyed the medico-legal landscape, thusly:

The physician who is called into court to testify in a case of spinal injury witnesses a curious spectacle. The lawyer for the prosecution waves before the jury a volume of "Erichsen Upon Spinal Concussion." He reads to them, in impressive accents, the statement that every injury to the spine, however slight, is full of danger to the sufferer. He asks, with sonorous emphasis, if Mr. Erichsen is not a surgeon of world-wide fame; and if he does not say that slight injuries to the back may cause chronic spinal disease of the most serious character. He sneers at the work of a certain Mr. Page, who is known to be professedly only a railway surgeon. He shows that his client has paralysis, anemia, meningitis, in fine, "spinal concussion." On the other hand, the lawyer for the defense brandishes triumphantly a larger work, by Mr. Herbert Page, on "Injuries to the Spine"; he reads to the jury cases of malingering therein related, shows that Mr. Erichsen has for years made a business of being an expert for people with injured spines, but that he has never yet found a case that proved fatal. He quotes Mr. Page's two hundred and thirty four cases of spinal concussion, in most of which recovery resulted, and shows, through his medical expert, that the spinal cord is so admirably protected that it could never possibly be injured by anything so utterly trivial as a railway collision. (Dana [1884] qtd in Caplan [1998])

Another American neurologist, Knapp (1888) echoed Dana's (1884) Erichsen-Page binary and at the same time credits the two with opening up an expansive area of research. Knapp's assessment of the field and its history was curt: "It is rather singular that the two most elaborate works on those affections of the nervous system which are supposed to follow injury should have been written by surgeons, and should have been based on the evidence of railway cases" (p. 621). It is unclear if Knapp, who is arguing for railway spine to fall under the disciplinary jurisdiction of neurology, knew that his English colleagues were loathe to create a new specialty from railway cases. He could not deny that the "influence" of Erichsen's work was "not yet dead" in the courts or that Page's treatise "reads like the work of a special pleader for the railway companies" (p. 621). More importantly, the circulation of these texts can be credited with "many valuable contributions to our knowledge ... in this country [the United States] and in

Europe, and the work done in Germany, especially, has brought the matter more fully to our attention” (p. 621).

The proliferation of knowledge about railway spine, not to mention the numerous appellations given to the condition, created a medico-legal problem for doctors and lawyers alike. In 1904, Allan McLane Hamilton, a physician with more than thirty years experience with the legal aspects of railway accidents, published *Railway and Other Accidents with Relation to Injury and Disease of the Nervous System: A Book for Court Use*. Hamilton’s text synthesizes nearly fifty years of medical opinion on post-traumatic conditions, or “accident aboulia” as he calls them, across America and Europe. His aim was to offer practical help to anyone entering the court in relation to an accident. This new genre of reference book, created to tame trauma and the knowledge created about it, would result in even more ambitious attempts to synthesize medical opinion for the courts, thus cementing medical jurisprudence into place as one of the cornerstones of law and civil life in the industrial age.

### **The Issue of Compensation**

Regardless of advances of in the medical science of railway spine, the discourse of malingering endured in the medical literature. Yet, like the nosology of railway spine, the issue of compensation evolved as physicians attempted to theorize the role that money played in the presentation of railway spine symptoms, as well as the prognosis of the patient. At stake in the evolution of compensation’s role in a patient’s recovery is the very issue of scientific observation itself.

In his treatise on the nature of medical evidence in railway accidents, Hall (1868) outright rejected the validity of patients' subjective descriptions of their wounds. He wrote, "I have never yet seen a case of injury, the result of a collision on a railway, where the symptoms have been *subjective only*, in which the favourable verdict of a jury, and the payment of the damages awarded, has not been followed sooner or later, (generally very rapidly,) by complete restoration to health and strength" (Hall 1868, pp. 18-19, emphasis in original). But even Hall himself could not uniformly abide by his monolithic pronouncement. On the page before his indictment of patients who present only subjective symptoms, he attested that "after a shock to the nervous system, symptoms altogether *subjective* are often present, similar to what we note in *real* affection of the nerve-centres" (p. 17). He continued, "This state of things I have very frequently seen after railway accidents, and that, too, in ladies, and in those who have no wish nor intention to deceive"; however, he is certain that the "absence of *objective symptoms* in such cases will always enable us to form a correct diagnosis, and to decide between real and imaginary spinal disease" (p. 17).

For Hall, and many of his contemporaries, a railway accident was not a crime of negligence on behalf of a railway company or an act of God. Rather it was a moral litmus test for the survivors. Only doctors and their methods of examination could read the state of the body through a person's moral habits and "probable motives" (p. 17). The malingerer was a doubly insidious personage because he or she attained a monetary reward for duping medicine and the law. In Hall's calculus, morally upright victims never exaggerated their symptoms or overstated the significance of their injuries. Although he never mentions the concept of justice in remuneration, his moral equation is balanced on

the proposition that the honest victim receives precisely his or her just desserts without ever having to fear being short-changed by a railway company.

Despite his certitude that physicians can detect the trace of a wound in an accident survivor's morals and motives, Hall advocated for the use of emerging technologies to observe minute changes in the body that correlate to central nervous system damage. Poor vision and paralysis were symptoms of railway spine. Both, however, were empirically detectable. The ophthalmoscope could aid a physician in determining the presence of any "changes in the optic nerve" (p. 12). In cases of paralysis, a thermometer could be used to compare the temperature of the affected area to the rest of the body. A lower reading at one extremity, often accompanied by a weakened pulse in that region of the body, signaled damage to the area. Hall was so impressed by the utility of these devices in forming correct diagnoses of railway spine that he posited, "The day cannot be far distant, when the use of them will be taught as generally in our Medical Schools, and Hospitals, as is now the use of the Stethoscope" (p. 52). Other practitioners of the era and afterward made similar claims about technologies and their capacities to accurately diagnose railway spine, such as Dercum (1895) on the ability of autopsy to detect lesions to the spinal cord and Carruthers (1923) on the diagnosis of spinal concussion by examining the patient with x-rays.

Not all members of the medical profession were convinced that technologically-mediated empirical observation was the correct paradigm for understanding the role of compensation in a patient's recovery from railway spine. In the latter half of the nineteenth century and into the twentieth century, governments from the United States to continental Europe encoded legal regulations on employers' liability and workmen's

compensation. Indeed, legal entitlement to remuneration exacerbated the discourses of malingering that circulated in the medical profession and in society. Medical texts on malingering, which were usually reserved for physicians affiliated with the military, saw a wider audience (Trimble, 2004). Gavin's (1843) *On Feigned and Factitious Diseases, Chiefly of Soldiers and Sailors* was the earliest book on the subject, but after the publication of Erichsen's (1866) *On Railway*, virtually every book on railway medicine or spinal concussion included a chapter on malingering. The term "compensation neurosis" was coined by a German psychiatrist in 1879 after Prussian laws changed in 1871 to permit compensation for invalidism following railway accidents (Trimble 2004).

The ubiquity of compensation in legal codes, perhaps combined with the legal authorization of personal gain for enduring a socially reprehensible experience, traces along with a shift in the way that some doctors viewed patients who survived train accidents and the symptoms they manifested. Buzzard (1867) may have been ahead of his time in suggesting that monetary compensation for an injury could alleviate the debilitating anxiety over financial matters caused by an accident. As previously mentioned, his research into railway spine was cut short before he could make any sound conclusions in his serial in *The Lancet*. In America, two other theories of the relationship between compensation and neurosis emerged in the late 1880s. In response to Knapp (1888), Dr. L. C. Gray offered an explanation based on the then-new concept of psychological suggestion—Charcot and Page's preferred mechanism for psychological trauma—and material medicolegal factors. To preserve the internal logic of his explanation, it deserves to be quoted at length:

So soon as a person was injured [in a railway accident] he was besieged by runners for legal firms. By means of these runner and the lawyers he was



impressed with the danger to which he had been subjected. The suit then followed, running on for two or three years, during which time the patient could not afford to get well, as he would thus become liable to a suit for conspiracy. Finally, after having kept up the disease for he tow or three years during the long trial, habit would prolong it for at least three or four years after. (Knapp 1888, p. 649)

Gray's fellow respondent, Dr. Zenner, further exculpated malingerers by citing that fact that "hysterical symptoms," like tremors, often accompanied organic diseases, like lead poisoning. He states, "Deception on the part of the patient should not ... lead [to] the exclusion of other serious disease" (Knapp 188, p. 650). Although Zenner's theory too boldly likened hysterical symptoms to acts of deception, his ideas indicated a softening of the connotations associated with deception. In fact, the act of feigning disease could be beneficial to the diagnosing physician in Zenner's calculus because "Deception in itself presupposed disease" (p. 650). The act of acting ill signified an actual illness. The shift in medical professional's attitudes toward malingering demonstrated that legal acceptance of a disease can change the underlying medical theory. The legal code assisted in persuading physicians to accept the idea that somatic symptoms—even if totally manufactured—could serve as biomarkers psychological illness (the somatic or neurological indicators of mental illness), which in turn would reveal a different ailment altogether in the body.

## **Conclusion**

Nearly 150 years after railway spine was first mentioned in the pages of *The Lancet*, the Victorian-era malady was once again a topic in the journal. This time, however, it appeared in a column bearing the title "Discarded Diagnoses," a time capsule piece offering the barest of facts about the diagnosis while championing the narrative of

medical progress. In the concluding paragraph, Bynum (2001) writes, “Historians, who generally don’t have to see patients or appear in court, like to see railway spine as part of the historical continuum that stretches through shell shock to post-traumatic stress disorder. But, then, most of them have never been in a railway accident, either” (p. 339). Sardonic tone aside, these sentences raise an important issue that this chapter has attempted to reckon with. That is, how does history operate as a rhetorical device in complex discussions of disease? And how is the rhetoric of history made into an actor in contemporary discussions of disease?

Bynum (2001) was written to be a tidbit of historical curiosity. It was published July 28 a vacation week in the United States and United Kingdom, when readership is likely lower than in non-summer months. A few weeks later, though, the United States suffered the terrorist attacks of 9/11, and thousands of survivors and family members of the dead took to the courts in hopes of recovering damages for lost lives, physical wounds, and psychological injuries. Bynum (2001) quoted William Osler’s wry nineteenth century remark, “In railway cases, so long as litigation is pending and patient is in the hands of lawyers the symptoms usually persist. Settlement is often the starting-point of a speedy and perfect recovery” (p. 339).

The notion of a “discarded diagnosis” may be too easily dismissed in Bynum’s (2001) piece. Discarded is a term laden with the reasons for which a thing might be relegated to the dustbin. In poking fun at Victorians for their fixation on railways as causes of disease, a reader may dismiss railway spine as a quack disease, a product of bad medicine and incomplete scientific understandings that were faulty from the beginning. Or, in a more productive light, we can understand railway spine as a useful diagnosis

whose moment has passed because it was very much the product of the circumstances and cultural formations that persisted in the latter half of the nineteenth century. In the rhetorical history of PTSD, railway spine marks the moment when in concert medicine and the law decided that trauma was a fact of civil life. Aspects of that formation, in the form of civil rhetorics of malingering, victimhood, and remuneration and medical rhetorics of patient surveillance, biomarkers, and the mind-body connection have become persistent features of the discourses of post-traumatic diagnoses.

In this chapter, I have traced the rhetorics produced in the deliberations on the codification of railway spine. Despite Bynum's critique of historical approaches to psychological trauma (which is itself an argument for disciplinary isolation akin to Erichsen's), railway spine is the starting point for historical discussions of PTSD because it was the first formally codified diagnosis that was used to explain a cultural phenomenon as the phenomenon occurred. It is a pertinent starting point for this dissertation because the rhetorical themes and practices introduced in the codification of railway spine persist in the following chapters. These themes include how do doctors use rhetoric to create knowledge about a disorder and have that knowledge verified by institutions? When does a social problem reach the point that it requires medical intervention? How do doctors interpret social problems and codify them as diagnoses? How does history function in the codification of a post-traumatic diagnosis? As outdated as railway spine is in the twentieth century, the themes generated during this episode in the rhetorical history of PTSD endure to this day.

Where railway spine was an accident of civil life, the following chapter moves to the battlefield to examine the rhetorical formations of shell shock during World War I

and their enduring influence on the discourses of our contemporary understandings and treatment of military mental health. It was during this period, that psychiatry emerged to recodify trauma as a wound to the mind rather than to the body.

## Chapter 3: From Shell Shock to War Neurosis

### **Introduction**

World War I was a conflict of unprecedented magnitude. From July 28, 1914 to November 11, 1918, virtually all the world's economic powers aligned on two opposing sides to wage a highly mechanized "total war" (Chickering and Forster, 2003). Over the course of hostilities, 70 million military personnel would participate in the global war, with approximately 9 million combatants dying in the conflict (Keegan, 1999). From the United Kingdom alone, 5.7 million men served in the military, of which 761,000 were killed and 1.2 million were wounded (Gilbert, 1994). From the time the British Expeditionary Force (UK's army) landed on the European continent to Armistice Day, 80,000 men suffered from shell shock (Shephard, 2001). Passels of psychiatric casualties amassed on both sides of the war, and each warring nation developed its own practices for treating and/or punishing soldiers who had broken down in combat (see Lerner [2003] for a history of war neurosis in WWI Germany).

Psychological war casualties first became a concern for the British government in December 1914, when the War Office in London received reports that large numbers of soldiers and officers in the British Expeditionary Force (BEF) were being evacuated from European front lines with nervous and mental shock. It was estimated that seven to ten percent of BEF officers and three to four percent of all other ranks were being returned to the United Kingdom for mental breakdown (Johnson & Rows, 1923, pp. 1-2). As one British neurologist reported, in October 1914 alone, the BEF fighting the Germans at Ypres lost a third of its fighting strength, with a considerable portion of those casualties

succumbing to paralysis from the shell fire or exhaustion from the grueling conditions of trench warfare (Shephard, 2001, p. 21).

Against this backdrop of military concern for maintaining a fighting force on the Western Front, Dr. Charles S. Myers, a British psychiatrist from Cambridge University, published the first medical paper using the term “shell shock” in the February 13, 1915 edition of *The Lancet*. Myers wrote “A Contribution to the Study of Shell Shock” while attending to casualties at The Duchess of Westminster’s War Hospital in Le Touquet, France. The three cases presented in the paper were, as Myers notes, of “remarkably close similarity” (p. 316). After one or more shells burst near the soldiers, they suffered reductions in their senses of vision, smell, and taste, as well as loss of memory.

Surprisingly, given their proximities to the explosions, none of the soldiers reported significant hearing loss. Although many of the symptoms the soldiers demonstrated were somatic ailments, Myers treated the soldiers with rest, suggestion, and hypnosis—treatments typical of psychological medicine—to some avail. Their vision and memories improved. One reported that he was less “nervy,” but the other two continued to endure nervous symptoms. At the time of publication, none of the soldiers, it seems, returned to combat, and at least one was transferred to England where he was discharged from duty. Myers concluded the paper by stating, “Comment on these cases seems superfluous. They appear to constitute a definite class among others arising from the effects of shell shock ... The close relation of these cases to those of ‘hysteria’ appears fairly certain” (p. 320). Myers’ certainty of the hysterical (meaning psychological) nature of shell shock would meet significant resistance from other doctors who believed that shell shock was a physical ailment rather than a mental condition. Yet, his ideas about treating soldiers’

minds coupled with additional psychological research would have far reaching implications on how and where British soldiers were treated for shell shock.

Myers' small case study of shell shock had limited influence on the British military in 1915. But as a stalemate (that would last nearly to the war's end in 1918) set in on the Western Front later that year, the horrific effects of trench warfare on soldiers would challenge the British military's firmly entrenched beliefs about mental breakdown and its treatment. The devastating magnitude of WWI and the numbers of shell shocked soldiers set into motion several changes in British neuropsychiatric medicine. This chapter takes up the rhetorical framework of magnitude to analyze three shifts in British medico-military practice that resulted from the epidemic of shell shock during WWI.

The first of these changes occurred in 1916, when a thorough investigation of shell shock conducted near the Western Front revealed that a soldier's proximity to an exploding artillery shell was not necessarily a cause of shell shock. Up to this point in the war, neurologists based in London believed that microscopic fragments of artillery shells, the changes in atmospheric pressure caused by the concussive forces of explosions, and the gases they emitted directly affected soldiers' central nervous systems, thus causing mental symptoms. New information gathered from battlefield casualties prompted doctors to accept psychological etiologies as the cause of shell shock. Furthermore, the terminology "shell shock" came under fire from both doctors and military brass who viewed the somatic explanation built into the name of the condition as inaccurate and misleading.

Second, after suffering tremendous casualties in the Battle of the Somme (July 1 to November 18, 1916), the Royal Army Medical Corps adopted psychologists'

recommendations of treating shell shock at psychiatric hospitals near the trenches to keep up the numbers of fighting men instead of transporting them to the UK for treatment with slim chances of them returning to duty. Not only was the change in mental casualty care successful, it also changed the relationship between war and mental breakdown. The success of this treatment model was taken up by the American Expeditionary Force when the United States joined the war in 1917, which employs frontline psychiatry as a means of maintaining troop levels to this day.

Lastly, the firmly entrenched rhetoric of mental breakdown as a form of “cowardice” (or malingering) subsided as war itself came to be understood as a cause of mental trauma, rather than trauma being the result of a soldier’s intrinsic deficiencies. Shell shock itself was rescaled from the level of the individual to the level of the army because it was no longer a potential contagion contracted by defective individuals, but, rather, a respectable psychological response to the war itself. As a result of this ideological shift and the need to maintain troop levels on the fighting front, the British military could no longer abide by its longstanding dictate that so-called cowards were to be court martialed and executed.

Given the monumental scope of the war and its psychological casualties, this chapter focuses almost entirely on the experience of the British Expeditionary Forces (BEF) for two reasons. The first is that the experience of these 80,000 men continues to haunt the Anglo-American imaginary of war. The second is the British experience with shell shock prompted large-scale changes and innovations in Western policy and medical practices that would designate WWI as a unique rhetorical situation that would set the stage for the management of psychological casualties in future conflicts.



First, however, magnitude as a concept of classical and contemporary rhetoric merits a brief discussion.

### **Magnitude in Classical and Contemporary Rhetoric**

“There will always be a tension between what rhetoric *makes* and what “makes” or produces rhetoric.” (Farrell, 2008, p. 470).

In “The Weight of Rhetoric: Studies in Cultural Delirium,” Thomas Farrell (2008) observes that “the rhetorical tradition is itself a multivocal and even conflicted array of perspectives where magnitude or ‘weight’ is concerned” (p. 476). The Greek concept *megethos*, which translates to mean magnitude, appears in the works of classical teachers of oratory. *Megethos*, in Greek and Roman rhetoric, was typically put into practice through the rhetorical technique of amplification (*auxesis*), which a rhetor should deploy in a speech to establish the greatness of a person, convey the importance of the topic at hand, or argue about what course of action is most advantageous. While *megethos* and its usefulness to a discourse through amplification are the stock and trade of rhetoric past and present, Farrell (2008) notes that the topic of *megethos* itself is “impossibly rich” and “very little of substance had been written on the multifarious ways in which largeness, degree, quantity, and priority are themselves composed and nuanced” (p. 474). I agree with Farrell (2008) that there may be no means of “getting to the bottom” of *megethos* because it is itself a topic of great magnitude.

In considering the scale of WWI as a persuasive agent that brought about changes in knowledge of and attitudes about soldiers’ mental breakdown on the battlefield and the means physicians used to treat them, it is necessary to demonstrate the difference between *megethos* as a rhetorical technique employed by a rhetor and the scale of an

overwhelming event that prompts change. In this section, I briefly discuss some of the voices of the multivocal rhetorical tradition, emphasizing their commitments to *megethos* as a means of rhetorically demonstrating what is best, grandest, or most impressive. I then will show how this conception of *megethos* is upended by two rhetors who composed books during WWI. These rhetors demonstrate that *megethos* can operate in an alternative manner to elicit belligerence, horror, and pity from an audience. Because *megethos* is traditionally associated with oratory and writing, this section concludes by demonstrating how an event itself can create a rhetorical situation of such great magnitude and horror that it prompts rhetorical action.

Aristotle conceptualizes *megethos* as a *koinon* (meaning an aspect of the subject being discussed) of degree of magnitude. For example, he employs degree of magnitude as a heuristic of excess in deliberating the virtues of things:

[W]hat is scarcer is greater than what is abundant (for example, gold than Iron), though less useful; for possession of it is a greater thing through being more difficult. But in another way the abundant [is greater] than the scarce, because it exceeds in usefulness; for *often* exceeds *seldom*; thus it is said, “Water is best.” (Aristotle, 2007, p. 68 emphasis in original)

*Megethos*, for Aristotle, typically pertains to questions of the good; therefore, questions of magnitude are relevant to all species of rhetoric. He specifically reinforces the connection between *megethos* and the good in his axioms on amplification in epideictic rhetoric. In epideictic, he wrote, “one should also use many kinds of amplification; for example, if the subject [of praise] is the only one or the first or one of the few who has done something; for all these things are honorable” (p. 81). He reinforces the connection by adding, “Amplification, with good reason, falls among forms of praise; for it aims to show superiority, and superiority is one of the forms of the honorable” (p. 82).

Where Aristotle uses *megethos* as a means of determining moral greatness, the Roman rhetorician, Longinus, draws a distinction between *megethos* and amplification, making them two distinct rhetorical devices. He wrote, “Greatness implies distinction, amplification implies quantity; the former can exist in a single thought, the latter always involves length and a certain abundance” (Longinus, 1991, p. 20). By differentiating importance from quantity, Longinus cleaves *megethos* into two categories, in effect doubling the impact of the concept. In this conception, something that can be small in quantity or physical aspect can be of great importance, just as something that maybe unremarkable in small numbers can be overwhelmingly impressive at a higher concentration. Although a categorical distinction between inherent greatness and multitude is useful but ultimately unnecessary, Longinus’ dual concept of *megethos* opens up the possibility of multiple rhetorical perspectives and subjects. That is, importance does not hinge entirely on intrinsic value or quantity, an admixture of ethics and materiality can contribute to *megethos*.

In contrast to Aristotle, who lived in a culture that valued justice, goodness, and beauty, the Roman Longinus argues that *megethos* is a value-neutral concept that “may be applied indifferently to sublimity, pathos, and the use of figurative language, since all these invest the discourse with some sort of grandeur” (qtd. in Balzotti & Crosby, p. 331). In considering WWI and shell shock, two issues arise from Longinus’ definition of *megethos*. This first is that his commitment to “greatness” and “grandeur” approximate Aristotle’s notion of the good and the inherent value of objects, thus forcing rhetors to convey importance in a restricted class of terms. This limits the rhetor from persuading an audience by invoking the egregious or terrible nature of a subject. The second problem

is that a rhetor must argue for the *megethos* of a topic. Topics themselves cannot convey their magnitude, nor can events act upon audiences.

A short passage on the rhetorical rules for effective vocal inflection when delivering a speech in *Rhetorica ad Herennium*, a treatise attributed to Cicero (1954), comes closest to equating *megethos* with negative emotions. A skilled rhetor with “flexibility of the voice” must appropriately employ the “Conversational Tone, Tone of Debate, and Tone of Amplification” (p. 197). “The Tone of Amplification,” he wrote, “includes the Hortatory and the Pathetic. The Hortatory, by amplifying some fault, incites the hearer to indignation. The Pathetic, by amplifying misfortunes, wins the hearer over to pity” (pp. 197-199). In this instance, amplification is suitable for eliciting emotions not commonly associated with Aristotle’s good or Longinus’ grandeur.

The contemporary cultural rhetorics of WWI deviate from the wholesale beneficence that was characteristic of Aristotelian rhetoric, conforming more with (but still surpassing) the boundaries implied by the Roman rhetors. From 1914-1918 and continuing today, rhetors invoked magnitude for purposes of belligerence rather than grandeur or beauty. H.G. Wells, epitomizes this rhetoric in his 1914 propaganda book, *The War that Will End War*. The title itself reverberates with conflicting ideas of bellicosity and peace, as it attempts to convince both the British people that the Great War is justifiable and to persuade the American forces to join the war effort. Wells (1914) justifies British involvement in the war by illuminating the egregious scale of the enemy: “This Prussian Imperialism has been for forty years an intolerable nuisance on this earth” (Wells, 1914, p. 11). His declamation exhibits Longinus’ distinction between importance and quantity because Prussian Imperialism is in itself “an intolerable nuisance

on this earth” for a great number of years. Grandeur is absent in his statement; instead of value, he conveyed opprobrium and its duration. Furthermore, in a rhetorical move echoing the Amplification of Tone in *Rhetorica ad Herennium*, he exhorted his audience: “Every sword that is drawn against Germany now is a sword for peace” (p. 14). Here again, he employed *megethos* as a key component of the fighting force needed to defeat the enemy. Wells’ (1914) book is nothing short of a hawkish piece of political writing intended to stir emotions to the point that action ensues. In his text, the good and the grand take a subservient, although implied, role to the contemptible nature of the Germans. To consolidate opinion around WWI, he appeals to war rather than peace and a German defeat rather than a British victory. Any nobility of purpose in service to the greater good is supplanted by the hatred for the enemy that he attempts to instill in his reader.

As we see in Longinus’ conception of *megethos*, importance is not only a matter of perspective, but also a quality that can exist independently of numerical quantity. Shifting from the scale of geopolitics and the territories consumed by Prussian Imperialism to the scale of personal experience, James Norman Hall (1916), an American who volunteered for the British Army while on a tour of the UK, described the war in ghastly terms. Of his time at the battle of Loos, he wrote,

Arms and legs stuck out of the wreckage, and on every side we saw distorted human faces, the faces of men we had known, with whom we had lived and shared hardships and dangers for months past. Those who have never lived through experiences of this sort cannot possibly know the horror of them. It is not in the heat of battle that men lose their reason. Battle frenzy is, perhaps, a temporary madness. The real danger comes when the strain is relaxed. Men look about them and see the bodies of their comrades torn to pieces as though they had been hacked and butchered by fiends. One thinks of the human body as inviolate, a

beautiful and sacred thing. The sight of it dismembered or disemboweled, trampled in the bottom of a trench, smeared with blood and filth, is so revolting as to be hardly endurable. (Hall, 1916, pp. 168-169)

Hall's unrelenting scene of carnage overwhelms the reader with an inescapable landscape of death "on every side." The *megethos* he employed extolls no virtues of war; rather, it operates by demonstrating how war violates the "beautiful and sacred" human body. Hall conveyed the excess of disgust and sacrilege produced in battle, finding virtue in neither the war nor his own survival of the horrific scene that faced him in its aftermath. His description surpassed *Rhetorica ad Herennium*'s amplification as pity or indignation because his book does not exhort the reading public to any specific action. Wells (1914) championed total war. Hall (1916) neither argued for nor against the war. He offered instead a written monument to what combatants endured. Like Longinus, his *megethos* is indifferent. He merely described a scene as it was from his vantage point, offering a singular perspective of the war to "[t]hose who have never lived through experiences of this sort."

WWI was dubbed "The Great War" because of its unprecedented scale rather than its measure of goodness. With each month of the war, the numbers of weapons and their power proliferated. Artillery technology advanced at an alarming rate. The shells that were thought to cause shell shock in the early days of the war grew in size from the French 75mm round at the war's outset to the earth shattering 105mm German howitzer that decimated the landscape in later years (Kinard, 2007, p. 241; p. 248). The shells' payloads became more lethal as well. In 1914, the Germans bested the British high explosive lyddite by delivering the more explosive TNT in their shells (p. 242). As the war progressed, shells containing ever more toxic gaseous chemicals, like arsenic,

phosgene, and mustard gas, fell on the frontlines. As artillery's capacity to kill escalated, so did the precision, distance, and rate at which it could be fired. To say nothing of tank, airplane, and submarine warfare, the *megethos* of The Great War was constructed by the power to kill and the numbers killed and wounded in body and mind.

