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A 21 Year Meta Analysis of the Effectiveness of Trauma-Sensitive Schools Initiatives

Megan Ann Blanton

A 21-YEAR META-ANALYSIS OF THE EFFECTIVENESS OF TRAUMA-SENSITIVE SCHOOLS INITIATIVES

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Megan Ann Blanton
A.A. Catawba Valley Community College, 2010
B. A., University of North Carolina at Chapel Hill, 2014
M.S.T. Fordham University, 2016
M.S. University of South Alabama, 2021
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LIST OF ABBREVIATIONS

ACE	Adverse Childhood Experience
ARC	Attachment, self-Regulation, and Competency
CLASS	Classroom Assessment Scoring System
fMRI	Functional Magnetic Resonance Imagining
GPA	Grade Point Average
HEARTS	Healthy Environments and Response to Trauma in Schools
HPA	Hypothalamic-Pituitary-Adrenal
MOST	Multiphase Optimization Strategy
PC	Practice Change
PD	Workforce Professional Development
PTE	Potentially Traumatic Event
PTSD	Posttraumatic Stress Disorder
PRISMA	Preferred Reporting Items for Systematic Reviews and Meta-Analyses
PROSPERO	International Prospective Register of Systematic Reviews
OC	Organizational Change
Q1	Question 1
Q2	Question 2

Q3	Question 3
SAMHSA	Substance Abuse and Mental Health Services Administration
SWPBIS	Schoolwide Positive Behavioral Interventions And Supports
TLPI	Trauma and Learning Policy Initiative
U.S.	United States
vmPFC	Ventromedial Prefrontal Cortex

ABSTRACT

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Childhood exposure to adversity is prevalent, with most individuals in the United states having experienced at least one adverse event in childhood (Child and Adolescent Health Measurement Initiative, 2019; Merrick et al., 2018). Low dosages of childhood adversity experienced within the context of a safe and caring home environment can promote the development of healthy coping skills that prepare children for future adversity. However, childhood adversity that is intense, chronic, or complex can result in a toxic stress response that leads to the development of mental illness, physical health concerns, cognitive deficits, academic performance deficits, and in severe cases, premature death (Berens et al., 2017; Blanchette & Caparos, 2016; Brown et al., 2009; Ehring & Quack, 2010; National Scientific Council on the Developing Child, 2014; Nelson et al., 2020).

Given these severe consequences, interrupting the pathways from childhood adversity to psychosocial dysfunction is critical, as is promoting the pathway from adversity to resilience. Accordingly, over the past 20 years there has been a substantial push for schools to be part of this effort by becoming “trauma sensitive.” Trauma-sensitive schools realize the prevalence of childhood adversity among their students and

staff, recognize the symptoms of trauma, respond effectively. and avoid re-traumatizing students. There currently exists a diversity of approaches, implementation methods, and measures of effectiveness for trauma-sensitive school initiatives, making it difficult to draw conclusions about the overall effectiveness of this approach. This study aimed to determine the effectiveness of the trauma-sensitive school approach by conducting a meta-analysis of existing empirical evidence. The scope of this meta-analysis was focused on the following research questions: **Q1.** Do trauma-sensitive schools positively impact student, staff, and school-climate outcomes? **Q2.** What are the specific components of trauma-sensitive schools that make them effective? **Q3.** What are the ideal dosages for staff professional development and overall intervention?

Overall, staff outcomes appeared to improve in both longitudinal and cross-sectional studies. Student outcomes improved longitudinally but not cross-sectionally. Similarly, school climate improved in longitudinal but not cross-sectional studies. Interestingly, staff reported significantly greater improvements in school climate than students. Regarding research question two, no differences in staff or student outcomes were found based on the number of trauma-informed elements included (i.e., professional development, organizational change, trauma-informed practice change). Aggregate effect sizes also did not significantly vary by dosage of trauma-informed professional development. However, is important to note that the methodology of the included studies severely limited the ability to draw strong conclusions about the impact of trauma-sensitive schools. Most longitudinal studies did not include control groups, over a third of outcome measures were either unvalidated or had mixed results for validity, and random assignment to condition was not common. Future directions for this body of research

include prioritizing methodologically rigorous studies and examining individual-level moderators (e.g., gender). Despite these limitations, this meta-analysis marks an important step in synthesizing the available data on trauma-sensitive schools and results indicate that continued investment in trauma-informed schools is warranted.

CHAPTER I

INTRODUCTION

Many children in the United States face some form of adversity before reaching adulthood, with current lifetime prevalence rates ranging from 40-60% (Child and Adolescent Health Measurement Initiative, 2019; Merrick et al., 2018). Childhood adversity can take many forms, including abuse and neglect, the death of a family member, and experiences of discrimination. While experiencing low dosages of adversity in the context of a caring and safe home environment can promote the development of positive coping skills and tolerable stress, exposure to severe, chronic, or multiple forms of childhood adversity, particularly in the context of an unsafe or unstable home environment, can produce a toxic stress response (National Scientific Council on the Developing Child, 2014). Experiencing toxic stress in childhood, particularly during sensitive periods of development, can result in severe and enduring anatomical and physiological changes in critical organ systems, including the nervous system, which in turn impact future physical and mental health, cognitive functioning, and psychosocial adjustment (Berens et al., 2017; National Scientific Council on the Developing Child, 2014; Nelson et al., 2020; Shonkoff et al., 2012). Examples of common physical and mental health issues for which risk increases as a result of toxic stress include asthma,

chronic obstructive pulmonary disease, ischemic heart disease, cancer, suicide attempts, substance use disorders, and anxiety disorders (Nelson et al., 2020). Declines in cognitive functioning associated with elevated exposure to childhood adversity include memory impairments, dysfunctional patterns of attending, and in some cases, lower global cognitive functioning (Blanchette & Caparos, 2016; Bücker et al., 2012; El Khoury-Malhame et al., 2011; Perfect et al., 2016). Children who experience childhood adversity may display poor psychosocial adjustment in adolescence and adulthood including family problems, job problems, early pregnancy, emotion dysregulation, academic difficulties, and financial problems (Brunton & Dryer, 2021; Ehring & Quack, 2010; Heleniak et al., 2016; Hillis et al., 2004). Perhaps the most severe association between childhood adversity and future poor outcomes is premature mortality. One study investigated the mortality rates of participants from the original Kaiser Permanente Adverse Childhood Experiences (ACE) study data approximately ten years after the study's conclusion and found that individuals with six or more ACEs died nearly 20 years earlier on average than those without ACEs (Brown et al., 2009).

Given the substantial mental health, physical health, cognitive, and psychosocial consequences of childhood adversity, efforts aimed at mitigating the impacts of toxic stress among children are crucial. Schools are uniquely situated to reduce childhood toxic stress and mitigate the development of poor outcomes following exposure to adverse childhood experiences. Specifically, it has been posited that universal school-based efforts to promote trauma-sensitive attitudes, behaviors, and policies represent a promising approach to mitigate the impacts of trauma on learning and psychosocial adjustment and enhance children's success in school and life (Chafouleas et al., 2016;

Overstreet & Chafouleas, 2016). Thus, the Substance Abuse and Mental Health Services Administration (SAMHSA) developed a framework for a trauma-informed approach upon which many schools, organizations, and research institutions have developed interventions to promote "trauma-sensitivity" among school staff (Substance Abuse and Mental Health Services Administration, 2014). Many of these programs contain common elements including improving teacher knowledge of trauma prevalence and impact, improving teacher attitudes towards children with exposure to serious or chronic adversity, promoting supportive rather than punitive discipline practices, promoting positive relationships within the school, and implementing policies and procedures that are trauma-sensitive (Avery, Morris, Galvin, et al., 2021). At present, the trauma-sensitive schools framework is a "flexible framework," meaning that its implementation can, and perhaps should, look different in each school, depending on school climate, resources, population, and readiness to change. However, this has also created a lack of shared knowledge within the research community as to which components, and in what dosage, are required for effectiveness in a trauma-sensitive schools intervention, making it difficult for practitioners to identify and implement evidence-based practices that are cost-effective related to dosage (Maynard et al., 2019).

Moreover, given that many studies examine either the impact of the intervention on intermediate mechanisms like staff trauma sensitive attitudes and behaviors, or the impact of the intervention directly on youth outcomes like resilience, some researchers have questioned whether staff trauma-sensitivity is actually the mechanism through which these interventions impact youth outcomes (Maynard et al., 2019). Thus, the present study aims to address this question through a meta-analytic synthesis of the effect

sizes of trauma-sensitive schools interventions on intermediate mechanisms like school climate, student connectedness, and teacher attitudes and behaviors, and on youth resilience. Additionally, it has not been established specifically which components of trauma sensitive schools interventions are necessary and in what dosage to produce trauma-sensitive attitudes, behaviors, and climate, or improved outcomes among youth. Therefore, another purpose of the present study is to examine which specific characteristics of trauma-sensitive schools interventions predict improved outcomes.

1.1 Childhood Adversity

Childhood adversity is a broad term that refers to serious or continued difficult events, situations, experiences, or circumstances that pose a threat to a child's well-being. Current estimates suggest that anywhere between 40 – 60% of people in the United States have experienced some form of childhood adversity (Child and Adolescent Health Measurement Initiative, 2019; Merrick et al., 2018). Common examples of childhood adversity include household dysfunction, abuse, neglect, exposure to natural disasters, serious accidents, severe poverty, sexual violence, community violence, experiences of discrimination, parental or guardian divorce; death of a parent or guardian; incarceration of a parent or guardian; domestic violence; serious mental illness or substance use disorder in a caregiver; physical, sexual, or emotional abuse; and neglect (Child and Adolescent Health Measurement Initiative, 2019; Finkelhor et al., 2015). In one nationally representative survey of parents and caregivers, 39.8% of parents and caregivers reported that their child had experienced at least one form of adversity (Child and Adolescent Health Measurement Initiative, 2019). Notably, this survey did not

include childhood sexual abuse or emotional abuse. In another nationally representative survey in which adult participants retrospectively report their childhood adversity, including sexual and emotional abuse, nearly 62% of participants reported at least one exposure to childhood adversity (Merrick et al., 2018). Researchers have categorized these adversities to better understand their prevalence and sequela.

1.1.1 Common Types of Adversity

One construct of childhood adversity was coined adverse childhood experiences (ACEs) by Vincent Felitti and colleagues in their 1998 study of childhood adversity and later health consequences. ACEs included seven specific domains of childhood adversity: physical, sexual, and emotional abuse; living with someone who was seriously mentally ill; incarceration of a family member in the home; witnessing one's mother being treated violently; and living with someone who abused substances. Broadly, the original ACEs can be conceptualized as childhood household dysfunction and abuse. This study found that over half of the 13,494 adults who had completed a standardized medical evaluation at a large Health Maintenance Organization had experienced at least one ACE, and that approximately 25% had experienced more than two categories of ACEs (Felitti et al., 1998). Since Felitti and colleagues' (1998) seminal study, researchers have broadened and expanded upon the original list of ACEs to include experiences such as parental divorce, death of a parent or guardian, neglect, experiences of discrimination, exposure to community violence, and concentrated poverty (Child and Adolescent Health Measurement Initiative, 2019; Merrick et al., 2018) .

In addition to ACEs, another construct of childhood adversity is potentially traumatic events (PTEs). PTEs are events that are either perceived as life-threatening or seriously injurious or involve sexual violence. Exposure to PTEs can occur by directly experiencing the event; witnessing, in person, the event as it occurred to others; learning that the event occurred to a close friend or relative; or experiencing repeated or extreme exposure to aversive details of the event (e.g., first responders; American Psychiatric Association, 2013). Potentially traumatic events can be interpersonal or impersonal in nature. Interpersonal PTEs are adverse events intentionally inflicted by a human perpetrator (Huang et al., 2016); examples of these events include physical or sexual violence (either through direct victimization or witnessing the event), kidnapping, stalking, and sex trafficking. For children specifically, sexually violent events may include developmentally inappropriate sexual experiences without physical violence or injury (American Psychiatric Association, 2013). Impersonal PTEs may be caused by natural origins or human origins; examples include serious accidents, learning of a family member dying by suicide, a medical catastrophe, combat exposure, and natural or human-made disasters (Fowler et al., 2013). Estimates of PTE exposure in childhood are comparable to ACE exposure with approximately 62% of adolescents reporting at least one lifetime PTE exposure (McLaughlin et al., 2013).

Finally, while PTEs and ACEs span many forms of childhood adversity, it should be noted that there are forms of childhood adversity that fall outside of ACEs and PTEs. General peer victimization and bullying victimization, for example, are forms of childhood adversity that are neither ACEs nor PTEs but can still produce elevated physiological stress responding and are still associated with negative outcomes including

substance use, internalizing symptoms, suicide ideation, lower school achievement, and poor academic performance (Gardella et al., 2017; Hong et al., 2018; Kliewer et al., 2012; Kowalski et al., 2014; Rohleder et al., 2004). Thus, for the purposes of this dissertation, the umbrella term childhood adversity was used to encompass ACEs, PTEs, and other forms of potentially threatening events or circumstances faced in childhood.

1.2 Characterization of Adversity

1.2.1 Sources of Childhood Adversity

Previous research has examined the sources of childhood adversity in relation to developmental outcomes. Studies in this area typically classify sources of childhood adversity as either impersonal or interpersonal in nature. Impersonal childhood adversity is distinguished by its occurrence outside the context of a human relationship. Impersonal childhood adversity is typically more weakly associated with long-term psychopathology (e.g., suicidality, depression, PTSD) than is interpersonal childhood adversity (Blankenship, 2018; Fowler et al., 2013; Yoo et al., 2018). Interpersonal violence may be particularly harmful given that parts of the human brain are highly reactive to interpersonal and social interactions (Friedman et al., 2014).

The severe impact of interpersonal adverse events may also be partially accounted for by disruptions it causes in attachment relationships. Given that most interpersonal childhood adversity is perpetrated by someone the child knows, and often by a caregiver (Department of Justice Office of Justice Programs Bureau of Justice Statistics, 2000), interpersonal violence may disrupt the development of secure attachment to a caregiver.

In particular, the absence of caregivers who serve as a safe haven and secure base results in insecure attachment. Insecure attachment, particularly disorganized attachment, can cause failure of behavior and affect regulation and impair a child's ability to establish trusting relationships (Ainsworth et al., 1978). For example, a child who experiences physical abuse from a person they trust (e.g., their mother) may then have difficulty trusting other adults after this experience, making it difficult for these children to seek help and support from adults (e.g., teachers) in times of need (Huang et al., 2016). This in turn may place children at increased risk of future victimization. Further, this disrupted attachment may result in deficits in emotion regulation, further increasing risk for experiencing a dysregulated and toxic stress response (Oshri et al., 2015).

A final factor that may partially account for the particularly negative outcomes associated with interpersonal violence, is the environments in which it occurs. Exposure to childhood interpersonal violence often occurs in the context of chronically stressful or under resourced environments (e.g., communities with high rates of community violence and concentrated poverty). For example, one study found increased physical abuse following periods of economic recession even when controlling for prior abuse, maternal depression, maternal age, race, relationships status, and child sex (Schneider et al., 2017). Empirical evidence also suggests that acute environmental stressors can increase the risk of interpersonal violence. For example, one study include increased rates of child abuse following natural disasters (Seddighi et al., 2021). Experiencing multiple forms of childhood adversity such as a natural disaster and interpersonal violence then increases one's risk of future poor mental and physical health outcomes (Hughes et al., 2017).

1.2.2 Complexity of Childhood Adversity

In addition to the source of childhood adversity, the complexity is also an important characteristic when considering developmental outcomes. The term "complex trauma" has confusingly been assigned the dual role of characterizing the actual adverse events experienced, and the impact of the exposure on intermediate and long term outcomes (Cook & Blaustein, 2003). Complex trauma exposure occurs when there is simultaneous or sequential exposure to multiple forms of maltreatment within the caregiving system (Cook & Blaustein, 2003). For example, a child who is simultaneously physically and sexually victimized by their brother has experienced complex trauma exposure and so has the child who is emotionally neglected by their mother and physically abused by their father. When referencing the sequelae of complex trauma exposure, complex trauma encompasses several domains including self-regulatory deficits, attachment disruptions, emotion dysregulation, addictive behavior, aggression, social helplessness, internalizing symptoms, problematic sexual behavior, and physical health disorders. For example, current evidence suggests that children with complex trauma exposure experience higher levels of generalized behavior problems and psychopathology compared to children who experienced acute interpersonal trauma, chronic interpersonal trauma beginning later in life, and acute non interpersonal trauma (Wamser-Nanney & Vandenberg, 2013).

One potential reason that complex trauma is associated with higher levels of dysfunction is that it occurs during the developmental period when brain development is particularly sensitive to experiences and environmental stimuli. Accordingly, complex

exposure to childhood adversity may result in structural and functional alterations to a child's brain via epigenetic mechanisms (Berens et al., 2017; National Scientific Council on the Developing Child, 2014).

1.2.3 Chronicity of Childhood Adversity

A final characteristic of exposure to childhood adversity that was briefly touched upon in the previous section, is chronicity, or the duration of the adversity. Acute exposure to childhood adversity typically refers to exposure to an adverse event that has a clear beginning and end such as an earthquake, a mugging, or a sexual assault by a stranger. Acute exposure may be interpersonal or impersonal in nature, and each source is associated with distinct outcomes. Specifically, acute interpersonal childhood adversity tends to have a larger effect size on childhood internalizing symptoms and externalizing symptoms than acute non-interpersonal childhood adversity (Wamser-Nanney & Vandenberg, 2013). Chronic exposure has been defined as lasting longer than six months or having no clear beginning and end points. For example, exposure to community violence and growing up in concentrated poverty are both forms of chronic childhood adversity. Chronic adversity exposure, like acute exposure, can leave children feeling overwhelmed, helpless, and trapped. However, chronic adversity exposure does not allow the child time to recover and return to equilibrium as the stress is ongoing (Wamser-Nanney & Vandenberg, 2013), and is therefore more likely to produce maladaptive levels of stress in a child. The chronic and dysregulated activation of a child's stress response is referred to as a toxic stress response (Johnson et al., 2013; National Scientific Council on the Developing Child, 2014; Shonkoff et al., 2012).

1.3 Toxic Stress as a Response to Childhood Adversity

The body's stress management system involves the coordinated release of chemical stress mediators, particularly glucocorticoids, by the hypothalamic-pituitary-adrenal (HPA) axis. The release of glucocorticoids is typically useful in the management of stress as it mobilizes energy into the bloodstream, increases cardiovascular tone, and delays non-essential bodily processes thereby allowing a stressed individual to engage in a survival response (e.g., fight or flight; Liu et al., 2017). However, under conditions of severe, frequent, or prolonged stress (e.g., chronic or complex exposure to childhood adversity), particularly in the absence of a safe, caring parent or guardian, a child's HPA axis may become dysregulated, resulting in a toxic stress response.

The toxic stress response is characterized by the prolonged and dysregulated release of glucocorticoids. This prolonged and dysregulated release of glucocorticoids can cause oxidative damage in stress-sensitive brain regions resulting in neuronal death and disruptions in neural sensitive periods (Berens et al., 2017; Danese et al., 2008; Danese & Baldwin, 2017; McLaughlin & Lambert, 2017) . Critically, these structural and functional brain alterations can lead to various forms of cognitive and emotional dysfunction and may ultimately serve as proximal risk factors for academic performance deficits and as distal risk factors for poor psychosocial functioning later in life. (Aristizabal et al., 2020; Berens et al., 2017; Ito et al., 1998; National Scientific Council on the Developing Child, 2014). For example, exposure to childhood adversity has been shown to increase risk of special education service involvement, lower grades, school dropout and imprisonment, suicide attempts, substance use, post-traumatic stress, early

pregnancy, and memory impairment (Hillis et al., 2004; Nelson et al., 2020; Perfect et al., 2016).

Currently, there are many school-based interventions that attempt to reduce children's toxic stress response to adversity and either prevent or attenuate the cascading consequences of this response. One approach, the "trauma-sensitive school" approach aims to do this by improving school personnel's awareness, understanding, and behavioral responses to childhood adversity and toxic stress in order to promote student resilience. Understanding the mechanisms of the toxic stress response can inform the development of these interventions by allowing developers to select specific evidence-based strategies to attenuate these mechanisms and ultimately reduce the risk of poor psychosocial outcomes and promote resilience. Thus, specific conceptual pathways between the toxic stress response and impairment were reviewed in detail below.

1.3.1 Sequalae of Toxic Stress

1.3.1.1 Hippocampal and striatal dysfunction: Learning impairments.

The hippocampus is a stress-sensitive brain region that experiences significant post-natal development and is critical for healthy learning and memory. As such, this brain region may be particularly vulnerable to the toxic stress response produced from chronic or complex exposure to childhood adversity. Accordingly, one hypothetical pathway from childhood adversity to psychopathology is as follows: exposure to chronic or complex childhood adversity produces a toxic stress response which structurally and functionally changes the hippocampus. These changes in the hippocampus produce deficits in learning and memory which manifest as academic performance deficits and

poor psychosocial adjustment. Currently, there is cross-sectional, empirical evidence supporting specific links in this pathway. For example, exposure to early childhood adversity has been significantly associated with decreased hippocampal volume. Additionally, decreased hippocampal volume has, in turn, been associated with psychopathology (Bick & Nelson, 2016; Levy-Gigi et al., 2014; Vythilingam et al., 2002), though it should be noted that to date no studies have been conducted to statistically analyze this entire pathway (i.e., ACEs lead to decreased hippocampal volume which leads to psychopathology).

The mechanism through which reduced hippocampal volume may confer risk for poor psychosocial functioning is impairments in relational memory including context overgeneralization (i.e., difficulty with learning that a previously negative context can later be associated with a positive outcome; Chaddock et al., 2010; Hassevoort et al., 2016; Lambert et al., 2019; Levy-Gigi et al., 2014). Relational memory (i.e., the ability to remember arbitrary associations between objects or events; Koenig, 2017), is critical for learning new information in class, integrating facts about people, places, and events into a coherent essay, or even remembering a teacher's name when a child sees their face (Hassevoort et al., 2016). Thus, deficits in relational memory would be expected to interfere with academic performance.

Dysfunctional reward learning as a result of the toxic stress response, is another hypothesized link between childhood adversity and poor psychosocial functioning (McLaughlin et al., 2019). Empirical research has demonstrated that individuals with exposure to childhood adversity display diminished striatal response to anticipated rewards (Teicher & Samson, 2016). This is concerning as prospective studies with

children have demonstrated that low motivation to experience reward predicts future depression even when controlling for previous depressive symptoms (Forbes et al., 2007; McLaughlin et al., 2019). Thus, the current literature suggests that childhood adversity is associated with deficits in striatal response to rewards and impairments in specific types of memory through changes in hippocampal structure and function. These deficits can manifest as academic performance deficits and confer risk for later psychopathology. This underscores the need for interventions to mitigate the relations between childhood adversity and dysfunctional learning processes.

1.3.1.2 Amygdala dysfunction: Attentional bias to threat.

The amygdala plays an important role in attending to and processing fearful and threatening stimuli, regulating emotion, and encoding memories and has therefore received significant research attention in childhood adversity research. One proposed conceptualization of the amygdala's role in the development of psychopathology and poor psychosocial adjustment is as follows: exposure to chronic or complex childhood adversity activates a toxic stress response which causes hyperactivity in the amygdala. Chronic hyperactivity in the amygdala then confers risk for poor psychosocial functioning (e.g., psychopathology, poor peer relations) via attentional biases towards threat. This attentional bias towards threat is likely a result of the positive feedback loop that the amygdala produces when chronically hyperactive, in which the amygdala cyclically triggers and re-absorbs high levels of cortisol (Shonkoff et al., 2012). Although this is an adaptive response in dangerous situations as it allows for constant vigilance for threat, in non-dangerous situations, attentional bias towards threat results in overidentification of threat and hostile attribution bias (de Castro et al., 2002; Verhoef et

al., 2019; Yaros et al., 2014). These social information processing deficits have been shown to partially mediate the relation between childhood abuse and conduct problems and produce in-the-moment aggression (Dodge et al., 1995; McLaughlin & Lambert, 2017; Ronkin, 2017; Schwerdtfeger Gallus et al., 2015).

Empirical evidence collected from a 2017 meta-analysis supports the conceptual link between childhood adversity and functional changes in the amygdala (Hein & Monk, 2017). Additionally, multiple studies have revealed heightened amygdala activity in response to angry and fearful faces as well as angry voices among children with chronic exposure to adversity compared to children without adversity exposure, indicating increased vigilance for threat (Bick & Nelson, 2016). Empirical evidence also supports the link between amygdala dysfunction and poor psychosocial functioning by demonstrating that attentional bias to threat and hostile attribution bias increase risk for anxiety, negative affect during adverse peer interactions, and relational aggression among youth (Bick & Nelson, 2016; Kokkinos et al., 2017). Once again, a limitation to this body of research is that it relies heavily on cross-sectional data collection, making it difficult to draw causal conclusions. Additionally, some researchers interpret this activation as emotional reactivity rather than threat vigilance (Bick & Nelson, 2016; McLaughlin et al., 2020). Regardless, the state of the literature suggests that childhood adversity is associated with patterns of attention that cause functional impairments in school environments. This underscores the need for interventions to mitigate the relations between childhood adversity and maladaptive attending.

1.3.1.3 Disruptions in neurocircuitry: Emotion dysregulation.

Not only are individual brain regions crucial for healthy functioning, but the connectivity between brain regions is also important. Coupling between the ventromedial prefrontal cortex (vmPFC) and the amygdala has received research attention for its role in the effective regulation of emotional responses (Bick & Nelson, 2016). Given that the prefrontal cortex and the amygdala both display high levels of plasticity during childhood, they are likely sensitive to the impacts of childhood adversity. Additionally, as emotion dysregulation is a hallmark feature of many forms of psychopathology (Berking & Wupperman, 2012; Ehring & Quack, 2010; Salters-Pedneault et al., 2006; Weinbach et al., 2018), dysfunction in the coupling of the vmPFC and amygdala would hypothetically be present among individuals with psychopathology as well. Thus, disrupted coupling of the vmPFC and amygdala may be one mechanism through which the toxic stress response confers risk for poor psychosocial functioning.

There is some neurological and cognitive evidence to support these hypothesized associations. For example, one fMRI study demonstrated that adolescents with abuse histories exhibit greater amygdala activation paired with lower vmPFC activation while viewing negative stimuli when compared to adolescents without childhood adversity (Peverill et al., 2019). In other words, adolescents with exposure to childhood adversity had an increased emotional response and less ability to downregulate that response. This study additionally demonstrated that negative vmPFC-amygdala coupling prospectively predicted externalizing psychopathology two years later (Peverill et al., 2019).

Cognitively, recruitment of the PFC during emotion regulation tasks corresponds with the use of reappraisal, a helpful emotion regulation strategy in which individuals

make a conscious effort to reinterpret a given stimulus (for review see Berboth & Morawetz, 2021). Thus, it is possible that the increased negative coupling between the vmPFC and amygdala seen among youth with significant childhood adversity indicates a lack of cognitive reappraisal and a reliance on other less effective emotion regulation strategies. Examples of less effective cognitive emotion regulation strategies include thought suppression (attempts to control or ignore intrusive thoughts), rumination (repetitive and passive focus on the causes and consequences of one's own distress), catastrophizing (overestimation of the negative consequences of an event), and impulsive responses to distress (e.g., risky sex, substance use, violence), all of which have been implicated in psychopathology (Heleniak et al., 2016; Jenness et al., 2016; Nitzan-Assayag et al., 2017; Trickey et al., 2012).

However, some research into the relation between these cognitive emotion-regulation strategies with childhood adversity and psychopathology does not support the adversity-toxic stress-emotion dysregulation-psychopathology pathway, and instead points to pre-existing levels of emotion dysregulation as predictors of psychopathology following exposure to adversity. For example, one study found that pre-adversity levels of rumination and catastrophizing predicted PTSD among adolescents who were exposed to a terrorist attack, even when controlling for pre-existing exposure to violence and internalizing symptoms (Jenness et al., 2016). These mixed findings highlight the importance of a universal rather than targeted approach to trauma-informed school interventions. Universal school-based trauma-sensitive interventions that promote emotion regulation can equip non-adversity-exposed students with coping techniques that may prevent the development of a toxic stress response in the event of future exposure to

adversity. Emotion regulation skills interventions would also be expected to attenuate the impact of the toxic stress response on school functioning among students who have been exposed to adversity.

Given the substantial social-emotional learning consequences of exposure to childhood adversity, efforts to mitigate these consequences are needed. Schools are ideal environments in which to intervene with adversity-exposed children, particularly given recent federal legislation emphasizing the importance of "trauma-informed" systems and infusing schools with substantial funding for systemic implementation of social-emotional learning interventions (Maynard et al., 2019; Overstreet & Chafouleas, 2016; Yarmuth, 2021). Trauma-sensitive schools is one intervention approach that has the potential to interrupt the mechanisms linking childhood adversity exposure to psychosocial impairment. The trauma sensitive approach involves viewing student social-emotional difficulties as a direct result of the adversity or "trauma" they have experienced and responding with supports that promote resilient functioning. Deciding on specific supports to include should be based on a firm understanding of pathways from adversity to impairment and factors that moderate these pathways.

1.4 Factors that Moderate the Impact of Childhood Adversity

Childhood adversity leads to a diversity of outcomes over time. While many children who experience adversity will develop a toxic stress response and experience serious, negative long-term psychosocial consequences, others will not. This reality is consistent with the idea of Multifinality. Multifinality is a core principle of developmental psychology that refers to the concept that the same environmental

condition or event may ultimately lead to varied developmental outcomes (Howe, 2011). For example, while over half of U.S. children have experienced adversity, the lifetime prevalence rate of PTSD is estimated at 8.7% for U.S. adults (American Psychiatric Association, 2013). Further, though there is an association between childhood adversity and depressive disorders, anxiety disorders, and externalizing behavior disorders, the percentage of people with exposure to childhood adversity is also much higher than lifetime risk of these disorders (American Psychiatric Association, 2013). Thus, most individuals who experience childhood adversity do not go on to develop psychopathology. Instead, resilience—the ability to maintain a stable, healthy levels of psychological and physical functioning—is the modal response to childhood adversity (Bonanno, 2004). This discrepancy has generated both theoretical and empirical research interest in risk factors for the development of psychosocial impairment following childhood exposure to adversity and protective factors that promote resilience against developing psychosocial impairment.

1.4.1 Theoretical Moderation of Vulnerability and Resilience

Researchers have adopted different approaches in conceptualizing vulnerability and resilience. Recently, the three-hit theory of vulnerability and resilience reconciled previous theoretical models including the cumulative risk model, the differential susceptibility to environmental influence model, and the match/mismatch model. The three-hit theory also uses data from animal experiments and gene-environment interaction studies in humans to explain the differential outcomes of exposure to childhood adversity. Specifically, the three-hit theory posits that genetic factors (Hit 1) interact with

early life experiences (Hit 2) to create “programmed phenotypes” (i.e., emerging personality traits). Programmed phenotypes then interact with later life circumstances (Hit 3) to precipitate vulnerability or resistance to stress-related mental health disorders (Daskalakis et al., 2013). According to the three-hit theory, it is the interaction of this early, experience-based, biological programming and an individual’s cognitive and emotional perceptions of later life stressors that ultimately determine vulnerability to psychopathology or resilience.

The three-hit theory aligns with social-ecological theories of child functioning as well by accounting for multiple levels of a child’s social-ecological determinants of health. Intrapersonal factors like genetics and temperament occur at the child level, while interpersonal factors like social support characterize the child’s microsystem, and still other contextual factors, such as school characteristics, can also occur in a child’s microsystem or mesosystem. For example, a child may develop resilience if their adverse experiences are brief, and their emerging phenotype combined with mesosystem factors like parent-school communication allow the child to successfully cope with the experience (i.e., stress inoculation; Hornor, 2017).

The three-hit model is particularly encouraging for those developing trauma-sensitive schools interventions, as it suggests that critical contributing factors to the development of resilience are modifiable. Children’s thought patterns can be restructured, behaviors can be learned, and environments can be modified. The only unmodifiable factors are genotype (although phenotype is responsive to environmental factors) and the timing of sensitive periods of development. Given that children spend most of the day in school, abundant opportunities exist for school systems to engage in a trauma-informed

approach to intervene on these modifiable factors in children's lives. This study plans to examine studies that have engaged in this trauma-informed approach to elucidate the overall effect of these programs on youth resilience and determine essential elements of a trauma-informed school.

Using the three-hit model as a guiding framework, a review of specific intrapersonal, interpersonal, and other contextual factors that interact to promote psychopathology or resilience is presented below. Importantly many factors at each level can either confer risk for psychopathology or promote resilience. While an abundance of risk and protective factors exist, this review was limited to factors that are likely to be targeted by trauma-sensitive schools interventions. Additionally, though many of the following studies provide evidence about the mechanisms link childhood adversity exposure with specific mental health disorders, it is likely that these mechanisms more broadly link childhood exposure and a range of poor outcomes. This is due to the fact studies have repeatedly shown that risk factors influence latent mechanisms and processes that create a generalized vulnerability for poor outcomes. That is, these risk factors have no unique effects on the development of specific mental health disorders after adjusting for associations with these latent processes (McLaughlin et al., 2020).

