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Outside, Looking In: A Dissertation on Mindful Walking and How Green Exercise Affects State Mindfulness and Connectedness to Nature

Dustin Wyatt Davis

OUTSIDE, LOOKING IN: A DISSERTATION ON MINDFUL WALKING AND HOW GREEN EXERCISE AFFECTS STATE MINDFULNESS AND CONNECTEDNESS TO NATURE

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ABSTRACT

INTRODUCTION: Mindfulness, green exercise, and connectedness to nature are increasingly popular topics among academics and the public. These three topics overlap in the underexplored area called *mindful green exercise*. Mindful green exercise is a blend of *mindful exercise* and *green exercise*. Mindful exercise is physical exercise during which people pay attention on purpose without judgment to each new present moment. The person applies an accepting awareness to internal phenomena (thoughts, emotions, and bodily sensations) and external phenomena (objects and events in the environment). Green exercise is exercise performed outdoors in natural environments. Despite its name, green exercise does not only occur in green natural environments. Studies have investigated the effects of mindful exercise and green exercise only modestly and without investigating possible interactions between the two types of exercise. Early evidence suggests that each type may independently improve mental and cardiovascular health in various populations. However, not all the evidence points to this conclusion, and the relationships among mindfulness, green exercise, and connectedness to nature are obscure. Additionally, practical barriers limit the broader appeal of mindful exercise and green exercise in the United States. The greatest barriers to participation are preconceived notions, unfamiliarity with mindful exercises, and many green exercises being vigorous. Considering the obscurity and barriers, the author conducted the present dissertation to achieve one overall purpose and three specific aims. The overall purpose was to expand what is known about mindful exercise and green exercise and how to measure state mindfulness and connectedness to nature. The first specific aim was to determine the effects of meditative and mindful walking on mental and cardiovascular health (Study 1). Studying meditative and mindful walking is essential because they have been researched less than the more popular qigong, tai chi, and yoga. Moreover, walking is a familiar and low-intensity exercise. Consequently, meditative and mindful walking are probably more accessible and appealing than qigong, tai chi, and yoga in the United States. The second and third specific aims were to determine the effects of sitting and walking in green space on state mindfulness (Study 2) and connectedness to nature (Study 3), respectively. Another part of Studies 2 and 3 was testing the concurrent validity and 24-hour test-retest reliability of novel measures of state mindfulness and connectedness to nature. For mindfulness, the Visual Analog Scale-Mindfulness (VAS-M) and State Mindfulness Scale for Physical Activity (SMS-PA) were tested against the State Mindfulness Scale (SMS) as the criterion (Study 2). For connectedness to nature, the Visual Analog Scale-Nature (VAS-N) was tested against the Love and Care for Nature Scale (LCN) as the criterion (Study 3). The last part of Study 3 was determining whether state mindfulness and connectedness to nature are associated with each other during green exercise (Study 3). **METHODS:** The first specific aim was achieved in Study 1 by conducting a systematic review without a meta-analysis according to the Preferred Reporting Items for Systematic Reviews and Meta-Analyses (PRISMA) guidelines. The second and third specific aims in Studies 2 and 3, respectively, were achieved by conducting randomized crossover studies. The studies had convenience samples comprised of faculty, students, and community members of two universities in the Western United States. **RESULTS:** The systematic review revealed that meditative and mindful walking significantly improve mental and cardiovascular health outcomes. However, it is unclear whether the improvements are clinically meaningful. The evidence comes from a small group of randomized controlled trials (RCTs) with a high risk of bias and from studies that were uncontrolled and non-randomized. The studies had a high degree of heterogeneity among their populations, interventions, comparators, outcomes, settings, and study designs. This heterogeneity precluded a meta-analysis and relegated the studies to a narrative synthesis. The present dissertation's studies showed that acute sitting and walking in green space significantly increased state mindfulness and connectedness to nature. The dissertation offers initial evidence that support the concurrent validity of the VAS-M with the SMS. The VAS-M and SMS scores increased similarly, but the correlations had wide 95% confidence intervals (CIs). The dissertation also offers evidence that support the concurrent validity of the SMS-PA with the SMS after green exercise. The VAS-M and SMS-PA scores correlated significantly, strongly, and positively. There was no evidence to support the test-retest reliability of the SMS before or after sitting or walking. There was evidence to support the test-retest reliability of the VAS-M and SMS-PA after walking. For connectedness to nature, there was evidence to support the concurrent validity of the VAS-N with the LCN before and after sitting and walking. No evidence was found to support the test-retest reliability of the LCN and VAS-N before or after sitting or walking. State mindfulness and connectedness to nature correlated significantly, moderately, and positively after sitting and walking. **CONCLUSIONS:** Meditative and mindful walking are promising types of mindful exercises because they improve mental and cardiovascular health outcomes, sometimes better than active control treatments (i.e., non-mindful, traditional walking). Meditative and mindful walking interventions in the literature vary starkly, and clear descriptions of the interventions are sparse. Well-defined interventions are needed so that robust RCTs can investigate them further to corroborate or contradict the original findings. After generating a critical mass of RCTs, researchers should conduct meta-analyses on specific interventions in specific populations. Such meta-analyses will determine if the population-specific interventions improve outcomes statistically and clinically better than control conditions. At a minimum, a handful of robust meta-analyses are required before recommending

specific meditative and mindful walking interventions in physical activity guidelines. New RCTs and meta-analyses should also investigate the effects of green exercise on state mindfulness, connectedness to nature, and health. The present dissertation showed that 10 minutes of sitting and 10 minutes of non-mindful, traditional walking in green space increased state mindfulness and connectedness to nature acutely. It is unclear how long the increases lasted or whether they affected the participants' mental or cardiovascular health. Other studies have shown that mindfulness practices and green exercise independently improve mental and cardiovascular health. Advisable next steps in the research are 1) clarifying the relationship between state mindfulness and connectedness to nature, and 2) determining the effects of acute sitting and walking in green space on mental and cardiovascular health outcomes. When conducting studies on these topics, researchers should deliberate on how to measure state mindfulness and connectedness to nature. In the context of the present dissertation, the SMS and LCN (the criterion measures) did not appear to be test-retest reliable across approximately 24 hours. There was evidence to support the concurrent validity of the VAS-M and SMS-PA with the SMS. There was also evidence to support the test-retest reliability of both the VAS-M and SMS-PA after walking. While the evidence supported the concurrent validity of the VAS-N with the LCN, neither scale appeared to be test-retest reliable across approximately 24 hours. Researchers should investigate the criterion and novel measures further before trusting them to be valid and reliable.

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DEDICATION

I dedicate this work to the students who are struggling. I am one among you who questions his abilities, worth, and path in life. Though we are always works in progress, we are inherently enough. Each of us holds the power to forge our path, persevere, and transmute hardship and pain into growth. Our path is right as long as we act from a place of goodness and kindness. Each of us must take the best action we can in every moment and reflect. Then, we must again take the best action we can. We can always do our best with our current situation and turn obstacles into opportunities. When people and events thwart our efforts, we can choose another good action to keep advancing. No person or event, however powerful or intimidating, can stop us from doing this at every moment. If at any point along our journey we need help, we will take it without shame. Humans are on this earth for cooperation. Lastly, we will love ourselves and love others. Goodness, kindness, action, reflection, cooperation, and love are actionable values that keep us growing forward.

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CHAPTER 1: INTRODUCTION

"There are moments when all anxiety and stated toil are becalmed in the infinite leisure and repose of nature" (Thoreau, 1849). —Henry David Thoreau

Written nearly 175 years ago, Thoreau's words still resonate with many people who pass time in nature. Nature may promote tranquility in humans because they evolved there. Nature was the birthplace of the first modern humans, *Homo sapiens*, at least 200,000 years ago (Hublin et al., 2017; Richter et al., 2017). For most of the species' history since then, humans have lived predominantly outdoors in nature. This fact is easy to forget because so many people now live indoors in cities. As of 2020, 80% of the U.S. population lives in urban areas (United States Census Bureau, n.d.). The percentages of people living in urban areas are similar in other populous countries that are rated highly on the Human Development Index: Australia (86%), Canada (82%), the European Union (75%), Japan (92%), and South Korea (81%) (The World Bank Group, n.d.-g, n.d.-b, n.d.-c, n.d.-d, n.d.-e, n.d.-f). There certainly are countries in which many people live rurally, but city living is more common now than in early human history.

Besides the high prevalence of city living, another fact often forgotten is that early humans often had more difficult lives than humans alive today. The lack of modern buildings and cities exposed the earliest *Homo sapiens* to the perils of nature. This harsh exposure made it more difficult for those humans than modern humans to survive and propagate. This fact might explain partly why *Homo sapiens*' existence and growth were tentative and slow for a few hundred thousand years until the rise of agriculture around 12,000 years ago (National Geographic Society, n.d.). This watershed in human history ignited the species' expansion.

Growth accelerated exponentially, bringing untold technological advancement. These advancements led to the First Industrial Revolution in the late 18th and early 19th centuries. What followed were the Second Industrial Revolution, Third Industrial Revolution (Digital Revolution), and the ongoing Fourth Industrial Revolution. These phases of industrial change have spurred innovations that have for many people made life easier, safer, healthier, and longer than for the earliest humans.

The innovations that have improved the average human life have also transformed how people pass their time compared to early humans, particularly in the United States. Whereas early humans lived predominantly outdoors, a study in 2001 reported that people in the United States were indoors approximately 87% of each day (Klepeis et al., 2001). Besides being indoors often, many U.S. adults also have chronic physical inactivity (CPI) during leisure. Early humans did not have the same amenities of modern living that reduce the need to labor for food, water, shelter, and transportation. As of 2018, about four in five U.S. adults were not meeting the minimum recommendations for aerobic physical activity and muscle-strengthening activities (U.S. Department of Health and Human Services, 2018). The recommendations are that every week adults complete at least 150 minutes of moderate-intensity or 75 minutes of vigorous-intensity aerobic physical activity. Adults should also complete whole-body muscle-strengthening activities on at least two nonconsecutive days per week (U.S. Department of Health and Human Services, 2018). The prevalence of CPI has not improved dramatically since 2018. As of January 2022, one in four people in the United States reported not engaging in physical activity outside of work (Centers for Disease Control and Prevention, n.d.-b). Beyond often being indoors and inactive, another difference between early and modern humans is the latter's use of digital devices, such as TVs, tablets, computers, and smartphones. One estimate is that U.S. adults use these devices for over 10 hours daily (The Nielsen Company, 2021). The duration of time that modern humans pass indoors, inactive, and on digital devices sets them apart from their ancestors. These transformations reduce how much time modern humans spend in nature, potentially robbing them of the tranquility that Thoreau felt and so eloquently expressed.

The reasons for modern humans so often being indoors, inactive, and on digital devices are likely myriad and complex. The three characteristics may be related. For example, being indoors and having access to digital devices may create more opportunities for sedentary behaviors. Potential relationships like this one are intriguing. However, claiming broadly that any of the characteristics causes another would require strong scientific evidence that does not yet exist. The characteristics are nonetheless concerning, especially CPI, which contributes greatly to the incidence of chronic disease and mortality (Bull et al., 2020; U.S. Department of Health and Human Services, 2018; World Health Organization, 2020).

One measure that expresses how much a risk factor such as CPI contributes to the incidence of chronic disease and mortality is the population attributable fraction (PAF) (Powell & Blair, 1994; Rockhill et al., 1998). The PAF of a risk factor estimates what percentage of new cases of a disease would be prevented if that factor was eradicated (Lee et al., 2012). In the United States, the PAF of CPI is 6.7% (95% confidence interval (CI) [2.5, 11.1]) for coronary heart disease, 8.3% [4.2, 12.9] for type 2 diabetes, 12.4% [5.8, 19.2] for breast cancer, 12.0% [6.7, 17.4] for colon cancer, and 10.8% [8.6, 13.1] for all-cause mortality (Lee et al., 2012). These data suggest there would be fewer cases of major chronic diseases and death if more people met the physical activity guidelines. The veracity of this claim as it relates to all-cause mortality is supported by analyses of dose-response relationships between that outcome and physical activity and sedentary time measured by accelerometry. Based on data from eight studies (*N* = 36,383 participants, 72.8% female) with a median follow-up of 5.8 years and 2,149 deaths (5.9%), any physical activity of any intensity was associated with a lower risk of all-cause mortality (Ekelund et al., 2019). Participants were organized into quartiles for physical activity and sedentary time. For physical activity, quartile one (referent group) and quartile four were the least and most active, respectively. The odds of all-cause mortality in quartile four during the study period were 0.27, 95% CI [0.23, 0.32]. For sedentary time, quartile one (referent group) and quartile four were the least sedentary and most sedentary, respectively. The odds of all-cause mortality in quartile four during the study period were 2.63 [1.94, 3.56] (Ekelund et al., 2019). These data indicate that people with CPI (i.e., sedentary people) are more likely to die over time than their active counterparts.

Given the high prevalence of CPI and its relationship with the incidence of chronic diseases and mortality, public health efforts in the United States should prioritize increasing physical activity. U.S. public health is facing an immense burden of chronic diseases. Around one million people in the United States die annually from cardiovascular, cerebrovascular, and metabolic diseases (Xu et al., 2021). A positive risk factor for each of these diseases is CPI (Bauer et al., 2014; Booth et al., 2012; Knight, 2012; Pratt et al., 2014). Critically, CPI is a modifiable lifestyle habit. Going from chronically inactive to exercising regularly can reverse some conditions that precipitate the aforementioned chronic diseases (Egan & Zierath, 2013). Combining regular aerobic and resistance exercise can improve the ratio of skeletal muscle to body fat, metabolic inflexibility, and low cardiorespiratory and muscular fitness (Egan & Zierath, 2013). This means that regular exercise may not just decrease the incidence of new cases of chronic disease but alleviate the existing disease burden. Doing so is important because millions of people survive each year with chronic diseases such as cardiovascular diseases (CVDs). From 2015–2018, nearly 127 million (49.2%) U.S. adults aged ≥ 20 years had a CVD when hypertension was included. Excluding hypertension, the prevalence of CVDs was 26 million (9.3%) (Tsao et al., 2022). The burden of CVDs is enormous. Data from 2017–2018 indicate that CVDs caused $378 billion annually in direct expenditures for treatment and nearly $152 billion annually in indirect costs via lost productivity (Tsao et al., 2022). For many people, there are personal costs beyond health care expenses and lost wages, such as a lower quality of life and more years lived with disease.

A good mental image of what people experience with chronic diseases is a pie, where the whole pie represents the total years lived (years without diseases + years with diseases). As life expectancy in the United States grew over the last several decades, the figurative pie for the population grew (more people lived more years) (Arias et al., 2022). However, the pie stopped growing in 2019 and shrunk through 2021 (Arias et al., 2022). Life expectancy declined by 2.7 years, meaning more people are living shorter lives (Arias et al., 2022). The pie is now smaller, and the slice representing years lived with chronic diseases will probably keep growing (Anderson & Durstine, 2019). These data illustrate the direness of the U.S. population's physical health (Figure 1).

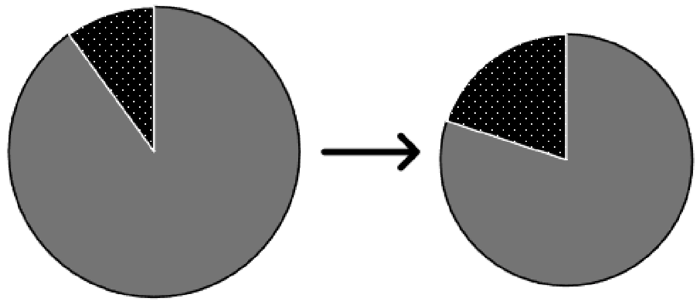

Figure 1. Illustration of the undesirable scenario in which the population life expectancy decreases (size of entire pie) and the number of years lived with at least one chronic disease increases (size of the black dotted slice). The solid gray slice is the number of years lived without at least one chronic disease. The figure is strictly illustrative and does not show data for a real population.

So far, this dissertation has used the term *chronic diseases* to mean physical health conditions that affect the body's normal physiological functioning. Some of these conditions are CVDs, cancer, diabetes, and stroke (Centers for Disease Control and Prevention, n.d.-a). A concerning fact is that these conditions are not an isolated risk to the U.S. population's health because many people also struggle with mental health conditions. These conditions can exist concomitantly with chronic diseases. At least one in five U.S. adults (53 million) have mental health conditions (National Institute of Mental Health, n.d.), and underreporting may mask the actual number of people afflicted. Some people do not seek treatment because of the stigma and cost. In 2019, medical spending to treat mental health conditions in the U.S. population totaled nearly $107 billion (Soni, 2022). A person's health care needs may be greater when they have both a chronic disease and mental health condition. A 2019 cohort study in Canada compared medical spending between people with only a chronic disease and people with both a chronic disease and depression, schizophrenia, or an alcohol or drug abuse disorder (Sporinova et al., 2019). People in the latter group spent almost 16,000 adjusted Canadian dollars (CAD) more on health care than those without such conditions; 38,250 CAD 95% CI [36,476, 39,935] vs. 22,280

CAD [21,780, 22,760], $p < 0.001$ (Sporinova et al., 2019). People in that group also visited the emergency department more often (3.75 vs. 1.75 times per 1,000 patient-days, $p < 0.001$), were hospitalized more often (0.88 vs. 0.43 times per 1,000 patient-days, $p < 0.001$), and were hospitalized for longer (11.6 vs. 4.7 days over three years, $p < 0.001$) than people with only a chronic disease (Sporinova et al., 2019). The prevalence and cost of chronic diseases and mental health conditions warrant innovative approaches to address both issues simultaneously in the United States. New approaches should aim to modify habitual lifestyle behaviors, or habits.

One promising approach is modifying two common habits among the U.S. population, being indoors most of the day and CPI. The first problem with being indoors so much is that buildings are enclosed artificial spaces. These spaces are distinguishable from the outdoors and disconnect people from nature physically and mentally. College students, who spend considerable time indoors, report thinking about being disconnected from nature (Taylor, 2019). The second problem is that being indoors typically confines people to a smaller space than the outdoors and creates opportunities for sedentary activities (e.g., sitting, and stationary work). Being sedentary is positively associated with the risk of anxiety and depression (Allen et al., 2019; Huang et al., 2020; Teychenne et al., 2015) and negatively associated with psychological well-being across the lifespan (Biddle et al., 2019; Cunningham et al., 2020; Galper et al., 2006; Rodriguez-Ayllon et al., 2019). The prevailing sedentary activity of consuming media via digital devices poses problems too. Screen time is positively associated with anxiety, depression, and low psychological well-being among U.K. adults (Smith et al., 2020). Consuming digital media is negatively associated with psychological well-being (Scott et al., 2017; Twenge & Campbell, 2018), especially among children and adolescents (Twenge & Martin, 2020). Among adolescents, media consumption is negatively associated with anxiety and depression (Twenge &

Campbell, 2018; Twenge & Farley, 2021). The associations among CPI, digital media consumption, and psychological well-being persisted during the COVID-19 pandemic (Ali & Kunugi, 2020; Werneck et al., 2021). The data collected during the pandemic give empirical support for the mental hardship many people experienced.

During much of the COVID-19 pandemic, people practiced social distancing through stay-at-home orders, quarantining, and isolating. Some of the population began telecommuting indefinitely for school or work. These adaptations were defining experiences of living through the pandemic. As essential and justified as these measures were for mitigating the spread of COVID-19, they restricted movement and in-person socializing. Many people spent more time at home than they ever did pre-pandemic. During stay-at-home orders in 2020, the feeling of being stuck inside was palpable. How people passed the time varied, but a widespread trend was apparent: people became more physically inactive than during the pre-pandemic period (Alfawaz et al., 2021; Dunton et al., 2020; Meyer et al., 2020; Silva et al., 2021; Spence et al., 2021). The use of digital devices also seems to have risen precipitously, based on data usage. Network traffic to popular websites exploded during the pandemic (Labovitz, 2020). People watched more Netflix (Chandler, 2020; *Netflix Gets 16 Million New Sign-Ups Thanks to Lockdown*, 2020) and accessed Facebook, TikTok, and YouTube more than ever before (Cohen & Dodgson, 2021; Isaac & Frenkel, 2020; Koeze & Popper, 2020; Romero, 2020). The drastic increases in time indoors and CPI triggered by COVID-19 may partly explain the high prevalence of mental health conditions during the pandemic (de Sousa et al., 2021) that worsened in some populations (Vahratian et al., 2021; Winkler et al., 2020). The data collected during the pandemic do not prove that being inside, chronically inactive, and using digital devices worsen mental health. Several other factors could explain the worsened mental health conditions during the pandemic, including the fear of contracting COVID-19, becoming unemployed, or losing a family member. Still, the data underscore the mental health crisis and that being excessively indoors, chronically inactive, and on digital devices are related to mental health conditions.

Considering the scientific literature cited thus far, a promising approach to addressing the U.S. population's burden of chronic diseases and mental health conditions is getting people outdoors and more physically active. The hypothesis is that spending time outdoors and being physically active increases physical and mental health. This hypothesis is tested regularly in the field of *green exercise* (GE). The GE literature is nascent but has grown exponentially in the last decade. In PubMed, the earliest article with "green exercise" in the title was published in 2005 (Pretty et al., 2005). Only two other articles in PubMed had "green exercise" in the title between 2005 and 2010. Since 2011, 29 articles with "green exercise" in the title have been stored in PubMed. Of the 29 articles, 11 (38%) were published in 2019 amid the COVID-19 pandemic. The spike in articles about GE illustrates its growing popularity and the rising interest in GE as an approach to improve chronic diseases and mental health conditions.

The field of GE is named after a term coined by Pretty et al. (2003). This group originally defined GE as "physical activities whilst at the same time being directly exposed to nature" (Pretty et al., 2003, 2005). The term's definition has been dynamic since that point. Barton and Pretty (2010) defined GE as "activity in green places (in the presence of nature)," and Gladwell et al. (2013) defined it as "exercising whilst being exposed to nature." A few years later, Calogiuri et al. (2016) defined GE as "any PA [physical activity] taking place in natural environments." In their 2019 systematic review, Lahart et al. (2019) used GE to mean "physical activity in the presence of nature," "exercise in the presence of nature," and "exercising in an outdoor natural environment." The group also discussed virtual GE, which they described as indoor exercise while watching a video of nature on a screen or with virtual reality technology (Lahart et al., 2019). The GE researcher Robert Salatto recently stated that many definitions of GE are acceptable, and that GE can occur in various biomes, including coasts, forests, mountains, urban cities and parks, or any other outdoor environment (Salatto, 2021). In his most recent publication, Salatto et al. (2021) specified that "green exercise is performed outdoors in natural environments, but do [sic] not necessarily need to be in settings with a preponderance of green space." The present dissertation mostly aligns with Salatto's specification. An important difference between the earlier GE literature and this dissertation is that the latter does not use "physical activity" and "exercise" interchangeably in its definition of GE. Physical activity and exercise are defined differently in the broader field of exercise science.

Physical activity is defined as "any bodily movement produced by the contraction of skeletal muscles that results in a substantial increase in caloric requirements over resting energy expenditure" (American College of Sports Medicine, 2017a, p. 1; Caspersen et al., 1985). Not all physical activity is exercise. Exercise is "a type of PA [physical activity] consisting of planned, structured, and repetitive bodily movement done to improve and/or maintain one or more components of physical fitness" (American College of Sports Medicine, 2017a, pp. 1–2; Caspersen et al., 1985). Given the differences between physical activity and exercise, the present dissertation does not define GE as merely outdoor physical activity. This dissertation's definition of GE is a blend of the published definitions: outdoor exercise while being exposed to nature, including any biome.

Presenting definitions of physical activity, exercise, and GE is important because the clarity helps differentiate exercise interventions. Further differentiation is possible by specifying and delivering exercise interventions by frequency, intensity, time, and type (American College of Sports Medicine, 2017b). This manner of prescribing exercise is called the FITT principle. Though the principle is already useful, it can be augmented by adding location (L) after the second T. Location is the exercise environment, a parameter that exercise professionals often overlook. Green exercise researchers recognize location as a variable worth considering when prescribing exercise. Location is not ineffectual and may modulate the effects of exercise.

The modulatory role of location is unclear because it has been studied and quantified less extensively than the roles of frequency, intensity, time, and type. The role of location will likely be clarified as the GE literature grows. The current literature shows that GE is enjoyable and changes people's inner experience of exercise. Pretty et al. (2007) reported that outdoor walking, cycling, and horse-riding among people in the United Kingdom increased self-esteem and mood acutely. Shin et al. (2013) reported that young South Korean women felt happier after walking in a forest than after walking in a gym. Nearly a decade later, Navalta et al. (2021) reported that undergraduate and graduate students felt calmer and more comfortable after sitting and walking in a mountainous green space than in a laboratory and outdoor urban area. The second and third studies show that people have different inner experiences during GE than indoor exercise.

Stronger positive emotions after GE than indoor exercise is an early sign that location is a worthy exercise parameter. These early findings are promising and warrant more studies on GE as a way to access nature's restorative benefits (Berman et al., 2008; R. Kaplan, 1973; S. Kaplan, 1983, 1993, 1995; Loewe, 2022). For some people, nature may restore the mind and body in ways that the indoors does not. An excellent introduction to the scientific studies that support this claim is environmental journalist Emma Loewe's book, *Return to Nature: The New Science of How Natural Landscapes Restore Us* (Loewe, 2022). Each chapter of her book explains the latest scientific studies on the benefits of different natural environments. Each chapter is dedicated to certain areas (e.g., one chapter to parks and gardens and another chapter to forests and trees). In every chapter, Loewe ties the scientific studies to the two leading theories that explain why people feel more positive and restored in nature than in artificial indoor environments such as buildings.

The first and more popular of the two leading theories is the attention restoration theory (ART) (Loewe, 2022). The ART, proposed by environmental psychologists Rachel and Stephen Kaplan (Berman et al., 2008; R. Kaplan, 1973; S. Kaplan, 1983, 1993; Loewe, 2022), contends that artificial indoor environments are frenetic and thus distracting and cognitively demanding. For example, someone in an office may be directing their attention to overlapping activities, conversations, and notifications from emails, texts, and phone calls. This environment and its activities deplete the person's mental bandwidth, leaving them feeling drained. Nature is a desperately needed respite that allows the person to recoup their cognitive resources and ability to focus. Nature uniquely facilitates this restoration because of its four qualities: extent, being away, compatibility, and fascination (S. Kaplan, 1995; Loewe, 2022). Extent is the greater size and grandeur of natural environments compared to artificial indoor environments. Being away is the idea that natural environments are often physically separate spaces from the spaces where attention is being sapped. Compatibility is natural environments being congruent with what people expect of them. And fascination is natural environments giving people a single object on which to focus their attention. Fascination encourages people to stop multitasking and rest their attention on one object (S. Kaplan, 1995; Loewe, 2022).

The second of the two leading theories is the stress reduction theory (SRT), proposed by Roger Ulrich (Loewe, 2022; Ulrich, 1983). Whereas ART theorizes that nature reduces cognitive load and restores cognitive resources, SRT theorizes that nature reduces stress and causes positive emotions (Loewe, 2022; Ulrich, 1983). The proposed mechanism is that nature offers vast areas for resting in, viewing, and admiring a natural landscape. Natural environments are conducive to rest because they often contain vital natural resources such as water and shelter (e.g., trees). Humans' evolutionary ancestors and every member of the species *Homo sapiens* have depended on these resources for survival. This environmental pressure instilled in humans a powerful biological drive to seek natural resources. The SRT contends that today's humans remain driven toward these resources and, after seeing or obtaining them, are rewarded with positive feelings that include safety and relaxation (Loewe, 2022; Ulrich, 1983). The intent of introducing an overview of ART and SRT was to provide a theoretical justification for studying the effects of GE.

Another important exercise parameter is emphasis (E). Like location, emphasis is understudied and usually not considered. The definition of emphasis is something that is given special importance (*Emphasis Definition & Meaning*, n.d.). This dissertation defines emphasis during exercise as giving special mental importance to the present moment. Practically, this means maintaining a moment-to-moment, non-judgmental awareness of the internal and external environments (Kabat-Zinn, 1994, 2015). The internal environment is one's thoughts, emotions, and bodily sensations. The external environment is one's surroundings, including people, plants, animals, non-living objects, and ongoing activities. By focusing on the immediate internal and external environments, special importance is given to them, hence the emphasis parameter.

Habitually refocusing one's attention on the present moment is challenging because it is not the prevailing way of being. People's minds tend to wander from the present moment during nearly all waking hours of the day (Killingsworth & Gilbert, 2010). This behavior is called stimulus-independent thought or mind-wandering (Killingsworth & Gilbert, 2010). Such behavior may be peculiar to humans and appears to be their brains' default state (Buckner et al., 2008; Christoff et al., 2009). It is troubling that wandering minds tend to ruminate on life's negative aspects (Killingsworth & Gilbert, 2010). The human mind's tendency to focus on the negative is called the negativity bias (Rozin & Royzman, 2001; Vaish et al., 2008). This bias assigns a greater cognitive weight to phenomena perceived as negative (Ito et al., 1998). The negativity bias persists without a conscious effort to change it. Conscious efforts to be more present-minded ameliorate the negativity bias (Kiken & Shook, 2011).

Mindfulness practices are one type of conscious effort to be more present-minded. The goal of these practices is cultivating mindfulness, a state of being aware and accepting one's present experience from moment to moment, non-judgmentally, non-reactively, and openheartedly (Kabat-Zinn, 1994, 2015). Mindfulness practices are conducted to train the mind to rest in the present moment with more acceptance and less wandering, judgment, and tension (Kabat-Zinn, 1994, 2015). The foundation of mindfulness practices is meditation, the practice of deliberate mindfulness: a person dedicates purposeful time to paying attention to their present experience (Kabat-Zinn, 2015). Paying attention to what passes through the mind is supposed to help people better understand their thinking patterns, behavioral habits, relationships, personal values, and life's meaning. Though mindfulness meditation is rooted in Eastern philosophy and traditions, Western health professionals have used mindfulness meditation as a complementary therapy (Bishop et al., 2004; Kabat-Zinn, 1994, 2003). Jon Kabat-Zinn first popularized mindfulness meditation in the United States as a form of medicine. He founded mindfulness-based stress reduction (MBSR), a program that decreases stress among imprisoned people, hospital patients, and medical students (Kabat-Zinn, 2003). Standard MBSR is a structured eight-week program, but there are adapted variations of different lengths (Kabat-Zinn, 2003).

Irrespective of its length, an MBSR program teaches mindfulness meditation and how to be mindful outside of meditation to feel calmer and more content. The effectiveness and popularity of MBSR explain the interest in other mindfulness-based interventions (MBIs). A shared goal among MBIs is to train people in mindfulness to alleviate chronic diseases and mental health conditions. The desire to know the most effective MBIs drives the research that blends mindfulness and exercise.