Even though they tend not to account for the horrific and bellicose rhetorics of writers in the modern age, classical conceptions of *megethos* can explain the techniques employed by these wartime rhetors. As the epigram for this section suggests, classical rhetoric struggles to conceptualize how an event, independent of a rhetor, can compel people to action. That is, Greek and Roman rhetoric is confined to oratory and to orators. Wells (1914) and Hall (1916), although certainly not the first, can be seen as modern rhetors extending classical conceptions of amplification, *koinon*, and grandeur into new arenas of horror and war—topics eschewed in the classical tradition's treatment of *megethos*. To return to the dichotomy Farrell (2008) sets out in the epigram, the classical and contemporary rhetors are “makers” of rhetoric. The products of Wells (1914) and Hall's (1916) texts are political unity in support of the war and understandings of the experience of fighting.

Having established that *megethos* is an effective rhetorical mechanism for conveying the horror of war, it is important to examine the scale of mental breakdown during WWI as a quantifiable military-medical phenomenon. Historians who have taken shell shock as their subject have dubbed it the “emblematic” injury of WWI. For historian Fiona Reid (2010), shell shock carries a symbolic weight which makes it an enduring emblem of the human events that transpired between 1914 and 1918. She writes, “The shell-shocked man—often the shell-shocked boy—who was too traumatized to fight

embodies contemporary popular judgements about the First World War: it was too brutal, too cruel and too futile” (Reid, 2010, p. 1). Shell shock is also credited with inaugurating a new era in psychological medicine that strove to understand how external events could injure the mind. Notably, this WWI-specific psychological diagnosis is credited as being the nosological ancestor of contemporary post-traumatic stress disorder.

On the other hand, a dispassionate perusal of (the albeit unreliable) statistics on shell shock during WWI shows that it represents a small proportion of the total numbers of British and American soldiers who were wounded or killed. Shell shock accounts for 6.6% of the 1.2 million British soldiers reported wounded. Mental breakdown, as I will discuss in the following section, was an accepted facet of British warfare, which was managed by the Royal Army’s cultural norms and disciplinary procedures. Considering these factors, someone unfamiliar with the cultural legacy of The Great War might ask why shell shock commanded the concerted attention of the British war effort and upended the medical status quo of its day.

Sigmund Freud’s (1915) “On Transience” helps us to understand the stakes of WWI as a rhetorical situation, an occurrence that commanded the introduction of discourse to augment it. Of the war, he writes,

[T]he war broke out and robbed the world of its beauties. It destroyed not only the beauty of the countrysides through which it passed and the works of art which it met with on its path but it also shattered our pride in the achievements of our civilization, our admiration for many philosophers and artists and our hopes of a final triumph over the differences between nations and races. It tarnished the lofty impartiality of our science, it revealed our instincts in all their nakedness and let loose the evil spirits within us which we thought had been tamed for ever [sic] by centuries of continuous education by the noblest minds. It made our country small again and made the rest of the world far remote. It robbed us of very much



that we had loved, and showed us how ephemeral were many things that we had regarded as changeless. (p. 307)

Without delving into the particulars of the war effort, the numbers of nations involved, and the sides they took or the vast array of munitions they unleashed upon each other, Freud (1915) conveyed the magnitude of the meaning of war. In sum, the war was an agent that effaced the planet as it erased all positive human achievements. And it would continue to devastate until it came to an end. Like Hall (1916), Freud made no call for victory. By virtue of his title, “On Transience,” he optimistically presented the war as a passing state of affairs. If we apply Freud’s insights to the nations waging war, we come to see why they were so deeply invested in victory and bringing a conclusion to hostilities.

Freud’s insights animate Farrell’s (2008) notion of *megethos* as a rhetorical complex of situation and rhetor. For Farrell, *megethos* is the rhetorical property of a situation that makes us care enough to respond with action. He builds upon classical rhetorics of magnitude, most notably Aristotle’s, by reintroducing an ethical component into the concept. “Magnitude,” he writes,

in its myriad of manifestations—seems essential to the most important concerns of traditional rhetoric: namely, whether an audience may care about any topic sufficiently to attend to it, to engage it, and to act upon it; what consequences will weigh most heavily upon their prospective deliberation; what priorities will finally tip the balance in their judgment; and what appetitive attachments will need to be overcome for rational reflection to be feasible. (p. 472)

Within this complex of topic, judgement, and rational reflection, *megethos* accrues a distinctive additional meaning not immediately present in Greek and Roman formulations. That is, magnitude is both a quality of a situation and the mechanism that prompts rhetors to care about said situation enough to commit to action. Farrell calls this

property of magnitude “weight.” As he observes, “All magnitude says is, ‘Hey, look at this! This is important!’” (p. 484). It is up to the rhetor to ask the value-laden question, “Why?”

Farrell grafts an ethical dimension to *megethos* through two theoretical maneuvers. First, he retains the heuristic properties of the Aristotelian notion of magnitude. Using the saturated news media landscape as a context rife with attempts to create attention grabbing stories, he writes, “‘big events’ have come to capture, and then overwhelm, and then exhaust public imagination” (p. 484). As a result, it becomes difficult to distinguish truly important events from manufactured cable news fodder. The significance of financial depression and world war become difficult to recognize as important or even deserving of attention. Farrell points to the picayune events that are made into national obsessions through media coverage. Our misplaced concern and “fetishistic absorption of the most minute relics of media celebrity,” are a type of “cultural delirium,” a concept created by Julia Kristeva to describe how we are “carried away” by our desires for objects (p. 485). To say that we as individuals or even as entire cultures can be “carried away” means that we can misplace value by prioritizing some rhetorical situations above others. The role of rhetoric then is to ensure that the most important events, those that have great weight, are the ones that receive attention.

Secondly, and presumably when the audience is not exhausted, he offers the rhetorical concept of *phronesis*, a Greek term meaning practical wisdom, as a means of contemplating a response to a situation. In the classical tradition, *phronesis* was “analogous to the priority of politics in civic life” and should “be considered the architectonic virtue of moral life because it allowed and enhanced the prospect for a

balanced ethical character” (Farrell, 1998, p. 8). As practical wisdom, *phronesis* helps the rhetor to ascertain what situation is of the utmost important and guides her response to that situation. It is the scale on which the rhetor measures the magnitude of the momentous.

As a result of these maneuvers, *megethos* takes on the meaning of “weight” (Farrell, 2008). The weight of rhetoric is based on the gravity of the situation; its weight being what makes the rhetor care enough to act. Understanding *megethos* as weight helps us to understand why a statistically less significant medical malady like shell shock could command the concerted rhetorical response from the British military and its physicians that I will discuss in the remainder of this chapter. In considering the stakes of WWI, as Freud presents them, along with the intense political sentiment forwarded by Wells, and Hall’s suffocating depiction of the phenomenon of war fighting, it becomes apparent how military mental breakdown could be ascribed great weight by the British military. As I’ll demonstrate below, shell shock stood as a threat to the Allied war effort. Given the weight of the war, shell shock, as its emblem, garnered sufficient rhetorical weight to command a response.

My aim in this section was not to correct the rhetoric’s classical tradition. Rather it has been to show that rhetorics of magnitude can and do exist in bellicose and horrific aspects of human dealings. Regarding shell shock, there was no paradigmatic rhetor to persuade an audience of the importance of tending to soldiers’ mental breakdown on the battlefield. No singular account of mental suffering proved to be a sufficient warrant for military action against shell shock. Rather, to stay with Farrell’s epigram, it can be said that it was overwhelming scale of the war and the psychological casualties it produced

which forced shell shock to become an issue deserving rhetorical intervention. In the next section, I review mental breakdown as endemic in the history of warfare and show that it was only when soldiers' psychological needs threatened the fighting force of the British military that doctors and the government acted.

### **The Issue of Scale in Shell Shock**

“Rhetoric is the art, the fine art, of making things matter” (Farrell 2008).

Although WWI was the first “total war” of the modern age (Chickering and Forster, 2003, p. 3), beginning in the mid-nineteenth century a small group of physicians had reported in the medical literature that the hardships of battle and the new mechanized weapons used to make war could promote higher rates of war neuroses than in previous engagements. During the American Civil War, large numbers of soldiers took ill with symptoms resembling cardiac weakness. Henry Harthshorne (1864), physician to the Army of the Potomac, classified the disorder as “cardiac muscular exhaustion” after being unable to find an organic explanation for the condition. Shortly after the Civil War, Dr. Jacob Da Costa, who termed the disorder “irritable heart,” examined suffering soldiers at the US Hospital for Injuries and Diseases of the Nervous System. He, too, was unable to find a uniform etiology for the disorder, which affected soldiers across the artillery, cavalry, and infantry (Dean, 1997).

Decades later, a similar condition manifested among British soldiers fighting in South Africa. In the Boer War, “disordered action of the heart” (DAH) explained the spontaneous psychosomatic cardiac problems soldiers developed after “the extremely harassing nature of the military operations” (Bowlby et al., 1901, p. 130). Centuries

earlier, Swiss doctor Johannes Hofer coined the term “nostalgia” in 1678 to describe the psychologically debilitating longings of Spanish mercenary soldiers fighting far from their native land (Jones and Wessely 2005, p. 3). Nostalgia (meaning homesickness in Greek) remained in medical circulation on the battlefields around the world until the mid-nineteenth century (McCann 1941; Rosen 1975).

In some cases, shell shock shares a passing resemblance to the etiologies and symptoms of previously defined war neuroses. Take “wind contusion” for example. Veterans of the late eighteenth-century Napoleonic Wars who had been in close proximity to flying missiles or explosions developed symptoms including twitching, tingling sensations, and partial paralysis (Anon., 1904). Yet, historians Jones and Wessely (2005) note that soldiers who manifested these symptoms in the field “were treated with scepticism [sic] by military physicians” and that the military slang term “‘windy’ (meaning cowardly) derived from this phenomenon” (p. 2). Other diagnoses, like “irritable heart,” DAH, and nostalgia, remained in circulation after their original coinage, but fell into disuse in the late nineteenth century, never to make it into the medical nosology of WWI.

While the stresses of war on the body and mind were well-documented in the medical literature, there are virtually no reports of armies developing systematic practices to minister to traumatized soldiers in the field until the early twentieth century. In 1904, a small article titled “Madness in Armies in the Field” (Anon., 1904) appeared in the *British Medical Journal*. The article conveys the experiences of Dr. Paul Jacoby, Physician-in-Chief to the Provincial Asylum of Orel, Russia, who “strongly urges the necessity of a special psychiatric service for soldiers on campaign” (p. 30). Jacoby built

his case for field psychiatry units on the large “number of cases of mental disorder, mostly degenerative and psychical traumatisms, which came under his observation” during the Franco-Prussian War. His inquiries into the incidence of mental disorder occurring during the Russian wars with Turkey (1877-8), China (1900), and Japan (1904-5) also found high incidences of mental illness. During these wars, traumatized soldiers were evacuated from battle and traveled distances up to 10,000 kilometers by train and ship, further exacerbating their symptoms.

In addition to the proximity of soldiers to psychiatric care on the battlefield, Jacoby posited that the implements of modern warfare would have profound effects on the minds of combatants. The doctor drew a comparison between “the sinking of ironclads by the explosion of torpedoes and mines to earthquakes and volcanic eruptions which, it is well known, are accountable for much mental disorder” (p. 30). Creating an analogy between explosions and natural disasters highlights both the magnitude of the effect that new weapons could have on soldiers as well as the unforeseen consequences of employing these devices on a large scale. Jacoby opined that “these new forms of shock will produce new forms of neurosis and mental disorder” (p. 30). Despite claiming that new weapons possessed powers akin to acts of God, Jacoby stressed that “if arrangements could be made for the immediate treatment of insane soldiers in separate tents under special care” they would have a good chance of recovery (p. 30). The Russian doctor was not alone in his thinking. At the end of the decade, Captain L. R. Richards of the American Medical Corps warned, “The tremendous endurance, bodily and mental, required for days of fighting over increasingly large areas and the mysterious and widely destructive effects of modern artillery fire will test men as they have never been tested

before. We can surely count, then, on a larger percentage of mental diseases—in a future war” (Richards, 1910 qtd in Leese, 2002, p. 20).

What is striking in this period is that none of the medical experiences of treating psychologically wounded soldiers appears to have convinced governmental powers of the need to implement standard procedures for dealing with an entire category of ostensibly inevitable casualties. Jacoby’s experiences in Russia were published in *The British Medical Journal*. However, the anonymous author who reported the doctor’s pronouncements offered one sentence indicative of a British response to the Russian battlefield dilemma: “In European wars the need for special provision for the care of lunatics during the war does not make itself acutely felt, for there are always asylums of some kind within reach” (Anon., 1904, p. 30). Such a statement serves as a refusal to posit the possibility of an epidemic of war trauma. Furthermore, it denies that war itself can be a cause of mental illness. It casts psychological casualties as individuals who must be dealt with rather than soldiers who were wounded in the line of duty. In the years leading up to WWI, the British military presumed it was in a position to discount the psychological toll of war, despite its own doctors’ entries into the medical literature of war neurosis.

Initially, the rhetorically charged analogies of explosions to volcanoes and the diction of magnitude employed in forewarning of the “tremendous endurance” demanded of soldiers “fighting over increasingly large areas” and the “widely destructive effects of modern artillery” were insufficient to move the BEF to implement recommendations like Jacoby’s. Yet, the magnitude of the shell shock problem led to changes. Initially, Great Britain thought the war on the continent would last three months (Gilbert, 1994). With no

end in sight, maintaining troop numbers was a constant concern for the British military. Historian Ben Shephard (2002) notes, by the end of 1914, most of the seasoned professional British Army “bled to death in the battles of that autumn” (p. 26).

Shell shock arose as a threat to the war effort in its early months because a growing number of soldiers was being evacuated from the frontlines with slim chance of returning to combat. The Royal Army Medical Corps was unprepared for the influx on psychologically traumatized soldiers. As historians Edgar Jones and Simon Wessely (2001) aptly observed, “In the First World War, *it was scale, rather than the nature of the problem* that caught the military medical services unaware” (p. 92, emphasis added). Thus, the magnitude of the psychiatric casualties eventually became a persuasive agent demanding British forces to take swift action to triage the number of soldiers exiting the battlefield with broken minds.

As this section has discussed, the toll of war on the warrior was well established in medico-military literature. When stalemate reached the trenches of the Western Front in late-1914 and early-1915, the horrific realities of total war challenged existing British ideas about the etiology of shell shock. That is, what military doctors examining the problem far from the front lines believed to be the cause of mental breakdown lost the primacy of their explanatory power when new medical studies conducted near the combat trenches of France demonstrated that shell shock was an emotional condition that could occur independently of artillery fire.

The following section reviews how one influential London-based pathologist’s conception of shell shock as a problem of the body was challenged by a watershed psychological study of shell shock conducted in France. This new medical evidence, as I



will later show, prompted changes in how and where British soldiers were treated for mental breakdown. Further, psychological understandings of shell shock prompted the British military to eventually revise, then ultimately abandon shell shock as a diagnostic category, replacing the condition with terminologies that could account for both somatic and psychological ways of thinking and treating break down on the battlefield.

### **The Incongruity of Magnitude: Relocating Shell Shock from the Body to the Mind**

A rhetoric of proximity permeates early medical literature on shell shock. Myers (1915) was among the first to observe that a factor common to shell shocked soldiers was their close proximity to exploding German artillery shells. Physicalist explanations of shell shock as a problem of neuroanatomy were commonly accepted by British psychiatrists who had learned from their predecessors in the nineteenth century that physical shocks and jolts, like those that occur during a railway accident, were an underlying cause of mental illness. Other British physicians made similar observations in the opening months of The Great War. Eliot (1914) reported a case of functional nervous disorder in a soldier who temporarily lost the ability to walk after surviving a shell blast. Not long after, Evans (1915) argued that shell concussions could produce lesions on soldiers' central nervous systems, which could result in a range of psychological and functional symptoms. A soldier's proximity to the enormous forces of an explosion was a reasonable explanation for shell shock that fit well within the physicalist paradigm of mental illness, as well as a confirmation of the oracular pronouncements of physicians who foresaw modern weaponry causing new forms of war neurosis (Anon, 1904; Richards, 1910).

The language of proximity, like the language of magnitude, is far from absolute. Rather, these terms serve a hierarchical function reminiscent of Aristotle's conception of *megethos*. There is no formula for how near or present a thing must be to persuade. Closeness and distance, as Farrell notes, are "paradoxes of magnitude." "Distance of a proper sort is essential to gaining perspective sufficient to adequately place an object's magnitude within focus. Since magnitude is always (sort of) relative to other related and recognizable values and 'weights,' it is simply not possible to gain a measurable position to place magnitude without a certain degree of distance" (p. 472-473). On the other hand, the paradox of magnitude arises because "One cannot gain any true appreciation of the intricacy of importance in any object without being willing to study it 'close up'" (p. 473).

I open this section with a brief discussion of proximity as a characteristic of magnitude to illustrate the complex (and paradoxical) relationship between rhetoric and distance as a concept inherent to *megethos*. During WWI, proximity, as a material condition of medical research, played an important persuasive role in the British military doctors' reconfiguration of shell shock as a problem of the mind rather than a problem of the body. To illustrate how proximity influenced the codification of shell shock by shifting medical thinking about the disorder from strictly somatic views to a hybrid of the corporeal and psychological, this section focuses on the works of two key rhetors in the psychiatric debate: Frederick W. Mott, an outspoken London-based neuropathologist and psychologist who researched the corporeal effects of shell shock on soldiers' central nervous systems and published more papers on shell shock than any other doctor at the time, and Harold Wiltshire, a British psychologist stationed in France who systematically

studied scores of shell shocked soldiers and found no significant linkage between bodily injury and soldiers' symptoms.

The result of this shift in medical theory is an “inversion of magnitude” (Farrell, 2008, p. 479). An inversion occurs when the thing perceived to be biggest and most important winds up being less significant than originally thought. As one of shell shock's inquisitors wrote after the war, “Undoubtedly in the popular mind ‘shell-shock’ signified that the patient had been exposed to, and had suffered from, the physical effects of explosion of projectiles [sic]” (Southborough, 1922, p. 69). He continues, revealing the inversion of magnitude, “It ...became apparent that numerous cases of ‘shell-shock’ were coming under the notice of the medical authorities where the evidence indicated that the patient had not been within hearing distance of a shell-burst” (p. 69). Beyond diminishing the importance of explosions in the etiology of shell shock, the inversion of magnitude stripped somatic theories of mental breakdown of their primacy and elevated psychological codifications of shell shock. Ironically, this inversion was only rhetorically available to the physicians who were themselves closest to the explosions.

The high explosives that rained down upon soldiers in battle also provided a convincing etiology for the biologically-oriented doctors to explain the phenomena of shell shock as the war dragged on into its second and third years. Frederick W. Mott, who served as a colonel in the RAMC during the war, presented the most complete theory of shell shock as a wound to the central nervous system. Prior to the war, Mott worked for twenty years as pathologist for London County Asylums where he conducted post-mortem experiments on syphilis patients. Syphilis, in Victorian England, was considered a moral disorder that in its later stages presented degenerative neurological symptoms,

including seizures, paralysis, dementia, and eventually death. By dissecting the brains of patients who succumbed to syphilis, Mott readily identified traces of the disease in the anatomy of the brain. His pathological investigations in this realm were far from medically novel. E. E. Southard (1919), who directed the U. S. Army Neuropsychiatric Training School, questioned whether shell shock was “a matter of spirochetes” (p. 1) and devoted an entire section of his compendious 1919 collection of shell shock cases to “syphilopsychoses” (pp. 8-43). Also, in the previous century, central nervous system autopsies of railway spine sufferers were not uncommon (Dercum, 1895). Mott’s syphilis research extended beyond the descriptive into the realm of eugenic theory, specifically into heritable madness that passed from one generation to the next. He collected the brains of relatives who expired in the asylums and compared their anatomies, “confirming the role of heredity in insanity and showing how in successive generations madness tended to intensify and come on at an earlier age” (Shephard, 2002, p. 6). These two lines of biological thinking, that madness was heritable and that the damage it caused could be observed in autopsy, informed his ideas of shell shock.

In late 1914 and early 1915, Mott collected “objective” data on the shell shock cases he encountered in London (Leese, 2002, p. 70). The pathologist first introduced his somatic theory of shell shock to the medical community in 1916, when he was invited to present his research at the prestigious Lettsomian Lectures in London, in a series of talks titled, “The Effects of High Explosives upon the Central Nervous System” (Mott, 1916). As he set the scene for his discussion of shell shock, his background as an asylum pathologist became apparent. He stated that the majority of shell shock cases occurred in soldiers with “an inborn timidity or neuropathic disposition, or an inborn germinal or

acquired neuropathic or psychopathic taint” (Mott, 1916, p. 331). In short, a soldier’s heredity predisposed him to succumbing to the “terrifying effects of shell fire and stress of the trench warfare” (p. 331).

In addition to soldiers’ heredity and the horrors they endured in combat, Mott contended that soldiers’ bodies were affected in three ways when they were exposed to blasts from enemy shells. He argued that the forces of the atmosphere compressing and decompressing near a soldier caused microscopic brain lesions (Mott, 1917). He also held that it was possible that tiny shell particles penetrated the brain (Shephard 2001, p. 30). Furthermore, he posited that the carbon monoxide exhausted during the explosion “might lead to cerebral poisoning” (Jones and Wessely, 2005, p. 23). To support his initial theory, Mott returned to the autopsy table. In 1917, he acquired the brains of two soldiers who died without any physical injuries after being exposed to shell fire. One soldier never regained consciousness after a German shell ignited the ammunition shed he was guarding, and the other survived a shell blast 10 feet from his dugout only to develop tremors, mutism, and “hysterical manifestations of melancholia” before dying (Mott, 1917, p. 612-613). After microscopic examination of the brains, he tentatively concluded, “Probably both the forces of compression and decompression act in producing vascular disturbances in the central nervous system, causing arterio-capillary ansemia and venous congestion” (p. 614).

Biological inquiries into the nature of shell shock offered little insight in terms of treatment at a time when the British military was concerned with maintaining troop levels on the Western Front. Although accurate shell shock statistics were never gathered during the war (Southborough, 1922, p. 7-8), the military’s perceived need to stem the tide of

shell shock as the war intensified made academic medical approaches to the shell shock problem less desirable than actionable medical data. In effect, Mott's work served purposes more ideological than practical. Furthermore, the military had no means of testing heredity, and neuroanatomical autopsies merely confirmed the common-sense military dictum that soldiers should avoid enemy fire.

The nation's efforts to raise a volunteer force, chidingly termed "Kitchener's Mob" because of the ragtag nature of the recruits, was haphazard at best. Recruiters were a conniving blend of opportunism and duplicity. As Hall (1916) recalled in his memoir, "I was frank with recruiting officers. I admitted, rather boasted, of my American citizenship" (p. 3). The recruiters briefly conferred about this would-be disqualifying information. When the conference concluded, Hall's "recruiting officer returned to his desk, smiling broadly. 'We'll take you, my lad, if you want to join. You'll just say you are an Englishman, won't you, as a matter of formality?'" (Hall, 1916, p. 4). Beyond recruiters' dubious practices, the sheer number of volunteers overwhelmed medical examiners, with one examiner estimating that "20 to 30% of the men [who volunteered] were never medically examined at all" (Johnson & Rows, 1923, p. 41).

A world away from the lecture halls and recruiting offices of England, RAMC psychologists contended with shell shock cases fresh from the trenches and conducted their own studies of mental break down. In June 1916, Harold Wiltshire, a respected London physician who saw 150 cases of shell shock during the year he spent at a base hospital in France, published his findings in *The Lancet* as "A Contribution to the Etiology of Shell Shock." In no uncertain terms, Wiltshire (1916) compiled a list of findings that challenged the primacy of biological theories of shell shock:

1. The wounded are practically immune from shell shock, presumably because a wound neutralizes the action of psychic causes of shell shock.
2. Exposure and hardship do not predispose to shell shock in troops who are well fed.
3. While it is theoretically possible that physical concussion resulting from a shell explosion might cause shell shock, it is certain that this must be regarded as an extremely rare and unusual case.
4. Chemical intoxication by gases generated in shell explosions cannot be more than a very exceptional cause of shell shock.
5. Gradual psychic exhaustion from continued fear is an important predisposing cause of shell shock, particularly in men of neuropathic predisposition. In such subjects it may suffice to cause shell shock *per se*.
6. In the vast majority of cases of shell shock the exciting cause is some psychic shock. Horrible sights are the most frequent and potent factor in the production of this shock. Losses and the fright of being buried are also important in this respect. Sounds are comparatively unimportant.
7. A consideration of the causes and frequency of relapses favours an original cause of psychic nature.
8. Any psychic shock or strain may cause a functional neurosis, provided it be of sufficient intensity relative to the nerve resistance of the individual. Such shock or strain need not have any connexion with "sex complexes."  
(Whiltshire, 1916, p. 1212)

By virtue of his proximity to the battlefield, Whiltshire (1916) was able to examine soldiers soon after they manifested symptoms of shell shock. The soldiers (and corpses) his London colleagues studied were far removed from the original contexts of their injuries because they had been funneled through the labyrinthine British military hospital system before being repatriated and assigned to a home front clinic. To lend credence to his findings, Whiltshire (1916) argued, "In shell shock changes in clinical condition may occur with such startling rapidity that the medical officer at the base or at home may see the patient in conditions so dissimilar that they are compelled to different conclusions" (p. 1207). In essence, he connected distance to the battlefield as a necessary condition of

making accurate medical observations. Therefore, his physical location near the frontlines provided him a perspective that was not available to doctors in England.

At least three of Whiltshire's (1916) eight findings reflect his privileged proximity to the physical conditions that shell shock patients endured. Foremost, his observation that physically wounded soldiers do not succumb to mental break down would have been foreclosed to Mott, whose work focused solely on patients previously diagnosed with shell shock. The same can be said of Whiltshire's observation about soldiers' nutritional intake because even a soldier who was rapidly transferred would be more rested and nourished by the time he arrived in England than he was in the trenches. Additionally, he argues, it would be nearly impossible for home front doctors to reconstruct the conditions of shell shocked soldiers' original contexts because "[i]t is very exceptional for any clinical notes to accompany these cases from the front" (p. 1207).

Given his proximity to the trenches, Whiltshire was in a position to immediately encounter patients who relapsed after treatment at other facilities. More than 27% of the cases he saw were soldiers who had not been completely rid of their symptoms, but were deemed fit to return to battle. The high incidence of relapse proved to him that physical explanations of shell shock were insufficient because a relapse would warrant that the soldier survived another close encounter with a shell blast. He explained, "This percentage of relapsed cases is greater than could be expected if the cause were an original finite physical trauma, but it is no more than we ought to expect in the case of an original trauma of psychic nature, and so still capable of conscious, or unconscious, action and reinforcement" (p. 1211). Throughout his report, Whiltshire argued that his



immediate access to soldiers provided him with insights that could not be gleaned by physically-oriented London-based doctors.

Although Whiltshire directly undermines Mott's (1916) claims that shell concussion and noxious gases cause shell shock, his writing demonstrates that he had taken care to integrate aspects of his medical colleagues' opinions into his article. His attention to his audience's beliefs demonstrates that he tends to the *phronesis* component of *megethos*. As Farrell (1998) wrote, "Magnitude can only be said to matter to us, if we are able to take it all in. And practical wisdom could never be a product of dictation alone" (p. 9 original emphasis). Whiltshire's findings may be unique to his proximity, but they must also be applicable to doctors in other contexts who may adhere to somatic theories of shell shock. As such, he demonstrates *phronesis* by aligning himself with his colleagues' distaste for the term shell shock, and more importantly, he distances his psychological theory from continental theories of psychic trauma, which Britons found distasteful because of their reliance on sex.