1.4.2 Intrapersonal Risk and Protective Factors

Intrapersonal risk and protective factors are any biological factors or personal history that influence how a person thinks, behaves, and feels. An example of a biological protective factor is an easy temperament that elicits positive responses from caregivers. Easy infant temperament has been found to longitudinally predict resilience following childhood adversity (Werner, 2005). Certain biological risk factors were identified in a

meta-analysis of neural responses to threat following maltreatment and included hyperactive amygdala and anterior insula responding to negative environmental stimuli (Hein & Monk, 2017). As previously discussed, hyperactive amygdala responding is associated with maladaptive attentional patterns that prioritize threat and result in social information processing deficits and increased emotion reactivity. Thus, a functional school environment that is routine, predictable, and safe represents one strategy to buffer the impact of this heightened threat detection and responding (Shamblin et al., 2016).

A meta-analysis conducted by Trickey and colleagues (2012) found that individual psychological factors among children with PTE exposure yielded significant effects on the development of PTSD even after controlling for publication bias. The individual psychological factor with the largest effect size was thought suppression (ρ_{pb}= .60). Additional factors identified include blaming others (ρ_{pb}= .52), distraction as a coping technique (ρ_{pb} = .42), and comorbid psychological problems (ρ_{pb}=.36). The authors additionally identified low self-esteem and social withdrawal as risk factors (Trickey et al., 2012). Additional studies have built on these findings regarding the role of cognitive emotion regulation strategies by demonstrating that rumination and catastrophizing prospectively confer risk for the development of psychopathology following exposure to adversity in childhood (Jenness et al., 2016).

The presence of trait-level positive re-appraisal as an emotion regulation strategy has been prospectively found to protect against psychopathology following exposure to childhood adversity in some cases. This is illustrated by one ongoing longitudinal study of Boston adolescents which included a time point of survey data collection two weeks after the Boston Marathon bombing (Jenness et al., 2016). Adolescents with high levels

of pre-incident positive reappraisal who had been exposed to high levels of media coverage of the event had reduced risk for PTSD symptoms compared to adolescents with lower levels of pre-incident positive reappraisal. However, adolescents with high positive reappraisal and low media exposure did not experience this buffering effect. A trauma-sensitive school approach is an ideal approach to address these psychological intrapersonal risk factors through the development of caring, supportive relationships between students and staff. Within the context of these caring relationships, protective cognitive coping strategies can be taught, self-esteem can be built, and any comorbid psychological problems may be identified.

A personal history factor that promotes resilience is practical problem-solving skills. For example, a prospective longitudinal study conducted over the course of 40 years followed every child born in Kauai in 1955 and followed up at ages 1, 2, 10, 18, 32, and 40. Children who displayed resilience and positive adjustment following childhood adversity differed from those who displayed poor adjustment in their ability to solve practical problems at age 10 (Werner, 2005). Given that children routinely face practical problems every day in school, schools are an ideal context in which to teach practical problem-solving skills to promote resilience. In sum, schools with trauma-sensitive personnel would be able to leverage their knowledge of these intrapersonal moderators to promote positive adjustment by creating predictable classroom routines that promote a sense of safety, developing warm and caring bonds with students, modeling emotion regulation strategies, and teaching problem solving.

1.4.3 Interpersonal Risk and Protective Factors

Interpersonal risk and protective factors include factors relating to relationships the child has with family, peers, and other adults. Low social support and poor family functioning are two interpersonal risk factors for psychopathology among adversity-exposed youth that have been found across multiple studies (Trickey et al., 2012). Meanwhile, the presence of a safe, caring adult has been repeatedly demonstrated to buffer children against the effects of childhood adversity. In one review of protective factors for youth exposed to violence in their communities, close family relationships were found to promote youth mental health stability in the face of community violence (Ozer et al., 2015). Another study examined the relation between childhood adversity and depressive symptoms in a general population sample of seventh grade youth and found that interpersonal violence exposure was not associated with depressive symptoms for youth who had high connectedness to their parents. However, youth who experienced childhood interpersonal violence and were disconnected from their parents were more likely to have depressive symptoms (Schwerdtfeger Gallus et al., 2015). Accordingly, trauma-sensitive schools typically attempt to positively involve parents in their children's lives. However, many studies involving parents do not specifically analyze whether parent involvement accounted for any of the changes in student outcomes, thus one of the aims of the present meta-analysis were to examine the extent and impact of parent and caregiver involvement across trauma-sensitive schools interventions.

Caring adults outside of the child's family may also help protect children from the damaging effects of childhood adversity. In Ozer and colleagues' review, social support was found to buffer the impact of community violence on internalizing symptoms (Ozer

et al., 2015). Thus, trauma-sensitive school systems staffed with trauma-informed personnel have the potential to supply the relational protective factors that promote resilience to adversity (Shamblin et al., 2016). However, many school personnel report uncertainty in their role as it pertains to responding to children with a history of adversity exposure as well as a struggle with balancing conflicting needs of exposed children and peers (Alisic, 2012). Consequently, one critical component of trauma-sensitive schools interventions is not only increasing school personnel awareness of trauma, but also building positive teacher attitudes towards trauma-informed care and self-confidence relating to trauma-sensitive practices (Liang et al., 2020). Many trauma-sensitive schools interventions include a dose of staff training aimed at promoting these outcomes. However, due to the variety of methodology and outcome measures, it is unclear how overall effective these staff trainings are at improving these intermediate outcomes. Therefore, an additional purpose of the present meta-analysis was to examine the effect size of staff training on improvements in teacher attitudes and beliefs.

1.4.4 Other Contextual Risk and Protective Factors

Bronfenbrenner's social ecological systems theory posits that a child's development and functioning is the result of the dynamic interaction between the child and all levels of the child's social ecology. Accordingly, features of a child's proximal environment, distal environments, and the interaction between these environments may confer risk for or protection against psychopathology as well. Situational factors such as the nature, chronicity, complexity, and severity of childhood adversity predict risk for developing psychopathology (McLaughlin et al., 2013). For example, rape is a traumatic event that is most likely to result in PTSD among children and adolescents, followed by

kidnapping, sexual assault, physical assault by a romantic partner, and physical abuse from a caregiver (McLaughlin et al., 2013). Notably, these forms of childhood adversity are all interpersonal in nature, consistent with the extensive literature base that suggests interpersonal trauma is more likely than impersonal trauma to result in psychopathology (Blankenship, 2018; Friedman et al., 2014; Huang et al., 2016; McLaughlin et al., 2013; Wamser-Nanney & Vandenberg, 2013; Yoo et al., 2018).

Adversity that is chronic or complex in nature confers additional risk over acute adversity as evidenced by negative associations between adversity chronicity and brain volume (De Bellis et al., 2002), and evidence that experiencing more than one form of adversity (i.e., complex exposure) increases the risk of future psychopathology (Felitti et al., 1998). Severity of adversity has also been implicated as a risk factor for the development of psychopathology. For example, some literature has shown that disasters with larger death tolls relative to disasters with smaller death tolls confer greater risk for child psychopathology, particularly if the death toll is over 1,000 people (Furr et al., 2018). Disasters are a unique form of childhood adversity from a social-ecological perspective, as they can profoundly impact the entire human ecosystem as well, by causing school disruptions, loss of home, loss of income, changes in law and policy, and peer group changes (Comer & Kendall, 2007).

School factors may also confer risk for or protection against maladjustment among children with exposure to childhood adversity. School factors that may promote resilience in the face of childhood adversity include teacher's ability to view students' difficult behavior as rooted in trauma exposure (i.e., have a "trauma-lens") and adjust their practice accordingly to meet students' social-emotional needs (Daniels, 2021;

Frerks, 2020; Liang et al., 2020). Relationships with teachers specifically have been shown to be protective against toxic stress. One study conducted among Israeli youth found that positive relationships with homeroom teachers were linked to higher levels of posttraumatic growth, though not to reduced symptoms of posttraumatic stress (Yablon & Itzhaky, 2013). Another study examined the associations between adverse childhood experiences, student-teacher relationships, and problematic use of prescription medications among eighth, ninth, and eleventh grade youth. Consistent with previous literature, this study found that adolescents with more ACEs were more likely to inappropriately use prescription medication (Forster et al., 2017). However, this relationship was moderated by the presence of positive student-teacher relationships such that students with higher ACEs and positive relationships with teachers were less likely to use substances than youth with the same number of ACEs who did not have these relationships (Forster et al., 2017). Additionally, a supportive school-wide environment can play a significant role in promoting resilience among children who have experienced adversity (Cole et al., 2013). For example, school connectedness broadly has been linked to lower depressive symptoms among trauma exposed seventh graders (Schwerdtfeger Gallus et al., 2015).

A school-related factor that may exacerbate the impact of the toxic stress response is exclusionary discipline policies in schools. School suspension is a common exclusionary discipline practice that is disproportionately experienced by youth of color, youth in foster care, and youth with justice system involvement, in short youth with higher rates of childhood adversity (Baroni et al., 2020). In fact, one recent study estimated that children with exposure to ACEs specifically, were nearly three times more

likely to receive multiple school suspensions compared to children with no exposure to ACEs (Bell et al., 2021). Exclusionary discipline practices not only bar students from accessing their school-based protective factors (e.g., caring adults, routine, physical safety), but additionally confer risk for psychopathology, externalizing behavior problems, delinquency, school dropout, and academic failure (Breedlove et al., 2021). For example, one study found that schools' use of exclusionary discipline practices increases the risk of youth being stopped by police. These youth were also more likely to experience officer intrusiveness during the stop regardless of early delinquent involvement and increased risk post-traumatic stress symptoms following this experience (Jackson et al., 2021).

1.5 Trauma-Sensitive Schools Interventions

Given the substantial health, cognitive, and psychosocial consequences of exposure to childhood adversity, efforts aimed at reducing toxic stress and rehabilitating its ensuing impairment among children are crucial. While the most effective way to eliminate poor outcomes resulting from a toxic stress response would be to eradicate childhood adversity, it is not possible to do so. Rates of child abuse and neglect data collected by the Children's Bureau remain high, and rates of self-reported history of childhood adversity remain high (Child trends, 2019; U.S. Department of Health & Human Services, Administration for Children and Families, et al., 2021; U.S. Department of Health and Human Services, Administration for Children and Families, Administration on Children, Youth and Families, Children's Bureau, 2015). Even if all forms of

intentionally perpetrated childhood adversity were eliminated (e.g., violent crimes), many stressors would remain (e.g., disasters, pandemics, unintentional injuries).

Universal school-based prevention strategies may mitigate the impact of adversity on children's development and functioning. Schools specifically may reduce youths' toxic stress response and attenuate the development of the poor outcomes can result, given their reach—that is, youth between specific ages are required to attend school. Schools also have clear structures and routines and generally have multiple safe and trustworthy adults (Chafouleas et al., 2016; Perry & Daniels, 2016), factors promotive of youth resilience. Thus, universal school-based efforts to promote trauma-sensitive attitudes, behaviors, and policies represent a promising approach to reduce the impacts of childhood adversity on psychosocial adjustment and enhance children's success in school and life. However, research on the impact of trauma-sensitive interventions for schools has varied widely, and the effectiveness of these strategies, and factors that may moderate effectiveness, are currently unclear, creating a need for a meta-analysis to synthesize the available information.

1.5.1 A Flexible Framework

The description and terminology of trauma-informed care varies across literature generated from a range of child-serving systems (Chafouleas et al., 2016). For example, the terms trauma-sensitive, trauma-informed, and trauma-responsive are all used to describe systems and interventions that have a focus on meeting the social-emotional needs of its trauma-affected members. Further, recent publications outlining trauma-informed frameworks and implementing trauma-informed interventions adopt varying

approaches to implementation, action planning and prioritization of content knowledge (Baker et al., 2020; Overstreet & Chafouleas, 2016). Thus, the need arose for a unifying framework to guide research and intervention efforts in this area.

To meet this need, SAMHSA's Trauma and Justice Strategic Initiative created SAMHSA's Concept of Trauma and Guidance for a Trauma-Informed Approach in 2014 (Substance Abuse and Mental Health Services Administration, 2014). SAMHSA developed this framework through convening national experts in trauma, including trauma survivors, researchers, and policymakers in behavioral health. The resulting ideas were then vetted by federal agencies that conduct trauma-related work and uploaded to SAMHSA's website for public comment. Based on the 20,000 comments received, SAMHSA made revisions and developed the following framework which conceptualizes trauma as, the result of an event, series of events, or set of circumstances that is experienced by an individual as physically or emotionally harmful or life threatening and that has lasting adverse effects on the individual's functioning and mental, physical, social, emotional, or spiritual well-being" (Substance Abuse and Mental Health Services Administration, 2014).

SAMHSA's trauma-informed approach involves four key assumptions, six key principles, and ten implementation domains. The four key assumptions are the "Four R's" of trauma-informed care: realize, recognize, respond, and resist re-traumatization. Realization involves all members of the system understanding the prevalence and impact of trauma on individuals, families, organizations, and communities. Recognizing trauma is the ability to detect post-traumatic stress symptomology and responding to trauma involves applying the six principles of the trauma-informed approach (see below) to all

components of a system including policies, language, and behaviors. Finally, resisting re-traumatization involves the understanding that organizations can inadvertently create toxic environments that interfere with their member's recovery and actively work to prevent this from occurring (Substance Abuse and Mental Health Services Administration, 2014).

To effectively implement the Four R's of a trauma-informed approach, SAMHSA created six guiding principles rather than a prescribed set of practices. The rationale behind this decision was to make these principles generalizable across multiple types of settings rather than prescribing restrictive practices which may or may not be applicable to different organizations. The six key principles of a trauma-informed approach are safety; trustworthiness and transparency; peer support; collaboration and mutuality; empowerment, voice, and choice; and cultural, historical, and gender issues. Safety refers to both physical and psychological safety within the system and goes hand in hand with trustworthiness and transparency. Peer (an individual with lived experiences of significant adversity) support and mutual self-help additionally promote safety and trust by enhancing collaboration among survivors. Collaboration and mutuality focus on leveling the power differences between staff and, in the case of schools, students, as well as among differing levels of staff (e.g., janitorial staff versus teaching staff). This power leveling allows for the recognition that everyone has a role to play in a trauma-informed approach and that healing happens in relationships (Substance Abuse and Mental Health Services Administration, 2014).

The principle of empowerment, voice, and choice emphasizes the primacy of people served, resilience, and the ability to heal and recognizes that trauma may be a

unifying aspect in the lives of all members of an organization. As such, organizations following this principle provide services to foster empowerment for staff and clients alike. In the case of schools, this means providing services not only for students, but for faculty and staff as well. This principle also supports shared decision making and the cultivation of self-advocacy skills. The final guiding principle, cultural, historical, and gender issues, guides organizations past cultural stereotypes and biases and towards responsive, community-based policies, processes, and protocols (Substance Abuse and Mental Health Services Administration, 2014).

Systems desiring to be trauma-informed use these principles to guide decision making at multiple levels of the system. To facilitate this process, SAMHSA's trauma-informed guidance includes 10 implementation domains across which to implement their guiding principles: governance and leadership; policy; physical environment; engagement and involvement; cross sector collaboration; screening, assessment, treatment services; training and workforce development; progress monitoring and quality assurance; financing; and evaluation. These are domains of organizational change that have appeared in the organizational change management literature and among various models for establishing trauma-informed care (Substance Abuse and Mental Health Services Administration, 2014).

1.5.2 Current Approaches to Trauma-Sensitive Schools

Given the broad and guiding, rather than instructing, nature of the trauma-informed framework, a diverse range of trauma-sensitive intervention strategies for implementation in schools has been developed. Importantly, trauma-sensitive schools interventions implement SAMSHA's Four Rs of trauma-informed care by using at least

two of the three following elements: workforce professional development, trauma-sensitive practices, and organizational environmental changes (Avery, Morris, Galvin, et al., 2021; Hanson & Lang, 2016; Maynard et al., 2019). Workforce professional development encompasses staff training that builds awareness of childhood adversity and its sequalae, provides restorative rather than punitive behavior management techniques, and teaches staff emotion regulation skills. These trainings may also include information about secondary traumatic stress (Hanson & Lang, 2016). Examples of trauma-informed practices or services include screening for exposure to childhood adversity, evidence-based student interventions, and intentionality towards relationships with students (Avery et al., 2021) Organizational or environment change includes restorative discipline policies, protocols for parent communication, physical environment changes, or partnerships with community health agencies (Avery et al., 2021; Hanson & Lang, 2016; Maynard et al., 2019). These three elements theoretically act upon the mechanisms linking exposure to childhood adversity with poor outcomes, increase protective factors, and decrease risk factors to ultimately promote youth resilience (see Figure 1).

These intervention components may benefit youth through two primary means. First, intervention components will act upon the mechanisms linking childhood adversity to psychopathology including the toxic stress response, social information processing deficits, learning and memory deficits, and emotion regulation deficits. By attenuating these mechanisms, the cascade of negative outcomes following adversity exposure can be interrupted and the risk of poor psychosocial adjustment reduced. Second, these intervention components increase the presence of promotive factors known to build resilience among children while also decreasing risk factors. Examples include

developing a trauma-lens among school personnel, thereby promoting positive staff-student relationships; increasing family involvement; reducing the use of exclusionary discipline practices; and developing children's practical problem-solving skills (Day et al., 2015; Dorado et al., 2016; Ijadi-Maghsoodi et al., 2017; Perry & Daniels, 2016). Aligning with the three-hit theory of vulnerability and resilience, these strategies are expected to create the necessary early life experiences and circumstances to help children develop programmed phenotypes that precipitate resilience in the wake of future stressful circumstances.

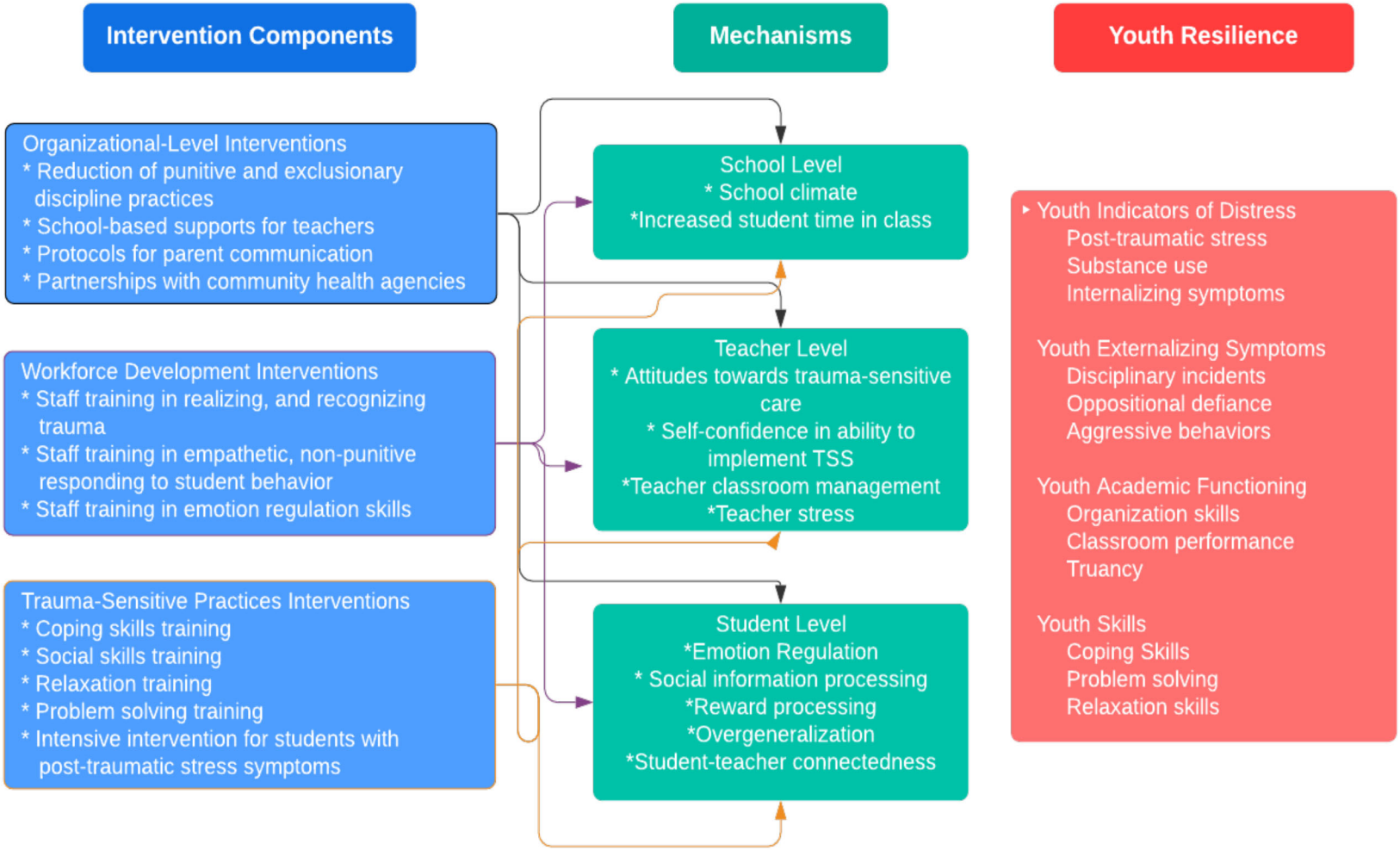


Figure 1. Theory of change for trauma-sensitive schools.

1.5.2.1 Work Force Professional Development.

Trauma-sensitive schools interventions typically begin with work force training as it is conceptualized as a change catalyst, or the necessary "ah-ha" moment, central to the up-take of trauma-sensitive practices. Indeed, one systematic review of school-wide trauma-sensitive interventions found that all studies included reported that staff training was found to assist staff in reframing challenging student behaviors, thereby decreasing their own potential reactive responses and the possibility of punitive practices (Avery et al., 2021). Though these results are promising, it is still unclear which specific components of work force development trainings and dosage are required to see positive outcomes (Avery, Morris, Galvin, et al., 2021; Purtle, 2020). A meta-analytic examination of effect sizes of specific components of these trainings has the potential to answer these questions.

1.5.2.2 Trauma-sensitive Practices.

Trauma-sensitive practices refer to a wide variety of interventions, behaviors, and strategies. For example, a classroom-based approach to engaging in trauma-informed practices may include universal classroom-based emotion regulation and social skills training. One study utilized an adapted version of the Resilience Classroom Curriculum, a 7 module curriculum delivered by social workers that utilizes direct instruction, videos, demonstration, discussion, role-plays, and experiential exercises (Ijadi-Maghsoodi et al., 2017). This study found through qualitative interviews that, though teachers were not required to participate or engage in this curriculum, the teachers appeared to enjoy and learn from the curriculum. One social worker reported that the teacher completely

overhauled their curriculum to match the module of the week (Ijadi-Maghsoodi et al., 2017). Another study implemented trauma-sensitive practices in rural Appalachia by utilizing consultants to deliver The Incredible Years or Second Steps to students based on school resources and preferences (Shamblin et al., 2016).

One strategy to dually promote trauma-sensitive behaviors among teachers and provide trauma-sensitive services to students, is to train teachers to implement emotion regulation and social skills lessons in the classroom. One study that used this approach found that following the intervention, more teachers reported that they felt prepared to respond to children who have been exposed to trauma, and over half of the teachers believed that these tools helped students manage their emotions (McConnico et al., 2016). Certain interventions additionally provide intensive trauma-focused services to students who display elevated levels of post-traumatic stress (Dorado et al., 2016; Holmes et al., 2015). These intensive interventions typically employ cognitive behavioral therapy techniques to address maladaptive trauma-related beliefs and behaviors. Evidence for trauma-focused trauma cognitive-behavioral therapy in schools is abundant and demonstrates that this intervention is particularly effective for youth with post-traumatic stress symptoms (Allison & Ferreira, 2017; Goodkind et al., 2010; Langley et al., 2015; Morsette et al., 2009; Orengo-Aguayo et al., 2020; Stein et al., 2003). Given the diversity of approaches within trauma-sensitive interventions, it is important to understand whether the presence or absence of trauma-sensitive practices impact intervention outcomes.

1.5.2.3 Trauma-Sensitive Organizational Environment Changes.

Finally, the third element of trauma-sensitive schools interventions- organizational environment change- includes restorative discipline policies, protocols for

parent communication, physical environment changes, or partnerships with community health agencies. (Avery et al., 2021). Once again, a variety of strategies has been used to create trauma-sensitive organizational environments. The Healthy Environments and Response to Trauma in Schools (HEARTS) intervention designated members of their research team to act as trauma-informed consultants within the school system. These consultants met with school leadership to create trauma-sensitive alternatives to exclusionary discipline policies. Examples included allowing students to "cool off" after disruptive behavior, an opportunity to return to the classroom and repair following a "meltdown," and positive behavior supports (Dorado et al., 2016). Results of this study found that after five years of HEARTS intervention, there was a 95% decrease in out-of-school suspensions across the four participating schools (Dorado et al., 2016). Another trauma-sensitive schools intervention conducted in a girls' residential facility school created a Monarch Room and Dream Catcher room as alternatives to traditional punitive discipline policies. These rooms served as safe physical spaces in which to calm down, re-regulate, and problem solve. Youth in this study used this room both voluntarily and at the direction of their teachers (Day et al., 2015). Another example of an organization-level change is building support systems for teachers and parents. One trauma-sensitive schools intervention for pre-schools developed a staff peer-based mentoring program as part of their trauma-sensitive pre-school intervention. Staff peer mentoring offered teachers and supervisors opportunities to support each other and promote sustainability of the other components of the intervention (Holmes et al., 2015). Finally, certain trauma-sensitive organizational change efforts involve community outreach and partnerships to connect youth and families to additional services (Beehler et al., 2012; Shamblin et al.,

2016). Because data across studies have not been statistically synthesized and analyzed, questions remain regarding which organizational policies and procedures have the biggest impact on intermediate mechanisms of change such as teacher attitudes and student time in class, as well as the primary outcome of student resilience.

1.6 Recent Reviews

During the past five years research efforts to synthesize trauma-informed schools programming have emerged. These include seven systematic reviews (Avery, Morris, Galvin, et al., 2021; Berger, 2019; Cohen & Barron, 2021; Fondren et al., 2020; Maynard et al., 2019; Roseby & Gascoigne, 2021; Zakszeski et al., 2017), one narrative review (Record-Lemon & Buchanan, 2017), one interdisciplinary review (Thomas et al., 2019), one scoping review (Stratford et al., 2020), and selected reviews (e.g., Blodgett & Dorado, 2016; Ventura, 2021). Of the systematic reviews, all except for Zakszeski and colleagues (2017) synthesized interventions that identified themselves as "trauma-sensitive," "trauma-informed," or "trauma responsive." Zakszeski and colleagues' 2017 review required that studies used trauma-*focused practices* but not necessarily a trauma-*informed approach*. Trauma-*focused practices* are specific interventions designed to treat trauma-related symptoms like post-traumatic stress, depression, and anxiety (Maynard et al., 2019; Zakszeski et al., 2017), whereas a trauma-*informed/responsive/sensitive approach* refers to the general framework of service delivery as described by SAMHSA (Zakszeski et al., 2017). Certain reviews additionally excluded studies that did not specifically focus on children who had been exposed to trauma (Fondren et al., 2020; Roseby & Gascoigne, 2021), For example, Fondren's 2020 systematic review required

that the trauma-informed/responsive interventions were administered, “in response to the exposure of or risk of exposure to a trauma” rather than as a broad preventive measure (Fondren et al., 2020).

Two systematic reviews limited studies to those that included two of the three major elements of trauma-informed approaches as identified by SAMHSA and the Trauma Learning Policy Initiative’s (TLPI) Flexible Framework: work force professional development, practice change, and organizational change (Avery et al., 2021; Maynard et al., 2019). This criterion was imposed to distinguish trauma-sensitive interventions from trauma-focused interventions which are specific interventions designed to treat trauma-related symptoms (Maynard et al., 2019). However, Maynard and colleagues (2019) were unable to conduct their proposed systematic review as no studies met their criteria. Perhaps most restrictive was the criterion that studies included must contain at least one between subjects’ comparison (i.e., 49 of the 67 studies assessed were excluded based on this criterion). Avery and Colleagues (2021) extended upon this work and Berger’s (2019) work by removing that criterion, thus allowing for mixed methods study designs that may or may not be within a multi-tiered framework. This approach resulted in four studies that met criteria. Perhaps the most inclusive systematic review was that of Cohen and Barron (2021) which additionally included gray literature sources and resulted in 9 eligible studies for review, though this review was limited to high schools only.

Overall, the reviews conducted to date indicate that there is high heterogeneity in the methods, measures, populations, and results of trauma-sensitive schools programs. However, a few notable broad patterns have been identified. For example, multitiered systems of support were identified as a common approach to implementing the trauma-

informed framework in school settings. Tiers included low intensity or universal interventions, intermediate or selected interventions, and high intensity or targeted interventions (Berger, 2019; Fondren et al., 2020; Roseby & Gascoigne, 2021; Zakszeski et al., 2017). Other approaches identified include using the attachment, self-regulation, and competency (ARC) model; the Berry St. education model; the heart of teaching and learning: compassion, resiliency, and academic success program; "calm-down spaces;" individualized models; PACE Centre for Girls; Risking connection, the Trust-Based Relational Intervention, the New Haven Trauma Coalition Program; and cognitive behavioral approaches (Avery et al., 2021; Fondren et al., 2020; Roseby & Gascoigne, 2021). Further all the systematic reviews, apart from Maynard et al. (2019) who was unable to review articles, concluded that studies evaluating the efficacy of trauma-sensitive schools initiatives are in their infancy, and there are many interventions that show promise (Cohen & Barron, 2021; Roseby & Gascoigne, 2021; Zakszeski et al., 2017).

Approximately 70% of the trauma-informed schools interventions reviewed found improvements in child externalizing behavior following interventions, while only 30% of interventions found no significant differences. Externalizing behaviors were measured through student self-report, teacher report, and official school records of suspensions and aggressive incidents (Avery et al., 2021; Cohen & Barron, 2021; Fondren et al., 2020; Roseby & Gascoigne, 2021). Similar rates were found among studies that measured changes in internalizing symptoms and youth distress (e.g., anxiety, depression, grief) with approximately 64% of reviewed studies finding improvements in these symptoms, and 36% finding no difference as a result of the trauma-informed intervention (Berger,

2019; Fondren et al., 2020; Roseby & Gascoigne, 2021). Many studies also examined changes in academic performance including performance on standardized tests, reading and math achievement, classroom engagement, and approximately 71% of studies reviewed indicated positive changes in these outcomes, while 29% of interventions found no changes (Avery et al., 2021; Berger, 2019; Cohen & Barron, 2021; Fondren et al., 2020; Roseby & Gascoigne, 2021). Finally, 71% of studies indicated improvements in posttraumatic stress symptoms, and 67% of studies found improvements in student adaptive functioning or wellbeing. Interestingly, one study found that certain resilience measures actually declined following intervention (Judge, 2018; Roseby & Gascoigne, 2021).

Some studies reviewed additionally examined intermediate factors that theoretically act as mechanisms through which the interventions exert their effects, such as teacher attitudes and knowledge. Interventions seemed particularly successful in increasing teacher knowledge related to trauma (i.e., 83% of studies reported increases with only 17% finding no change) and improving relevant teacher attitudes (e.g., attitudes related to trauma-informed care, perceived self-efficacy and self-confidence). Approximately 78% of trauma-informed interventions reviewed in these systematic reviews reported improved teacher attitudes, with only 22% reporting no change, and no studies reporting declines (Avery et al., 2021; Berger, 2019; Cohen & Barron, 2021; Fondren et al., 2020; Roseby & Gascoigne, 2021). School climate, another hypothesized intermediate mechanism, was examined by some studies. Of these studies, 43% indicated improvement in factors related to school climate such as student-teacher relationship

quality (Avery et al., 2021; Cohen & Barron, 2021; Fondren et al., 2020; Roseby & Gascoigne, 2021).

These systematic reviews represent important efforts to synthesize and generalize findings about trauma-sensitive approaches in schools. However, certain limitations present in these reviews warrant further efforts in this area. Many reviews pose age restrictions (e.g., ages 6-18, high schools only) that limit the body of research available to synthesize. Additionally, restrictions on school type (e.g., residential versus non-residential), and inclusion of studies that use only a whole-school or multitiered approach may limit the ability to draw conclusions about the impact of applying a trauma-informed framework in school settings. Another important limitation is the variety in operationalization of the term trauma-informed/sensitive/responsive. Some reviews require that a study only identify itself as a "trauma-sensitive" intervention to be included in analysis, while other reviews (i.e., Avery et al., 2021; Maynard et al., 2019) required that studies include two of the three SAMHSA identified elements of trauma-sensitive interventions: trauma-informed work force development, trauma-informed practice change, and trauma-informed organizational change to be considered a trauma-sensitive approach. While the definition restrictions in the Avery and Maynard reviews are important for conceptual clarity, it remains unclear if including multiple elements is more effective than interventions that include one of the three elements. Similarly, some reviews excluded studies that did not use multi-tiered designs (Berger, 2019), making it impossible to evaluate whether this approach is more effective than non-tiered approaches to trauma-sensitive programs in schools.

Finally, given that the systematic reviews did not include statistical analysis of effect size, drawing conclusions about the overall impact of interventions on youth resilience, and the intermediate mechanisms that theoretically lead to youth resilience, has not been possible. This is important in light of the mixed findings highlighted by existing systematic reviews. Given these limitations, further efforts to understand the efficacy of trauma-sensitive schools interventions are warranted.