Recall that people spend most of their waking time mind-wandering (Killingsworth & Gilbert, 2010). While it appears that no published research has investigated mind-wandering during exercise, it is fair to presume that mind-wandering during exercise is the norm. Considering this presumption and the success of MBSR, a field of research has emerged to study the effects of blending mindfulness and exercise. The field of mindful exercise is motivated by the understanding that practicing mindfulness and exercising improve health independently (Egan & Zierath, 2013; Howarth et al., 2019; Rogers et al., 2017; Spijkerman et al., 2016). The central hypothesis of mindful exercise research is that mindfulness and exercise synergize to improve health better than either type of intervention alone. This hypothesized synergy motivates the study of mindfulness-based exercise interventions (MBEI).

The first step of introducing MBEIs is explaining mindful exercise. During mindful exercise, people exercise while maintaining “a profound inward mental focus” (La Forge, 2016). In other words, people pay inward attention on purpose to each new present moment without judging their experience. The goal during mindful exercise is to apply an accepting awareness to the thoughts, emotions, and bodily sensations without labeling them (e.g., “good” or “bad”). The accepting and non-judgmental awareness can be extended to the external environment. The mindful approach is different from the approach to non-mindful exercise, which lacks an intentional, contemplative, and profound inward focus. Mindful exercise emphasizes a deliberate allocation of attention while prescribing general guidelines on where and how to apply that attention.

If definitions draw a line between mindful exercise and non-mindful exercise, further explanation makes the line a groove. From beginning to end during a bout of mindful exercise, the exerciser practices mindfulness via a four-step cycle: 1) paying attention, 2) noticing distraction, 3) letting go, and 4) returning to now (Figure 2). During non-mindful exercise, the four-step cycle is absent because the emphasis parameter (E) is switched off. While the person may occasionally be spontaneously mindful, that is not the intent, and deliberate mindfulness is not practiced. Other names for non-mindful exercise are conventional exercise, traditional exercise, and normal exercise (Davis et al., 2022; La Forge, 2005; So et al., 2020; Tsang et al., 2008; Yin et al., 2021). The adjective "normal" is admittedly non-specific, but it underscores that mindful exercise is less common and intuitive than non-mindful exercise.

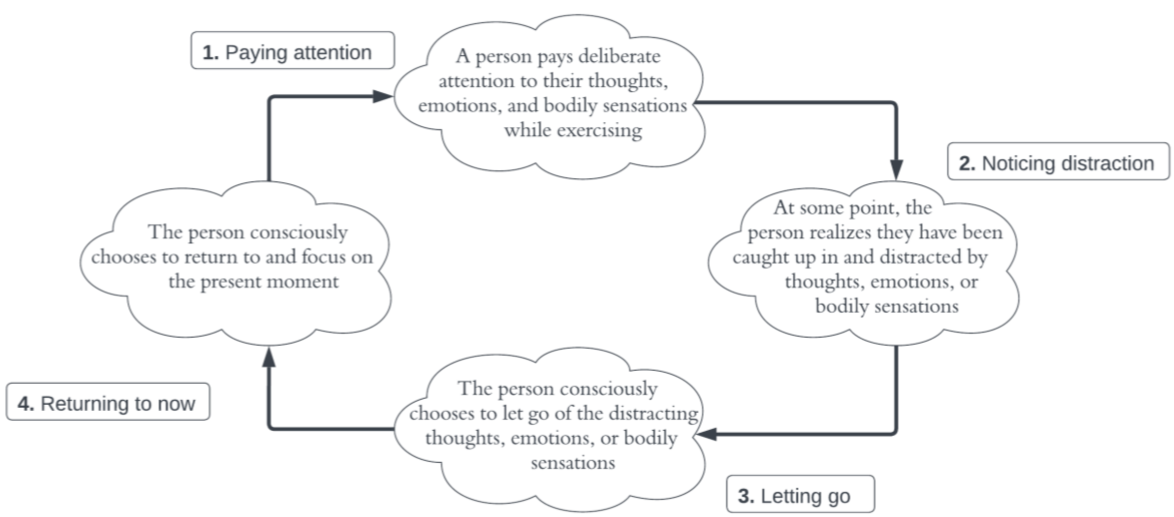

Figure 2. Four-step cycle of mindful exercise.

The intuitive approach to exercise for many people, particularly nonathletes, is probably exercising with an undirected consciousness. This state is informally called being on autopilot. Fleeting thoughts, feelings, emotions, and bodily sensations come and go without necessarily being perceived consciously. Thoughts of the past or future may flow heavily, causing the mind to be swept away to those mental locations. When this occurs, a separation puts the mind in a different place than the physical body. Though the person may be lifting dumbbells in a recreation center, they may be mentally at home or work. The mind's tendency to leave the immediate environment can be exacerbated by multitasking while exercising. Activities that evoke strong emotions or require intense cognitive processing tend to make exercise non-mindful. Listening to music, audiobooks, or podcasts tends to contract one's headspace by drawing the mind out of the present. The differences between mindful and non-mindful exercise illustrate the need to consider the emphasis (E) when delineating exercise.

An L and E should be added to the original FITT principle to recognize the importance of location and emphasis when delineating exercise. Adding the L and E create a new acronym, the FITTLE principle. Read aloud as a word, FITTLE sounds like fiddle, the informal English term for a violin (*Fiddle Definition & Meaning*, n.d.). Whereas the fiddle is an instrument to play music, the FITTLE principle is an instrument by its other definitions: a tool or a device used for a particular purpose (*Instrument Definition & Meaning*, n.d.). The evolution of the FITT principle into the FITTLE principle offers an advanced tool. The tool will help researchers, practitioners, and exercisers build well-defined exercise prescriptions to achieve their health and fitness goals. The effects of any exercise prescription on physical and mental health can be considered in the context of the FITTLE principle. Then, any of the six parameters (F, I, T, T, L, or E) can be adjusted to pursue new health and fitness goals.

As with the original FITT principle, more than one exercise parameter of the FITTLE principle can be adjusted simultaneously. For example, switching from exercise without an emphasis to exercise with an emphasis applies the E. Within the new context of mindful exercise, a person may choose from different types, thereby applying the second T. There are several popular types of mindful exercises in the United States. Three popular types are qigong, tai chi, and yoga. Both qigong and tai chi originated in ancient China. Qigong is meditative and involves focus, deep breathing, and smooth movements. The practice is related to tai chi, which was traditionally a form of martial arts but is now practiced mainly as light exercise (La Forge, 2005). Tai chi also involves focus, deep breathing, and smooth movements that flow together. These qualities have led tai chi to be called “meditation in motion” (*Video*, 2022). Tai chi differs from qigong because the former is about stringing together sequences of martial movements. Qigong is about cultivating a calm inner experience, performing postures, and creating slow movements from one location (i.e., not moving as much about the surrounding space). People in the United States practice both qigong and tai chi, but neither is as popular as yoga. The percentage of U.S. adults who engage in qigong, tai chi, or yoga increased from 5.8% to nearly 14.5% between 2002 and 2017 (Wang et al., 2019). While Wang et al. (2019) did not break down prevalence by the type of exercise, Zhang et al. (2021) reported that participation in yoga among U.S. adults rose from 5.1% to 13.7% between 2002 and 2017. These percentages indicate that, of the three popular types of mindful exercises in the United States, yoga is the most popular.

Like tai chi, yoga practiced today has a different purpose and approach than its traditional form. Traditional yoga originated in ancient India as a physical and spiritual discipline but is now popularly practiced as exercise in the West (La Forge, 2016). Western yoga practices are usually variations of Hatha yoga. In Hatha yoga, a person moves through sequences of asanas, which are body postures or poses (e.g., the lotus position, tree pose, and downward dog). Asanas train balance, flexibility, and muscular strength while facilitating mental relaxation and well-being (La Forge, 2005, 2016).

The popularity and purported benefits of qigong, tai chi, and yoga have motivated researchers to investigate their physical and mental effects. A recent systematic review and meta-analysis on qigong reported that the practice might improve sleep quality in adults with and without diseases (Ko et al., 2022). Other systematic reviews and meta-analyses reported that qigong might improve depression, fatigue, and quality of life among women without diseases, women with chronic diseases, and people with cancer (Kuo et al., 2021; Leung et al., 2021). Nevertheless, one of those reviews and a separate review reported that the studies on qigong tend to have methodological issues and a high risk of bias (Guo et al., 2018; Leung et al., 2021). A substantial risk of bias affects tai chi research too. A bibliometric review reported that tai chi improves at least one health or well-being outcome (G.-Y. Yang et al., 2021). However, the review did not evaluate study quality and noted that most studies were reported poorly (G.-Y. Yang et al., 2021). The findings of yoga reviews are mixed. One systematic review and meta-analysis hailed yoga as effective in controlling body weight, waist circumference, blood pressure, glucose, and lipids in people with prediabetes (Ramamoorthi et al., 2019). However, the 95% CIs and effect sizes did not show that the yoga group improved better than the control arms. Another systematic review and meta-analysis reported that yoga improves balance, limb flexibility and strength, depression, and sleep quality among older adults without diseases (Sivaramakrishnan et al., 2019). Separately, Gothe et al. (2019) reported in their systematic review that yoga improves the structure or function of several brain regions, which could protect people against age-accompanied neurodegenerative decline. And So et al. (2020) reported in their systematic review and meta-analysis that one session of yoga decreases anxiety more than one session of non-mindful exercise. Viewed collectively, the reviews on qigong, tai chi, and yoga reveal mixed statistical findings and small effect sizes for differences from control groups. Rather than deter interest, these data are fueling a greater interest in determining whether mindful exercises improve physical and mental health more than non-mindful exercises (i.e., control interventions).

More randomized controlled trials (RCTs) with low risks of bias are critical to determining the efficacy of mindful exercises for improving physical and mental health (Davis et al., 2022). These trials are also needed to dispel misconceptions and cynicism about complementary medicine, which encompasses all MBEIs. Part of the cynicism is toward the merit of MBEIs. The scant scientific literature causes doubt about MBEIs' potential physical and mental benefits. Health professionals trained in Western medicine tend to adhere to evidence-based practices. These professionals are careful about recommending their patients try unconventional treatments, particularly if a treatment's safety and efficacy have not been shown consistently in scientific studies. The cynicism is also probably based on Western health professionals' doubt that their patients will adopt one of the popular mindful exercise modalities. For many patients in Western cultures, such as the United States, qigong and tai chi may be unfamiliar and seen as out there if recommended as a treatment. Even yoga, practiced by over 32 million U.S. adults (Zhang et al., 2021), may seem unappealing or too demanding to patients. Disinterest among patients exacerbates the cynicism of health professionals toward mindful exercises. What is left is a tension of two opposites: On one end is evidence that mindful exercises may be beneficial, and on the other end are the exercises being underused or disregarded. Resolving this tension and unleashing the potential of mindful exercises requires innovation.

The best use of *innovation* is not as a noun, such as something created. The best use of innovation is as a verb, meaning thinking innovatively. Practically, this means creating something that is not just new but also better (Couros, 2015). In the context of mindful exercise, thinking innovatively means conveying the potential benefits of mindful exercise in a form both health professionals and patients are likely to receive well. Familiarity fosters favorability. Walking is a widely accepted form of exercise in the United States, where an estimated 116 million people at least six years of age walked at least once a year for fitness in 2021 (Statista, 2022). That same year, the U.S. population was nearly 332 million people (The World Bank Group, n.d.-a). These data show that almost one in three people in the country walked for fitness in 2021. The popularity of non-mindful walking makes mindful walking one of the most sensible mindful exercise modalities to investigate in the country.

Mindful walking is what it sounds like: walking mindfully. During mindful walking, a person cultivates an accepting and non-judgmental awareness of their thoughts, feelings, emotions, and bodily sensations without attaching labels such as “good” or “bad” (Davis et al., 2022). If able and comfortable doing so, the person can extend their awareness to the external environment. If desired, the person can also meditate on a particular sensation (e.g., the arms swinging past the body) or a short phrase to repeat aloud or in the mind (e.g., a mantra such as “I am grateful.”). This variation of mindful walking, where the person focuses on a specific sensation or mantra, is called meditative walking. The central hypothesis regarding meditative and mindful walking is that mindfulness and walking synergize to improve physical and mental health more than mindfulness or regular walking alone. When completed independently over time (i.e., chronically), mindfulness training and traditional walking improve physical and mental health independently (Galante et al., 2021; Kelly et al., 2014, 2018; Marino et al., 2021). In this case, independently does not mean in the absence of other people but instead mindfulness without walking or walking without mindfulness.

In various populations, an array of MBIs reduce physical and mental suffering from conditions such as chronic pain, fatigue, insomnia, stress, anxiety, and depression (Cillessen et al., 2019; Creswell et al., 2019; Grossman et al., 2004; Hofmann & Gómez, 2017; Toivonen et al., 2017). Chronic traditional walking (non-mindful) increases a person's aerobic capacity and decreases cardiovascular risk factors, such as hypertension, excess body weight, and adiposity (Murtagh et al., 2015). All-cause mortality, adjusted for other physical activity, also decreases by 11% 95% CI [4, 17] in people who walk traditionally at a dose of 11.25 metabolic equivalent hours per week (Kelly et al., 2014). Besides improving physical health, chronic traditional walking reduces the risk of depression and improves depressive symptoms (Kelly et al., 2018). One meta-analysis reported a large effect on depression, with a standardized mean difference of −0.86; 95% CI [−0.61, −1.12] (Robertson et al., 2012). Additionally, traditional walking programs lasting 6–12 weeks decrease anxiety (Kelly et al., 2018). The following Venn diagram summarizes some physical and mental effects of practicing mindfulness and traditional walking (Figure 3).

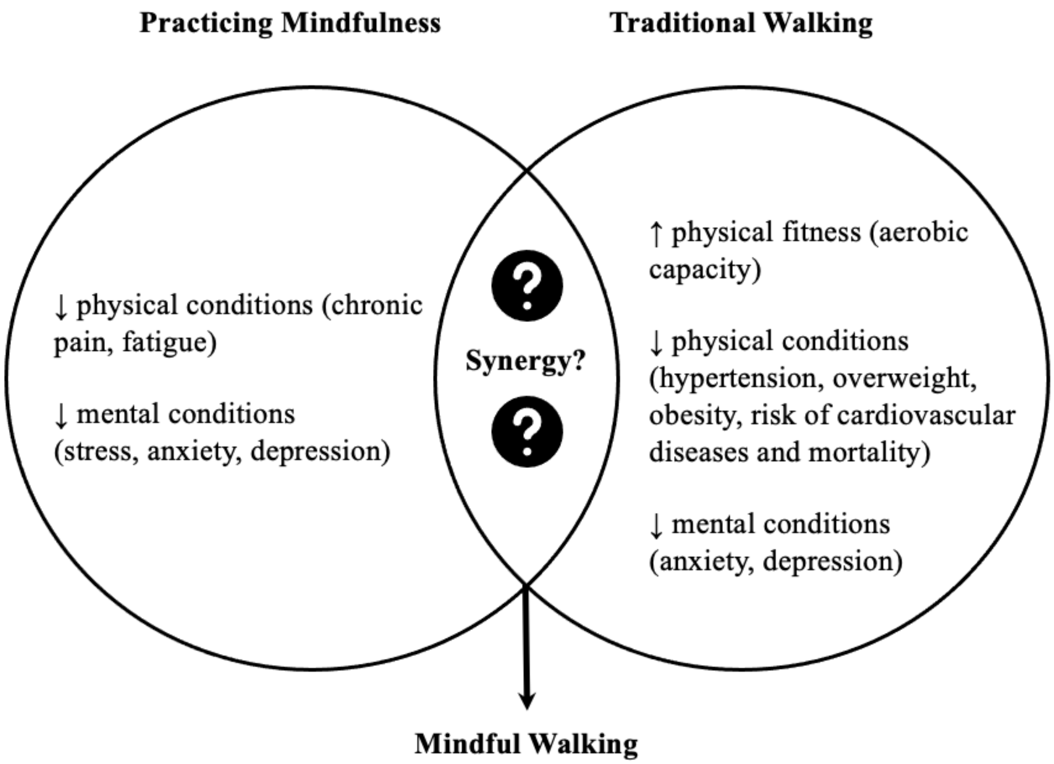

Figure 3. The potential synergy between practicing mindfulness and traditional walking. Except for physical fitness, a down arrow (↓) for physical and mental conditions means the intervention improves them.

At the center of the Venn diagram, the overlap of the two circles represents an under-explored area of research: mindful walking. At the outset of this dissertation, the effects of mindful walking on physical and mental health had hardly been described. Searches in major repositories for peer-reviewed journal articles returned scant studies and no systematic reviews or meta-analyses. The ill-defined effects of mindful walking motivated the first of the three dissertation studies. Study 1 was a rigorous systematic review to summarize and appraise the small body of scientific literature on meditative and mindful walking (Chapter 2). A noteworthy finding of the systematic review was an overlap between the GE and mindful walking literature (Figure 4). Several studies had participants walk meditatively or mindfully indoors and outdoors to compare the effects between the settings. Another interesting finding was that some studies reported state mindfulness alongside the physical and mental effects of mindful walking (Ameli et al., 2021; Bigliassi et al., 2020; Davis et al., 2022; Gotink et al., 2016). *State mindfulness* is one way of measuring mindfulness as a construct, the other being *trait mindfulness* (Bishop et al., 2004; Ruimi et al., 2022; Tanay & Bernstein, 2013). State mindfulness is behavior-like, dynamic, context-specific, and how mindful someone is in a specified moment or period (Bishop et al., 2004; Ruimi et al., 2022; Tanay & Bernstein, 2013). Trait mindfulness is more stable than state mindfulness and indicates how mindful someone tends to be as part of their personality (Bishop et al., 2004; Ruimi et al., 2022; Tanay & Bernstein, 2013). Seeing state mindfulness reported alongside measurements of physical and mental health sparked the idea that state mindfulness may be a covariate or modulator of the relationship between mindful walking (the independent variable) and the physical and mental effects (the dependent variables). Based on the GE literature, another potential modulator in outdoor settings may be people's connectedness to nature. Connectedness to nature is a concept of the human-nature relationship. Connectedness can refer to cognitive or emotional connection, beliefs about appreciating nature, wanting to spend time there, and making lifestyle choices to protect it from destruction (Perkins, 2010). The potential modulatory roles of state mindfulness and connectedness to nature made them variables of interest in the present dissertation.

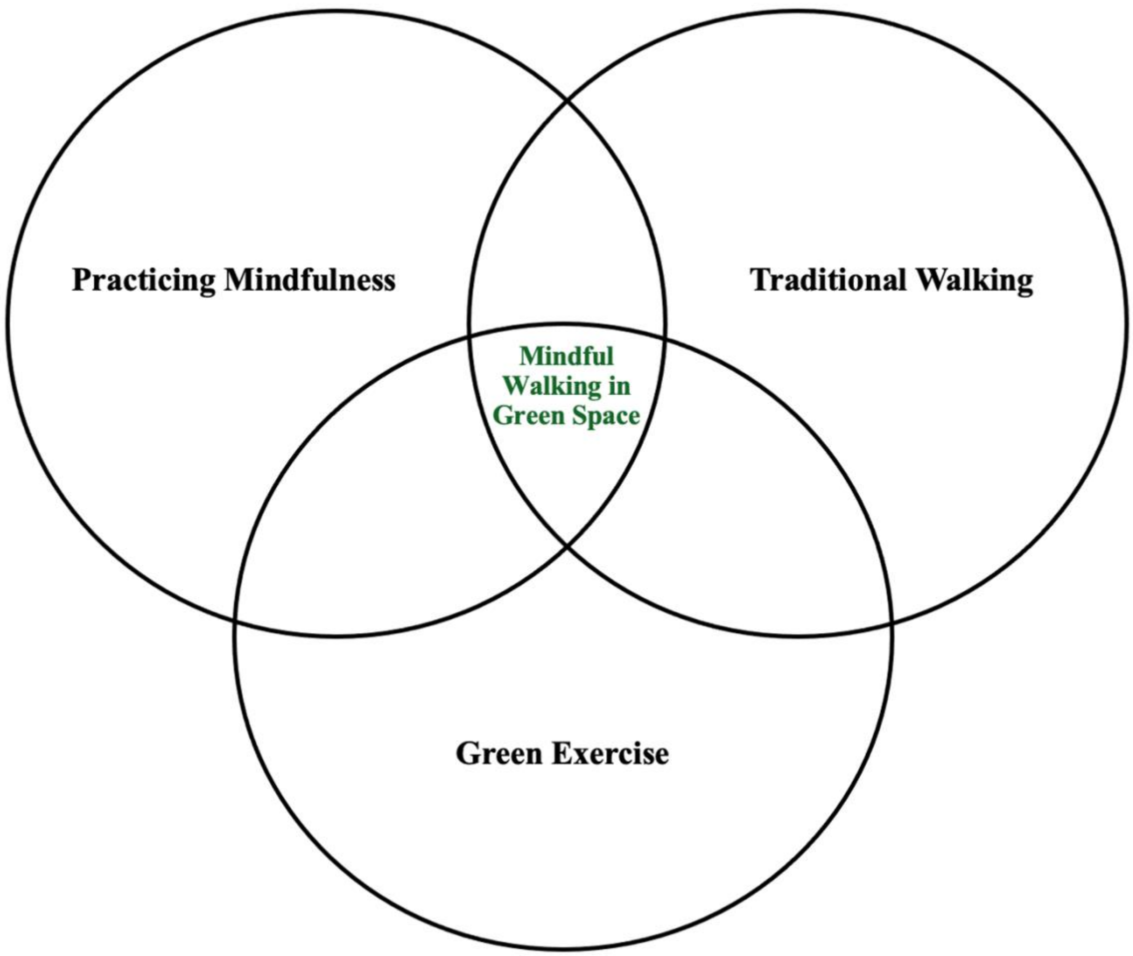

Figure 4. An overlapping area of study among the bodies of literature on mindfulness, traditional walking, and green exercise (GE).

It became clear that the second and third dissertation studies needed to investigate whether spending time in nature changed state mindfulness or connectedness to nature independent of a mindfulness intervention. If spending time in nature changed the two variables without an MBI, the finding would support two ideas. First, studies of the effects of mindful walking in green space on state mindfulness should consider connectedness to nature as a covariate. Second, studies of the effects of GE on connectedness to nature should consider state mindfulness as a covariate. This dissertation's author is unaware of published research that has discerned the effects of inactive and active time spent in nature on state mindfulness and connectedness to nature. Because novelty alone is an insufficient reason for a scientific study, it was not the only reason for this dissertation. Another reason was to establish a sound foundation for future research on mindfulness-based GE interventions (MBGEIs), particularly mindful green walking. When studying the effects of mindful green walking on physical and mental health, researchers should identify as many influencing variables as possible. Only then can the potential benefits of mindful green walking be better understood.

Detecting and quantifying changes in state mindfulness and connectedness to nature requires measuring the variables at multiple timepoints. Few published studies have done this around exercise. This realization made it clear that the second and third dissertation studies also needed to investigate how to measure the variables around exercise. Two prevailing measures of state mindfulness and connectedness to nature, respectively, are the State Mindfulness Scale (Ruimi et al., 2022; Tanay & Bernstein, 2013) (SMS; Appendix A) and the Love and Care for Nature Scale (Perkins, 2010) (LCN; Appendix B). State mindfulness is also measured via the State Mindfulness Scale for Physical Activity (Cox et al., 2016) (SMS-PA; Appendix C). The SMS-PA was created from the SMS to offer a shorter measure for efficient completion around physical activity and exercise. The 12 questions on the SMS-PA (vs. 21 questions on the SMS) are framed around thoughts and bodily sensations related to physical activity. The SMS, SMS-PA, and LCN are Likert-type scales (SMS = 21 items; SMS-PA = 12 items; LCN = 15 items). Respondents usually respond to each item with a 1 to 5 on the SMS, a 0 to 4 on the SMS-PA, and a 1 to 7 on the LCN. On each scale, all the items are scored positively (lower numbers represent less state mindfulness or connectedness to nature). Published studies have used all three scales, some of which have provided evidence of the scales' utility and internal consistency (Cox et al., 2016; Pasca et al., 2020; Peixoto et al., 2019; Perkins, 2010; Ruimi et al., 2022; Tanay & Bernstein, 2013; Ullrich-French, Cox, et al., 2017; Ullrich-French, González Hernández, et al., 2017). Drawing conclusions about the scales' validity and test-retest reliability requires more data. Checking test-retest reliability is especially important because pilot testing in the field by the author of this dissertation revealed that the SMS and LCN are prone to data loss caused by people missing items. While it is unclear whether items are ignored or overlooked, missed items jeopardize the scales' test-retest reliability and invalidate the SMS or LCN because total scores depend on summing all the items. This limitation of the SMS and LCN led the author of this dissertation to create novel visual analog scales for state mindfulness (Visual Analog Scale-Mindfulness; VAS-M; Appendix D) and connectedness to nature (Visual Analog Scale-Nature; VAS-N; Appendix E). Answering each scale requires only a single vertical dash, which may reduce the number of opportunities for data loss. Before the scales can be trusted for use in MBGEI studies, the two new VAS must be validated and shown to be test-retest reliable. This mandate was another purpose of the dissertation.

The previous two paragraphs described the insights from the systematic review (Study 1) that helped frame dissertation Studies 2 and 3. Given the focus on two constructs, state mindfulness and connectedness to nature, it seemed reasonable to focus on each construct in separate studies. Consequently, Study 2 concentrated exclusively on state mindfulness. Study 3 concentrated firstly on connectedness to nature and secondly on the relationship between state mindfulness and connectedness to nature. The primary aim of Study 2 was to determine whether inactive immersion in green space (i.e., sitting undisturbed at a trailhead) or walking in green space changed people's state mindfulness (measured via the SMS). The secondary aim was to test the concurrent validity of two quicker measures of state mindfulness (the VAS-M and

SMS-PA) against the SMS. The tertiary aim was to assess the test-retest reliability of the SMS, SMS-PA, and VAS-M. The primary aim of Study 3 was to determine whether inactive immersion in green space (i.e., sitting undisturbed at a trailhead) or walking in green space changed people's connectedness to nature (measured via the LCN). The secondary aim was to test the concurrent validity of a quicker measure of connectedness to nature (the VAS-N) against the LCN. The tertiary aim was to assess the test-retest reliability of the LCN and VAS-N. The quaternary aim was to determine if connectedness to nature was related to state mindfulness while sitting and walking in green space. The rationale for this fourth aim was to test, for the first time, whether paying attention to the present moment explains any measurable variance in connectedness to nature.

Before presenting the three dissertation studies, the author believes it is important that he communicates the dissertation's overarching aims. In the author's view, a reader deserves to know not just the purpose and aims of each project but the rationale for the entire dissertation. While completing the dissertation, the author attempted to:

1. Exceed the standards of academic rigor held by the author's doctoral advisory committee.
2. Meet the expectations of the Graduate College of the University of Nevada, Las Vegas.
3. Present arguments logically while introducing as little bias as possible.
4. Explain and extend published literature judiciously and in the correct context (i.e., by not misrepresenting the hard work of other researchers).
5. Help de-silo the bodies of literature on mindfulness, exercise, and being in nature.
6. Inspire other graduate-student and professional researchers to develop and test hypotheses about mindful green walking and other MBGEIs.

7. Offer academics without field-specific expertise an accessible introduction to research on mindfulness, exercise, and being in nature.

CHAPTER 2: A SYSTEMATIC REVIEW OF THE EFFECTS OF MEDITATIVE AND MINDFUL WALKING ON MENTAL AND CARDIOVASCULAR HEALTH

2.1 Abstract

INTRODUCTION: Meditative and mindful exercise are types of physical exercise during which people pay attention, on purpose, to each new present moment without judging their experience. The goal is to apply an accepting awareness of the environment, bodily sensations, thoughts, and emotions without labeling them (e.g., good or bad). The literature centers on qigong, tai chi, and yoga, which are types of mindful exercise that improve mental and cardiovascular health. It is unclear if meditative and mindful walking also improve these health domains. To the authors' knowledge, this question has not been addressed by a published systematic review. The purpose of this systematic review without a meta-analysis was to synthesize the literature on meditative and mindful walking to determine their effects on mental and cardiovascular health. **METHODS:** The protocol follows the PRISMA guidelines, is registered in PROSPERO (CRD42021241180), and is published elsewhere in a peer-reviewed journal. **RESULTS:** The systematic review contains 14 studies that had various populations, interventions, and outcomes. In 13 studies, the interventions statistically significantly improved scores on at least one outcome of mental or cardiovascular health (e.g., affect, anxiety, depression, distress, state mindfulness, stress, blood pressure, and six-minute walk distance). **CONCLUSIONS:** The improved outcomes should be interpreted cautiously because their clinical meaningfulness is unclear, and the studies had severe methodological limitations. Determining if meditative and mindful walking meaningfully improve mental and cardiovascular health will require randomized controlled trials that use rigorous designs, transparent protocols,

and clinically meaningful outcomes that indicate physical function, mental well-being, morbidity, and mortality.

Note

A version of this chapter was published as a peer-reviewed paper in the *International Journal of Exercise Science* (IJES) (Davis et al., 2022). Tables 1–3 were published in that paper and another peer-reviewed paper in *PLOS One* (Davis et al., 2021). The study described in this chapter and those papers did not involve research on human subjects and did not need approval by an Institutional Review Board. The author of the present dissertation was the first and corresponding author of both papers. The other authors of the IJES paper were Bryson Carrier, Kyle Cruz, Brenna Barrios, Dr. Merrill Landers, and Dr. James Navalta. The other authors of the *PLOS One* paper were Bryson Carrier, Brenna Barrios, Kyle Cruz, and Dr. James Navalta. Both IJES and *PLOS One* are open-access journals that allow authors to reproduce their published works while being protected under the Creative Commons Attribution 4.0 International Public License.

2.2 Introduction

The global population is suffering under a double burden of poor mental and cardiovascular health. In a year, nearly one-fifth of adults have a mental health condition (Steel et al., 2014). In a lifetime, the prevalence is almost one-third of adults (Steel et al., 2014). This mental burden is compounded by physical diseases of the heart and blood vessels. More adults live with disability and die because of cardiovascular diseases (CVD) than any other non-communicable disease (Kyu et al., 2018; Roth et al., 2018). Alleviating this double burden requires treatments that are cost-effective, physiologically effective, and widely accessible. Walking has all three features. It is a free and natural human activity that can be completed by most adults. Crucially, walking treats various mental and cardiovascular diseases effectively. With respect to mental health conditions, walking protects against and improves depression (Kelly et al., 2018), with one meta-analysis reporting that walking decreases symptoms by a large effect; standardized mean difference of −0.86, 95% confidence interval (CI) [−1.12, −0.61] (Robertson et al., 2012). Walking is also negatively associated with anxiety and improves anxiety when completed for 6–12 weeks (Kelly et al., 2018). Besides the mental benefits, walking also improves cardiovascular health. Walking increases aerobic capacity and decreases CVD risk factors such as body weight, adiposity, and blood pressure (Murtagh et al., 2015). After adjusting for other physical activity, walking also decreases all-cause mortality by 11% [4, 17]; estimate based on a dose of 11.25 metabolic equivalent hours/week) (Kelly et al., 2014).