Whiltshire's (1916) findings resounded with several of the concurrently held medical opinions of physicalists and psychologists. He dismissed "shell shock" as "bad terminology" that was "used indiscriminately to include all functional nerve cases, whether due to shell explosions or not, and also not a few cases of organic nature" (p. 1207). In this regard, he echoed the biologically-oriented psychologist, Henry Head, who stated that shell shock was a "heterogenous collection of different nervous afflictions from concussion to sheer funk, which have merely this much in common, that nervous control has at last given way" (Anon., 1916, p. 306). Furthermore, Whiltshire demonstrated through his case studies what Army doctors had long known but

fastidiously ignored; namely, that repeated exposure to the horror of war took a psychic toll on soldiers, regardless of “neuropathic predisposition” or “family history.” He wrote, “While histories of 142 patients reveal that horrible sights formed a definitive causative factor in 51 cases, and a probably factor in 13 more, it is certain that these figures are far below the true mark, owing to the remarkable repression of memory which takes place in connexion with sights” (p. 1210). Additionally, he found that fright alone was sufficient to cause shell shock (p. 1211).

As Whiltshire constructed this new etiology of shell shock, he was careful to distance British military psychology from the sexual aspects of continental European theories of psychology. Historian Chris Feudtner (1993) observed that the theories of Charcot, Breuer, and Freud, although controversial, “were presented in a favourable light repeatedly by general medical journals; the usual appraisal of Freudian doctrine, embracing many of its principles while rejecting its emphasis on sexuality, foreshadowed the consensus that emerged among educated laypersons after the war” (pp. 386-387). Quite plainly, Whiltshire states, “To connect such psychic trauma [e.g. shell shock] with a ‘sex complex’ is impossible” (p. 1211). Although there is no reason to believe otherwise, his pronouncement forecloses the possibility of British critics reading culturally unacceptable facets of psychology into his own theory.

Mott appears to be swayed by Whiltshire’s psychological codification of shell shock’s etiology. Although he never fully abandoned his studies of the central nervous system, Mott made inquiries into “psychic trauma and the effect produced by terrifying dreams” in 1918, citing Shakespeare’s Queen Mab speech as a dream authority and eschewing any mention of Freud. Mott’s later work on dreams, however, finds untapped

value in Freud's theory of "active repression of a painful experience," noting that since the war, "the sexual aspect of the theory" is "no longer tenable" (p. 287). A special committee on shell shock convened by Parliament at the war's end found similar usefulness in Freudian theory and psychoanalytic treatments, so long as they were devoid of any sexual underpinnings (War Office, 1922).

With the sexual aspects of psychological theory purged, the British military became more amenable to psychological treatments for three overlapping reasons. The first is that, as the war progressed, the general opinion turned against physicalist treatments for shell shock, viewing them as more punishment than cure. For example, physician Major William J. Adie recounted the torturous means by which he used ether to treat mutism in shell shocked patients. "My method," he stated,

was to place the first patient on an operating table, and after explaining that his voice would certainly come back I gave him a whiff or two of ether. (I had no suitable electrical apparatus or I should have used it.) After a few whiffs the man would attempt to remove the mask. I then said to him "I shall remove the mask when you say 'take it away.'" At the same time I pricked the skin over the larynx rather vigorously with a pin. Very soon the patient said in a tone of disgust: "Oh! take it away." I then asked him for his name, number, regiment, etc. and after a short conversation sent him off. (War Office, 1922, pp. 17-18)

The procedure undoubtedly struck fear into the patients awaiting their turns in the hallway. Secondly, despite the immediate results produced by physicalist treatments, soldiers frequently developed new shell shock symptoms (Feudtner, 1993, p. 390). Since these treatments were typically administered to patients who had already been evacuated to England, the production of new symptoms further encumbered home front treatment centers and rendered the soldiers unlikely to return to the front lines. Lastly,

psychological treatments, such as persuasion, suggestion, and hypnosis, developed the reputation for their “economy of time” and “more potent therapeutic effects” (qtd. in Feudtner, 1993, 390; Whiltshire, 1916). Because little equipment was needed, psychological treatments could be effectively applied without evacuating soldiers from the theater of war.

Throughout the war, the treatment a shell-shocked soldier received depended largely on the predisposition of the physician to attend to him and his proximity to the frontlines. The physicalist doctors of London rebuffed psychoanalytic techniques because they required additional training and demanded that busy practitioners devote more attention to their patients (Mott, 1916, p. 553). Others had what Rivers (1918) called an “instinctive aversion to the practice of hypnosis” because “hypnosis savours of the uncanny, mysterious, and unknown” (p. 177). Given the legacy of British military medicine as a catch-as-catch-can enterprise (see Shephard, 2001), it was highly unlikely that the RAMC could retrain all of its doctors in the art of healing the mind, especially as casualty rates grew throughout the war.

The shift from a biological to a psychological codification of shell shock represents an inversion of magnitude that is indicative of *megethos* at work. By arguing that proximity afforded them insights unavailable to distant researchers, Whiltshire was able to articulate how the dominant somatic model did not fit the realities of the warzone. With the new model accepted by British medical authorities, physicians could tend to the situation with the greatest weight, which was healing and returning soldiers to combat.

In the following section, the issue of proximity emerges again in physicians' arguments to alter the British military hospital system to include psychological treatment centers located near the frontlines of combat.

### **Psychology on the Frontlines**

Following Whitshire's revelation that shell shock was the somatization of psychological trauma, physically-oriented psychiatrists, like Mott, incorporated aspects of psychological theory into their own understandings of mental breakdown on the battlefield. In this instance, distance, as a kind of magnitude, exhibited the rhetorical property of heuristic, which helped Whitshire's audience to understand the superior value of his codification of shell shock. With this new codification in place, RAMC psychiatrists convinced the British military command of the value of psychological treatments and bring psychological medicine nearer to the frontlines to maintain troop levels.

Whitshire's study brings to light the fact that shell shock involved a complex rhetorical situation brought into being by the magnitude of WWI. Lloyd Bitzer (1968) observes in "The Rhetorical Situation" that the presence of rhetorical discourse is indicative of an exigence in the world that commands rhetorical action to augment it. Rhetoric, in Bitzer's formulation, has the capacity to move audiences to action. "The rhetor," he wrote, "alters reality by bringing into existence a discourse of such a character that the audience, in thought and *action*, is so engaged that it becomes mediator of change" (Bitzer, 1969, p. 4 emphasis added). In considering *megethos* as a means of determining what situations have the most "weight" and should command an audience's

attention (Farrell, 2008), it stands that *megethos* is also a means of determining which situations are most deserving of action. Keeping with the rhetorical tradition, the heuristic aspect of *megethos* also contributes to determining which course of action is most appropriate in a given rhetorical situation (see Aristotle [2007]).

In this section, I will demonstrate how British psychiatrists, newly empowered by the shift to a psychological etiology of shell shock, used the *megethos* of proximity to create a network of frontline psychiatric hospitals to attend to the British army's growing concern about shell shock casualty rates. By employing their knowledge on the battlefield, they not only altered the immediate rhetorical situation of shell shock by lowering psychiatric casualty rates, but also indelibly transformed the role of psychological medicine in warfare, making "forward psychiatry," as it came to be called, a fixture in future British and American military campaigns.

Before I demonstrate how frontline psychology used the *megethos* of proximity, it is first necessary to describe the labyrinthine nature of British shell shock hospitals in the early years of the war, as well as their consequences for maintaining an Allied fighting force. From 1914-1916 the common method of handling shell shocked soldiers was to evacuate them from the frontlines to a network of hospitals in England. For example, in early 1916, a shell-shocked soldier would be transported to the UK and assessed at one of two clearing hospitals: The Royal Victoria Hospital or No. 4 London General. Once diagnosed, the afflicted serviceman would then be dispatched to one of seven treatment hospitals in England or Scotland, depending on whether he was a soldier or an officer (Jones and Wessely 2005, p. 23). Depending on the hospital and its attending physician, a serviceman would receive one or more of an inconsistent panoply of therapies to mitigate

the specific symptoms he demonstrated. Pat Barker (1995) vividly dramatizes the differences between the physicians and therapies employed at two of these hospitals, Craiglockhart and Queens Square, in her historical novel, *Regeneration*. In it, she starkly contrasts the psychological methods of abreaction and hypnosis W.H.R. Rivers used to treat officers with the physicalist methods, including electroshock therapy, that Lewis Yealland employed on enlisted men. While historians have recently taken issue with her characterizations of the hospitals, physicians, and treatments (Linden, Jones, & Lees, 2013; Reid, 2014), Barker's novel reflects but one aspect of the problem of the distribution of knowledge about shell shock and its concomitant practices. It is important to note that Barker (1995) accurately demonstrates that effective psychological treatments were indeed available to soldiers. The subject of this section, however, is the issue of bringing these treatments nearer to the frontlines of combat, where they were previously unavailable to soldiers.

Of great concern to the British army was that soldiers evacuated from the frontlines to British shell shock hospitals were unlikely to return to the trenches after receiving treatment in the UK. For instance, at Maghull Red Cross Hospital, only 21% of the 731 soldiers discharged after treatment returned to their previous duties, with very few resuming their positions at frontline battalions (Shephard 1996, p. 445). One army neurologist estimated that across all shell shock treatment hospitals in the UK, the return rate was as low as 4% (Holmes, 1939).

The British military command realized the pitfalls of evacuating shell shocked soldiers during the bloodiest battle of WWI, The Battle of the Somme. From July 1 to November 18, 1916, the BEF suffered more than 415,000 casualties, reducing its fighting

force by more than 70% (Hart, 2008). The Battle of the Somme created a dire need not only to maintain troop levels, but also to keep seasoned service men on the front. On August 2, 1916, a month into the Battle of the Somme, when more than 200,000 BEF soldiers were either killed or wounded, the British general command called for “economy in men and reserves.” One of the key means it suggested was to eliminate the “wastage” caused by evacuating men for shell shock (Shephard, 2001, p. 46). Commanders proffered the nebulous order that “the number of cases arriving at Casualty Clearing Stations with a tally marked shell shock must *somehow* be reduced” (qtd. in p. 47, emphasis added). The codification of shell shock as a collection of psychological phenomena that could be treated in-theater gave RAMC psychologists the power to implement new medical practices at forward positions near the battlefield. The idea of treating men’s minds on the battlefield became the *somehow* to help stem the tide of British attrition.

The pressing need for the BEF to retain seasoned soldiers in the face of high casualty rates created an opportunity that British psychologist Charles S. Myers seized upon to treat soldiers psychologically closer to the frontlines. Myers and the Royal Army Medical Corps (RAMC) were aware that their French allies had created a network of forward neuropsychiatric centers in 1915 to maintain their own troop numbers at the front. French reports cited astonishing rates of return for shell shocked soldiers. One doctor reported that 91% of patients were successfully treated in December 1916 (Jones & Wessely, 2003, p. 412). These centers eschewed somatic treatments like shock therapy, employing instead “a simple and energetic psychotherapy” (Roundebush (1995) quoted in Jones and Wessely 2003, p. 412). The proximity to the front and the general



environment of disciplined care in the centers factored into their success. Andre Leri, head of the French Second Army's center, commented on the conditions important for forward psychiatry's efficacy: "Their [the neuropsychiatric centers'] only degree of comfort, their strict military discipline, their proximity to the front, their remoteness and their inaccessibility to friends and relations render them specially [sic] suitable for this form of treatment and ensure much easier and quicker cure than in the interior" (Roussy & Lhermitte, 1918 qtd. in Jones & Wessely, 2005, p. 25).

Myers, a medical volunteer to the RAMC who had an eclectic background in anthropology, academic medicine, and psychology, had spent several weeks at British Casualty Clearing Stations in early 1916. Like his medical colleagues, he was dismayed by the attrition rates of shell shock. Unlike the RAMC, he believed that the French method of treating shell shock was suitable for British troops and began pressing for specialist psychological hospitals to be created within miles of the frontline. The first request Myers tendered in May 1916 was denied. With casualty rates from the Battle of the Somme mounting in August 1916, Myers was made Consulting Psychologist to the Army (Myers, 1940). He immediately composed a long memorandum to the British command stressing the need for specialty hospitals to be placed near the front lines using treatments based on "promptness of action, suitable environment and psychotherapeutic measures" (War Office, 1922, p. 123). Considering the situation, Army commanders reluctantly accepted a psychological model of shell shock because it offered the potential to return wounded soldiers to active duty (Jones & Wessely, 2014, p. 1709). The Office of the Adjutant-General granted Myers' request in November 1916, too late to stem the tide of soldiers being evacuated from the Somme to England.

Following approval of his request, Myers established four specialist psychology units within ten miles of the French frontlines. He argued in his 1940 memoir that ten miles was “as remote from the sounds of warfare as is compatible with the preservation of the ‘atmosphere’ of the front” (Myers, 1940, p. 124). As the French doctor Anre Leri had observed in his own neuropsychiatric units, maintaining the atmosphere of military operations was key to rehabilitating soldiers and preparing them to return to the trenches. Keeping British soldiers’ focused on the fight was a constant challenge because of the “ridiculous proximity” of the French trenches to England. As Paul Fussell (1975) wrote in *The Great War and Modern Memory*, “There were constant reminders of just how close England was” (p. 65). French newsboys sold the *Daily Mail* at the entrances to the trenches, and the British post operated with an alarming swiftness. As one officer noted during the bloodiest days of the Somme, “It is extraordinary how the post manages to reach one out here” (p. 65).

To keep the war nearer than home, the psychology units treated soldiers presenting the symptoms of shell shock with a disciplined yet salubrious regimen. Upon entering the unit, soldiers were fed and permitted to rest. Then they engaged in a program of graduated physical exercise and route marches, all within earshot of the battlefield (Jones & Wessely, 2005, p. 26). Doctors administered medicine to soldiers who had trouble sleeping. Depending on the attending physician’s familiarity with psychological therapies, soldiers were also treated with hypnosis and abreaction.

In the midst of the war, the British military and its physicians considered the psychological treatment centers to be a success. During the war and immediately after the armistice, clinicians published the outcomes of their frontline treatments in medical

journals. William Brown, who headed the unit for the Fourth Army, reported in *The Lancet* that he treated between two and three thousand soldiers for psychoneurosis within 48 hours of their breakdowns. After administering two weeks of rest and abreaction, 70% of the soldiers returned to duty (Brown, 1918, p. 197). William Johnson, whose unit treated over five thousand soldiers during the Battle of Passchendaele, estimated that 55% of his psychiatric casualties returned to combat (Johnson & Rows, 1923, p. 41). The Third Army's forward psychiatrist, Frederick Dillon (1939), reported in the *British Medical Journal* that 63.5% of the 4,235 shell shocked soldiers it treated returned to the fight (p. 66). Such statistics cemented the importance of psychological treatments near the battlefield. As Captain C. B. Farrar (1917) wrote in the *American Journal of Insanity*, “[I]t seems to be a fact that treatment is more satisfactorily carried out and cures more speedily accomplished in hospitals close to the front and where the spirit of army discipline is most felt” (p. 711). Likewise, Frederick Mott regarded the centers as a means of preventing wastage, deeming them to be “of supreme importance both in respect to the welfare of the individual and from the economic point of view of the state” (qtd. in Salmon, 1917, p. 521). While these statistics and professional endorsements were necessary to maintain the presence of psychological medicine near the frontlines, contemporary historians argue that the statistics were inflated, with less than 20% of shell shocked soldiers returned to their combat units (Jones, Thomas, & Ironside, 2007, p. 220). Most rehabilitated soldiers were reassigned to non-combat duties.

The implementation of forward psychiatry units served to resolve an issue that bedeviled doctors and the military commanders from the war's outset (see Whiltshire, 1916; Myers, 1940; and Head, 1916). The units carried the name “Not Yet Diagnosed

(Nervous)” (NYDN) centers to distance the work they did from the ambiguous term shell shock. One reason for the change in terminology was to prevent soldiers from being officially diagnosed with shell shock, a condition that warranted their evacuation to the UK.

Another, more bureaucratic, reason was to lay to rest the previous confusion the British military command created with its system of categorizing shell shock as either a wound or an injury. Although soldiers and doctors had used the term shell shock from the beginning of the war (Myers, 1940), the British military begrudgingly acknowledged it as a diagnosis in late 1915 (Shephard, 2002, pp. 28-29). It was not uncommon during the early months of the war for doctors to label a soldier “Mental” or “Insane” or “GOK (God Only Knows)” and then send him down the line to be diagnosed again (p. 29). To standardize medical terminology, the Army Council employed its paradigm of sick, well, wounded, or mad to create two versions of shell shock distinguished by enemy action. As Myers (1940) recalled in his memoir, the army instructed its doctors in France that “Shell-shock and shell concussion cases should have the letter W prefixed to the report of casualty, if it was due to the enemy: in that case the patient would be entitled to rank as ‘wounded’ and to wear on his arm a ‘wound stripe’” (p. 93). Shell shock cases that could not be directly attributed to enemy action were to be labeled “Shell-shock, S (for sickness)” (p. 94). Service men diagnosed as Shell-shock, S were not entitled to wound stripes or military pensions for their injuries. Historians have noted that there is little evidence that this decree was uniformly followed (Jones and Wessely, 2005; Shephard, 2001, p. 54-55). By June 1917, the British army had tired of the confusion it had created with its Shell-shock, W and Shell-shock, S labels. Without consulting with its own

physicians, it advised that all soldiers reporting neurological and psychological symptoms be labeled with the blanket term NYDN until their commanding officers could verify that they were indeed cases of Shell-shock, W. This procedure proved to be too distracting to commanding officers in the trenches. With military protocols failing to achieve their desired ends, the British command forbade the use of the term shell shock and abolished the use of Shell Shock, W policy in September 1918 (Myers, 1940, p. 101). The forbidden term was replaced with “war neurosis,” another capacious (but less evocative) medical designation used throughout the war that reflected the psychological underpinnings of shell shock.

Perhaps the most enduring effect of these changes to psychological medicine was that it helped the American Expeditionary Force, who joined the fight in 1917, to prioritize their treatment of battlefield mental breakdown. In 1916, the U. S. Army dispatched Major Thomas W. Salmon, the medical director of the National Committee for Mental Hygiene, to France to reconnoiter Allied care and treatment of shell shock. Salmon was quick to confirm what three years of medical debate had proven, that “psychological factors are too obvious and too important in these [shell shock] cases to be ignored” (p. 514). Perhaps because his mission was proactive in treating potential cases as opposed to the British mission of reacting to depleting manpower, the report Salomon published after his return from Europe, focused on the strains soldiers themselves endure. Tending to these strains, he was convinced, would keep men in the fight, staving off their “escape” in either mind or body. “The conditions which may make intolerable the situation in which a soldier finds himself hardly need stating,” he wrote,

Not only fear, which exists at some time in nearly all soldiers and in many is constantly present, but horror, revulsion against the ghastly duties which

must be sometimes performed, intense longing for home, particularly in married men, emotion situations resulting from the interplay of personal conflicts and military conditions, all play their part in making an escape of some sort mandatory. (Salmon, 1917, p. 515)

He argued that approaching shell shock casualties from “the psychological viewpoint” would increase a soldier’s “ability to adapt” (p. 516).

In his final recommendation to the U.S. Army, Salmon advised against creating an elaborate network of hospitals where soldiers could be “passed on” (p. 532). Instead, he advocated for two types of hospitals to be built in France for the American forces. The first should resemble the British NYDN centers of 100 beds where soldiers could recover “within the sound of artillery” and promptly return to the front lines. For more severe cases, he suggested a rural location on 30 acres with 500 beds in the south of France be built and outfitted with a gymnasium and space for occupational therapy to restore “the lost or impaired functions by re-education” (p. 534). Both facilities would provide a full range of psychological treatments. Salmon reserved evacuation to America for only the severest cases that could not be cured within six months.

The U. S. Army adopted most of Salmon’s recommendations for WWI (Jones & Wessely, 2003, p. 412). The principles of British NYDN centers would be codified in American military medicine with the acronym “PIE,” standing for Proximity to combat, Immediacy of treatment, and Expectancy of recovery (Artiss, 1963). PIE became and still is a centerpiece of American frontline psychiatry. Following WWI, American medics put PIE into effect and the number of psychological casualties being evacuated from the fighting front diminished with each conflict. The practice continues today.

In this section, I have attempted to demonstrate treating shell shocked soldiers in close proximity to the frontlines of combat produced significant rhetorical effects.

Foremost, enacting psychological knowledge in a regimen of therapies prevented large numbers of troops from being evacuated to the UK, effectively bolstering the numbers of troops in actively engaged in the fight. Additionally, the new structure of battlefield care was accompanied by a more streamlined medical lexicon for shell shock. The success of the NYDN demonstrates that the British military felt the weight of shell shock, if only too late for it to be used in larger battles like the Somme. Once psychological medicine was put in proximity to combat, this form of medical knowledge proved its worth on the battlefield, as is evident in the fact that frontline psychiatry remains a fixture of modern warfare. With the shift in shell shock policy came cultural changes for the British military. The next section argues that shell shock was a disruptive discourse that prompted revisions to military culture and jurisprudence.

### **An Inversion of Magnitude: Shell Shock and the Rhetoric of Cowardice**

On the battlefields of The Great War, the *megethos* of proximity persuaded the British army and its doctors of the value of a psychological codification of shell shock. The magnitude of shell shock produced rhetorical effects on the British military long after warring nations signed the Armistice on November 11, 1918. In the aftermath of the world's first total war, the Army Council commissioned a committee in 1920 to

Consider the different types of hysteria and traumatic neurosis, commonly called "shell-shock"; to collate the expert knowledge derived by the service medical authorities and the medical profession from the experience of the war, with a view to recording for future use the ascertained facts as to its origin, nature, and remedial treatment, and to advise whether by military training or education, some scientific method of guarding against its occurrence can be devised. (War Office, 1922, p. 3)

As a historical artifact, this “reference,” as the committee’s charge was called, demonstrated that British government felt the weight of the psychological trauma of warfighting, and, to some extent, came to comprehend the warnings proffered by physicians, like Jacoby (Anon., 1904) and Richards (1910), before the outbreak of WWI. The Right Honourable Lord Southborough, an aristocrat sympathetic to the plight of shell-shocked soldiers, headed the committee, which is commonly referred to as the Southborough Committee in historical documents. The 215-page “Report of the War Office Committee of Enquiry into ‘Shell-Shock’” documented the findings of the committee’s 41 meetings along with excerpts of testimony from 59 witnesses, including doctors, officers, and “victims of one form or another of war neurosis” (p. 3). The report served the important rhetorical function of cataloguing vast swaths of scientific information on shell shock’s causes and treatments, and, in effect, it enshrined psychological codifications of shell shock, or war neurosis, the committee’s preferred term for the disorder.

The report also provided evidence that a psychological codification shell shock undermined British military laws concerning cowardice and malingering. As the committee gathered testimony on “Cowardice and Shell-shock” and “Malingering and Shell-shock,” it became apparent that witnesses had trouble reconciling *ex cathedra* British military doctrine with the experience of shell shock on the battlefield. As a result of this conflict of expert opinion, the committee recommended that the British military should in future take a more nuanced approach to the dispensation of military justice for soldiers charged with cowardice in the face of the enemy. On malingering, expert



opinions differed so greatly that the committee offered no guidance for detecting manufactured symptoms of war neurosis in future conflicts.

The committee's findings demonstrate another rhetorical effect of the *megethos* of shell shock. As we have seen, bringing psychological medicine into the proximity of the trenches mitigated the shell shock problem and extolled the value of understanding war trauma as a mental disorder rather than a physical wound. When the committee brought psychological medicine into the proximity of military law, a rhetorical inversion of magnitude occurred, rendering experts and the committee itself uncertain about issues that were previously viewed as ironclad truths. In the remainder of this section, I will briefly discuss Thomas Farrell's inchoate concept of the inversion of magnitude, and, using examples from historical documents and the committee testimony, show how the *megethos* of shell shock shifted British military rhetoric on cowardice and malingering.

Thomas Farrell (2008) introduces the concept of the inversion of magnitude more by example than direct explication. Taking the installation of a new university president at his then home academic institution as an example, Farrell contrasts the pomp, regalia, and "inflated symbolism" of the inauguration with the personage of the incoming president. An inversion of magnitude occurred when the new president, a "smallish fellow" resembling "Woody Allen," presented himself at the podium. All audience expectations of an individual befitting the "institutionalized aggrandizement" of the ceremony were dashed (p. 482). While his choice of examples is not without its problems, Farrell used it illustrate three characteristics of the inversion of magnitude. The first characteristic is that magnitude can produce "a gap between anticipation and performance," meaning that which is expected to be grand, impressive, or important is

actually not in practice (p. 483). His second point, which closely follows the first, is that magnitude arises from unexpected places, and “cannot be created top-down” (p. 483). Lastly, and most pertinent to the work of the Southborough Committee, *megethos* can produce the “collective rejection of an inflated sense of weight” when the significance of an institution is challenged in light of another issue (p. 483). As I will show below, this was the case when the committee privileged psychological explanations of shell shock and thereby challenged British military laws on cowardice.

Farrell’s (2008) essay only suggests these characteristics of the inversion of magnitude, as he does not take the space to delve more deeply into their rhetorical implications. However, I would like to suggest that a more recent essay on the *megethos* of Roman architecture contributes additional insights into the inversion of magnitude, although it somehow overlooks Farrell’s contributions to the concept. In “Diocletian’s Victory Column: *Megethos* and the Rhetoric of Spectacular Disruption,” Jonathan Balzotti and Richard Crosby (2014), examine the rhetorical effects of a 30-meter tall monument erected by the Romans in the third century. They argue that the column demonstrates the rhetorical effects of *megethos* because the Romans used it to redefine the city of Alexandria’s landscape and disrupt and supplant memories of the city prior to Roman rule (p. 336). Their claim that *megethos* can produce disruptive rhetorical effects rests upon their reading of Longinus’ *On the Sublime*. Longinus wrote that the primary effect of *megethos* is to “transport them [the audience] out of themselves” (qtd in Balzotti & Crosby, 2014, p. 338). He explained, “Invariably what inspires wonder, with its power of amazing us, always prevails over what is merely convincing and pleasing” (qtd. in p. 338). The rhetorical power of *megethos* to overwhelm then changes what can be expected

of an audience, disturbing the normal rhetorical processes of argumentation and persuasion. *Megethos* makes that which is familiar to an audience remote and opens the possibility for new discourses to supplant those that were previously privileged. The disruptive power of *megethos* incorporates two rhetorical moves. It first transports the audience out of themselves and prepares them to be receptive to a new discourse. Then, the heuristic power of *megethos* works against established norms to elevate discourses that were previously deemed inferior by an audience. To recast this disruption in Farrell's (2008) terms, the elevation of a new discourse inverts magnitude, diminishing that which is expected and elevating the unexpected to a privileged rhetorical position. The Southborough Committee's inability to uphold the primacy of military laws on cowardice and malingering when faced with the discourses of shell shock represents the disruptive and inverting rhetorical power of *megethos*.

To understand the rhetorical disruption caused by the *megethos* of shell shock, it is first important to understand the cultural and legal discourses of cowardice and malingering in the British military during WWI. The Southborough Committee tersely summarized its understanding of the British military's policy on cowardice as "Cowardice is a military crime for which the death penalty may be exacted" (p. 138). The British Army operated on a strict code of self-discipline, honor, and *esprit de corps*. As Meyers (1940) wrote in his war memoir, "the frequency of 'shell-shock' in any unit is an index of its discipline and unity" (p. 39). Breakdowns in military order were a constant concern during WWI, and shell shock posed a distinct threat to discipline on the battlefield. Like desertion, Myers (1940) wrote, "'shell-shock' is of a highly contagious nature; it may be rife in one unit, while rare, because regarded as a disgrace, in another"

(p. 40). At times, military jurisprudence used cowardice as a blunt instrument to stave off the spread of shell shock by punishing traumatized soldiers.