1.7 Present Study

The purpose of the present study is to synthesize research on the effectiveness of trauma-sensitive schools interventions through a systematic review and meta-analysis. Effects of trauma-sensitive schools were assessed according to their impacts on intermediate mechanisms (school climate, staff wellbeing, trauma-sensitive attitudes, knowledge, and behaviors) and primary outcomes (youth indicators of distress/internalizing symptoms, youth externalizing behaviors, youth academic functioning, youth coping, and youth access to services). Currently there is a lack of clarity of the effectiveness of trauma sensitive schools due to the heterogeneity in study design, procedures, populations, and outcome measures. This gap in the extant literature is important to address, as this information is vital to schools with limited resources deciding between a variety of social-emotional interventions for their system. Therefore, this study aimed to meta-analyze the effects of trauma-sensitive schools interventions. Accordingly, the first hypothesis is that the meta-analysis of trauma-sensitive schools programs will yield significant positive mean effects across intermediate mechanisms,

including staff and school climate characteristics, as well as the primary outcomes related to youth resilience (hypothesis 1).

Ultimately, school-based interventions are unlikely to have much practical utility or gain widespread adoption if there is no consensus on their necessary component parts. Thus, the second aim was to determine what intervention elements are needed to effect desired change and in what dosage. Bronfenbrenner's social ecological systems theory posits that a child's development and functioning is the result of the dynamic interaction between the child and all levels of the child's social ecology. Therefore, trauma-sensitive interventions that target multiple factors at each level of implementation (i.e., workforce development, trauma-sensitive practices, and organizational change) would be expected to be more successful. Therefore, the second hypothesis is that interventions utilizing multiple levels of implementation (e.g., workforce development and practice change) will have larger effects than interventions utilizing fewer levels of implementation (hypothesis 2).

Finally, a key factor in uptake of interventions by schools is the minimum dosage necessary to effect change. Single dosage options may be efficient and feasible for trauma-sensitive work force professional development and perhaps for developing plans for organizational environment change, but there is evidence to suggest that longer duration interventions and higher intensity social emotional programs result in larger effect sizes for academic outcomes and behavioral change (J. Durlak, 2016; Rosenblatt & Elias, 2008). However, other evidence suggests that quality of implementation rather than quantity is most important for student outcomes. For example, one study of the RULER Feeling Words Curriculum approach to social emotional learning found that students only

had more positive outcomes if their teachers taught more lessons at a moderate or high quality (Reyes et al., 2012). Further, despite funding at the federal level, state level and local level decisions ultimately determine individual school resources which may result in limited funding. It is therefore crucial that schools have evidence of dosage requirements to justify their decision making. Thus, the impact of dosage of professional development on intermediate mechanisms and student outcomes were meta-analytically explored.

CHAPTER II

METHODS

2.1 Eligibility Criteria

The following criteria were used for inclusion: (a) the article is in English and appears in published or unpublished form by January 1st 2001; (b) the age of youth participants is up to 22 years old; (c) the study examines the effectiveness of a trauma-informed or trauma-sensitive intervention in a school setting using at least one comparison (e.g., including pre-post and quasi-experimental designs); (d) an intervention was considered trauma-sensitive if it identified itself as such (i.e., includes the terms "trauma-sensitive," "trauma-informed," or "trauma-responsive" and includes at least one of the following components: work force professional development, practice change, or organizational or environmental change); (e) the study must include outcome measures assessing the primary outcome of youth resilience (i.e., youth distress/internalizing symptoms, externalizing behaviors, coping or health maintenance behaviors, wellness, or academic functioning) or measures of theoretical mechanisms of change (i.e., teacher attitudes, behaviors or knowledge; school climate). Studies were excluded if the trauma-informed intervention under study was solely the provision of school-based individual or group mental health services. Single participant case studies were also excluded.

2.2 Information Sources

PRISMA guidelines for the reporting of systematic reviews were followed, and the protocol was pre-registered in PROSPERO. The formal search for candidate articles were conducted using SCOPUS, Web of Science, PsycINFO, PubMed, ERIC, and ProQuest Dissertations and Theses. The search terms were as follows: trauma sensitiv* or trauma* OR trauma informed OR trauma respon* AND intervention or program or initiative or educat* or prevention AND school* or class* or K12. Additional studies were identified using backward and forward reference searching for each identified study and existing systematic reviews on trauma-informed interventions in schools.

2.3 Selection Process

One reviewer conducted the initial search in all sources. Titles and abstracts were uploaded into Rayyan for review. Studies were examined at the title and abstract level by at least one reviewer. Titles and abstracts that were obviously ineligible (non-empirical report, book review, medical trauma, prior to 2001, etc.) were discarded. Articles that were not obviously ineligible were uploaded in full-text format to Rayyan. At least one reviewer reviewed all full text articles for inclusion or exclusion in the meta-analysis. Any article that did not clearly meet inclusion or exclusion criteria was resolved through discussion and consensus with a second reviewer.

2.4 Data Extraction

Following finalization of studies selection for analysis, essential information from each was coded into Comprehensive Meta Analysis. The extraction, coding, and analysis

process was based on an initial review of the literature. To ensure reliability of coding, given that this author extracted all study data, intra-coder agreement was analyzed by randomly re-coding 14% (k = 11; 56 effect sizes) of the included studies. Pearson's r was calculated to determine the strength of the relationship between continuous variables, and Kappa was calculated to determine the intra-rater agreement for dichotomous variables. Intra-rater reliability among continuous variables ranged from r = .73 – 1.00, with an average of r = .99. Among dichotomous variables, Kappa values ranged from .64 - .96 (mean Kappa = .87), indicating substantial to near perfect agreement. Extracted information included independent variables (i.e., intervention characteristics), methodological variables, and dependent variables (i.e., mechanisms of change and youth resilience-related outcomes).

2.4.1 Independent Variables

The major independent variables were intervention characteristics (see Table *1*). Specifically, the presence or absence of the three major elements of trauma-sensitive schools interventions were extracted (i.e., trauma-informed workforce/professional development, trauma-informed practice change, and trauma-informed organizational/environment change), and relatedly, the dosage of these elements (i.e., included one element, two elements, or all three elements) were coded as well. The dosage of trauma-related workforce professional development was coded as: none, less than eight hours, and eight to 40 hours. The total amount of time the entire intervention lasted was also extracted.

Table 1. Independent variable coding

Intervention Characteristics		
Data to Extract	**Qualitative Code**	**Quantitative Code**
Workforce/professional development (Staff training about trauma-related topics including ability to recognize the signs, symptoms, and effects of trauma)	Absent Present	0 1
Practice change (Examples include implementing screening students for exposure to childhood adversity and trauma symptoms, evidence-based student intervention provided directly or indirectly by the school [i.e., by school staff or by providers collaborating with the school staff], referring students to services in the community, or intentionality toward relationship with students)	Absent Present	0 1
Organizational-level change (Restorative discipline policies, protocols for parent communication, physical environment changes, modeling respectful relationships, or partnerships with community health agencies; Maynard, 2019) procedures to reduce risk for re-traumatization, written policies that include and support TIC principles, presence of a defined leadership position or job function specifically related to TIC (Hanson & Lang, 2016)	Absent Present	0 1
Multiple intervention elements (i.e., workforce, practice, organizational)	Included one Included two Included three	1 2 3
Dosage: Workforce/professional development (how much/how long professional development did they get?)	None Less than 8 hours 8-40 hours (include "full day) Unknown	0 1 2 3
Total length of time the intervention was implemented (includes consultation/coaching)	Up to one day 2 days – one week 8 days to 1 month 32 days – 6 months 181 days - 1 year Greater than 1 year Not Available	0 1 2 3 4 5 6

2.4.2 Methodological Variables

To assess how methodological features might influence outcomes, eight variables were coded dichotomously, and eight variables were coded categorically. The outcome sample code accounts for whether the outcomes were assessed for all members of the group (i.e., universal sample) or only for youth identified as having elevated risk, such as youth who experienced trauma or adversity (i.e., targeted/selected/indicated sample). Finally, the length of time between intervention and follow-up were coded categorically. Three different lengths for post-intervention data collection were coded: immediately, within one year, and more than one year.

Table 2. Methodological variable coding

Methodological Factors		
Data to Extract	**Qualitative Code**	**Quantitative Code**
Full statistical information Or Inferential statistics	Used Hedge's g with the *raw* mean differences and the pooled SD for the denominator (i.e., independent groups OR pre-post when we calculate by hand)	1
	Used the *standardized* mean difference approach (i.e., entered the results of a t-test, or entered raw mean diff + SE. This will usually be the case when we don't calculate hedge's g by hand for paired groups)	2
Study Design	Pre-Post	1
	Independent Groups	2
	Pre-Post Control	3
Between-subjects analysis	Within-subjects analysis	0
	Between-subjects analysis	1
Random assignment	Not randomly assigned to groups	0
	Random assignment to groups	1
Outcome sample	Universal sample (assessed outcomes for all members of the group)	0
	Targeted/selected/indicated (only assessed outcomes for youth identified as having elevated risk, such as youth who experienced trauma or adversity)	1
Length of follow-up	Absent	0
	Immediately	1
	Up to one year	2
	More than one year	3
	Unknown	4
Peer Reviewed	Absent	0
	Present	1

2.4.3 Dependent Variables

The dependent variables used in this meta-analysis will include student outcomes, teacher outcomes, and school climate outcomes (see Table 4). Student outcomes will include measures of externalizing behavior, internalizing symptoms, and academic functioning. Teacher outcomes will include teacher attitudes towards trauma-informed

care and teacher knowledge related to trauma-informed care. School climate will include measures of student-teacher relationships as well as staff relationships.

Table 3. Dependent variable coding

Dependent Variables		
Data to Extract	**Qualitative Code**	**Quantitative Code**
Youth Externalizing	School-level data for suspensions, detentions, and expulsions for aggressive or disruptive behavior Symptom measures for externalizing behavior disorders Teacher report or student self-report of aggressive or disruptive behaviors	Hedge's g
Youth Internalizing	Symptom measures for internalizing symptoms such as anxiety, depression, posttraumatic stress Teacher reported or youth self-reported stress Teacher reported or youth self-reported social withdrawal	Hedge's g
Youth Academic Functioning	Standardized test scores Psychoeducational achievement tests School grades Teacher perceptions of academic performance GPA	Hedge's g
Youth Health Behaviors	Physical health maintenance including sleep and exercise	Hedge's g
Youth Coping	Ability to cope with stress Ability to express and regulate emotion	Hedge's g
Youth Executive Functioning	Sustained attention task Response inhibition/self-control Working memory Problem solving Planning	Hedge's g
Other youth wellness Indicator	Self-esteem Ability to trust others Ability to cope with stress Physical health	Hedge's g
Youth Access to Services	Access to mental health care	Hedge's g

Table 3, cont.

	Dependent Variables	
	Qualitative Code	**Quantitative Code**
Staff Attitudes	Attitudes Related to Trauma Informed Care Teacher self-efficacy Teacher confidence Teacher readiness to implement trauma-sensitive practices Teacher attitudes toward students	Hedge's g
Staff Knowledge	Teacher knowledge of trauma symptoms Teacher knowledge of trauma prevalence Teacher knowledge of vicarious trauma Teacher knowledge of trauma-sensitive classroom management skills	Hedge's g
Staff Behavior	Teacher behavior management strategies Teacher classroom organization strategies	Hedge's g
Staff Wellbeing	Teacher professional quality of life Teacher stress/burnout/compassion fatigue Teacher attendance	Hedge's g
School Climate	Student-teacher relationships Staff-staff relationships Ability to give and receive feedback	Hedge's g

2.4.3.1 Student Outcomes.

Student outcomes were clumped into four categories: externalizing symptoms, internalizing symptoms, academic performance, and other wellness indicators. Externalizing behavior may include measures of different types of behavior problems including aggression, bullying, school suspensions, noncompliance, disruptive class behavior, and delinquent acts. Reports of this information may come from a variety of sources including teacher report, self-report, parent-report, or official school records. Student internalizing symptoms/distress will consist of measures of mental health issues related to emotional over-control or internalizing symptoms such as depression, anxiety, stress, or social withdrawal. These reports may also derive from various sources

including teacher report, self-report, or parent-report. Academic performance may include standardized test scores from state-mandated testing or from psychoeducational measures of achievement such as the Wechsler Individual Achievement Test, school grades, teacher perceptions of academic performance, or GPA. Reports of this information will derive from school records. Finally, other wellness indicators may include youth thoughts, behaviors, and health status that indicate wellbeing. Examples include self-esteem, coping skills, and physical health.

2.4.3.2 Staff Outcomes.

This category includes evaluations of school staff's attitudes and knowledge related to trauma-informed care which theoretically serve as mechanisms of change in trauma-sensitive schools interventions. Specifically, teacher attitudes related to trauma-informed care, teacher confidence or self-efficacy in their ability to implement trauma-sensitive practices, teacher readiness to implement trauma-sensitive interventions, and teacher attitudes toward students were included. This information was self-reported from staff. Teacher knowledge related to trauma-informed care will include measures of teacher knowledge of the prevalence of childhood trauma, the symptoms of childhood exposure to trauma, or knowledge related to the elements of trauma-sensitive schools. Once again, this information will derive from staff self-report. Finally measures of teachers' classroom behaviors including behavior management were included. These measures included self-report assessments as well as observations of teacher behavior.

2.4.3.3 Climate Outcomes.

This category includes measures of student-staff relations such as how well students and staff get along with one another and how connected students feel to the

school staff. This information was gathered through student or teacher self-report survey or through teacher observation. This category will additionally include measures of staff relations. Measures of staff relations may include levels of positivity in staff interactions, staff connectedness, and staff ability to give and receive appropriate feedback. Authors were contacted for more detail on these characteristics when they were not available in the article.

2.5 Risk of Bias Assessment

To check for evidence of publication bias, differences in effect sizes between published and gray literature were examined using the meta-regression procedure. Effect sizes were regressed onto the categorical moderator "peer review," in which 0 was absent and 1 was present. Random-effects modeling was used to determine if the model explained any of the variance in effect sizes, and alpha was set to .05. The goodness of fit test was used to determine whether publication bias accounted for all of the variance in effect sizes, and alpha was set to .1 Further, for each main analysis, risk of bias was evaluated using funnel plots and subsequently corrected using Duval and Tweedie's trim-and-fill procedure under the fixed effect model (Borenstein et al., 2009). Funnel plots plot outcomes based on effect size and sample size. The distribution of points should be shaped like a funnel in the absence of publication bias, given that studies with smaller sample sizes would be expected to show more variation in the magnitude of effect sizes. However, in the presence of publication bias, the distribution would be asymmetrical such that the lower right side of the plot would have a disproportionately high number of studies compared to the lower left portion of the plot. This asymmetry was examined

with Duval and Tweedie's trim-and-fill procedure which estimates the true center of the funnel by removing the asymmetric side of the funnel. The trimmed studies are then replaced around the center (Duval & Tweedie, 2000).

2.6 Data Analysis

Quantitative meta-synthesis of selected studies was performed using Comprehensive Meta-Analysis (v4) software. To account for presumed heterogeneity between and within studies, all analyses were modeled by way of random effects (Borenstein et al., 2009; Field & Gillett, 2010; Hunter & Schmidt, 2004).

2.6.1 Effect Size Computation and Combination

To examine the effect of the interventions on youth, teacher, and school climate outcomes, mean differences were examined using Hedge's *g*. Hedge's *g*, like Cohen's *d*, represents the standardized difference between means. However, given that Hedge's g uses *n*-1 opposed to *n* as in its calculation of pooled standard deviation, it is considered to be less biased than Cohen's *d*. Ideally, Hedge's g is calculated using pre-intervention and post-intervention (or treatment and control) sample size (n), mean (M), and standard deviation (SD). However, if studies did not report these data, effect sizes were estimated using inferential statistical information, including t-test statistics and *p* values.

Aligning with the second hypothesis that specific intervention characteristics including the levels at which the intervention was implemented, and dosage of the intervention will differentially impact outcomes, mixed-effect models were conducted to examine these potential moderators. However, in cases for which there were insufficient

studies to analyze moderators, tables were used to qualitatively describe study characteristics.

Various studies had complex data structures meaning that multiple outcome domains were assessed for the same group of participants, and within each outcome domain, multiple outcome measures were employed. For example, Judge (2018) assessed the impact of a trauma-informed intervention on youth satisfaction with life (other youth wellness indicator), youth academic self-sufficiency (youth academic functioning), coping skills frequency (youth coping), coping skills utility (youth coping), exercise frequency (youth health behavior), and sleep frequency (youth health behavior), and perceived stress (youth internalizing). Outcome measures falling in the same broad domain (e.g., sleep frequency and exercise frequency both fall in the youth health behavior domain) were averaged by Comprehensive Meta Analysis to produce an overall effect size for the broad domain in question. To compare the effect sizes between outcome domains would require that the sample in one broad outcome domain be independent from the sample in another broad outcome domain. This assumption was not met for 23 of the studies that evaluated student outcomes. To account for this dependency, a multi-pronged analysis strategy was used.

First, to compute the aggregate effect of trauma-informed interventions on students, a meta-analysis of all studies that measured student-level outcomes was conducted. Effect sizes within each study were combined prior to aggregating the results over studies. Second, separate meta-analyses were performed for each student outcome domain to determine the aggregate effect of trauma-informed interventions on each outcome. Third, effect sizes were compared across domains assuming independence of

outcomes. Though the samples are not in fact independent, assuming independence for the comparison of outcomes is a conservative approach to assessing between-group heterogeneity. This is because the formula for the variance of differences between effect sizes involves subtracting the correlated error from the sum of the variances of each outcome (Borenstein et al., 2009). Thus, the larger the correlation between outcomes, the smaller the variance of the effect estimate, and the smaller the correlation (i.e., the larger the degree of independence), the larger the variance of the estimated effect. Thus, the variance, confidence intervals of the estimated effect, and the significance of the estimated effect are larger when independence is assumed in comparing outcomes. The drawback of this method is that it decreases power which increases the probability of a Type II error.

2.6.2 Assessing Moderators

Thirteen potential categorical moderators were extracted from the included studies. By convention, 10 studies are the minimum sample per moderator characteristic required for analysis (Borenstein et al., 2009). Thus, categorical moderators were formally assessed within the overall student outcomes meta-analysis and the overall staff-level meta-analysis and not by broad outcome domain within these samples, to maximize statistical power for each analysis. Importantly, potential moderators were only explored when sufficient between-study heterogeneity was present as assessed by Q and I^2 (Borenstein et al., 2009).The moderating effect of covariates was assessed using mixed effects modeling in which fixed moderators were evaluated in the context of conditional random heterogeneity (Borenstein et al., 2010; Card, 2012). Mixed effects modeling is most appropriate when there is unexplained heterogeneity, when the goal of the analysis

is to draw conclusions that generalize to a larger population as opposed to drawing conclusions only about the studies included in the meta-analysis, and when there are extreme sample sizes or effect sizes (Card, 2012). Thus, mixed-effects modeling was most appropriate for moderation analysis.

When complex data structures were present (i.e., multiple outcome measures, outcome domains, or timepoints per study) effect sizes within studies were combined as appropriate. However, when the moderator in question was outcome domain, length of follow up, or study design (i.e., cross-sectional vs. longitudinal), effect sizes were assumed to be independent.

2.6.3 Power Analysis

A power analysis was conducted using the formula recommended by Valentine and colleagues (2010). Power analysis for meta-analyses of social-emotional interventions is not common given the number of estimates a researcher is required to make about the unique parameters involved in a random effects model. However, given the nascent nature of the extant literature on trauma-sensitive interventions in schools, even a preliminary power analysis is beneficial. Conservative assumptions were made. Assuming that a typical within-study sample size per group is 50 and that population effect size to detect is small to medium ($g = .25$), the number of studies required to yield sufficient statistical power under a random-effects model is 20 (assuming high heterogeneity).

CHAPTER III

RESULTS

3.1 Study Characteristics

The initial search returned 61,293 results which were reduced to 53,027 after duplicates were removed. Following title and abstract screening, 619 articles remained eligible for a full-text review, though full texts were unavailable for four studies. Of the 615 articles that were subjected to full-text review, 555 were excluded for the following reasons: published before 2001 (k = 1); the study was not published in English (k = 1); not an intervention study (k = 303); excluded setting (k = 28); the study did not identify itself as "trauma-informed" (k = 112); the intervention was solely the provision of school-based mental health services (k = 43); qualitative data only (k = 29); no comparison group (k = 13); insufficient data (k = 13); single participant case study (k = 4); wrong population (k = 4); published results available elsewhere (k = 2); data overlapped with another study (k = 1); ineligible outcome (k = 1). Three additional studies were identified through searching the references in the studies included for analysis and searching the references in systematic reviews of trauma-sensitive interventions in schools (see Figure 2). Through this process, a total of 63 studies met inclusion criteria and were selected for meta-analysis, and 393 effect sizes were extracted from these studies.

Of the included studies, several collected data from independent groups of participants based on factors such as school location, year the intervention was received, or dose of the intervention received. Thus, each subgroup was analyzed as an individual study with independent outcomes increasing the total number of studies to 80. Studies in which school staff provided data totaled 44, and studies in which students provided data totaled 36.

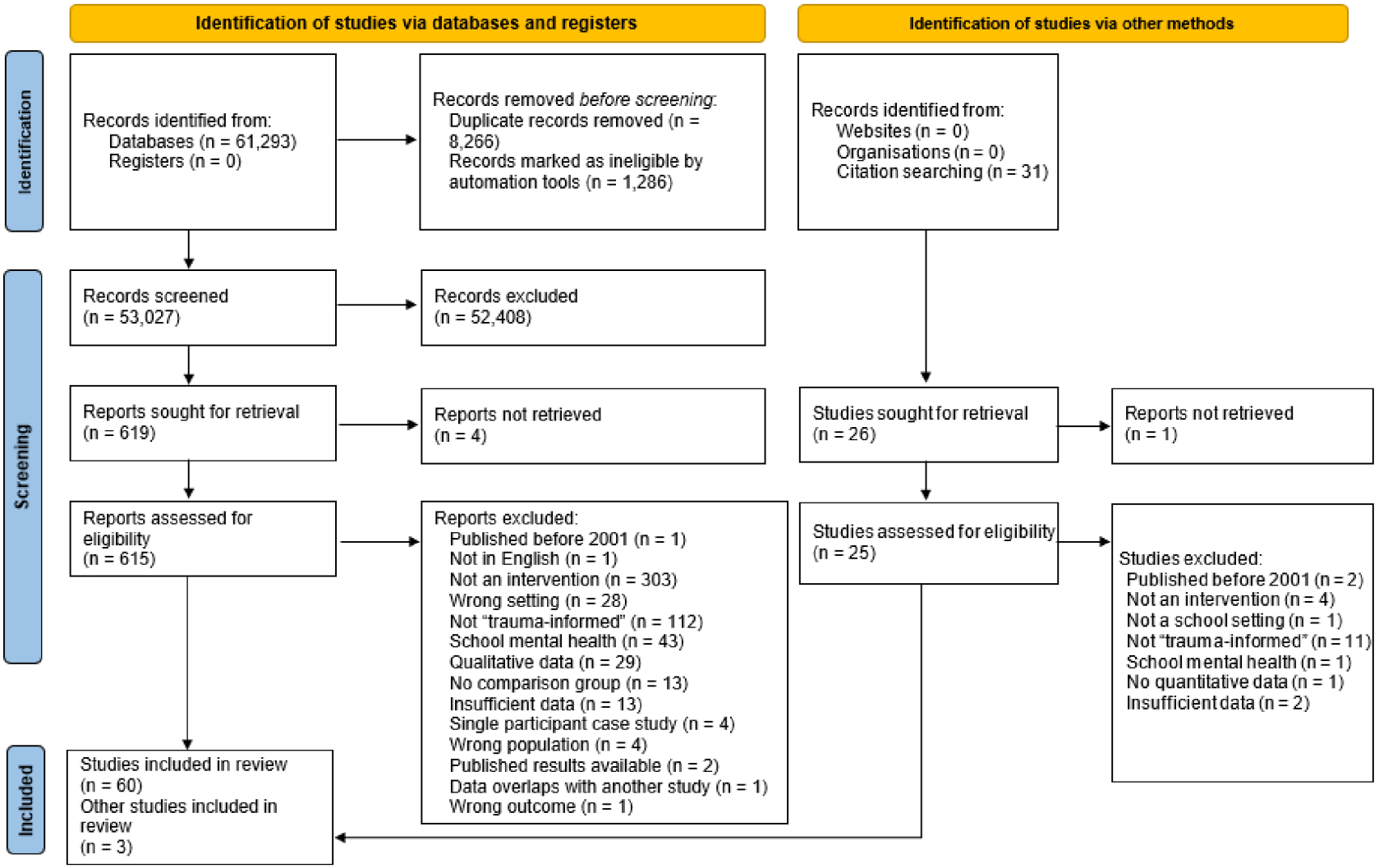


Figure 2. PRISMA flow diagram for systematic reviews

Studies that reported staff-related outcomes by broad outcome domain were as follows: staff attitudes (k = 11), staff behaviors (k = 2), staff knowledge (k = 2), and staff wellness (k = 3). An additional 23 studies provided effect sizes for multiple staff outcome domains, and three studies measured school climate as reported by school staff. Studies that reported student outcomes by broad outcome domains were as follows: youth academic functioning (k = 3), youth executive functioning (k = 3), youth externalizing (k =2), youth internalizing (k = 3), other youth wellness indicators (k = 3), and 22 studies assessed multiple outcome domains including those previously listed as well as school climate, access to services, coping, and health behaviors. Studies that yielded effect sizes for multiple domains are parsed out in the sections below.

Out of the 80 studies and 393 effect sizes, Hedge's g was estimated directly from the means and standard deviations of 58 studies (72.5%). Six studies (7.5%) contained some effect sizes that were directly estimated with full statistical information and some effect sizes that were estimated from inferential statistics. Finally, Hedge's g was estimated solely from inferential statistics for 16 studies (20%). A total of 300 extracted effect sizes (76.34%) were able to be directly estimated from means and standard deviations. When only partial statistical information was available, Hedge's g was estimated under the following alternative circumstances: chi-squared statistic and sample size (k = 3, 3 effect sizes); cohort 2x2 rates or events and sample size for intervention and control groups (k = 2, 5 effect sizes); difference between independent groups, sample size, and p-value (k = 1, 7 effect sizes); matched 2x2 events and sample sizes for pre- and post-intervention results (k = 4, 14 effect sizes); pre-test/post-test means sample sizes and paired groups p-value (k = 1, 3 effect sizes); paired t-statistic with pre and post means

and sample sizes (k = 8, 40 effect sizes); and raw difference in paired means, standard error, and sample size (k = 3, 21 effect sizes);

3.2 Aggregate School Staff Findings

Thirty-eight longitudinal studies measuring school staff outcomes, yielding 139 unique effect sizes, were represented in the primary longitudinal meta-analysis (i.e., changes in school staff professional functioning following trauma-informed schools interventions). Meta-analysis resulted in a significant, medium combined effect size (Hedge's g = 0.58, 95% CI = 0.43 to 0.73, $p < .001$). Specifically, school staff displayed significant gains following the implementation of trauma-informed interventions in their schools (see Figure 3).

Seven cross-sectional studies measuring staff outcomes, yielding 41 unique effect sizes, were represented in the primary cross-sectional meta-analysis (i.e., assessing effect of trauma-informed intervention on school staff outcomes compared to school staff who did not receive trauma-informed intervention). Meta-analysis resulted in a significant, small, combined effect size (Hedge's g = 0.24, 95% CI = 0.03 to 0.45, p = .03). Specifically, school staff who received trauma-sensitive interventions in school had better outcomes than school staff who did not receive trauma-sensitive interventions (see Figure 4).

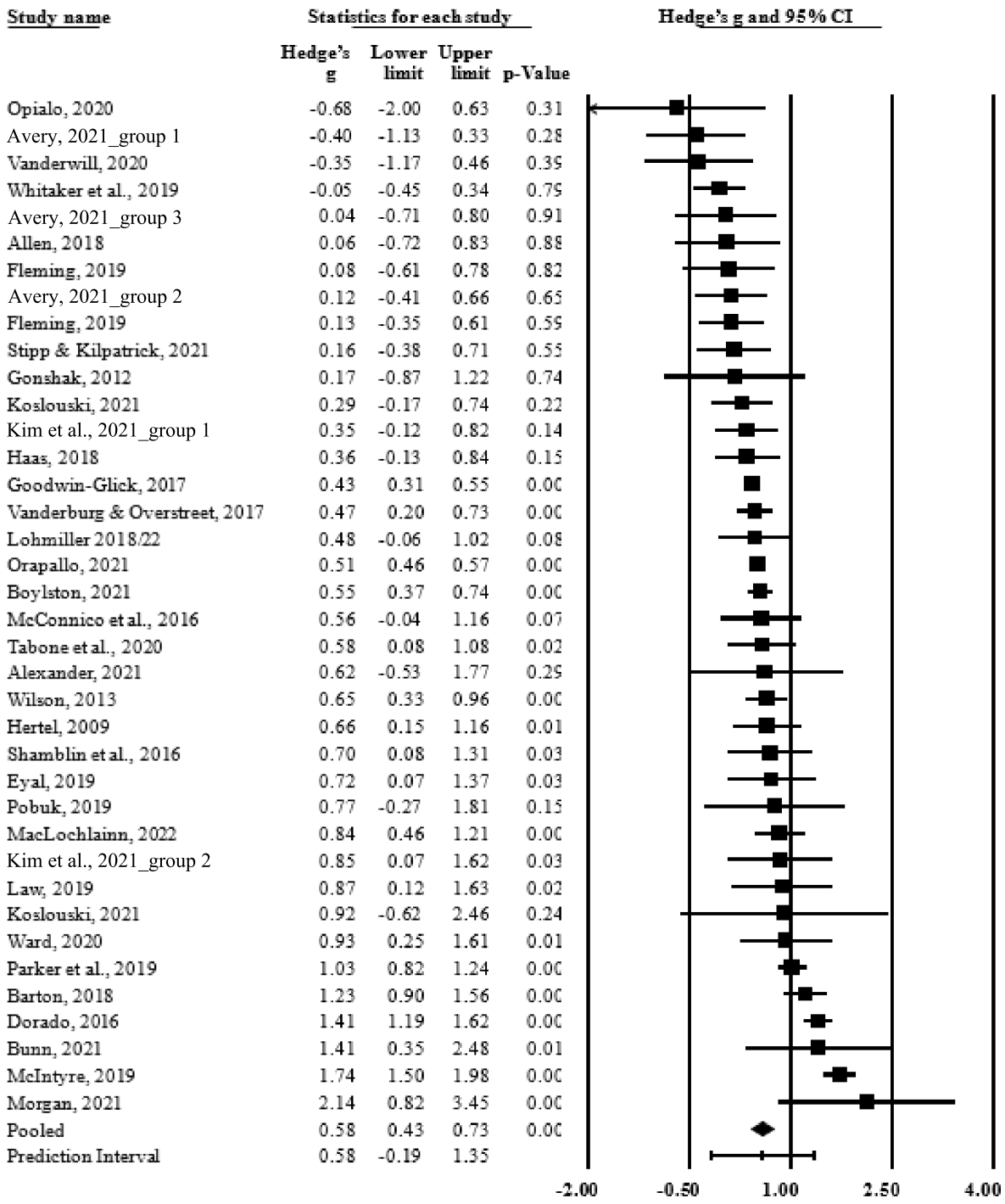


Figure 3. Forest plot for longitudinal studies that measured staff-related outcomes.

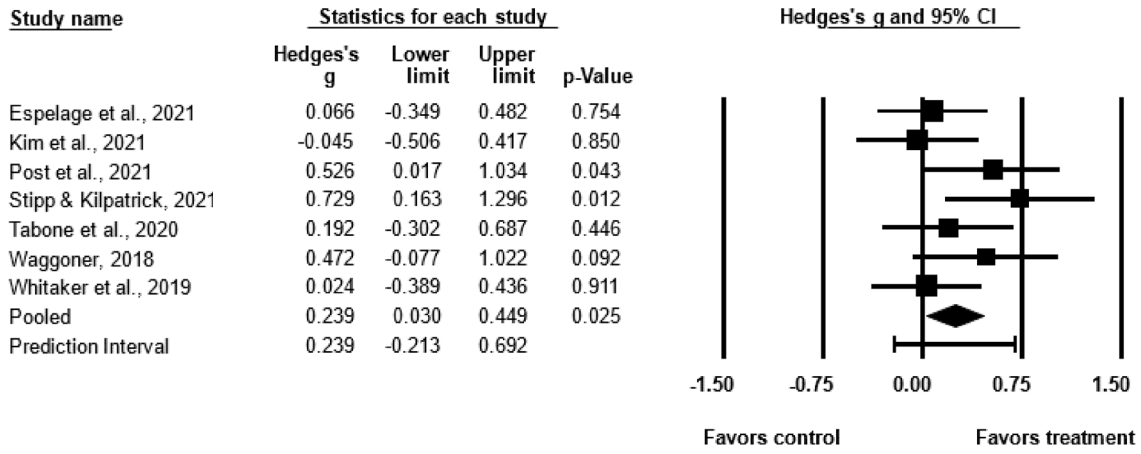


Figure 4. Forest plot of between-subjects effect sizes for school-staff outcomes.