Motivated by the evidence that walking improves mental and cardiovascular health, a niche in the relevant literature has investigated whether the benefits of normal walking (i.e., traditional walking) are surpassed by the effects of meditative walking or mindful walking. The latter two types of walking are mindful exercises. There are several definitions of mindful exercise, but the one accepted for this systematic review is physical exercise that involves focusing one's attention earnestly on the inner experience (La Forge, 2005, 2016). During mindful exercise, people pay attention on purpose to each new present moment without judging their experience. In contrast, traditional exercise often involves mind-wandering without a profound inwardly directed contemplative focus. During meditative and mindful walking, the goal is to apply an accepting awareness to the thoughts, emotions, and bodily sensations without attaching labels such as "good" or "bad." This accepting and non-judgmental awareness can also be extended to the external environment. Meditative walking and mindful walking are similar except that, during meditative walking, people typically repeat mantras (i.e., short phrases) to maintain their awareness and focus. The rationale behind comparing traditional walking with meditative and mindful walking is that other types of mindful exercise improve mental and cardiovascular health, sometimes more than non-mindful exercise.

Literature on mindful exercise centers on qigong, tai chi, and yoga. Qigong and tai chi originated in China and are types of light exercise. Each type has many subtypes, but the common elements are deep breathing, smooth movements, assuming postures, and cultivating a focus on the present. The key difference between qigong and tai chi is that the latter began as martial arts training and involves stringing together martial movements. Qigong has fewer to no martial movements and involves less movement from one location (i.e., a person does not typically move around a room as much during qigong as during tai chi). Yoga originated in India and has sprouted many types. The most popular type in Western countries is Hatha yoga, during which people move through sequences of asanas (held postures such as cat-cow pose, downward dog pose, and tree pose).

Qigong, tai chi, and yoga improve depression in various populations (Tsang et al., 2008) and psychiatric symptoms in people with schizophrenia (Li et al., 2018). In 2021, a meta-analysis reported that one session of yoga decreases anxiety slightly (a small, significant effect) (Yin et al., 2021). This result aligned with another finding that yoga reduces anxiety more than non-mindful exercises (also a small, significant effect) (So et al., 2020). Yoga also improves cardiovascular health (Barrows & Fleury, 2016; Chu et al., 2016). The evidence for qigong and tai chi suggests these mindful exercises improve cardiovascular health too (Hartley et al., 2014; Hung et al., 2016; Klein et al., 2019). Systematic reviews and meta-analyses about qigong, tai chi, and yoga continue to inform readers about their efficacy in a manner that individual studies cannot. It seems that only individual studies about meditative and mindful walking exist so far.

To the author's knowledge, no published systematic review or meta-analysis has synthesized the individual studies of meditative and mindful walking. The primary purpose of the present systematic review was to determine the effects of meditative and mindful walking on mental and cardiovascular health. The secondary purpose was to evaluate the quality of the studies on meditative and mindful walking and compare their findings to those of studies about other types of mindful exercise.

2.3 Methods

The present systematic review was conducted as per the Preferred Reporting Items for Systematic Reviews and Meta-Analyses (PRISMA) guidelines. All researchers adhered to the ethical standards of the International Journal of Exercise Science (Navalta et al., 2020). The protocol of the systematic review is registered in the international prospective register of systematic reviews called PROSPERO (Registration Number: CRD42021241180) and is published elsewhere (Davis et al., 2021). In that published protocol, readers can read our methods in greater detail than what is given in this dissertation (Tables 1–3). The details explained in that publication are the Population, Intervention, Comparator, Outcomes, and Study Design (PICOS) criteria, eligibility criteria, search strategy (databases, team, techniques, and search terms), screening process, data extraction, and risk of bias assessment (Davis et al., 2021).

The present review did not include a meta-analysis because the included studies did not meet the four aspects of homogeneity required to conduct a meta-analysis (Boland et al., 2017). Specifically, the included studies differed by their 1) participants, 2) interventions and comparators, and 3) outcomes and the time frame over which the outcomes were measured. Additionally, 4) most of the included studies reported different treatment effects in different directions (Boland et al., 2017). The lack of homogeneity and meta-analysis warranted a longer results and discussion than is typical of standard systematic reviews. The results and discussion summarize the similarities and differences among the studies' populations, methods, and outcomes.

Table 1. Review question and PICOS table.

Review Question	What is the evidence for meditative and mindful walking as therapies for improving mental and cardiovascular health in adults with and without psychological disorders or cardiovascular diseases?
Population	Adults with or with no psychological disorders or cardiovascular diseases - Will extract participants' age, sex, gender, nationality, disease status, medication use, and history of meditation or mindfulness practice
Intervention	Meditative walking or mindful walking - Any form of walking with a meditative or mindful component used to reduce anxiety or depression, increase mindfulness, or improve cardiovascular risk factors - Operational definition of meditative and mindful walking: Walking with an inwardly directed mental focus and a concentration on muscular movements, body alignment, and/or breath - Will extract the frequency, intensity, type, duration, and location (e.g., indoors, outdoors) of the intervention
Comparator	Placebo or negative control in controlled studies No comparator in uncontrolled studies
Outcomes	Any beneficial or adverse changes in any quantitative measure of anxiety, depression, mindfulness, or cardiovascular health or risk - Any subjective self-reported measures of anxiety, depression, or mindfulness - Any objective cardiovascular biomarkers
Setting	Any physical environment (indoors, outdoors, urban, rural, built-up, or natural)
Study Design	Only studies with interventions, and no observational studies - Controlled or uncontrolled - Randomized or nonrandomized - Crossover design (participants complete the intervention and control arms) or parallel design (participants complete only the intervention or control arm)

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Table 2. Eligibility criteria.

Participants	Adults of any age, sex, gender, nationality, disease status, medication use, and history of meditation or mindfulness practice
Inclusion Criteria	1. The source is a published article in a peer-reviewed journal or is an unpublished or published master's thesis or doctoral dissertation 2. The source is written in English 3. The source reports the findings of an interventional study a. The intervention is any walking with a meditative or mindful component used to reduce anxiety or depression, increase mindfulness, or improve cardiovascular risk factors b. At least one reported outcome is a measure of anxiety, depression, mindfulness, or cardiovascular health
Exclusion Criteria	1. The source is not a published, peer-reviewed journal article or an unpublished or published master's thesis or doctoral dissertation 2. The source is written in any language other than English 3. The source reports the findings of an interventional study with an intervention or outcomes irrelevant to this systematic review a. There is a walking intervention without a meditative or mindful component b. None of the reported outcomes are a measure of anxiety, depression, mindfulness, or cardiovascular health 4. The source reports the findings of an observational study (i.e., there is no walking intervention)

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Table 3. Search strategy.

Investigators	Team A: DD and BC Team B: BB and KC Arbiter: JN	
Techniques	Search research databases for sources, including them in four stages: 1. Include sources by title 2. Include sources by abstract 3. Include sources by full text 4. Include sources from the reference lists of sources included by full text (journal articles, master's theses, and doctoral dissertations)	
Databases	Academic Search Premier, APA PsycInfo, Google Scholar, PubMed, and SPORTDiscus	
Included Types of Literature	Published, peer-reviewed journal articles; unpublished and published master's theses and doctoral dissertations	
Publication Date Range	No limit	
Intervention Search Terms	Outcome Search Terms	
"Meditative walk*" "Walk* meditat*" "Mindful* walk*" "Buddhis* walk*"	"Stress" "Anxiety" "Depress*" "Mindfulness" "Health" "Fitness" "Allostatic load" "Disease"	"Cardiovascular" "Hypertens*" "Blood pressure" "Cholesterol" "Hyperglycem*" "Blood sugar" "Insulin*"
Search Combination	((Meditative walk*) OR (walk* meditat*) OR (mindful* walk*) OR (Buddhis* walk*)) AND (stress OR anxiety OR depress* OR mindfulness OR health OR fitness OR allostatic load OR disease OR cardiovascular OR hypertens* OR blood pressure OR cholesterol OR hyperglycem* OR blood sugar OR insulin*)	

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2.4 Results

The initial search for sources by title provided 2,800 hits from Academic Search Premier, APA PsycInfo, Google Scholar, PubMed, and SPORTDiscus (Figure 5). These 2,800 hits were screened to the 14 full-text sources included in the systematic review. The top reasons for screening out sources by abstracts were the sources 1) were not original research studies, 2) lacked a meditative or mindful walking intervention, or 3) provided meditation and walking separately. All 14 sources were peer-reviewed journal articles, meaning no master's theses or doctoral dissertations were identified or included. In the references of the included sources, no other sources eligible for inclusion were identified. In the Results of the present systematic review, the populations, interventions, study designs, and results of the 14 studies are reported and compared (Tables 4–8). From this point on, studies are called single-session or multi-session studies. Single-session studies reported only the acute effects of one session of meditative or mindful walking at a time (e.g., the effects of one mindful walk on stress). Multi-session studies reported the cumulative effects of more than one session of meditative or mindful walking (e.g., stress before and after 8 weeks of 3 sessions of mindful walking/week).

Unless stated otherwise, the data in the text and tables are expressed as means ± standard deviations (SD). Except for when the phrase "absolute percent increase/decrease" is used, all percent increases/decreases in outcomes are relative to baseline values. The phrase "absolute percent increase/decrease" is used to describe percent increases/decreases when the unit of an outcome is already a percent (e.g., percent body fat and flow-mediated dilation). Percent changes relative to baseline values were calculated in this way: Percent increases = ((final value − starting value) / |starting value|) × 100, and percent decreases = ((starting value − final value) / |starting value|) × 100.

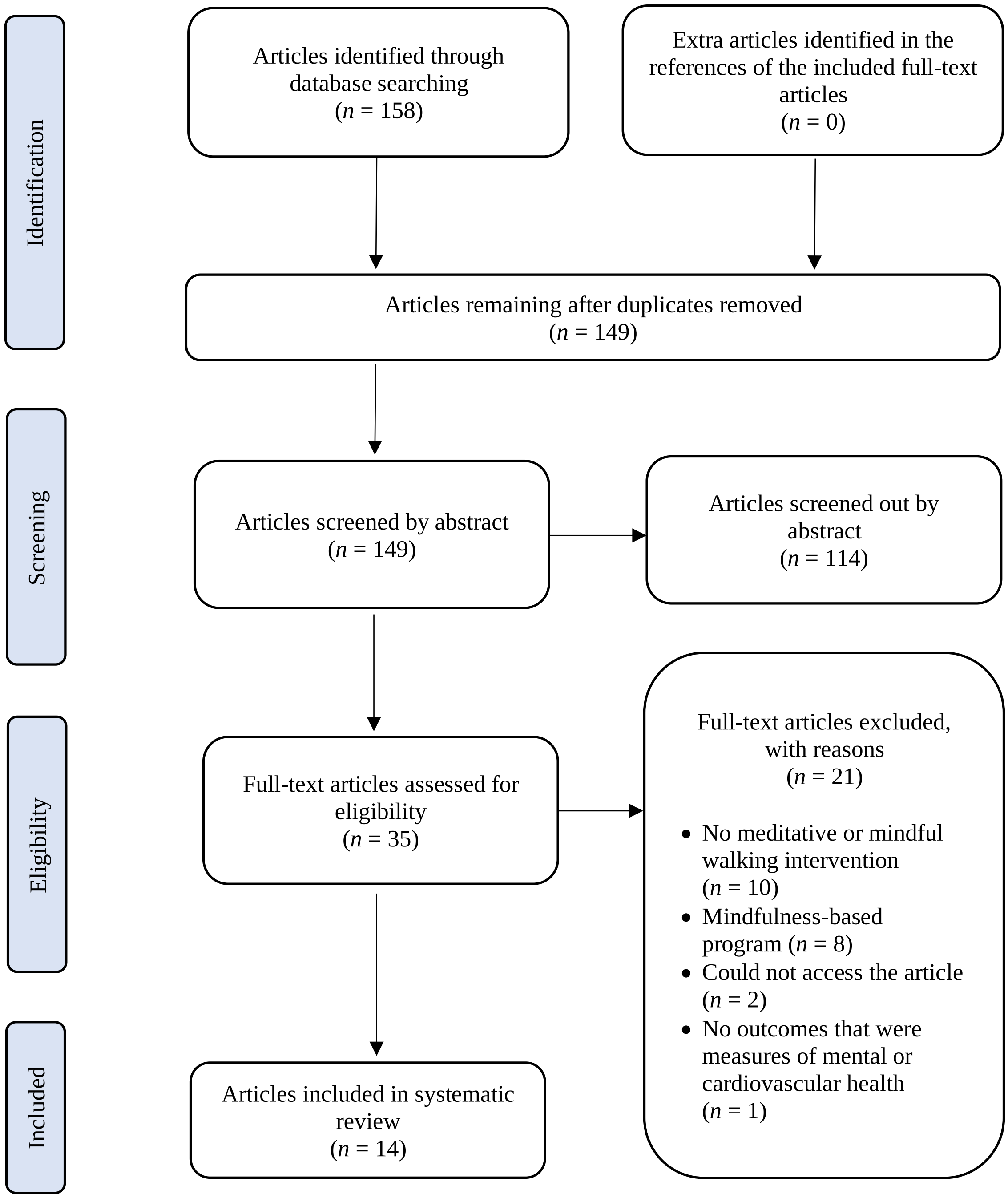

Figure 5. Flow diagram that depicts how articles were included in the systematic review.

Table 4. Design characteristics of the 14 studies included in the systematic review.

Publication Range	Origin	# of Studies	Meditative Walking[a]	Mindful Walking[b]	Single-Session[c]	Multi-Session[d]	Indoors	Outdoors	Indoors & Outdoors	Indoors/ Outdoors Not Specified
2013–2021	Brazil Germany Netherlands South Korea Taiwan Thailand United States	14	6/14 (43%)	8/14 (57%)	4/14 (29%)	10/14 (71%)	6/14 (43%)	5/14 (36%)	2/14 (21%)	1/14 (7%)

[a]The authors described the intervention as meditative walking or walking meditation. [b]The authors described the intervention as mindful walking, walking while practicing mindfulness, or breathing-based walking. [c]Single-session means the studies reported only the acute effects of one session of meditative or mindful walking at a time. [d]Multi-session means the studies reported the cumulative effects of more than one session of meditative or mindful walking.

Table 5. Populations sampled in the 14 studies included in the systematic review.

Populations of Apparently Healthy Adults	Populations of Adults with Diseases
Fairly physically inactive or sedentary Older adults (≥ 65 years of age) Physically active Previous meditation and mindfulness experience Undergraduate students with low intrinsic motivation for physical activity Military personnel and their family and caregivers	Chronic Obstructive Pulmonary Disease Heart failure Type 2 Diabetes Mellitus Depressive symptoms Increased psychological distress

Table 6. Measures of mental and cardiovascular health reported in the 14 studies included in the systematic review.

Mental Health		Cardiovascular Health	
Activation	Happiness	Aerobic Capacity	Heart Rate Variability
Affect	Health-Related Quality of Life	Blood Pressure[a]	Physical Activity[b]
Anxiety	Post-Traumatic Thoughts and Emotions	Body Fat Percentage	Six-Minute Walk Distance
Attentional Focus	Ruminative Thoughts	Body Mass Index	Blood Glycemia Variables
Brooding	Self-esteem	Flow-mediated Dilation	Blood Lipidemia Variables
Depression	Self-worth	Heart Rate	
Distress	State Mindfulness		
Emotional Awareness	Stress		
Enjoyment of Physical Activity	Trait Mindfulness		

[a]Systolic and diastolic; [b]Objective (measured) and subjective (self-reported). The units and time frames in which the outcomes were measured are explained in the sections Mental Health (Single- and Multi-Session Studies) and Cardiovascular Health (Single- and Multi-Session Studies).

Table 7. Single-session studies that reported the effects of meditative and mindful walking on mental or cardiovascular health.

Authors, Year (Country)	Study Design	Participants[a]	Meditative or Mindful Walking Intervention	Mental Health	Cardiovascular Health
Ameli et al., 2021 (U.S.)	RCT, Crossover, Single session	Current and former U.S. military personnel and their family and caregivers, 18–60 y of age *n* = 12; Age = 35 ± 12 y; 75% female; 92% college-educated; 25% Asian, 25% Black; 33% White; 17% Other	Control[b]: 20-min session that included mindful walking along the Urban Road Intervention: 20-min session that included mindful walking along the Green Road Mindful walking: Walking with focused attention and present-moment orientation Setting: The Urban Road (busy campus road in a medical treatment facility) and The Green Road (a healing garden/woodland environment); Participants did not walk together	Distress: ↓ in intervention group State mindfulness: ↑ in intervention group	NR

Bigliassi et al., 2020 (Brazil)	RCT, Crossover, Single session	Apparently healthy, active adults *n* = 24; Age = 24 ± 4 y; Mass = 69 ± 17 kg; Height = 170 ± 10 cm; Active minutes = 376 ± 192 min/wk	Controls[b]: Mindlessness walking meditation and walking control Intervention: 4–6-min session of mindfulness walking meditation Mindfulness walking meditation: Walking while focusing on the feet, legs, and environment Setting: Small outdoor park on a university campus; Participants did not walk together	State mindfulness: ↑ in intervention group Affect: More positive in intervention group Perceived activation: ↓ in intervention group Perceived enjoyment: ↑ in intervention group	NR
Cox et al., 2018 (U.S.)	Non-randomized controlled study, Crossover, Single session	Undergraduate students with low intrinsic motivation for physical activity, 18–35 y of age *n* = 23; Age = 19 ± 1 y; BMI = 24.8 ± 5.0 kg· m^{-2}; 83% female; 17% Asian/Pacific Islander; 4% Black; 13% Hispanic/Latino; 4% Multi-Racial; 4% Native American/Alaskan Native; 52% White; 4% Other	Control[b]: Traditional walking Intervention: ~30-min session that included mindful walking Mindful walking: 10-min session; Walking while listening to a mindfulness script Setting: Treadmill in a university laboratory; Participants did not walk together	Attentional focus: ↑ internal focus in intervention group Positive affect: ↑ in intervention group State mindfulness of the body: ↑ in intervention group vs. control group Enjoyment: ↑ in intervention group vs. control group	NR

Shin et al., 2013 (South Korea)	Randomized, uncontrolled study, Parallel, Single session	Community-dwelling adult females, 18–25 y of age *n* = 139; Age = 20 ± 2 y; BMI = 21.4 ± 2.7 kg· m^{-2}; 45.9% religious	Interventions: 90-min sessions on four consecutive days (athletic walking in gym, athletic walking in forest, meditative walking in gym, meditative walking in forest); Each session included 35 min walking + 10 min rest + 35 min walking + 10 min rest Meditative walking: Walking while focusing on bodily sensations and breathing Setting: 100-m track (indoors) and forest (outdoors); Unclear if participants walked alone or together	Anxiety: ↓ in meditative walking group more than in athletic walking in both settings Self-esteem: ↑ in meditative walking group more than in athletic walking group in both settings Happiness: ↑ in meditative walking group more than in athletic walking group in both settings; ↑ in forest more than in gym	NR

The ↑ and ↓ represent statistically significant within-group changes or between-group differences in the measures of mental and cardiovascular health. [a]The *n* given in the table is the number of participants for which data were analyzed. [b]This study had at least one control group, none of which was a non-walking control group. Abbreviations: RCT: randomized controlled trial; U.S.: United States; y: years; min: minute(s); NR: not reported; kg: kilogram(s); cm: centimeters; wk: week(s); BMI: body mass index; m: meters.

Table 8. Multi-session studies that reported the effects of meditative and mindful walking on mental or cardiovascular health.

Authors, Year (Country)	Study Design	Participants[a]	Meditative or Mindful Walking Intervention	Mental Health	Cardiovascular Health
Srisoongnern et al., 2021 (Thailand)	RCT, Parallel, Multi-session	Adults with heart failure, 18–80 y of age *n* = 48; Age = 65 ± 12 y; 50% female	Control[b]: Aerobic exercise program Intervention: 30–40-min session of Buddhism meditative walking on ≥ 3 days/wk for six wk Buddhism meditative walking: Walking while focusing on the rhythmic swinging of the legs and mentally repeating the mantra "left" and "right" with each leg swing Setting: 5-m, straight indoor path in a hospital; Participants' homes; Participants did not walk together	Quality of life (specific to heart failure): NS	SBP: ↓ in control group DBP: NS Six-min walk distance: NS

			Control: Usual care		
Lin and Yeh, 2021 (Taiwan)	RCT, Parallel, Multi-session	Adults with mild-to-severe COPD *n* = 78; Age = 71 ± 8 y; BMI = 24.4 ± 4.0 kg· m^{-2}; 2.6% female; 26% college-educated; 26% Buddhists; 29% currently smoking; Active minutes = 195 ± 98 min/wk	Intervention: Usual care + 35-min session that included mindful walking on five days/wk for eight wk (total = 40 sessions) Mindful walking: 20 min of each 35-min session; Walking while focusing on and controlling breathing Setting: Participants' homes and communities; Participants did not walk together	Emotional awareness: ↑ in intervention and control groups from baseline to Wk 4, 8, and 12	Six-min walk distance: ↑ in intervention group from baseline to Wk 4, 8, and 12; Distance in intervention group > distance in control group at Wk 8 and 12 HRV: NS
Shi et al., 2019 (U.S.)	RCT, Parallel, Multi-session	Sedentary adults, ≥ 18 y of age *n* = 38; Entire Sample: Age = 49 ± 14 y; BMI = 30.1 ± 7.6 kg· m^{-2}; 87% female; 3% Asian; 18% Black; 3% Native American/Alaskan Native; 76% White; 84% college-educated; 29% had a chronic medical condition	Control: Only received biweekly emails encouraging physical activity Intervention: 60-min session that included mindful walking on one day/wk for four wk (total = four sessions) Mindful walking: 40 min of each 60-min; Walking while focusing on bodily sensations and breathing Setting: Unspecified indoor space; Participants walked both alone and together	Stress: ↓ in intervention group at Wk 4 Depression: NS Trait mindfulness: NS Health-related quality of life: NS	Self-reported weekly physical activity: ↑ in intervention and control groups Step count: NS

			Control: Usual care		
Lin et al., 2019 (Taiwan)	RCT, Parallel, Multi-session	Adults with mild-to-severe COPD *n* = 78; Age = 71 ± 8 y; 72% chronic exercisers; 29% currently smoking	Intervention: Usual care + ~30-min session that included mindful walking on five day/wk for eight wk (total = 40 sessions) Mindful walking: 23 min of each ~30-min session; Walking while focusing on and controlling breathing Setting: Walking space not described; Participants did not walk together	Anxiety and Depression: ↓ in intervention group from baseline to Wk 4, 8, and 12 (lower in intervention group at Wk 4, 8, and 12)	NR
C.-H. Yang and Conroy, 2019 (U.S.)	Uncontrolled study, multi-session	Adults, ≥ 65 y of age *n* = 27; Age = 73 ± 6 y; 83% female; Only race reported was White (79%); 62% college-educated; 10% had a history of mindfulness or meditation	Intervention: 30-min session on eight separate days within four wk (total = 8 sessions; Sessions 2–7 included mindful walking) Mindful walking: Number of min of walking that were mindful walking increased from 0 min in Session 1 to 30 min in Session 8 Setting: Outdoor arboretum; Participants did not walk together	Negative affect: ↓ pre- to post-walk State mindfulness: ↑ pre- to post-walk	NR

Shors et al., 2018 (U.S.)	RCT, Parallel, Multi-session	Adult females not engaged in a chronic exercise program or a formal meditation practice, 18–40 y of age *n* = 105; Age (median and range) = 20 (18–32) y	Controls: No training (wait-list), only mental training, and only physical training Intervention: 60-min MAP Training My Brain™ session that included meditative walking on two days/wk for six wk (total = 12 sessions) Meditative walking: 10 min of each 60-min session; Walking in a circle while focusing on the feet Setting: Group exercise room in a recreational facility; Participants walked together	Post-traumatic thoughts and emotions: ↓ in MAP Training and only mental training groups Ruminative thoughts: ↓ in MAP Training group Self-worth: ↑ in MAP Training group	NR

Gotink et al., 2016 (Netherlands)	Non-randomized controlled study, Parallel, Multi-session	Adults who had previously participated in either a MBCT or MBSR course *n* = 29; Age = 54 ± 9 y; 69% female; 70–80% college educated; 50–100% had a history of depression	Control: The period leading up to the intervention (control period and intervention period equal in length) Intervention: 1-, 3-, or 6+-day mindful walking retreats Mindful walking: Walking while paying attention to the senses, emotions, thoughts, and automatic behavioral patterns Setting: Along the river Rhine; Participants walked alone or together, depending on the study arm	Positive affect ↑ across the intervention Negative affect: ↓ across the intervention State mindfulness: NS Trait mindfulness: ↑ across the intervention Allowing negative emotions and thoughts: NS Depression, anxiety, stress, and brooding: NS change across intervention vs. across control	NR

Gainey et al., 2016 (Thailand)	RCT, Parallel, Multi-session	Adults with type 2 diabetes, 40–75 y of age *n* = 23; Age = 58 ± 10 y; Mass = 66 ± 9 kg; Height = 156 ± 7 cm; BMI = 26.3 ± 4.5 kg·m^{-2}; %BF = 33 ± 8%; 83% female	Control[b]: Traditional walking Intervention: 50-min session that included Buddhist meditative walking on three day/wk for 12 wk (total = 36 sessions) Meditative walking: 30 min of each 50-min session; Walking while focusing on the feet contacting the treadmill and repeating the mantra “Budd” and “Dha” with each foot strike Setting: Treadmill in a university laboratory; Participants did not walk together	NR	SBP and DBP: ↓ in intervention group BMI and %BF: NS VO_{2max}: ↑ in intervention and control groups FMD: ↑ in intervention and control groups PWV: ↑ in intervention group ABI: NS FBG: ↓ in intervention and control groups HbA1c: ↓ in intervention group TC, HDL-C, LDL-C, and HOMA-IR: NS

Prakhinkit et al., 2014 (Thailand)	RCT, Parallel, Multi-session	Adult women with mild-to-moderate depressive symptoms and normal mobility who could provide independent self-care, 60–90 y of age *n* = 40; Age = 74 ± 7 y; Mass = 57 ± 11 kg; Height = 149 ± 8 cm; BMI = 25.5 ± 4.1 kg·m^{-2}; %BF = 38 ± 8%	Controls: No walking (sedentary control) and traditional walking Intervention: 20–30-min session of Buddhism meditative walking on three day/wk for 12 wk (total = 36 sessions) Buddhism meditative walking: Full time of each session; Walking while practicing mindfulness, focusing on the rhythmic swinging of both arms, and repeating the mantra "Budd" and "Dha" with each arm swing Setting: 50-m indoor track at a university hospital; Unclear if participants walked alone or together	Depression: ↓ in intervention group	SBP: ↓ in intervention and traditional walking groups from baseline (lower in intervention group) DBP: ↓ in intervention and traditional walking groups from baseline (lower in intervention group) RHR: NS %BF: ↓ in intervention group Six-min walk distance: ↑ in intervention and traditional walking groups from baseline FMD: ↑ in intervention and traditional walking groups from baseline TC, TG, and CRP: ↓ in intervention and traditional walking groups from baseline LDL-C, IL-6, and cortisol: ↓ in intervention group Nitric oxide: ↑ in intervention and traditional walking groups from baseline

Teut et al., 2013 (Germany)	RCT, Parallel, Multi-session	Adults with increased psychological distress, 18–65 y of age *n* = 74; Age = 52 ± 9 y; BMI = 24.6 ± 4.8 kg· m^{-2}; 89% female	Control: Wait-list Intervention: 60-min session that included mindful walking on two days/wk for four wk (total = eight sessions) Mindful walking: 10 min of each 60-min session; Walking while focusing on bodily sensations and breathing Setting: Local streets and outdoor parks; Unclear if participants walked alone or together	Psychological distress: ↓ in intervention and control groups from baseline to Wk 4; NS differences between groups at Wk 12 Stress: ↓ in intervention group from baseline to Wk 4 and 12 Quality of Life-Physical Component: NS Quality of Life-Mental Component: ↑ in intervention group from baseline to Wk 4 and 12 Quality of life scales for mental health, vitality, and emotional role functioning: ↑ in intervention group from baseline to Wk 4 (only emotional role functioning at Wk 12)	NR

Unless stated otherwise, the ↑ and ↓ represent statistically significant within-group changes or between-group differences in the measures of mental and cardiovascular health. [a]The *n* given in the table is the number of participants for which data were analyzed. Unless indicated otherwise with the phrase "Entire Sample," the participant demographics represent the intervention group. Abbreviations: RCT: randomized controlled trial; y: years; min: minute(s); wk: week; NS: non-significant; SBP: systolic blood pressure; DBP: diastolic blood pressure; COPD: chronic obstructive pulmonary disease; BMI: body mass index; kg: kilogram(s); m: meter(s); HRV: heart rate variability; U.S.: United States; NR: not reported; MAP: mental and physical; MBCT: mindfulness-based cognitive therapy; MBSR: mindfulness-based stress reduction; cm: centimeter(s); %BF: percent body fat; VO_{2max}: aerobic capacity; FMD: flow-mediated dilation of the brachial artery; PWV: pulse-wave velocity; ABI: ankle-brachial index; FBG: fasting blood glucose; HbA1c: glycated hemoglobin; TC: total cholesterol; HDL-C: high-density lipoprotein cholesterol; LDL-C: low-density lipoprotein cholesterol; HOMA-IR: homeostatic model assessment of insulin resistance; RHR: resting heart rate; TG: triglycerides; CRP: C-reactive protein; IL-6: interleukin-6

Mental Health (Single- and Multi-Session Studies)

Four single-session studies evaluated the effects of meditative or mindful walking on mental health and reported significant improvements (Ameli et al., 2021; Bigliassi et al., 2020; Cox et al., 2018; Shin et al., 2013). Significant improvements were in affect (Bigliassi et al., 2020; Cox et al., 2018), anxiety (Shin et al., 2013), attentional focus (Cox et al., 2018), distress (Ameli et al., 2021), enjoyment of physical activity (Bigliassi et al., 2020; Cox et al., 2018), happiness (Shin et al., 2013), perceived activation (Bigliassi et al., 2020), self-esteem (Shin et al., 2013), state mindfulness overall (Ameli et al., 2021; Bigliassi et al., 2020), and state mindfulness of the body (Cox et al., 2018).