The case of Private Harry Farr provides a prime example of shell shock being treated as cowardice. Farr, an otherwise exemplary soldier, was executed for cowardice in the face of the enemy on October 16, 1916. His military records indicated that he was treated for shell shock on three separate occasions in 1915 and 1916 (Wessely, 2006). On September 17, 1916, Farr's military unit, the 1<sup>st</sup> Battalion West Yorkshire Regiment, moved up from the rear to the front line to take part in the battle of the Somme, a battle that claimed tens of thousands of British lives. According to testimony at his court martial, Farr attempted to see a medical officer, but was refused because he had no physical injuries. Upon returning to his regiment, he told his commanding officer Regimental Sergeant Major Haking that he would not go into the trenches because he "could not stand it" (Wessely, 2006, p. 440). Haking's response forecasted Farr's fate: "You are a fucking coward and you will go to the trenches. I give fuck all for my life and I give fuck all for yours and I'll get you fucking well shot" (p. 440). While being escorted to the trenches, Farr broke away from his escorts and retreated. During his trial, Farr stated that the sound of the artillery in distance disturbed him. Running out of earshot from the guns quieted his nerves. In the early years of the war, mental breakdown did not fit into the British Army's health paradigm. "Men were either sick, well, wounded or mad; anyone neither sick, wounded, nor mad but nonetheless unwilling to or incapable of fighting was necessarily a coward, to be shot if necessary" (p. 25). Unfortunately for Farr, the doctor who could validate his shell shock claims was wounded and unavailable for his court martial.

It remains unclear if or how many other soldiers executed during WWI had suffered from shell shock. Execution, as Corns (2001) writes in *Blindfold and Alone: British Military Executions and the Great War*, was not a common occurrence. From August 1914 to October 1918, nearly a quarter million soldiers were court martialed, with 3,080 receiving death sentences. Of those sentenced to death, 346 were carried out. Farr, it seems, was one of the “unlucky” few (Wessely 2006, p. 440).

Malingering was historically understood as a form of cowardice in the face of the enemy. Peter Leese (2002) locates the earliest use of the term malingering pertaining to military service in a 1795 book titled *Graves Dictionary of the Vulgar Tongue*: “a military term for one who under pretense of sickness evades duty” (p. 21). In 1843, H. Gavin’s *On Feigning and Factitious Diseases Chiefly in Soldiers and Seamen* contained a reference system for doctors to assess the truth of servicemen alleging illness (Leese 2002, p. 21). Up to the beginning of WWI, there was no explicit military literature on feigning mental illnesses to avoid duty. It appears that the literature on malingerers fabricating the symptoms of railway spine had not drawn the attention of the Royal Army Medical Corps (RAMC). The RAMC’s 1911 training manual contained procedures for insanity and brain damage, but offered no guidance on mental breakdown or war neurosis (Leese, 2002). As one historian conceded in 1922, “No doubt there were men who from one cause or another broke down in every campaign,” but such cases tended not to be documented as they were “not differing greatly from cowardice” (qtd. in Shephard 2001, p. 25). But as Lord Southborough (1922) wrote, “No great military organization could ever accept the proposition that cowardice in the face of the enemy should be looked upon as nothing but a nervous disorder” (p. 72).

When set against the backdrop of the British military's procedures for adjudicating cowardice and malingering, the Southborough Committee's report demonstrates that shell shock transported the witnesses out of themselves, leaving them unable to reach a consensus the definition of cowardice. The committee and its witnesses "were agreed that cowardice should be regarded as a military crime to be punished when necessary by death" (War Office, 1922, p. 139). However, this would be the only point of consent reached. The report cites that "Some witnesses declined to define it [cowardice] and others did so with reservation" (p. 138). The motives of those who declined and those who defined the term with reservation were not catalogued. Their decisions to remain silent or to express their reticence suggests disruptive power of shell shock's *megethos*. By protesting the question of cowardice's definition, witnesses in effect rejected the British military's cultural practices of punishing soldiers who were psychologically incapable of fighting. At its extreme, their protests can be read as the wholesale rejection of a link between shell shock and cowardice, a significant rhetorical inversion of British medico-military precedent.

Testimony included in the official report suggests that no hard and fast definition could be proffered because of witnesses hedged and equivocated their definitions. Major Dowson, "a barrister of considerable court-martial experience," was the only witness to offer a unilateral definition: "Cowardice is showing signs of fear in the face 'of the enemy'" (p. 138). The Committee outright rejected his definition, stating, "Such a definition is not helpful to the medical officer who may be called on to decide between cowardice and 'shell-shock'" (p. 138). With absolutist definitions off the table, the committee attempted to arrive at a definition that used a soldier's spirit rather than his

actions as evidence of cowardice. In their definition, the British service man was inherently brave and courageous. Because no inherently brave and courageous soldier would willingly violate his own nature, some other factor, like exhaustion, must be responsible for undermining him: “Cowardice, if regarded as a lack of or failure to show requisite courage, renders discussion more feasible and assists us in comprehending how the brave after much stress may temporarily fail to show their wonted courage without deserving to be called by an opprobrious term” (p. 138). Without overtly invoking the psychological realm of wear and tear on the body and mind, the committee cites stress as the culprit that can “temporarily” mitigate the iron will of the soldier. Although the serviceman may wish to enact his bravery, the stresses of combat could intercede.

The committee acknowledged that fear was another factor common to both cowardice and shell shock, but fear alone was not sufficient for either condition. The committee stated, “Fear is an emotion common to all and evidence was given of very brave men who frankly acknowledge it” (p. 139). The committee agreed that “fear alone does not constitute cowardice” (p. 138). With the definition of fear agreed upon as a natural emotion, determining what accomplice makes fear a crime proved to be equally difficult as defining cowardice itself. Neurologist Dr. Farquhar Buzzard postulated a neo-Freudian explanation of fear as an unconscious process with a “very definite physical manifestation” (p. 138). He defines cowardice as the “voluntary attitude” adopted by a soldier, and not the outward appearance of fear. To make the distinction legible for the committee, he couches fear in terms of phobia: “the fact that my knees shake when I am looking over the side of a building is an absolute physical thing over which I have no control” (p. 138). Prof. Roussy’s testimony attempts to delimit Buzzard’s definition of

“no control,” defining courage as self-control and marking some actions, like running away, as cowardice because the soldier purportedly has control over such a response to fear.

Again, the committee was left to negotiate extremes. Were all physical responses to fear justified or only those that could not be controlled by the soldier? The committee concluded, “It may then be accepted that neither feeling fear nor manifesting psychological signs of fear—pallor, shaking, tremors, quick pulse do of themselves constitute cowardice though they more or less essential to it (p. 139). As for what physical responses were appropriate, the committee noted that there was an “indefinite line which divides normal emotional reaction from neurosis with impairment of volitional control” (p. 139). Rather than make attempts to draw that indefinite line, the committee advised that “expert medical evidence” should be presented in courts-martial “in future” (p. 140).

The committee came to four conclusions on shell shock and cowardice:

1. That the military aspect of cowardice [i. e. court martial] is justified.
2. That seeming cowardice may be beyond the individual’s control.
3. That experienced and specialised medical opinion is required to decide in possible cases of war neurosis of doubtful character.
4. That a man who has already proved his courage should receive special consideration in a case of subsequent lapse. (p. 140)

Rather than making a firm set of pronouncements on the relationship between shell shock and cowardice, the committee enshrined psychological knowledge as an indelible facet of military jurisprudence. Therefore, it was necessary for medical officers to offer testimony at court-martial proceedings, in much the same way it was necessary for surgeons to offer testimony in railway spine court cases. However, the report contains no guidance for medical officers.



Additionally, the magnitude of shell shock inverted the discourses of malingering, so much so that the committee failed to advise the British Army on how to detect whether soldiers were feigning mental illness or not. Testimony on malingering was even more diverse than on the issue of cowardice. In the absence of statistics, first-hand estimates of soldiers claiming shell shock to shirk duty were highly inconsistent. One commanding officer, Major Adie, stated, “We did not see much malingering” (p. 144). Another officer, Lieutenant-Colonel Scott Jackson, claimed, “Many cases of neurasthenia and ‘shell shock’ were scrimshanking of the worst type” (p. 144). Some doctors, like Dr. Hampton, offered equivocal testimony: “Many cases were on the border line between conscious and unconscious malingering” (p. 144), while others insisted that simulating shell shock was nearly impossible. As Dr. Dunn said, “In acute shock a man abandons himself to his terror. I have not seen an attempt to simulate it, and I cannot imagine such an attempt deceiving anyone” (p. 144). Like cowardice, malingering in its purest juridical form remained a punishable crime, but in the absence of any sound medical guidance, the committee relented and came to no conclusions about the relationship between malingering and shell shock.

Placing psychological medicine closer to the frontlines also influenced the dispensation of military justice. Some military doctors were less apt to offer evidence against soldiers who had suffered mental break downs in courts martial. There is no evidence to support that this shift in military-medico practice was uniform across doctors or trials, but notable cases are documented in the historical literature. Feudtner (1993) wrote that NYDN centers served a “medico-disciplinary function” (p. 398) on the battlefield. If a soldier had been charged with a military crime, like desertion or

cowardice in the face of the enemy, and pleaded before his court martial that he was suffering from shell shock at the time of his crime, he would be admitted to an NYDN center for a period of observation. Over the course of approximately four weeks, NYDN doctors would research the soldier's shell shock claim, "through interviews and examinations to be used in the court-martial" (p. 398). Indeed, this practice added an extra bureaucratic layer to the already Baroque measures of military justice. Consequently, though, the reach of psychological medicine and the knowledge it produced were extended into another facet of soldiers' medico-military experiences.

Dr. William Brown, a psychologist who directed one of the NYDN centers and treated 2,000 to 3,000 soldiers for shell shock (Brown, 1918), recollected the practice at a formal inquiry into shell shock following the war. He stated before the committee that although he found participating in courts-martials to be an "extremely difficult and distasteful task," the evidence he accumulated from the soldier and his comrades persuaded him to always argue on behalf of the accused (War Office, 1922, p. 43-44). Brown justified his sympathy for the accused in terms of medical knowledge, stating that it was impossible for a medical officer to know what was going through the soldier's mind at the time of his offence. He stated, "[A]fter my first two or three courts-martial I found that I was practically in every case giving evidence in favour of the man. The reason was that I felt that his state of mind in the line, when he was under heavy shell fire, was not the same as his state of mind when he was at the Base or somewhere between the Base and line" (p. 44). The distribution of psychological knowledge to the front lines authorized acts that were otherwise criminal in the British military's traditional medical views. Not only was the mind susceptible to the horrors of war, the closer the

soldier's proximity to death, as far as Brown was concerned, the less he was responsible for his actions. Once again, the psychological medicine on the battlefield mitigated the absolutism of medico-military discourses, interjecting a case-based ethics where the absolutism of right and wrong, guilty or innocent had long presided.

The Southborough Committee's collective uncertainty about the nature of cowardice and malingering and the individual doctors' reticence to fastidiously participate in the British military bureaucracy illustrate the far-reaching implications of the *megethos* of WWI. Rather than affirming the primacy of the British military's historical discourses on cowardice and malingering, expert testimony created an inversion of magnitude, elevating the concerns of shell shock above the jurisdiction of military law. This surprising rhetorical performance cemented war trauma as legitimate military concern and gave psychological medicine a new ethos on the battlefield.

## **Conclusion**

In this chapter, I have shown that shell shock exploded onto the battlefields of The Great War, simultaneously fulfilling medical prophecy that mechanized war would cause new forms of mental illness and catching the British military off-guard. Shell shock was perplexingly new, but entirely inevitable. The sheer scale of the First World War made shell shock an overwhelming problem. A minority of British casualties may have succumbed to psychological trauma on the battlefield, but the threat their wounds posed to the Allied war effort made shell shock a rhetorical situation of unprecedented magnitude. Considering the stakes of shell shock, not only for victory on the battlefield, but also for medical theory and practice, this chapter analyzed shell shock and the

discourses it produced through the lens of *megethos*. It is befitting that a term connoting size and value is rich in meanings, meanings it has accrued over the course of two millennia of rhetorical usage.

A chapter accounting for every possible connection between the rhetoric of magnitude and shell shock would itself be overwhelmingly long. With this in mind, I selected three of the most enduring rhetorical situations created by shell shock and analyzed how *megethos* operated in creating their outcomes. In brief, I have attempted to show that the ascendancy of the psychological codification of shell shock benefitted from the rhetoric of proximity, a type of *megethos* that combines the magnitude of distance with the value of the perspective it affords. Battlefield studies of shell shock favored psychological explanations of the disorder over explanations advanced by psychiatrists far from the frontlines. Secondly, treating shell shocked soldiers near the frontlines mitigated the rhetorical situation of maintaining troop levels in combat. This shift in treatment led to rhetorical efforts to redefine shell shock and its treatment. Lastly, I argued that the discourse of shell shock created a rhetorical inversion in military jurisprudence. Shell shock disrupted long-established discourses of cowardice and malingering, causing one highly influential military committee to revise cultural values enshrined in military law.

Each of these historical moments and their effects created a new role for psychologically-oriented psychiatry in the military. The role of physicians on the battlefield expanded to caring for the mind in addition to the body as understandings of war trauma shifted from heredity to the psyche. In military courts, psychiatrists would be the determinants of cowardice rather than lawyers. The hellish realities of war and its

effects on the mind gave psychiatric medicine, and its psychological theories, an accepted place in Western medicine.

While psychiatric medicine proved its worth to armies on the battlefield, psychiatrists faced new challenges when veterans returned from war. This was the case in Vietnam, where fewer than 5% of military personnel were evacuated from combat zones for psychological trauma. Yet when these “psychologically fit” soldiers returned to civilian life, a new epidemic of mental illness exploded, requiring psychiatrists to invent new rhetorical means to codify war trauma. Post-Vietnam Syndrome is the subject of the next chapter.

## Chapter 4: Post-Vietnam Syndrome

### **Introduction**

The contemporary post-traumatic stress disorder (PTSD) diagnosis is inextricably linked to the antiwar movement of the 1960s and 1970s (Nicosia, 2001). The Vietnam War era (1965-1975) was a time of tremendous cultural upheaval and systemic change in both social attitudes and psychiatric medicine. Vietnam veterans were at the center of a rhetorical formation that brought them into collaboration with activist psychiatrists who gave voice to the psychological problems they developed *after* serving their country. Together, they would codify a post-traumatic illness called Post-Vietnam Syndrome (PVS) in 1972, which set into motion an advocacy network that would persuade the American Psychiatric Association (APA) to include PTSD in the third edition of its *Diagnostic and Statistical Manual of Mental Disorders* (DSM-III) in 1980.

In the buildup to the ground war in Vietnam, the U. S. military had put in place a robust medical network that saved countless lives that would have been lost a mere decade earlier in the Korean War. The military also embraced an industrial psychological model of mental health that focused on preventing breakdown by limiting the length of soldiers' tours-of-duty, requiring periods of rest and relaxation, and embedding psychiatrists throughout the frontlines (Artiss, 1963; Talbott, 1969; Westmoreland, 1963). The military was convinced its preventive measures worked because troop evacuations for psychiatric care declined from 10 to 12 per 1,000 troops in 1965-68 to a low of 5 per 1,000 in 1972 (Jones & Johnson, 1975). Compared to the evacuation rates during WWII, which fluctuated between 28 and 101 per 1,000 troops (Tiffany &

Allerton, 1967), preventive psychiatry was ostensibly effective. Because most Vietnam veterans manifested psychiatric symptoms after the war, it was nearly impossible to link their mental illness with their military service. As a result, they could not receive compensation for their disabilities.

To compound matters, there was no codified psychiatric diagnosis that explained veterans' mental illness. Historically, as we've seen in the previous chapters, post-traumatic illnesses were codified for specific reasons at specific moments in time to meet specific social ends. During WWII, the U. S. Army compiled one of the first American nosologies of mental illnesses, *War Department Technical Bulletin, Medical 203*, to ensure that psychiatrists and psychologists uniformly diagnosed soldiers (Hout, 2000). *Medical 203* included an entry for "Combat Exhaustion," a temporary diagnosis that was typically reserved for soldiers who had experienced a period of extreme combat and developed either neurotic symptoms or psychotic reactions afterwards. The APA used *Medical 203* as the basis for its first diagnostic manual (DSM-I) in 1952, but did not retain combat exhaustion as a diagnosis. In its place, the APA added "Gross Stress Reaction," which specifically mentioned combat as a cause for a broad range of reactions to extreme stress. Like combat exhaustion, gross stress reaction was "to be regarded as a temporary diagnosis to be used only until a more definitive diagnosis is established" (APA, 1952, p. 40). When the APA revised DSM-I in 1968—the same year as the Tet Offensive—it removed gross stress reaction, leaving DSM-II without a post-combat diagnosis. Effectively, there was no recognized diagnosis that could explain the relationship between Vietnam veterans' service and their mental problems. Although there was substantial research conducted during and after WWII demonstrating the

potential for the delayed onset of psychological trauma (Grinker & Spiegel, 1945), the military and APA never recognized the connection. Without a post-combat diagnosis, clinicians were left with no option other than to explain soldiers' psychological symptoms in terms of other diagnoses that did not fit their mental problems. Misdiagnosis and nontreatment were common in the DSM-II era (Blank, 1985).

Initially, there was considerable resistance to the notion that Vietnam veterans were psychologically and emotionally wounded by their experiences in the war. Neither the U. S. military, the APA, nor the American public were receptive to the idea that the veterans were suffering from a unique form of mental illness. On the other hand, Vietnam veterans had become deeply distrustful of the government that sent them to war and refused to use the medical resources at VA hospitals their service entitled them. Rather than participate in the VA system, members of Vietnam Veterans Against the War (VVAW) in New York City created their own self-help "rap groups" to build community, find allies, and engage in group healing. It was when VVAW contacted Robert Jay Lifton in January 1970 that a new therapeutic alliance was born, giving shape to the PVS diagnosis and the unconventional healing practices it entailed. Both PVS and PTSD are rearticulations of the concerns expressed by veterans in rap groups as filtered through Lifton's own psychoanalytic "advocacy research" (1972a).

In this chapter, I will trace the constitutive maneuvers of a small group of activist psychiatrists as they worked to introduce a delayed-stress reaction diagnosis into the third edition of the American Psychiatric Association's *Diagnostic and Statistical Manual of Mental Disorders* (DSM-III) in 1980. I will demonstrate that they employed a psychoanalytically informed theory called psychohistory as a counter rhetoric to the



scientifically-based management model of mental health employed by the U. S. military and the APA. By constructing the Vietnam veteran's identity through the lens of psychohistory, they constituted the veteran as the psychologically-wounded survivor of a human-made atrocity on par with the Holocaust and the atomic bombings of Japan in WWII.

In the next chapter, I continue tracing the rhetorical trajectory of PVS by examining how the activist psychiatrists successfully lobbied the APA to include PVS in the DSM-III under the name post-traumatic stress disorder. PTSD's inclusion in DSM-III was controversial and profoundly ironic because PTSD was primarily a psychoanalytically-informed diagnosis and the DSM-III was intended to be an atheoretical text. The inclusion of a psychoanalytically informed diagnosis had therapeutic and theoretical repercussions that endure to this day.

Each chapter employs a different type of constitutive rhetorical analysis, which corresponds to the two types of constitutive rhetorics employed by the activist psychiatrists. In this chapter, I follow the work of James Jasinski, Michel Foucault, and Ian Hacking to examine the exterior constitutive rhetorics employed to circulate a veteran-specific diagnosis called Post-Vietnam Syndrome. In the following chapter, I add S. Scott Graham's (2015) concept of constitutive calibration, which is also derived from Foucault, to examine how the activists persuaded the APA to accept a delayed-stress codification in the DSM-III.

For both chapters, I continue to follow Condit's concept of rhetorical formations and Hacking's historical ontology by compiling a large corpus of medical, governmental, journalistic, and popular press texts from the Vietnam Era and analyzing the rhetorics

operating in it. These texts, which comprise several thousand pages, illustrate how the activist psychiatrists broadly circulated their message to the public, the US Congress, and mental health professionals with the goal of reconstituting the identity of the Vietnam veteran to gain support for a delayed stress diagnosis. As a consequence constitutive rhetorical analysis seems the most appropriate framework for this epoch in the rhetorical history of PTSD because constitutive rhetorics focus on how the circulation of texts and discourses creates and defines subjective and communal identity. The corpus also includes evidence of a positive reception of the activists' constitutive discourses.

At the heart of these chapters remains the same questions I explored with railway spine in the Victorian Era and shell shock in WWI. What rhetorical formations created the exigence for the codification of a post-traumatic illness? How did medicine interpret the exigence and operate within the constraints of the institutions that called upon it to codify a diagnosis? And what were the lasting rhetorical effects and material consequences of that codification? These questions permit me to trace the trajectory of the moving target (Hacking 2002, 2007) that is psychological trauma and the respective efforts to understand it. The target, post-traumatic illness, moves because the circumstances surrounding its codification change. Old situations and the rhetorics they produced fall away. New situations arise and command new rhetorics.

The shift from PVS to PTSD is significant for several reasons. Unlike previously codified post-traumatic illnesses, PVS was the product of activism. No institutions, like legal courts or the military, called upon psychiatric medicine to intervene in a situation. PVS was a grass-roots effort to cast a new light on the psychological suffering of a generation of young people who served in an unpopular war. The psychoanalytic

methods Lifton and his activist employed were intentionally unscientific, and their goals were as equally aimed at ending the war as changing psychiatric medicine. Yet, as I will argue in Chapter 4, to effect changes that would result in the recognition of veterans' psychological suffering, they shifted their rhetoric to accommodate the discourses of the APA, the medical institution that, in part, created the veterans' mental health crisis. Lastly, the diagnosis the APA codified was itself an attempt to halt psychological trauma as moving target, by creating a universal diagnosis that would not have to be attuned to future exigences and rhetorical formations.

The turbulent 1970s produced a complex rhetorical formation that witnessed significant social and medical changes. This chapter reflects that complexity. As Condit (1999) writes about the challenges of working with rhetorical formations, “[P]ublic advocates speak to attract the assent and identification of multiple audience members with multiple agendas of their own. Thus the rhetoric that gets created and repeated is multidimensional. It consists of a complex lacework of connected ideas” (p. 15). Condit’s lacework metaphor is particularly fitting for this era. In what follows, I speak to the complex rhetorical formation the American soldier returned to as a veteran during the Vietnam war. Then I discuss constitutive rhetorics in greater depth and demonstrate how they operated in the codification of Post-Vietnam Syndrome.

### **Background: The Vietnam Era Rhetorical Formation**

The rhetorical formation pertaining to the Vietnam veteran, both during the era and in some regards to this day, was significantly different than the rhetorical formation of military veterans of previous generations. World War II veterans, who would later be

heralded as “The Greatest Generation,” returned home with the comrades they fought beside to victory parades great and small. In the public mind, they were victors over a genocidal enemy set on world domination. Vietnam veterans, many of whom were the sons and daughters of WWII veterans, made solitary returns to an indifferent, if not hostile, public that could not comprehend the frontier war they fought in a small decolonized country in Southeast Asia (Dean, 1997). According to Gallop’s polls, sixty-four percent of Americans approved of the war in 1965. By January 1969, less than forty percent approved of the war with fifty-two percent indicating they thought our involvement in Vietnam was a mistake. In 1970, fifty-five percent wanted to bring all service members home by the end of 1971 (Stempel, 1988, n. p.).

The young soldiers themselves—who were on average 22.7 years old, six years younger than WWII veterans (Pannill, 1970, p. 475)—grew increasingly disillusioned by their public reception and the morally ambiguous war they waged. Sociologist and Vietnam veteran, Charles Moskos (1975) found that American combat soldiers’ attitudes toward the war declined steadily over three distinct periods of the war. From the beginning of the war in 1965 to 1967, troop cohesion and morale were relatively high; 1968 to 1969 was a “transition period” that saw increasing demoralization; and 1970 to 1972 was marked with significant breakdowns in discipline, including more than 300 instances of enlisted soldiers “fragging” (i. e. killing) their commanding officers (Moskos, 1975, p. 25).

Although the anti-war movement began simultaneously with the war, it did not have a widespread effect on active duty service members until 1969, when the now infamous massacre at Mylai 4 became public knowledge (Hersh, 1970). The massacre of

Vietnamese civilians perpetrated by U. S. Army Lt. Calley and Charlie Company, 1<sup>st</sup> Battalion, 20<sup>th</sup> Infantry, on March 16, 1968 (although not reported for several months) was the breaking point for the American public's and some Congressional Representatives' sympathies for the war. Of Lt. Calley's trial, Senator George S. McGovern said in a television interview,

I think that for the first time millions of Americans are realizing that we have stumbled into a conflict where we not only of necessity commit horrible atrocities against the people of Vietnam, but where in [sic] a sense we brutalize our own people and our own nation. I think it's more than Lieutenant Calley involved here. I think a national policy is on trial. (qtd. in Hersh, 1970, pp. 157-158)

This singular event was a black mark on all military personnel, which changed not only the public's perceptions of soldiers, but also their perceptions of themselves. One officer serving in Qui Nhon, Vietnam, went so far as to publish an op-ed lambasting the war and the American public in the January 18, 1971, edition of *The New York Times*. Captain B. C. Ewing, no military branch affiliation mentioned, lamented,

We are no longer gloriously fighting an enemy. We are tragically destroying ourselves. ... While you are talking of "a just peace," of reduced casualty rates, and of all the magnificent plans to end this conflict, your sons are being physically and morally corrupted by your war. It saddens me to think that when I go home in August, someone else will have to take my place. (1971, p. 38).

Four months to the day after Capt. Ewing's letter was published, Vietnam Veterans Against the War (VVAW) marched on Washington, D. C., in what became known as Operation Dewey Canyon III: A Limited Incursion into Congress. On the fifth day of the six-day protest that brought together more than one thousand veterans, John Kerry famously asked the Senate Foreign Relations Committee, "[H]ow do you ask a man to be the last man to die in Vietnam? How do you ask a man to be the last to die for

a mistake?” (Kerry, 1971, p. 18). Kerry also questioned the morality of military operations in Vietnam. He admonished the U. S. military, asserting, “[W]e are more guilty than any other body of violations of those Geneva Conventions; in the use of free-fire zones, harassment of interdiction fire, search-and-destroy missions, the bombings, the torture of prisoners, the killing of prisoners, all accepted policy by many units in South Vietnam” (p. 22). Social historian Wilber J. Scott (1993) observed that emphasizing atrocities was a preferred VVAW rhetorical tactic for demonizing American military policy; however, it also stoked the fires of the public’s contempt for Vietnam veterans (p. 19). In addition to delineating what he perceived as the numerous injustices of the war, Kerry (1971) pointed to the poor quality of veterans’ healthcare as a breach of trust between the U. S. government and its warriors, declaring “so many of those best men have returned as quadriplegics and amputees—and they lie forgotten in Veterans Administration hospitals” (p. 16).

Stateside care for the wounded was an issue that drew Congress’s attention early in the war. In November 1969, the U. S. Senate Subcommittee on Veterans’ Affairs held its first of seven hearings on the “Oversight of Medical Care of Veterans Wounded in Vietnam.” In his opening remarks, subcommittee chair, Sen. Alan Cranston marveled,

Enormously devastating wounds are suffered by our men there due to the nature of the weapons they face. Then, after they suffer these awful wounds, unprecedented things happen. We pick up the wounded men from the field, where they have fallen, with helicopters—in a matter of moments, often—and have them in a matter of minutes in a field hospital where miracle drugs are administered and where the most modern advanced techniques in medicine are applied. (Cranston, Nov. 21, 1969, p. 1)

The swift and expert medical attention wounded soldiers received contributed to the highest wounded-to-killed ratio in any American conflict to that point. Many lived who

previously would have succumbed to their injuries. As a consequence, the large number of traumatized and physically disabled veterans placed a significant strain on the VA system (Scott, 1991, pp. 8-9). The discrepancy in quality between the medical treatment soldiers received in Vietnam and their care in VA hospitals when they returned was heatedly debated throughout the hearings.

Capt. Max Cleland, who testified on behalf of Paralyzed Veterans of America, recounted the psychological toll of his life-threatening wounds. Cleland lost part of his right arm and both legs in an explosion. In his testimony, Cleland lauded “the most fantastic processes known to modern science” that saved his life, and indicted his VA rehabilitation as “based on World War II and the Korean experience” (Cleland, 1969, p. 272). He also spoke of the psychological challenges of returning to an ambivalent if not hostile American public:

To the devastating psychological effect of getting maimed, paralyzed, or in some other way unable to reenter American life as you left it, is the added psychological weight of that it may not have been worth it, that the war may have been a cruel hoax, an American tragedy, that left a small minority of young American males holding the bag. (p. 273)

Cleland added that he was not alone in needing help readjusting to civilian life. “Anyone who deals with a Vietnam returnee, *wounded or not*, must understand this delayed, severe psychological symptom” (p. 274, emphasis added). While veterans like Cleland understood the psychological toll of fighting, the better part of a decade would pass before the U. S. government and the APA would recognize this specific type of psychological suffering as a legitimate mental illness.