Among longitudinal studies evaluating staff-related outcomes, there was considerable heterogeneity between studies (Q[37] = 245.55, p < .001, I^2 = 84.93), indicating that the true effect size of trauma-informed interventions varied across studies. Specifically, an I^2 value of 84.93 meant that 84.93% of the observed variance reflected variance in true effects, whereas only 15.07% of the observed variance was attributable to sampling error. Publication bias was assessed as a potential contributor to between-study heterogeneity. Meta-regression of mean effect size for student outcomes on peer review indicated that effect size did not vary as a function of peer review status (F[1, 36] = 0.04, p = .85) and the goodness of fit test (i.e., that the unexplained variance in the model is zero) was significant (Q[36] = 239.97, R^2 = 0.00, p < .001) indicating that there is unexplained variance in the moderation model after accounting for peer review status.

Visual inspection of the funnel plot for longitudinal studies of staff outcomes did not reveal any immediately apparent asymmetry in the lower half of the plot, nor did the Trim and Fill procedure identify any studies likely missing due to publication bias (see Figure 5). Thus, it may be concluded that publication bias did not impact the student-level outcomes results. However, visual inspection did reveal one study (Morgan, 2021) with a slightly larger effect size than expected. A sensitivity analysis was conducted to determine the impact of removing any one given study on the mean effect size across student outcomes. Results demonstrate that the mean effect size across studies would range from 0.55 – 0.60 (*p*s < .001) depending on which study were to be removed.

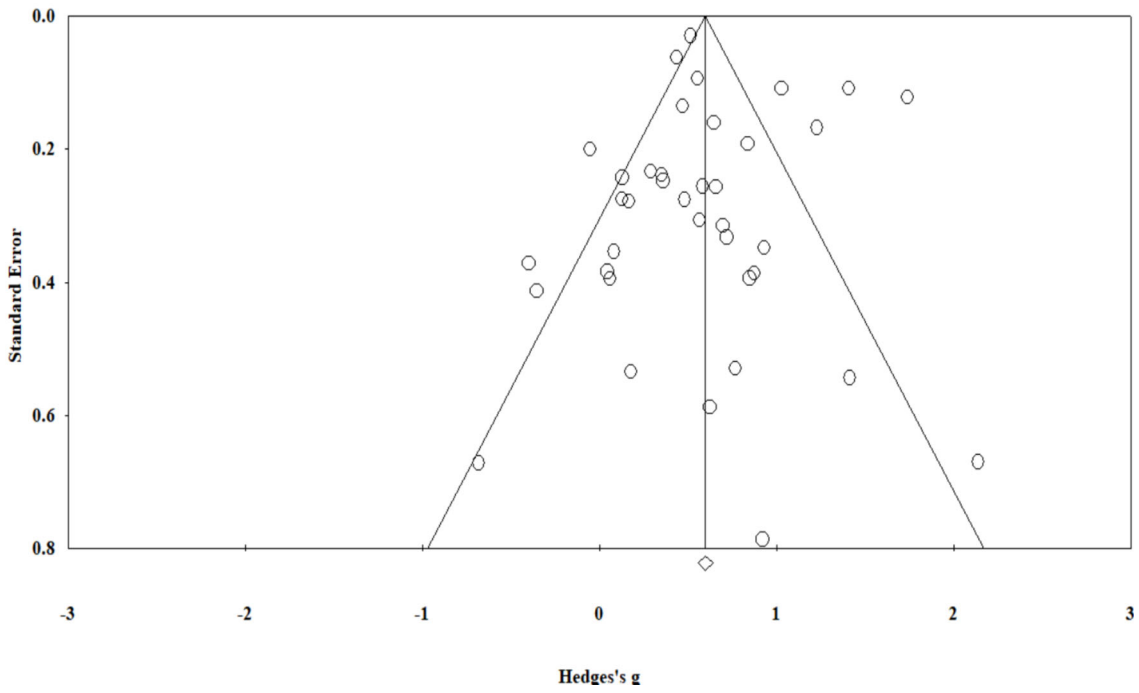


Figure 5. Funnel plot of standard error by Hedge's g for longitudinal studies measuring school staff outcomes

In contrast to longitudinal studies measuring changes in staff outcomes, there was a nonsignificant amount of heterogeneity between studies among cross-sectional studies ($Q[6] = 7.97$, $p = .24$, $I^2 = 24.73$), indicating that the true effect size of trauma-informed interventions was consistent across studies. Specifically, an I^2 value of 24.73 means that only 24.73% of the observed variance reflects variance in true effects, whereas 75.27% of the observed variance is attributable to sampling error. Given the small sample size of cross-sectional studies, model results would be expected to change if a study were removed. Results of a sensitivity analysis confirmed this. If Stipp & Kilpatrick, (2021), Post and colleagues (2021), or Waggoner (2018) were removed, the aggregate effect size would change to Hedge's $g = 0.17$, $p = .08$; Hedge's $g = 0.20$, $p = .08$; and Hedge's $g = 0.21$, $p = .07$ respectively. If any of the other four studies were removed, effect sizes would range from Hedge's $g = 0.25 – 0.29$ with all p-values falling below .05.

Though heterogeneity was low among cross sectional studies, publication bias was still assessed. Meta-regression of mean effect size for student outcomes on peer review was not indicated given only one study (Waggoner, 2018) was not peer reviewed. Thus, publication bias was assessed with a funnel plot. Visual inspection of the funnel plot revealed asymmetry in the lower half of the plot with four studies in the lower right quadrant and only one study in the lower left quadrant. Accordingly, Duval and Tweedie's (2000) Trim and Fill procedure identified two studies likely missing due to publication bias (see Figure 6). Thus, it is likely that publication bias was present in these analyses. Together, the small sample size of studies, the small effect size, and the presence of publication bias indicate that these results should be interpreted with caution.

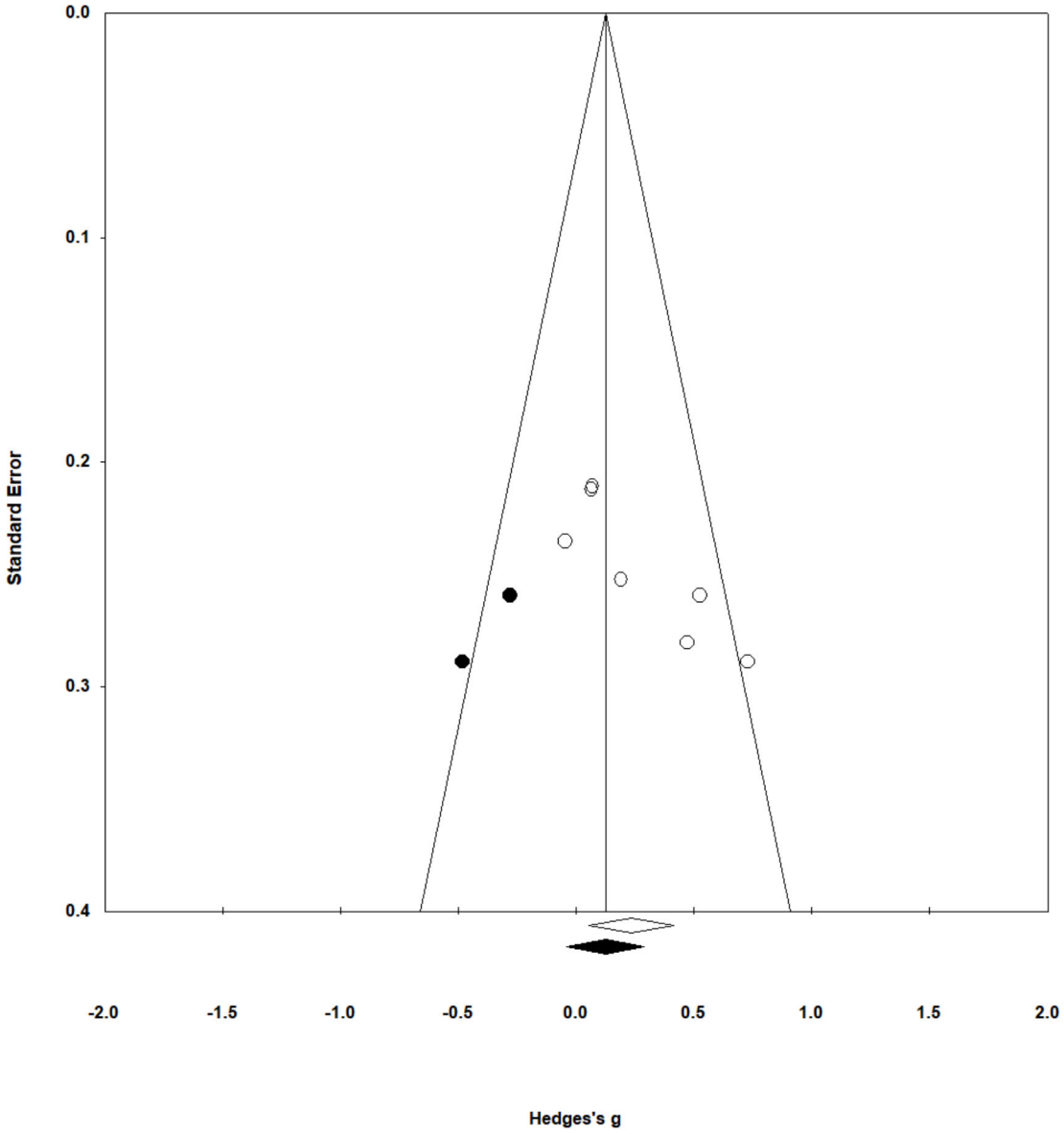


Figure 6. Funnel plot depicting observed effect sizes (empty circles) and imputed effect sizes (shaded circles) of cross-sectional studies evaluating school staff outcomes of trauma-sensitive interventions in schools.

Given the difference between staff outcomes reported in longitudinal studies (k = 38, Hedge's g = 0. 58, 95% CI = 0.43 to 0.73, $p < .001$) and cross-sectional studies (k = 7, Hedge's g = 0.24, 95% CI = 0.03 to 0.45, p = .03), a moderation analysis was conducted

to determine if this difference was statistically significant. Because four studies included both within-subjects and between-subjects data (i.e., (Kim et al., 2021; Stipp & Kilpatrick, 2021; Tabone et al., 2020; Whitaker et al., 2019), all outcomes and timepoints were analyzed under the assumption of independence. With the assumption of independence applied to outcome domain, outcome measure, and timepoint, the total *k* increased to 180. Moderation analysis was significant ($Q[1] = 8.89$, $p = .003$), indicating that studies utilizing longitudinal data collection methods yielded a significantly larger aggregate effect size than studies utilizing a cross-sectional, or between subjects, design. See Figure 7 for a visual representation of this analysis.

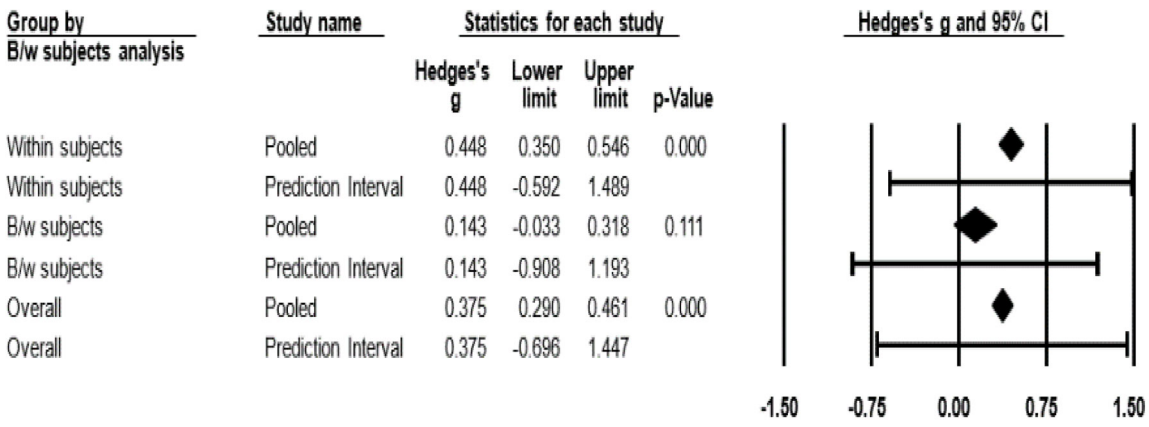


Figure 7. Visual representation of effect size differences between within-subjects outcomes and between-subjects staff outcomes.

3.2.1 Categorical Moderators of Aggregate Staff Outcomes

Given the significant heterogeneity in the meta-analysis of longitudinal staff outcomes (k = 38), further analyses were conducted to determine whether effect sizes differed across different levels of the proposed categorical moderators. Given the insignificant amount of heterogeneity present in cross-sectional studies in conjunction with the small sample size (k = 7), moderators were not formally examined, though they are qualitatively described in Table 4.

Table 4. Number of longitudinal studies (k) measuring school staff outcomes per category for all categorical moderators.

Categorical Moderator	Level of moderator and number of studies per level (k)					
	0	1	2	3	4	5
Categorical moderators pertaining to the study level[a]						
Workforce professional development						
Longitudinal	1	37	--	--	--	--
Cross-sectional	0	7	--	--	--	--
Practice change						
Longitudinal	12	26	--	--	--	--
Cross-sectional	4	3	--	--	--	--
Organizational level change						
Longitudinal	20	18	--	--	--	--
Cross-sectional	6	1	--	--	--	--
Number of trauma-informed elements						
Longitudinal	--	13	7	18	--	--
Cross-sectional	--	4	2	1	--	--
Dosage of workforce professional development						
Longitudinal	1	12	22	3	--	--
Cross-sectional	0	3	4	0	--	--
Length of intervention						
Longitudinal	2	5	17	9	4	1
Cross-sectional	1	0	3	3	0	0
Study design						
Longitudinal	--	31	--	7	--	--
Cross-sectional	--	--	2	5	--	--

Table 4, cont.

Categorical Moderator	Level of moderator and number of studies per level (*k*)					
	0	1	2	3	4	5
Random assignment						
Longitudinal	34	4	--	--	--	--
Cross-sectional	6	1	--	--	--	--
Outcome Sample						
Longitudinal	37	1	--	--	--	--
Cross-sectional	7	0				
Peer reviewed						
Longitudinal	21	17	--	--	--	--
Cross-sectional	1	6				
Estimation of Hedge's *g*[b]						
Longitudinal	--	30	7	--	--	--
Cross-sectional	--	--	6			
Categorical moderators pertaining to the outcome level						
Length of time between the end of intervention and administration of outcome measures[c]						
Longitudinal	--	106	33	--	--	--
Cross-sectional	--	6	1			

Outcome Domain[d]	Attitudes	Knowledge	Behavior	Well-being
Longitudinal	27	12	8	10
Cross-sectional	6	2	3	4

[a] Total longitudinal $k = 38$; total cross-sectional $k = 7$. Multiple outcomes and timepoints within studies are assumed to be dependent and combined.
[b] Total $k = k$-1 due to assumption of dependence of outcome domains. 1 study (Whitaker et al., 2019) is not represented in the table because some effect sizes were estimated from full statistical information and others were estimated with inferential statistics.
[c] Total $k = 139$ for longitudinal analyses due to assumption of independence of outcome domains, outcome measures, and timepoints within studies. $K = 7$ for cross-sectional studies because each study collected data at only one timepoint, thus timepoint of data collection could be analyzed at the study level (rather than the effect size level).
[d] Total $k = 57$ for longitudinal studies and $k = 15$ for cross-sectional studies due to assumption of independence of outcome domains. Outcome measures within each domain were combined as were timepoints within each study.

3.2.1.1 Outcome Domain Findings.

As described in the data analysis section, to compare staff outcome domains, independence of outcome domains was assumed. If a study collected multiple measures of any given outcome domain (e.g., the Attitudes Related to Trauma-informed Care scale and an author-generated measure of teacher attitudes towards students were both used to measure teacher/staff attitudes), outcome measures were combined into a single effect size to represent the broad outcome domain (e.g., teacher/staff attitudes). This strategy yielded 57 longitudinal studies and 15 cross-sectional studies and is a conservative approach of estimating differences between dependent outcomes. There were sufficient studies per outcome domain to conduct moderation analysis for longitudinal studies but not for cross-sectional studies. Of note, teacher behavior was only measured longitudinally by eight studies. While this is below the convention of 10, an exception was made based on a power analysis that indicated sufficient power to detect an effect given the effect size and average number of participants per study. Analysis revealed that effect sizes differed significantly across broad domains ($Q[3] = 15.94$, $p = .001$). See Figure 8 for a visual representation of this analysis.

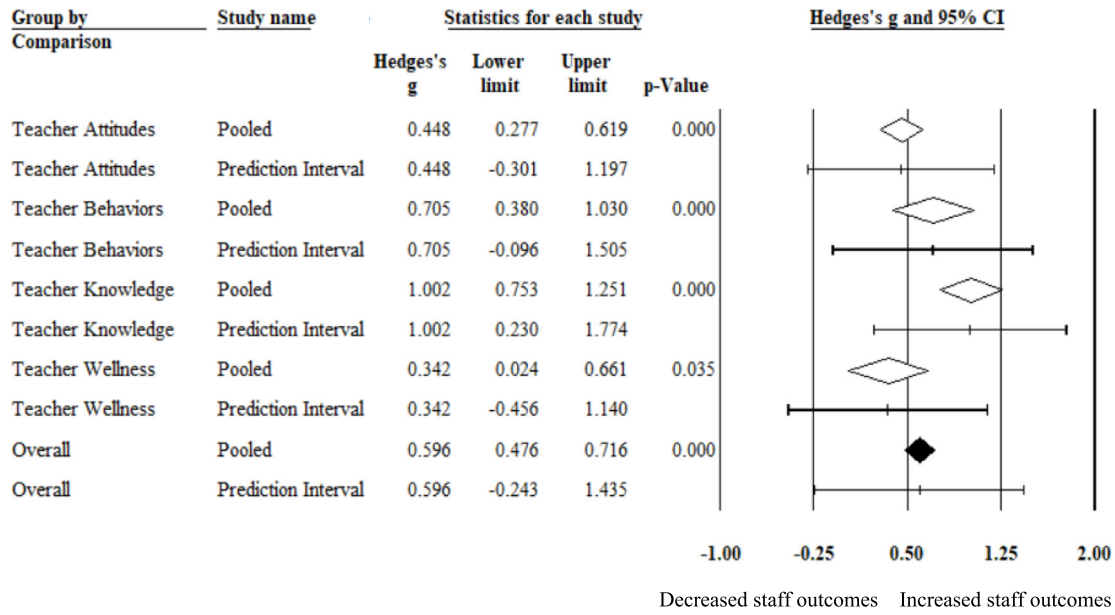


Figure 8. Subgroup analysis of studies across broad outcome domain aggregates.

According to pairwise comparisons, the aggregate effect size for teacher knowledge was significantly larger than teacher wellness and significantly larger than teacher attitudes (see Table 5 for full results). All other broad outcome aggregate effect sizes were comparable.

Table 5. Categorical moderators for broad domain outcomes.

Broad Domain	***Q***	***df***	***p***	Pairwise
Staff Knowledge vs. Staff Wellness	6.35	1	.01*	TK > TW
Staff Behaviors vs. Staff Wellness	1.69	1	.19	TB = TW
Staff Attitudes vs. Staff Wellness	0.44	1	.51	TA = TW
Staff Behaviors vs. Staff Knowledge	0.93	1	.34	TB = TK
Staff Attitudes vs. Staff Knowledge	15.34	1	<.001***	TA < TK
Staff Attitudes vs. Staff Behavior	2.24	1	.14	TA = TB

* $p < .05$
*** $p < .001$

3.2.1.2 Trauma-informed Intervention Elements.

Key elements, as identified by SAMHSA, of trauma-informed interventions include professional development, organizational change, and practice change. To test the hypothesis that interventions containing all three elements of trauma-informed interventions have significantly better outcomes than studies that do not contain these elements, two sets of analyses were conducted. First, a moderation analysis was conducted among longitudinal studies measuring staff outcomes to determine whether effect sizes significantly differed based on the number of trauma-informed elements present. Once again, there were insufficient studies to test this moderator among effect sizes yielded from between subjects (i.e., cross-sectional) analyses ($k = 7$). Because each trauma-informed element is dichotomously coded at the study rather than outcome level, effect sizes were able to be combined across outcome domains within studies. Thus, the total number of longitudinal studies included in these analyses is 38. Analysis revealed that effect sizes did not differ significantly across number of trauma-informed elements ($Q[2] = 0.30$, $p = .86$). See Figure 9 for a visual representation of this analysis.

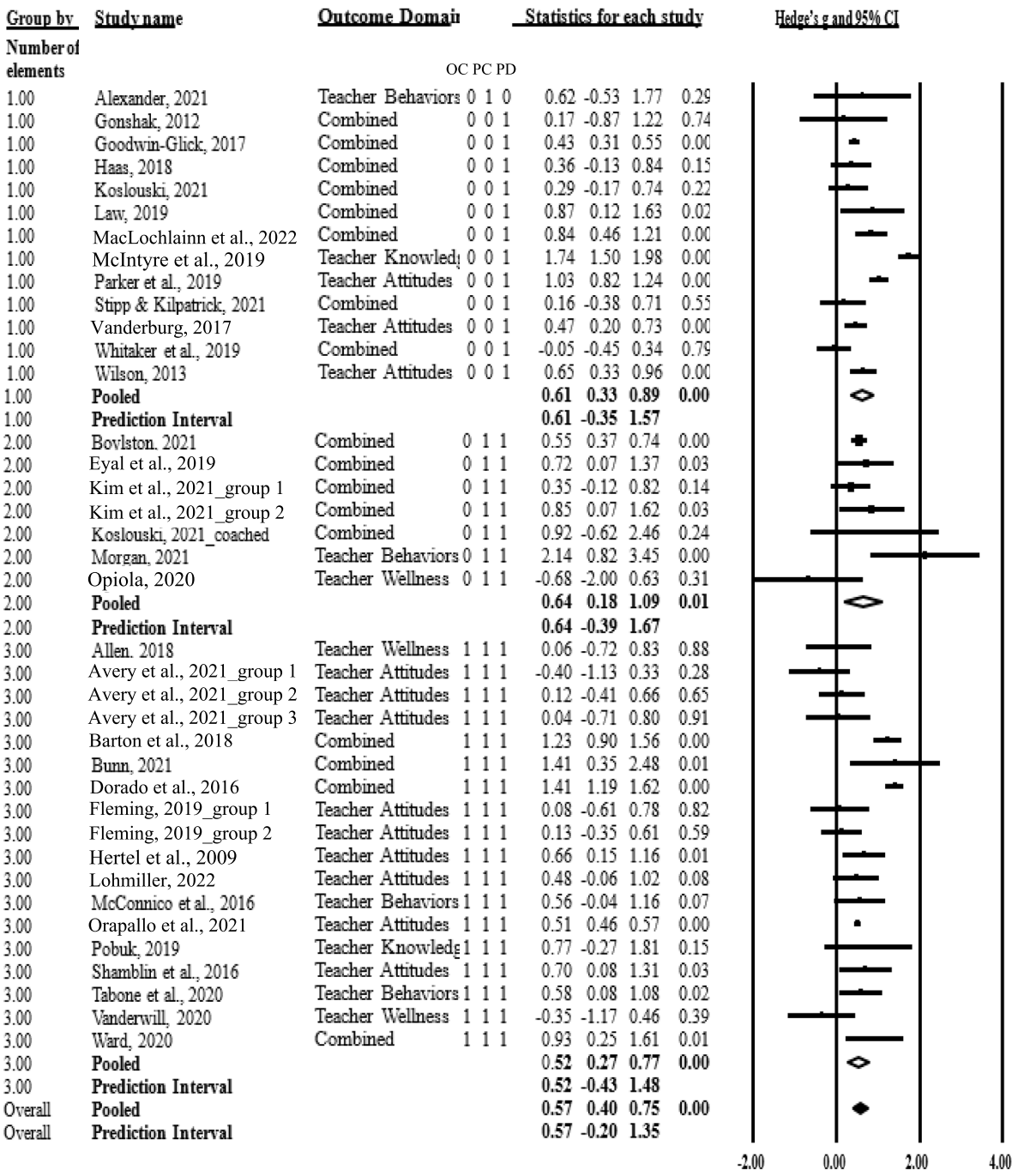


Figure 9. Subgroup analysis of studies across number of trauma-informed elements.
Note. An underscore and a descriptor following a study denotes multiple comparisons used within the study.

The second set of analyses determined whether effect sizes from longitudinal studies measuring changes in staff outcomes varied based on the presence or absence of each trauma-informed element. Thirty-seven studies included workforce professional development while 1 study did not. Thus, moderation analysis was not conducted for this element due to the small sample of studies without workforce professional development.

Effect sizes for 18 studies that implemented trauma-informed organizational change were compared to 20 studies that did not include organizational level change. Between-study heterogeneity was not significant ($Q[1] = .30$, $p = .58$), indicating that the effect sizes yielded by studies that did not include organizational change (Hege's $g = .62$, 95% CI = 0.39 - 0.85, $p < .001$) were equal to studies that did include organizational level change (Hedge's $g = .52$, 95% CI = 0.28 – 0.77, $p < .001$). Figure 10 visually depicts the effect sizes for school staff outcomes as a function of organizational change.

Group by OC	Study name	Statistics for each study				Hedge's g and 95% CI
		Hedge's g	Lower limit	Upper limit	p-Value	
0.00	Pooled	0.62	0.39	0.85	0.00	
0.00	Prediction Interval	0.62	-0.29	1.53		
1.00	Pooled	0.52	0.28	0.77	0.00	
1.00	Prediction Interval	0.52	-0.39	1.44		
Overall	Pooled	0.57	0.41	0.74	0.00	
Overall	Prediction Interval	0.57	-0.20	1.35		

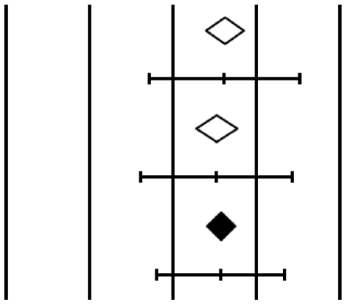


Figure 10. Subgroup analysis of studies across presence or absence of trauma-informed organizational-level change (OC).

Effect sizes for 26 studies that implemented trauma-informed practice change were compared to 12 studies that did not include practice change. Between-study heterogeneity was not significant ($Q[1] = .13$, $p = .72$), indicating that the effect sizes yielded by studies that did not include practice change (Hege's $g = .61$, 95% CI = 0.35 - 0.88, $p < .001$) were equal to studies that included practice change (Hedge's $g = .55$, 95% CI = 0.35 – 0.76, $p < .001$). Figure 11 visually depicts the effect sizes for school staff outcomes as a function of practice change.

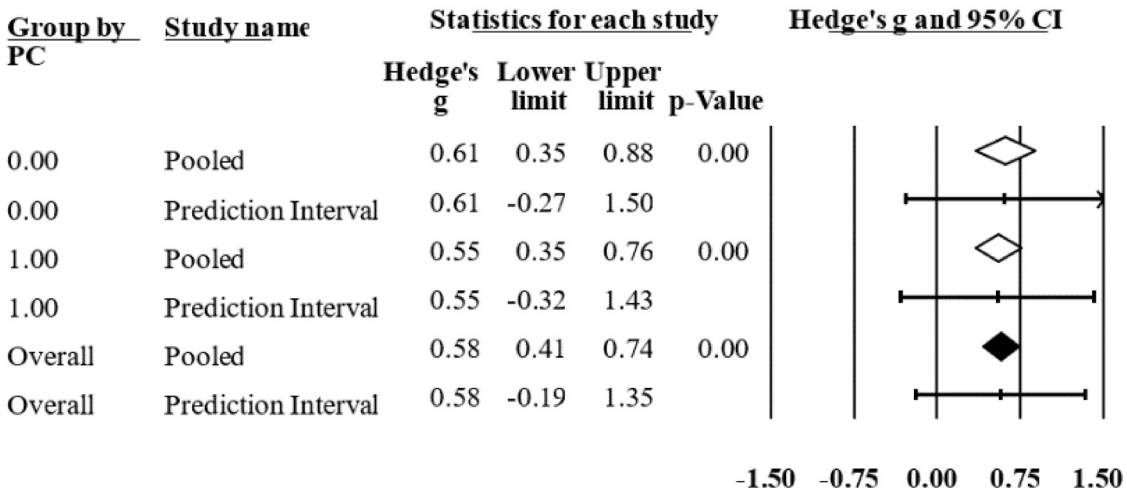


Figure 11. Subgroup analysis of longitudinal studies measuring school staff outcomes across levels of practice change (PC).

3.2.1.3 Dosage of professional development.

Thirty-seven of the 38 longitudinal studies measuring changes in staff outcomes included trauma-informed professional development. However, only 34 of these studies

reported the dose of professional development. Thus, subgroup analysis examining the combined effect size difference across dosage of professional development was conducted with these 34 studies. Once again, the sample of cross-sectional studies was too small to warrant moderation analysis ($k = 7$). Twelve longitudinal studies included less than eight hours of trauma-informed professional development for school staff (Hedge's $g = 0.57$, $CI = 0.30 – 0.84$, $p < .001$) while 22 longitudinal studies included between 8-40 hours of staff professional development (Hedge's $g = 0.56$, $CI = 0.39 – 0.73$, $p < .001$). These effect sizes were not significantly different ($Q[1] = 0.01$, $p = .94$). Figure 12 visually depicts the effect sizes of studies measuring changes in teacher outcomes following trauma-informed intervention grouped by dosage of professional development, where one signifies less than a full day (i.e., eight hours) of professional development was provided, and two signifies between eight to 40 hours of trauma-informed professional development was provided. Figure 12 additionally displays the number of trauma-informed elements per study and the total length of time the trauma-sensitive intervention occurred.

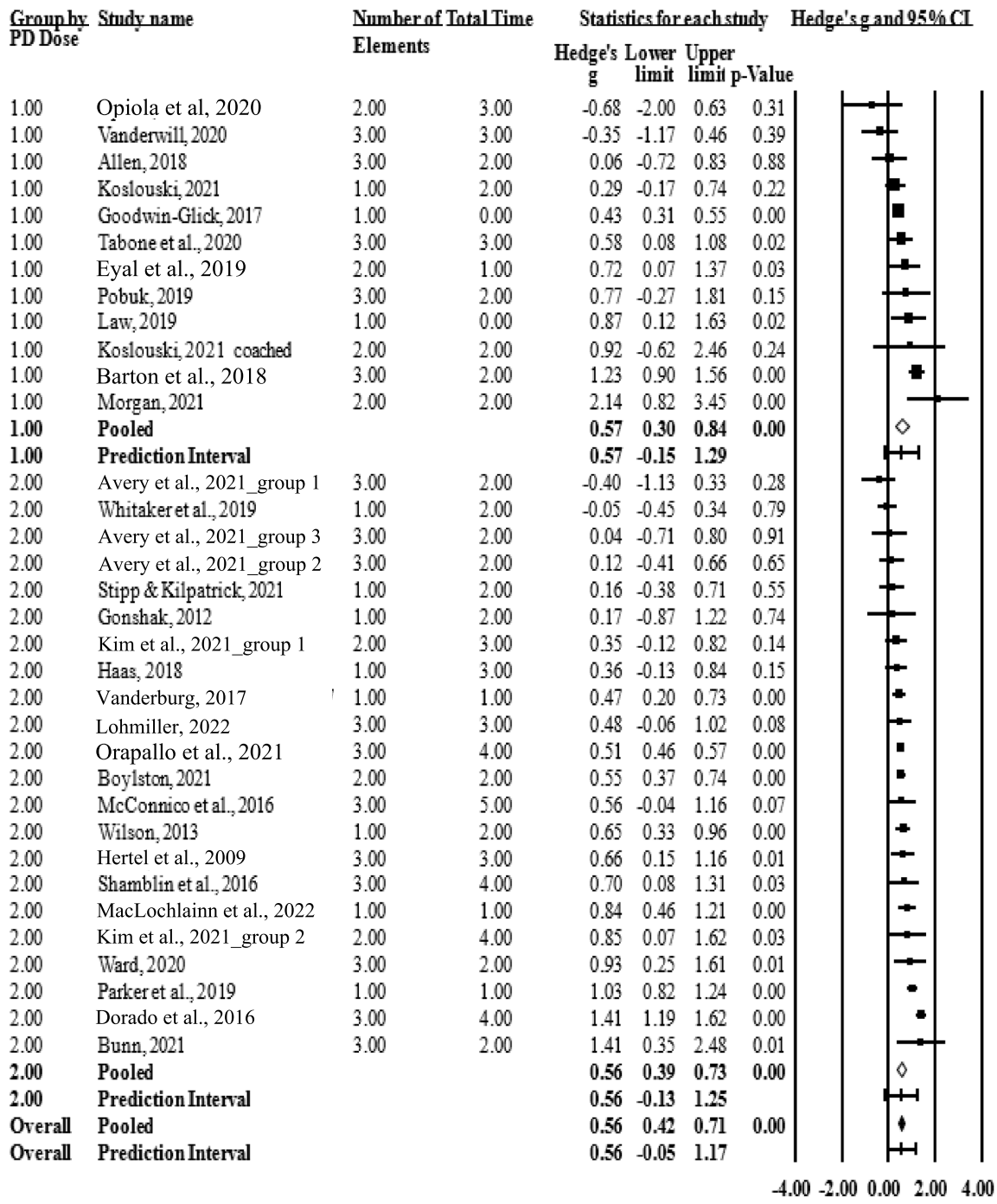


Figure 12. Subgroup analysis of longitudinal studies measuring teacher outcomes across dosage of trauma-informed professional development.
Note. An underscore and a descriptor following a study denotes multiple comparisons used within the study.