Nine multi-session studies evaluated the effects of meditative or mindful walking on mental health (Gotink et al., 2016; Lin et al., 2019; Lin & Yeh, 2021; Prakhinkit et al., 2014; Shi et al., 2019; Shors et al., 2018; Srisoongnern et al., 2021; Teut et al., 2013; C.-H. Yang & Conroy, 2019). All studies except one (Srisoongnern et al., 2021) reported significant improvements on at least one measure of mental health, including affect (Gotink et al., 2016; C.-H. Yang & Conroy, 2019), anxiety (Lin et al., 2019), depression (Lin et al., 2019; Prakhinkit et al., 2014), distress (Teut et al., 2013), emotional awareness (Lin & Yeh, 2021), stress (Shi et al., 2019), post-traumatic thoughts (Shors et al., 2018), quality of life (Teut et al., 2013), ruminative thoughts (Shors et al., 2018), self-worth (Shors et al., 2018), state mindfulness overall (C.-H. Yang & Conroy, 2019), and stress (Teut et al., 2013).

In the following subsections, the single- and multi-session studies are collated and described in the context of each other. The measures of mental health have been grouped by how they are reported together in the literature.

Affect and Enjoyment of Physical Activity

Affect and enjoyment of physical activity were measured in arbitrary units via questionnaires at multiple timepoints in the studies. Two single-session studies reported an increase in positive affect and used mindful walking as the intervention (Bigliassi et al., 2020; Cox et al., 2018). Participants walked while listening to a mindfulness script to direct their attention to the present moment. The scripts facilitated mindful walking sessions, which lasted 4–6 minutes (Bigliassi et al., 2020) and 10 minutes (Cox et al., 2018), respectively. Affect was more positive after 4–6 minutes of mindful walking than after the same duration of mindless walking and traditional walking (Feeling Scale: 3.3 ± 0.2 vs. 1.7 ± 0.4 vs. 2.6 ± 0.2, $p \leq 0.018$ for mindful vs. mindless and mindful vs. traditional). Enjoyment of physical activity was also higher after mindful walking than after both controls (Physical Activity Enjoyment Scale: 96.7 ± 2.9 vs. 78.8 ± 3.7 vs. 83.4 ± 2.4, $p < 0.001$ for mindful vs. mindless and mindful vs. traditional) (Bigliassi et al., 2020). Similarly, affect was more positive and enjoyment was higher during 10 minutes of mindful walking than during the same duration of traditional walking (Feeling Scale: 1.39 ± 1.66 vs. 0.87 ± 2.00, $p = 0.02$, $\eta_p^2 = 0.22$, a moderate effect; Physical Activity Enjoyment Scale: 4.3 ± 1.1 vs. 3.8 ± 1.0, $p < 0.001$, $\eta_p^2 = 0.36$, a moderate effect) (Cox et al., 2018). Collectively, these findings suggest that just 4–10 minutes of indoor or outdoor guided mindful walking increase positive affect and are more enjoyable than traditional walking. An important caveat to these conclusions is that neither the 4–6-minute walk nor the 10-minute walk was an independent intervention. Other parts of the intervention may have confounded the effects of mindful walking on affect and enjoyment. Exercise intensity may also modulate how mindful walking influences affect and enjoyment. One study allowed walking at a self-selected pace (Bigliassi et al., 2020), and the other study required walking at 65% of participants' respective heart rate reserve (Cox et al., 2018). Future studies should evaluate interventions that comprise nothing else but mindful walking at varying intensities.

In addition to intensity, the frequency of mindful walking is also important. Two multi-session studies reported significant effects of more than one mindful walking bout on affect (Gotink et al., 2016; C.-H. Yang & Conroy, 2019). In one study, affect was measured by 18 Likert-type items in participants before and after a 1, 3, or 6+-day retreat that included mindful walking (Gotink et al., 2016). Participants' affect data were combined across all three retreat lengths. After the retreat, participants' positive affect was higher ($\beta = 0.91$, 95% CI [0.48, 1.33], $p < 0.001$) and negative affect was lower ($\beta = -0.71$ [−1.08, −0.34], $p < 0.001$). An important note is that the participants already had mindfulness experience before the retreat and completed seated meditation during the retreat. These potential confounders may influence affect independently of mindful walking, underscoring the need for studies that require mindful walking as an independent intervention.

The only other multi-session study that reported affect was an 8-session study in which mindful walking took place in an outdoor arboretum (C.-H. Yang & Conroy, 2019). All eight sessions lasted 30 minutes, and Sessions 2–7 included 5–30 minutes of mindful walking. Mean negative affect after the sessions was lower than before the sessions (Patient-Reported Outcomes Measurement Information System: $p < 0.01$, $d = -0.61$, a moderate effect). Negative affect after the sessions was negatively associated with increases in state mindfulness across the sessions (estimate ± SE: $\beta = -0.27 \pm 0.09$, $p < 0.01$). The findings suggest that mindful walking lifts the mood and that being more mindful while walking lifts the mood more (C.-H. Yang & Conroy, 2019). The study took place outdoors like the studies by Bigliassi et al. (2020) and Gotink et al.

(2016) that also reported greater positive affect because of mindful walking. It is worth exploring if walking setting modulates the effects of mindful walking on affect.

Attentional Focus

Attentional focus was measured in arbitrary units via a questionnaire after an intervention. One of the single-session studies reported the effects on attentional focus (Cox et al., 2018). Attentional focus was more associative during 10 minutes of mindful walking than during the same duration of traditional walking (Tammen's attentional focus scale: 20.65 ± 18.07 vs. 57.70 ± 25.38, $p < 0.001$, $\eta_p^2 = 0.67$, a strong effect). This suggests participants focused more on internal stimuli and interoceptive cues (e.g., breathing, heart rate, and skeletal muscle contraction) during mindful walking than during traditional walking. Despite only one article on this topic, this finding aligns with the yoga literature (Mackenzie et al., 2014). Thus, mindful walking may be a convenient way to recenter the attention on internal cues instead of external cues.

Distress, Stress, and Quality of Life

Distress, stress, and quality of life were measured in arbitrary units via questionnaires at multiple timepoints during the studies. Distress was reported by one single-session study that had participants complete two separate 20-minute mindful walks, one in an urban area and one in a natural area (Ameli et al., 2021). The urban area was a campus with buildings, sidewalks, crosswalks, and some grassy areas and trees. The natural area was a two-acre healing garden with woodlands, stones, a natural stream, and wildlife. Distress decreased after mindful walking in the natural area (Distress Thermometer: $p < 0.01$; effect size r = [Wilcoxon's z/# of observations] = 0.61, a strong effect). Also, post-walk distress was lower ($p = 0.02$, $r = 0.51$, a strong effect) after walking in the natural area than after walking in the urban area. A natural area may play a role in mindful walking decreasing distress. However, this role is uncertain because the only multi-session study to report distress showed mindful walking does not have to occur in a natural area to decrease distress (Teut et al., 2013).

The study by (Teut et al., 2013) assigned distressed adults to a wait-list control or mindful walking intervention. The intervention was 60-minute sessions that included 10 minutes of mindful walking on city streets and in parks two days per week for four weeks. After four weeks, the intervention group was told to keep exercising alone until the end of the study (8 more weeks). At Week 4, distress had decreased from baseline in the control and intervention groups, but more in the intervention group (100-millimeter visual analog scale, intervention vs. control: −24.0 95% CI [−31.4, −16.7] vs. −10.4 [−17.5, −3.3], $p = 0.010$). The difference between groups was not present at Week 12 ($p = 0.562$), so the decrease in distress from mindful walking was not sustained.

The same washing out, whereby the effects of mindful walking appear to wear off, has been reported for stress. Sedentary adults completed 4 weeks of mindful walking (intervention group), while the control group only received emails that encouraged them to exercise (Shi et al., 2019). The intervention group received the same encouragement and attended one, 60-minute session per week that included 10 minutes of group mindful walking and 30 minutes of individual mindful walking. The intervention decreased stress from baseline to Week 4 (Perceived Stress Scale: $\beta = -1.21$, 95% CI [−2.41, −0.01], $p < 0.05$). This decrease was larger than the decrease in the control group ($p = 0.025$). But four weeks later at the Week 8 follow-up, the intervention group's stress did not differ from baseline ($p > 0.05$) (Shi et al., 2019). The studies by Teut et al. (2013) and Shi et al. (2019) suggest the effect of mindful walking on distress and stress may be transient. Maintaining the effects may require an ongoing mindful walking practice.

In addition to the effects possibly being transient, there may be a dose threshold after which mindful walking decreases stress. Just 1, 3, or 6+ days of a mindful walking retreat did not decrease stress in another study (Gotink et al., 2016). Several weeks, a duration used in the interventions by Teut et al. (2013) and Shi et al. (2019), may be required. Further evidence for this is that the four-week intervention by Teut et al. (2013) decreased stress from baseline to Week 4 only in the intervention group (Cohen's Perceived Stress Scale: −8.8, 95% CI [−10.8, −6.8], $p < 0.001$ vs. the control group). By Week 12, stress had decreased from baseline in both the control and intervention groups, but more in the intervention group (intervention vs. control: −7.2 95% CI [−9.4, −5.0] vs. −3.8 [−5.7, −1.7], $p = 0.031$). In this case, the effect on stress was sustained at Week 12. Why stress remains lower after mindful walking in some studies by not others is unclear. Studies should implement follow-ups more frequently than just once or twice across 4–8 weeks after a mindful walking intervention. More frequent follow-ups will allow researchers to discern precisely when the effects of mindful walking on distress and stress cease.

Another commonality between the studies by Teut et al. (2013) and Shi et al. (2019) is that both studies reported quality of life. Overall health-related quality of life did not change in either study (Teut: Short Form-36 Health Survey [SF-36], $p \geq 0.05$; Shi: Mental Health Inventory-5, $p \geq 0.05$) (Shi et al., 2019; Teut et al., 2013). However, Teut et al. (2013) reported that the SF-36-Mental Component increased from baseline to Weeks 4 and 12 only in the intervention group (Week 4: 9.1, 95% CI [6.2, 12.0], $p < 0.001$ vs. the control group; Week 12: 7.5 [4.2, 10.8], $p = 0.021$ vs. the control group). The SF-36 scores for mental health, vitality, emotional role functioning, and social role functioning increased from baseline to Week 4 only in the intervention group (all higher than in the control group, $p < 0.05$). The increases were sustained at Week 12, but only emotional role functioning was higher than in the control group ($p = 0.027$). This study's key takeaway is that a mindful walking program decreased distress and stress while increasing facets of mental-health quality of life for at least four weeks (Teut et al., 2013). Whether mindful walking increases physical-health quality of life or overall quality of life is less promising. The same is true for meditative walking because six weeks did not increase the quality of life of people with heart failure (Srisoongnern et al., 2021).

The data from Ameli et al. (2021), Teut et al. (2013), and Shi et al. (2019) justify new studies to explore the effects of mindful walking on distress, stress, and mental-health quality of life. New studies should explore the possible effects of walking setting and minimize confounders that were present in the three studies described here: not having a traditional walking control group and having other physical exercise and/or mindfulness practices.

Perceived Activation

Perceived activation was measured in arbitrary units via a questionnaire after an intervention. In the study of 4–6 minutes of mindful walking, perceived activation (being mentally worked up) was also reported (Bigliassi et al., 2020). Perceived activation was lower after mindful walking than after the mindless walking and traditional walking (Felt Arousal Scale: 2.3 ± 0.2 vs. 3.3 ± 0.3 vs. 2.7 ± 0.2; $p = 0.002$ for mindful vs. mindless, and $p = 0.039$ for mindful vs. traditional). Mindful walking seems to decrease perceived activation in a way that traditional walking or meditation alone does not. In one parallel-arm study, participants stretched, walked (traditional), or meditated for 10 minutes (Edwards et al., 2018). After 10 minutes, perceived activation increased in the walking group (Felt Arousal Scale: pre vs. post = 1.9 ± 0.9 vs. 3.2 ± 0.9, $p < 0.001$). Perceived activation increasing after traditional walking (Edwards et al., 2018) but decreasing after mindful walking (Bigliassi et al., 2020) suggests mindfulness may reverse the typical arousal response to walking. Another area to be explored is whether mindful walking decreases a person's tendency toward low or high arousal as a general personality characteristic.

State and Trait Mindfulness

State and trait mindfulness were measured in arbitrary units via questionnaires at multiple timepoints during the studies. Three single-session studies mentioned above also reported state mindfulness as either overall state mindfulness (Ameli et al., 2021; Bigliassi et al., 2020) or state mindfulness of the body (Cox et al., 2018). Mindful walking for 20 minutes in a natural area, but not in an urban area, increased overall state mindfulness (Mindful Attention Awareness Scale, state version, lower scores denote more mindfulness based on how the authors used the scale, medians [interquartile ranges]: pre vs. post = 4.5 [3.0, 9.5] vs. 1.5 [0.0, 4.0], $p = 0.01$, $r = 0.52$, a strong effect) (Ameli et al., 2021). In a separate study, overall state mindfulness was higher after 4–6 minutes of mindful walking in an outdoor park than after mindless walking or traditional walking in the same setting (State Mindfulness Scale, mean ± standard error: 76.3 ± 2.0 vs. 58.5 ± 2.3 vs. 64.8 ± 1.6, $p \leq 0.025$ for mindful vs. mindless and mindful vs. traditional) (Bigliassi et al., 2020).

Similar findings were reported for state mindfulness of the body, which was greater during indoor mindful walking on a treadmill than during traditional walking in the same setting (State Mindfulness Scale for Physical Activity, mean score per item within the body subscale: 3.1 ± 0.5 vs. 2.4 ± 0.8, $p < 0.001$, $\eta_p^2 = 0.40$, a moderate effect) (Cox et al., 2018). Participants in this study listened to a mindfulness script (Cox et al., 2018), as the participants did in the study by Bigliassi et al. (2020). However, a mindfulness script may not be needed to increase overall state mindfulness because Ameli et al. (2021) only verbally instructed participants to be mindful. Cultivating mindfulness without scripts could save money and time. Additionally, determining the most effective form of instruction is important because maximizing state mindfulness may amplify the effects of mindful walking on other measures of mental health. This argument is supported by the finding that state mindfulness of the body is moderately correlated with more associative attentional focus ($r = -0.56$, $p = 0.01$) and a greater enjoyment of exercise ($r = 0.44$, $p = 0.04$) (Cox et al., 2018).

Mindfulness scripts were not used in either of the two multi-session studies that reported the effects of mindful walking on overall state mindfulness (Gotink et al., 2016; C.-H. Yang & Conroy, 2019). In one study, participants were assigned mindfulness tasks before each walk in an outdoor arboretum (C.-H. Yang & Conroy, 2019). Throughout the 8-session mindful walking program, overall state mindfulness was greater post-walk than pre-walk ($p < 0.01$, $d = 0.55$, a moderate effect) (C.-H. Yang & Conroy, 2019). In the other study, participants received verbal instructions before a 1-, 3-, or 6+-day walking retreat (Gotink et al., 2016). State mindfulness was reported before and after the retreat. Changes across the retreat did not differ from changes across the control period that preceded the retreat (Curiosity and Decentering subscales of the Toronto Mindfulness Scale: $p \geq 0.05$, CIs for Cohen's d included the null value 0) (Gotink et al., 2016). The findings of C.-H. Yang and Conroy (2019) and Gotink et al. (2016) disagree, and this may be because that latter group analyzed the data for all participants collapsed across retreat duration. In other words, the state mindfulness data were analyzed for all participants from the 1-, 3, and 6+-day retreats combined. Just 1–3 days of mindful walking may not be enough to change state mindfulness. Based on the studies in the present systematic review, we first recommend future studies report correlations between state mindfulness and other measures of mental health. It may be worth including state mindfulness as a covariate in inferential statistical analyses that test hypotheses about the effects of mindful walking on other measures of mental health. Second, researchers should measure state mindfulness with scales of state mindfulness, not trait mindfulness.

The reason for the second recommendation is that one study reported state mindfulness when the data indicated trait mindfulness. The study of 1-, 3-, or 6+-day walking retreats reported data from five Likert-type items as state mindfulness, but four of the items were from the Five Facet Mindfulness Questionnaire, a measure of trait mindfulness. The four items point to general tendencies to focus one's attention, not a person's capacity to do so in a specific situation (i.e., state). Thus, we believe Gotink et al. (2016) reported trait mindfulness from the five items. From our point of view, we conclude that trait mindfulness increased across the intervention (five items: $\beta = 0.98$, 95% CI [0.56, 1.40], $p < 0.001$) (Gotink et al., 2016). In contrast to this finding, Shi et al. (2019) reported that their four-week walking program did not change trait mindfulness (Freiburg Mindfulness Inventory) at Week 4 ($p = 0.34$) or Week 8 ($p = 0.37$) of follow-up. Two studies are not enough evidence to conclude the effects of multiple sessions of mindful walking on trait mindfulness. New studies should implement weeks-long interventions of mindful walking and compare pre- and post-intervention measurements on scale questions about general tendencies toward or away from mindfulness (i.e., trait mindfulness).

Anxiety, Depression, Happiness, and Self-Esteem

Anxiety, depression, happiness, and self-esteem were measured in arbitrary units via questionnaires at multiple timepoints during the studies. Only one single-session study reported the effects of meditative walking on anxiety, happiness, and self-esteem (Shin et al., 2013). Participants completed two, 70-minute bouts of meditative walking and two, 70-minute bouts of athletic walking (all bouts also included two, 10-minute bouts of rest). One bout of each walking type was completed in a gymnasium, and the other bout was completed in a forest. In each respective setting, meditative walking decreased state anxiety by 25% and 32% (Spielberger State-Trait Anxiety Inventory-Form X1; $p < 0.05$) and increased self-esteem by 13% and 19% (Rosenberg Self-Esteem Scale, $p < 0.05$). Meditative walking in the gymnasium and forest also increased happiness by 34% and 73%, respectively ($p < 0.05$). State anxiety, self-esteem, and happiness were all changed more by meditative walking than by athletic walking ($p < 0.01$). Notably, regardless of walking type, happiness increased more in the forest than in the gymnasium (all comparisons $p < 0.05$) (Shin et al., 2013). This finding is similar to the finding by Ameli et al. (2021) on distress and the finding by Navalta et al. (2021) that sitting and traditional walking outdoors elicited greater comfort and calm in desert and forest environments than indoors and outdoors in an urban environment. Collectively, the findings of Ameli et al. (2021), Shin et al. (2013), and Navalta et al. (2021) bolster the case for future studies of meditative and mindful walking to prioritize outdoor interventions in natural areas (e.g., green areas with woodlands and foliage).

Among anxiety, self-esteem, and happiness, only anxiety has been reported in multi-session studies, often alongside depression. One 12-week study reported both anxiety and depression in people with mild-to-severe chronic obstructive pulmonary disease (COPD) who received usual care (control) or usual care plus mindful walking (intervention) (Lin et al., 2019). Participants were measured across an eight-week mindful walking intervention and a four-week follow-up. Anxiety and depression scores decreased across 12 weeks only in the intervention group (Hospital Anxiety and Depression Scale [HADS]). The respective adjusted estimates in HADS-Anxiety and HADS-Depression were $\beta = -2.51$ 95% CI [−3.69, −1.33] ($p < 0.001$) and

$\beta = -5.19$ [−6.86, −3.53] ($p < 0.001$). The respective percent decreases from baseline to Week 12 were −57% (Mean: 3.03 → 1.29; SD not reported; $p < 0.05$) and −62% (Mean: 7.00 → 2.63; SD not reported; $p < 0.05$). At Week 12, HADS-Anxiety and HADS-Depression scores in the intervention group were 60% and 87% lower, respectively, than in the control group (both $p < 0.05$) (Lin et al., 2019). The study by Lin et al. (2019) suggests multiple sessions of mindful walking decrease anxiety and depression. However, the decreases may have come from other parts of the intervention or merely exercising regardless of the mindfulness component. The intervention group was not compared to a traditional walking control group.

These potential confounders also limited the other multi-session study to report anxiety or depression (Gotink et al., 2016; Shi et al., 2019). Anxiety and depression did not change across a 1-, 3-, or 6+-day mindful walking retreat more than across a control period before the retreat (Depression Anxiety Stress Scale-21: $p > 0.05$ and 95% CIs for Cohen's *d* included the null value 0) (Gotink et al., 2016). Comparably, four weeks of mindful walking did not change depression by Week 4 or the follow-up at Week 8 (Brief Edinburgh Depression Scale: $p = 0.92$ and $p = 0.80$, respectively) (Shi et al., 2019). Control groups that walk but do not practice mindfulness are sorely needed.

A traditional walking control group was included in the only other multi-session study to report depression. The study compared 12 weeks of no walking, traditional walking, and meditative walking (Prakhinkit et al., 2014). Depression decreased from baseline to Week 12 only in the meditative walking group (Thai Geriatric Depression Scale: −49%, $p < 0.05$). In the same group, depression was 116% and 80% lower than in the no walking and traditional walking groups (both $p < 0.05$), respectively (Prakhinkit et al., 2014). The 12-week meditative walking intervention distinguishes this study from the study by Lin et al. (2019), who used an eight-week mindful walking intervention. Despite the difference in duration and walking type, both studies reported decreases in the intervention group's depression. More studies will need to determine the efficacy of 8-12 weeks of meditative and mindful walking in populations beyond people with COPD (Lin et al., 2019) and women over 60 years of age with mild-to-moderate depression (Prakhinkit et al., 2014). The precise location of mindful walking was not reported by Lin et al. (2019), but Prakhinkit et al. (2014) reported that meditative walking took place indoors at a university hospital. As in single-session studies, the walking setting in multi-session studies is an important variable to examine in future studies.

Emotional Awareness

Emotional awareness was measured in arbitrary units via a questionnaire at multiple timepoints during one study. That one study was a single multi-session study that reported the effects of mindful walking on emotional awareness (Lin & Yeh, 2021). Adults with mild-to-severe COPD completed usual care (control) or usual care plus mindful walking (intervention). The emotional awareness scale of interoceptive awareness decreased from baseline to Weeks 4, 8, and 12 in the control group (all $p < 0.05$) but increased from baseline to Week 4 ($\beta = 1.39$, 95% CI [1.09, 1.70], $p < 0.0001$), Week 8 ($\beta = 1.66$ [1.35, 1.96], $p < 0.0001$), and Week 12 ($\beta = 1.79$ [1.49, 2.10], $p < 0.0001$) in the intervention group (Lin & Yeh, 2021). This finding makes mindful walking a promising method to increase emotional awareness. However, separating the effects of mindful walking from other parts of this study's intervention is impossible.

Post-Traumatic Thoughts, Ruminative Thoughts, and Self-Worth

Post-traumatic thoughts, ruminative thoughts, and self-worth were measured in arbitrary units via questionnaires at multiple timepoints during one study. As with emotional awareness, only one multi-session study reported the effects of meditative walking on post-traumatic thoughts (Post-Traumatic Cognitions Inventory), ruminative thoughts (Ruminative Responses Scale), and self-worth (Best Self Scale). The study compared the effects of a wait-list control group, only mental training (seated meditation and mindful walking), only physical training (aerobic exercise), and mental and physical training combined, called MAP Training My Brain™. Both the mental training and MAP Training decreased post-traumatic thoughts from baseline to Week 6 ($p = 0.005$ and $p < 0.05$, respectively). Only the MAP Training decreased ruminative thoughts ($p < 0.005$) and increased self-worth ($p < 0.05$) (Shors et al., 2018). All three outcomes were separately evaluated in a sub-sample of participants who had experienced sexual violence. In this sub-sample, only MAP Training decreased post-traumatic thoughts and ruminative thoughts and increased self-worth ($p < 0.05$) (Shors et al., 2018). Though an important contribution to the literature, this study does not offer data on the independent effects of mindful walking. To investigate these effects, a future study should compare a wait-list control group, seated meditation group, traditional walking group, and meditative walking group.

Cardiovascular Health (Single- and Multi-Session Studies)

None of the four single-session studies evaluated the effects of meditative or mindful walking on cardiovascular health (Ameli et al., 2021; Bigliassi et al., 2020; Cox et al., 2018; Shin et al., 2013). This finding reveals a major gap in the literature that needs to be filled with high-quality studies. These studies would begin revealing the acute effects of one bout of meditative or mindful walking on measures of cardiovascular health. Important measures to consider are arterial stiffness, endothelial function, heart rate variability, systolic blood pressure (SBP), diastolic blood pressure (DBP), and the serum concentrations of glucose, insulin, lipids, and lipoproteins.

Four of the multi-session studies that evaluated the effects of meditative or mindful walking on mental health also evaluated the effects on cardiovascular health (Lin & Yeh, 2021; Prakhinkit et al., 2014; Shi et al., 2019; Srisoongnern et al., 2021). One separate multi-session study only evaluated the effects on cardiovascular health (Gainey et al., 2016). All five studies except one (Srisoongnern et al., 2021) reported significant improvements on at least one measure in the intervention group. The significant improvements were in aerobic capacity (Gainey et al., 2016), C-reactive protein (Prakhinkit et al., 2014), cholesterol (total and low-density lipoprotein) (Prakhinkit et al., 2014), cortisol (Prakhinkit et al., 2014), fasting blood glucose (Gainey et al., 2016), glycated hemoglobin (Gainey et al., 2016), flow-mediated dilation of the brachial artery (Gainey et al., 2016; Prakhinkit et al., 2014), interleukin-6 (Prakhinkit et al., 2014), nitric oxide (Prakhinkit et al., 2014), percent body fat (Prakhinkit et al., 2014), pulse-wave velocity (Gainey et al., 2016), self-reported physical activity (Shi et al., 2019), six-minute walk distance (Lin & Yeh, 2021; Prakhinkit et al., 2014), SBP and DBP (Gainey et al., 2016; Prakhinkit et al., 2014), and triglycerides (Prakhinkit et al., 2014).

In the following subsections, the multi-session studies are collated and described in the context of each other. The measures of cardiovascular health have been organized into subsections.

Exercise Capacity, Functional Status, and Physical Activity Level

Exercise capacity was measured as maximum oxygen consumption before and after an intervention. Functional status was measured via the six-minute walk test before and after an intervention. The test provided the six-minute-walk distance (6MWD), expressed as the total meters that patients walked in six minutes around a course or corridor (the length of the pathway was 25 to 50 m, depending on the study). Physical activity level was measured as self-reported step-counts and step-counts measured via wrist-worn Fitbits before and after an intervention. Only one study reported the effects of meditative walking on aerobic capacity (Gainey et al., 2016). Meditative walking was compared to traditional walking for 12 weeks. Both conditions increased maximum oxygen consumption from baseline (meditative: 28%; traditional: 15%; within-group pre vs. post $p < 0.05$). Based on the study's report, it does not appear that the increase differed between groups ($p \geq 0.05$). Meditative walking may be equally effective at increasing aerobic capacity. If so, this mind-body modality may be better than traditional walking because the former may benefit mental health more. New studies that test this hypothesis should employ rigorous parallel designs; report both mental and cardiovascular outcomes; and match the frequency, intensity, and duration of meditative and mindful walking.

Whereas aerobic capacity indicates a person's cardiorespiratory fitness, the 6MWD indicates a person's physical function. One study reported no effect of meditative walking on the 6MWD (Srisoongnern et al., 2021), and another reported positive effects (Prakhinkit et al., 2014). Srisoongnern et al. (2021) reported six weeks of meditative walking did not change the 6MWD in adults with heart failure ($p > 0.05$). In contrast, Prakhinkit et al. (2014) reported 12 weeks of meditative walking increased the 6MWD by 84% in adults over 60 years with mild-to-moderate depression ($p < 0.05$). The opposing findings of these two studies may have been caused by them sampling different populations and using different doses of meditative walking. The participants' heart failure in the study by Srisoongnern et al. (2021) may have hampered improvements in the 6MWD. Additionally, these participants completed meditative walking for

30–40 minutes on at least three days per week for 6 weeks. Participants in the study by Prakhinkit et al. (2014) did not have heart failure and completed meditative walking for 20–30 minutes on three days per week for 12 weeks. To clarify the effects of meditative walking on the 6MWD, new studies should evaluate several doses in various populations.

Another study reported positive effects of mindful walking on the 6MWD (Lin & Yeh, 2021). Eight weeks of mindful walking increased the 6MWD in people with COPD from baseline to Week 4 ($\beta = 22.11$, 95% CI [1.58, 89.56], $p = 0.01$), Week 8 ($\beta = 32.71$ [12.68, 52.75], $p = 0.002$), and Week 12 ($\beta = 25.38$ [4.04, 46.71], $p = 0.02$). At Weeks 8 and 12, the distance was greater in the intervention group than the control group by 13.1% ($p = 0.03$) and 12.7% ($p = 0.04$), respectively (Lin & Yeh, 2021). These positive effects suggest dose is important because participants walked mindfully for 20 minutes on five days per week for eight weeks. A standard practice of future studies should be quantifying the dose of mindful walking and comparing the effects to the same dose of traditional walking.

Besides aerobic capacity and the 6MWD, physical activity level has been reported as self-report and step count data (Shi et al., 2019). After four mindful walking sessions across four weeks, participants self-reported increased physical activity (Rapid Assessment of Physical Activity questionnaire; $\beta = 1.74$, 95% CI [0.80, 2.68], $p < 0.05$). However, participants' step counts did not corroborate their self-reported increase in physical activity because the step counts did not change from baseline ($p \geq 0.05$) (Shi et al., 2019). Chronic physical activity at a light-to-moderate intensity promotes cardiovascular health (American College of Sports Medicine, 2017c; Office of Disease Prevention and Health Promotion, n.d.). For this reason, it is important to determine whether mindful walking causes participants to increase their physical activity level more than traditional walking. This hypothesis is worth testing because participants have reported enjoying one bout of mindful walking more than one bout of traditional walking (Bigliassi et al., 2020; Cox et al., 2018). To circumvent recall bias, objective measures of physical activity from accelerometers should be used together with or in place of self-reported measures.

Body Composition

Body composition was measured as percent body fat via bioelectrical impedance analyses before and after an intervention. Two studies reported conflicting effects of meditative walking on body composition. Gainey et al. (2016) reported neither 12 weeks of meditative walking nor traditional walking changed percent body fat ($p \geq 0.05$). Separately, Prakhinkit et al. (2014) reported 12 weeks of meditative walking, but not traditional walking, decreased percent body fat. Percent body fat decreased by 5% (absolute percent decrease [38% → 33%], $p < 0.05$) from baseline to Week 12 (Prakhinkit et al., 2014). The cause of these conflicting findings is not immediately clear because both studies required a similar dose of meditative walking (20–30 minutes per session, three sessions per week, 12 weeks). A potential explanation is the participants in the study by Gainey et al. (2016) were less likely to lose body fat because they had a lower percent body fat at baseline than the participants in the study by Prakhinkit et al.(2014). Another perplexing issue in the study by Prakhinkit et al. (2014) is that meditative walking decreased percent body fat but traditional walking did not. Energy expenditure should have been similar among the groups because both groups walked at the same frequency and intensity for the same duration. Follow-up studies are needed before it can be verified that meditative walking improves body composition better than traditional walking. Baseline percent body fat should be explored as a confounder in the relationship between meditative walking and percent body fat.