The idea that soldiers needed help transitioning back to civilian life was common in the era. The military’s DEROS (Date Eligible for Return from Overseas) policy—one

of its innovations to prevent psychological breakdown in combat—was frequently cited as a cause of soldiers’ readjustment issues (Bourne, 1972b). Unlike WWII, where soldiers went to fight indefinitely, DEROS limited soldiers’ tours of duty to twelve months (thirteen months for Marines). Therefore, soldiers who served in Vietnam knew the dates when they were scheduled to return home. While the policy was a strong prophylaxis against combat stress, it undermined unit cohesion. Since WWI, psychiatrists were aware that the *esprit de corps* of a unit and comradery among its men predicted whether soldiers would succumb to the privations of war (See Myers, 1941). Yet because each Vietnam soldier knew the date his tour ended, he was unlikely to make close human connections with those around him. The war itself became a game of survival, where the soldier could count down the days until he cycled out of Vietnam back to the U. S. As psychiatrist Peter Bourne (1972a) observed, DEROS made the war an individual effort. Upon arriving in Vietnam, the soldier “has no investment in the war and its outcome; his war begins the day he arrives and it ends the day he leaves” (p. 23). Also, the personalized cycling schedules contributed to unpredictable personnel shifts in leadership and hostility among seasoned soldiers toward “F. N. G.’s”—fucking new guys (Shatan, 1985, p. 16).

Additionally, even though each soldier fought a delimited, personalized war, the common trope of bringing the war back home emerged at some point during the Vietnam era. Psychologically affected veterans frequently claimed that their bodies were in America, but their minds remained at war in Vietnam (Bourne, 1972, p. 24). The unpopularity of the war contributed to “reentry shock” (p. 24). As Bourne (1972) explains, young veterans returned from the most significant events in their lives to being



treated as “some kind of traitor to his generation”; “it is not so important that he be treated as a hero, but that some recognition be given to the experiences and suffering he has endured” (p. 24). This lack of recognition would later be cited as a cause of PVS.

Adjustment issues, however, plagued even the war’s greatest heroes. On May 26, 1971, *The New York Times* published a lengthy obituary for Dwight (Skip) Johnson. President Lyndon Johnson awarded Johnson the Congressional Medal of Honor for “uncommon valor” after he single-handedly took on the battalion of North Vietnamese soldiers who attacked his tank platoon. The commendation made Johnson a home-town hero in his native Detroit. Although major corporations courted him with high-paying jobs, Johnson remained in the Army as a recruiter and financial problems ensued. One week before his 24<sup>th</sup> birthday, he was shot dead as he attempted to rob a grocery store.

To punctuate the suffering of the tragic hero, Johnson’s obituary’s writer, Jan Nordieimer (1971), included excerpts of Johnson’s VA Hospital records and quotes from his psychiatrist. When Johnson missed several days of work as a recruiter, he was referred to the Valley Forge Army Hospital in Pennsylvania, where he recalled in detail his nightmares, flashbacks, and survivor guilt about the 30-minute rampage that earned him the Medal of Honor. His psychiatrist noted, “The subject remembered coming face to face with a Vietnamese [sic] with a gun. The gun jammed. ... He asked: ‘What would happen if I lost control of myself in Detroit and behaved like I did in Vietnam?’ (Nordieimer, 1971, n. p.). Johnson’s VA diagnosis was “Depression caused by post Vietnam [sic] adjustment problem” (n. p.).

Johnson’s tragic story and its explicit mention of adjustment problems figured prominently in the continual efforts throughout the era to understand who the Vietnam

veteran was and why his return was so painful. A flurry of studies was conducted at research institutions, one of which found that, “This is probably the most capable and highly educated generation of veterans in history, with powerful latent motivation to contribute to the rebuilding of America, and their own society thinks of them as dregs and dropouts, dehumanized killers and drug addicts, or pitiful victims of a hated war to be avoided and shunned” (Wren, 1973, p. 234). Historian Murray Polner (1971) conducted qualitative case studies of nine Vietnam returnees and found that despite their political orientations, soldiers returned from the conflict questioning why they went. He concluded, “never before in American history have as many loyal and brave young men been as shabbily treated by the government that sent them to war; never before have so many of them questioned as much, as these veterans have, the essential rightness of what they were forced to do” (Polner, 1971, 165).

With the cultural revolution in full swing and WWII fresh in the cultural imaginary, institutions like the VA sought to better understand a new generation of veterans. Who the Vietnam veteran was figured prominently in conceptualizations of why he had trouble readjusting to society and how that trouble could be alleviated. In the next section, I discuss how theories of constitutive rhetoric create a generative framework for understanding how the identity of the Vietnam veteran and his relationship to society led to the codification of Post-Vietnam Syndrome, a diagnosis that medicalized the veterans’ war experiences and suggested unconventional therapeutic measures tailored to his identity.

## **Constitutive Rhetorics and Competing Theories of the Vietnam Veteran**

In May 1972, the Veterans Administration's Department of Medicine and Surgery published *The Vietnam Veteran in Contemporary Society*, an edited collection of scholarly essays and Congressional testimony aimed at giving the interested reader a "ready reference" of "all phases of his [the Vietnam veteran's] military and civilian adjustment" (Sherman & Caffey, 1972, p. I-2). The collection circulated after the VA's Chief Medical Director instructed all VA hospitals to establish Vietnam Era Veterans Committees to "critically review operating policies and procedures governing the hospital environment in light of the personal characteristics and human needs of the young veteran" (p. I-1-I-2). Among the findings, which foreshadow the patient first rhetoric typical of medical care facilities today, were hospitals should treat the patient as a "person rather than a passive recipient of an array of depersonalized services," and "We [VA] are dealing with a qualitatively different veteran in a qualitatively different social environment" (p. I-2).

The VA's struggle to understand Vietnam veterans and the war they fought was common throughout the era. Social, medical, and government texts all strove to define the Vietnam veteran and his experience of war. The VA's struggles were that of an institution from a bygone era recognizing the need to shift its theories and practices of medical care for a new generation of veterans. Dueling conceptions of who the Vietnam veteran was made the practice of medicine difficult for an institution accustomed to treating the needs of an older generation of veterans. A common trope of the era was that the Vietnam veteran was "unique," but what exactly made the veteran unique among other veterans was highly contested. Even though VA administrators intuited that the

Vietnam veteran was different, their patient-first rhetoric was insufficient for dealing with a generation of soldiers who distrusted the medical institution created for their fathers.

In this section, I discuss constitutive rhetoric, a type of discourse that aims to create identity and community. Specifically, I will examine the relationship between medicine and constitutive rhetoric to set up a discussion of how medical theories construct the patient and the symptoms she presents in different ways. By reviewing constitutive rhetorics of medicine, I set up the analysis in the remainder of this chapter that shows how the broad circulation of the rhetorics produced by a small group of influential psychiatrists created a particular image of the Vietnam veteran and cast his readjustment difficulties as Post-Vietnam Syndrome. After analyzing the initial circulation of the PVS diagnosis, I will demonstrate in Chapter 4 how this group of psychiatrists calibrated their diagnosis to accommodate the constitutive requirements of the largest and most influential U. S. psychiatric organization, the APA, which eventually included PVS in the DSM-III under the name post-traumatic stress disorder. To illustrate how the process of rhetorical constitution of the patient takes place, I begin with the example of the rhetorical construction of the migraine patient.

In her study of migraine sufferers, Judy Segal (2005) makes two nested arguments. The first is that the migraine patient is the rhetorical construction of the published discourse of migraine headache experts, that is medical doctors. From this first claim follows a second: the published discourse operates ideologically in the clinical setting and affects the care the patient receives (Segal, 2005, p. 50). In a review of 130 years of medical texts on migraine, Segal observes an important rhetorical shift from examining the symptoms of the migraine to who the migraine patient is. This shift

created a “migraine personality” that doctors could then “profile” and treat through “personality reform” (p. 41). From the late nineteenth to the mid-twentieth centuries, the identity of the patient became central to the clinical encounter because doctors had only to compare the patients before them to the archetype of the migraine patient to diagnose and treat the condition. This procedure shifted accountability to the patients who were then responsible for altering their personal attributes to cure their migraine problems (p. 52). In the 1990s, the migraine discourse shifted with the creation of pharmaceutical drugs targeted at migraines, and the clinical encounter shifted accordingly. With new biological treatments for migraine, patients were no longer viewed as victims of their own personalities. They became agents who could manage their condition through pharmacotherapies (pp. 56-57).

Implicit in Segal’s case study is the observation that changes in medical theory led to changes in the codification of migraine. Each theory created a different identity for the migraine sufferer. That is, from the nineteenth through mid-twentieth centuries, migraine patients were people who brought about their own suffering through the lifestyle choices they made. Later in the twentieth century, that version of the patient gave way to biochemical conceptions of migraine as something happening to migraine sufferers’ neuroanatomy. Segal (2005) uses the term “literature effect” to account for the ways that doctors circulate observations generally and how those observations are acted on in clinical encounters (p. 40). Rather than treating a patient as a totally unique individual, the doctor uses the circulated information as a schema to treat the patient as a special kind of person. She reinforces this rhetorical process of identity creation by stating: “My point is not that patients ought to be treated only as individuals in the

medical interview and not as instances of kinds. *Kinds* is an essential notion of diagnosis” (p. 40, emphasis in original).

Segal uses a version (albeit light) of Charland’s (1987) constitutive rhetoric to explain the “literature effect” in which doctors constitute patients in certain ways in the clinical encounter. Briefly, in Charland’s (1987) theory, audiences are constituted by speech. Following Althusser’s theory of interpellation, in which a subject is invited or hailed by a discourse and asked to participate as a certain identity, Charland argues that discourse creates an audience. His primary example of this procedure is the publication of the 1979 “White Paper” that addressed French-speaking Canadians as a particular kind of audience, the “people Quebecois.” He argues that because audiences do not exist outside of rhetoric, the “White Paper called on those it has addressed to follow narrative consistency and the motives through which they are constituted as audience members” (p. 232). He continues, “the very moment of recognition of an address constitutes an entry into a subject position to which inheres a set of motives that render a rhetorical discourse intelligible” (p. 232). In other words, recognizing yourself as a member of an audience automatically makes you part of that audience and aligns you with that audiences’ aims.

I would like to point to two problems here before moving onto my own examples and a more robust version of rhetorical constitution. The first problem is with Charland’s theory. Charland argues that naming an audience calls that audience into being. I am reminded of Michael Warner’s aphorism that texts do not create publics. That is, while giving a potential audience a name may create a new way for people to identify themselves, there is no guarantee that people will indeed take up that mantle, nor does it necessarily follow that the named audience will ever exist. What Charland describes is

the constitutive *potential* of a text. A text may create an audience, or it may not. The second problem is Segal's application of Charland's theory of constitutive potential. Like Charland, Segal presumes that because a migraine authority composes a text on migraine patients, the migraine patients automatically became an audience. A more accurate rendition of the constitutive process would be that a migraine expert advances a theory of the migraine patient. Then if the theory is adopted by other clinicians and applied in the clinical encounter, then the patient is rhetorically constituted as this or that type of patient. The creation of an identity is only part of the rhetorical process of constitution; its circulation and adoption round out the process.

In "A Constitutive Framework for Rhetorical Historiography," James Jasinski (1998) traces two trajectories of rhetorical identity formation. Extending the work of Michael Leff, Jasinski (1998) argues that a distinction can be drawn between a text's intentional constitutive potential and extensional constitutive force (p. 74). The intentional realm includes the text's potential to create an audience through tropes and arguments that invite an audience to identify with the text. Charland's (1987) examination of the White Paper exemplifies this type of analysis because he focuses on the capacity of the document's audience-building potential entirely, rather than if it successfully created an audience or was used to certain end—the White Paper did neither. By contrast, the extensional realm of rhetoric is concerned with the ways a text enables or limits subsequent action through its cultural circulation, adoption, and articulation. Jasinski (1998) explains, "Texts invite their audience to experience the world in certain ways via concrete textual forms; audiences, in turn, appropriate, articulate, circulate,

and/or subvert these textual forms in ways that release and transform their potential constitutive energy” (p. 74-75).

The concepts of constitutive potential and constitutive force complement Ian Hacking’s (2002; 2007) theory of historical ontology and bring a much-needed rhetorical dimension to one of the theory’s central processes, people making. Hacking derived his concept of historical ontology from Michel Foucault (2003), who coined the term in his essay “What Is Enlightenment?” In this essay, Foucault argues against “transcendental” studies of history that “search for formal structures with universal value” as a form of critique (p. 53). The grounds for his argument are that the things we take for granted as “universal, necessary, obligatory” are the products of “whatever is singular, contingent, and the product of arbitrary constraints” (p. 53). Whether intentionally or not, Foucault and Hacking, move the philosophical enterprise of critique into the realm of rhetoric by focusing on practical considerations instead of metaphysical ones. Foucault further elaborates that this type of critique is “a historical investigation into the events that have led us to *constitute* ourselves and to recognize ourselves as subjects of what we are doing, thinking, and saying” (p. 53, my emphasis). The central questions of Foucault’s historical ontological method are, “How are we constituted as subjects of our own knowledge? How are we constituted as subjects who exercise or submit to power relations? How are we constituted as moral subjects of our own actions?” (p. 56).

Hacking (2002) generalizes Foucault’s historical ontology into a type of inquiry that “is concerned with objects or their effects which did not exist in any recognizable form until they are objects of scientific study” (p. 11). Both Hacking and Foucault are interested in the constitution of people. Hacking, in particular, is interested in the



categorization of people, which he calls “making up people” (2002, 2007). He offers a heuristic framework for the process based on five interacting elements: classification, the people being classified, the institutions that would benefit from those classifications, knowledge about the classified person, and experts who legitimize the knowledge (Hacking, 2007, p. 296-297). Following Foucault, Hacking admitted that the process of people making is far from a universal given. “On the contrary,” Hacking (2002) wrote, “we constitute ourselves at a place and time, using materials that have a distinctive and historically formed organization” (p. 3).

I endorse Hacking’s concept of making up people as a way of constituting ourselves. As this dissertation argues, the process of codifying post-traumatic illnesses is a process of people making that is historically contingent. Railway spine, shell shock, and post-Vietnam syndrome are creations of and for their times. Up to 1980, when PTSD was officially recognized, each historical shift in circumstances commanded a new post-traumatic diagnosis. PTSD, as I will discuss later, was and is an attempt to create a universal diagnosis that exists outside of history and culture. My contention is that rhetorical action is necessary in each case of making up people, even if it is overlooked. Hacking’s process of people making and Foucault’s questions of constitution beg the question of how we use rhetoric constitute to ourselves and others. I think new ways of constituting ourselves do involve Hacking’s five interacting elements, but what makes those elements create new objects or identities is the constitutive potential and constitutive force of the discourse circulated by experts, and in the case of PVS/PTSD, the people being classified.

As I will argue in the cases of PVS and PTSD below, circulation, articulation, and reception are the central rhetorical processes of making up people. In what follows, I demonstrate how the circulation of a psychoanalytically-informed view of the Vietnam soldier shifted the understanding of who the Vietnam veteran was and why he had trouble reintegrating into society. Prominent psychoanalyst, author, and antiwar advocate, Robert Jay Lifton's theory of psychohistory recast the Vietnam veteran as a survivor of a human-made atrocity, thus shifting the social concerns away from the actions of the soldier in combat and toward the event of the war itself and its psychological effects on those who participated in it.

### **Rap Groups**

In January 1970, Jan Barry the president of the Vietnam Veterans Against the War (VVAW) sent a letter to Robert Jay Lifton, a notable psychoanalyst and public intellectual who was at the vanguard of articulating the psychological effects of the Vietnam War on soldiers. Years later, Lifton recalled the invitation Barry extended in the letter: "He said two things in the letter: 'Guys are hurting. They're opposed to the war, and they want to deal with their hurt, and they done want to go to the VA. They also want to make known to the world what the war is like. Can you help us in some way?'" (Nicosia, 2001, p. 161). Lifton's staunch antiwar politics and ability to publish his opinions across media outlets made him a natural fit for the VVAW and its mission to heal the minds of soldiers and halt the war. Lifton accepted the invitation and invited Chaim Shatan, a tireless antiwar advocate and co-director of New York University's postdoctoral psychoanalytic training clinic, to join the endeavor.

One of the lingering questions for Lifton, who was more theorist than clinician, was what form the therapy sessions should take. Along with Shatan and Barry, Lifton met with members of the VVAW who told them how intensely they “rapped” with each other in the VVAW about their experiences in the war, their perceptions of society, and their difficulties adjusting to civilian life. The veterans wanted to formalize the rap group sessions and invited Lifton and Shatan to participate as “coequals” (Scott, 1993, p. 15) in “anti-war collegueship” (Lifton, 2005, p. 76). The veterans sought out the partnership because they reasoned that both parties brought special knowledge to the process of healing. As Shatan recalled, “They said shrinks could join provided that we joined as peers. They knew more about the war than we did, and we knew more about what makes people tick” (Scott, 1993, p. 15).

The first rap session took place on December 12, 1970 in the VVAW’s New York City headquarters. Lifton (1973) described the initial meeting between the veterans and psychiatrists as an “explosion of feeling” about the war; the two-hour planned meeting stretched to nearly four hours (p. 76). Both Lifton and Shatan quickly realized that they were pioneering “a new group form” of therapy that drastically departed from the established roles of doctor and patient typical of psychoanalysis (p. 77). The veterans questioned the psychoanalysts’s (who were referred to as “professional” or “shrinks” instead of “therapists”) motives and commitments to the antiwar effort, which Lifton, Shatan, and the other invited psychologists and psychoanalysts from New York City found “a bit jarring,” but eventually valued and even enjoyed (p. 77).

Lifton embraced the idea of a nonhierarchical form of dialogic therapy and wanted to preserve the self-organized feel of the groups imposing the barest of guidelines

to facilitate the group. Three principles guided the rap group. The first was “*affinity*, the coming together of people who share a particular (in this case overwhelming) historical or personal experience.” The second principle was “*presence*, a kind of being there or full engagement and openness to mutual impact—no one ever being simply a therapist against whom things are rebounding.” Lastly, “*self-generation*, the need on the part of those seeking help ... to initiate their own process and conduct it largely on their own terms so that even when calling in others with expert knowledge, they retain major responsibility for the shape and direction of the enterprise” (Lifton, 2005, p. 77-78). Although the group was constituted as a means of personal psychological transformation, its secondary goal was to publicize the “the destructive personal experiences of the Vietnam War” to the American public (p. 81). Traditional media coverage proved to be too intrusive to the core of veterans who participated in the weekly Saturday meetings. Early on, the veterans even discouraged Lifton from taking notes during the sessions because he could not be fully present as both a participant and a documentarian. As the number of veterans attending in the sessions grew, the original rap group was split in two. After the split, one group changed locations and lasted a few months. Lifton’s group lasted another three years. In total, 115 veterans would rotate would come and go through his group. During that time, a number of spin-off groups were formed across the country, proliferating the new group form and creating a network of allies who would be crucial to the successful inclusion of PTSD in DSM-III in the years to come.

Although a small number of the 2,594,000 veterans (Statistic Brain) who served in South Vietnam ever participated in a rap group session, all were affected by the constitutive rhetorics circulated by Shatan, Lifton, and others who wrote about their

experiences with the group. Through publications and media coverage, the Saturday group therapy sessions were the rhetorical foundation of a sea-change in the identity of the Vietnam veteran. To understand how the shift in discourse surrounding the Vietnam veteran took place, it is important to understand the psychoanalytic theory that Lifton brought to the group and how it contributed to the codification of Post-Vietnam Syndrome. In the next section, I review the theory of psychohistory that recast the veteran in terms of the events he survived.

### **The Constitutive Rhetoric of Psychohistory**

In addition to his notoriety in the public sphere, Lifton brought his theory of psychohistory to the rap groups, which would inform his and Shatan's scholarly and popular writings on the findings from the sessions. Lifton was first introduced to the concept of psychohistory by Erik H. Erikson<sup>1</sup> in the mid-1950s, who described it as a means of vivifying the stagnant psychoanalytic enterprise by applying the principles of psychoanalysis to individuals who had experienced moments of historical change (Lifton, 1974, p. 11). In 1964, Lifton and Erikson created a research network in Wellfleet, MA, to develop the inchoate idea of examining the relationship between the individual and the collective during and after significant events.

More than a dozen researchers participated in the Wellfleet meetings, but it was Erikson and Lifton who limned out the scope of psychohistory with their two polar

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<sup>1</sup> Erik H. Erikson began his career as child psychoanalyst studying the child rearing practices of Native American tribes in the 1930s and 1940s. During WWII, he worked for the U. S. government as a psychological expert examining the psychodynamics of enemy leaders. He introduced the concept of psychoanalytic psychohistory in his 1958 biography of Martin Luther, titled *Young Man Luther* (Pietikainen & Ihanus, 2003).

positions. At the more traditional pole, Erikson relied upon Freud's theories of "man's collective individual and collective past" (p. 27) to examine historical figures, like Ghandi and Martin Luther, through "essentially ... the psychoanalyst's interpretive detachment, but modified by an active awareness of his own historical subjectivity" (p. 16). Lifton focused on the horrors of the world as experienced by average (nonfamous) individuals who lived through them. In Lifton's psychohistory, atrocity was a catalyst for social change, which shifted the relationship between individuals who directly experienced atrocity and the collective they attempted to rejoin afterward. Unlike Erikson, Lifton foregrounded his politics and perceived his scholarship as a means of advocating against atrocity-producing situations and for those who survived them.

The earliest articulation of Lifton's theory is found in his study of the "*hibakusha*" or "explosion-affected person(s)" who directly experienced the detonation of the atomic bomb on Hiroshima in WWII (Lifton, 1967, p. 6). It was during his six-month study that he refined his psychohistorical method of abandoning science, seeking out survivors, and interviewing them to identify "themes, forms, and images that in significant ways shared, rather than upon the life of a single person as such" (Lifton, 1974, p. 31). In *Death in Life* (1967), his study of the *hibakusha*, he explains his decision to eschew the individual, "I have assumed ... that psychological occurrences in Hiroshima have important bearing upon all of human experience. ... [W]e are all survivors of Hiroshima and in our imaginations, of future nuclear holocaust" (p. 478). Lifton argued on numerous occasions that his ethical involvement, intellectual rigor, and disclosure of his advocacy makes his work more scientific than biological psychiatry because scientific accuracy suffers when researchers claim that they are neutral toward

the ethical aspects of their work (Lifton, 1978, p. 211). “We have got into the habit of being ‘neutral screens’ and detached therapists. That part of our tradition still bedevils us. We have to begin to combine our professional skills with a return to the ethical passion that we seem to have lost at many points along the way” (Lifton, 1972a, p. 271).

The concept of the survivor is central to Lifton’s project. He defines the survivor as “one who has come into contact with death in some bodily or psychic fashion and has himself remained alive. From this broad perspective we may compare patterns we have observed in Hiroshima to those of other ‘extreme’ historical experiences, particularly the Nazi persecutions, but also the plagues of the Middle ages” (Lifton, 1967, p. 479). In the wake of the My Lai massacre, Lifton added the Vietnam War to that list. The concept of the survivor includes five thematic patterns that can be seen across atrocities. The first is “the death imprint” that encompasses the survivor’s loss of a sense of invulnerability and internalization of images of death and the dead (p. 480-488). Concomitant with the death imprint is “death guilt,” or the belief that one’s survival was purchased at the cost of another’s life. It leads the survivor to question why she survived instead of another (p. 489-499). “Psychic numbing” is the survivor’s psychological distancing from the world in response to the experience of death (p. 500-510). The theme of “Nurturance and Contagion,” later called the “death-taint” (1972), is mutual suspicion the survivor and her society share for each other. Society—Lifton’s preferred term for social communities small and large—discriminates against the survivor because the survivor is perceived as contaminated by death, and although the survivor acknowledges her need for help, she resents the help offered by others (p. 511- 524). Finally, “formulation” is the survivor’s

need to grieve for the dead and to recognize that world has changed by forging new relationships (p. 539).

I have enumerated Lifton's theory of psychohistory to show its constitutive potential as a medical theory. Psychohistory operates by identifying an "atrocious" and claiming that those directly affected in body or indirectly affected in psyche are survivors of the extreme situation. Rhetorically, the psychoanalytically-informed notion of trauma as the experience of an event in the world operates differently than previous conceptualizations of trauma in psychiatric medicine. Thus far in this dissertation, we have seen how trauma was configured as a problem of the body in Victorian England with the codification of railway spine by British surgeons who argued that trauma is an invisible injury to the nervous system. The shell shock phenomenon in World War I gave rise to a different understanding of the post-traumatic injury. Empirical research on shell-shocked soldiers conducted near the battlefields of France suggested that shell shock was a psychological injury induced by fear rather than a physical wound to the central nervous system caused by the concussive effects of exploding munitions. The theoretical shift in the location of trauma contravened earlier theories of shell shock as either a trauma to the body, hereditary disorder, or a manifestation of cowardice. However, both railway spine and shell shock were pathological individual responses to the outside world. In psychohistory, it is the event in the world that is pathologized as an atrocity, thus rhetorically constituting the person who experienced it as a survivor. The survivor's identity is further constituted by her negative or suspicious reception by her society. In previous codifications of post-traumatic illness, surgeons or psychiatrists constituted the individual as traumatized by arguing for the relationship between her symptoms and an



event. In psychohistory, the patient is constituted by her membership in a group of people who collectively experienced an atrocious historical event. Psychohistory brings together the event and the symptoms in a manner that previous codifications of trauma did not. While the logic of psychohistory can readily be applied to both railway spine and shell shock because both entail people living through (more or less) atrocious events, psychohistory hinges on the five themes of the survivor that are absent in those codifications. Psychohistory does not account for functional symptoms of trauma like paralysis, tremors, or stuttering. It focuses purely on the survivor's mind and her relationship to society. The only corporeal element is that the survivor lived during an atrocity and was a member of a society.

### **Framing the Vietnam War as Atrocity: Early Circulations of Psychohistory**

The public circulation of Lifton's theory of psychohistory maps neatly onto the Vietnam War era. *Death in Life* was published by Random House in 1967. Prior to its publication, Lifton already had an expert's ethos on Asian wars and atrocities. During the Korean War, from 1951 to 1953, he served as an Air Force psychiatrist in Korea and Japan. In 1961, he published *Thought Reform and the Psychology of Totalism: A Study of "Brainwashing" in China*, in which he presented data collected from interviews of 25 American and European prisoners of war and 15 Chinese citizens who escaped indoctrination training at Chinese universities. In 1954, Lifton accompanied Betty Jean Kirschner, his wife, on a journalism assignment to Vietnam (Indochina), and "stayed long enough to become impressed with the Vietnamese Revolution" (Lifton, 1973, p. 15). He was also associated with Harvard University's Center for East Asian Studies.

In 1969, at the height of public outrage about the My Lai massacre, *U. S. News & World Report* printed an interview with Lifton, titled “Why Civilians Are Victims.” In the wide-ranging interview, he explained how racism, the military’s process of indoctrinating new recruits into the mentality of war (which would later be called combat brutalization), and the historical background of the conflict contributed to the massacre. When asked if Vietnam veterans would bring back “mental scars” that would affect their adjustment to civilian life, Lifton (1969) acknowledged that all veterans return from war with some degree of guilt about “being in a situation where it has been legitimate to kill,” but that Vietnam veterans bring home the “inner sense” that “one has done evil things for a very dubious or nonexistent cause” (p. 27). The implication here is that justified killing or killing that is approved by the soldiers’ nation and its citizens do not result in psychological discord. The collective’s acceptance of the individual’s deeds in war through victory celebrations or other forms of ceremonial recognition exculpates the veteran and reintegrates him or her into society.