3.2.1.4 Length of Intervention.

In addition to the dosage of trauma-specific professional development, the dosage of the entire intervention (i.e., the length in days) was considered as a moderator as well. Length of intervention was estimated from all 38 longitudinal studies that measured changes in staff outcomes. However, there were insufficient studies to examine differences between interventions occurring for one week or less (k = 7) and interventions occurring for longer than six months (k = 5). Thus, the sample was restricted to interventions that lasted between eight to 30 days (k = 16) and interventions that lasted 32 days to 180 days (k = 9). The aggregate effect size estimated from studies lasting eight days to one month (Hedge's g = 0.48, CI = 0.26 – 0.71, p < .01) was not significantly different from the aggregate effect size estimated from studies that lasted 32 days to six months (Hedge's g = 0.28, CI = -0.01 – 0.58, p = .06; Q[1] = 1.13, p = .29). Figure 13 visually depicts the effect sizes of studies measuring changes in teacher outcomes following trauma-informed intervention grouped by dosage (i.e., length) of intervention.

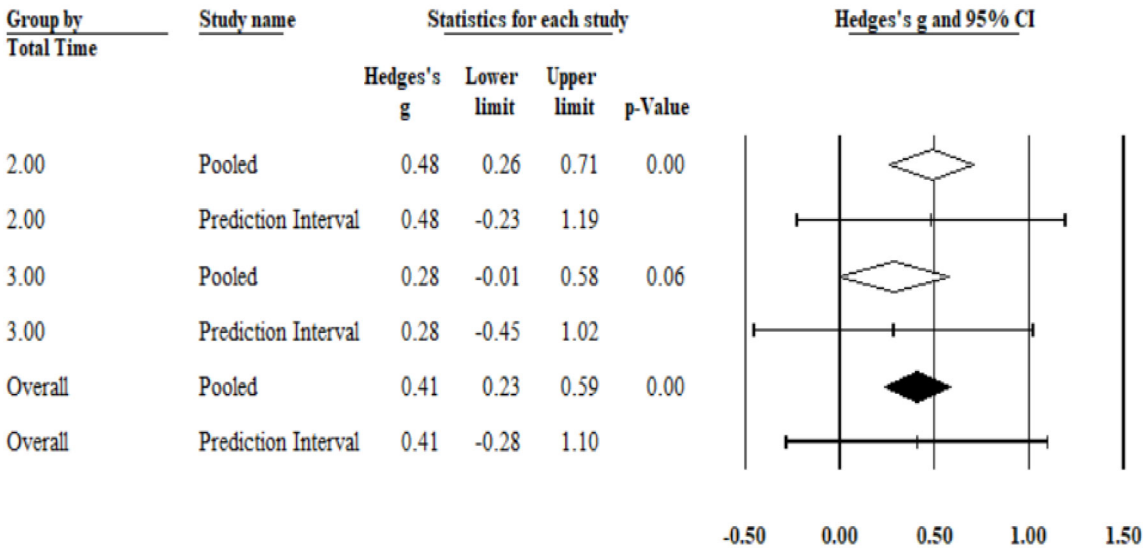


Figure 13. Subgroup analysis of longitudinal studies measuring school staff outcomes between interventions that lasted 8 – 30 days (2) and interventions that lasted 31 – 181 days (3).

3.2.1.5 Methodological Moderators.

Study design, random assignment, outcome sample, and peer review status were coded at the study level. Estimation of Hedge's *g* was coded at the outcome level; however, it was analyzed at the study level. This means that dependence of samples was assumed which resulted in the inability to parse out full versus partial estimation of effect sizes for one study (Whitaker et al., 2019). Some effect sizes in this study were estimated with full statistical information while others were estimated using inferential statistics. This approach was chosen because a Type 2 error for this analysis would lead to an anticonservative interpretation of the data. Thus, in this instance, assuming dependence of outcomes was the more conservative approach. Length of time between the end of intervention and administration of outcome measures was coded at the outcome level.

Accordingly, outcomes and time points were treated as independent samples, though they were derived from dependent samples.

There were sufficient longitudinal studies of changes in staff outcomes per category to conduct formal moderation analyses for the following methodological variables: study design, peer review status, and length of time between the end of intervention and administration of outcome measures. There were insufficient cross-sectional studies of changes in staff outcomes to formally examine methodological moderators ($k = 7$). Study design and peer review status were not significant methodological moderators of effect size. However, length of time between the end of intervention and administration of outcome measures was a significant moderator of effect size, such that outcome measures administered up to one year following intervention yielded a smaller aggregate effect size than measures administered immediately following the conclusion of the intervention. Table 6 provides full statistical information for each moderation analysis for methodological variables.

Table 6. Moderation analysis of methodological variables across longitudinal studies measuring the impact of school-based trauma-informed interventions on school staff outcomes.

Categorical moderator (*k*)	Hedge's g	*CI*	B/w group heterogeneity	Pairwise
Categorical moderators pertaining to the study level				
Study design (38)				
Pre-post (31)	0.61	0.44 – 0.78	$Q(1) = 0.53$ $p = .47$	Equal
Pre-post, control (7)	0.47	0.14 – 0.80		
Random assignment (38)				
Not randomly assigned to groups (34)	0.66	0.50 – 0.81	Insufficient *k*	Cannot be determined
Random assignment to groups (4)	--	--		
Outcome Sample (38)				
Universal sample (assessed outcomes for all members of the group; 37)	0.57	0.42 – 0.72		
Targeted/selected/indicated (assessed outcomes for youth identified as having elevated risk; 1)	--	--	Insufficient *k*	Cannot be determined
Peer reviewed (38)				
Not peer reviewed (21)	0.56	0.33 – 0.79	$Q(1) = 0.04$ $p = .84$	Equal
Peer reviewed (17)	0.59	0.36 – 0.83		
Estimation of Hedge's *g*[a] (38)				
Estimated with full information (30)	0.56	0.37 – 0.76		
Estimated with inferential statistics (7)	0.71	0.33 – 1.09	$Q(1) = 0.46$ $p = .5$	Equal[b]
Combination (1)	--	--		
Categorical moderators pertaining to the outcome level				
Timepoint of data collection[c] (139)				
Immediately (106)	0.52	0.40 – 0.63		
Up to one year (33)	0.22	0.01 – 0.42	$Q(1) = 6.20$ $p = .01$	Immediately > Up to one year
More than one year (0)	--	--		
Unknown (0)	--	--		

[a] Though the categorical code for Hedge's *g* was applied to the outcome level and not the study level, treating the outcomes as independent would have been an anti-conservative approach in this instance because the analysis would have been less likely to return a significant result. Thus, dependence was assumed.
[b] One study contained effect sizes estimated from full statistical information and effect sizes estimated from inferential statistics (Whitaker et al., 2019). This study was excluded from analyses to allow moderation analysis.
[c] Independence of outcomes and time-points was assumed to analyze differences in follow up times.

3.2.2 Staff Attitude Domain Findings

Twenty-seven studies, yielding 54 effect sizes, longitudinally evaluated changes in school staff attitudes following school-based trauma-informed interventions. Six cross-sectional studies yielding 11 effect sizes evaluated staff attitudes in trauma-informed intervention groups compared to control groups. These studies were assumed to be a random sample from a universe of potential studies, and this random-effects analysis was used to make an inference to that universe (Borenstein, 2019; Borenstein et al., 2010, 2021; Hedges & Vevea, 1998; Higgins et al., 2019). The mean effect size estimated from longitudinal studies is small (Hedge's g = .45, 95% CI = 0.31 to 0.58, p < .001) but significant, indicating that school staff's professional attitudes towards themselves and their students significantly improved following trauma-informed interventions were implemented in their school. Meta-analysis was not conducted for cross-sectional studies due to small sample size. Though there was insufficient power to formally analyze an aggregate effect size yielded from cross-sectional studies, effect sizes ranged from Hedge's g = -0.11 - 0.47 and all p-values were greater than p = .05

There is considerable heterogeneity between longitudinal studies as indicated by a significant Q-statistic (Q[26] = 114.86, p < .001, I^2 = 77.36). Specifically, an I^2 value of 77.36 means that 77.36 % of the observed variance reflects variance in true effects, while only 10.57% of the observed variance is attributable to sampling error. Thus, moderation analyses were conducted to determine if any of the unexplained variance could be accounted for by methodological variables or intervention characteristics. There were insufficient studies to examine all moderators (Table 7), but there were sufficient studies

to examine the impact of practice change, organizational-level change, and peer review status, all of which had insignificant amounts of between-study heterogeneity.

Table 7. Moderation analysis across longitudinal studies measuring the impact of school-based trauma-informed interventions on school staff attitudes.

Categorical moderator (*k*)	Hedge's g	*CI*	B/w study heterogeneity	Pairwise
Categorical moderators pertaining to the study level[a]				
Workforce PD (27)				
Included (27)	--	--	Insufficient *k*	Cannot be determined
Not included (0)	--	--		
Practice change (27)				
Included (17)	0.41	0.21 – 0.62	$Q(1) = 0.26$	All equal
Not included (10)	0.49	0.26 – 0.73	$p = .61$	
Organizational level change (27)				
Included (12)	0.41	0.17 – 0.65	$Q(1) = 0.16$	All equal
Not included (15)	0.47	0.27 – 0.67	$p = .69$	
Number of trauma-informed components (27)				
One element (10)	0.49	0.26 – 0.73		
Two elements (5)	--	--	$Q(1) = 0.25$	1 element = 3 elements
Three elements (12)	0.41	0.17 – 0.65	$p = .62$	
Dosage of workforce professional development (27)				
Less than one day (6)				
One day to one week (19)	0.44	0.26 – 0.61	Insufficient *k*	Cannot be determined
Unknown length (2)				
Length of intervention (27)				
One day (2)	--	--		
2 days – 1 month (4)	--	--		
32 days – 6 months (13)	0.31	0.10 – 0.53	Insufficient *k*	Cannot be determined
181 days - 1 year (5)	--	--		
Greater than 1 year (3)	--	--		
Study design (27)				
Pre-post (21)	0.46	0.30 – 0.62	Insufficient *k*	Cannot be determined
Pre-post, control (6)	--	--		
Random assignment (27)				
Not randomly assigned to groups (23)	0.52	0.37 – 0.66	Insufficient *k*	Cannot be determined
Random assignment to groups (4)	--	--		
Outcome Sample (27)				

Table 7, cont.

Categorical moderator (*k*)	Hedge's g	*CI*	B/w study heterogeneity	Pairwise
Universal sample (assessed outcomes for all members of the group; 27)	0.45	0.31 – 0.58		
Targeted/selected/indicated (only assessed outcomes for youth identified as having elevated risk, such as youth who experienced trauma or adversity; 0)	--	--	Insufficient *k*	Cannot be determined
Peer reviewed (27)				
Not peer reviewed (i.e., gray literature; 14)	0.44	0.25 – 0.64	$Q(1) = 0.003$ $p = .96$	All equal
Peer reviewed (i.e., published in a peer reviewed journal; 13)	0.45	0.25 – 0.65		
Categorical moderators pertaining to the outcome level				
Estimation of Hedge's *g* (27)				
Estimated with full information (23)	0.42	0.25 – 0.59	Insufficient *k*	Cannot be determined
Estimated with inferential statistics (4)	--	--		
Length of time between the end of intervention and administration of outcome measures[b] (54)				
Immediately (47)	0.40	0.29 – 0.52		
Up to one year (7)	--	--	Insufficient *k*	Cannot be determined
More than one year (0)	--	--		
Unknown (0)	--	--		

[a] Total $k = 27$. Multiple outcomes and timepoints within studies are assumed to be dependent and combined.
[b] Total $k = 54$ due to assumption of independence of outcome domains, outcome measures, and timepoints within studies

3.2.3 Staff Knowledge Domain Findings

Twelve studies, yielding 22 effect sizes, longitudinally evaluated the impact of school-based trauma-informed interventions on school staff knowledge related to childhood trauma and trauma-informed care. Only two studies (Espelage et al., 2021; Stipp & Kilpatrick, 2021) evaluated the impact of trauma-informed schools interventions on staff knowledge cross-sectionally (Hedge's $g = .64$, $p = .003$ and Hedge's $g = 1.46$, p

< .001 respectively); thus, meta-analysis of cross-sectional studies was not conducted due to low power. These studies were assumed to be a random sample from a universe of potential studies, and this random-effects analysis will be used to make an inference to that universe (Borenstein, 2019; Borenstein et al., 2010; Borenstein et al., 2021; Hedges & Vevea, 1998; Higgins et al., 2019). The estimated mean effect size is large (Hedge's g = 1.06, 95% CI = 0.98 to 1.14, $p < .001$] and significant, indicating that school staff significantly increased their trauma-related knowledge following participation in trauma-informed interventions. There is considerable heterogeneity between studies in this meta-analysis as indicated by a significant Q-statistic (Q[11] = 113.08, $p < .001$, $I^2 = 90.27$). Specifically, an I^2 value of 90.27 means that 90.27 % of the observed variance reflects variance in true effects, while only 9.73% of the observed variance is attributable to sampling error. Given the small sample size, formal moderation analysis was not possible. Number of studies (k) per moderator are described in Table 8 below.

Table 8. Number of studies (k) measuring gains in school staff's trauma-informed knowledge by level of intervention characteristic and methodological variables.

Categorical Moderator	Level of moderator and number of studies per level (*k*)					
	0	1	2	3	4	5
Categorical moderators pertaining to the study level[a]						
Workforce professional development						
Longitudinal	0	12	--	--	--	--
Cross-sectional	0	2	--	--	--	--
Practice change						
Longitudinal	6	6	--	--	--	--
Cross-sectional	2	0	--	--	--	--
Organizational level change						
Longitudinal	8	4	--	--	--	--
Cross-sectional	2	0	--	--	--	--
Number of trauma-informed elements						
Longitudinal	--	6	2	4	--	--
Cross-sectional	--	1	0	0	--	--
Dosage of workforce professional development						
Longitudinal	--	5	6	1	--	--
Cross-sectional	--	1	1	--	--	--
Length of intervention						
Longitudinal	2	2	6	1	1	
Cross-sectional	1	--	1	--	--	--
Study design						
Longitudinal	--	11	--	3	--	--
Cross-sectional	--	--	1	1	--	--
Random assignment						
Longitudinal	12	0	--	--	--	--
Cross-sectional	2	0	--	--	--	--
Outcome Sample						
Longitudinal	12	0	--	--	--	--
Cross-sectional	2	0	--	--	--	--
Peer reviewed						
Longitudinal	8	4	--	--	--	--
Cross-sectional	0	2	--	--	--	--
Estimation of Hedge's *g*						
Longitudinal	--	10	2	--	--	--
Cross-sectional	--	0	2	--	--	--
Categorical moderators pertaining to the outcome level						
Length of time between the end of intervention and administration of outcome measures						
Longitudinal	--	11	1	--	--	--
Cross-sectional	--	2	0	--	--	--

[a] Total longitudinal $k = 12$; total cross-sectional $k = 2$. Multiple outcomes and timepoints within studies are assumed to be dependent and combined.

3.2.4 Staff Behavior Domain Findings

Eight studies, yielding 12 effect sizes, longitudinally evaluated the impact of school-based trauma-informed interventions on school staff behavior. Three studies, yielding five effect sizes, cross-sectionally evaluated the impact of trauma-sensitive interventions in schools on staff behavior. Meta-analysis was not indicated for cross-sectional studies due to lack of power. Though there was insufficient power to formally analyze an aggregate effect size yielded from cross-sectional studies, effect sizes ranged from Hedge's g = -0.33 – 1.09 and *p*-values ranged from $p < .001$ - .45. Studies were assumed to be a random sample from a universe of potential studies, and this random-effects analysis will be used to make an inference to that universe (Borenstein, 2019; Borenstein et al., 2010; Borenstein et al., 2021; Hedges & Vevea, 1998; Higgins et al., 2019). The mean effect size estimated from longitudinal studies is medium (Hedge's g = 0.72, 95% CI = 0.31 to 1.14, p = .001) and significant, indicating that school staff significantly improved their trauma-informed behavior at work following participation in trauma-informed interventions (Figure 14).

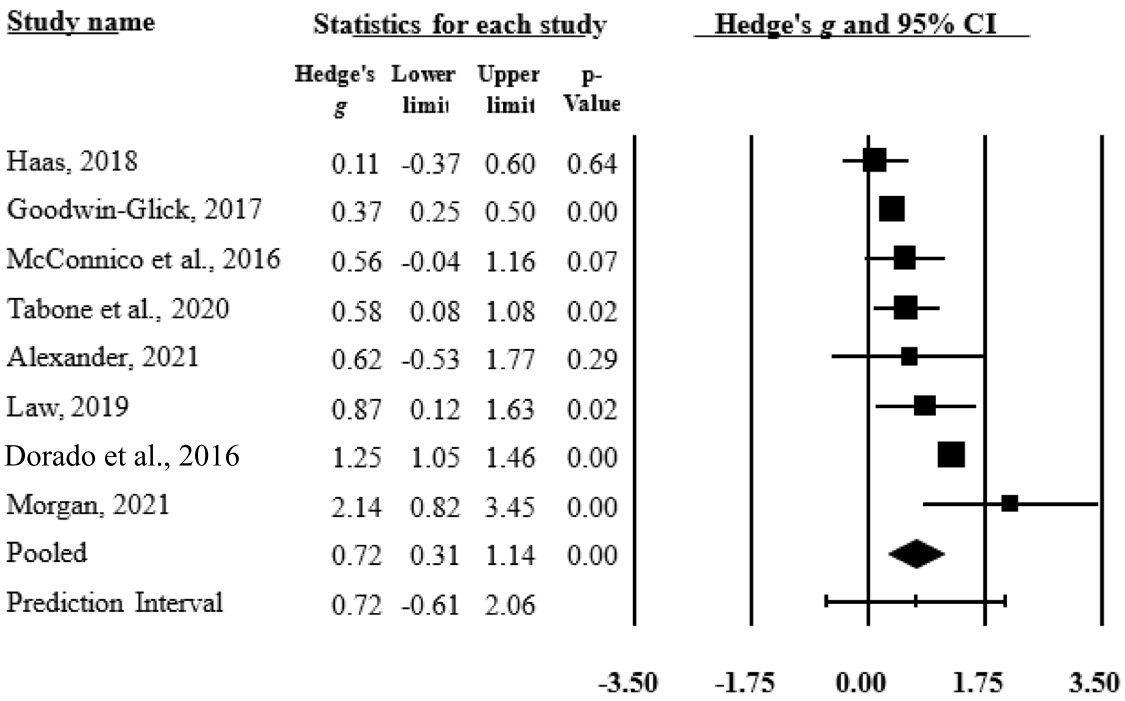


Figure 14 Forest plot of effect sizes estimated from longitudinal analyses of changes in staff trauma-informed behavior following trauma-informed interventions in their schools.

There is considerable heterogeneity between studies in this meta-analysis as indicated by a significant *Q*-statistic ($Q[7] = 62.22$, $p < .001$, $I^2 = 88.75$). Specifically, an I^2 value of 88.75 means that 88.75% of the observed variance reflects variance in true effects, while only 11.25% of the observed variance is attributable to sampling error. Given the small sample size, formal moderation analysis was not possible. However, the number of studies (*k*) per moderator are described in Table 9 below.

Table 9. Number of studies (k) measuring gains in school staff's trauma-informed behaviors by level of intervention characteristic and methodological variables.

Categorical Moderator	Level of moderator and number of studies per level (*k*)					
	0	1	2	3	4	5
Categorical moderators pertaining to the study level[a]						
Workforce professional development						
Longitudinal	1	7	--	--	--	--
Cross-sectional	0	3	--	--	--	--
Practice change						
Longitudinal	3	5	--	--	--	--
Cross-sectional	1	2	--	--	--	--
Organizational level change						
Longitudinal	5	3	--	--	--	--
Cross-sectional	2	1	--	--	--	--
Number of trauma-informed elements						
Longitudinal	--	4	1	3	--	--
Cross-sectional	--	1	1	1	--	--
Dosage of workforce professional development						
Longitudinal	1	4	3	0	--	--
Cross-sectional	--	2	1	0	--	--
Length of intervention						
Longitudinal	2	0	2	2	1	1
Cross-sectional	1	0	1	1	0	--
Study design						
Longitudinal	--	7	--	1	--	--
Cross-sectional	--	--	1	2	--	--
Random assignment						
Longitudinal	8	0	--	--	--	--
Cross-sectional	3	0	--	--	--	--
Outcome Sample						
Longitudinal	7	1	--	--	--	--
Cross-sectional	3	0	--	--	--	--
Peer reviewed						
Longitudinal	6	2	--	--	--	--
Cross-sectional	0	3	--	--	--	--
Estimation of Hedge's *g*						
Longitudinal	--	6	2	--	--	--
Cross-sectional	--	3	0	--	--	--
Length of time between the end of intervention and administration of outcome measures						
Longitudinal	--	7	1	--	--	--
Cross-sectional	--	3	0	--	--	--

[a] Total longitudinal $k = 12$; total cross-sectional $k = 2$. Multiple outcomes and timepoints within studies are assumed to be dependent and combined.

[b]Goodwin-Glick, 2017; Haas, 2018; and Law, 2019 consisted of work force professional development only. The surveys in Goodwin-Glick, 2017 and Law, 2019 were administered the day after receiving the training and immediately after the training respectively. Thus, staff behaviors were self-reported *intentions* to engage in behavior. At the time of data-collection, no trauma-sensitive practices had yet been implemented, so practice change was coded as "0" Haas, 2018 contained four professional development sessions over the course of the school year. While classroom behavior management was discussed, it was not required. The workshops were informational only.

3.2.5 School Staff Wellbeing Domain Findings

Ten studies, yielding 51 effect sizes, longitudinally evaluated the impact of school-based trauma-informed interventions on school staff wellbeing. Four studies, yielding 21 effect sizes, cross-sectionally evaluated the impact of trauma-sensitive interventions in schools on staff wellness. Meta-analysis was not indicated for cross-sectional studies due to lack of power. Though there was insufficient power to formally analyze an aggregate effect size yielded from cross-sectional studies, effect sizes ranged from Hedge's g = -0.19 - 0.12 and all *p*-values were greater than $p = .05$ These studies were assumed to be a random sample from a universe of potential studies, and this random-effects analysis was used to make an inference to that universe (Borenstein, 2019; Borenstein et al., 2010; Borenstein et al., 2021; Hedges & Vevea, 1998; Higgins et al., 2019). The estimated mean effect size is small (Hedge's $g = 0.35$, 95% CI = -0.04 to 0.73, $p = .08$) and nonsignificant (see Figure 15). Given the small sample size, formal moderation analysis was not possible. However, the number of studies (*k*) per moderator are described in Table 10 below. There is significant heterogeneity between studies in this meta-analysis as indicated by a significant *Q*-statistic ($Q[9] = 31.92$, $p < .001$, $I^2 = 71.81$). Specifically, an I^2 value of 71.81 means that 71.81% of the observed variance reflects variance in true effects, while only 28.19% of the observed variance is attributable to sampling error.

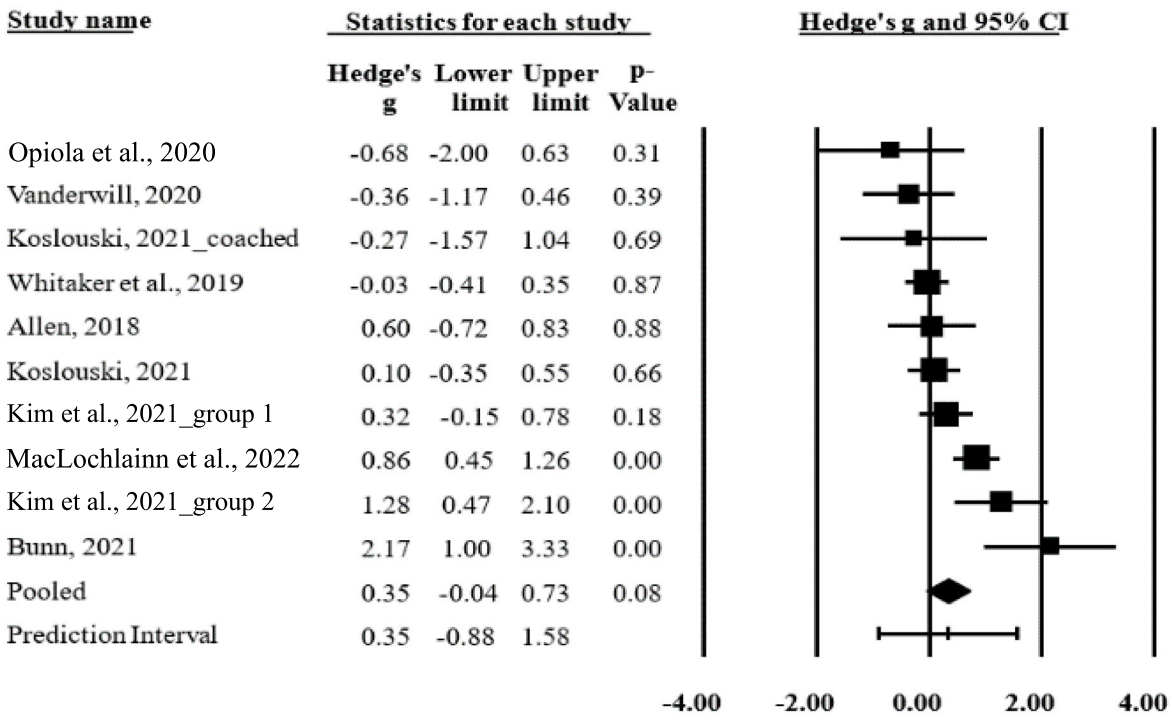


Figure 15. Forest plot of aggregate effect sizes estimated from longitudinal studies measuring changes in staff wellness.
Note. An underscore and a descriptor following a study denotes multiple comparisons used within the study.

Table 10. Number of studies (k) longitudinally measuring gains in school staff's wellness by level of intervention characteristic and methodological variables.

Categorical Moderator number of studies per level (*k*)	Level of moderator and					
	0	1	2	3	4	5
Categorical moderators pertaining to the study level[a]						
Workforce professional development						
Longitudinal	0	10	--	--	--	--
Cross-sectional	0	4	--	--	--	--
Practice change						
Longitudinal	3	7	--	--	--	--
Cross-sectional	2	2	--	--	--	--
Organizational level change						
Longitudinal	7	3	--	--	--	--
Cross-sectional	4	0	--	--	--	--
Number of trauma-informed elements						
Longitudinal	--	3	4	3	--	--
Cross-sectional	--	2	2	0	--	--
Dosage of workforce professional development						
Longitudinal	--	5	5	0	--	--
Cross-sectional	--	1	3	0	--	--
Length of intervention						
Longitudinal	0	1	5	3	1	0
Cross-sectional	1	0	2	1	--	--
Study design						
Longitudinal	--	6	--	4	--	--
Cross-sectional	--	--	1	3	--	--
Random assignment						
Longitudinal	9	1	--	--	--	--
Cross-sectional	3	1	--	--	--	--
Outcome Sample						
Longitudinal	10	0	--	--	--	--
Cross-sectional	4	0	--	--	--	--
Peer reviewed						
Longitudinal	5	5	--	--	--	--
Cross-sectional	0	4	--	--	--	--
Estimation of Hedge's *g*[b]						
Longitudinal	--	8	1	--	--	--
Cross-sectional	--	3	0	--	--	--
Categorical moderators pertaining to the outcome level						
Length of time between the end of intervention and administration of outcome measures						
Longitudinal[c]	--	6	3	--	--	--
Cross-sectional	--	4	0	--	--	--

[a] Total longitudinal $k = 10$; total cross-sectional $k = 4$. Multiple outcomes and timepoints within studies are assumed to be dependent and combined.

[b, c] Total $k = k$-1 due to assumption of dependence of outcome domains. 1 study (Whitaker et al., 2019) is not represented in the table because some effect sizes were estimated from full statistical information and others were estimated with inferential statistics.

3.3 Aggregate Student Findings

Thirty-two longitudinal studies measuring student outcomes, yielding 97 unique effect sizes, were represented in the primary longitudinal meta-analysis (i.e., assessing longitudinal changes in student psychosocial functioning in the presence of trauma-informed interventions). Meta-analysis of longitudinal studies resulted in a significant, medium combined effect size (Hedge's $g = 0.65$, 95% CI = 0.23 to 1.07, $p = .002$). Specifically, students had significantly better psychosocial outcomes following the implementation of trauma-informed interventions in their schools (see Figure 16). Seventeen cross-sectional studies measuring student outcomes, yielding 46 unique effect sizes, were represented in the primary cross-sectional meta-analysis (i.e., assessing the difference psychosocial functioning in students who received trauma-informed interventions and students who did not). Meta-analysis resulted in a non-significant, small, combined effect size (Hedge's $g = 0.04$, 95% CI = -0.09 to 0.16, $p = .58$). Specifically, students who received trauma-sensitive interventions in school had the same levels of psychosocial functioning following intervention as students who did not receive trauma-sensitive interventions in school (see Figure 17).

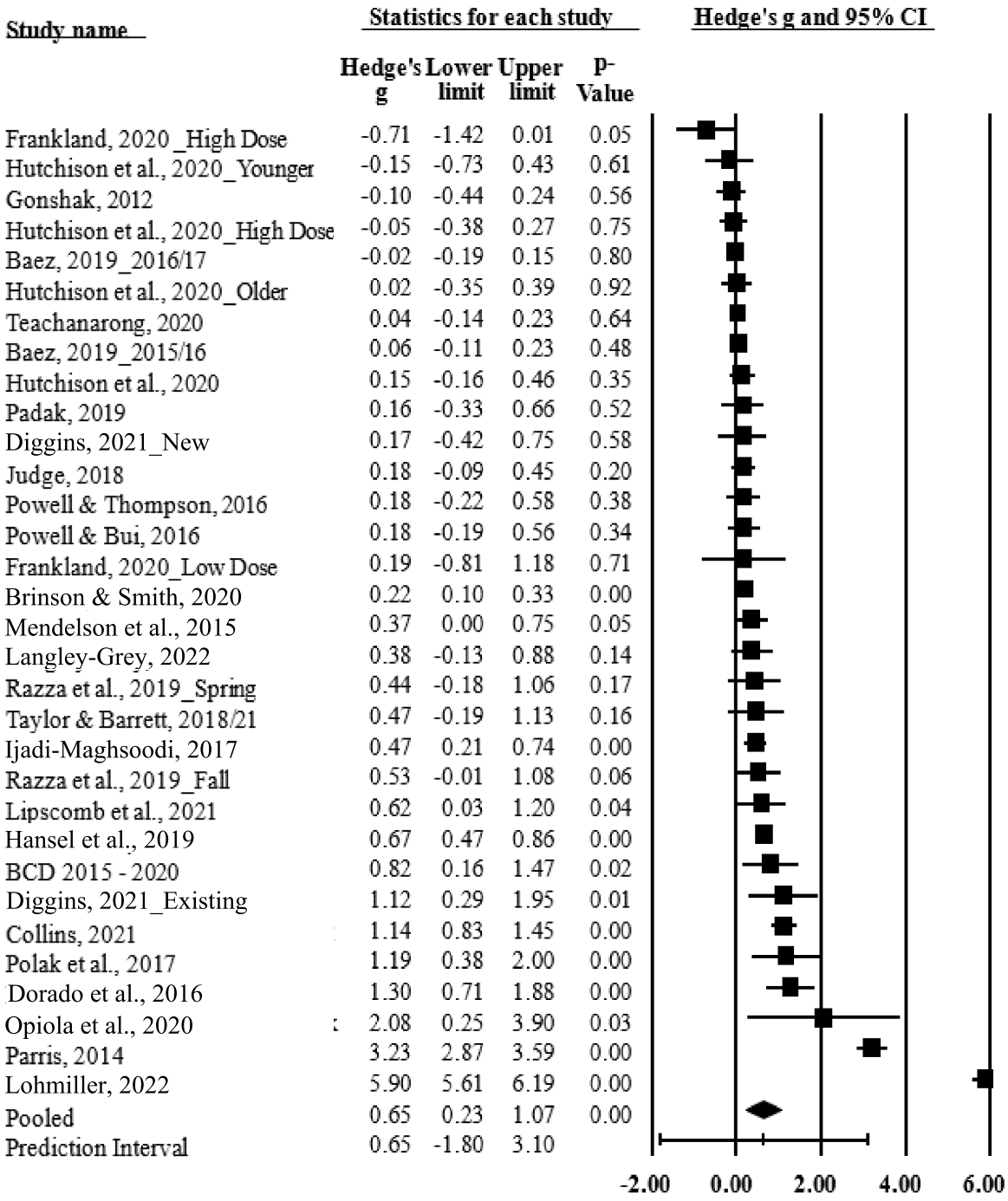


Figure 16. Forest plot for longitudinal effect sizes for student-level outcomes.
Note. An underscore and a descriptor following a study denotes multiple comparisons used within the study.