Blood Markers of Glycemia, Lipidemia, and Inflammation

Blood markers of glycemia, lipidemia, and inflammation were measured before and after an intervention in blood taken from the antecubital vein after an eight-hour overnight fast. The venous blood was centrifuged to separate the erythrocytes from the plasma. The plasma marker concentrations were mostly measured in a certified clinical laboratory and expressed conventionally. The effects of meditative walking on glycemia and lipidemia have been reported by two studies. Gainey et al. (2016) reported that both meditative walking and traditional walking decreased fasting blood glucose (meditative: −12%, $p < 0.05$), but only meditative walking decreased long-term glycemia measured as glycated hemoglobin (−10%; $p < 0.05$). This decrease occurred without a change in insulin resistance ($p \geq 0.05$). Follow-up studies should expand on this initial study to determine whether meditative walking truly improves glycated hemoglobin better than traditional walking. A physiological mechanism should be sought because it is unclear why an equal dose of traditional walking did not similarly decrease glycated hemoglobin. Apart from the glycemic variables, Gainey et al. (2016) reported that meditative walking did not improve total cholesterol, high-density lipoprotein cholesterol (HDL-C), or low-density lipoprotein cholesterol (LDL-C) (all $p \geq 0.05$). Contrary to this finding, Prakhinkit et al. (2014) reported both traditional walking and meditative walking decreased total cholesterol (meditative: −9%, $p < 0.05$) and triglycerides (meditative: −27%, $p < 0.05$). Only meditative walking decreased LDL-C (meditative: −12%, $p < 0.05$).

The conflicting findings could have been caused by the different study populations. Prakhinkit et al. (2014) studied people without any cardiovascular diseases or type 2 diabetes. The unimproved blood lipids reported by Gainey et al. (2016) may have been because the participants were taking oral medications for type 2 diabetes. The specific medications were not reported, and certain diabetes medications affect blood lipid concentrations. Future studies should evaluate the effects of meditative walking on glycemia and lipidemia in different populations and whether these effects are modulated by the concurrent use of medications.

Besides glycemia and lipidemia, markers of inflammation (e.g., C-reactive protein and interleukin-6) and stress (cortisol) suggest cardiovascular health and risk. Only Prakhinkit et al. (2014) reported inflammatory and stress markers. Both traditional walking and meditative walking decreased C-reactive protein from baseline (meditative: −25%, $p < 0.05$), but only meditative walking decreased interleukin-6 (−22.2%, $p < 0.05$) and cortisol (−1.6% absolute percent decrease [11.9 → 10.3%]) from baseline (both $p < 0.05$) (Prakhinkit et al., 2014). This single study is not enough evidence to conclude that meditative walking decreases inflammation and physiological stress more than traditional walking. Nonetheless, the mindfulness applied during meditative walking may bring a sense of mental calm and clarity not achieved via traditional walking. A calmer and more accepting headspace may translate into lower physical inflammation and stress. Testing this hypothesis will require new studies that report together measures of mindfulness, mental calm, mental stress, inflammation, and physical stress.

Blood Pressure and Arterial Function

Blood pressure was measured before and after an intervention. The measurement method was not specified, but the SBP and DBP were expressed conventionally as mmHg. Arterial function was measured via flow-mediated dilation (FMD) of the brachial artery in millimeters or percent and via brachial-ankle pulse-wave velocity (PWV) in centimeters per second before and after an intervention. Both SBP and DBP were reported by three studies of meditative walking. Srisoongnern et al. (2021) reported six weeks of meditative walking did not decrease SBP or DBP ($p \geq 0.05$). In contrast, Gainey et al. (2016) reported 12 weeks of meditative walking but not traditional walking decreased SBP and DBP by 12% and 8%, respectively ($p < 0.05$). Similarly, Prakhinkit et al. (2014) reported 12 weeks of meditative walking decreased SBP and DBP by 7% and 8%, respectively ($p < 0.05$). In this study, 12 weeks of traditional walking also decreased SBP and DBP, but both SBP and DBP were lower in the meditative walking group at Week 12 ($p < 0.05$). The two studies in which meditative walking decreased SBP and DBP (Gainey et al., 2016; Prakhinkit et al., 2014) were twice as long as the study that showed no effect of meditative walking (Srisoongnern et al., 2021). Meditative walking may take more than six weeks before having significant effects on blood pressure. To determine whether meditative walking affects SBP and DBP differently than traditional walking, new studies should explore interaction effects between walking type and time (i.e., how long someone has been walking traditionally in the study).

Two of the three studies that reported decreases in SBP and DBP also reported improvements in either FMD of the brachial artery or brachial-ankle PWV. Improvements in the respective outcomes are increases in FMD and decreases in PWV. Gainey et al. (2016) reported meditative walking and traditional walking increased FMD similarly by 4.9% and 4.6%, respectively (absolute percent increases, $p < 0.05$). Only meditative walking decreased PWV (−10%, $p < 0.05$). In line with the finding of Gainey et al. (2016), Prakhinkit et al. (2014) reported a 5.7% and 3.8% increase in FMD because of meditative walking and traditional walking, respectively (absolute percent increases, $p < 0.05$). Nitric oxide, a potent vasodilator produced by endothelial cells that line the arteries and capillaries, was also increased by 225% and 178% by meditative walking and traditional walking, respectively (Prakhinkit et al., 2014). The FMD and nitric oxide improving with both types of walking suggest that walking per se—not the meditative part—causes the improvements. However, PWV only improving with meditative walking suggests that the meditative part may cause some of the improvement in arterial function. Future studies that compare meditative walking and traditional walking should match the frequency, intensity, and duration of walking completed by both groups. The groups should have similar health statuses, physical activity levels, and arterial function at baseline. Also, the statistical analyses that compare the groups should include baseline FMD, PWV, and nitric oxide as covariates. These features of the study design and statistical analysis plan will help identify whether meditation synergizes with walking to improve arterial function more than traditional walking.

Risk of Bias in the Included Studies

The risk of bias should be considered. Two studies were uncontrolled (Shin et al., 2013; C.-H. Yang & Conroy, 2019), meaning they are inherently at risk of selection bias and cannot indicate whether meditative or mindful walking improves mental or cardiovascular health more than no walking. In the eight studies that were randomized controlled trials (RCTs) with a parallel design (Gainey et al., 2016; Lin et al., 2019; Lin & Yeh, 2021; Prakhinkit et al., 2014; Shi et al., 2019; Shors et al., 2018; Srisoongnern et al., 2021; Teut et al., 2013), their risk of bias was assessed across five domains (Domains 1–5) by using the revised Cochrane Risk of Bias Tool for Randomized Trials (RoB 2) tool (Sterne et al., 2019). Seven of the eight studies had some concerns or a high risk of bias (Figure 6). These ratings were mostly caused by the randomization process not being concealed from the researchers, the researchers not being blind to the participants' groups, and the studies apparently lacking a pre-specified statistical analysis plan. The risk of bias in the two RCTs that had a crossover design (Ameli et al., 2021; Bigliassi et al., 2020) was assessed across six domains (Domains 1–5 and Domain S) by a preliminary and supplementary tool to the RoB 2 tool, called the RoB 2 for Crossover Trials (Sterne et al., 2019).

Like the RCTs with a parallel design, both crossover trials had some concerns or a high risk of bias (Figure 7) because of the randomization process and the researchers not being blinded to the participants' groups. The risk of bias in the two non-randomized controlled studies (Cox et al., 2018; Gotink et al., 2016) was assessed across seven domains (Domains 1–7) using the Risk of Bias in Non-randomized Studies - of Interventions (ROBINS-I) tool (Sterne et al., 2016). Both studies had a serious risk of bias because of the ratings in most domains except the classification of the interventions and selection of the reported results (Figure 8). In summary, 11 of the 12 controlled studies had a concerning risk of bias. Collectively, these 11 studies and the two uncontrolled studies constitute weak evidence that meditative or mindful walking improve mental or cardiovascular health.

Author and Year	D1: Randomization Process	D2: Deviations from the Intended Interventions	D3: Missing Outcome Data	D4: Measurement of the Outcome	D5: Selection of the Reported Results	Overall
Srisoongnern et al., 2021	+	+	+	+	+	+
Lin and Yeh, 2021	+	+	+	+	!	-
Shi et al., 2019	-	+	+	-	!	-
Lin et al., 2019	+	+	-	+	+	-
Shors et al., 2018	!	!	+	-	!	-
Gainey et al., 2016	-	+	!	!	!	-
Prakhinkit et al., 2014	!	+	+	!	!	!
Teut et al., 2013	+	+	+	+	!	!

+ Low risk
! Some concerns
- High risk

Figure 6. Risk of bias in the eight randomized controlled trials that had a parallel design, assessed across five domains (Domains 1–5) by using the revised Cochrane Risk of Bias (RoB 2) tool for trials that have a parallel design. D: Domain

Author and Year	D1: Randomization Process	DS: Period and Carryover Effects	D2: Deviations from the Intended Interventions	D3: Missing Outcome Data	D4: Measurement of the Outcome	D5: Selection of the Reported Results	Overall
Ameli et al., 2021	!	+	+	+	+	+	!
Bigliassi et al., 2020	!	+	+	+	-	!	-

+ Low risk; ! Some concerns; - High risk

Figure 7. Risk of bias in the two randomized controlled trials that had a crossover design, assessed across six domains (Domains 1–5 and Domain S) by using the revised Cochrane Risk of Bias (RoB 2) tool for trials that have a crossover design. D: Domain

Author and Year	D1: Confounding	D2: Selection of Participants into the Study	D3: Classification of the Interventions	D4: Deviations from the Intended Interventions	D5: Missing Outcome Data	D6: Measurement of the Outcome	D7: Selection of the Reported Results	Overall
Cox et al., 2018	Serious	Moderate	Low	Low	Low	Serious	Low	Serious
Gotink et al., 2016	Moderate	Serious	Low	Moderate	Serious	Serious	Low	Serious

Figure 8. Risk of bias in the two non-randomized controlled studies, assessed by using the Cochrane Risk of Bias in Non-randomized Studies - of Interventions (ROBINS-I) tool. Risk of bias in each domain is rated as Low, Moderate, Serious, Critical, or No Information. D: Domain

2.5 Discussion

The purpose of the present systematic review was to determine the effects of meditative and mindful walking on mental and cardiovascular health. To achieve this purpose, 14 studies from five online repositories for peer-reviewed journal articles were identified and evaluated by using a methodologically rigorous and replicable protocol. The review's main finding was that meditative and mindful walking may improve mental and cardiovascular health. However, these improvements must be interpreted cautiously because of the limitations of the included studies. Moreover, the meaningfulness of the improvements is unclear. The main finding and important caveats are explained in the next three subsections.

Effects on Mental and Cardiovascular Health

Of the studies reporting measures of mental health, all four single-session studies (Ameli et al., 2021; Bigliassi et al., 2020; Cox et al., 2018; Shin et al., 2013) and eight of the nine multi-session studies (Gotink et al., 2016; Lin et al., 2019; Lin & Yeh, 2021; Prakhinkit et al., 2014; Shi et al., 2019; Shors et al., 2018; Teut et al., 2013; C.-H. Yang & Conroy, 2019) reported significant improvements. Significant improvements are interesting to researchers but not inherently meaningful to participants. Therefore, how meaningful the improvements are to participants should be determined. A concept intended to capture this meaningfulness is the minimal clinically important difference (MCID) (Jaeschke et al., 1989; McGlothlin & Lewis, 2014). Researchers and clinicians establish MCIDs to make sense of observed changes in outcomes because of an intervention. An MCID is an estimate of the smallest change in an outcome that actually or practically improves the health or lives of a particular group. The best MCIDs are decided with input from the people themselves so that the MCIDs reflect the size of a change that matters to them. The value of MCIDs is that they go beyond statistics to gauge the true value of changes in outcomes. For example, in one study, a decrease in SBP of 1 mmHg after an intervention may be statistically significant but not meaningfully change the participants' health or life. An MCID for SBP asks, "what is the minimum decrease in SBP after an intervention that actually improves the health or lives of a given population?"

Population-specific MCIDs have not been established for most of the measures of mental health in the present review. However, three of the measures have relevant MCIDs that were exceeded because of meditative or mindful walking. First, in adults who smoke but have normal spirometry and no unstable diseases, the MCID on the State-Trait Anxiety Inventory (STAI) has been established as ~10 points (Corsaletti et al., 2014). One study that sampled adult females (smoking status not reported) reported that from pre- to post-walk, meditative walking in a gymnasium and forest decreased the mean STAI-Form X score by over 10 points (Shin et al., 2013). Thus, one bout of meditative walking may meaningfully reduce anxiety. Second, in people with COPD, the MCIDs for the Hospital Anxiety and Depression Scale (HADS) subscales HADS-Anxiety and HADS-Depression have both been established as a ~1.50 (~20%) decrease from baseline (Puhan et al., 2008). Another study reported that mindful walking in people with COPD decreased the mean scores on both subscales from baseline by more than 1.50 points and 20% (Lin et al., 2019). These decreases in anxiety and depression are meaningful. Third, in people with chronic musculoskeletal pain and comorbid anxiety and/or depression, the MCID for the Short Form-36 (SF-36) Mental Health subscale has been established as ~9 points (Kroenke et al., 2019). One study reported that mindful walking in adults with psychological distress increased scores on the SF-36 Mental Health subscale from baseline to Week 4 by 9.1 points and to Week 12 by 7.5 points (Teut et al., 2013). Adults with increased psychological distress do not match the population for which the MCID was established. However, the improved SF-36 Mental Health scores may be meaningful. The meaningfulness of the other measures of mental health is not clear.

Overall, the evidence of meditative and mindful walking improving mental health is promising but limited by a small quantity of studies that have a high risk of bias. The evidence of other mindful exercises is limited by the same problem. A 2021 meta-analysis evaluated the effects of a single session of mindful exercise (qigong, tai chi, or yoga) on anxiety (Yin et al., 2021). The standardized mean effect size for yoga decreasing anxiety was small at 0.32 (95% CI [0.16, 0.48], $p = 0.0002$), and no conclusions could be drawn about qigong or tai chi because of the limited number of studies and their high risk of bias (Yin et al., 2021). The efficacy of yoga reducing anxiety more than non-mindful exercises was supported by a separate 2020 meta-analysis that reported a standardized mean difference of −0.45; 95% CI [−0.81, −0.09], $p = 0.01$ (So et al., 2020). However, when qigong and yoga studies were pooled and compared against non-mindful exercises, anxiety did not differ ($p = 0.18$) (So et al., 2020). In 2018, another meta-analysis of seven studies examined the effects of mindful exercises (qigong, tai chi, and yoga) and purely physical exercise in people with schizophrenia (Li et al., 2018). Based on just two studies deemed to be low-quality evidence, mindful exercises improved psychiatric symptoms more than purely physical exercise (mean difference on the Positive and Negative Syndrome Scale: −8.94; 95% CI [−14.53, −3.35], $p < 0.05$) (Li et al., 2018). Separately, a 2008 systematic review without a meta-analysis of 12 studies examined the effects of mindful exercises ($n = 6$ studies) and non-mindful exercises ($n = 6$ studies) on depression scores in adults (Tsang et al., 2008). The mindful exercises were qigong, tai chi, and yoga, and the non-mindful exercises were aerobic exercise, walking, pram walking, jogging, and other unspecified exercises. The review's qualitative synthesis showed that five of the six studies of mindful exercises and all six studies of non-mindful exercise improved depression scores, suggesting both mindful and non-mindful exercises are efficacious (Tsang et al., 2008). Comparative efficacy could not be determined quantitively by a meta-analysis because of the included studies' heterogeneous populations, interventions, and measures and methodological limitations. Fourteen years after this 2008 systematic review, the same issue hinders the present 2022 systematic review and precludes a meta-analysis about the effects of meditative and mindful walking.

The literature about the effects of meditative and mindful walking on mental health is also in a similar state as the literature about the effects of traditional walking on mental health. A 2018 scoping review evaluated studies and systematic reviews about the effects of walking on anxiety, depression, self-esteem, psychological stress, and other mental health outcomes (Kelly et al., 2018). The review concluded that the evidence of walking improving mental health is developing but not definitive. The largest evidence base is for walking consistently improving depression scores. The evidence base for walking improving anxiety is less conclusive, but studies have shown negative associations between walking and anxiety scores and acute walking interventions to decrease anxiety (Kelly et al., 2018). Similar improvements in depression and anxiety occurred after the meditative and mindful walking interventions described in the present systematic review. However, firmer conclusions about the improvements will require a meta-analysis once more studies are conducted with similar populations, interventions, and measures of depression and anxiety.

Unlike the measures of mental health, the measures of cardiovascular health are relatively easier to interpret for meaningfulness. In one study in the present review, meditative walking decreased SBP from the Hypertension Stage 2 category (≥ 140 mmHg) to the Hypertension Stage 1 category (130–139 mmHg) (Gainey et al., 2016; Whelton et al., 2018). Meditative walking also decreased DBP from the Hypertension Stage 1 category (80–89 mmHg) to the Elevated category (< 80 mmHg) (Gainey et al., 2016; Whelton et al., 2018). In another study, meditative walking decreased SBP from the Elevated category (120–129 mmHg) to the Normal category (< 120 mmHg) (Prakhinkit et al., 2014; Whelton et al., 2018). These decreases may meaningfully reduce the risk of cardiovascular diseases if sustained long-term (Whelton et al., 2018). The potentially hypotensive effects of meditative and mindful walking over traditional walking deserve more attention.

The ability of meditative and mindful walking to increase the six-minute walk distance also deserves more attention. The MCID for the six-minute walk distance has been established as 14.0–30.5 meters across diverse populations (Bohannon & Crouch, 2017), 25 meters in people with COPD (Holland et al., 2010), 22–42 meters in adults with lung cancer (Granger et al., 2015), and 17.8 meters in older Asian adults with frailty and a fear of falling (Kwok et al., 2013). In one study in the present review, usual care supplemented with mindful walking increased the distance from 388 ± 114 meters to 418 ± 123 meters (a 30-meter improvement) among Taiwanese adults with COPD (Lin & Yeh, 2021). In another study, meditative walking increased the distance in older Thai women with depressive symptoms from 164 ± 13 meters to 302 ± 18 meters (a 138-meter difference) (Prakhinkit et al., 2014). These increases exceed the MCID for people with COPD and older Asian adults. But walking, not mindfulness, likely caused the increase in distance because mindful walking did not increase the distance more than traditional walking (Prakhinkit et al., 2014). However, this conclusion is based on one study, so more studies are needed to determine if meditative or mindful walking improve the distance more than traditional walking.

Regarding the other measures of cardiovascular health, too few studies in the present review showed that meditative or mindful walking improve aerobic capacity, percent body fat, FMD, PWV, glycemia, lipemia, or inflammation at all or more than traditional walking. Thus, definitive conclusions cannot be drawn, and more studies are needed. Researchers should especially address the lack of studies that evaluate the cardiovascular effects of one session of meditative and mindful walking.

Limitations of the Included Studies

In summary, the significant improvements in mental and cardiovascular health because of meditative and mindful walking seem congruent with the literature about other mindful exercises. However, the improvements reported by the studies included in the present review should be interpreted cautiously. The primary reason for interpreting the improvements cautiously is that the included studies rarely implemented meditative or mindful walking in an intervention without other physical exercises, seated meditation, or discussions about the intervention between the participants and researchers. These extraneous parts the intervention potentially confounded the relationship between meditative and mindful walking and mental and cardiovascular health. It cannot be stated definitively whether the meditative or mindful walking part of the intervention caused the improvement in mental or cardiovascular health.

The secondary reason for interpreting improvements cautiously is that the included studies had a moderate-to-high risk of bias. The studies were often limited by not having any control groups or at least a non-walking control group and not blinding the participants or researchers to group allocation. These limitations may have introduced selection bias, response bias, and confirmation bias. First, selection bias may have been present if some participants' measured or unmeasured baseline characteristics predisposed their mental or cardiovascular health to improve regardless of the intervention. People who participate in studies of meditation or mindfulness may already view these practices favorably. These favorable views may lead participants to report positive perceptions and experience elevated physiological responses. Without a control group and random allocation, it is unclear if mental or cardiovascular health improved because of the intervention, confounders, or random chance. Second, response bias may have biased the mental health data. Participants may have reported improved mental health on the questionnaires to please the researchers or meet their expectations that meditative and mindful walking would help. Third, because some of the studies did not blind the researchers to participants' groups, confirmation bias may have altered researchers' behavior toward participants in the meditative or mindful walking groups. Though unintentionally and subtly, the researchers may have primed participants receiving the intervention to feel calmer or happier and try harder on the measures of cardiovascular health. Thus, confirmation bias could have distorted the data for outcomes such as affect, anxiety, depression, enjoyment, blood pressure, heart rate, six-minute walk distance, and maximal oxygen consumption. Collectively, these biases preclude drawing valid inferences about the effects of meditative and mindful walking in the studies' target populations.

Another factor that precludes drawing valid inferences from the included studies relates to their statistical analyses. Most of the studies did not conduct an intent-to-treat analysis to account for the potential bias introduced by participants' not complying with the protocol (i.e., missing sessions) or withdrawing from the study. Analyzing only the data from the participants who completed the study may have produced biased estimates of the true effect of meditative or mindful walking (e.g., the beta coefficients and mean differences between pre- and post-intervention timepoints).

Besides interpreting the significant improvements cautiously, readers should consider whether the improvements are meaningful. Only three studies reported effect sizes to describe the magnitude of significant improvements as standardized pre-post or between-group differences (Ameli et al., 2021; Cox et al., 2018; C.-H. Yang & Conroy, 2019). The effect sizes described the magnitude of improvements in positive affect, negative affect, attentional focus, distress, enjoyment of exercise, and state mindfulness (Ameli et al., 2021; Cox et al., 2018; C.-H. Yang & Conroy, 2019). A fourth study reported effect sizes for some non-significant outcomes but not for the significant improvements in positive affect, negative affect, or trait mindfulness (Gotink et al., 2016). The remaining studies in the present review that reported significant improvements did not report effect sizes (Bigliassi et al., 2020; Gainey et al., 2016; Lin et al., 2019; Lin & Yeh, 2021; Prakhinkit et al., 2014; Shi et al., 2019; Shin et al., 2013; Shors et al., 2018; Teut et al., 2013). Some of these studies reported beta coefficients that suggest the magnitude of the effect of the intervention on the outcome. However, beta coefficients are statistical estimates and not inherent indicators of meaningfulness. The meaningfulness of improvements is determined by whether they are clinically relevant or important to the participants.

Notably, MCIDs in the sampled populations have not been established for many of the measures of mental health used in the studies. These MCIDs are needed to ascertain the meaningfulness of the improvements. Therefore, MCIDs in populations that are targeted for studies of meditative and mindful walking should be established. To the authors' knowledge, relevant MCIDs do not exist for measures such as the Feeling Scale, Felt Arousal Scale, Geriatric Depression Scale, Perceived Stress Scale, Physical Activity Enjoyment Scale, or Rosenberg Self-Esteem Scale. It is also unclear if improved scores on the Five Facet

Mindfulness Questionnaire, State Mindfulness Scale, or State Mindfulness Scale for Physical Activity are clinically relevant or important to the participants. The benefits of mindful walking will be clearer once MCIDs for more measures of mental health are available.

Limitations of the Present Review

Alongside the limitations of the included studies, readers should consider the limitations of the present systematic review. First, the search terms and search combinations of this review were not evaluated by a science librarian. Science librarians are important resources when searching for, organizing, and analyzing articles during a systematic review (Harris, 2005). The current systematic review did not seem to be affected by the lack of a science librarian because no new articles were included from the references of the 14 studies we included initially. Second, this review did not include a meta-analysis. However, doing so would have been inappropriate because the included studies did not meet the four aspects of homogeneity required to conduct a meta-analysis (Boland et al., 2017). Conducting a valid meta-analysis requires the included studies to meet four aspects of homogeneity: 1) similar participants, 2) the same interventions and comparators, and 3) the same outcomes recorded over the same time frame. Also, 4) most of the included studies must report similar treatment effects that are in the same direction (mostly positive or mostly negative effects) with overlapping confidence intervals (Boland et al., 2017). The included studies of meditative and mindful walking were clinically and methodologically heterogeneous. Across studies, participants differed by age, biological sex, race, ethnicity, nationality, height, weight, BMI, and health and disease status. The interventions also differed by the type of meditative and mindful walking and the frequency, intensity, and duration of walking. Few of the included studies evaluated the same measures of mental and cardiovascular health, so the studies did not report the same treatment effects in the same direction. Because the included studies did not meet the four aspects of homogeneity required for a meta-analysis, conducting a valid meta-analysis was not possible.

The third and fourth limitations of the present review are language bias and publication bias. The review only included published journal articles written in English. Articles written in English are more likely to report positive study findings than articles not written in English (Boland et al., 2017). The articles included in the present review were not used in formal tests of publication bias. Such tests would not be useful because the articles described studies with different interventions and outcomes. These differences mean the articles do not describe a discrete meditative or mindful walking intervention for which there may be publication bias toward positive findings for a particular outcome. If in the future a meta-analysis analyzes the effects of a discrete meditative or mindful walking intervention on a particular outcome, formal tests of publication bias would be warranted.

The fifth limitation of the present review is that studies of labyrinth walking were omitted. Labyrinth walking is a unique form of meditative and mindful walking along a winding path to facilitate calm, insight, mindfulness, and occasionally spirituality. Labyrinth walking is a niche within a niche that should be considered separately (i.e., labyrinth walking is a subsection of the meditative and mindful walking literature within the broader walking literature). We recommend that interested readers view a relevant literature review (Davis, 2021) published recently by the first author of the present review.

Recommendations for Future Research

The first recommendation is that future research about meditative and mindful walking should minimize bias introduced because of the study design and statistical analysis. To minimize selection bias, studies should randomly allocate participants to at least one control group and the intervention group. To minimize confirmation bias, the researchers who collect and analyze the outcome data should be blinded to participants' groups. Preferably, future studies will have a non-walking control group, traditional walking group, and meditative or mindful walking group. This design would allow the studies to show whether meditative or mindful walking interventions improve mental and cardiovascular health more than no intervention or traditional walking. In studies with a parallel design, participants in the meditative or mindful walking group should not know about the other groups so that they do not report improvements on subjective measures merely because they think the intervention should help (i.e., response bias). In studies with a crossover design, participants should complete the traditional walking arm before the meditative or mindful walking arm. Not randomizing the arms may introduce bias because of confounders associated with life circumstances during the arms (e.g., disease severity, family dynamic, and work stress). However, requiring the traditional walking first would reduce the risk of participants applying mindfulness techniques during traditional walking (i.e., carryover effect). Once the planned number of participants have completed all the arms in either a parallel or crossover study, the data should be analyzed for all participants who were randomized by using an intent-to-treat analysis in addition to the per-protocol analysis. This analysis will reduce bias in the estimates of the treatment effects because of participants' non-compliance, swapping treatment arms, or withdrawing from the study. Alongside statistical point-estimates with *p*-values to show significance, researchers should report measures of precision (e.g., 95% CIs) and effect size (e.g., Cohen's *d* or η_p^2). The meaningfulness of these effect sizes should be considered in relation to participants' function, morbidity, and mortality. Using measures of mental health with population-specific MCIDs should be prioritized. For measures without population-specific MCIDs, new MCIDs should be established.

The second recommendation is also intended to ascertain the significance and meaningfulness of meditative and mindful walking by itself. Fulfilling this purpose requires an intervention of only meditative or mindful walking instead of combining it with extraneous physical and mental exercises such as stretching, calisthenics, seated meditation, and group discussion. These exercises may confound the relationship between meditative and mindful walking and mental and cardiovascular health. When implementing only meditative or mindful walking, it would be interesting to determine whether the effects depend on conducting the practice indoors or outdoors and independently or in a group. These characteristics of the intervention should be reported explicitly in future studies.

In summary, the purpose of the present systematic review was to determine the effects of meditative and mindful walking on mental and cardiovascular health. Among the 14 included studies, the target populations, interventions, and outcome measures varied considerably. Most of the studies (13 of 14) showed that their interventions statistically significantly improved scores on at least one measure. However, readers should interpret these improvements cautiously because of the methodological limitations of the studies and the unclear meaningfulness of the improvements. The key takeaway is that meditative and mindful walking is a promising type of mindful exercise for improving mental and cardiovascular health. Determining the value of this type of exercise will require methodologically rigorous randomized controlled trials that report detailed explanations of their interventions and key prognostic measures of participants' function, morbidity, and mortality. Improvements on these measures should be evaluated for meaningfulness by considering the change in scores relative to population-specific MCIDs.

Relevant MCIDs should be established where they do not exist. Significant improvements alone are not sufficient evidence to conclude that meditative and mindful walking meaningfully improve mental and cardiovascular health.

The Takeaway and Link Between Studies 1 and 2 (Chapters 2 and 3)

The systematic review summarized what is currently known about the effects of meditative and mindful walking. The available evidence indicates that both modalities have promising effects that could improve markers of mental and cardiovascular health. Still, the evidence does not indicate that meditative or mindful walking are effective at treating cardiovascular or mental diseases. Answering this question will require large randomized controlled trials that test precise meditative or mindful walking interventions in a random sample from a well-defined population. Additionally, readers are advised not to try extrapolating the findings of the systematic review to exercise modalities beyond meditative and mindful walking. Both activities are mindful exercises that involve a person deliberately focusing on their thoughts, feelings, emotions, and bodily sensations. Given this deliberate focus, it is not surprising that participants often reported significantly more state mindfulness after the exercise than before it. Readers are also cautioned not to exaggerate the early evidence that meditative and mindful walking in green space benefit people more than indoor versions. Meditative and mindful walking in green space are not panaceas but are instead lush fields open for systematic scholarly study. It is unknown whether the fields abound with ripe fruit—evidence of efficacy and effectiveness.

Contemplating the results of the systematic review to find a deeper meaning required sinking below their surface value. While submerged, the idea of meditative and mindful green walking floated nearby. Two linked questions recurrently came to mind: 1) Does passing quiet solitary time in nature increase state mindfulness independent of deliberately focusing on the thoughts, feelings, emotions, and bodily sensations? 2) Can state mindfulness be measured more efficiently than is allowed by one of the prevailing measures of the construct, the State Mindfulness Scale (SMS)?