Although Lifton doesn’t name psychohistory in the interview, his answer to the question of adjustment includes the notion of the veteran as survivor. Twice in the interview he implies that the soldier is a victim of the war because he was “put” in “situations” where he had to commit atrocities. For example, he states that the Vietnam veteran has “been put in the situation where, if one really is honest with oneself, one wasn’t sure whether the people one was killing were civilians or enemy guerillas, or why one was killing anyone at all in Vietnam” (p. 27). The etiology of the soldiers’ psychological suffering (guilt) was the situation itself combined with how they were forced to act resulting in uncertainty about who was killed. When asked if Vietnam

veterans would have greater difficulty adjusting than previous veterans, Lifton couches his answer in the theme of formulation: “[W]hen one is a survivor of this kind of disturbing experience, readjustment depends upon finding significance in one’s survival—giving it meaning” (p. 27). He avers that there are two ways veterans can find significance in their service. The first is defending their actions and the war itself. The second way, “which a number of veterans of this war seem to be finding, is by looking critically at the nature of the war as a way of finding significance in one’s own experience in it, and then perhaps saving others the pain of having to go through this kind of experience” (p. 27). In other words, protesting the war is a psychiatric therapy.

Two months after his interview in *U. S. News & World Report*, Senator Alan Cranston, chair of the Senate Subcommittee on Veterans Affairs, invited Lifton to testify on the psychological readjustment issues veterans faced in relation to their service in Vietnam. Lifton opened his remarks with a brief overview of psychohistory, then proceeded to cast the Vietnam War as an “extreme situation” and made the case for why the veteran should be considered a survivor when reentering society. “Upon returning to civilian life,” he stated, “the war veteran faces several important psychological tasks in relation to deaths he witnessed. He must, first of all, struggle with anxiety he continues to feel, often in association with the indelible images of death, dying, and suffering that constitute the survivor’s ‘death imprint’” (Lifton, 1970, p. 492). However, the negative emotions, namely guilt and shame, the veteran feels after surviving the war can be alleviated if he or she is “able to find meaning in all else he does afterward in civilian life” (p. 492).

Before the subcommittee, Lifton argued that characteristics of the war and the society to which the veteran returned contributed to the veteran's psychological troubles. With My Lai still making headlines, Lifton took the occasion to state that, based on his interactions with veterans, the massacre was the *de facto* state of warfare in Southeast Asia. "Virtually all of them [the veterans he spoke to] had either witnessed or heard of similar incidents, if on a somewhat smaller scale" (p. 493). Soldiers were trained to dehumanize the enemy, which contributed to the "psychic numbing" that made the perpetuation of atrocities commonplace (p. 494). While this scenario may have been true of previous wars, Lifton observes that the death taint from a "filthy, ambiguous war" prevents the veteran from being socially absolved of the "terrible burden of guilt" when he returns home: "We sent him as an intruder in a revolution taking place in a small Asian society, and he returns as a tainted intruder in our own society" (p. 496). As an intruder, the veteran develops a profound distrust of his own society, to the point that he refuses even to seek the help offered by the VA.

The rhetorical move from the individual to the collective is characteristic of the theory of psychohistory, making it a powerful identity-constituting discourse. Lifton universalized the veteran-as-intruder experience to all who served in Vietnam, constituting them as a class of veterans who are uniquely in need of psychological treatment: "Whatever kind of adjustment the returning Vietnam veteran appears to be making, he must continue to carry images of these experiences inside of him. Survivors of a special kind of war, these men constitute a special kind of veteran's group" (p. 494). Lifton, and his colleagues as we will see, was not speaking about a certain subtype or percentage of Vietnam veterans. He was constructing the identity of all veterans with his

testimony. Anyone who participated in the war was a survivor; therefore, this concept includes all who served in Vietnam.

Lifton's testimony added another dimension to the psychohistorical constitution of the Vietnam veteran, the concept of delayed stress. Some veterans, he argued, manifested the guilt, shame, and distrust immediately upon returning home, "but in others lie dormant for a period of months or even years and then emerge in response to various internal or external pressures" (p. 495). In other words, the psychological effects of surviving an extreme situation could develop later in the veteran's life without any ostensible connection to the Vietnam War. This claim, based on Lifton's research on the *hibakusha* and published research on Holocaust survivors, echoed the studies on delayed stress conducted after WWII that were ignored by the U. S. Army in *Medical 503* and the APA in DSM-I.

Senator Alan Cranston, obviously persuaded by Lifton's psychohistorical assessment of the war and its survivors, thanked him for his "remarkable and very, very helpful testimony" and homed in on the policy implications of delayed stress (p. 496). Cranston asked if the law limiting service connected mental illness to two years after discharge should be revised. Lifton responded, "I would be willing to claim that a condition that occurred 40 years after a war, if in my psychiatric evaluation it was related to that exposure 40 years ago, had some connection, and should be legally recognized as such" (p. 499). Lifton also implied that delayed stress could possibly be prevented if the American public shared in veterans' sacrifices by acknowledging their suffering and working to authentically accept the veteran into society (p. 503-504).

Not all present at the hearing were as receptive to Lifton's constitution of the Vietnam veteran as survivor. In his testimony, the psychiatrist mentioned that his observations were based on formal and informal conversations with approximately 50 soldiers who served in Vietnam and the U. S. and four or five psychiatrists who worked with Vietnam veterans. Senator Schweiker, the only other member of Congress to speak during Lifton's testimony, inquired whether the 50 were randomly selected, hinting at a bias in Lifton's sampling. With palpable indignation, Lifton responded that they were random people that he had met throughout the country, adding, "I haven't attempted myself to make a systematic study of veterans" (p. 509). However, by the end of the year, in December 1970, Lifton would begin his study of the VVAW rap groups and publish his influential book on Vietnam veterans, *Home from the War*, in 1973. And the issue of statistically-significant systematic research would reemerge as a problem in 1975, when Lifton and his colleagues approached the APA requesting PVS be included in DSM-III.

Lifton's testimony circulated broadly in the press, academic circles, and in edited collections on the Vietnam veteran, including the VA's *The Vietnam Veteran in Contemporary Society*. Lifton's reconstitution of the Vietnam veteran as a survivor of an atrocity competed with other common portrayals, including the veteran as baby killer (DeFazio, 1975), the veteran as ticking time-bomb (Dean, 1997), and the veteran as a gung-ho John Wayne figure (Gustainis, 1993). His psychohistorical analysis of the war and its veterans did not put these other constitutions to rest; however, Lifton's testimony, in the words of Cranston, "added a new and important dimension to our [Congress's] efforts to understand the psychological impact of the Vietnam war, and it will be helpful to us as we seek to write legislation" (p. 510). By the end of the decade, Cranston would

pass legislation acknowledging Vietnam veterans' post-traumatic illnesses and the delayed effects of the war.

Although Lifton convinced a key Senator with his analysis of Vietnam veterans' adjustment issues, Chaim Shatan would add to the constitutive force of psychohistory by codifying it into an easy-to-understand diagnosis for the American public, and Sarah Haley, a social worker in Boston VA hospital, would publish a study that transformed how clinicians perceived and treated Vietnam veterans. In the remainder of this chapter, I review these two kairotic moments of conceptual change in the rhetorical history of PTSD, focusing on how they reconstituted the image of the Vietnam veteran.

### **Post-Vietnam Syndrome and Arguments to the Public**

The U. S. Congress was not the only audience that needed persuading about the plight of the returning veteran, although it was most directly responsible for sending U. S. soldiers to Vietnam. The rap group participants and their psychoanalyst leaders understood the importance of the public's perceptions of veterans in the rhetorical constitution of the Vietnam veteran. The New York media, whether intentionally or not, offered the VVAW and those affiliated with it a national platform for circulating their ideas, and, concomitantly, for reconstituting the image of the Vietnam veteran. It did so first by publicizing the existence of rap groups, then by acting as a venue for the publication of the psychoanalysts' findings from the group. This was a serendipitous union for Lifton and Shatan, who understood support for the veterans' cause was a matter of persuading the general public. As Shatan (1973) wrote,

Our goal is to give the widest publicity to the unique emotional experiences of these men; to do so, we go—together with the veterans—

wherever we will be heard: conventions, war crimes hearings, churches, Congress, even abroad. As a psychoanalyst, I find it hard to write and talk about this work ... Yet I accept the responsibility to disseminate the knowledge imparted by veterans. (Shatan, 1973, 650-651)

Jasinski (1998) wrote that there were at least four dimensions of constitutive rhetorical practice. Much like psychohistory's emphasis of the relationship between the individual and the collective, constitutive discourses can be organized hierarchically from the subjective outward. Constitutive discourse facilitates self-constitution and the formation of subjectivity or subject positions; provides linguistic resources for a culture; shapes the norms of political culture; and organizes individual and collective experiences of culture (p. 75). Guided by the aegis of Lifton's psychohistory and the veteran's own self-help ethos, the rap groups created new rhetorical subject positions for their participants by offering veterans a venue to make meaning of their military service. Lifton's congressional testimony also interjected psychohistorical discourse into the political culture of the day. However, a chasm remained in the domain of the public that occupied the social terrain between the individual veterans and the government that sent them to war. Lifton (1973) articulated this constitutive need in a *New York Times* op-ed: "Whatever his [the veteran's] struggles upon return, many Americans continue to see him in terms of those two roles—as a 'junkie,' 'powderkeg,' or 'murderer,' rather than as the lovable G.I. who came back from earlier wars" (p. 31). These images, whether they were forged by veterans' actions or media portrayals, disidentified the public with the veteran, cleaving society from its responsibilities for the veterans' actions in Vietnam and for the veterans' psychological recovery from the suffering caused by those actions. "What we must begin to realize," he wrote,



is that large segments of American society have a stake in this psychological separation of veterans from the rest of us. For veterans' very existence among us can serve as a reminder of events and images we wish desperately to forget. This division between them as deviant and disturbed and the rest of us as all right can serve the further psychological function of placing the full onus of the Vietnam war on the men sent to fight it rather than upon the society that sent them. (Lifton, 1973, p. 31)

Lifton's constitutive discourse in the *New York Times* not only reshaped the collective identity of Vietnam veteran, but also created "a particular collective identity to legitimate particular ways of collective life by transcending individual differences" (Drzewiecka, 2002, p. 3). By circulating a national discourse of the Vietnam war, Lifton, Shatan, and the VVAW implicated all of America in the suffering of the 115 who participated in the New York City rap group. Through the constitutive logic of psychohistory, those 115 were no different than the 2.5 million who served in the war.

By the time the Lifton penned this op-ed aimed at organizing individual and collective experiences of culture, readers of the *New York Times* and *The New Yorker* magazine, were already familiar with rap groups. Less than a year after Lifton and Shatan joined VVAW, *The New Yorker* magazine gave its readers an insider's view into the rap groups. Its September 4, 1971, edition featured a lengthy investigative essay, in which writer Daniel Lang told the story of one veteran's journey from boot camp in 1965 through one tour in Vietnam to a successful career in industry and finally to the VVAW's Manhattan office. Lang (1971) reported on the grief and bewilderment of the veterans who participated in the rap groups, as well as the veterans' penchants for alternately acting out at the group and consoling one another. The story gave the impression of a rag tag bunch shepherded by "college professors" who guided the group with sensitive discussion prompts like "I sense hostility here, but I do not hear it expressed" (p. 49).

Lang's portrayal of the rap groups was gritty, but it ultimately elucidated how a Saturday afternoon of "random military recollections and responses to civilian life" (p. 49) could play such an influential role in the readjustment of the successful, college-educated veteran he followed into the group. He wrote, "But, whatever the V. V. A. W.'s faults, he told me, being in it sustained in him a sense that he had not yet numbed himself to what he had seen in Vietnam" (p. 52-53). Despite his subject's ostensibly smooth transition to civilian life (represented by a college degree and steady employment), he, too, was psychologically affected by the war.

Lang's "Home Again" warrants mention because it is one of few examples of public writing that focused on the rap group participant's experience. Historical studies of the Vietnam Era and the veterans' antiwar movement frequently cite men's interest magazines, particularly *Penthouse*, as forums for personal investigations into the veteran experience (Nicosia, 2001; see O'Brien, 1974), but whether pornographic magazines constitute public discourse is debatable when circulation and rearticulation are considered. Other writing about rap groups was circulated to niche audiences in academia.

Arthur Engendorf, a listless ivy leaguer turned Saigon spy handler, has written most prolifically about his subjective experiences in rap groups. His stories of how he found the VVAW and ascended to a prominent role in the rap groups were published in academic journals and edited collections, obviously skewed toward scholarly audiences by his professional ambitions in psychiatry as well as his desire to justify the rap group as a legitimate therapeutic form (Engendorf, 1985). Yet his academic writings provided a lucid explanation of how and why veterans like himself sought communion in the rap

groups. In “Vietnam Veteran Rap Groups and Themes of Postwar Life,” he recalls that the rap groups served as a philosophy circle for those involved. He explained, “Others’ desire to deny what we did or deny their own feelings about the war seems to heighten our need to explore what it means to be a veteran, to make sense out of what we in the groups consider to be the single most important period of our adult lives” (Engendorf, 1975, p. 113).

Engendorf’s personal recollection demonstrates the constitutive force of Lifton’s psychohistory. He describes postwar life as consisting of themes, a parallel to the themes of survivorship, and he construes the group’s mission as a meaning-making enterprise, which is suggestive of the formulation stage of survivorship. My point here is that Lifton’s theoretical influence became naturalized in the discourse of rap groups to the extent that it organized the participants’ own experiences of the self-help talk therapy form. Yet bringing psychohistory and the therapeutic benefits of rap groups to the public required equipping the public with the linguistic resources of the psychoanalysts themselves. That is, the public had to be convinced that veteran’s difficulties readjusting to civilian life constituted an unacknowledged but real medical condition called “Post-Vietnam Syndrome.”

According to historian Gerald Nicosia, Shatan sparked a “raging wildfire” and “psychiatric revolution” with the publication of a 1972 *New York Times*’ op-ed titled “Post-Vietnam Syndrome” in 1972 and the scholarly article “The Grief of Soldiers” in 1973. Both pieces were based on his presentation at the 1972 annual meeting of the American Orthopsychiatric Association. The audiences for the publications were different, but the codification of PVS as a delayed stress disorder based on Lifton’s

psychohistory was the same. In the op-ed, Shatan (1972) recounts his work with the self-help rap groups. Although the groups were intimately associated with the antiwar movement, Shatan omits any mention of the VVAW and offers a condensed sympathetic picture of the rap groups that relates their formation to the veterans' delayed stress. He wrote, "The meetings were initiated in 1970 by veterans themselves, either because of their distrust of 'establishment' psychiatric services or because their disturbances manifested themselves too late to prove the 'service connection' required for Veterans Administration treatment" (Shatan, 1972, p. 35). Following Lifton's thematic analysis, Shatan asserts that six "commonly shared themes" emerged in the self-help discussions. Because the themes are consistent across participants and "do not fit any standard diagnostic label, we refer to them loosely as the post-Vietnam syndrome" (p. 35). The basic themes (i.e. the symptoms) of the syndrome were survivor guilt, feelings of victimization by the government and American public, rage at the government for duping them into service and at the public for not acknowledging their sacrifice, "combat brutalization" or the feeling that their combat training and actions in the war zone dehumanized them, alienation and emotion numbing, and doubt about their ability to give or accept affection in their relationships with others (p. 35).

In concluding his codification of PVS, Shatan (1972) posited the etiology of the syndrome was that soldiers had survived an extreme situation on par with "death camps" (p. 35). This experience, coupled with a military culture that would not permit them to grieve their fallen comrades, resulted in a psychiatric condition: "The post-Vietnam syndrome confronts us with the unconsummated grief of soldiers—'impacted grief' in which an encapsulated, never-ending past deprives the present of meaning" (p. 35).

The op-ed was a watershed moment in the history of PTSD. Shatan later recalled in an interview, “After that [the publication of the op-ed], the telephone was jumping off the wall ... things started mushrooming” (Scott, 1993, p. 43-44). With the codification of PVS, Shatan became a “delayed stress gadfly” who travelled the country to share his research at professional conventions and with veterans groups (Nicosia, 2001, p. 180). Rap groups sprang up in virtually every major metropolitan area with VVAW’s New York City office functioning as a clearinghouse for information on PVS. In addition to resounding with veterans and psychiatrists working with veterans, the concept of PVS drew the attention of Holocaust survivors and the robust global research community that studied them. At academic conferences, Shatan’s conversations with survivors of the Holocaust and the 1918 influenza pandemic helped him to see common symptoms across survivor groups. His relentless quest to share the psychological suffering of a small group of New York City antiwar veterans with audiences far and wide constituted the Vietnam veteran as a survivor in the public mind. It also constituted the Vietnam veteran as a member of a class of people who had lived through the worst human-made catastrophe in history, the Holocaust.

### **Atrocity and Arguments to Psychiatrists**

The idea that the Vietnam veteran was a survivor of atrocities that he witnessed or perpetrated was codified in PVS and the within the psychohistorical framework undergirding the diagnosis. Lifton and Shatan’s endeavors to publicize the suffering of the rap group veterans was at its core an effort to reconstitute Vietnam veterans’ collective identity. As Drzewiecka (2002) observes in her study of the Polish diaspora in

America, “National discourses play an important role in group’s expression of their belonging, identities, and political affiliations” (p. 2). Like Jasinski (1998), she argues the circulation of these discourses invites members of the group to participate in the identity being created through the constitutive potential of a discourse. When members accept that invitation, they enter the extensional realm of constitutive discourse because they rearticulate the rhetorically constructed identity. To put it another way, they become Hacking’s made up people. I argue that Lifton and Shatan successfully accomplished their rhetorical goal of reconstituting the identity of the Vietnam veteran to Congress and the American public. Here I would like to review two examples of the constitutive rhetoric circulated to clinicians who worked with Vietnam veterans. This is a key group for reconstituting veterans’ identities as survivors because the clinic is where veterans relate the atrocities they committed and where they receive treatment for the psychological suffering caused by those acts. In this section I will review two widely circulated articles instructing clinicians on how to interact and treat veterans who report atrocities. This type of constitutive discourse is crucial to the eventual codification of PVS as PTSD because it orients clinicians to perceive veterans sympathetically when they confess to horrific actions that have the potential to impede a beneficial therapeutic relationship.

Cases of clinicians terminating therapeutic relationships with veterans or otherwise not upholding their professional responsibilities to their Vietnam veteran clients are well documented in the academic and historical literature of PTSD (Blank, 1985; Haley, 1974; Nicosia, 2002; Scott, 1993; Shephard, 2000). Blank (1985) catalogues the “irrational reactions” that were “sufficiently common as to produce remarkable

nationwide professional phenomena” of clinical bias against Vietnam veterans prior to 1980, the year that PTSD became an official diagnosis (p. 83). The two most common of these types of bias were nondiagnosis and nontreatment of veterans’ symptoms. Blank (1985) attributes these biases to a “generalized professional paralysis,” a clinical term for failure to do one’s job caused by fear of veterans or confusion about their cases (p. 85). Other identifiable patterns of bias include “generalized hostility and contempt toward Viet Nam veterans” and hostility toward other professionals who treat them (pp. 86-87).

Gault (1971) addressed the problem of clinician bias after he encountered numerous veterans who confessed to slaughtering defenseless Vietnamese civilians while taking patient histories at the Mental Hygiene Consultation at Service at Fort Knox (p. 451). Like Lifton’s (1973) notion of atrocity, Gault sees slaughter as an integral aspect of the Vietnam war itself. However, he cautions his colleagues to avoid “the conventional mistake of assuming that [the] patient sees things more or less” the same way they do (p. 451). Since most psychiatrists are “totally unacquainted with the necessary brutal realities of combat,” they may project their own beliefs and emotions onto the veteran rather than treating the veteran’s symptoms accordingly. He also observes that slaughter may or may not cause feelings of guilt in a patient, which can lead the clinician to conclude that the guilt-free patient is concealing real emotions about his actions. Gault (1971) does not “draw any large lessons from [his] observations;” rather, he advises his colleagues to perceive veterans as “fairly ordinary young men” who have engaged in incomprehensible acts (p. 454).

Haley (1974) reported on her clinical experience with a soldier who claimed to have been present at the My Lai massacre, but did not participate in the killings. From her

sessions with this veteran and 40 others who claimed to have participated in atrocities, she learned that the greatest challenge to helping the patient was keeping him in therapy. Haley distilled her professional experience nurturing “therapeutic alliances” with veterans claiming atrocious behavior. “When the patient reports atrocities,” she wrote, “my experience has been that the first task of treatment is for the therapist to confront his/her own sadistic feelings, not only in response to the patient, but in terms of his/her own potential as well” (p. 194). Following Lifton and Shatan, both of whom she cites in the paper, she adopts the rhetoric of psychohistory, inviting the therapist to imagine herself in an extreme situation where she may be compelled to commit an atrocity. Once the therapist understands her own capacity to perpetuate horrors, then she and the veteran can enter an alliance. In the parlance of psychoanalysis, “the therapist must align himself with that part of the patient’s ego that now views his actions as ego-alien, and explore with the patient those factors that occurred when his usual sense of right and wrong gave way” (p. 195). In a nod to Lifton’s theme of formulation, which requires the survivor to forge new relationships in the wake of catastrophe, Haley writes, “Establishment of a therapeutic alliance for this group of patients [atrocious-committing veterans] *is* the treatment rather than the facilitator of treatment” (p. 195). The therapist functions as a social microcosm that models the acceptance the veteran needs to heal but cannot find elsewhere. Haley encourages therapists to employ this strategy with all Vietnam veteran patients, even those who do not claim atrocities, because, “The only report that should not be accepted at face value, although one may choose not to challenge it initially, is the patient’s report that combat in Vietnam had no effect on him” (p. 196). In effect, Haley extended the philosophy that shaped the rap groups and PVS into the clinical setting.



Influenced by psychohistory, she constructed a totalizing Vietnam veteran identity by asserting that all soldiers are psychologically affected by their service. With the immediate popularity of “When the Patient Reports Atrocities” in the psychiatric community, Haley successfully reconstituted the Vietnam veteran in traditional therapeutic circles. With Shatan and Lifton, she would become a driving force for the inclusion of PTSD in DSM-III.

### **Conclusion: People Making and Psychohistory**

In this chapter, I have argued that Vietnam Veterans returning from the war entered a rhetorical formation that was injurious to their mental health and antagonistic to their mental health needs. In response to this formation, a small but dedicated group of antiwar and proveteran psychiatrists reconstituted the image of the Vietnam veteran by circulating arguments based on Lifton’s theory of psychohistory to the U. S. Congress, the general public, and the community of psychiatric professionals. These constitutive efforts are an example of Hacking’s concept of people making because the acceptance and rearticulation of the Vietnam veteran as survivor identity created a new type of Vietnam veteran whose readjustment difficulties were the product of the war in which he fought rather than the product of his own antisocial behaviors.

The broad circulation of Lifton, Shatan, and Haley’s writings not only created a new identity for the Vietnam veteran, but also interjected a radical form of psychoanalytic theory into medical, governmental, and public discourse on veterans. Delayed stress, a concept that was actively denied by the military and APA following WWII, gained traction as a viable explanation for veterans’ difficulties readjusting to

civilian life. The constitutive force of PVS with its rhetoric of atrocity reconstituted the Vietnam veteran as a member of a class of people that included survivors of human-made disasters, like the Holocaust. Although Lifton, Shatan, and Haley successfully constituted PVS as a legitimate explanation for veteran's readjustment problems in government, public, and clinical spheres, PVS was not an actionable psychiatric diagnosis because it was not officially recognized by the APA in DSM-II. Without this formal recognition, Vietnam veterans were not entitled to benefits or healthcare from the VA, regardless of their newly reconstituted identities as survivors of atrocity.

As the Vietnam Era drew to a close, the June 1974 issue of *Psychiatric News* included the announcement that another revision to the DSM was in works. When Shatan discovered that there was no plan to include delayed stress or combat-related trauma in the DSM-III, he mobilized delayed stress advocates and researchers. To have PVS officially recognized in DSM-III, it would once again need to be reconstituted, this time to meet the APA's scientific criteria for a diagnostic entity. In the next chapter, I examine how the ardently antiscientific psychoanalytic activists adopted the APA's atheoretical rhetoric to recalibrate PVS as PTSD.

## Chapter 5: From Post-Vietnam Syndrome to Post-traumatic Stress Disorder

### **Introduction**

On March 29, 1973, the last American military unit left Vietnam. Two years later, on April 30, 1975, Saigon, Southern Vietnam's capital city, was occupied by the communist Army of the Republic of Vietnam, officially ending the United States involvement in the small Southeast Asian country. The end of the Vietnam War may have brought a sense of closure to the antiwar movement, but it did not stifle the delayed-stress advocates' efforts to have disciplinary psychiatry officially recognize the psychological suffering of Vietnam veterans. For the duration of the 1970s, Robert Jay Lifton, Chaim Shatan, Sarah Haley, and their colleagues from Vietnam Veterans Against the War (VVAW) advocated for the inclusion of a combat-related delayed stress diagnosis in the third edition of the American Psychiatric Association's (APA) *Diagnostic and Statistical Manual of Mental Disorders* (DSM-III). Their quest ended in 1980 with the publication of DSM-III. However, Post-Vietnam Syndrome (PVS), the adjustment disorder Shatan and Lifton codified from their observations of the Vietnam VVAW rap groups, never made it into DSM-III. Instead, a new diagnosis called Post-Traumatic Stress Disorder (PTSD) was added to the manual's section on anxiety disorders.

There were substantial nosological and ideological differences between PVS and PTSD. Where PVS delineated the symptoms of rage, alienation, psychic numbing, and grief that soldiers experienced when they returned from the Vietnam War, PTSD's "essential feature [was] the development of characteristic symptoms following a

psychologically traumatic event that is generally outside the range of usual human experience” (APA, 1980, p. 236). Those characteristic symptoms were “reexperiencing the traumatic event; numbing of responsiveness to, or reduced involvement with, the external world; and a variety of autonomic, dysphoric, or cognitive symptoms” (p. 236). Like PVS’s characteristic delayed-onset of symptoms, PTSD’s symptoms could manifest “immediately or soon after the trauma” or “emerge after a latency period of months or years following trauma” (p. 237). Although PTSD resembled PVS in many respects, PTSD was not PVS.

The transfiguration of the officially-unrecognized PVS codification into the officially-sanctioned PTSD diagnosis in the DSM-III is insufficiently accounted for in the medical literature about PTSD. For example, one prominent psychiatry textbook offers this one-sentence acknowledgement of PTSD’s relationship to the Vietnam War: “The psychiatric morbidity associated with Vietnam War veterans finally brought the concept of posttraumatic stress disorder into full fruition as we know it today” (Kaplan, Sadock, & Grebb, 1994, p. 606). The entry, while accurate, elides the political conditions pertaining to Vietnam veterans and completely omits PVS. At the opposite end of the spectrum, there is some literature that seeks to discredit PTSD because of its political origins. One government employee associated with the Congressional Committee on Veterans Affairs summarized the critical position thusly:

Early proponents of post-traumatic stress disorder were characterized by critics as “crackpots, screwball, self-serving psychologists and psychiatrists who were probably all against the war anyway and were only looking for a surefire way to get some money out of the Veterans Administration.” Or worse, in the minds of traditionalists, these proponents were part of the most visible and activist symbol of the antiwar movement in the United States: the Vietnam Veterans Against the War

who were “probably all crazy before they got into the service in the first place.” (Fuller, 1985, p. 6)

Others acknowledge the history of PTSD in passing. For example, one evolutionary biologist wrote, “There is evidence that PTSD is likely to have existed in much earlier times” (Canter, 2005, p. 18). Most contemporary PTSD researchers avoid the question of history altogether, as is the case in published research reports on various vectors intersecting with the diagnosis. History, it appears, simply isn’t relevant to their research programs.