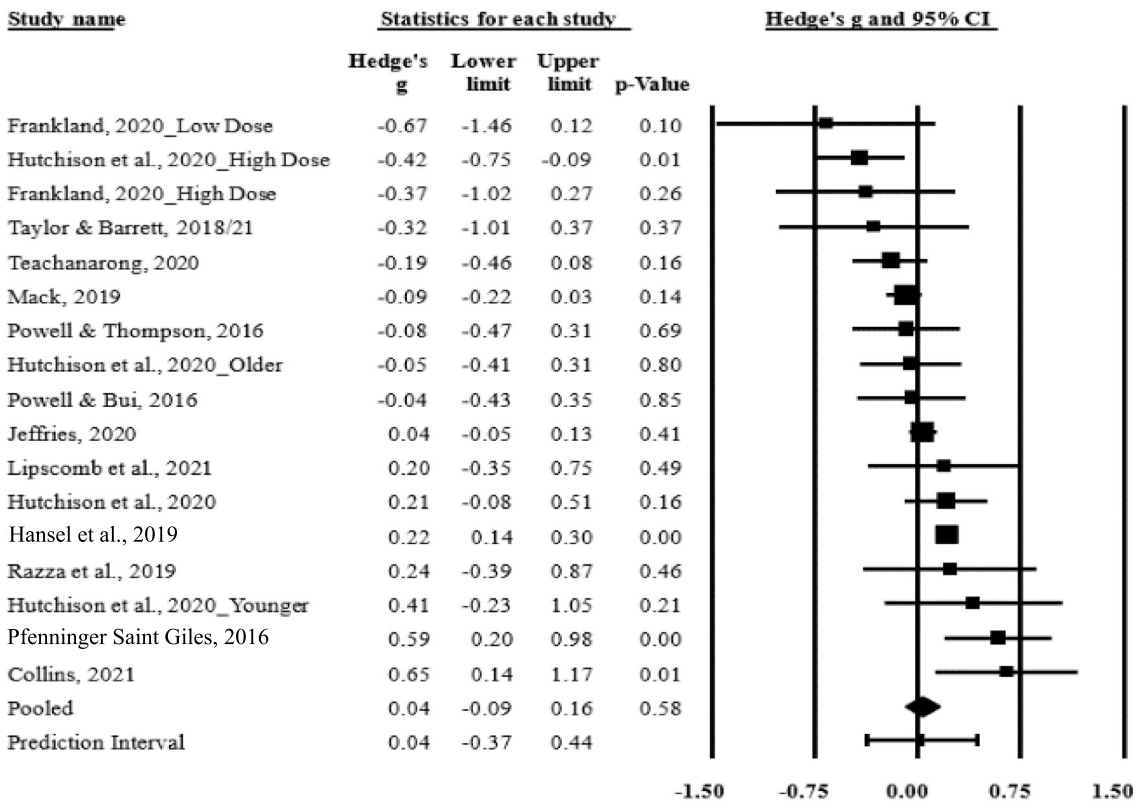


Figure 17. Forest plot of cross-sectional effect sizes for student-level outcomes.
Note. An underscore and a descriptor, following a study denotes multiple comparisons used within the study.

There was considerable heterogeneity between studies in both the longitudinal (Q[31] = 1,739.03, p < .001, I^2 = 98.22) and cross-sectional studies (Q[16] = 54.59, p < .001, I^2 = 70.69) meta-analyses, indicating that the true effect size of trauma-informed interventions varies across studies. Publication bias was assessed as a potential contributor to between-study heterogeneity in both longitudinal and cross-sectional studies. Meta-regression of mean effect size for student outcomes on peer review indicated that effect size did not vary as a function of peer review status among longitudinal studies (F[1, 30] = 1.85, p = .18), and the goodness of fit test (i.e., that the

unexplained variance in the model is zero) was significant (Q[30] = 1,662.40, R^2 = 0.00, p < .001) indicating that there was unexplained variance in the moderation model. Visual inspection of the funnel plot (Figure 18) did not reveal any immediately apparent asymmetry in the lower half of the plot, nor did the Trim and Fill procedure identify any studies likely missing due to publication bias. Thus, it may be concluded that publication bias did not impact the student-level outcomes results in longitudinal studies. However, visual inspection did reveal two studies (Lohmiller et al., 2022; Parris et al., 2014) with notably larger effect sizes than the rest. A sensitivity analysis was conducted to determine the impact of removing any one given study on the mean effect size across student outcomes. Results demonstrated that the mean effect size across studies would range from 0.45 – 0.69 (all ps < .01) depending on which study were to be removed.

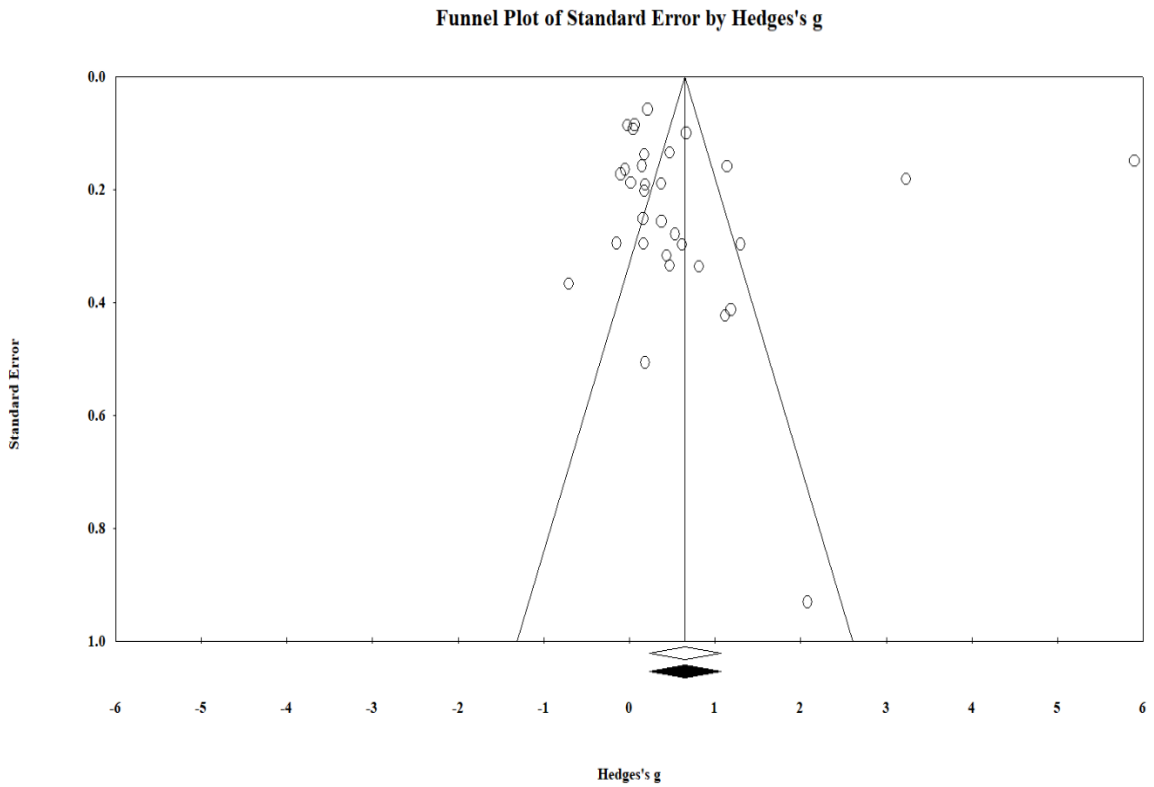


Figure 18. Funnel plot of standard error by Hedge's g for aggregate student-level outcomes in longitudinal studies.

Publication bias was assessed as a potential contributor to between-study heterogeneity among cross-sectional studies as well. Meta-regression of mean effect size for student outcomes on peer review indicated that effect size did not vary as a function of peer review status (F[1, 15] = 0.00, p = .97) and the goodness of fit test (i.e., that the unexplained variance in the model is zero) was significant (Q[15] = 44.38, R^2 = 0.00, p = 0.001) indicating that there is unexplained variance in the moderation model. Visual inspection of the funnel plot did not reveal any immediately apparent asymmetry in the lower half of the plot, nor did the Trim and Fill procedure identify any studies likely

missing due to publication bias (see Figure 19). Thus, it may be concluded that publication bias did not impact the cross-sectional student-level outcome results.

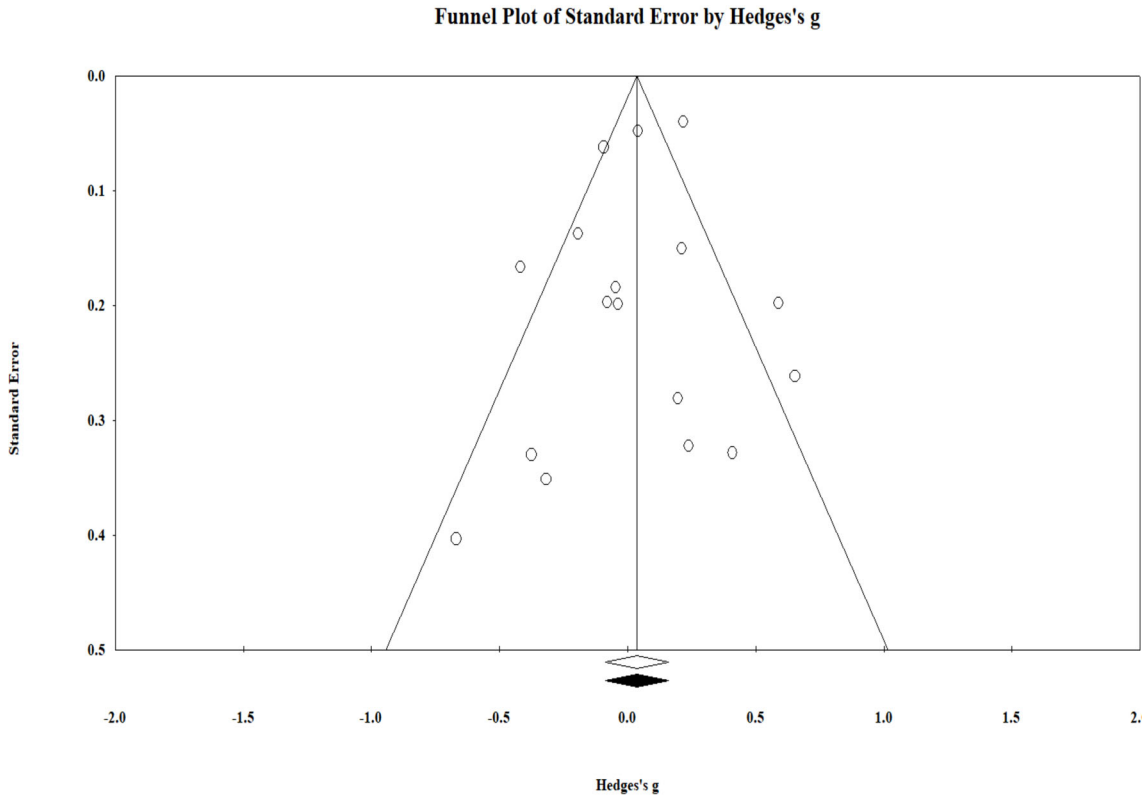


Figure 19. Funnel plot of standard error by Hedge's g for cross-sectional studies measuring student-related outcomes

Additionally, a sensitivity analysis of cross-sectional studies was conducted to determine the impact of removing any one given study on the mean effect size across student outcomes. Results demonstrate that the mean effect size across studies would range from 0.00 – 0.07 (*p*s > .05) depending on which study were to be removed. Thus, the removal of any single study would not significantly alter the results.

Given the difference between student psychosocial outcomes estimated from longitudinal studies (k = 32, Hedge's g = 0.65, 95% CI = 0.23 to 1.07, p = .002) and between-subjects studies (k = 17, Hedge's g = 0.04, 95% CI = -0.09 to 0.16, p = .58), a moderation analysis was conducted to determine if this difference was statistically significant. Because nine studies included both longitudinal and cross-sectional data (I. Collins, 2021; Frankland, 2020; Hansel et al., 2019; Hutchison et al., 2020; Lipscomb et al., 2021; Powell & Bui, 2016; Powell & Thompson, 2016; Taylor & Barrett, 2018, 2021; Teachanarong Aragon, 2020), the assumption of independence was applied to outcomes in order to effectively compare effect sizes based on this moderator. With the assumption of independence applied to outcome domain, outcome measure, and timepoint, the total k increased to 143. As previously stated, when assessing differences across outcomes, assuming independence is a conservative approach. Moderation analysis was significant (Q[1] = 13.30, p < .001), indicating that studies utilizing longitudinal data collection methods yielded a significantly larger aggregate effect size than studies utilizing a cross-sectional, or between subjects, design. See Figure 20 for a visual representation of this analysis.

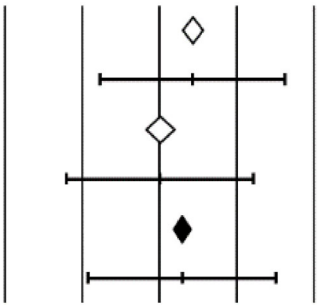
Group by B/w subjects analysis	Study name	Statistics for each study				Hedge's g and 95% CI
		Hedge's g	Lower limit	Upper limit	p-Value	
Within Subjects	Pooled	0.44	0.31	0.57	0.00	
Within Subjects	Prediction Interval	0.44	-0.76	1.64		
Between Subjects	Pooled	0.02	-0.17	0.21	0.84	
Between Subjects	Prediction Interval	0.02	-1.19	1.23		
Overall	Pooled	0.30	0.20	0.41	0.00	
Overall	Prediction Interval	0.30	-0.91	1.52		

Figure 20. Visual representation of effect size differences between within-subjects and between-subjects student outcomes.

3.3.1 Categorical Moderators

As described in the methods section, a variety of categorical moderators were coded for each study. Counts of categorical moderators for longitudinal and cross-sectional studies are described in Table 11.

Table 11. Number studies (k) measuring student outcomes per category for all categorical moderators.

Categorical Moderator	Level of moderator and number of studies per level (*k*)					
	0	1	2	3	4	5
Categorical moderators pertaining to the study level[a]						
Workforce professional development						
Longitudinal	8	24	--	--	--	--
Cross-sectional	7	10	--	--	--	--
Practice change						
Longitudinal	1	31	--	--	--	--
Cross-sectional	0	17	--	--	--	--
Organizational level change						
Longitudinal	10	22	--	--	--	--
Cross-sectional	4	13	--	--	--	--
Number of trauma-informed elements						
Longitudinal	--	4	11	17	--	--
Cross-sectional	--	2	7	8	--	--
Dosage of workforce professional development						
Longitudinal	8	6	16	2	--	--
Cross-sectional	7	4	6	0	--	--
Length of intervention						
Longitudinal	0	1	15	9	6	0
Cross-sectional	1	1	6	4	5	0
Study design						
Longitudinal	--	16	--	16	--	--
Cross-sectional	--	--	2	15	--	--
Random assignment						
Longitudinal	27	5	--	--	--	--
Cross-sectional	14	3	--	--	--	--
Outcome sample[b]						
Longitudinal	27	4	--	--	--	--
Cross-sectional	16	1				
Peer reviewed						
Longitudinal	9	23	--	--	--	--
Cross-sectional	7	10	--	--	--	--
Estimation of Hedge's *g*[c]						
Longitudinal	--	21	9	--	--	--
Cross-sectional	--	14	2	--	--	--
Length of time between the end of intervention and administration of outcome measures[d]						
Longitudinal	--	88	9	--	--	--
Cross-sectional	--	40	6	--	--	--

Outcome Domain[e]	Other wellness	Academic	Cope	Executive Functioning	Externalize	Health Behavior	Internalize
Longitudinal	12	11	4	4	13	1	12
Cross-sectional	7	7	1	2	3	4	4

[b] Total $k = k$-1 due to assumption of dependence of outcome domains. One study (Dorado, 2016) is not represented in the table because five effect sizes were estimated from universal samples and five were estimated from a targeted sample.
[c] Total longitudinal $k = k$-2 and total cross-sectional $k = k - 1$ due to assumption of dependence of outcome domains. Some effect sizes in the missing studies were

Table 11, cont.
estimated with full statistical information and some were estimated from inferential statistics.
[d] Total longitudinal $k = 97$ due to assumption of dependence of outcome domains. Total cross-sectional $k = 46$ because all outcome domains, outcomes, and timepoints were assumed to be independent.
[e] Total $k = 97$ for longitudinal studies and $k = 28$ for cross-sectional studies due to assumption of independence of outcome domains. Outcome measures and timepoints were combined within each study. This was necessary due to the complex data structure of many studies.

3.3.1.1 Outcome Domain Findings.

As described in the data analysis section, to compare student outcome domains, independence of outcome domains was assumed. However, for studies that used multiple outcome measures to assess a single domain, measures were combined, as were multiple timepoints of data collection within each study. This strategy yielded 57 longitudinal studies and 28 cross-sectional studies and is a conservative approach of estimating differences between dependent outcomes. Even assuming independence of observations, there were insufficient studies to determine whether effect sizes differed across domains among cross-sectional studies. Among longitudinal studies, there were insufficient studies to determine differences in effect size across all domains (youth coping $k = 4$, youth executive functioning $k = 4$, youth health behavior $k = 1$), but there were sufficient studies to compare select domains (other youth wellness indicators $k = 12$, youth academic functioning $k = 11$, youth externalizing $k = 13$, and youth internalizing $k = 12$). Analysis revealed that effect sizes differed significantly across broad domains ($Q[3] = 10.23$, $p = .02$). See Figure 21 for a visual representation of this analysis.

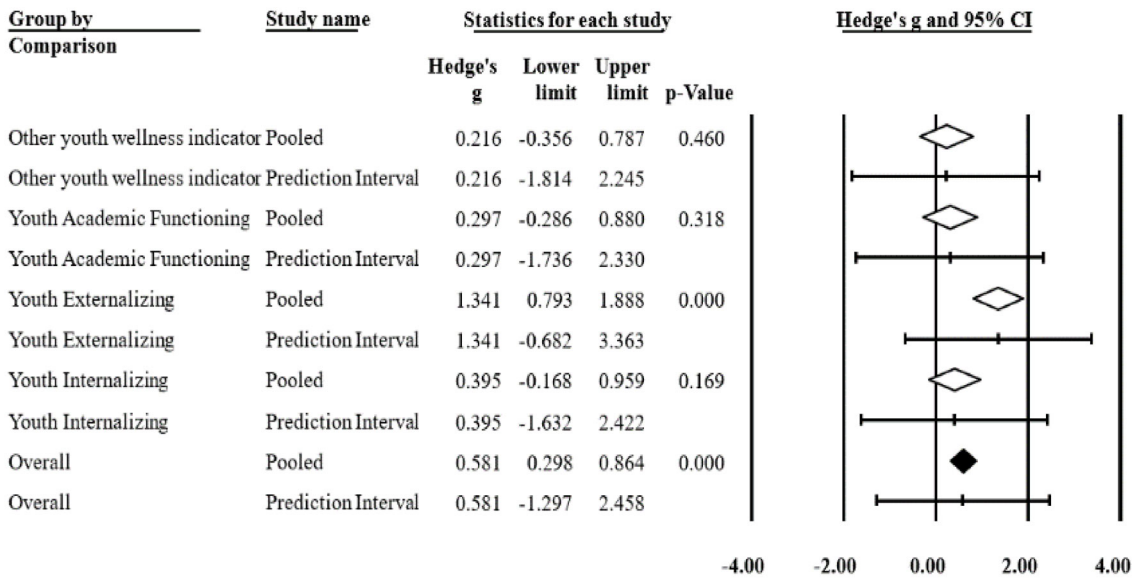


Figure 21. Subgroup analysis of studies measuring student outcomes across four broad outcome domains.

According to pairwise comparisons, the aggregate effect size for youth externalizing behaviors was the only effect size that significantly differed from other outcome domains. Specifically, externalizing behaviors improved significantly more than youth wellness and youth academic functioning (see Table 12 for full results). Due to issues of multicollinearity, multivariate meta-regression was not possible. Thus, it could not be determined if trauma-informed interventions would continue to yield larger effects on youth externalizing behavior after controlling for methodological and intervention characteristics.

Table 12. Categorical moderators for broad domain outcomes in longitudinal evaluations of student outcomes.

Broad Domain	***Q***	***df***	***p***	Pairwise
Other youth wellness indicator vs youth academic functioning	0.65	1	.42	OWI = YAF
Other youth wellness indicator vs. youth externalizing behavior	4.33	1	.04*	OWI < YEB
Other youth wellness indicator vs youth internalizing symptoms	1.81	1	.18	OWI = YIS
Youth academic functioning vs youth externalizing behavior	4.29	1	.04*	YAF < YEB
Youth academic functioning vs youth internalizing symptoms	0.10	1	.75	YAF = YIS
Youth externalizing behavior vs youth internalizing symptoms	3.05	1	.08	YES = YIS

3.3.1.2 Trauma-Informed Intervention Elements.

Key elements as identified by SAMHSA, of trauma-informed interventions include professional development, organizational change, and practice change. To test the hypothesis that interventions containing all three elements of trauma-informed interventions have significantly better outcomes than studies that do not contain these elements, two sets of analyses were conducted. First, a moderation analysis was conducted among longitudinal studies measuring staff outcomes to determine whether effect sizes significantly differed based on the number of trauma-informed elements present. Once again, there were insufficient studies to test this moderator among effect sizes yielded from between subjects (i.e., cross-sectional) analyses (k = 17). Because each trauma-informed element is dichotomously coded at the study rather than outcome level, effect sizes were able to be combined across outcome domains within studies. Thus, the total number of longitudinal studies included in these analyses is 32. There were

insufficient studies to compare interventions utilizing only one trauma-informed element (k = 4) to interventions that utilized two (k = 11) or three elements (k = 17), thus, the sample was restricted to interventions that included two or more elements. Analysis revealed that effect sizes yielded from interventions using two trauma-informed elements (Hedge's g = 0.49, CI = -0.31- 1.28, p = .23) did not differ significantly from studies that used three trauma-informed elements (Hedge's g = 0.81, CI = 0.19 - 1.44, p = .01; Q[1] = 0.41, p = .53). See Figure 22 for a visual representation of this analysis

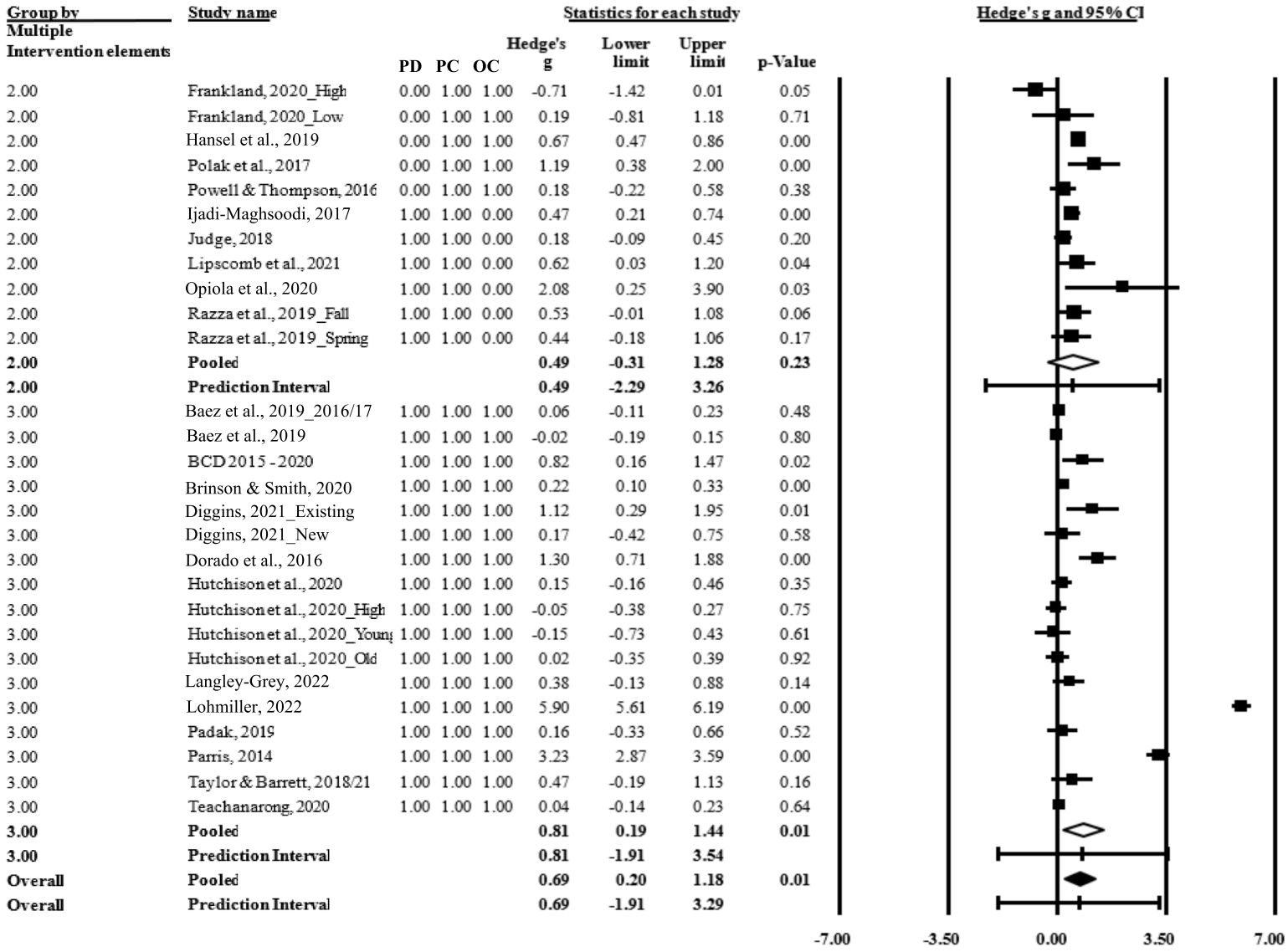


Figure 22. Subgroup analysis of longitudinal studies measuring student outcomes across number of trauma-informed elements.
Note. An underscore and a descriptor, denotes multiple comparisons used within the study.

The second set of analyses determined whether effect sizes from longitudinal studies measuring changes in student outcomes varied based on the presence or absence of each trauma-informed element. Once again, moderation analysis was not possible for cross-sectional studies given the small sample size (k = 17). Twenty-four longitudinal studies included workforce professional development while eight studies did not. Thus, moderation analysis was not conducted for this element due to the small sample of studies without workforce professional development. Similarly, it was not possible to formally examine the moderating role of practice change because only one longitudinal study did not include practice change.

Effect sizes for 22 longitudinal studies that implemented trauma-informed organizational change were compared to 10 studies that did not include organizational level change. Between-study heterogeneity was not significant (Q[1] = 0.12, p = .73), indicating that the effect sizes yielded by studies that did not include organizational change (Hedge's g = .54, CI = -0.24 - 1.31, p = .18) were equal to studies that did include organizational level change (Hedge's g = .70, CI = 0.19- 1.22, p = .01). Figure 23 visually depicts the effect sizes for student outcomes as a function of organizational change.

Group by OC	Study name	Statistics for each study				Hedge's g and 95% CI
		Hedge's g	Lower limit	Upper limit	p-Value	
0.00	Pooled	0.54	-0.24	1.31	0.17	
0.00	Prediction Interval	0.54	-2.06	3.13		
1.00	Pooled	0.70	0.19	1.22	0.01	
1.00	Prediction Interval	0.70	-1.82	3.22		
Overall	Pooled	0.65	0.22	1.08	0.00	
Overall	Prediction Interval	0.65	-1.80	3.10		

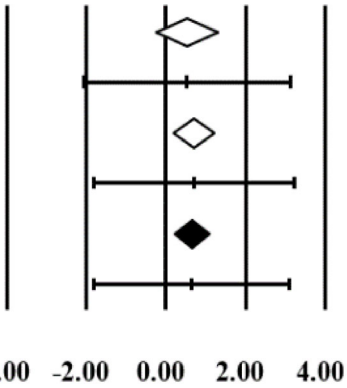


Figure 23. Subgroup analysis of studies measuring student outcomes across presence or absence of trauma-informed organizational-level change (OC).

3.3.1.3 Dosage of Professional Development.

Formal subgroup analysis examining the combined effect size difference across dosage of professional development was not conducted among longitudinal or cross-sectional studies measuring student outcomes due to low sample size. Specifically, among studies longitudinally measuring changes in student outcomes following school-based trauma-informed interventions, eight interventions did not include trauma-informed staff professional development. Six interventions included less than one day of professional development, and 16 interventions provided between one day to one week of professional development. Finally, two studies (Baez et al., 2019) did not report the dosage of professional development provided to staff. Among studies investigating the impact of trauma-sensitive schools cross-sectionally, seven did not provide staff with trauma-informed professional development. Of the remaining 10 studies that did provide

trauma-informed staff professional development, four provided less than one day and six provided between one day to one week.

3.3.1.4 Length of Intervention.

In addition to the dosage of trauma-specific professional development, the dosage of the entire intervention (i.e., the length in days) was considered as a moderator as well. Length of intervention was estimated from all 32 longitudinal studies that measured changes in student outcomes and all 17 cross-sectional studies measuring changes in student outcomes. However, there were insufficient studies to examine differences between interventions based on length of intervention (see Table 11).

3.3.1.5 Methodological Moderators.

Study design, random assignment, outcome sample, and peer review status were coded at the study level. Estimation of Hedge's *g* was coded at the outcome level; however, it was analyzed at the study level. This means that dependence of samples was assumed which resulted in the inability to parse out full versus partial estimation of effect sizes for certain longitudinal studies (Baroni et al., 2020; Crosby et al., 2019; Day et al., 2015; Razza et al., 2019). Of note, Baroni et al. (2020); Crosby et al. (2019); and Day et al. (2015) were treated as one study for the purposes of these analyses because they reported data from the same dataset. Effect sizes were also estimated from a combination of full statistical information and inferential statistics in one cross-sectional study (Mack, 2019).

This approach was chosen because a Type 2 error for this analysis would lead to an anticonservative interpretation of the data. Thus, in this instance, assuming dependence of outcomes was the more conservative approach. Length of time between

the end of intervention and administration of outcome measures was coded at the outcome level. Accordingly, outcomes and time points were treated as independent samples, though they were derived from dependent samples.

There were sufficient longitudinal studies of changes in student outcomes per category to conduct formal moderation analyses for study design only. There were insufficient cross-sectional studies of changes in staff outcomes to formally examine methodological moderators (k = 17). Study design was not a significant methodological moderator of effect size (Q[1] = 3.20, p = .07), indicating that studies utilizing a single group pre-post design (Hedge's g = 1.05, CI = 0.43 - 1.66, p = .001) yielded comparable effect sizes to studies that utilized a pre-post, control group design (Hedge's g = 0.23, CI = -0.35 - 0.87, p = .01).

3.3.2 Youth Academic Functioning Aggregate Findings

Eleven studies, yielding 21 effect sizes, longitudinally evaluated the impact of school-based trauma-informed interventions on student academic functioning. Seven studies, yielding 13 effect sizes, cross-sectionally evaluated the impact of trauma-sensitive schools interventions on student academic functioning. These studies were assumed to be a random sample from a universe of potential studies, and this random-effects analysis will be used to make an inference to that universe (Borenstein, 2019; Borenstein et al., 2010; Borenstein et al., 2021; Hedges & Vevea, 1998; Higgins et al., 2019). The estimated mean effect size from longitudinal studies small (Hedge's g = .30, 95% CI = 0.05 to 0.56, p = .02] but significant, indicating that students' academic functioning was significantly higher after trauma-informed interventions occurred in their school than before these interventions occurred (Figure 24). Given the small sample of

cross-sectional studies, an aggregate effect size was not generated. Though there was insufficient power to formally analyze an aggregate effect size yielded from cross-sectional studies, effect sizes ranged from Hedge's g = -0.30 - 0.65, and *p*-values ranged from $p < .001$ - .54. There is considerable heterogeneity between longitudinal studies in this meta-analysis as indicated by a significant *Q*-statistic ($Q[10] = 94.58$, $p < .001$, $I^2 = 89.43$). Specifically, an I^2 value of 89.43 means that 89.43% of the observed variance reflects variance in true effects, while only 10.57% of the observed variance is attributable to sampling error. Moderation analyses were not possible due to the small sample size of studies.