At the time he formulated question one, the author was unaware of any acute studies that had tested whether state mindfulness responds to non-mindful interventions in green space, such as seated rest and walking. Answering this question would reveal whether merely being in the presence of nature, such as green space, guides a person's attention inward toward their immediate experience. Such a discovery would offer empirical evidence for the spiritual claim that immersing oneself in nature centers, grounds, and raises the awareness of a person. The discovery would also suggest that, by being in green space, people can connect with themselves without needing to learn a formal meditation or mindfulness practice.

A pillar of sound empirical evidence is valid and reliable instruments. Empirical evidence that is neither accurate nor repeatable is amorphous, unhelpful, and incongruent with evidence-based practice. Understanding this idea prompted question two, which was concerned with the concurrent validity and test-retest reliability of novel measures of state mindfulness. One of these measures is an adaptation of the SMS, called the State Mindfulness Scale for Physical Activity (SMS-PA). The SMS-PA has nine fewer questions than the SMS and is used to measure state mindfulness overall, of the mind, and of the body after an acute bout of physical activity. Whether the SMS-PA is valid and test-retest reliable still needs to be demonstrated. The shortness of the SMS-PA makes it enticing for field research, but its use is limited to the post-exercise period. While formulating question two, the author noticed that no comparably short measure existed for measuring state mindfulness irrespective of exercise. This gap inspired the author to create a new scale, the Visual Analog Scale-Mindfulness (VAS-M), and report the first data about its concurrent validity and test-retest reliability.

The two questions formulated while contemplating the findings of the systematic review are the topic of the Study 2. The study aimed to determine whether sitting and walking in green space without any guidance in meditation or mindfulness affected state mindfulness and whether there was evidence for the concurrent validity and test-retest reliability of the VAS-M and SMS-PA.

CHAPTER 3: DETERMINING THE EFFECTS OF SITTING AND WALKING IN GREEN SPACE ON STATE MINDFULNESS AND TESTING THE CONCURRENT VALIDITY AND TEST-RETEST RELIABILITY OF NOVEL MEASURES OF THE CONSTRUCT

3.1 Abstract

INTRODUCTION: It is unclear whether immersion in green space affects state mindfulness. This study's primary aim was to determine whether non-mindful sitting or traditional walking in green space affect state mindfulness. The secondary aim was to test the concurrent validity of the Visual Analog Scale-Mindfulness (VAS-M) and State Mindfulness Scale for Physical Activity (SMS-PA) against the State Mindfulness Scale (SMS). The tertiary aim was to assess the test-retest reliability of the scales across approximately 24 hours. **METHODS:** On two days separated by approximately 24 hours, participants (22 F, 20 M, 26 ± 9 y, 170 ± 9 cm, 70 ± 16 kg, 24 ± 5 kg·m^{-2}) arrived in green space, consented to participate, and completed the scales. Participants then sat for 10 minutes, completed the scales again, walked for 10 minutes, and completed the scales once more. Two 1 × 3 repeated-measures ANOVAs with post hoc paired-samples *t* tests were conducted to evaluate SMS and VAS-M scores among pre-sit, post-sit, and post-walk. Pearson's (*r*) and Spearman's (*ρ*) correlations were conducted to compare the VAS-M and SMS-PA scores with the SMS scores. Coefficients of variation and intraclass correlation coefficients were calculated to assess the 24-hour test-retest reliability of the SMS, VAS-M, and SMS-PA. The α-level for all analyses was 0.05. The effect size Cohen's *d* was calculated for all the paired-samples *t* tests and interpreted as follows: 0.41 = practically significant, 1.15 = moderate, and 2.70 = strong. **RESULTS:** The SMS scores increased from pre-sit to post-sit ($p < 0.001$, Cohen's $d = 1.68$, moderate) and from pre-sit to post-walk ($p < 0.001$, $d = 1.89$, moderate) but not from post-sit to post-walk ($p = 0.232$, $d = 0.31$, not practically significant). The VAS-M scores increased from pre-sit to post-sit ($p < 0.001$, $d = 0.88$, practically significant), from post-sit to post-walk ($p = 0.006$, $d = 0.38$, not practically significant), and from pre-sit to post-walk ($p < 0.001$, $d = 1.22$, moderate). There were significant, weak-to-moderate, positive correlations between the VAS-M and SMS scores at each timepoint (r or $\rho = 0.34$–0.58, $p < 0.05$). The 95% confidence intervals (CIs) were wide. There was a significant, very strong, positive correlation between the SMS-PA and SMS scores at post-walk ($\rho = 0.89$, $p < 0.001$). Neither the SMS nor the VAS-M met the reliability criteria at pre-sit or post-sit (CV < 10%, significant ICC > 0.70, and lower-bound of 95% CI of ICC > 0.70). The VAS-M and SMS-PA, but not the SMS, met the criteria at post-walk (VAS-M: CV = 7.8%, ICC = 0.95; SMS-PA: CV = 5.5%, ICC = 0.90). **CONCLUSIONS:** Sitting in green space for 10 minutes cultivated a present-moment and nonjudgmental awareness that was not increased by 10 minutes of walking. There was evidence to support the concurrent validity of the VAS-M and SMS-PA with the SMS, but the questionable test-retest reliability of the SMS and VAS-M warrant using all three scales cautiously. New studies in diverse samples and settings are needed to draw definitive conclusions about the validity and test-retest reliability of the SMS, VAS-M, and SMS-PA when used in green space.

3.2 Introduction

In Chapter 2, my recently published systematic review (Davis et al., 2022), the effects of meditative and mindful walking on mindfulness were reported under the category of mental health outcomes for two reasons. First, state mindfulness fit better there than under the category of physical health outcomes. Second, increasing mindfulness improves mood, psychological well-being, and symptoms of mental health conditions such as anxiety and depression (Brown &

Ryan, 2003; Galante et al., 2021; Hofmann & Gómez, 2017). Some researchers who study mindfulness and mental health have explored how the relationship between those two variables is influenced by a third variable, nature (Djernis et al., 2019; Howell et al., 2011; Nisbet et al., 2019; Schutte & Malouff, 2018; Van Gordon et al., 2018; Wolsko & Lindberg, 2013). And, as Chapter 2 illustrated, some researchers have specifically explored the effects of mindfulness-based green exercise interventions (MBGEIs) on mental health outcomes (Ameli et al., 2021; Shin et al., 2013). More recently, in a study published after the systematic review ended, Ma et al. (2022) studied outdoor mindful walking in green space and an urban area. The study's primary aim was to determine the effects of outdoor mindful walking on college students' subjective sleep quality, total mood disturbance, state and trait mindfulness, and connectedness to nature. The secondary aim was to determine if the effects differed by setting. Across the two settings, outdoor mindful walking increased participants' sleep quality, mood, and trait mindfulness similarly (Ma et al., 2022). This study and the studies in the systematic review show there is an interest in determining how MBGEIs affect mental health outcomes.

The few MBGEI studies published thus far have been valuable but limited by their respective designs. Only Shin et al. (2013) included a non-mindful (traditional) walking group. In studies lacking a traditional walking control group, it is difficult to determine whether the observed effects on mental health are caused by the mindfulness practice, traditional walking, or a synergy between these intervention components. Of particular interest is a question not yet answered in the literature. To what extent, if any, are the observed increases in state mindfulness after MBGEIs caused by exercising in green space? In other words, does green exercise (GE) increase state mindfulness independent of a mindfulness intervention? Answering this question would add value to the literature by clarifying the two-way relationship between state mindfulness and GE. Clarifying this relationship will inform future research on the three-way relationship between state mindfulness, mindfulness practices, and GE (and their convergence on MBGEIs).

With this opportunity in mind, the present study was built with the primary aim of determining whether inactive immersion (i.e., sitting undisturbed at a trailhead) or traditional walking in green space changed people's state mindfulness. State mindfulness was the primary outcome and trait mindfulness was not measured for two reasons. The first reason is conceptual. While trait mindfulness is measured and reported in the literature, traditional Buddhist scholarship and modern cognitive-behavioral research view mindfulness less as dispositional and stable and more as behavior-like, context-specific, and dynamic (Anālayo, 2004; Bishop et al., 2004; Bodhi, 2006; Langer, 1989; Shulman, 2010; Tanay & Bernstein, 2013; Thera, 1972; *Thus Have I Heard*, 1987). The second reason is theoretical. An acute intervention of sitting and walking in green space would not be expected to change trait mindfulness. Consequently, state mindfulness was measured via the State Mindfulness Scale (SMS). The SMS was chosen over other measures of state mindfulness because their scope and content make them unsuitable for MBGEIs. The State Mindful Attention Awareness Scale (State-MAAS) (Brown & Ryan, 2003) measures mindful attention to and awareness of daily activities. The Toronto Mindfulness Scale (TMS) (Lau et al., 2006) measures only curiosity and decentering; curiosity is only one aspect of mindfulness, and decentering is generally considered a separate construct and outcome of mindfulness. Neither the State-MAAS nor the TMS measure mindfulness of bodily sensations, an important outcome of MBGEIs. The SMS is superior for MBGEIs because the measure covers multiple aspects of mindfulness and objects to which mindful awareness and attention may be directed (i.e., bodily sensations and mental events).

Interest in people's mindfulness of bodily sensations and mental events after physical activity and exercise led researchers to develop the State Mindfulness Scale for Physical Activity (SMS-PA) (Cox et al., 2016). The SMS-PA is shorter than the SMS, possibly making data loss less likely in field research settings such as green space. As discussed in Chapter 2, the author of the present dissertation noticed that no comparably short measure existed for measuring state mindfulness irrespective of exercise. The author responded by developing the Visual Analog Scale-Mindfulness (VAS-M) as a novel, quick measure of state mindfulness to be used in or out of exercise settings. The VAS-M is intended for use anywhere, including field research settings, such as before, during, and after MBGEIs. Given the uncertainty about the validity of the SMS-PA and VAS-M, the present study's secondary aim was to test the concurrent validity of both scales against the SMS. Obtaining valid data from the SMS-PA and VAS-M requires not just that they measure the intended construct (state mindfulness) but that they do so consistently over time. This latter characteristic is called test-retest reliability. Test-retest reliability is integral to any scale used in MBGEI research because the interventions often have repeated-measures designs. If the SMS-PA and VAS-M are unreliable, the data they provide will neither be valid nor useful. Therefore, the tertiary aim was to assess the test-retest reliability of the measures of state mindfulness. Achieving the second and third aims would add value to the literature by clarifying the concurrent validity and test-retest reliability of measures of state mindfulness before, during, and after MBGEIs. The present study and its three aims were approved by the author's doctoral advisory committee. The following protocol was developed by the author and his committee chair.

3.3 Methods

This study was completed as per the following methods.

3.3.1 Participants

Convenience sampling was used for two separate periods of data collection. Each period lasted two days and occurred at a different site. At both sites, participants were recruited via word of mouth and emails from kinesiology faculty to their students. Across both sites, 42 participants were enrolled and completed at least one of the two days. Two participants dropped out, one at each site after the first day. Consequently, the attrition rates at the Thunderbird Gardens Trailhead (TGT; site one) and the Clark County Wetlands Park (WP: site two) were 5% (1/19) and 4% (1/23), respectively. Demographic and anthropometric characteristics are provided for the overall sample (Tables 9–10) and the TGT and WP subsamples (Appendix H, Tables 20–23).

Table 9. Self-reported biological sex and race among the overall sample (N = 42).

Classification	Count (n)	Percent (%)
Total	42	100
Females	22	52
Males	20	48
Intersex	0	0
African American or Black	4	10
Asian	4	10
Caucasian or White	19	45
Hispanic or Latino	9	21
Mediterranean	1	2
Middle Eastern	1	2
Multi-Racial	3	7
Polynesian	1	2

The percentages for the self-reported races do not sum to precisely 100% because of rounding.

Table 10. Age, height, mass, and body mass index of the overall sample (N = 42).

Age (y)	Height (cm)	Mass (kg)	BMI ($kg{\cdot}m^{-2}$)
26.0 ± 9.0	169.7 ± 8.7	69.6 ± 15.9	23.9 ± 4.5

Y: years; cm: centimeters; kg: kilograms; BMI: body mass index; m: meters. Arithmetic mean ± standard deviation.

3.3.2 Procedures

The first period of data collection occurred between approximately 0800 and 1700 on October 1–2, 2021, at the TGT in Cedar City, UT. The first period received parallel approval by the respective Institutional Review Boards (IRBs) of the University of Nevada, Las Vegas (UNLV-2021-29), and Southern Utah University (SUU #08-142021). The second period occurred between approximately 0800 and 1700 on November 6–7, 2021, at the WP in Las Vegas, NV. This second period was approved by UNLV's IRB (UNLV-2021-39). The Global Positioning System (GPS) coordinates in decimal degrees (latitude, longitude) of the TGT and WP are 37.690442600284165°, −113.04359862955735°, and 36.10117842658103°, −115.02306610191594°, respectively. The TGT connects to the Thunderbird Canyon Trail system to the east, which is nestled on the north side of Salt Creek Canyon. The trailhead and trail system are replete with vegetation, including trees and shrubs with grayish trunks and foliage of various shades of green, including junipers and pinyon pines (Figure 9). The soil and rocks are various shades and combinations of beige, brown, orange, and red (e.g., based on HEX codes and their associated names: atomic tangerine, copper, light salmon, sandy brown, and Sienna). The WP is a large nature preserve that spans 0.85 km^2 (210 acres). In the WP, the base of operations for data collection was an area with grass, Freemont cottonwood trees, large boulders, and a view of Frenchman Mountain (Figure 10). The nearby paths were surrounded by trees and low-lying shrubs (e.g., alkali sacaton, catclaw acacia, four-wing saltbush, quail bush, salt grass, and screwbean mesquite). The paths had various shades and combinations of brown, gray, green, orange, and red (e.g., based on HEX codes and their associated names: burning sand, cool gray, green Waterloo, light taupe, Navajo white, and scrub).

At the time of data collection, the altitude of the TGT was 1,717 meters (5,632 feet). The temperature, relative humidity, and wind speed were taken once in the morning between 0900 and 1100 and once in the afternoon between 1200 and 1400 on each day of the study (Table 11).

Figure 9. Photos of the Thunderbird Gardens Trailhead and trail system.

Figure 10. Photos of the Clark County Wetlands Park.

Table 11. Temperature, relative humidity, and wind speed at the TGT.

Variable	Temperature		Relative Humidity		Wind Speed	
Time	AM	PM	AM	PM	AM	PM
Day 1	10.6 °C (51.1 °F)	20.9 °C (69.6 °F)	37.4%	19.8%	0.7 $m \cdot s^{-1}$ (1.5 mph)	0.6 $m \cdot s^{-1}$ (1.4 mph)
Day 2	14.6 °C (58.2 °F)	22.1 °C (71.7 °F)	30.2%	17.0%	0.6 $m \cdot s^{-1}$ (1.3 mph)	0.9 $m \cdot s^{-1}$ (2.1 mph)

TGT: Thunderbird Gardens Trailhead; AM: morning; PM; afternoon; °C: degrees Celsius; °F: degrees Fahrenheit; $m \cdot s^{-1}$: meters per second; mph: miles per hour.

The altitude at the base of operations in the WP was 488 meters (1,600 feet). The temperature, relative humidity, and wind speed were taken once in the afternoon (1200–1300) on each day of the study (Table 12).

Table 12. Temperature, relative humidity, and wind speed at the WP.

Variable	Temperature	Relative Humidity	Wind Speed
Day 1	26.6 °C (79.8 °F)	22.5%	1.34 $m \cdot s^{-1}$ (3.0 mph)
Day 2	29.6 °C (85.2 °F)	15.3%	1.30 $m \cdot s^{-1}$ (2.9 mph)

WP: Clark County Wetlands Park; °C: degrees Celsius; °F: degrees Fahrenheit; $m \cdot s^{-1}$: meters per second; mph: miles per hour.

Both periods of data collection began with people interested in the study arriving at the trailhead or park and reading an IRB-approved informed consent form. Only the people who gave verbal and written consent to participate became prospective participants who were screened for eligibility. The steps of the eligibility screening were 1) assigning the prospective participant a participant number; 2) recording their reported biological sex (female, male, or intersex), race, age, and body height; 3) measuring their body mass and body mass index (BMI); and 4) completing a health screening. Body mass was measured clothed via the Tanita TBF-521 Bodyfat Monitor/Scale (Tanita, Tokyo, Japan). Before stepping on the scale, prospective participants removed their shoes and emptied their pockets. For the health screening, prospective participants received help completing the American College of Sports Medicine (ACSM) Preparticipation Health Screening Questionnaire for Exercise Professionals. This screening tool checked whether people had signs and symptoms of cardiovascular, renal, or metabolic diseases that would have increased their risk of adverse reactions to exercise. The outcome of the health screening was one of three mutually exclusive recommendations: 1) medical clearance was not necessary, 2) medical clearance was not necessary before light or moderate exercise but was recommended before vigorous exercise, or 3) medical clearance was recommended before any exercise.

Each prospective participant's characteristics and health screening were compared to the inclusion and exclusion criteria to determine whether the person was eligible to participate. The main inclusion criteria were having an age of 18 to 64 years and any biological sex, race, height, mass, or BMI. The exclusion criteria were having an age under 18 years or over 64 years or the third outcome of the health screening (medical clearance recommended before any exercise). People who had the second outcome of the health screening (medical clearance not necessary before light or moderate exercise) were eligible because the study intervention was not expected to be vigorous at either study site. The other exclusion criteria were reporting to be pregnant or lactating; reporting to have a physical limitation that impaired walking or made it dangerous or painful; and reporting to have any cognitive or intellectual disabilities. People who were imprisoned or on parole or probation were neither recruited nor eligible to participate.

Prospective participants who passed the eligibility screening were enrolled in the study. Immediately upon enrollment, each participant was given a one-inch, three-ring binder. The binder contained, in the following order, the participant data sheet, the completed health screening questionnaire, the completed informed consent form, and several series of four or five measurement questionnaires. The series of four measurement questionnaires can be considered Series A, and the series of five measurement questionnaires can be considered Series B (the series were not called A and B during the study, but this naming helps conceptualize the binder page order).

Series A contained four measurement questionnaires: the SMS, VAS-M, Love and Care for Nature Scale (LCN), and Visual Analog Scale-Nature (VAS-N). Series B contained the same four measurement questionnaires, plus the SMS-PA. Each of the five measurement questionnaires fit on the front side of one sheet of 8.5"-by-11" copy paper. One Series A followed the last page of the informed consent form. This first Series A was followed by a sheet of color paper and another Series A. Another sheet of color paper separated this second Series A from a Series B. This Series B marked the end of day one in the binder. Then, the day-one pattern repeated for day two. Thus, the order of the binder after the informed consent form was Series A, color paper, Series A, color paper, Series B, color paper, Series A, color paper, Series A, color paper, Series B. Two researchers assembled the series and binders the week before each data collection. Each researcher held the other accountable to ensure the order of the measurement questionnaires was not systematic or biased intentionally. To minimize the risk of unintentional bias, the researchers conducted a randomization procedure for every series in each binder (i.e., shuffled the measurement questionnaires in every series).

Though the LCN and VAS-N data are not reported in the present study, the measurement questionnaires were part of the protocol and were thus included in the methods. Study 3 presents the analysis of the LCN and VAS-N data.

The SMS is a Likert-type scale that has 21 items: 15 items for state mindfulness of the mind and six items for state mindfulness of the body (Ruimi et al., 2022; Tanay & Bernstein, 2013) (Appendix A). Each item is a declaratory statement (e.g., Item 1: I was aware of different emotions that arose in me). Respondents read each statement and report how much they agree or disagree by marking a checkmark or X under 1 (*not at all*), 2, 3, 4, or 5 (*very well*). On the SMS version used, choices 2–4 did not have text descriptions as did 1 and 5. All 21 items are summed for a maximum total score of 105 arbitrary units (AU; 21 items × 5 = 105). A high total score indicates a more focused level of state mindfulness than does a lower total score. The first data to support the SMS's validity were published in 2013 (Tanay & Bernstein, 2013). First, the SMS showed convergent validity because total and subscale SMS scores correlated significantly, moderately, and positively with total and subscale scores (Curiosity, Decentering) on the TMS (r = 0.31–0.43, $p < 0.01$). Total and subscale SMS scores also correlated significantly, moderately, and positively with Five Facet Mindfulness Questionnaire Observing subscale scores (r = 0.39–0.47, $p < 0.01$). Second, the SMS showed discriminant validity via non-significant correlations between total and subscale SMS scores and Mindful Attention Awareness Scale scores (MAAS; r = 0.00–0.07), which is different from the State-MAAS and measures dispositional mindful awareness of and attention to daily activities. Third, the SMS showed incremental convergent validity. When predicting Five Facet Mindfulness Questionnaire Observing subscale scores, total SMS scores explained unique variance above that explained by total TMS scores ($\beta = 0.41$, $p < 0.01$) but not vice versa ($\beta = 0.08$, $p > 0.05$) (Tanay & Bernstein, 2013). The same publication that presented these validity data reported that the SMS demonstrated internal consistency (Cronbach's α = 0.85–0.97), temporal stability, construct validity, incremental sensitivity to change, and incremental predictive validity (Tanay &

Bernstein, 2013). To test the SMS' temporal stability, baseline scores were compared with scores taken one and six weeks later in a control group and mindfulness meditation group (Tanay & Bernstein, 2013). In the control group, baseline SMS scores were correlated with the scores at one and six weeks of follow-up ($r = 0.65$ [$p < 0.01$] and $r = 0.68$ [$p < 0.01$], respectively). The same was true for the meditation group ($r = 0.64$ [$p < 0.01$] and $r = 0.63$ [$p < 0.01$], respectively). Separately, Andrade et al. (2019) had participants complete the SMS before and after a two-week mindfulness program that included three 90-minute sessions. The correlation coefficients between the pre- and post-program SMS scores were $r = 0.40$ ($p < .05$) for the total score, $r = 0.42$ ($p < .01$) for the mind dimension, and $r = 0.32$ ($p < .05$) for body dimension.

The SMS-PA is a Likert-type scale that has 12 items: six items for state mindfulness of the mind and six items for state mindfulness of the body (Appendix C). The SMS-PA's six mind items are copied from the SMS, but the six body items differ. Whereas the SMS' body items focus on general awareness of bodily sensations, the SMS-PA's body items focus on awareness of bodily movement and sensations caused by physical activity. On the SMS-PA, each item is a declaratory statement (e.g., Item 7: I focused on the movement of my body). Respondents read each statement and report how much they agree or disagree by marking a checkmark or X under 0 (*not at all*), 1 (*a little*), 2 (*moderately*), 3 (*quite a bit*), or 4 (*very much*). In this study, participants reported a 1 for *not at all*, 2 for *a little*, 3 for *moderately*, 4 for *quite a bit*, and 5 for *very much*. All 12 items were summed for a maximum total state mindfulness score of 60 AU (12 items × 5 = 60), a maximum state mindfulness of the mind score of 30 (6 items × 5), and a maximum state mindfulness of the body score of 30 (6 items × 5). Higher total scores, mind scores, and body scores indicate a more focused level of state mindfulness than do lower total and subscores. In the publication that introduced the SMS-PA, the scale showed internal consistency (Cronbach's α = 0.87–0.93) and convergent validity among U.S. adults (Cox et al., 2016). There were significant, moderate-to-strong positive correlations between SMS-PA total and mind subscale scores and TMS subscale scores (r = 0.30–0.75, $p < 0.01$) (Cox et al., 2016). At the time of this dissertation, the author was not able to locate any test-retest data for the SMS-PA.

The VAS-M is a visual analog scale with one statement and one question (Appendix D). The statement is declaratory: "From moment-to-moment, I noticed and accepted my thoughts, feelings, bodily sensations, and environment without judging them." The question asks, "How well does the sentence describe your experience over the last 10 minutes?" Respondents read the statement and answer the question by marking a vertical dash along a non-graduated 100-millimeter (mm) horizontal line. The horizontal line is anchored on the left by the statement *not at all like my experience* and on the right by the statement *exactly like my experience.*

The LCN is a Likert-type scale that has 15 items that measure connectedness to nature (Perkins, 2010) (Appendix B). Each item is a declaratory statement (e.g., Item 1: I feel joy just being in nature). Respondents read each statement and report how much they agree or disagree by marking a checkmark or X under 1 (*very strongly disagree*), 2 (*strongly disagree*), 3 (*disagree*), 4 (*neutral*), 5 (*agree*), 6 (*strongly agree*), or 7 (*very strongly agree*). All 15 items are summed for a maximum total score of 105 AU. (15 items × 7 = 105). A high total score indicates strong feelings of connectedness to nature. The first data to support the validity of the LCN were published in 2010 (Perkins, 2010). The LCN's convergent and construct validity were demonstrated by the relationships between LCN scores and scores on other validated measures of connectedness to nature and similar constructs. There were significant, moderate-to-strong positive correlations between LCN scores and Connectedness to Nature Scale scores (CNS;

$r = 0.79$, $p < 0.001$), Inclusion of Nature in Self scores (INS; $r = 0.57$, $p < .001$), and New Ecological Paradigm scores (NEP; $r = 0.41$, $p < .001$). The CNS, INS, and NEP reflect pro-environmental orientations, including people's values, beliefs, and behaviors. (Perkins, 2010). The same publication that presented these validity data reported that the LCN demonstrated internal consistency (Cronbach's $\alpha = 0.97$), discriminant validity, and criterion-related validity. Regarding criterion-related validity, there were significant, weak-to-moderate positive correlations between LCN scores and the self-reported frequency of pro-environmental behaviors ($r = 0.32$–0.51, $p < 0.001$). Additionally, LCN scores significantly predicted the willingness to make lifestyle sacrifices to protect the environment (Perkins, 2010). Besides the evidence to support the LCN's validity, there is evidence to support the scale's test-retest reliability. Salatto (2021) reported the LCN had excellent test-retest reliability across two mountain biking rides separated by 10 minutes: coefficient of variation (CV) = 2.3%, intraclass correlation coefficient (ICC) and 95% CI = 0.94 [0.85, 0.98].

The VAS-N is a visual analog scale with one statement and one question (Appendix E). The statement is declaratory: "Interacting with nature brings me joy and makes me feel a sense of personal connection to and care for nature." The question asks, "How well does the sentence describe your present feeling?" Respondents read the statement and answer the question by marking a vertical dash along a non-graduated 100-mm horizontal line. The horizontal line is anchored on the left by the statement *very strongly disagree* and on the right by the statement *very strongly agree*.

The methods at the TGT and the WP were identical from the time that potential participants arrived to when they gave verbal and written consent and were enrolled as participants. Beyond that point, the methods at the two sites differed slightly and are next described separately.

Having just completed the informed consent and now possessing the binder, the participants at the TGT were scheduled to complete two conditions separated by approximately 24 hours: 1) music and 2) no music. One condition was completed on day one (October 1) and one condition was completed on day two (October 2). The order of the conditions was determined by generating a random number between one and two on Google Sheets (Google LLC, Mountain View, CA) via the function =RANDBETWEEN(1,2). If the function returned a one, a participant completed condition one on day one and condition two on day two. If the function returned a two, a participant completed condition two on day one and condition one on day two.

The two conditions were nearly identical, and their flow from the researcher's perspective is provided as a list (Appendix F). Briefly, both conditions had participants complete the first Series A, choose a spot away from the trailhead and sit alone for 10 minutes, complete the second Series A, walk alone on the trail for 10 minutes, and complete Series B. Each condition thus exposed participants to 10 minutes of inactive immersion and 10 minutes of light-to-moderate aerobic exercise in green space. How the conditions differed was whether participants sat and walked while listening to music. In condition one (music), participants listened to music of a self-selected genre from the moment they sat. Before leaving to sit, participants reported what genre they chose and were asked not to change the genre or skip songs. They were also asked not to use their phones other than to start the music and use timers for sitting and walking. Participants listened to the music on their phones via headphones. Anyone who did not have headphones was lent a pair. In condition two (no music), participants did not listen to music and were asked not to use their phones other than to use timers for sitting and walking.

At the WP, having just completed the informed consent and now possessing the binder, the participants were scheduled to complete a single condition twice, separated by approximately 24 hours. The first completion was on day one (November 6) and the second completion was on day two (November 7). The protocol was the same across both days and was identical to condition two (no music) of the TGT protocol. For this reason, randomization was not required. The flow of the conditions from the researcher's perspective is provided as a list (Appendix G). Between day one and two at the WP, the responses from day one were removed from the binders.

The author of the dissertation copied the participants' demographic and anthropometric data from the binders from each study site to a Google Sheet. Copying the response data from the five measures in the binders to a Google Sheet was a collaboration between the author and his committee chair. The chair read the responses aloud to the author as he entered the data into the Google Sheet. For each VAS, the chair measured the response by using a standard 12-inch ruler. When the chair was uncertain about a response, the chair and author discussed the response in question and reached a consensus.

3.3.3 Statistical Analysis

The participants' demographic and anthropometric characteristics were summarized as frequencies, arithmetic means, and standard deviations via Google Sheets. Proportions were calculated to determine how many participants had complete data for the SMS, VAS-M, and SMS-PA at each timepoint at each study site (Calculator, Apple Inc., Cupertino, CA). The null hypotheses for the primary, secondary, and tertiary aims were tested statistically with the appropriate parametric or non-parametric statistical analyses and, when warranted, post hoc analyses. All the statistical analyses for this study were run in IBM SPSS Statistics v28 (IBM, Armonk, NY, United States) with an α-level of 0.05. Unless stated otherwise, all data are presented as arithmetic means ± standard deviations ($\overline{x}$ + SD). The null hypotheses (H_0) and alternative hypotheses (H_1) for the primary aim were:

- H_{0A}: The population arithmetic mean SMS score does not differ among pre-sit, post-sit, and post-walk ($\mu_{pre\text{-}sit} = \mu_{post\text{-}sit} = \mu_{post\text{-}walk}$).
- H_{1A}: The population arithmetic mean SMS score at ≥ 1 timepoint differs from the population arithmetic mean of SMS at ≥ 1 other timepoints (i.e., at least two means differ).
- H_{0B}: The population arithmetic mean VAS-M score does not differ among pre-sit, post-sit, and post-walk ($\mu_{pre\text{-}sit} = \mu_{post\text{-}sit} = \mu_{post\text{-}walk}$).
- H_{1B}: The population arithmetic mean VAS-M score at ≥ 1 timepoint differs from the population arithmetic mean of VAS-M at ≥ 1 other timepoints (i.e., at least two means differ).

The null hypotheses and alternative hypotheses for the secondary aim were:

- H_{0C}: The population arithmetic mean VAS-M score is not correlated with the population arithmetic mean SMS score at pre-sit, post-sit, and post-walk.
- H_{1C} The population arithmetic mean VAS-M score is correlated with the population arithmetic mean SMS score at pre-sit, post-sit, and post-walk.
- H_{0D}: The population arithmetic mean SMS-PA score is not correlated with the population arithmetic mean SMS score at post-walk.