In tracing the rhetorical history of PTSD, neither the easy causation of the textbook’s explanation nor the satire of the government worker’s rendition are helpful in examining the rhetorical processes by which PVS became PTSD. In fact, compared to previous chapters in this dissertation, this chapter, which aims to recount a brief but crucial episode in the history of PTSD, is at disadvantage because no *in media res* accounts of the creation of the PTSD codification process were published in the medical literature. As I will explain below, the delayed-stress advocates joined the APA’s Committee on Reactive Disorders to negotiate a diagnosis that was amenable to both parties. Where in previously codified post-traumatic illnesses, such as railway spine and shell shock, there is a dialogue among researchers in the published medical literature, there is no such record for the early codification of PTSD. Therefore, the deliberations that historically took place in front of the medical community were sealed off from outsiders under the auspices of committee work.

However, there is a limited secondary literature on the negotiations of the Committee on Reactive Disorders that includes interviews with some of the key stake holders and decision makers, some of whom are now deceased. The secondary literature

does not grant me direct access to the exact arguments and tactics used to negotiate a combat-related delayed stress disorder into the DSM-III, but there is sufficient information from those directly involved in the committee's negotiations as well as from the two historians whom have documented the inclusion of PTSD in DSM-III (Nicosia, 2001; Scott, 1993). The available information demonstrates that the delayed-stress advocates adopted an additional constitutive rhetorical strategy to align PVS with the inclusion criteria of the DSM-III project, which required the delayed-stress advocates to accumulate empirical scientific data on delayed stress.

This episode in the rhetorical history of PTSD raises the issue of the increasingly bureaucratic nature of Western biomedicine and the institutional constraints on the codification of mental health diagnoses that heretofore have only been hinted at in this dissertation. While a thorough review of the professionalization of medicine and psychiatry are beyond the scope of this dissertation, this dissertation has indirectly traced the processes by which doctors create and govern medical knowledge<sup>2</sup>. For example, in the latter half of the nineteenth century, surgeons published their accounts of railway spine and its mechanisms and treatments in medical journals, monographs, cyclopedias, and at meetings and lectures. These fora, especially journals and books, sustained and documented their deliberations and provided a convenient record of the evolution of medical codifications for researchers then and now. In the nineteenth century, there was little institutional regulation of the codification process, as railway spine was preliminarily codified for use in legal courts where lawyers called upon surgeons to offer expert testimony. Medical consensus and success in court were the dominant metrics of

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<sup>2</sup> Starr (1982) offers an excellent history of the professionalization of American medicine. Shorter (1998) and Burns (2014) each trace the major developments of psychiatry.

railway spine diagnosis's utility. Considering that shell shock was codified and recodified under the auspices of the British Royal Army, military psychiatrists enjoyed a good deal of freedom to research and publish in academic medical journals. The strength of the research in the journals, and more importantly the efficacy of the research outcomes and treatments, were sufficient to persuade the military bureaucracy of the benefits of a psychological model of shell shock during the war. However, by the Vietnam Era, psychiatrists and researchers had begun to express their concerns about psychiatry's relationship to bureaucratic social institutions, like the military (Daniels, 1969; Lifton, 1973). The professionalization of American psychiatry, which brought with it the need to regulate and standardize diagnoses, made the process of codifying new mental health diagnoses more rigorous than it previously had been in the history of post-traumatic illnesses.

Against this backdrop of medical bureaucracy, this chapter tells the story of how the small group of activists that codified and circulated PVS gained access to the bureaucratic structure of the APA and influenced it to include a combat-related delayed stress diagnosis in its nosology. This chapter follows the line of inquiry developed in Chapter 4 that employed constitutive rhetoric as a theoretical framework for understanding how PVS became a codified syndrome. However, building on my discussion of psychiatric bureaucracy, I argue in this chapter that delayed stress advocates engaged in a different type of constitutive rhetorical practice than they used to reconstitute the identity of the Vietnam veteran in public and governmental spheres. Noting that Jasinski (1998) left open the possibility of non-public constitutive rhetorics, I synthesize insights from Barton's (2000) conceptualization of institutional discourse,

Lyne's (1985) rhetorics of inquiry, and Graham's (2015) constitutive calibration to examine how the advocates succeeded in negotiating aspects of PVS into the PTSD diagnosis in DSM-III. This chapter also demonstrates that as the shift from PVS to PTSD uncoupled delayed stress from the antiwar politics of VVAW, it also downplayed the role of combat-stress as a cause of psychological disturbance by making it but one of many potential causes of PTSD.

To set up my discussion of how the delayed-stress advocates negotiated the inclusion of a diagnosis based on PVS in the DSM-III, I must first briefly recount what some historians and critics of psychiatry refer to as the "crisis of legitimacy" in the psychiatric profession. The series of events that constitute the crisis gave rise to a theoretical change in disciplinary psychiatry that influenced the creation of DSM-III. Then I will briefly recount some psychiatrists' resistance to the official recognition of a delayed-stress diagnosis.

### **The Crisis in Psychiatry and DSM-III**

The history of post-traumatic illnesses is also the history of tensions between the biological and psychological paradigms of psychiatry. In the latter half of the nineteenth century, Erichsen (1863) codified railway spine as a biological wound to the central nervous system that manifested functional (i.e. mental) symptoms. Page (1883) adamantly argued against this codification, citing evidence that the mental symptoms of railway spine were the product of autosuggestion caused by fear; intense emotion, he concluded, rather than lesions caused by train accident survivors' psychic ills. The shell



shock phenomenon during WWI shifted the psychiatric paradigm away from the biological model toward a psychosocial understanding of mental trauma.

For the next sixty years, the psychosocial model, which integrated aspects of psychoanalysis and psychodynamics, remained the dominant theory in psychiatry (Shorter, 1997). Menninger (1963), one of the foremost authorities on psychosocial psychiatry, described the practice thusly: “We must attempt to explain how the observed maladjustment came about and what the meaning of this sudden eccentricity or desperate or aggressive outburst is. What is behind the symptom?” (p. 8). Like psychohistory, the psychosocial model of mental illness focused on the relationship between the individual and the outside world. Mental illnesses (maladjustments) arose when the individual failed to adapt to changes in her environment. In effect, all mental illnesses shared the etiology of failed adaptation; the only difference was the magnitude of the maladjustment (Wilson, 1993, p. 400). As Menninger wrote, symptoms were less interesting than identifying the root cause of the maladjustment.

By the 1970s, a confluence of factors convinced the APA that a paradigm shift away from psychosocial psychiatry to a descriptive biological model was in order (Wilson, 1993; Strand, 2011; Mayes and Horowitz, 2005; Butler, 1999). The earliest challenges to the psychosocial model came from outside the profession by the pharmaceutical and insurance industries. By the 1960s, medications, such as lithium carbonate, were proven to be clinically successful for stabilizing patients’ moods. However, pharmaceutical researchers demanded more explicit diagnostic criteria to develop new and better medications (Wilson, 1993, p. 404). Private insurance companies

demanded greater clarity for the treatments they covered, with most viewing talk psychotherapy as a “financial bottomless pit” (Mayes and Horowitz, 2005, p. 253).

The state of research within the profession also suffered. From 1965 to 1972, the National Institutes of Mental Health decreased research funding by five percent each year. Later in the 1970s, President Carter’s commission on mental health attacked the current state of psychiatry: “Documenting the total number of people who have mental health problems ... is difficult not only because opinions vary on how mental health and mental illness should be defined, but also because the available data are often inadequate or misleading” (as cited in Wilson, 1993, p. 403).

Also in the early 1970s, a series of events occurred in which individuals or entities outside of disciplinary psychiatry questioned or challenged the reliability of diagnoses in DSM-II. Historians and critics refer to this period as psychiatry’s “crisis of legitimacy” (Mayes and Horowitz, 2005; Rosenberg, 2006; Wilson, 1993). For example, in 1973 the journal *Science* published “On Being Sane in Insane Places,” a study that demonstrated that psychiatrists at twelve separate mental hospitals failed to detect individuals who were simulating schizophrenia. In the widely read and reprinted article, Rosenhan (1973), a professor of psychology and law at Stanford University, asserted that disciplinary psychiatry could not tell the difference between mentally ill and mentally well people: “The facts of the matter are that we have known for a long time that diagnoses are often not useful or unreliable, but we nevertheless continued to use them” (p. 257). Additionally, the judge presiding over the ACLU’s successful lawsuit against the mining companies that caused the Buffalo Creek Disaster, in which ninety percent of Buffalo Creek’s residents developed signs of psychological distress after a series of dams

collapsed, criticized the APA and DSM-II. He wrote in his opinion that “he, a nonmedical person, had to assign a phrase to cover their psychological damage because of the inadequacy of the ‘reactive’ diagnoses ... officially available to him” (Nicosia, 2001, p. 203).

Another important episode in the “crisis of legitimacy” actually demonstrated the APA’s ability to adapt the cultural revolution taking place during the 1970s. After two years of opposition to the APA’s classification of homosexuality as a “sexual deviation” in DSM-II (1968), gay rights activists disrupted the APA’s 1970 convention in San Francisco, eventually shutting down the event (Scott, 1993, p. 40). In 1973, the APA’s board of trustees voted to remove “Homosexuality” from DSM-II. The narrow decision was vehemently opposed by both biological psychiatrists and psychoanalysts “who viewed the deletion of homosexuality as a scientifically indefensible response to gay pressure, as a capitulation to mob action, and as a sad reflection of the temper of the times” (Bayer & Spitzer, 1982, p. 32). Although there is no explicit connection between the omission of homosexuality from DSM-II and the inclusion of PTSD in DSM-III, some critics have viewed the removal of homosexuality as evidence that the APA was not indifferent to social causes and thus receptive to Vietnam veterans’ issues (Scott, 1993; Young, 1995).

In the midst of these events, the DSM was scheduled to for a planned revision in 1978. Since the adoption of DSM-II in 1968, an agreement between the U. S. Department of Health, Education and Welfare and the World Health Organization (WHO) mandated that revisions to American psychiatry’s nosology be scheduled to coincide with revisions for WHO’s *International Classification of Diseases (ICD)* (Wilson, 1993, p. 404). WHO

had scheduled for a revision of the ICD to be published in 1978. In 1974, the APA's president-elect appointed Robert L. Spitzer to head the Task Force on Nomenclature and Statistics, the body responsible for deciding which diagnoses would be included in DSM-III.

In comparison to many of his psychosocially-oriented psychiatrist colleagues, Spitzer believed that “mental disorders are a subset of medical disorders” (Spitzer, Sheehy, & Endicott, 1977, p. 4). Spitzer populated the Task Force with likeminded researchers who held that psychiatry should codify diagnoses based on testable diagnostic criteria that were empirically observable, the *modus operandi* of biological medicine. The aim of this approach was diagnostic reliability among psychiatrists. As Mayes and Horwitz (2005) explain, “Increasing the DSM's reliability meant, for instance, that if ten psychiatrists saw the same depressed patient separately, all ten should conclude—based on the patient's observable symptoms—that the patient had a depressive disorder” (p. 260). Under Spitzer's Task Force, psychiatry would no longer aim to unravel what was behind the patient's symptoms (i.e. etiology) because this psychoanalytic procedure was perceived to be diagnostically unreliable. In effect, this paradigm shift away from the psychosocial model usurped the power of clinicians treating patients and placed the lion's share of responsibility in the hands of researchers who studied mentally ill patients. Instead of searching for causation, the symptoms themselves were to be the object of diagnosis, with notable exception. As the introduction to DSM-III explains: “The approach taken in DSM-III is atheoretical with regard to etiology or pathophysiological process except for those disorders for which this is well established and therefore included in the definition of the disorder” (APA, 1980, p. 7). It would be only through

research and diagnostic validity that new diseases would enter DSM-III. More importantly, the PVS advocates would have to prove that combat was an etiology for delayed stress.

### **Resistance to a Delayed-Stress Diagnosis**

Spitzer's professional affiliations posed a significant challenge to the delayed stress advocates. He populated the Task Force with researchers from Washington University-St. Louis, an institution that was well-versed in the methods Spitzer preferred and was openly hostile toward Shatan and PVS. For example, in "Depressive Disorders in Vietnam Returnees" (note the intentional avoidance of the term veteran), Washington University researchers Helzer, Robins, and Davis (1976) randomly selected 460 participants from a sample of 13,760 depressed Army enlisted men to investigate the extent to which participation in the Vietnam War caused the soldiers' depression. The study found only "that an association between combat and later depression does not necessarily signify a causal relationship" (p. 184). Nor could the relationship between stress and "affective illness" be substantiated because it was, at best, "indirect" (p. 184). In short, the researchers concluded that the Vietnam war did not cause mental illness.

Most notably, the researchers took Shatan to task in their report. They stated:

Shatan, for example, *feels* that depression and apathy are frequently seen as part of a "post-Vietnam syndrome" and that the onset of this disorder may not occur until 1 to 2 years after return. For the most part, however, these and other reports have been *anecdotal accounts*. The large group of men reported here [in the Washington University study] give a more accurate estimate of the magnitude of the problem. (Helzer, Robins, & Davis, 1976, p. 183, emphasis added)

The Washington University researchers not only dismissed the reality of PVS, they disparaged the methods by which Shatan and others had arrived at their conclusion by labeling their data as anecdotal. In a similar study employing 898 soldiers, the same group concluded that the Vietnam War was not the cause of veterans' substance abuse problems (Helzer, Robins, & Davis, 1975/1976). While the rap groups provided a narrative to persuade the American public, the U. S. Congress, and clinicians who worked with veterans, the new research paradigm endorsed by the APA threatened to undermine a half-decade's efforts to have the Vietnam veterans' psychological sufferings officially codified and recognized.

Given this new rhetorical formation within professional psychiatry, which privileged empirical research over clinical findings such as those from the rap groups, Shatan, Lifton, and their delayed stress colleagues recognized that they would have to retool their approach if they were to succeed in adding PVS to the DSM-III. Spitzer's mission to make the next DSM a scientific nosology meant that their clinical findings from a small number of rap group participants would not meet the threshold for evidence. The activist psychotherapists would have to assume the practices and discourse of scientific psychiatry to advance their cause by proving that the symptoms of PVS constituted a unique mental illness with a known etiology (i.e. the Vietnam War).

In the following section, I explain how the delayed stress advocates adopted the institutional rhetorical practices of the APA to successfully argue for the inclusion of a delayed stress diagnosis in DSM-III. I then discuss this shift in their rhetorical practice in terms of Michel Foucault's "Discourse on Language," which elucidates how institutions erect discursive barriers to control the production of knowledge. In the process of

adopting the institutional discourse of the APA, the delayed stress advocates demonstrated the statistical significance of the syndrome by expanding the codification of PVS to include survivors of other natural and human-made disasters, thus erasing “Vietnam” from Post-Vietnam Syndrome. This meant that there would be no singular post-combat stress disorder in DSM-III. Rather, combat would be an example of a stressor in a new post-traumatic stress disorder that was broadly applicable to individuals who experienced a range of life-threatening events.

### **The Vietnam Veterans Working Group**

In 1975, Shatan and Lifton approached Robert Spitzer at APA’s annual conference to discuss including PVS in DSM-III. Spitzer informed them that the Task Force had no intention to create a new category for combat-related delayed stress. Their decision was based on the Washington University-St. Louis researchers’ studies of Vietnam veterans as well as meetings they held with the Task Force. The Washington University group deemed that no separate classification was necessary for a post-combat diagnosis because existing diagnostic categories sufficiently represented the symptoms veterans demonstrated. However, Spitzer left open the possibility of a delayed stress category, if Shatan and Lifton could disprove the Washington University group’s claim (Scott, 1995, p. 60). This brief meeting convinced the PVS advocates that if they were to persuade the APA to include a post-combat delayed stress diagnosis in DSM-III they would have to centralize their research efforts to generate data convincing to the empirically-oriented Task Force. As a result, Shatan with Lifton and Sarah Haley formed

the Vietnam Veterans Working Group (VSWG) to compile data for a “post-combat disorder” (p. 61).

The creation of the VSWG marks a surprising shift in both Lifton and Shatan’s theoretical orientations toward psychiatry. After conducting his study of Hiroshima survivors, Lifton had written against the “professional technoism” of “the new psychiatry,” advising psychiatrists to “feel” rather than collect statistics under the guise of neutrality (Lifton, 1972; Lifton 1973). Similarly, Shatan (1973) took a rigid stance against empirical methods in his widely-circulated essay, “The Grief of Soldiers,” where he acknowledged that the ethnographic data he gathered to support the codification of PVS would not pass the litmus test of scientific psychiatry. The nonhierarchical activist nature of his and his psychiatric colleagues’ involvement with the VVAW rap groups did not lend itself to the laboratory conditions associated with the objective standards of the scientific method. Shatan (1973) deflected potential criticism by appealing to his audience’s beliefs that the war was an atrocity: “Our evidence that Vietnam combat is psychopathogenic does not lend itself to statistical analysis” (p. 645). His defense also lampooned the perversity of scientific methods for psychiatry by showing how a comprehensive study of Vietnam veterans would be impossible to conduct: “We have neither universal samples of a total population of counter-guerrilla warriors, nor adequate matching controls” (p. 645). He did not shy away from using magnitude as a rhetorical device to substantiate the delayed-stress problem, however. Shatan cited “impressive evidence that thousands of combat veterans experience severe psychic suffering, and tens of thousands may be experiencing milder suffering that is never recognized” (p. 645). However, given the opportunity to have PVS recognized in DSM-III, it appears that



Lifton and Shatan reconsidered their positions as they set out to amalgamate “impressive evidence” for their cause.

For Shatan, the shift from clinical advocacy work to empirical data collection was relatively seamless. In 1973, the same year he published “The Grief of Soldiers,” which was originally presented at the 1972 American Orthopsychiatry Association Conference, he took the first steps to amass data on Vietnam veterans’ delayed stress problems. He participated in two conferences that forged alliances with researchers studying delayed stress in nonveteran populations and solidified the research efforts among advocates working with Vietnam veterans in clinical capacities. In March 1973, Holocaust survivor, Emmanuel Tanay invited Shatan to speak at the Michigan Psychiatric Society’s symposium on Vietnam in Detroit. There Shatan forged bonds with researchers studying and treating concentration camp survivors, thus cementing the bond between the rap groups and the international stress research community (Nicosia, 2001, p. 181). The watershed moment for PVS researchers followed a month later, when the Missouri Synod of the Lutheran Church sponsored the First National Conference on Emotional Problems of Vietnam Veterans in St. Louis (Nicosia, 2001; Scott, 1995). The St. Louis conference brought together 90 veterans, 60 psychiatrists, 30 military chaplains, and 10 VA personnel to share strategies on how to meet the psychological needs of Vietnam veterans. As historian Gerald Nicosia (2001) wrote, “The miracle of the St. Louis conference in 1973 was that, despite all these political agendas, there emerged in the workshops a harmonious rapport and a genuine willingness to cooperate in making further breakthroughs” (p. 197). With this informal research network already in place, the VVWG had a conduit for collecting and distributing research data.

Soon after the VVWG was formed, two Holocaust researchers who found similarities between veterans' and concentration camp survivors' adjustment difficulties joined the group. Combining research findings from the two survivor groups with Lifton's psychohistorical study of Hiroshima survivors, the VVWG conceptualized the diagnostic category as a broader post-catastrophe phenomenon with combat being a single example of a catastrophic event. Broader conceptualization of the post-catastrophic stress diagnosis meant the availability of more and diverse data to substantiate the need for a new diagnostic category. It also represented a move away from a singular etiology, which would bring the codification of the diagnosis into closer accord with the APA's scientific paradigm.

The creation of the VVWG represents a rhetorical shift in the delayed stress advocates' constitutive practices. As Jasinski (1998) notes, constitutive rhetoric's efficacy hinges upon its capacity to create subjective and collective identities in "public practice" (p. 75). As Chapter 4 demonstrated, the delayed stress advocates successfully reconstituted Vietnam veterans as survivors of an atrocity to explain that their difficulties adjusting to civilian life were the result of a unique, service-connected mental disorder that did "not fit any standard diagnostic label" (Shatan, 1972, p. 35). Although Jasinski (1998) limited his analysis of constitutive rhetorics to the public sphere, he tacitly acknowledges that rhetorical constitution can take place in nonpublic venues for nonpublic audiences because his analysis of constitutive potential and constitutive force in identity formation does "not circumscribe the range of rhetorical influence" of constitutive discourse (p. 75). Shatan's (1972) mention of PVS not fitting any "standard diagnostic label" indicated that a different type of rhetorical constitution must take place

if the new codification was to become part of the psychiatry's "standard" diagnoses in DSM-III. Because the APA and its Task Force were the gatekeepers for the inclusion of new diagnostic codifications, the delayed stress advocates attuned their constitutive discourse to the language and procedures of that institution. In sum, constituting PVS as an official diagnosis in DSM-III meant that VVWG would have to meet Spitzer's criteria for inclusion.

In "The Discourse on Language," Michel Foucault (1972) argues that the production of discourse is regulated by certain procedures "whose role is to avert [a discourse's] powers and its dangers, to cope with chance events, to evade its ponderous, awesome reality" (p. 216). As I argued earlier, institutional medicine, as a site of discourse, developed increasingly complex bureaucratic procedures to regulate the knowledge permitted into the discipline. In the early history of PTSD, "chance events" were the exigences for the codification of post-traumatic illnesses. For example, physicians (re)codified railway spine to create knowledge about what happens to train passengers during a collision that caused them to manifest certain illnesses with invisible wounds. When the events dissipated, so too did the codification because it fell out of use. Another more bureaucratic example of this phenomenon is found in the Southborough Committee dictating in 1922 that the term shell shock could no longer be used to describe the psychological traumas of British soldiers. In this case, the committee averted the power of the shell shock discourse by outlawing it. Foucault (1972) acknowledges that the prohibitions governing discourse, including those that determine "the privileged or exclusive right to speak of a particular subject," are "continually subject to modification" (p. 216). In the case at hand, Spitzer's dictum that new psychiatric diagnoses must be

empirically and statistically verifiable represents this type of modification. As a result, it was rhetorically impossible for the delayed stress advocates to codify PVS in the same ways that Erichsen codified railway spine or Whiltshire codified shell shock because Spitzer's Task Force dictated who would be officially recognized when they spoke on the codification of psychiatric disorders.

For a speaker to be recognized, according to Foucault, she must create a discourse that meets the truth conditions of the discipline in which he or she wishes to advance knowledge. He wrote, "Within its own limits, every discipline recognizes true and false propositions, but it repulses a whole teratology of learning. ... In short, a proposition must fulfil some onerous and complex conditions before it can be admitted within a discipline, before it can be pronounced true or false ... it must be 'within the true'" (Foucault, 1972, 223-224). The Task Force on Nomenclature's new reliance on the empirical paradigm of biological medicine erected a disciplinary barrier to the psychoanalytically-informed codification of PVS, placing it, in Foucault's words, in the "teratology," or deviant realm, of academic psychiatry. The creation of the VVWG signaled that the delayed stress advocates attempted to calibrate their discourse to bring the notion of combat-related delayed stress "within the true" of DSM-III. Ironically for the staunch antiwar activists, bringing PVS within the true meant erasing the social reality of veterans' adjustment problems. Adapting PVS to the APA's empirical paradigm effectively erased the "chance event"—the exigence—for codifying PVS in the first place.

Rhetoric scholars have explained the phenomenon of negotiating discourse so that it can achieve institutional recognition in a variety of ways. Following legal scholar,

James Boyd White (1984), John Lyne (1985) wrote that, “Communities as well as practices can be constituted rhetorically” (p. 68). He observes that “dynamic rhetoric” of a discipline has a “socializing power” that can pull participants into the conversations of that discipline (p. 68). Ellen Barton’s (2000) study of doctor-patient interactions demonstrates the power dynamics in these insider-outsider conversations. Specifically, she examined how patient narratives were sanctioned or not sanctioned by medical authority figures. In clinical encounters between a doctors and patients, she found that doctors permitted patients to speak if their narratives conformed to the well-established rhetoric of the doctor-patient dialogue. However, she found that when doctors disagreed with patient narratives, the doctors either interrupted or reformulated the narrative to bring the patient within the institutionally sanctioned discourse of medicine (Barton, 2000, pp. 352-354).

Barton’s example resembles the power dynamics at play in the delayed-stress advocates quest to incorporate PVS into DSM-III. In both cases, representatives of institutional medicine halted the outsider discourse and then recalibrated their narratives to fit within institutional parameters. Scott Graham (2015) calls this procedure “constitutive calibration” (p. 145-174) because the outsider must align, or calibrate, his or her discourse to the expectations of the institution he or she wishes to address. In each of these examples from the field of rhetoric, be they sanctioning discourse (Barton, 2000), socializing rhetoric (Lyne, 1985), or constitutive calibration, what is being explained is how outsiders are granted access to the inside of an institution to have their voices heard and their ideas validated and constituted as knowledge within that discipline.

Ian Hacking's (2007) framework for people making captures the dynamic process by which knowledge is integrated into a discipline. To review Hacking's framework,

[T]here are (*e*) the *experts* or professionals who generate or legitimate the knowledge (*d*), judge its validity, and use it in their practice. They work within (*c*) institutions that guarantee their legitimacy, authenticity, and status as experts. They study, try to help, or advise on the control, of the (*b*) people who are (*a*) classified as a given kind. (Hacking, 2007, p. 297)

Hacking's framework applies most directly to the Task Force, the experts who are in a position to validate codifications of diagnoses for the APA. Constitutive calibration must take place for a new classification of patients to be accepted, if those proposing the classification are not experts within classification-granting institution. Therefore, it was necessary for the VVWG to gain access to the people-making process to have their arguments for a new post-combat delayed stress disorder heard and accepted. I explain how this process took place for the VVWG in the following section.

### **The Committee on Reactive Disorders**

Spitzer had proven to be a politically savvy bureaucrat during the 1973 redaction of homosexuality from DSM-II. It was he who led the charge to develop the "Sexual Orientation Disturbance" diagnosis that would fill the gap left in homosexuality's place (Bayer & Spitzer, 1982). Historians and critics of DSM-III concur that the revision of the codex was mired in politics that were obfuscated by the "rhetoric of scientific justification" (Butler, 1999). While there is no explicit discussion of the APA's internal politics available in the published medical literature on the creation of DSM-III, Spitzer's continued correspondence with the VVWG implies his awareness of the magnitude of

Vietnam veteran's post-combat mental health problems. As one APA insider explained in an interview published in Scott (1993), Spitzer

started out being a very data-driven person ... The pressure groups began to rise and say, "Look, this should be in and this should be out." ... And what he ended up doing was a two-part process, one setting up task groups that were experts in the field, and two, subjecting it to a political process in the American Psychiatric [Association] that ensured it would be adopted by them. (as cited in Scott, 1993, p. 61)

In the summer of 1975, the VVWG invited Spitzer to Columbia Presbyterian Hospital in New York City to present its ideas and the data its members had collected. Spitzer informed the group that the committee required additional proof to substantiate the case for a post-combat delayed stress codification in DSM-III. However, he offered them an official point of entry to the APA's rarefaction process by appointing them to the Committee on Reactive Disorders, a new body tasked with compiling evidence on delayed stress, which would report directly to the Task Force. The committee was comprised of three APA representatives. Nancy Andreasen, a specialist on burn victims' post-traumatic reactions, chaired the committee. Lyman Wynne, a family therapist, and Spitzer himself also participated. Shatan, Lifton, and another member of the VVWG, Jack Smith, rounded out the committee. By creating the Committee on Reactive Disorders, Spitzer granted the VVWG access to the APA's rarefactive process. This maneuver summarily included them in Hacking's process of people making: the VVWG were now constituted as *experts* who could *validate* knowledge about those suffering from delayed stress and codify it for the *institution*.

The VVWG's affiliation with the APA bolstered its ethos in the psychiatric community, which it leveraged to form alliances with other researchers working on synergistic issues in stress psychiatry. The group's membership and the diversity of the

research perspectives it represented steadily grew throughout 1975. The chief of psychiatry at Roosevelt Hospital in New York, Harley Shands, and Mardi Horowitz, professor at University of California, San Francisco, were among the earliest to join ranks with the VVWG. Shands worked extensively with victims injured in workplace accidents. In his own research, he had seen notable similarities between workers' adjustment issues and those of veterans and concentration camp survivors. Horowitz, one of the preeminent researchers on the psychology and physiology of stress helped to substantiate the group's case for a post-combat disorder and connect the symptoms to other common etiologies including, rape, automobile, accidents, deaths of family members, and other extraordinary "life events" (Horowitz, 1976, p. 34). In addition to accruing data from an ever-growing number of researchers, the VVWG mounted its own systematic research effort and gathered data on 724 veterans nationwide (Nicosia, 2001, p. 206).