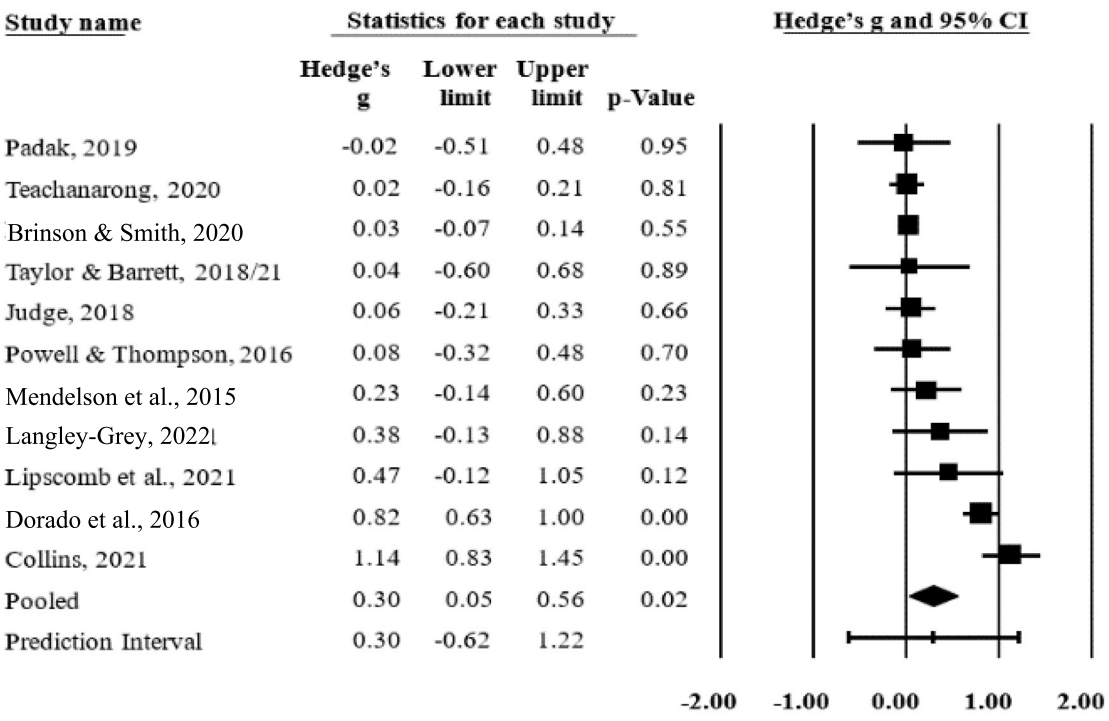


Figure 24. Forest plot of studies longitudinally measuring the impact of school-based, trauma-informed interventions on youth academic functioning.

3.3.3 Youth Externalizing Behavior Aggregate Findings

Thirteen longitudinal studies, yielding 20 effect sizes, and four cross-sectional studies yielding six effect sizes, evaluated the impact of school-based trauma-informed interventions on student externalizing behaviors. These studies were assumed to be a random sample from a universe of potential studies, and this random-effects analysis will be used to make an inference to that universe (Borenstein, 2019; Borenstein et al., 2010; Borenstein et al., 2021; Hedges & Vevea, 1998; Higgins et al., 2019). There was insufficient power to formally examine an aggregate effect size yielded from cross-sectional studies within the domain of youth externalizing behavior. Though there was insufficient power to formally analyze an aggregate effect size yielded from cross-sectional studies, effect sizes ranged from Hedge's g = -0.48 - 0.50 and p-values ranged from $p < .001$ - .52.

The estimated mean effect size of longitudinal studies is large (Hedge's $g = 1.37$, 95% CI = 0.43 to 2.31, $p = .004$] and significant, indicating that students' externalizing behavior significantly improved after trauma-informed interventions occurred in their school. There is considerable heterogeneity between studies in this meta-analysis as indicated by a significant Q-statistic ($Q[12] = 1{,}605.34$, $p < .001$, $I^2 = 99.25$). Specifically, an I^2 value of 99.25 means that 99.25% of the observed variance reflects variance in true effects, while only 0.75% of the observed variance is attributable to sampling error. Moderation analyses were not possible due to the small sample size of studies. See Figure 25 for a visual representation of youth externalizing behaviors effect sizes yielded from longitudinal studies.

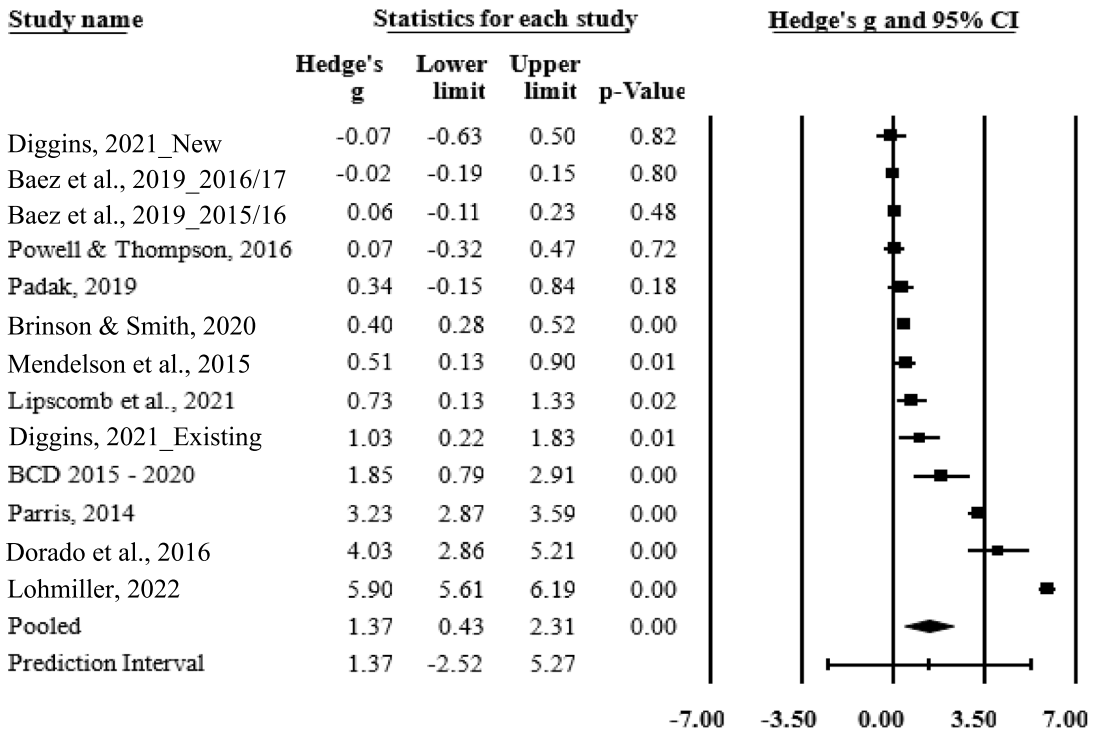


Figure 25. Forest plot of studies longitudinally measuring the impact of school-based, trauma-informed interventions on youth externalizing behaviors.
Note. An underscore and a descriptor, following a study denotes multiple comparisons used within the study.

3.3.4 Youth Internalizing Symptoms Aggregate Findings

Twelve longitudinal studies, yielding 16 effect sizes, and four cross-sectional studies yielding six effect sizes evaluated the impact of school-based trauma-informed interventions on student internalizing symptoms. These studies were assumed to be a random sample from a universe of potential studies, and this random-effects analysis will be used to make an inference to that universe (Borenstein, 2019; Borenstein et al., 2010; Borenstein et al., 2021; Hedges & Vevea, 1998; Higgins et al., 2019). There was insufficient power to formally examine an aggregate effect size yielded from cross-sectional studies within the domain of youth internalizing symptoms. Though there was

insufficient power to formally analyze an aggregate effect size yielded from cross-sectional studies, effect sizes ranged from Hedge's g = -0.05 - 0.72, and *p*-values ranged from $p < .001$ - .80

The estimated mean effect size of longitudinal studies is small (Hedge's $g = 0.33$, 95% $CI = 0.16$ to 0.54, $p < .001$) but significant, indicating that students' internalizing symptoms significantly improved following school-based, trauma-informed interventions compared to before receiving school-based, trauma-informed intervention (see Figure 26).There is substantial heterogeneity between studies in this meta-analysis as indicated by a significant *Q*-statistic [$Q(11) = 35.04$ $p = .001$, $I^2 = 68.60$]. Specifically, an I^2 value of 68.60 means that 68.60% of the observed variance reflects variance in true effects, while only 31.40% of the observed variance is attributable to sampling error. Moderation analyses were not conducted due to the small sample size of studies.

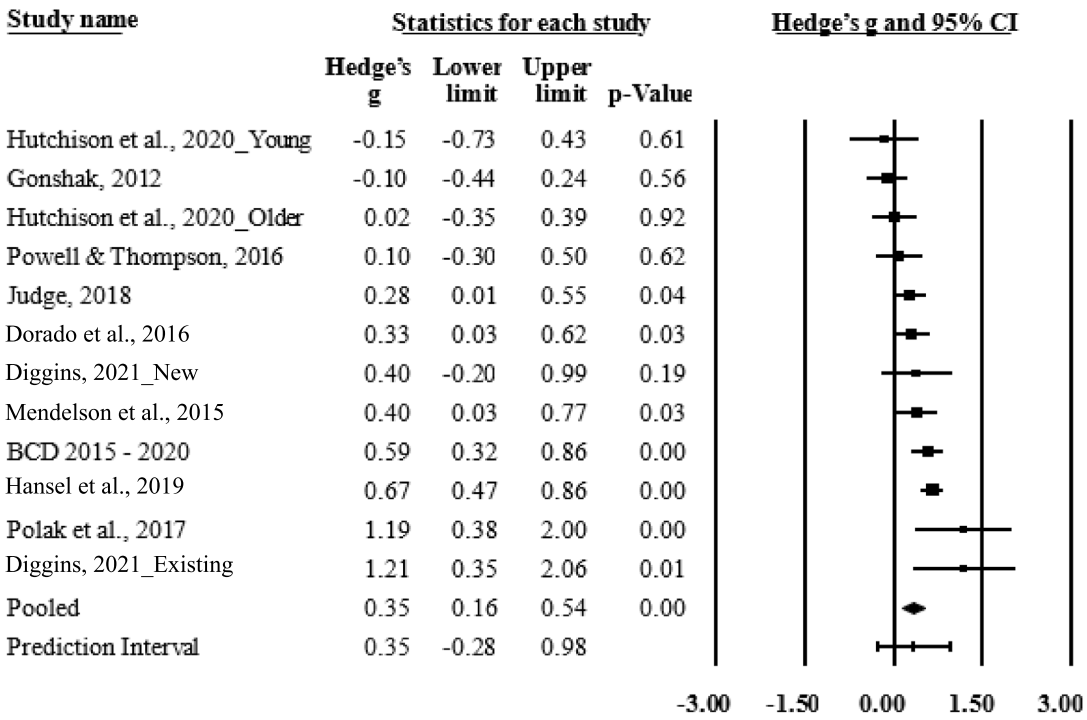


Figure 26. Forest plot of studies longitudinally measuring the impact of school-based, trauma-informed interventions on youth internalizing symptoms.
Note. An underscore and a descriptor, following a study denotes multiple comparisons used within the study.

3.3.5 Youth Coping Aggregate Findings

Four longitudinal studies, yielding 7 effect sizes, and two cross-sectional studies yielding four effect sizes, evaluated the impact of school-based trauma-informed interventions on student coping behaviors. These studies were assumed to be a random sample from a universe of potential studies, and this random-effects analysis will be used to make an inference to that universe (Borenstein, 2019; Borenstein et al., 2010; Borenstein et al., 2021; Hedges & Vevea, 1998; Higgins et al., 2019). While sample size was too small to warrant formal examination of an aggregate effect size of youth coping, effect sizes will be qualitatively described. Among longitudinal studies, effect sizes

ranged from small to moderate (Hedge's g = 0.14 – 0.72). Two longitudinal studies yielded significant effect sizes (Dorado et al., 2016 and Powell & Thompson, 2016) and two longitudinal studies yielded insignificant effect sizes (Judge, 2018; Powell & Bui, 2016). The two cross-sectional studies (Powell & Bui, 2016 and Powell & Thompson, 2016) that measured the impact of trauma-sensitive schools interventions on youth coping yielded negative effect sizes (Hedge's g = -0.10, p = .63 and Hedge's g = -0.19, p = .33 respectively). Moderation analyses were not possible due to the small sample size of studies.

3.3.6 Youth Executive Functioning Aggregate Findings

Four longitudinal studies, yielding nine effect sizes, and three cross-sectional studies yielding five effect sizes, evaluated the impact of school-based trauma-informed interventions on student executive functioning. These studies were assumed to be a random sample from a universe of potential studies, and this random-effects analysis will be used to make an inference to that universe (Borenstein, 2019; Borenstein et al., 2010; Borenstein et al., 2021; Hedges & Vevea, 1998; Higgins et al., 2019). While sample size was too small to warrant forma examination of an aggregate effect size of youth coping, effect sizes will be qualitatively described. Among longitudinal studies, effect sizes ranged from small to large (Hedge's g = 0.44 – 0.90). Two longitudinal studies yielded significant effect sizes (Lipscomb et al., 2021; Taylor & Barrett, 2018, 2021). Data from the two Taylor & Barrett studies were combined into one study for the purposes of this analysis as they pulled from the same dataset. The two longitudinal studies pulled from Razza and colleagues' 2019 study yielded insignificant effect sizes. Among cross-sectional studies that measured the impact of trauma-sensitive schools interventions on

youth executive functioning, effect sizes ranged from Hedge's g = -0.05 – 0.24, and p-values ranged from p = .85 - .34. Moderation analyses were not possible due to the small sample size of studies.

3.3.7 Youth Health Behaviors Functioning Aggregate Findings

One longitudinal study (Judge, 2018) yielding two effect sizes, and zero cross-sectional studies evaluated the impact of a school-based trauma-informed intervention on student health behaviors. Thus, meta-analysis of these outcomes was not performed, and the results will be qualitatively described instead. The Judge (2018) study measured students' exercise frequency and sleep frequency (Judge, 2018) before and after a school-based trauma-informed intervention that included staff professional development and school practice change but not organizational level change. The effect size for change in exercise frequency was small (Hedge's g = .31, SE = .14), as was the effect size for change in sleep frequency (Hedge's g = .40, SE = .14).

3.3.8 Other Youth Wellness Aggregate Findings

Twelve longitudinal studies, yielding 22 effect sizes, and seven cross-sectional studies yielding 11 effect sizes evaluated the impact of school-based trauma-informed interventions on student other youth wellness indicators including self-esteem, social competence, developmental assets, thriving, resilience, satisfaction with life, protective factors, and self-efficacy. These studies were assumed to be a random sample from a universe of potential studies, and this random-effects analysis was used to make an inference to that universe (Borenstein, 2019; Borenstein et al., 2010; Borenstein et al., 2021; Hedges & Vevea, 1998; Higgins et al., 2019). The estimated mean effect size of longitudinal studies is small (Hedge's g = 0.17, 95% CI = 0.01 to 0.34, p = .04] and

significant, indicating that other forms of student wellness were significantly changed following school-based, trauma-informed interventions (see Figure 27). There is substantial heterogeneity between studies in this meta-analysis as indicated by a significant *Q*-statistic [$Q(10) = 28.79$ $p = .001$, $I^2 = 61.79$]. Specifically, an I^2 value of 61.79 means that 61.79% of the observed variance reflects variance in true effects, while only 38.21% of the observed variance is attributable to sampling error.

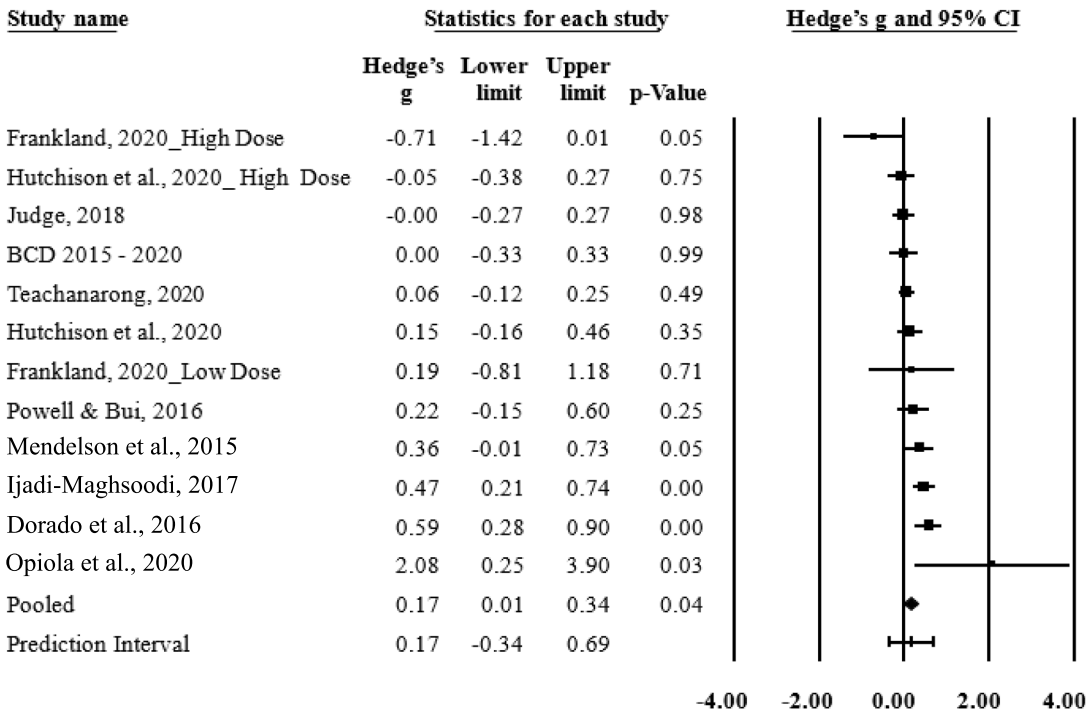


Figure 27. Forest plot of studies longitudinally measuring the impact of school-based, trauma-informed interventions on other youth wellness indicators.
Note. An underscore and a descriptor, following a study denotes multiple comparisons used within the study.

There were insufficient cross-sectional studies to formally analyze a mean effect size, thus results will be described qualitatively. The forest plot below (Figure 28) will visually

display the estimated effect sizes for each of the cross-sectional studies measuring student wellness following trauma-sensitive schools interventions as compared to students who did not receive trauma-sensitive schools interventions. However, the forest plot does not include an overall estimate of effect size due to power limitations. Cross-sectional studies measuring student wellness had mixed results with just over half of the studies reporting small to moderate negative effect sizes, and just under half of the studies reporting small to moderate positive effect sizes. In other words, about half of the interventions favored the control groups while about half favored the intervention group.

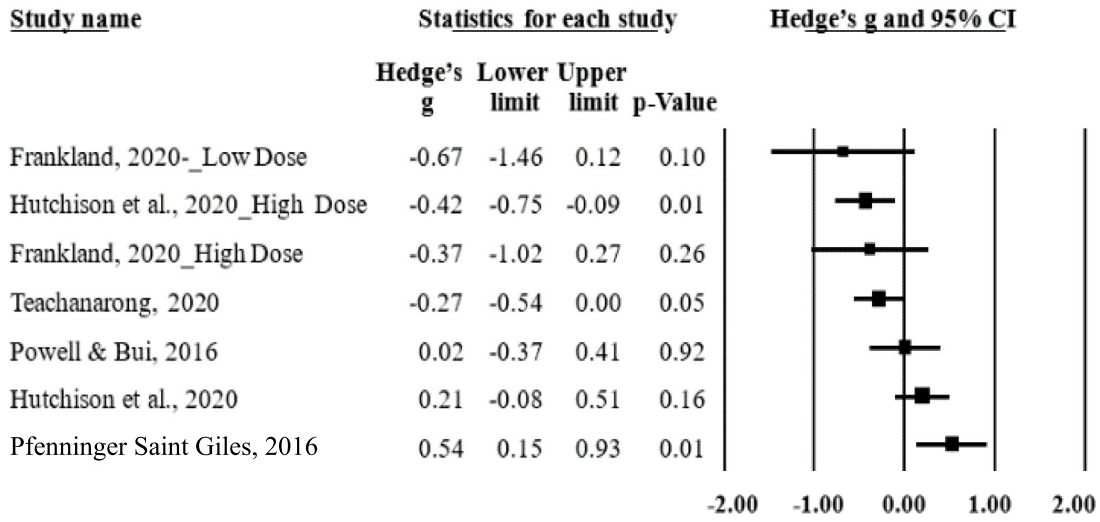


Figure 28. Visual representation of effect sizes estimated from cross-sectional studies measuring student wellness-related outcomes.
Note. An underscore and a letter, or descriptor, following a study denotes multiple comparisons used within the study.

3.4 Aggregate School Climate Results

Twenty-three longitudinal studies measuring school climate outcomes, yielding 46 unique effect sizes, and 12 cross-sectional studies, yielding 24 unique effect sizes, were represented in the primary longitudinal meta-analysis (i.e., assessing effect of trauma-informed interventions on school climate outcomes). Meta-analysis of longitudinal studies resulted in a significant, small, combined effect size (Hedge's g = 0.19, 95% CI = 0.04 to 0.33, p = .01). That is, school climate significantly improved following the implementation of trauma-informed interventions in schools (see Figure 29). However, meta-analysis comparing schools that did not receive trauma-sensitive interventions to schools that received trauma-sensitive interventions (see Figure 30), revealed no significant differences in school climate outcomes (Hedge's g = -0.02, 95% CI = -0.19 – 0.15, p = .82).

There was high heterogeneity between studies in both the longitudinal meta-analysis [$Q(22) = 67.82$, $p < .001$, $I^2 = 67.56$], and the cross-sectional meta-analysis [$Q(11) = 28.54$, $p = .003$, $I^2 = 61.46$], indicating that the true effect size of trauma-informed interventions varies across studies. Sensitivity analyses were conducted to determine the impact of removing any one given study on the mean effect size across student outcomes. Results demonstrate that the mean effect size across studies would range from 0.15 – 0.22 (all ps < .05) among longitudinal studies, and from -0.06 – 0.04 (ps > .05) among cross-sectional studies depending on which study were to be removed.

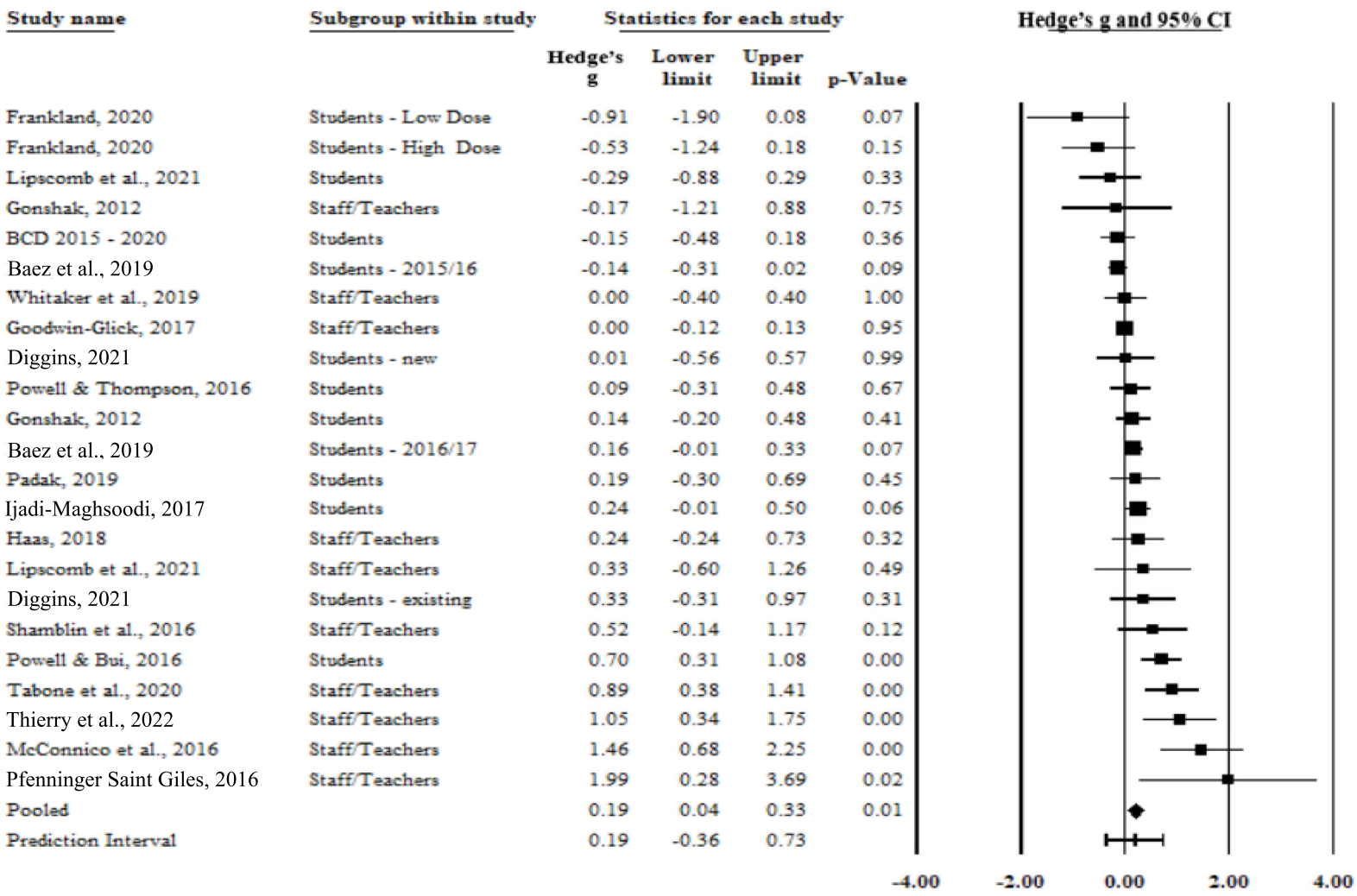


Figure 29. Visual representation of longitudinal analyses of trauma-sensitive schools on school climate outcomes.

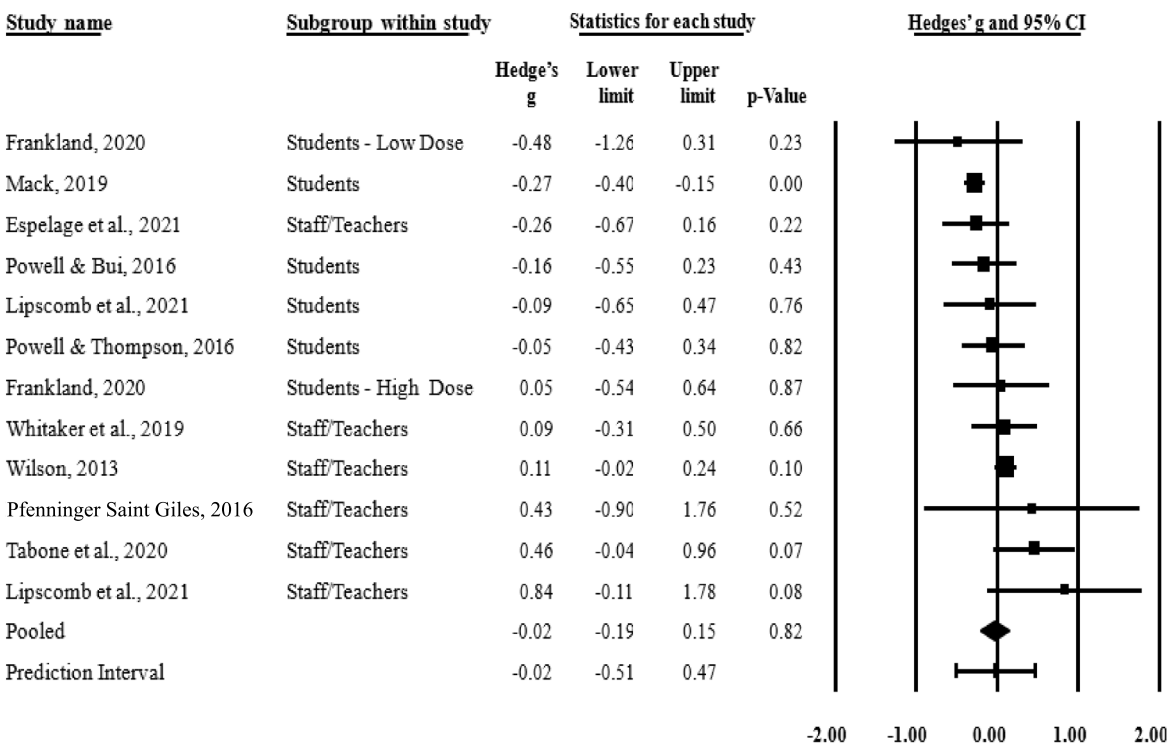


Figure 30. Visual representation of cross-sectional analyses of trauma-sensitive schools on school climate outcomes.

Publication bias was assessed as a potential contributor to between-study heterogeneity. Among longitudinal studies, a total of nine studies were not peer reviewed meaning that they were either unpublished, or published in a medium that is not peer reviewed, and 14 studies were peer reviewed. Meta-regression of mean effect size for student outcomes on peer review indicated that effect size is not significantly predicted by peer review status [$F(1, 21) = 0.13$, $p = .72$], and the goodness of fit test (i.e., that the unexplained variance in the model is zero) was significant [$Q(21) = 66.88$, $R^2 = 0.00$, $p < .001$] indicating that there is unexplained variance in the moderation model. However, Visual inspection of the funnel plot revealed apparent asymmetry. One study with a smaller sample size yielded a large effect size, and no studies with small sample sizes

yielded large negative effects. Further, Duval and Tweedie's Trim and Fill procedure identified four studies likely missing due to publication bias (see Figure 31).

Publication bias was assessed as a potential contributor to between-study heterogeneity among cross-sectional studies as well. However, meta-regression of mean effect size for student outcomes on peer review was not indicated due to the small sample size of studies (i.e., not peer reviewed $k = 5$, peer reviewed $k = 7$). Visual inspection of the funnel plot revealed apparent asymmetry in the lower half of the plot. Two studies with small sample sizes yielded positive, moderate to large effect sizes. However, there were no studies with small sample sizes that yielded negative effects. Accordingly, the Trim and Fill procedure identified three studies likely missing due to publication bias (see Figure 32).

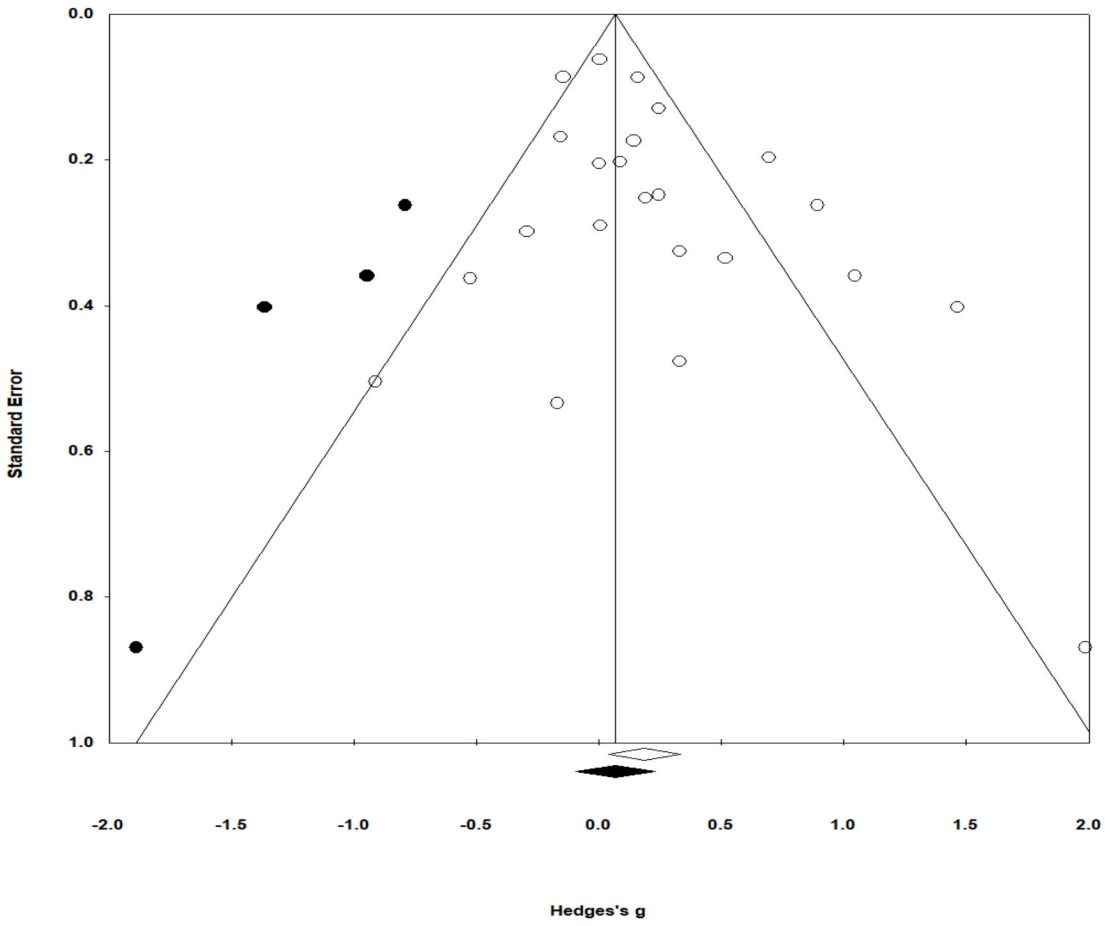


Figure 31. Funnel plot of standard error by Hedge's g for longitudinal studies measuring changes in school climate outcomes.