- H_{1D}: The population arithmetic mean SMS-PA score is correlated with the population arithmetic mean SMS score at post-walk.

The null hypotheses and alternative hypotheses for the tertiary aim were:

- H_{0E}: SMS scores are not reliable across approximately 24 hours.
- H_{1E}: SMS scores are reliable across approximately 24 hours.
- H_{0F}: VAS-M scores are not reliable across approximately 24 hours.
- H_{1F}: VAS-M scores are reliable across approximately 24 hours.
- H_{0G}: SMS-PA scores are not reliable across approximately 24 hours.
- H_{1G}: SMS-PA scores are reliable across approximately 24 hours.

Before testing the hypotheses, a power analysis was needed to estimate the minimum sample size required for adequate statistical power. The power analysis was based on the primary outcome, state mindfulness. The author of this dissertation could not find similar-enough studies to run an optimal power analysis. This dilemma existed because, to the author's knowledge, no study before this one reported state mindfulness data that were collected several times over a brief non-mindful intervention. Other studies measured state mindfulness before and after brief mindful walks (Ameli et al., 2021; Bigliassi et al., 2020; Cox et al., 2016) or once a week in a non-intervention control group across 10 weeks (Tanay & Bernstein, 2013). The published studies also had different participants and outcomes than the current study. Given the dilemma, several power analyses were run (G*Power 3.1, University of Düsseldorf, Düsseldorf, Germany) to make an educated guess about the sample size needed (Faul et al., 2007). These power analyses were based on related studies found in Study 1, the systematic review.

One of these studies gave participants instructions on being mindful and had them walk in green space for 20 minutes (Ameli et al., 2021). The effect size of the pre- to post-walk increase in state mindfulness was $r = 0.52$ (Wilcoxon's $z/\sqrt{\text{number of observations}}$). Another study had participants walk for 4–6 minutes three times, once each while listening to no audio, audio to induce mindfulness, and audio to induce mindlessness (Bigliassi et al., 2020). Walking while listening to audio affected mindfulness ($\eta_p^2 = 0.61$). A third study—closer to the current study than the other two studies—had participants walk on a treadmill for 10 minutes twice (Cox et al., 2018). The first walk was the control walk, and the second walk was the mindful walk, which was preceded immediately by the participants reading mindful walking instructions and examples. The effect sizes between the control and mindfulness conditions for attentional focus and state mindfulness of the body were $\eta_p^2 = 0.67$ and $\eta_p^2 = 0.40$, respectively. These three studies differ from the current study because they gave instructions or audio to induce mindfulness. However, the studies are like the current study because they had repeated-measures (RM) designs for measuring changes in mindfulness caused by a walking intervention.

The effects sizes reported by Ameli et al. (2021), Bigliassi et al. (2020), and Cox et al. (2018) were inputted separately into the "Effect size f" input parameter in G*Power 3.1. The test family selected was "*F* tests," and the test selected was "ANOVA: Repeated measures, within factors." The type of power analysis was "A priori: Compute required sample size - given α, power, and effect size." The "α err prob" inputted was 0.05, and the "Power (1−β err prob)" inputted was 0.80. The number of groups inputted was one group with three measurements (for the 1 × 3 RM ANOVA). The default values of 0.5 for "correlation among rep measures" and 1 for "nonsphericity correction ε" were not changed. Based on these settings and the G*Power 3.1 calculations, the current study needed at least 6–12 participants for the design to achieve 80% statistical power with an α-level of 0.05. If it is assumed that state mindfulness changes less from pre- to post-walk without a mindfulness intervention, the effect sizes of the present study's intervention are probably smaller too. If this supposition is true, sample sizes bigger than 6–12 participants would be needed to reach a sufficient statistical power. Considering this possibility, the goal became to recruit 12–24 participants to match the sample size of the studies on acute mindful walking (Ameli et al., 2021; Bigliassi et al., 2020; Cox et al., 2018).

The null hypotheses of the primary aim were tested statistically via two separate 1 × 3 RM analyses of variance (1 × 3 RM ANOVAs), one each for the SMS and VAS-M. Before each ANOVA, the data were checked for alignment with the five assumptions of the one-way RM ANOVA.

1. Dependent Variable (DV: SMS or VAS-M scores): There is one continuous DV.
2. Independent Variable (IV: Time): There is one categorical within-subjects IV that has three or more categories.
3. Outliers: There are no significant DV outliers in any category of the within-subjects IV.
4. Normality: The DV is approximately normally distributed in every category of the within-subjects IV.
5. Sphericity: The variances of the differences between all combinations of the categories of the within-subjects IV are equal.

The data from the no-music day at the TGT and day one at the WP were analyzed together. The data met the DV and IV assumptions and were checked for outliers by inspecting boxplots of pre-sit, post-sit, and post-walk SMS and VAS-M scores. Outliers were considered data points greater than 1.5 box-lengths (interquartile range) from either edge of the box (the top edge was Q_1, and the bottom edge was Q_3). There were no SMS outliers at pre-sit or post-sit, but there were two outliers at post-walk. There were no pre-sit VAS-M outliers, but there was one outlier at post-sit and two outliers at post-walk. None of the SMS or VAS-M outliers seemed to be errors in measurement or data entry. Rather, the outliers seemed to be genuinely unusual data points from the others in the dataset. Keeping the three outliers was not ideal for the validity of the ANOVAs, but there was no good reason to reject the outliers as invalid data points. After consulting his committee chair, the author of this dissertation kept the outliers for the ANOVAs.

Besides outliers, the data were checked for normality and sphericity. Normality was checked by inspecting histograms and running six separate Shapiro-Wilk tests: one each for pre-sit SMS, post-sit SMS, post-walk SMS, pre-sit VAS-M, post-sit VAS-M, and post-walk VAS-M. Pre-sit and post-sit SMS were normally distributed ($p > 0.05$) but post-walk SMS was not ($p < 0.001$). Pre-sit, post-sit, and post-walk VAS-M were not normally distributed ($p < 0.042$, $p = 0.026$, and $p < 0.001$, respectively). Despite the non-normality, the ANOVAs were conducted because they are robust to this assumption. The last assumption, sphericity, was checked via Mauchly's test. Both the SMS and VAS-M data violated the sphericity assumption ($p = 0.03$ and $p < 0.001$, respectively), so the respective ANOVAs were corrected to prevent the risk of a type 1 statistical error from inflating. The epsilon (ε) correction was estimated via the Greenhouse-Geisser method and used to adjust the degrees of freedom used in calculating the *p*-values for the ANOVAs. The effect sizes for the ANOVAs were calculated as partial omega squared (ω_p^2), which is a population estimate derived from the sample estimate partial eta squared (η_p^2). All ω_p^2 values were classified according to the thresholds published by Ferguson (2009) and used in mindful walking research published by Cox et al. (2018):
0.04 = "recommended minimum effect size representing a 'practically' significant effect [RMPE]," 0.25 = moderate, and 0.64 = strong. The ω_p^2 value for each 1 × 3 RM ANOVA was interpreted as the proportion of variance in the dependent variable (i.e., SMS or VAS-M scores) explained by the independent variable in the experimental design (i.e., time). The ANOVAs that

yielded significant *F*-statistics were followed by post hoc pairwise comparisons, specifically paired-samples *t* tests that were each corrected by the Bonferroni adjustment. The effect sizes for the paired-samples *t*-tests were Cohen's *d*. All *d* values were classified according to the thresholds published by Ferguson (2009): 0.41 = RMPE, 1.15 = moderate, and 2.70 = strong.

The null hypotheses of the secondary aim were initially going to be tested statistically via five separate two-tailed Pearson's product-moment correlations (Pearson's *r*).

1. Pre-sit: VAS-M vs. SMS
2. Post-sit: VAS-M vs. SMS
3. Post-walk: VAS-M vs. SMS
4. Post-walk: VAS-M vs. SMS-PA
5. Post-walk: SMS-PA vs. SMS

Before each correlation, the data were checked for alignment with the five assumptions of Pearson's product-moment correlations.

1. Level of Measurement: The two variables should be continuous, meaning they are measured at the interval or ratio level.
2. Linear Relationship: There should be a linear relationship between the two variables.
3. Normality: Both variables should be approximately normally distributed.
4. Related Pairs: Each observation in the dataset should have a pair of values.
5. No Outliers: There should be no extreme outliers in the dataset.

As was done for the primary aim, the data from the no-music day at the TGT and day one at the WP were analyzed together. The SMS, VAS-M, and SMS-PA scores at pre-sit, post-sit, and post-walk met the assumptions of level of measurement, related pairs, and linear relationship, the latter of which was confirmed by inspecting scatterplots. Shapiro-Wilk tests and boxplots showed that all the scores violated the normality and outlier assumptions except pre-sit VAS-M, pre-sit-SMS, and post-sit SMS ($p = 0.087$, $p = 0.203$, and $p = 0.474$, respectively). Given how the data violated the assumptions, Pearson's product-moment correlation was only a valid analysis for pre-sit VAS-M vs. pre-sit SMS. Correlations 2–5 listed above were calculated by using the non-parametric alternative to Pearson's product-moment correlation, Spearman's rank-order correlation (Spearman's rho; ρ). Every Spearman's rank-order correlation was two-tailed.

As another test of the concurrent validity of the VAS-M against the criterion SMS, a 1×3 RM ANOVA was run on the VAS-M data. This analysis was chosen to determine whether there were statistically significant differences among pre-sit, post-sit, and post-walk VAS-M scores. The VAS-M scores changing over time in the same direction as the SMS scores would be evidence of concurrent validity. As with the SMS data, the VAS-M data were checked for alignment with the five assumptions of the one-way RM ANOVA. Decisions on the assumptions were made as per the same rules as for the SMS data.

The null hypotheses of the tertiary aim were tested statistically by calculating absolute reliability as the CV and relative reliability as the ICC. The CVs and ICCs were calculated from the day one and day two data from the WP. Three CVs and ICCs were calculated for the SMS and VAS-M, one each for pre-sit, post-sit, and post-walk. One CV and ICC were calculated for the SMS-PA at post-walk. At each timepoint, a measure was considered reliable if it met the a priori criteria of a CV < 10%, a significant ICC > 0.70 ($p < 0.05$), and the lower-bound of the 95% confidence interval (CI) of the ICC > 0.70.

3.4 Results

The proportions of participants who completed the SMS, VAS-M, and SMS-PA at each timepoint are reported for the overall sample (Table 13) and by study site (Appendix I, Tables 24–25). Participants were not asked why they missed or skipped measures. The completion rate of the SMS was lower than the completion rate of the other scales because of the author's error during Session 1 at the TGT (Appendix I, Table 24). He missed a typographical error before printing and administering the SMS. The text of SMS Question 17 ("I noticed thoughts come and go") was listed for Questions 17 and 18. The correct text of Question 18 ("I felt in contact with my body") was missing. The mistake was caught on day one after nine participants already had their binders. Those nine participants' SMS data were consequently lost. The erroneous Question 18 was the only question missed during Session 1 by seven participants at pre-sit, eight participants at post-sit, and nine participants at post-walk. This fact suggests that, without the error, the overall Session 1 SMS proportions would have been pre-sit (39/42, 93%), post-sit (41/42, 98%), and post-walk (40/42, 95%). The data loss was not distributed evenly across the music and no music conditions at the TGT (2/9 [22%] and 7/9 [78%], respectively). Given the within-subjects design of the study (music vs. no music), the nine participants' data could not be used to compare SMS scores between the music and no music conditions. The SMS was corrected before being issued to participants 10–19 for Session 1 and to the 18 participants who returned to complete Session 2.

Table 13. Proportion of participants with complete SMS, VAS-M, and SMS-PA data in the overall sample (*N* = 42).

Session	Timepoint	SMS*	VAS-M	SMS-PA
Session 1	Pre-Sit	32/42 (76%)	42/42 (100%)	-
	Post-Sit	33/42 (79%)	42/42 (100%)	-
	Post-Walk	31/42 (74%)	41/42 (98%)	40/42 (95%)
Session 2	Pre-Sit	38/40 (95%)	39/40 (98%)	-
	Post-Sit	40/40 (100%)	39/40 (98%)	-
	Post-Walk	40/40 (100%)	40/40 (100%)	38/40 (95%)

SMS: State Mindfulness Scale; VAS-M: Visual Analog Scale-Mindfulness; SMS-PA: State Mindfulness Scale for Physical Activity. The denominator for Session 2 is 40 instead of 42 because one participant dropped out after Session 1 at each study site.

The 1 × 3 RM ANOVA warranted rejecting the null hypothesis that the population arithmetic means of SMS scores are equal at pre-sit, post-sit, and post-walk. Not all three of the arithmetic means were equal. At least one of the arithmetic means was statistically significantly different from at least one other arithmetic mean; $F(1.65, 49.40) = 54.35$, $p < 0.001$, $\omega_p^2 = 0.53$, a moderate effect. The post hoc pairwise comparisons showed that sitting and walking in green space increased participants' SMS scores from their baseline upon arriving at the trailhead or park (Figure 11). The mean SMS scores increased from 51.8 ± 19.5 AU at pre-sit to 81.0 ± 15.1 AU at post-sit (+29.2 AU, 95% CI [20.1, 38.4], $p < 0.001$, $d = 1.68$, a moderate effect) and to 86.0 ± 16.6 AU at post-walk (+34.2 AU, [23.6, 44.7], $p < 0.001$, $d = 1.89$, a moderate effect). The mean SMS scores did not change from post-sit to post-walk (+4.9 AU, [−1.9, 11.8], $p = 0.232$, $d = 0.31$, no practically significant effect). These results show that sitting undisturbed for 10 minutes near a trailhead or in a park increased SMS scores. After sitting for 10 minutes, walking for 10 minutes on the trail or in the park did not increase SMS scores further.

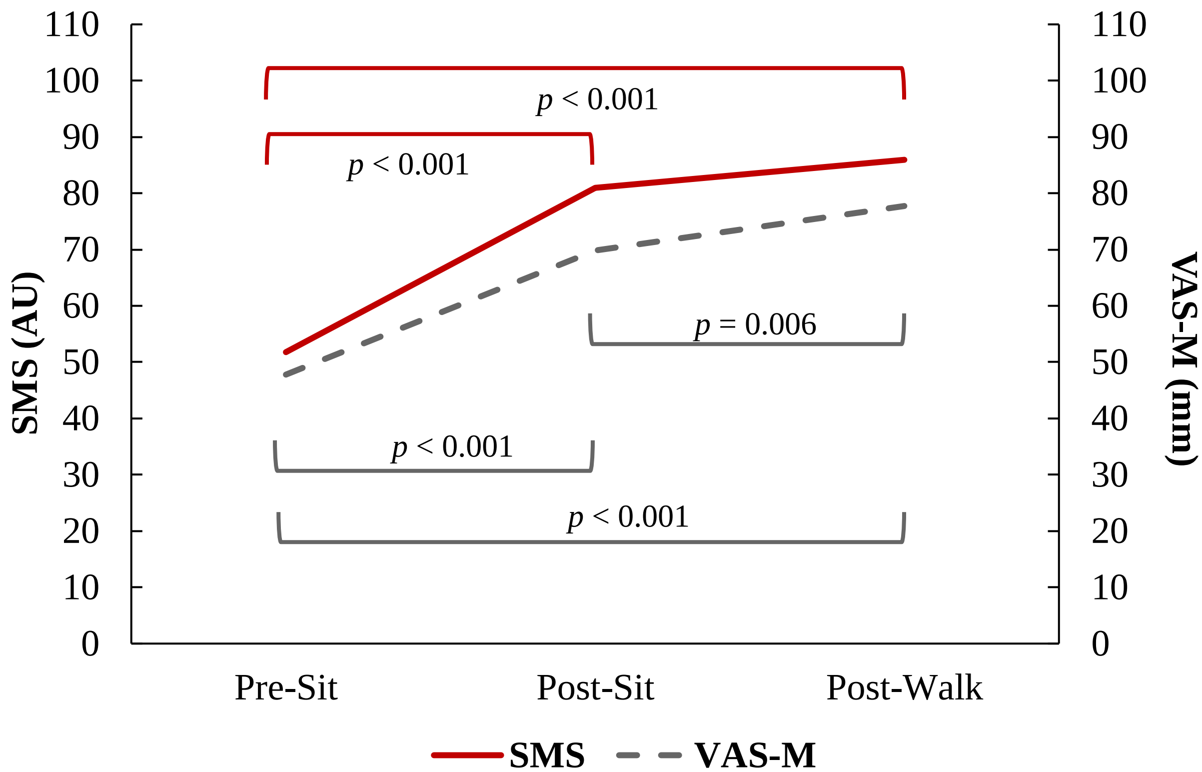

Figure 11. SMS and VAS-M scores from pre-sit to post-sit to post-walk. Standard deviations of SMS and VAS-M scores at each timepoint are provided in the dissertation's text. SMS: State Mindfulness Scale; VAS-M: Visual Analog Scale-Mindfulness; AU: arbitrary units; mm: millimeters.

The VAS-M scores responded similarly to the SMS scores from pre-sit to post-sit to post-walk (Figure 11). The 1 × 3 RM ANOVA warranted rejecting the null hypothesis that the population arithmetic means of VAS-M scores are equal at pre-sit, post-sit, and post-walk. Not all three of the arithmetic means were equal. At least one of the arithmetic means was statistically significantly different from at least one other arithmetic mean; $F(1.36, 53.05) = 35.05$, $p < 0.001$, $\omega_p^2 = 0.36$, a moderate effect). The post hoc pairwise comparisons showed that sitting and walking in green space increased participants' VAS-M scores from their baseline upon arriving at the trailhead or park (Figure 11). The mean VAS-M scores increased from 47.8 ± 28.4 mm to 69.9 ± 21.6 mm from pre-sit to post-sit (+22.1 mm, 95% CI [12.8, 31.4], $p < 0.001$, $d = 0.88$, a practically significant effect) and to 77.7 ± 20.1 mm at post-walk (+29.9 mm [18.2, 41.7], $p < 0.001$, $d = 1.22$, a moderate effect). The mean VAS-M scores also increased by 7.9 mm [2.0, 13.8] from post-sit to post-walk ($p = 0.006$, $d = 0.38$, no practically significant effect). These results show that the VAS-M scores tracked the SMS scores upon arrival at a trailhead or park and after sitting there undisturbed for 10 minutes. Though the mean VAS-M score significantly increased from post-sit to post-walk while the mean SMS score did not, the mean scores of both measures moved in the same direction (Figure 11).

The correlations between VAS-M and SMS at pre-sit, post-sit, and post-walk warranted rejecting the null hypotheses that the VAS-M and SMS are not correlated (Table 14 and Figures 12–14). The null hypotheses that the SMS-PA is not correlated with the SMS and the VAS-M is not correlated with the SMS-PA are also rejected. At post-walk, there was a statistically significant, very strong positive correlation between SMS-PA and SMS scores; $\rho(31) = 0.89$, 95% CI [0.78, 0.95], $p < 0.001$ (Figure 15). There was also a statistically significant, strong positive correlation between the VAS-M and SMS-PA scores; $\rho(31) = 0.67$ [0.42, 0.83], $p < 0.001$ (Figure 16).

Table 14. Correlations between VAS-M and SMS at pre-sit, post-sit, and post-walk.

Timepoint	Coefficient(df) (95% CI)	Direction, Strength	*p*-value
Pre-Sit	$r(31) = 0.58$ (0.30, 0.77)	Positive, Moderate	< 0.001
Post-Sit	$\rho(33) = 0.34$ (−0.003, 0.61)	Positive, Weak	0.046
Post-Walk	$\rho(32) = 0.55$ (0.25, 0.75)	Positive, Moderate	< 0.001

df: degrees of freedom; CI: confidence interval. The pre-sit correlation was a two-tailed Pearson's product-moment correlation (Pearson's *r*). The post-sit and post-walk correlations were two-tailed Spearman's rank-order correlations (Spearman's rho; ρ). The 95% CIs were estimated based on Fisher's *r*-to-*z* transformation. When estimating the 95% CIs for Spearman's ρ, the standard error was estimated by using Fieller, Hartley, and Pearson's formula in IBM SPSS Statistics v28.

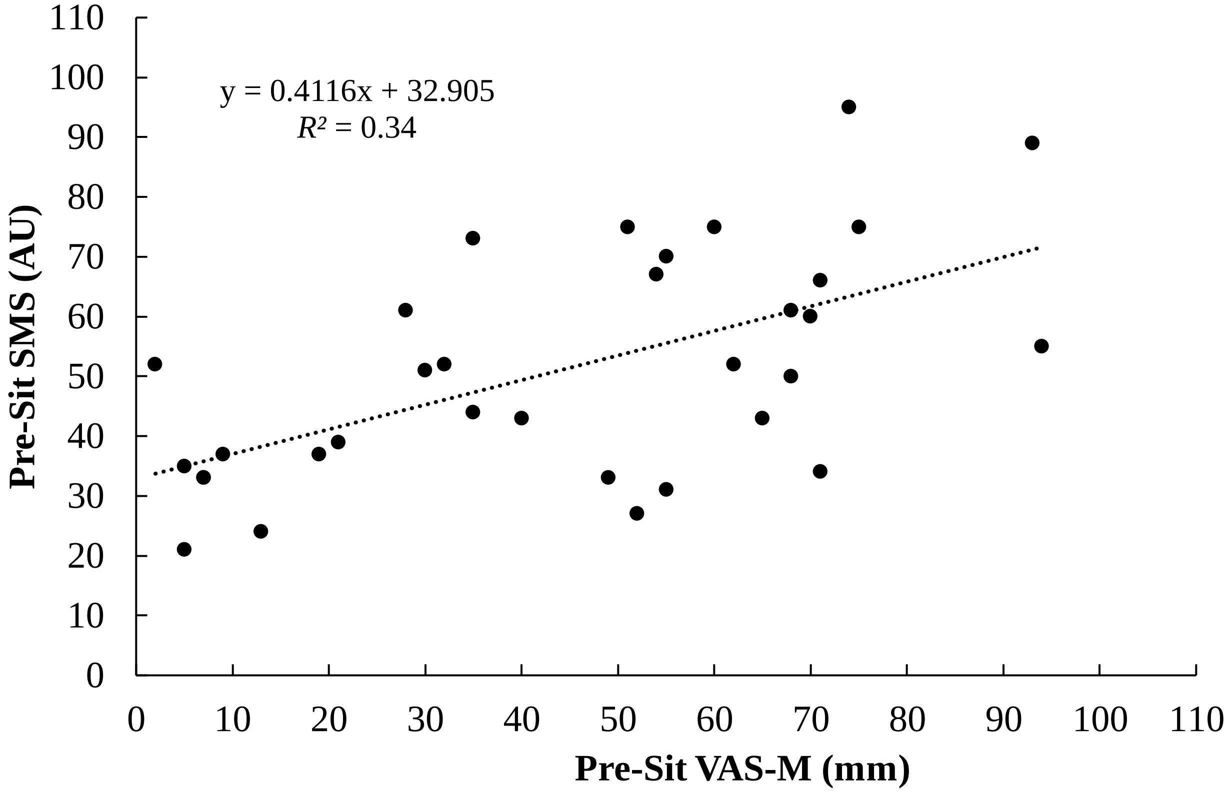

Figure 12. Scatterplot of pre-sit VAS-M and pre-sit SMS scores (*n* = 33). Maximum VAS-M and SMS scores are 100 mm and 105 AU, respectively. Line is the linear trendline. VAS-M: Visual Analog Scale-Mindfulness; SMS: State Mindfulness Scale; mm: millimeters; AU: arbitrary units.

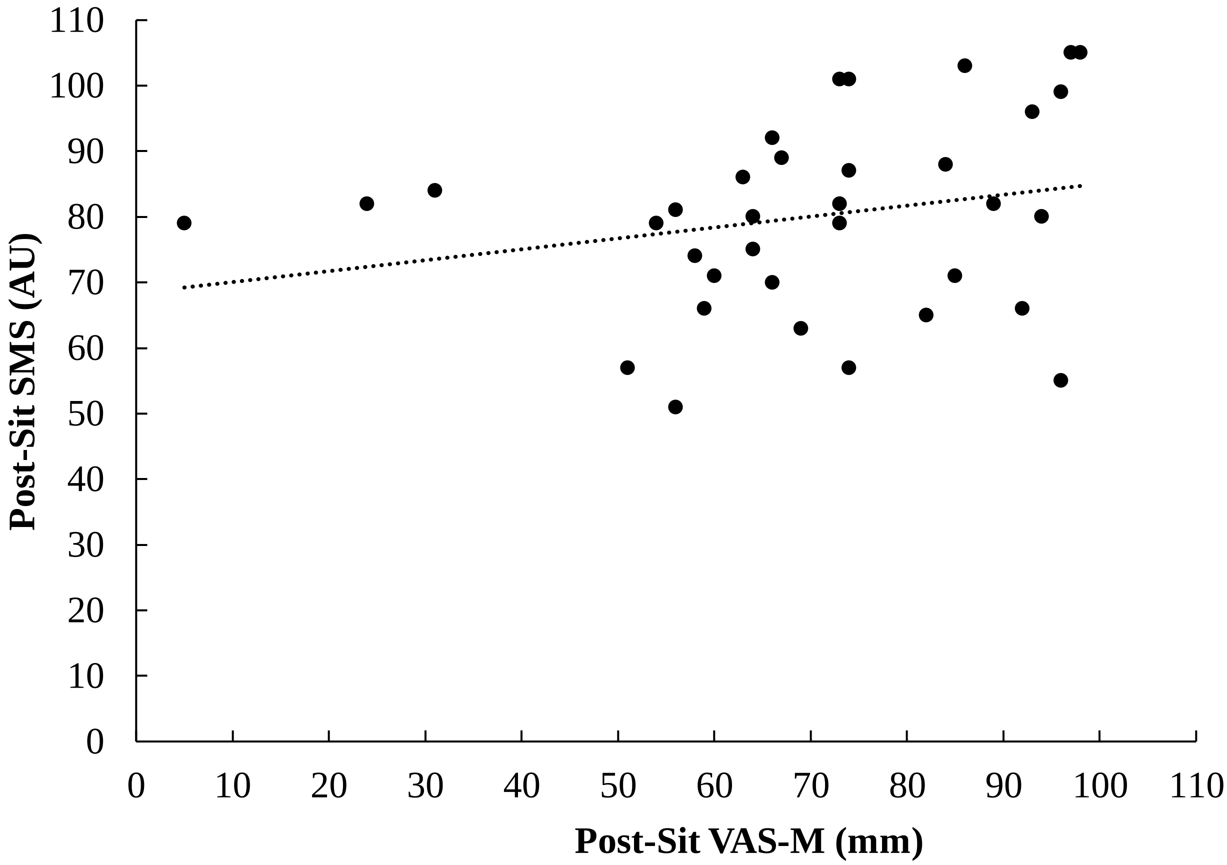

Figure 13. Scatterplot of post-sit VAS-M and post-sit SMS scores (*n* = 35). Maximum VAS-M and SMS scores are 100 mm and 105 AU, respectively. Line is the linear trendline. Neither the regression equation nor the coefficient of determination is shown because the data violated at least one assumption of Pearson's product-moment correlation. VAS-M: Visual Analog Scale-Mindfulness; SMS: State Mindfulness Scale; mm: millimeters; AU: arbitrary units.

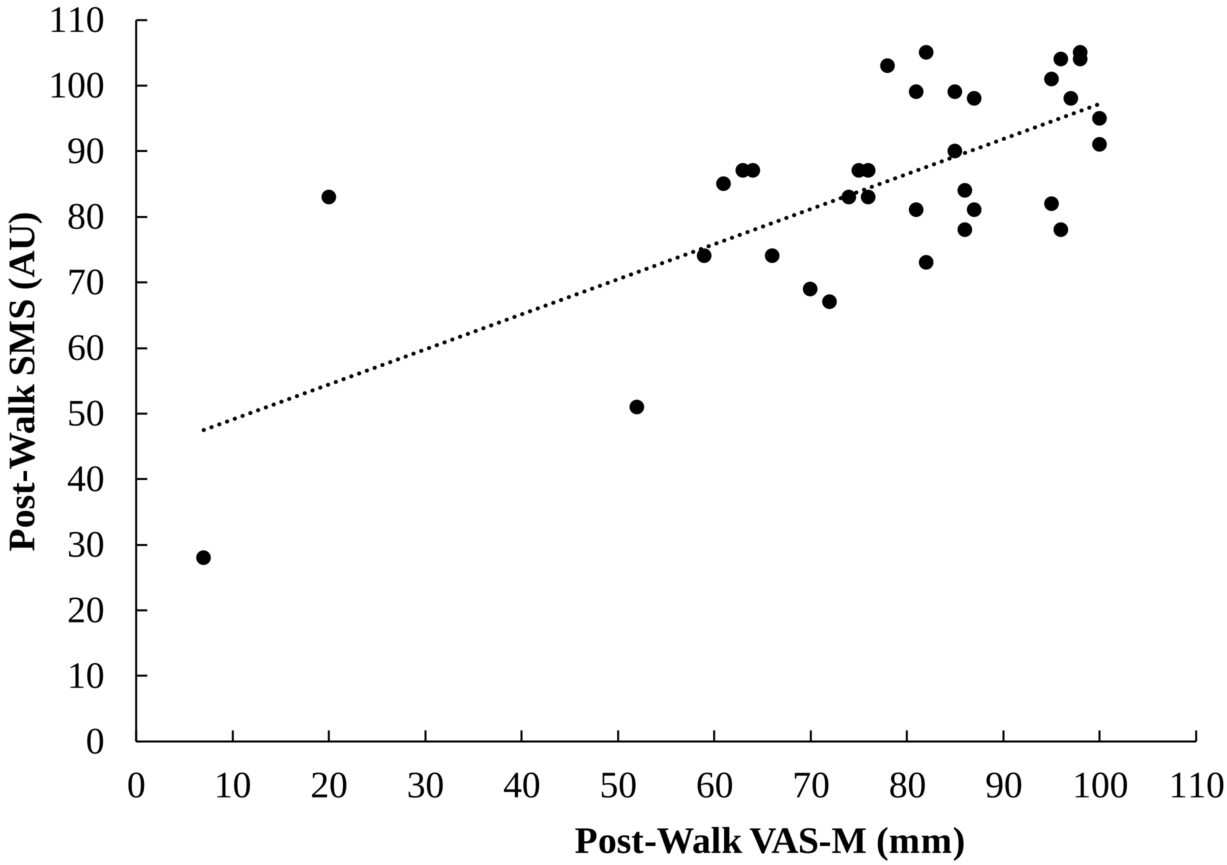

Figure 14. Scatterplot of post-walk VAS-M and post-walk SMS scores (n = 34). Maximum VAS-M and SMS scores are 100 mm and 105 AU, respectively. Line is the linear trendline. Neither the regression equation nor the coefficient of determination is shown because the data violated at least one assumption of Pearson's product-moment correlation. VAS-M: Visual Analog Scale-Mindfulness; SMS: State Mindfulness Scale; mm: millimeters; AU: arbitrary units.