In March 1976, the VVWG invited Nancy Andreasen to attend their workshop on post-combat disorders at the American Orthopsychiatry Association's annual meeting. With the expanded research perspectives of the VVWG's membership, the group reclassified post-Vietnam syndrome as a "catastrophic stress disorder" (Wilson, 1993; Shatan, 1985). After they presented the data they gathered from the previous year, Andreasen bombarded them with methodological questions, leaving the group dejected and doubtful that they would be able to convince the Task Force of the need for a new disorder in DSM-III (Nicosia, 2001, p. 207).

The supplemental data they brought proved to persuade Andreasen, though. VVWG member Sarah Haley, who claimed no expertise in statistics, had conducted a



simple study of the patient files at her Boston VA outpatient clinic. Over the course of a year, Haley remained after work to review the file of each Vietnam veteran treated, isolating the files of veterans who had directly experienced combat. She found that ninety percent of the combat veterans were given official DSM-II diagnoses. However, as she recalled in an interview,

the working diagnosis was usually “traumatic war neurosis.” And so what I said was, “Look it, Nancy, we had to give these guys diagnoses [consistent with DSM-II], but if you look at what [some] clinicians are actually doing ... they’re basing their treatment on the fact that they recognize in these fellows similar traumatic war neurosis as they saw in World War II and Korean War veterans.” (as cited in Scott, 1993, p. 63)

The discrepancy between documented diagnosis and the reality of clinical practice moved Andreasen. Two months later, at the APA’s annual conference, Andreasen approached Haley and confided that she had observed similar delayed-stress reactions in her own severely burned patients.

Spitzer reconvened the Committee on Reactive Disorders reconvened in 1978. Shatan, Lifton, and Smith presented their evidence for the inclusion of catastrophic stress disorder, a delayed-stress diagnosis that included combat, as well as human-made and natural disasters. Weeks later, the group received word that their proposed codification was accepted by the APA under the new name “post-traumatic stress disorder.” Rather than write the definition of post-traumatic stress disorder herself, Andreasen instructed the VVWG to draft it, but retained editorial authority. On March 5, 1978, Shatan wrote to the members of the VVWG to announce, “the successful completion of our enterprise”: “[T]he latest draft version of DSM-III (Jan. 1978) incorporates most of our formulations on stress disorders, not only for combat veterans but also for Holocaust survivors and victims of other disasters, both man-made and otherwise” (as cited in Scott, 1993, p. 66).

While the VVWG succeeded in creating a diagnosis that would help a range of traumatized persons, most verbiage relating to military veterans was lost in the process of creating a delayed-stress diagnosis that was acceptable to the Task Force. The VVWG's first draft of the PTSD definition included references to the unpopularity of the Vietnam war, as well as references to WWII and Korean War veterans breaking down decades after their exposure to combat stress (Nicosia, 2001, p. 208). None of these references made it into DSM-III. PTSD was a depoliticized version of PVS that replaced Lifton's psychohistorical themes with quantifiable criteria.

There are four diagnostic criteria for PTSD in DSM-III:

- A. Existence of a recognizable stressor that would evoke significant symptoms of distress in almost everyone.
- B. Reexperiencing of the trauma as evidenced by at least one of the following:
  - (1) recurrent and intrusive recollections of the event
  - (2) recurrent dreams of the event
  - (3) sudden acting feeling as if the traumatic event were reoccurring, because of an association with an environmental or ideational stimulus
- C. Reexperiencing of the trauma as evidenced by at least one of the following:
  - (1) markedly diminished interest in one or more significant activities
  - (2) feeling of detachment or estrangement from others
  - (3) constricted affect
- D. At least two of the following symptoms that were not present before the trauma:
  - (1) hyperalertness or exaggerated startle response
  - (2) sleep disturbance
  - (3) guilt about surviving when other have not, or about behavior required for survival
  - (4) memory impairment or trouble concentrating
  - (5) avoidance of activities that arouse recollection of the traumatic event

- (6) intensification of symptoms by exposure to events that symbolize or resemble the traumatic event (APA, 1980, p. 238)

The most distinctive of these was Criterion A: “Existence of a recognizable stressor that would evoke significant symptoms of distress in almost everyone” (APA, 1980, p. 236). PVS’s original codification presumed that the cause of Vietnam veterans’ distress was the war itself, an atrocity perpetuated on behalf of the American people. In codifying a diagnosis that was both inclusive of other etiologies and acceptable to the APA, Criterion A undermined the unique psychiatric identity of the Vietnam veteran even as it enshrined the suffering that was brought to bear in the original VVAW rap groups. Previously, Vietnam veterans had access to PVS by virtue in their participation in the Vietnam War. The most significant rhetorical effect of the shift to the PTSD diagnosis was that they would have to prove that their experience in the war caused them distress and that they re-experienced that distress. Participating in the war alone was not sufficient for being traumatized according to DSM-III: to attain the PTSD diagnosis and its related benefits, veterans would have to argue for their trauma. There was no longer a mass diagnosis (PVS) for veterans’ adjustment issues; each veteran would have to argue his or her case to a clinician. It was up to the clinician to determine the extent to which the cause of his or her distress matched the codification of PTSD in DSM-III. The constitutive rhetorical actions of the delayed-stress advocates resulted in Vietnam veterans needing to yet again reconstitute themselves as sufferers of PTSD. The veterans themselves would now have to take rhetorical action in the clinical setting. In effect, the loneliness of cycling in and out of Vietnam was replayed as individual veterans cycled in and out of VA hospitals, pleading their cases alone.

Most of PVS's other key characteristics, such as intrusive recollections of the traumatic event, survivor's guilt, and psychic numbing, were retained in the DSM-III codification of PTSD. However, PVS's most acerbic themes, "impacted grief," the feeling of being scapegoated, and rage at the American government and society for sending them to war and not recognizing their sacrifice, were lost in the APA's new checklist codification. The politics that reconstituted Vietnam veterans' identity and fueled the constitutive force of PVS in the government, clinical, and public spheres was undermined by the politics of constitutive calibration that resulted in the inclusion of PTSD in DSM-III.

### **Conclusion: Collapsing the Traumatic Situation**

As the Vietnam War was subsumed under Criterion A, the creation of the PTSD diagnosis subsumed all previous post-traumatic illnesses. Although anachronistic, if a soldier who served in a twentieth-century war preceding the Vietnam War, walked into a VA hospital after 1980, he or she would not be treated for shell shock, battle fatigue, or war neurosis. Instead he or she would be given a diagnosis for a mental disorder that didn't exist at the time of his or her traumatization. The previous diagnoses would be unavailable to them. The specific situations that were known to cause the symptoms of trauma were now woven into the tapestry of traumatic events, or "recognizable stressors." With PTSD, there was no explicit recognition of the traumatic situation, it was but one of four criteria defining the wounds it left on survivors' minds.

An effect of PTSD being codified to recognize and even anticipate all (potential) traumatic events as stressors is that the diagnosis ostensibly removes the need for there to

be new post-traumatic illnesses that directly respond to events in the real world. Railway accidents, surviving a barrage of mortar fire, and losing a comrade in combat all become the same thing. There is a historical presentism to PTSD. Its universalizing criteria collapse the grief of an ancient Greek soldier into the same category as the trauma experienced by an Air Force drone pilot in 2017.

The differences between the rhetorical processes that resulted in the codification of PTSD and previous post-traumatic illnesses is significant and indicative of the shifting contexts in which human suffering is transformed into psychiatric knowledge. In the Victorian Age, when post-train accident ills affected railway passengers, railway spine was codified to assist British courts in adjudicating compensation claims. It was when train accidents posed a threat to rule of law, as administered in the courts, that the law called upon medicine to intervene. Pressure from the courts prompted surgeons, who were already clinically familiar with traumatized accident survivors, to codify railway spine in a manner that would clear up confusion about the medical phenomenon and justify the courts' decisions to award compensation to victims. As Erichsen (1866) argued in *On Railway Spine*, this process required surgeons to treat medicine as an art rather than a science. Art was the realm of the surgeon's individual judgement; science was the process of acting according to accumulated knowledge. While a conversation about the etiology and course of railway spine ensued in medical texts, the predominant codification—the one most useful to the courts—was that passengers sustained physical injuries that caused their psychological ills. Medicine functioned as a servant to the courts with Erichsen's somatic theory of railway spine triumphing because it was most useful in addressing the exigences created in the courtroom.

The railway spine episode elucidates how codifying post-traumatic illnesses is a rhetorical process of attending to trauma's situations and idiosyncratic formations and not merely the product of disinterested scientific observation. That is, world events (human or nonhuman) create social problems in the form of mysteriously ill people. The social institutions most affected by the social problem then call upon medicine to ameliorate the traumatic situation by creating an actionable medical diagnosis. The diagnosis succeeds to the extent that it 1) meets the needs of the institution that called upon medicine to intervene, and 2) mitigates the social problem created by traumatized people in an institutionally acceptable manner. The response(s) to the traumatic situation, the event, the stakes, the interlocutors, and the institutional constraints comprise the traumatic formation.

Much like the codification of railway spine was intended to facilitate jurisprudence rather than minister to traumatized people, shell shock was codified to sustain a fighting force for the Allied militaries in WWI. Considering the slapdash nature of British military medicine and the swift harshness of courts martials in the early years of the war (Shephard, 2001), it is reasonable to infer that the state of medical thought and practice was not a primary concern of the War Office. When the Royal Army perceived traumatized soldiers as a threat to its capacity to maintain a fighting force, psychologically-oriented psychiatrists proved that their codification of shell shock as a wound to the mind bested somatic codifications simply because psychological medicine could keep soldiers in the fight. Once again, the codification of a post-traumatic illness succeeded because it could address the traumatic situation and serve the needs of the institution (British military) that perceived psychological trauma as problem.

Attending to context is a central rhetorical feature of codifying post-traumatic illnesses. PVS marks a transition period in the rhetorical history PTSD. Like railway spine and shell shock, PVS was codified to address a specific social problem, the reintegration of Vietnam veterans into American civilian life. This social problem lacked institutional recognition, though. The U. S. military cited the low statistical incidence of psychiatric evacuations in Vietnam as evidence that there was no connection between the war and veterans' psychological problems. The victims were blamed for their own adjustment issues. Without an institution to formally recognize the traumatic situation, activists were compelled to act, and as a result, they codified PVS in terms of the patients' needs rather than the needs of institutions. Yet, they faced a major hurdle. The social context in which they codified PVS was hostile toward veterans. Therefore, they reconstituted veterans' identities to gain sympathy for their plight and created a traumatic situation that was legible to the public. With the Vietnam War reconstituted as an atrocity and the veteran recast as a survivor and victim of the war, PVS achieved rhetorical momentum in the public, governmental, and clinical spheres.

Historically, as evidenced in railway spine and shell shock, codifying a post-traumatic illness that addressed the traumatic situation would have been enough for the diagnosis to be recognized and acted upon. The activists advocating for PVS faced another barrier, indicative of how the context of creating psychiatric knowledge had changed between WWI and the 1970s. In addition to addressing the social problem of veterans' readjustment issues, the activists would have to align their diagnosis with the institutional needs of the APA, a bureaucratic entity that, despite its origins in nineteenth-century asylum psychiatry, did not exist in earnest until the 1950s. PVS would not only

have to meet the needs of Vietnam War veterans, but also would have to advance the APA's agenda for creating a diagnostically reliable nosology in form of DSM-III. They succeeded in meeting these needs by appealing to the universality of psychological trauma rather than in the special plight of war veterans. The diagnosis was no longer for Vietnam veterans—as railway spine had been for train accident survivors—because veterans became but one instance of a class of traumatized people who qualified for the PTSD diagnosis. The rhetorical success of the PTSD was and is that it collapsed all traumatic situations in the world into a single actionable diagnosis, making trauma appear to be universal and independent of its immediate context.



## Chapter 6: Conclusion: Contexts, Moving Targets, and Common Themes

This dissertation has focused on four distinct post-traumatic illnesses and the historical rhetorics that influenced how psychiatrists codified those illnesses. Within each historical moment, I triangulated the social, medical, and institutional discourses that contributed to the creation of each unique psychiatric diagnosis. My aim in taking this approach to examining the past of post-traumatic stress disorder (PTSD)—a psychiatric diagnosis that has become a fact of twenty-first-century American life—was to understand the rhetorical mechanisms by which post-traumatic illnesses were codified at different moments in history and how those rhetorics accrued over time. By attending to the specific contexts in which railway spine, shell shock, Post-Vietnam Syndrome (PVS), and PTSD were codified, we can see that each of these post-traumatic illnesses was negotiated into existence under vastly different social, medical, and political circumstances. Beyond the nuanced accounts I have tried to (re)assemble in this study, the names of the diagnoses that predate PTSD enshrine the socio-temporal circumstances that demanded psychiatric intervention: horrific train accidents in Victorian England, mechanized warfare in the early twentieth century, and Vietnam War veterans’ difficulties readjusting to civilian life in the turbulent America of the 1960s and 1970s. PTSD, as a term, breaks this pattern of signification. As I argued in Chapter 5, the capaciousness nonspecificity of the term post-traumatic stress disorder invites anachronism and historical presentism. By attending to the contexts in which differently interested stakeholders negotiated actionable psychiatric diagnoses, my aim was to create

space for the analysis of the rhetorics that produced each historical post-traumatic diagnosis.

My approach to understanding PTSD as the product of historical rhetorical negotiations blends Judy Segal's (2005) concept of kairology with Ian Hacking's (2007) notion of the moving target. Taken individually, there is a tension between the kairotic moments when shifts in medical knowledge and practice occur and the notion that there is a stable thing called psychological trauma that exists in abstraction until it is defined. Kairology, as a method of identifying and analyzing rhetorics, focuses on individual moments of change, the before and after of a singular rhetorical shift. Pan-history (Olson and Hawhee) adds greater depth to kairology, making it analytically more powerful because more kairotic moments are addressed over a longer timeframe. More instances of change means greater texture and a wider perspective on PTSD. For example, the publication of DSM-III ushered in a disciplinary preference for somatic codifications of psychological trauma. If this study were only to examine the kairotic moment when PVS as a psychological phenomenon became the embodied mental disorder PTSD, it would ignore the fact that before psychiatrics codified psychological trauma as a mental wound, nineteenth-century physicians codified psychological trauma as a symptom of a physical wound. The benefit of the pan-historical approach is that it offers additional analytic possibilities: DSM-III might be more of course-correction in psychiatric theory than an actual revolution.

By following Celeste Condit's (1999) method of identifying the formations in which change occurs and analyzing the rhetorics that compel and respond to change, medical concepts, such as psychological trauma, can be productively understood as

dynamic entities that are shaped and negotiated by differently interested interlocutors. Attending to the cultural and temporal contexts in which change takes place enriches rhetorical analysis by making apparent the interlocutors' motivations for codifying post-traumatic illnesses in ways that help them to achieve certain goals. Indeed, there are two distinct but interrelated aspects to codifying psychological trauma: helping those who suffer and mitigating the social problems caused by that suffering.

Placing railway spine, shell shock, PVS, and PTSD in their original contexts shows that each is a distinct product of the rhetorical process of codification. Each is an instantiation of the moving target of psychological trauma as defined in a specific context. Across the traumatic situations there are common themes. These themes, I argue, are important for rhetoricians of medicine and mental health to examine because, in my analysis, these recurring themes tend to be the objects of deliberation and negotiation. In short, they are the stuff of rhetoric, and moreover, they are what make psychological trauma a moving target—the thing that exists in abstract across time until it is codified at a given moment when a post-traumatic illness is required to ameliorate some problem in society. Contexts may change, but the issues present in the themes resurface to be deliberated anew with each new situation.

In the conclusion to Chapter 1, I invited the reader to trace five themes across the subsequent chapters. These themes are:

1. *Demonstrating post-traumatic illness as a problem.* How is the traumatic situation itself created, interpreted, and mitigated? That is, when does a post-traumatic illness become enough of a problem to a government or institution

that it calls upon medical practitioners to intervene in the situation by creating an actionable diagnosis?

2. *Knowledge creation.* How do doctors interpret the social problem and codify it into an actionable diagnosis? What rhetorics do doctors use to create knowledge about psychological trauma?
3. *Knowledge validation.* What rhetorical actions must doctors take to have that knowledge made valid in the psychiatric discipline? How do they align their knowledge with the goals of the institution they serve? And, how do they mitigate discourses that deny the existence of psychological trauma?
4. *Appeals to history.* How does history function rhetorically in the process of codifying post-traumatic illnesses?
5. *Post-traumatic illness as change agent.* To what extent did the codification of a post-traumatic illness mitigate the social or institutional problem? What new rhetorics or rhetorical effects were made possible by virtue of the codified post-traumatic illness?

To conclude this dissertation and illustrate the contextual portability of these themes, I will briefly show how the themes operated in each of the contexts examined in this dissertation. Lastly, I will offer future directions for this research.

#### *Demonstrating post-traumatic illness as a problem*

In each historical epoch examined in this dissertation, there came a moment when psychological trauma resulting from a widespread social problem commanded the attention of a social institution. The social institution then called upon disciplinary

medicine to intervene in the situation with the goal of alleviating the problem. I focused specifically on this issue in Chapter 3, by using *megethos* as a rhetorical lens; however, magnitude played a rhetorical role in each of the cases in this dissertation. In each case study, there is a moment when the weight of psychological trauma bears down upon an institution and motivates it to take action by creating knowledge about the phenomenological experiences of those suffering from invisible wounds.

In the Victorian period, railway accidents were horrible facts of civil life. Transportation technology simply could not reliably stand up to labor demands placed on railway workers and the equipment they operated by shareholders who demanded returns on their investments. The devastating psychological effects of these accidents did not become a problem until they caused consternation in liability law suits. Courts required the assistance of surgeons to adjudicate lawsuits. The railway spine diagnosis provided a reliable means of determining whether or not a passenger actually sustained invisible injuries in an accident and the extent to which he or she was due compensation.

Decades later, shell shock presented a similar hindrance to the British government. But instead of psychological trauma impeding the due course of litigation, the Royal Army perceived traumatized British soldiers as threats to its capability to field an effective fighting force in WWI. Soldiers had historically broken down in combat, but never to the extent that they created a logistical problem for the prosecution of war. Codifying shell shock as a psychological wound permitted military psychiatrists to triage shell-shocked soldiers close to the frontlines and return them to combat, rather than returning them to England for hospitalization and eventual discharge from duty.

Post-Vietnam Syndrome occurred in a different institutional context. The president of the Vietnam Veterans Against the War (VVAW) invited psychiatrists to help veterans with their adjustment problems. The presumption in this case was that the problems facing a small number of traumatized veterans in New York City were also felt by others who had served in the war. Despite its countercultural clout, VVAW was social institution on society's fringe with no power to authorize PVS as a codified post-traumatic illness. Although the psychiatrists affiliated with VVAW successfully argued that PVS was indeed a problem for American society, additional rhetorical action was necessary to demonstrate the magnitude of psychological trauma to the American Psychiatric Association, the institution responsible for authorizing and cataloging mental illnesses. In the process of making their appeal to the APA, the advocates had to demonstrate that psychological trauma was a large-scale problem that affected people who were not Vietnam veterans.

### *Knowledge Creation*

Examining knowledge-making practices across time is an effective way to trace shifts in psychiatric theories about psychological trauma. In each historical instance of codification, doctors were faced with the issue of connecting events in the world to the suffering of individuals. Their attempts at arriving at an etiology of psychological trauma began with patient examinations, but these examinations were necessarily influenced by doctors' underlying theories of trauma. In the case of railway spine, John Erichsen, a surgeon trained to detect and remove pathology in the body, codified railway spine as the product of a physical blow to the central nervous system which, in turn, created

microscopic lesions on the spinal cord. The lesions then caused functional disturbances similar to hysteria.

The somatic codification of psychological trauma was endorsed by neurologists, like F. W. Mott, in the early years of WWI. Mott, a pathologist, was convinced that his autopsies of shell-shocked soldiers' brains demonstrated evidence of the physical nature of shell shock. However, evidence gathered by Harold Whiltshire near the trenches in France offered a competing etiology that could account for mental break down in soldiers who were not in proximity to exploding shells. Whiltshire's examinations of living soldiers who had recently endured hardships convinced him that shell shock was a psychological phenomenon. Psychological trauma then became a wound to the mind rather than the body.

During the Vietnam era, Robert Jay Lifton's commitments to the theory of psychohistory continued the momentum that psychological codifications of trauma had gathered. A soldier need not be physically harmed to be traumatized; rather he only had to experience an atrocity either as a perpetrator or a witness. When Lifton and his colleagues approached the APA with their codification of PVS, they were forced to reconfigure the diagnosis to fit its biological theory of mental illness.

### *Knowledge validation*

The shift from the psychological codification of PVS to the somatic codification of PTSD highlights the rhetorical maneuvers doctors made to have their codifications accepted by institutions. Also, it underscores how the professionalization of medicine intervened in the relationship between doctors codifying post-traumatic illnesses and the

problems that called them to codify the illness in the first place. That is not to suggest that the codification of post-traumatic illnesses was historically an arhetorical process. Instead, the increased professional regulation of psychiatry added another layer of validation to the process of codifying post-traumatic illnesses.

In each of the cases examined in this dissertation, doctors advanced theories and deliberated about the etiology and treatments of trauma in medical journals and books. However, consensus within the medical profession was not sufficient to validate a codification. Prior to the professionalization of American psychiatry, as represented by the publication of the first edition of the APA's DSM-I in 1952, institutions, such as British courts and the Royal Army, could select among codifications to find the one that best fit their needs. In no small measure, institutions and their goals influenced medical theories of trauma, with a somatic codification working best for liability lawsuits and a psychological codification meeting the needs of fielding a fighting force in WWI.

The cases of PVS and PTSD mark a shift in the validation process. Even though the U. S. Congress and the VA were (somewhat) sympathetic to Vietnam veterans' mental health needs, they could not treat those needs without the PVS being authorized by the APA. As Chapter 5 explained, PVS was transformed into a different diagnosis altogether in order to meet the APA's goal of compiling a diagnostically-reliable nosology of mental illnesses. PTSD was born out of the negotiations between the psychiatrists who codified PVS and the APA Task Force that was committed to upholding the APA's mission. Once PTSD was accepted by the APA in DSM-III, it could then be used by Congress and the VA to mitigate the problems caused by veterans' adjustment issues.



### *Appeals to History*

This project began with the question of how we arrived at the contemporary codification of PTSD. Within each of the traumatic situations examined in this dissertation, history itself was used as a rhetorical device to either link a traumatic situation to the past or to differentiate it from circumstances that had come before it. In either case, rhetorical appeals to history functioned to legitimize post-traumatic illnesses and those they afflicted.

John Erichsen successfully argued for his codification of railway spine by claiming, in part, that railway spine was not a new medical phenomenon. Passenger railways were new, but he contended that the invisible illnesses that plagued accident survivors were like a condition called spinal concussion that predated railway travel. Using this line of historical argument, he convinced jurists that the cause of trauma was new, but the disease itself was not. As a result, his historical argument bolstered the ethos of passengers seeking monetary compensation for their invisible illnesses. The historical medical record proved that they were victims and not malingerers.

The shell shock phenomenon represented a break with history of military medicine. Prior to WWI, doctors foresaw new, increasingly powerful weapons inflicting new forms of mental illness on soldiers. In the opening months of WWI, the Royal Army Medical Corps realized that it was ill-prepared to handle the influx of traumatized soldiers. Its past practices of treating soldiers who broke down were futile in the face of an epidemic of traumatized soldiers. A break with the past of military medicine was necessary. New paradigms for understanding treating shell shock as a psychological wound rather than as a form of cowardice.

Similar to the doctors who predicted new forms of mental illness in the early twentieth century, the psychiatrists working with Vietnam veterans explained the need for a PVS diagnosis by arguing that the war was unlike any conflict in American history. Vietnam and its atrocities stood apart from other wars because of the military conditions in which soldiers fought and the social malaise they met when they returned home. Because the war was unlike any previous war, they argued that PVS was an uncoded diagnosis specific to those who fought in Vietnam.

*Post-traumatic illness as change agent*

My historical analysis demonstrates that when post-traumatic illnesses are codified, they have rhetorical effects that extend beyond the immediate circumstances of diagnosing and treating patients. Because psychological trauma is a response to an event in the world, configuring that event as the cause of mental harm necessarily influences social and institutional perceptions of both the event and the traumatized person. Over time, post-traumatic illnesses have significantly affected social, medical, and institutional rhetorics.

A profound rhetorical effect common to each of the historical epochs examined in this dissertation is the elevation of the traumatized people's ethos in society and institutional (legal) settings. The codification of railway spine and shell shock served as a counterrhetoric to claims that traumatized people were manufacturing mental illness for person gain, whether in court or in military service. Railway spine as an authorized medical diagnosis substantiated survivors of train accidents as legitimate plaintiffs deserving of compensation in Victorian courts. Similarly, in British courts martial cases

in WWI, the psychological codification of shell shock disrupted institutionally ingrained discourses of malingering and cowardice, potentially saving the lives of soldiers on trial. In each case, the codified diagnoses extended beyond the realm of medical knowledge and practice into the rhetorical realm of institutional deliberation to give traumatized individuals—who were previously perceived as dubious—greater standing in the eyes of the court and the military tribunal.

PVS and PTSD continued this trend in broader circumstances. PVS reconstituted the identity of Vietnam veterans by demonstrating that they were survivors of atrocities, regardless of whether they witnessed or committed those atrocities. This new identity cast veterans as more sympathetic figures deserving of greater social support, government programs, and clinical psychological help. The PTSD diagnosis continued PVS's constitutive momentum and achieved the support of institutional psychiatry, thus certifying that Vietnam veterans' adjustment problems (among problems experienced by other survivor groups) were in fact the product of a distinct medical condition. When the APA added PTSD to DSM-III in 1980, military veterans of all conflicts almost instantaneously became entitled to benefits that were previous denied to them.

### **Future Directions**

Nearly forty years have passed between the time that PTSD was officially recognized as a legitimate mental disorder in DSM-III and the writing of this dissertation. In those interim years, the APA has been significantly recodified PTSD three times. The method I have developed to conduct the research in this dissertation can be applied to the three post-1980 instances of redefinition to examine the social, medical, and institutional

factors that drove those changes. Such an analysis would enrich our understanding of contemporary PTSD by providing a background against which the rhetorical effects of those recodifications could be analyzed. As this project develops into a larger, more comprehensive study this method should also be used to fill in the gaps between the cases I have examined herein. For example, WWII was a time of tremendous medical and governmental change, which resulted in complex understandings of stress on the human body and mind. A more complete rhetorical history of PTSD would have to account for this important moment in the history of psychiatry.

My original goal in proposing this research was to understand the here and now of PTSD as a medical-cultural phenomenon and as a rhetorical product. This dissertation has given me a theoretical basis for conducting that research. It has also shown me where there are gaps in my analysis. Because this research focused on social, medical, and institutional discourses and how they interpret and intervene in traumatic situations, the voices of those who suffer(ed) have been underrepresented. Indeed, some of these voices have been lost to posterity, like those of railway spine sufferers. However, the voices of traumatized persons in the twentieth century have been better preserved either in texts or embodied in people themselves. Accounting for these voices, their personal experiences, and their rhetorical influence on the discourses of PTSD is necessary to include in future iterations of this project. As this dissertation argues that post-traumatic illnesses are the products of rhetorical action, it is incumbent upon me to represent those with a stake in the codification of psychological trauma.

History is never a foolproof guide to the future. However, from this conducting this research, I am certain that new traumatic situations will arise, prompting more and

different rhetorical action. With the benefit of having analyzed how traumatic situations operate in rhetorical formations, I hope to be able to productive contribute to future codifications of PTSD or whatever new diagnosis comes after it.

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