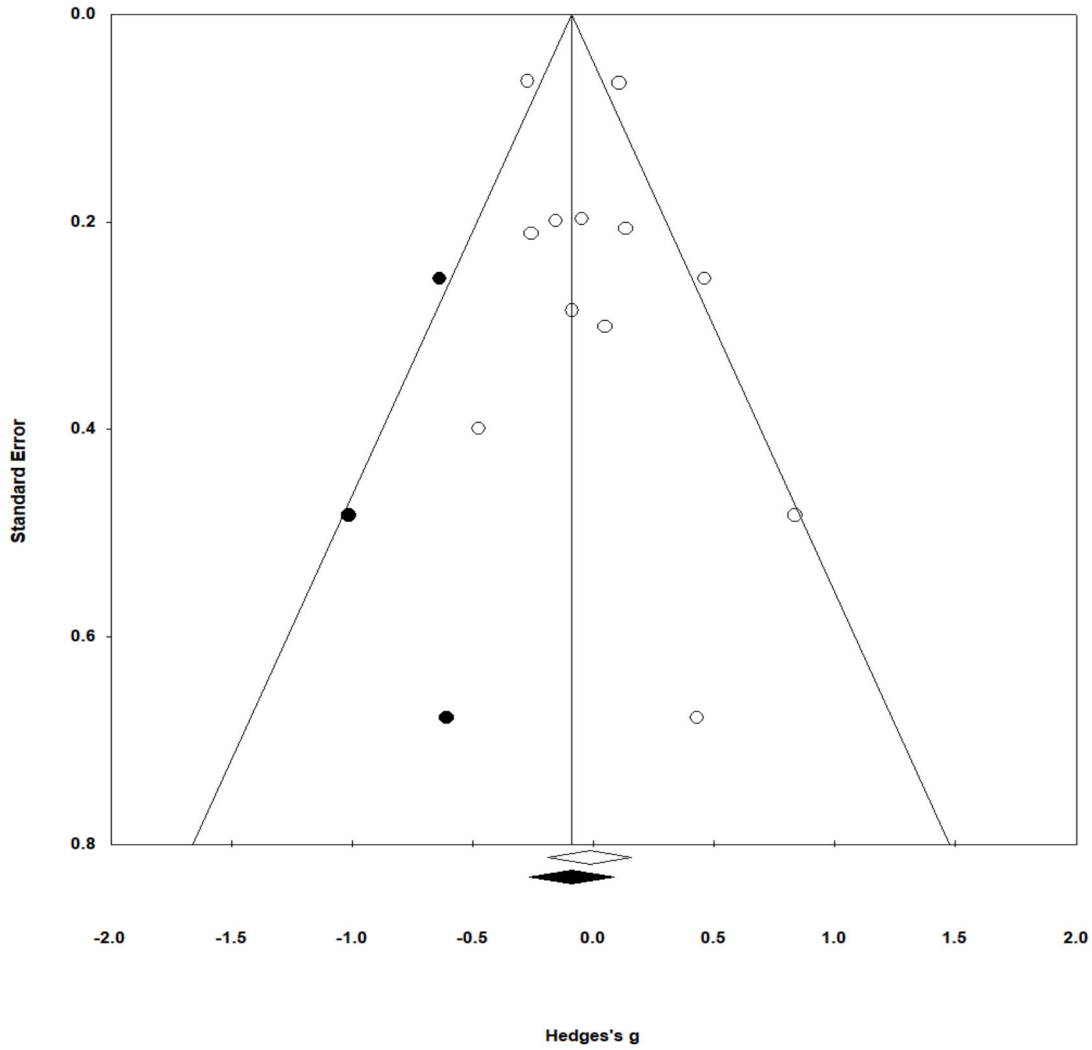


Figure 32. Funnel plot of standard error by Hedge's g for cross-sectional studies measuring changes in school climate outcomes between subjects.

Given the qualitatively apparent difference between school climate outcomes reported in longitudinal studies (k = 23, Hedge's g = 0. 19, 95% CI = 0.04 to 0.33, p = .01) and between-subjects studies (k = 7, Hedge's g = -0.02, 95% CI = -0.19 – 0.15, p = .82), a moderation analysis was conducted to determine if this difference was statistically significant. Because nine studies included both longitudinal and cross-sectional data related to school climate outcomes (Frankland, 2020; Lipscomb et al., 2021; Pfenninger Saint Gilles, 2016; Powell & Bui, 2016; Powell & Thompson, 2016; Tabone et al., 2020; Whitaker et al., 2019), outcomes were treated independently to compare effect sizes based on this moderator. With the assumption of independence applied to outcome domain, outcome measure, and timepoint, the total k increased to 70. As previously stated, when assessing between-study heterogeneity, assuming independence is a conservative approach. Moderation analysis was not significant [Q(1) = 0.88, p = .35], indicating that studies utilizing longitudinal data collection methods yielded similar effect sizes as studies utilizing a cross-sectional, or between subjects, design. See Figure 33 for a visual representation of this analysis.

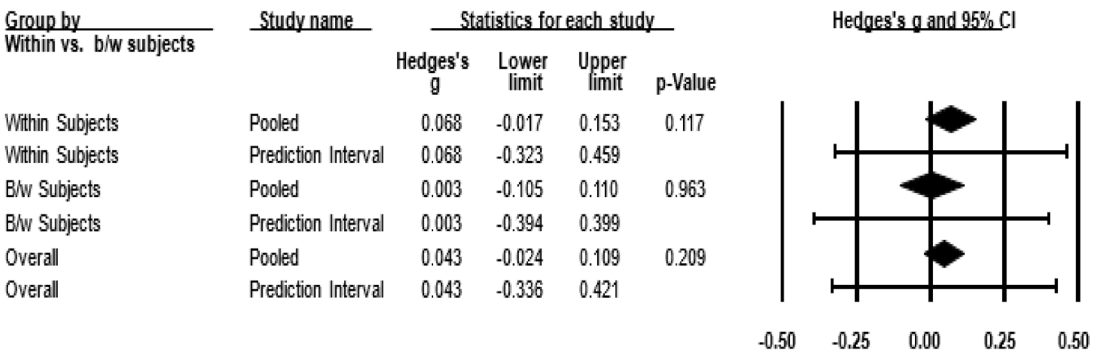


Figure 33. Subgroup analysis of studies across within-subjects study design aggregates and between-subjects study design aggregates.

3.4.1 Categorical Moderators of School Climate Results

A variety of categorical moderators were coded for each study. Given the significant heterogeneity in the meta-analysis of longitudinal student outcomes, further analyses were warranted to determine whether effect sizes differed across different levels of the proposed categorical moderators. However, there were insufficient studies to formally analyze most potential moderators (see Table 13 for number of studies *k* per moderator). There were sufficient longitudinal studies to compare differences in effect sizes by presence or absence of organizational-level change, timepoint of data collection (i.e., immediately after the intervention or up to one year following the intervention) and by teacher-report vs. student-report. Effect sizes were equal among studies that collected longitudinal data immediately following intervention and longitudinal studies that collected data up to one year following intervention. Effect sizes were significantly different between student-report of climate and staff-report of school climate such that staff-reported effect sizes were larger (see Table 14). There were insufficient cross-sectional studies to formally examine any moderator.

Table 13. Number of studies (k) cross-sectionally measuring school climate outcomes by level of intervention characteristic and methodological variables.

Categorical Moderator	Number of studies (*k*) across levels of categorical moderators				
	0	1	2	3	4
Categorical moderators pertaining to the study level[a]					
Workforce PD					
Longitudinal	4	19	--	--	--
Cross-sectional	4	8	--	--	--
Practice change					
Longitudinal	2	21	--	--	--
Cross-sectional	3	9	--	--	--
Organizational level change					
Longitudinal	9	14	--	--	--
Cross-sectional	6	6	--	--	--
Number of trauma-informed elements					
Longitudinal	--	6	6	11	--
Cross-sectional	--	4	5	3	--
Dosage of workforce professional development					
Longitudinal	4	4	13	2	--
Cross-sectional	4	5	3	0	--
Length of intervention					
Longitudinal	1	12	7	2	1
Cross-sectional	2	0	8	1	1
Study design					
Longitudinal	--	14	--	9	--
Cross-sectional	--	--	2	10	--
Random assignment					
Longitudinal	20	3	--	--	--
Cross-sectional	9	3	--	--	--
Outcome Sample					
Longitudinal	21	2	--	--	--
Cross-sectional	11	1	--	--	--
Peer reviewed					
Longitudinal	9	14	--	--	--
Cross-sectional	5	7	--	--	--
Categorical moderators pertaining to the outcome level					
Estimation of Hedge's *g*					
Longitudinal	--	17	6	--	--
Cross-sectional	--	12	0	--	--
Timepoint of data collection[b]					
Longitudinal	--	35	10	0	1
Cross-sectional	--	10	2	--	--

[a] Total $k = 12$ due to combination of outcome domain effect sizes within studies

[b] Total $k = 46$ due to assumption of independence of outcome domains. Outcome measures within each domain were combined as were timepoints within each study.

Table 14. Categorical moderators of school climate outcomes.

Categorical moderator (*k*)	Hedge's g	*CI*	B/w study heterogeneity	Pairwise
Organizational Change				
Absent	0.15	-0.08 – 0.39	$Q(1) = 0.17$ $p = .68$	Absent = Present
Present	0.22	0.01 – 0.43		
Length of time between the end of intervention and administration of outcome measures[b] (46)				
Immediately (35)	0.08	-0.01 – 0.18		
Up to one year (10)	-0.08	-0.29 – 0.13	$Q(1) = 1.96$[c] $p = .16$	Immediately = up to one year
More than one year (0)	--	--		
Unknown (1)	--	--		
Staff or student sample				
Staff (10)	0.45	0.18 – 0.71	$Q(1) = 5.44$ $p = .02$*	Staff > Students
Students (13)	0.05	-0.14 – 0.25		

[a] Total $k = 23$. Multiple outcomes and timepoints within studies are assumed to be dependent and combined.
[b] Total $k = 46$ due to assumption of independence of outcome domains. Outcome measures within each domain were combined as were timepoints within each study.
[c] The one study with an unknown length of follow up (McConnico et al., 2016) was excluded from moderation analysis due to low power.

CHAPTER IV

DISCUSSION

The past 21 years have brought increased awareness of the roles that schools can play in mitigating the impact of childhood trauma and adversity. With this awareness have come efforts to create training programs, policies, and best practices to equip schools to help and educate traumatized students. One approach that has risen to popularity is implementing the principles of trauma-informed care in school settings to create trauma-informed, or trauma-sensitive, schools. However, research on the impact of trauma-sensitive interventions for schools has varied widely, and the effectiveness of these strategies, and factors that may moderate effectiveness, are currently unclear. This dissertation aimed to meta-analyze the available information on the effects of trauma-sensitive schools on students, staff, and school climate.

The scope of this meta-analysis was focused on the following research questions: **Q1.** Do trauma-sensitive schools work? Do they positively impact student, staff, and school-climate outcomes? **Q2.** What are the specific components of trauma-sensitive schools that make them effective? **Q3.** What are the ideal dosages for staff professional development and overall intervention? To answer these questions, a systematic review was conducted, and the following information was generated from relevant studies: overall effect sizes for student, staff, and school climate outcomes as well as effect sizes

of specific outcome domains within each population; differences in effect sizes as a function of specific components of trauma-sensitive interventions; and differences in effect sizes as a function of intervention dose.

4.1 Research Question One: Do Trauma-Sensitive Schools Yield Significant Effect Sizes for Staff, Student, And School Climate Related Outcomes?

Overall, the hypothesis that trauma-sensitive school-based interventions would yield positive effects for staff, student, and school climate related outcomes was largely supported. Specifically, the aggregate effect size for staff outcomes was medium and significant for longitudinal studies and small and significant among cross-sectional studies. This indicates that not only did staff improve when compared to themselves prior to intervention, but they also had better outcomes following trauma-sensitive schools interventions than school staff that did not participate in a trauma-sensitive school interventions. However, when these overall findings were broken down into specific outcome domains, it became clear that some domains improved more than others. Specifically, staff knowledge increased significantly more than staff attitudes. One potential explanation for this is that increased understanding of trauma does not necessarily lead to improved trauma-informed attitudes. For example, it is possible that once learning about the prevalence and impact of trauma, teachers may feel overwhelmed and actually experience decreased self-efficacy for helping their traumatized students. Another potential explanation is that other mechanisms are involved in changing staff attitudes beyond simply increasing understanding of trauma. For example, staff affective responses to the information, staff belief in the information they are presented, and staff

perception of the utility and relevance of this information also likely contribute to trauma-informed attitudes. Staff knowledge also increased significantly more than staff wellness (e.g., compassion satisfaction, burnout, number of sick days).

In fact, staff wellness was the only staff outcome domain that did not significantly increase following trauma-sensitive intervention. One potential explanation for the nonsignificant effect sizes related to wellness among staff is that the meta-analyses failed to detect true effects. Thus, it is possible that the sample size was not large enough to detect the small effect. Alternatively, it is possible that the substantial heterogeneity of outcomes grouped within this domain made it a weak construct. In other words, the internal consistency of this construct is likely quite low, making it a poor measure of wellness. Finally, it is possible that trauma-sensitive interventions in schools have no significant impact on staff wellness.

The hypothesis that trauma-sensitive schools would positively impact student outcomes was only partially supported. The aggregate effect size yielded from longitudinal analyses testing this hypothesis was medium and significant, indicating that compared to themselves, students had significantly improved outcomes following participation in a trauma-sensitive school intervention. However, the aggregate effect size estimated from studies comparing the outcomes of students who participated in trauma-informed schools interventions to students who did not (i.e., intervention vs. control) was small and not significant. In other words, though students improved compared to themselves following trauma-sensitive intervention in schools, students did not have higher outcomes than children who did not participate in trauma-sensitive schools.

One possible explanation for these student findings is that these between-subjects analyses did not account for baseline levels of each construct. Thus, it is possible that the control groups had higher (or lower) baseline levels of each construct than the intervention groups. This is particularly likely given how few studies utilized random assignment to condition. Among student outcome studies, only three (21%) used random assignment (Lipscomb et al., 2021; Razza et al., 2019; Teachanarong Aragon, 2020). Moreover, within-subjects analyses tend to have less error than between subjects analyses because an individual is being compared to themselves (Howell, 2012). Thus, it is possible that the error present in the between-subjects analyses is obscuring true effects.

Another possible explanation for the difference between longitudinal and cross-sectional student-related outcomes effect sizes is that the meta-analyses failed to detect true effects. There were few cross-sectional studies of student-related outcomes to synthesize. Thus, it is possible that these analyses were underpowered. It is also possible that the measures employed in the between-subjects analyses were not sensitive to detecting changes between subjects. Only seven student studies (50%) used reliable (i.e., internal consistency or interrater reliability at or above .6) and valid measures (Frankland, 2020; Hansel et al., 2019; Jeffries, 2020; Pfenninger Saint Gilles, 2016; Powell & Thompson, 2016; Razza et al., 2019). One study used reliable measures but a combination of valid and unvalidated measures (Powel & Bui, 2016). Three studies used measures for which reliability was either poor or unreported but were validated (I. Collins, 2021; Mack, 2019; Taylor & Barrett, 2018, 2021). One study used measures that were neither reliable nor valid (Teachanarong, 2020), and two studies used validated measures but did not report reliability (Hutchison et al., 2020 & Lipscomb et al., 2021).

Thus, it is possible that measurement limitations contributed to the nonsignificant effect sizes. Finally, it is also possible that the strength of the interventions used in the between-subjects analyses were not strong enough to produce different outcomes between individuals.

Similar to school staff findings, specific areas of student functioning appear more responsive to trauma-sensitive schools interventions than others. Specifically, youth externalizing behaviors, as measured by longitudinal analyses, improved significantly more than student academic functioning and student wellness. Effect sizes for student externalizing behaviors and internalizing symptoms were comparable, and there were insufficient studies to compare the remaining student outcome domains (coping, health behavior, access to services, and executive functioning). One potential explanation for this finding is that the trauma-sensitive interventions acted as a buffer for children experiencing toxic stress. Given that sequelae of toxic stress include increased attention to threat and in-the-moment aggression (McLaughlin et al., 2019), interventions that buffer this toxic stress would be expected to reduce externalizing behaviors. Another possible explanation is that school policy for managing externalizing behaviors changed more than school policy for addressing student academic functioning or wellness. For example, a type of organizational change recommended in trauma-sensitive interventions is changing the policies around exclusionary discipline. Given that suspension and detention data were used as proxies for externalizing behaviors in certain studies, it would be expected that these would decrease in interventions with policy changes. For example, Lohmiller and colleagues (2022) used “refocus” assignment as a measure of student externalizing behavior. Refocus is like detention. However, part of the trauma-

sensitive intervention was changing the referral method for refocus, so it is possible that students were still engaging in externalizing behaviors, but staff were responding differently to them.

The hypothesis that trauma-sensitive schools would improve school climate was partially supported. Longitudinal studies yielded an average effect size that was small but significant, while cross-sectional studies yielded an aggregate effect size that was small, negative, and not significant. Interestingly, compared to students, the effect sizes yielded from staff tended to be larger. One possible reason that staff may have noticed larger improvements in school climate is that they were often the individuals implementing aspects of the intervention. Therefore, they would be more likely to notice the fruits of their labors than students. Further, staff may have a broader perspective than students, on systems-level factors like school climate, which may lead to more accurate reporting. Another potential explanation for this finding could be because staff data was collected through a combination of survey and observational methods. For example, the Emotional Support domain of the Classroom Assessment Scoring System (CLASS) was used to measure climate in five studies, and the effect sizes generated from these observations were large and significant in three out of the five studies (McConnico et al., 2016a; Pfenninger Saint Gilles, 2016; Tabone et al., 2020). Observations may at times yield larger effect sizes than survey data because they are conducted by observers trained to notice specific behaviors and nuances that may be missed by an untrained reporter (i.e., school staff).

In conclusion, trauma-sensitive schools appear to produce some positive gains within staff and student participants as well as positive changes in school climate within

the school. Further, school staff in trauma-sensitive schools appear to have more positive outcomes than staff in schools that are not trauma-sensitive. However, it is still unclear if trauma-sensitive schools produce more benefits among students and school climate compared to non-trauma-sensitive schools.

4.2 Research Question Two: What are the Specific Components of Trauma-Sensitive Schools that Make them Effective?

Currently, the literature on trauma-sensitive schools describes three components that make an intervention "trauma-informed:" workplace professional development related to trauma-informed care, trauma-informed practice change, and trauma-informed organizational-level change. Theoretically, these three components together produce the strongest trauma-informed intervention. Although this was unable to be tested for most outcome domains given the insufficient number of studies, it was possible to test this hypothesis among longitudinal studies of staff and student outcomes (i.e., there were insufficient cross-sectional/between-subjects analyses to test moderations), when outcome domains were combined. No differences in staff or student outcomes were found based on the number of trauma-informed elements included. There were insufficient studies to test this hypothesis for school climate.

Specifically, among longitudinal studies analyzing staff or student related outcomes, interventions that included multiple trauma-informed components had the same combined effect sizes as interventions that contained only one. However, it is possible that an interaction exists between outcome domain and number of trauma-informed elements, that is undetectable due to small sample size. Thus, these results may

not be particularly meaningful because it is likely that the number of trauma-informed components required to yield positive effects varies by outcome domain. That is, the number of trauma-informed elements required to change levels of staff knowledge is likely different than the number of trauma-informed elements necessary to effect change in staff behavior. For example, six studies (Gonshak, 2012; Goodwin-Glick, 2017; Haas, 2018; Law, 2019; McIntyre et al., 2019; Stipp & Kilpatrick, 2021) measuring changes in staff levels of trauma-informed knowledge contained only workforce professional development and no other trauma-informed elements. The aggregate effect size estimated from these six studies was moderate and significant (Hedge's $g = 0.78$, $p = .001$). This makes sense, as workforce professional development is typically didactic in nature, thus immediate gains in knowledge would be expected.

However, staff behavior is likely less impacted by a didactic professional development alone. Staff behavior would be expected to change and improve over the course of an intervention in which staff are required to comply with new organizational policies, practice new classroom management techniques, or implement social-emotional learning curricula with students. Indeed, among longitudinal studies measuring staff behavioral changes, the aggregate effect size yielded from four studies that included at least professional development and practice change (Dorado, 2016; McConnico et al., 2016; Morgan, 2021; Tabone et al., 2020) was large (Hedge's $g = 0.98$, $p < .001$). Conversely, the aggregate effect size of staff behavior changes estimated from the three studies that included workforce professional development only (Goodwin-Glick, 2017; Haas, 2018; Law, 2019) was small (Hedge's $g = 0.41$, $p = .04$). Though meta-analyses of with such small numbers of studies is likely underpowered (which was why it was not

calculated in the staff behavior change results section), qualitatively, studies with more trauma-informed elements may have larger effect sizes within certain outcome domains.

Similarly, while the total number of trauma-informed elements may not significantly predict effect size for student outcomes, it is possible that rather than the quantity of trauma-informed elements, the specific combination of trauma-informed elements is what truly makes a difference. For example, if the only trauma-informed element included is professional development for school staff, it is unlikely that student-related outcomes would change substantially. However, if practice change is the only trauma-informed element included, it would be much more likely that student-related outcomes would change because the practices would be directly implemented with the students. In both cases, the total number of trauma-informed elements is one, but different outcomes would be expected. Though there were insufficient studies to test this hypothesis statistically, qualitatively, there is some indication of this pattern.

One longitudinal study measuring student-related outcomes included only workforce professional development (Gonshak, 2012), and its aggregate effect size was small (Hedge's g = -0.10, p = .56). However, three studies (I. Collins, 2021; Mendelson et al., 2015; Powell & Bui, 2016) also included only one trauma-informed element (practice change), but their aggregate effect size was moderate, though not significant (Hedge's g = 0.57, p = .06). Additionally, compared to the study that included only workforce professional development, the studies that included workforce professional development and practice change (Ijadi-Maghsoodi et al., 2017; Judge, 2018; Lipscomb et al., 2021; Opiola et al., 2020; Razza et al., 2019), appeared to have a slightly larger aggregate effect size (Hedge's g = 0.65, p < .001).

Overall, it can be concluded that the effectiveness of trauma-sensitive schools in improving staff outcomes is not related to the number of trauma-informed elements it contains. Future research should consider using a Multiphase Optimization Strategy (MOST) within each specific domain of staff outcomes. This is a three phase approach to developing and evaluating interventions that focuses on efficient selection of active components of an intervention and refining the dosage prior to conducting a randomized control trial (L. Collins et al., 2007). Testing specific combinations of trauma-informed elements may help parse out which elements in what combination and at what does have the maximum impact for the least cost. However, the current findings indicate that the number of trauma-informed elements alone are not helpful in understanding intervention effectiveness among staff outcomes. Thus, other factors should be considered such as whether the intervention used an evidence-based curriculum, whether staff received continued coaching, and the quality and fidelity of implementation.

Aggregate effect sizes of combined staff outcome domains also did not vary based on the presence or absence of practice change nor the presence or absence of organizational change. As there was only one study that did not include workforce professional development (Alexander, 2021), moderation analysis was not possible for this element. However, once again, these analyses were conducted using all effect sizes across outcome domains. Thus, one possible explanation for the null finding is that outcome domain may moderate the relationship between the presence/absence of any given trauma-informed element and effect size. For example, it is possible that the presence or absence of organizational change would be a significant moderator of effect

sizes among studies measuring staff behaviors but not among studies measuring staff knowledge.

Within the domain of staff attitudes (the only outcome domain with enough longitudinal studies to analyze the impact of including each trauma-informed element), attitudes changed equally in interventions that included practice change and interventions that did not include practice change. Staff attitudes also improved equally following interventions that included organizational change and interventions that did not include organizational change. There were insufficient studies to conduct formal moderation analyses within the remaining staff outcome domains.

Overall, it may be concluded that the presence or absence of trauma-informed practice change and the presence or absence of trauma-informed organizational change is not related to intervention effectiveness. Theoretically, it is likely that effect sizes would vary based on the presence or absence of trauma-informed professional development, but this hypothesis was not able to be empirically tested given that nearly all studies included trauma-informed professional development.

4.3 Research Question Three: What is the Ideal Dose of Intervention?

The hypothesis that dose of trauma-informed professional development would impact staff outcomes was not supported. The samples of studies measuring student outcomes and school climate were too small to test the impact of dosage of professional development. Specifically, when comparing interventions that provided less than eight hours of professional development to studies that provided between eight and 40 hours, staff outcomes were the same. While it is possible that dose of professional development

actually has no impact on staff outcomes, it is also possible that the coding used was not specific enough. For example, perhaps studies that included only one hour would have different outcomes than studies that provided a full day. It is also possible that the density of the dose is important, and that was not captured in the present study. For example, a study that provided one week (i.e., 40 hours) of training received the same code as a study that provided 40 hours of training spread out over the course of 6 months. Indeed some literature related to teacher professional development indicates that dense delivery of professional development produces modest improvements in outcomes compared to less dense delivery (Landesman et al., 2011).

4.4 Limitations

It is important to note that the methodology of the included studies severely limits the ability to draw strong conclusions about the impact of trauma-sensitive schools. First, the most common study design used is a single-group pretest-posttest design. Although this does allow one to measure gains in an outcome, it precludes conclusions of causality due to the lack of a control condition. Further, 38% of all effect sizes were generated from measures that were either unvalidated or had mixed results regarding validity. This makes it difficult to draw strong conclusions because the studies using unvalidated measures may not be accurately assessing the constructs they are trying to assess. Moreover, given that the goal of meta-analysis is to establish consensus in a given field, validated measures are crucial because they are comparable to other validated measures of the same construct. Thus, the use of unvalidated measures means that the results of one study may not be comparable to another. Additionally, 29% of studies either did not

report the internal consistency of their measures or reported internal consistency below acceptable limits. This is problematic as well because the use of these measures may produce inconsistent results over time.

The substantial heterogeneity of the studies included also limits the ability to draw strong conclusions. Given that this is the first meta-analysis of its kind, breadth was prioritized in inclusion criteria. Thus, to be considered a trauma-sensitive school intervention, the only criteria that a study had to meet was calling itself "trauma-sensitive" or some variation of that term (e.g., trauma-responsive, trauma-aware, trauma-informed). The advantage of this approach is that it increases sample size, but the disadvantage is that the heterogeneity is so substantial that meaningful comparison is limited. There were studies in this meta-analysis that consisted of a day-long teacher workshop, and there were studies that implemented school-wide social-emotional learning curriculum, provided in-the-moment teacher coaching, and provided intensive trauma-focused therapy for its students. Though technically both studies operate under the framework of a trauma-informed approach, their methods are so vastly different, it is difficult to meaningfully compare them.

Not only is there substantial heterogeneity of approaches to trauma-sensitive interventions, but it is also likely that there was heterogeneity in implementation quality. Effectiveness of school-based interventions is typically affected by implementation quality characteristics such as administrator support of the intervention, staff enthusiasm and self-efficacy for implementation, and fidelity to the intervention (J. A. Durlak & DuPre, 2008). Other important contextual factors are also likely to influence implementation quality and sustainability including the presence of competing initiatives,

district priorities, and length of intervention implementation (Lendrum et al., 2013). Because the quality of implementation was not formally evaluated in most studies included in this meta-analysis, it was not possible to determine the extent to which effect sizes varied based on implementation quality.

Finally, due to the high-inference nature of coding required for this study, future attempts at meta-analysis of trauma-sensitive school-based interventions would benefit from having a team of coders. This would add another layer of reliability to the results obtained. Though the present study demonstrated strong intra-rater reliability, it is possible that additional coders would have made different inferences. For example, school suspensions were coded as student externalizing behavior for the purposes of this meta-analysis. However, school suspensions are also related to school safety, chaos, and culture. Thus, it would have been possible to code school suspensions as a measure of school climate.

4.5 Implications and Future Directions

Despite the significant limitations of this meta-analysis, the results of this dissertation made an important contribution to the existing literature on trauma-sensitive schools by acting as the first quantitative synthesis of the existing literature. Quantitatively demonstrating the overall utility of a trauma-informed approach in schools is a crucial step in advancing trauma-informed care. Though substantial methodological limitations exist in the current literature, this comprehensive meta-analysis demonstrates that school staff, school climate, and students demonstrate significant improvements after

participating in a trauma-sensitive school. This has important implications for practice, policy, and future research.

In practice, trauma-sensitive schools appear to be able to meet the needs of their students and staff. Schools that implement trauma-sensitive interventions can expect to see reductions in student behavior problems and internalizing symptoms as well as increases in their general wellness. While it cannot yet be stated that trauma-informed schools are better able to meet the needs of their students than non-trauma-informed schools, it may be said that trauma-informed schools are better able to meet the needs of their staff than non-trauma-informed schools. Schools that implement trauma-informed interventions can expect to see improvements in staff understanding of childhood trauma, and increased responsiveness to their students. When considering an appropriate framework to guide the vision, mission, and goals of a school, a trauma-informed approach is a viable option.

This meta-analysis also provided mixed evidence about changes in school climate following trauma-informed interventions. School staff tended to report that school climate improved following implementation of a trauma-informed approach, but students did not. Further, when compared to schools that were not trauma-sensitive, schools that were trauma-sensitive had equivalent ratings of school climate. Thus, trauma-informed approaches may not be the most effective approach to improve school climate, particularly given the thorough research on existing interventions that are highly effective in improving school climate including schoolwide positive behavioral interventions and supports (SWPBIS) and social and emotional learning interventions (Charlton et al., 2021). However, most studies that measured school climate in this meta-analysis

measured constructs such as staff relationships, staff-student relationships, classroom organization, and physical spaces in the school. General school safety or chaos was not typically measured. Therefore, future studies should explore whether trauma-sensitive interventions improve school safety.

One potential policy implication is continued investment in trauma-informed education. Given that the results of this meta-analysis indicate that trauma-sensitive schools benefit school communities, continued investment in this approach may be beneficial. However, more research is needed to determine exactly how much investment should occur depending on which outcomes a school is hoping to achieve. This meta-analysis was unable to parse out differences in outcomes based on dosage, density, and complexity of intervention. Moreover, only two studies (Alexander, 2021; Pobuk, 2019) included in this meta-analysis provided cost estimates for at least a portion of the intervention. Thus, future research should consider discussing the cost of intervention and including an analysis related to the social return on investment. With this information, schools and districts would be equipped with the knowledge they need to appropriately select trauma-informed interventions for their schools. In the meantime, schools should proceed cautiously when determining the time and financial commitment required for a trauma-sensitive intervention.

Another policy implication of the current findings is that emphasizing prevention and early intervention is a worthwhile endeavor. Given that the majority of the studies included in this meta-analysis yielded significant effects from universal samples and did not just target students with known trauma histories or psychopathology, it is clear that a trauma-informed approach is beneficial for all. This type of prevention is crucial for

schools wanting to address potential trauma-related concerns before they develop into serious issues.

Implications and future directions for research include prioritizing methodologically rigorous research on the effectiveness of trauma-informed approaches in schools. Specifically, future research could utilize multiple baseline designs or step-wedge designs using validated and reliable instruments. Additionally, the scope of this study was broad, and the sample was small. Thus, important relations were not examined in part due to small power and in part to maintain a focus on the original research questions. Specifically, the question of density of professional development, and specific combinations of trauma-informed elements within more specific outcome domains would benefit from further study. The question of implementation quality should also be addressed in future research. Given that previous research suggests that schools are likely to implement interventions with low fidelity (Durlak & DuPre, 2008), understanding the minimum fidelity necessary to achieve positive gains with trauma-informed interventions is important. Similarly, future research would benefit from elucidating specific barriers to implementation of trauma-informed interventions in schools. It is also possible that trauma-informed interventions have different outcomes depending on individual-level moderators such as student age, gender and race. Indeed, some research on social emotional learning interventions and SWPBIS have demonstrated significant differences in effects based on gender (Raimundo et al., 2013; Wienen et al., 2019). Therefore, it is possible that demographic moderators may also impact effect sizes of trauma-informed interventions as well. Once equipped with this information, school districts can make

more informed decisions about which type of trauma-informed intervention to implement based on their schools' characteristics.

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BIOGRAPHICAL SKETCH

Name of Author: Megan Ann Blanton

Graduate and Undergraduate Schools Attended:

Catawba Valley Community College, Hickory, North Carolina
University of North Carolina at Chapel Hill, Chapel Hill, North Carolina
Fordham University, New York City, New York
University of South Alabama, Mobile, Alabama

Degrees Awarded:

Associate of Arts, 2010
Catawba Valley Community College

Bachelor of Arts in Psychology, 2014
University of North Carolina at Chapel Hill

Master of Science in Teaching Exceptional Adolescents Grades 7-12, 2016
Fordham University

Master of Science in Psychology, 2020
University of South Alabama

Doctorate of Philosophy in Clinical and Counseling Psychology, 2023
University of South Alabama

Awards and Honors:

Jennifer Langhinrichsen-Rohling Clinical Services Award, 2021
University of South Alabama

Psi Chi National Psychology Honor Society Member, 2019-2023
University of South Alabama

Phi Kappa Phi Honors Society Member, 2017-2018
Fordham University

Segal AmeriCorps Education Award, 2014-2016
Teach for America

Publications

Basu, N., **Blanton, M.A.,** Gonzales, J., Hendricks, K.E, Fadoir, N.A., Mehari, K., & Smith, P.N. (In press). Upward Social Comparisons and Suicidal Ideation on Facebook: Moderating Role of Thwarted Belongingness. *Psychology of Popular Media.*

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