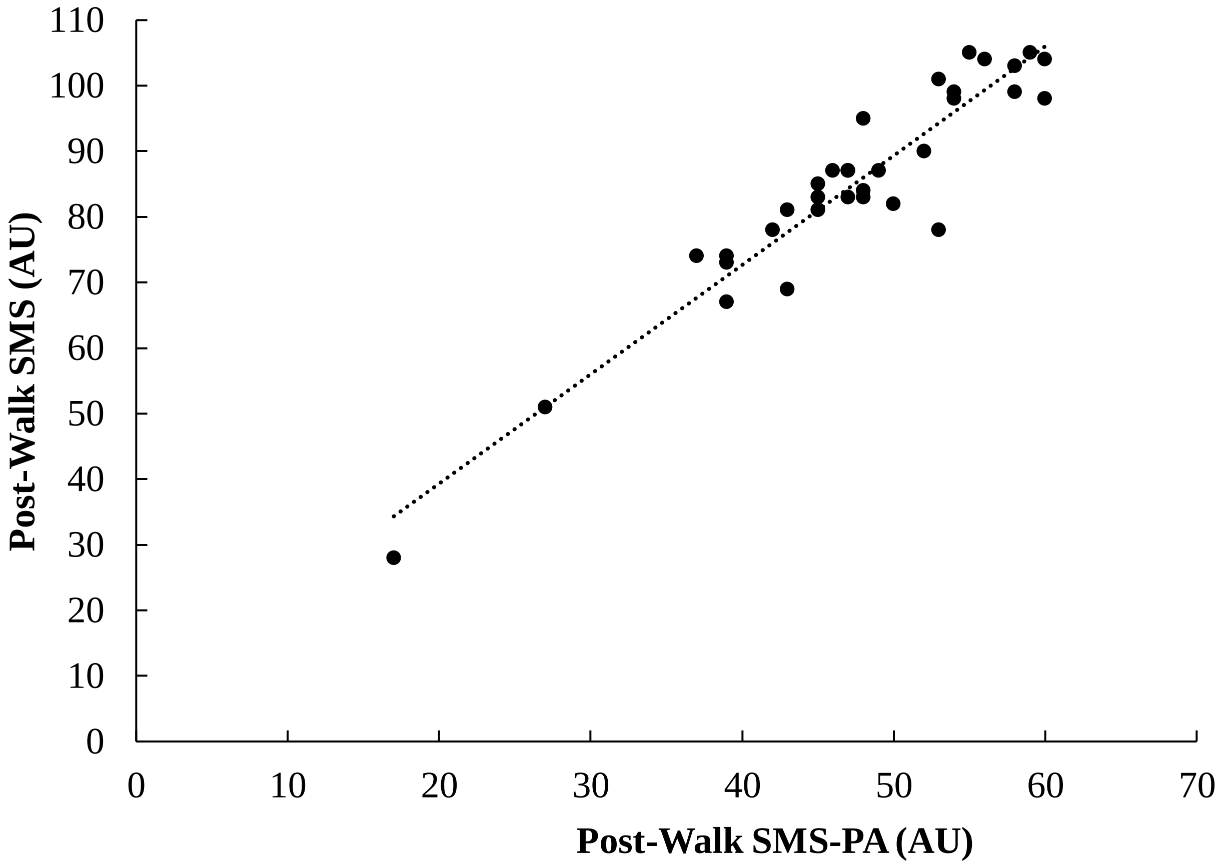

Figure 15. Scatterplot of post-walk SMS-PA and post-walk SMS scores (*n* = 33). Maximum SMS-PA and SMS scores are 60 and 105 AU, respectively. Line is the linear trendline. Neither the regression equation nor the coefficient of determination is shown because the data violated at least one assumption of Pearson’s product-moment correlation. SMS-PA: State Mindfulness Scale for Physical Activity; SMS: State Mindfulness Scale; AU: arbitrary units.

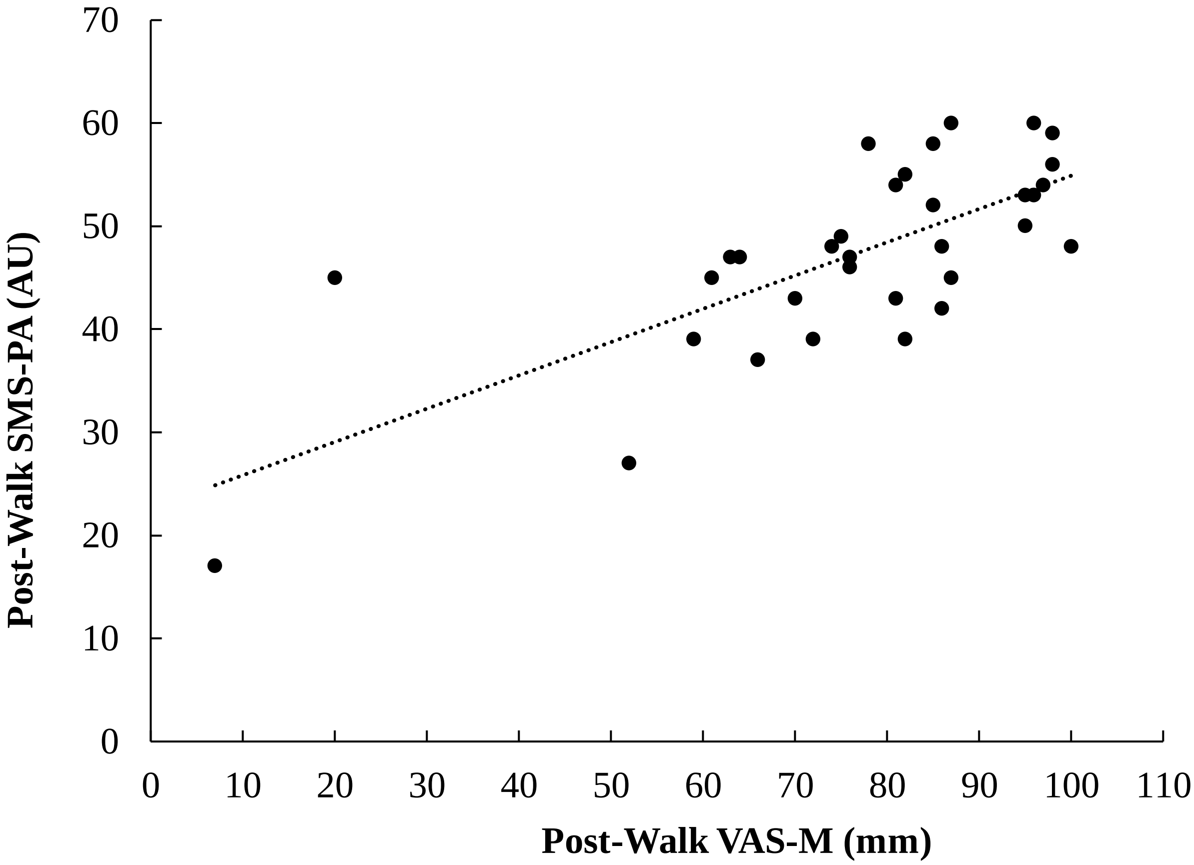

Figure 16. Scatterplot of post-walk VAS-M scores and post-walk SMS-PA scores (*n* = 33). Maximum VAS-M and SMS-PA scores are 100 mm and 60 AU, respectively. Line is the linear trendline. Neither the regression equation nor the coefficient of determination is shown because the data violated at least one assumption of Pearson's product-moment correlation. VAS-M: Visual Analog Scale-Mindfulness; SMS: State Mindfulness Scale for Physical Activity; mm: millimeters; AU: arbitrary units.

Based on the reliability criteria, there was no evidence to support the test-retest reliability of the SMS or VAS-M at pre-sit or post-sit. At post-walk, there was no evidence to support the test-retest reliability of the SMS, but there was such evidence for the VAS-M and SMS-PA (Table 15).

Table 15. CVs and ICCs for the SMS, VAS-M, and SMS-PA.

	Pre-Sit		Post-Sit		Post-Walk	
	CV (%)	ICC	CV (%)	ICC	CV (%)	ICC
SMS	16.2	0.62 (0.27, 0.83) $p = 0.001$ Moderate	7.2	0.50 (0.11, 0.75) $p = 0.007$ Moderate	5.3	0.85 (0.66, 0.93) $p < 0.001$ Moderate
VAS-M	36.9	0.54 (0.16, 0.78) $p = 0.004$ Moderate	18.1	0.68 (0.37, 0.86) $p < 0.001$ Moderate	7.8	0.95 (0.87, 0.98) $p < 0.001$ Good
SMS-PA	-	-	-	-	5.5	0.90 (0.76, 0.96) $p < 0.001$ Good

SMS: State Mindfulness Scale; VAS-M: Visual Analog Scale-Mindfulness; SMS-PA: State Mindfulness Scale for Physical Activity; CV: coefficient of variation; ICC: intraclass correlation coefficient (95% confidence interval [CI]). Interpretation of ICC based on 95% CIs per the guidance of Koo and Li (2016). Red, yellow, and green shading indicate, respectively, that the CV or ICC did not meet, was close to, or met the threshold for test-retest reliability. The threshold for reliability per the CV was < 10% and per the ICC was a significant coefficient > 0.70 ($p < 0.05$) with the lower-bound of the 95% CI > 0.70.

3.5 Discussion

This study had three aims. The primary aim was to determine whether sitting or walking in green space changes people's state mindfulness. The secondary aim was to test the concurrent validity of two quicker measures of state mindfulness (the VAS-M and SMS-PA) against the criterion measure (SMS). The tertiary aim was to assess the test-retest reliability of 1) the SMS and VAS-M before sitting, after sitting, and after walking, and 2) the SMS-PA after walking.

The proportions of participants who responded completely to the SMS, VAS-M, and SMS-PA at pre-sit, post-sit, and post-walk were high. The only exception was the SMS during Session 1 at the TGT, which is a limitation explained later. The otherwise high proportions of complete responses on the SMS and SMS-PA were likely supported by the author's modification of the scales. Rather than issue the measures in their default format, the author provided the SMS and SMS-PA in tables in which the rows alternated white and gray. This format probably kept the number of missed items on each measure low. The large volume of SMS, VAS-M, and SMS-PA responses allowed the present study to achieve its three aims.

The study achieved the primary aim and showed that state mindfulness increased after sitting and walking for 10 minutes each in green space. This finding is novel because apparently no study before now showed that acute GE increased state mindfulness without involving mindfulness practices. Published studies induced mindfulness in participants by giving them instructions before walking or audio recordings to listen to while walking. Participants in the study by Ameli et al. (2021) reported greater state mindfulness after a 20-minute walk in green space, but they were instructed to be mindful while walking. Similarly, participants in the study by Yang and Conroy (2019) were instructed to be mindful while walking and reported greater state mindfulness after 30-minute walks in an outdoor arboretum than before the walks. Participants in the study by Bigliassi et al. (2020) listened to audio recordings to induce mindfulness and reported greater state mindfulness after a 4–6-minute walk in an outdoor university park. In contrast to these published studies, participants in the study by Gotink et al. (2016) did not report greater state mindfulness after 1-, 3-, or 6+-day mindful walking retreats that involved mindfulness practices. When interpreting these studies, it is difficult to determine whether state mindfulness increased because of the GE, mindfulness, or both. The present dissertation makes it clear that GE without mindfulness instructions or audio recordings increased state mindfulness.

Green exercise may help people connect with their immediate inner experience and environment even without knowledge of or access to mindfulness practices. Though they did not measure state mindfulness, Navalta et al. (2021) reported that one bout of sitting and walking in green space raised participants levels of calm and comfort more than one bout of sitting and walking indoors. One may speculate that calm and comfort are supported by feeling in tune with one's thoughts, feelings, emotions, bodily sensations, and environment. The new findings presented here could motivate studies that measure state mindfulness, calm, comfort, anxiety, and distress before and after acute GE. Acute GE may be a promising self-directed therapy that alleviates mental hardship at least temporarily. No adverse effects of acute GE were reported by participants in the present dissertation. If found to alleviate mental hardship, acute GE would be preferable to many of the unhealthy coping strategies people already use (e.g., overeating and using alcohol, tobacco, and other drugs).

In addition to achieving its primary aim, the present dissertation achieved its secondary aim of testing the concurrent validity of two quicker measures of state mindfulness (the VAS-M and SMS-PA) against the criterion measure (SMS). The VAS-M was found to be a simple and quick measure of state mindfulness with scores that are significantly and positively correlated with the SMS scores. Despite these correlations, the data are not definitive evidence for the VAS-M's concurrent validity with the SMS under the conditions tested in the present study. The wide 95% CIs at pre-sit and post-walk indicate that the true correlation between the VAS-M scores and SMS scores could be very strong to very weak. The 95% CI at pre-walk crosses the null value of 0 by 0.003, indicating a non-significant correlation despite the *p*-value of 0.046. These correlations indicate more evidence is needed to consider the VAS-M a valid measure of state mindfulness in people before or after they briefly immerse themselves in nature or complete GE. This finding was surprising because, after GE, scores on the VAS-M correlated significantly, strongly, and positively with scores on the SMS-PA. There was evidence to support the SMS-PA being concurrently valid with the SMS. To the author's knowledge, this study is the first to report evidence that supports the concurrent validity of the SMS-PA with the SMS when administered after GE. This finding makes sense because six of the 12 items on the SMS-PA are copied from the SMS verbatim. The other six of 12 SMS-PA items are original and ask about thoughts, feelings, and bodily sensations related to physical activity and exercise.

The six questions unique to the SMS-PA may explain why the VAS-M is showed concurrent validity with the SMS-PA but maybe not the SMS. The participants probably understood the SMS-PA better than they understood the SMS. While not asked, no participant spoke about or mentioned any meditation or mindfulness experience. Meanwhile, several participants reported being confused by the wording of SMS items and wondering why the items "seemed to be asking the same thing." The SMS items may seem otherworldly or intangible to people without meditation and mindfulness experience (e.g., "I actively explored my experience in the present moment"). In contrast, the six items about the physical experience of walking may have felt like concrete concepts that participants could connect to and rate on the SMS-PA. The items, such as "I was aware of how my body felt" and "I was in tune with how hard my muscles were working," are concepts that even people without meditation and mindfulness experience can probably understand. Concurrent validity was tested only in mostly undergraduate and graduate students from two public universities in the Western United States. To what extent participants had experience with meditation and mindfulness was unclear. Such experience may be important for getting valid responses on the SMS, which in turn may affect the relationship between VAS-M and SMS scores. While there are no data yet to support this claim, maybe there would be evidence to support the VAS-M being concurrently valid with the SMS when administered to a sample that has meditation and mindfulness experience. It should not yet be concluded definitively that the VAS-M is valid or invalid. The concurrent validity of the VAS-M should be evaluated in other populations, such as among people with meditation and mindfulness experience. The VAS-M should also be evaluated in other settings that vary by location (e.g., indoor, outdoor) and activity (e.g., exercise and non-exercise).

Another point worth discussing about the concurrent validity of the VAS-M is how those scores responded similarly to the SMS scores. Though the correlations between the two measures had wide 95% CIs, the mean scores of the two measures responded similarly from pre-sit to post-sit to post-walk. The lines on the line chart are nearly parallel. The mean VAS-M scores were consistently lower than the mean SMS scores, and this makes sense because the maximum scores on the VAS-M and SMS are 100 mm and 105 AU, respectively. Based on the present dissertation, it seems that changes in VAS-M scores over time reflect changes in SMS scores over time when scores from both scales are collected simultaneously. Notably, the VAS-M scores but not the SMS scores were significantly greater at post-walk than their respective post-sit scores. This finding could mean the VAS-M is more sensitive than the SMS to acute changes in state mindfulness. Whether this characteristic is useful or indicates oversensitivity is not clear and should be explored further. Readers are encouraged to weigh the correlations and line chart together to decide whether to use the VAS-M with or in lieu of the SMS.

Aside from concurrent validity, test-retest reliability should be considered when deciding whether to use the VAS-M with or in lieu of the SMS. Such consideration is informed by the present dissertation, which achieved its tertiary aim of assessing the test-retest reliability of the SMS, VAS-M, and SMS-PA. Before the present dissertation, the literature about the test-retest reliability of the SMS was seemingly limited to the two studies mentioned earlier (Andrade et al., 2019; Tanay & Bernstein, 2013). The authors of those studies argued that their data demonstrated acceptable test-retest reliability of the SMS across 1–6 weeks. However, the threshold for reliability per Pearson's r was not stated.

When considering the threshold for reliability in the present dissertation, the SMS did not have acceptable test-retest reliability across the relatively shorter duration of approximately 24 hours. Two possible interpretations of this finding are given. The first interpretation is that, because reliability is a prerequisite for validity, the finding calls into question 1) whether the SMS is a valid measure of state mindfulness over approximately 24 hours, and 2) whether the SMS should be the criterion against which the VAS-M is compared. The second and preferred interpretation is that evaluating the test-retest reliability of state mindfulness measures is difficult because state mindfulness is a behavior-like, context-specific, and dynamic construct (Bishop et al., 2004; Ruimi et al., 2022; Tanay & Bernstein, 2013). Given these characteristics, state mindfulness can be expected to change across time and contexts. Though each participant in the present study arrived at the same location on day one and day two, other factors could have caused pre-sit, post-sit, and post-walk state mindfulness to differ between the days. These between-day differences may have made the SMS appear to have low test-retest reliability. In any case, the post-walk SMS scores were the closest to meeting the reliability criteria because there was an acceptable CV and ICC, but the lower-bound of the 95% CI was about 0.04 short of surpassing 0.70. This observation may be explained by the post-walk SMS scores being consistently higher than the scores at pre-sit and post-sit. One could interpret this finding in several ways. One interpretation is that taking the SMS three times in short succession causes people to respond more consistently over time. This interpretation it outwardly appealing because of the progressive increase in VAS-M reliability from pre-sit to post-sit to post-walk. Yet this interpretation seems unlikely because the SMS was less reliable at post-sit than pre-sit and rebounded to the be most reliable at post-walk. An alternative explanation is that GE completed for the same duration on two consecutive days raises someone to the same level of state mindfulness on both days. Perhaps GE elevates people to a set-point of state mindfulness unique to the individual. This interpretation is supported by the reliable VAS-M and SMS-PA scores at post-walk. Giving weight to this interpretation is the finding that there was no evidence to support the VAS-M's test-retest reliability until post-walk. Ultimately, the present dissertation contributes to the academic conversation about test-retest reliability of state mindfulness measures. This dissertation offers the first data on the test-retest reliability of the VAS-M and apparently also the SMS-PA. Researchers are advised to consider whether the data presented indicate that the SMS and VAS-M actually are not reliable or that merely they appear unreliable because state mindfulness is variable. Until the ambiguity is clarified, researchers should employ the SMS, VAS-M, and SMS-PA cautiously and justify the decision to do so.

Another way the present dissertation adds to the literature is by providing data on the test-retest reliability of the SMS in a diverse sample of U.S. adults. The two previous studies had homogeneous samples. In one study, the reliability analyses were conducted on a subsample of 41 Portuguese participants, approximately 88% female with a mean age of 38 ± 10 years (Andrade et al., 2019). In the other study, there were two Israeli cohorts, both approximately 65% female with mean age in the early thirties ± 10–12 years (Tanay & Bernstein, 2013). The present dissertation evaluated a sample that was younger, more racially diverse, and had near-equal proportions of males and females. These sample characteristics support drawing inferences about the test-retest reliability of the SMS, VAS-M, and SMS-PA among the broader population of U.S. university students. Universities are hubs for mindfulness research that often recruit university students for studies. Conclusions drawn from the present dissertation can inform the planning of future studies that use the scales in a sample of U.S. university students.

The practical conclusion supported by the present dissertation is that, if caveats are appreciated and reported transparently, the VAS-M may be used in lieu of the SMS before or after GE and in lieu of the SMS-PA after exercise. The logical flow of this conclusion is: 1) The data suggest the VAS-M may be concurrently valid with the SMS and SMS-PA. 2) Neither the VAS-M nor SMS had high test-retest reliability until after GE. 3) The VAS-M was equally unreliable before GE but more reliable afterward. 4) The VAS-M was similarly if not more reliable than the SMS-PA after GE.

Three known caveats to the conclusion given are that the VAS-M is less granular than the SMS and SMS-PA, using any unreliable scale is questionable, and that different analyses from those used in the present dissertation may lead to different conclusions about the scales' test-retest reliability. First, the VAS-M only indicates overall state mindfulness, whereas the SMS and SMS-PA indicate that construct as well as the mind and body dimensions. Researchers who want to evaluate the effects of GE on these dimensions would need to use the SMS and SMS-PA. In these instances, the VAS-M could be used in pilot testing as a mindfulness indicator. The VAS-M would reveal whether a particular GE increases state mindfulness but not how the GE affects state mindfulness granularly. Second, the unreliability of the VAS-M and SMS before GE may lead some researchers not to use either scale, at least not in repeated-measures designs across two days. The scales' unreliability justifies such hesitancy and a search for the time frames over which the VAS-M and SMS are reliable before GE. Third, the ICC and CV are just two of many plausibly appropriate analyses. Other analyses may lead to different conclusions about the scales' test-retest reliability. For example, in the present dissertation, bivariate correlations for the VAS-M scores between day one and day two were $r = 0.72$ (95% CI [0.41, 0.88], $p < 0.001$), $\rho = 0.72$ ([0.40, 0.88], $p < 0.001$), and $\rho = 0.83$ (0.61, 0.93], $p < 0.001$) at pre-sit, post-sit, and post-walk. Bivariate correlations for the SMS scores between day one and day two were $r = 0.76$ ([0.48, 0.90], $p < 0.001$), $r = 0.52$ ([0.13, 0.77], $p = 0.013$), and $\rho = 0.76$ ([0.47, 0.90], $p < 0.001$) at pre-sit, post-sit, and post-walk. And at post-walk, the bivariate correlations for the SMS-PA scores between day one and day two was $\rho = 0.87$ ([0.69, 0.95], $p < 0.001$). These point-estimates of r and ρ meet or exceed the point-estimates accepted as evidence of test-retest reliability for the SMS in published studies (Andrade et al., 2019; Ruimi et al., 2022; Tanay & Bernstein, 2013). In their article that introduced the SMS, Tanay and Bernstein (2013) considered the SMS relatively stable if scores taken in the same context correlated at $r > 0.60$. It is possible that the CV and ICC criteria are too conservative and stringent.

The author's research team has used the CV and ICC criteria for measuring test-retest reliability (Salatto, 2021). The team adopted these criteria after seeing them used in the wearable technology literature. That criterion has been used to measure the between-device reliability among devices of the same model (e.g., two Fitbit Surges) in measuring calories expended, distance traveled, heart rate, and step count during physical activity (Carrier et al., 2020; Evenson et al., 2015; Fokkema et al., 2017; Montes et al., 2020). Wearable technology such as fitness trackers and smartwatches certainly differ from measures by being objective devices that measure outcomes via sensors rather than being subjective scales that measure outcomes via participants' thoughts and feelings. Nonetheless, the summed outcomes of calories, distance, and step count are like the summed scores of the SMS and SMS-PA, making the ICC an appealing and suitable measure of test-retest reliability. The CV criterion was adopted from the fields of medical imaging and analytical chemistry, where precision and repeatability are paramount. One well-cited study deemed CVs < 10% excellent, 10–20% good, 20–30% acceptable, and >30%

poor (Aronhime et al., 2014). While CVs are quick and simple to calculate, it is unclear whether they are the best measure of test-retest reliability of the SMS, VAS-M, and SMS-PA. The benefit of using CVs, ICCs, and clear thresholds to evaluate the test-retest reliability of a measure is that there is a precedent for doing so. Another benefit of the established thresholds is that they allowed definite reject/fail-to-reject decisions on the null hypotheses about test-retest reliability. Without thresholds, decisions would have been made on merely the existence of associations between scores measured at two timepoints.

Ultimately, the author of the present dissertation hopes his findings move the conversation on test-retest reliability of the SMS forward. As the conversation continues, the next researchers to evaluate the SMS, VAS-M, and SMS-PA may choose to create Bland-Altman plots or calculate CVs, ICCs, Pearson's *r* (for parametric data), Spearman's *ρ* (for non-parametric data), mean differences, or mean absolute percentage errors between scores at different timepoints. Mean differences could be compared statistically via paired-sample *t* tests or Wilcoxon signed-rank tests for parametric and non-parametric data, respectively. Besides considering different measures, researchers should assess the test-retest reliability of the scales over different periods and identify the minimum washout period needed between repeated measurements.

Having explored the findings of the present study, it is worth discussing the limitations and potential critiques of the study. Only factors specific to Study 2 are discussed here. The limitations shared among Studies 2 and 3 and the limitations of the overall dissertation are discussed in Chapter 5.

The first factor is the risk of response bias. Participants may have believed that the researchers expected state mindfulness to increase across the study. If participants held this belief, they may have reported feeling more mindful at post-sit and post-walk despite not feeling so. The risk of response bias may thus explain the observed increases in SMS and VAS-M scores. To reduce the risk of response bias, the researchers were careful not to discuss any hypotheses around participants. State mindfulness was never discussed with the participants before the post-participation debriefing other than to explain the informed consent form. Researchers also stood away from participants as they completed the measures so as not to influence or pressure them. To reduce the risk that participants would remember their pre-sit answers and mark higher scores at post-sit and post-walk, the names of the measures were concealed, and the order of the measures was randomized at each timepoint. No researcher observed participants flipping back to pre-sit or post-sit responses or day one responses on day two at the TGT. To eliminate the risk of flipping back, researchers adapted the WP protocol by removing the day one responses from the binders before the start of day two. Despite the concealed measure names, randomized measure order, and 4–5 measures with 38–50 items at each timepoint, the effect of response bias could be substantial. The researchers could have reduced response bias better by including questionnaires about various other constructs besides state mindfulness. These other questionnaires would have obscured the study's purpose and made it difficult for the participants to try to please the researchers by how they answered the questions. While well-meaning, the researchers' choice to randomize the questionnaire order at each time point likely introduced an order effect on the data. In future studies, the researchers will add questionnaires to obscure the study's purpose, administer the questionnaires in a consistent order, and focus on only one dependent variable at a time.

The second study factor that readers may critique is the statistical analysis. The first criticism of the analysis may be about including outliers in the dataset when running the one-way

RM ANOVAs for SMS and VAS-M. One could argue that the non-parametric alternative, the Friedman test, would have been superior. The author of this dissertation considered the Friedman test but did not use it because one-way RM ANOVAs are fairly robust to a few outliers and slight non-normality. Running the ANOVAs without the outliers led to the same statistical decisions on the omnibus test and most of the pairwise comparisons. The exception is the pairwise comparison between post-sit and post-walk SMS scores. The mean difference between these timepoints became statistically became significant after removing the SMS outliers. The second criticism of the ANOVAs could be that the Greenhouse-Geisser method for estimating ε was not optimal. Some statisticians argue that the Huynh-Feldt method is better when $\varepsilon > 0.75$. Yet others argue that the Greenhouse-Geisser and Huynh-Feldt methods are equally appropriate, so the former was used in this study (Laerd Statistics, 2015; Maxwell & Delaney, 2003). A third criticism may be about using ω_p^2 instead of η_p^2. Though η_p^2 is reported more often than ω_p^2, η_p^2 is an estimate of the sample effect size based on within-subjects factor variability. This means it tends to overestimate the population effect size. The estimate of ω_p^2 was reported instead because is a better, less-biased estimate of the effect size in the population (Kinnear & Gray, 2010; Kirk, 2012; Laerd Statistics, 2015; Stevens, 2007).

The third factor that readers may critique is the author's error that caused SMS data to be lost at the TGT. Data loss is never welcomed, and it was unfortunate to no longer be able to test the effect of listening to music on state mindfulness. The author (primary investigator), his committee chair, and another faculty member inspected the measures before printing them and collating them in the binders. To avoid issuing flawed measures and the resulting data loss, the author will complete every measure before future data collections. He made this change before the study at the WP.

The third factor that readers may critique is the utility of the measure data. In other words, readers may ask, "So what? What do the data offer in terms of practical meaning?" The data offer the first glimpse into the minds of people who sat and walked in green space for 20 minutes in total. A sample of mostly undergraduate and graduate students reported feeling acutely more aware of their lived experience and nature's beauty. How infrequently in modern Western societies do people disconnect from their phones to sit and stroll undisturbed by other people and technology? If nothing else, the data contribute quantitative evidence from an experimental study that just 10–20 minutes in nature helped them re-center on the present moment and admire something bigger than themselves. Admittedly, quantitative data on state mindfulness are only concise, summative expressions of patterns of thought and feeling. Future studies should not rely exclusively on Likert-type scales to measure either construct. Many participants in studies of this dissertation said or wrote how positive sitting and walking in nature made them feel. These anecdotes were not solicited and are not admissible findings in this dissertation, but they point to the value of and need for mixed methods studies on GE.

Having discussed the findings, limitations, and conclusions of this study, it is wise to return to the two linked and recurrent questions that came to mind when contemplating the deeper meaning of Study 1 (the systematic review): 1) Does passing quiet solitary time in nature increase state mindfulness independent of deliberately focusing on the thoughts, feelings, emotions, and bodily sensations? 2) Can state mindfulness be measured more efficiently than is allowed by one of the prevailing measures of the construct, the SMS? The answers to questions one and two are "yes" and "probably yes," respectively. Excitingly, questions answered always raise new questions to be answered. Considering Study 2's findings, three questions became most important to answer in Study 3. The first question was whether passing quiet solitary time in nature increases connectedness to nature. Investigating whether connectedness to nature is affected by inactive immersion or GE is still uncommon. When investigated, the inactive immersion has been long, and the GE modality has been moderate-to-vigorous. Study 3 tested a relatively short inactive immersion and light-to-moderate GE modality. The second question was whether connectedness to nature could be measured more efficiently than is allowed by a prevailing measure of the construct, the Love and Care for Nature Scale (LCN). At the time the author had this question, he was not aware of any measure shorter and quicker than the LCN. He created the Visual Analog Scale-Nature (VAS-N) and reported the first data about its concurrent validity and test-retest reliability. The third and final question was whether connectedness to nature was associated with state mindfulness. The author's curiosity about a potential relationship stems from an intuitive yet unproven idea he had: to feel connected to nature while in green space, people must generate and maintain an awareness of their surroundings and inner experience. If this idea is true, then connectedness to nature and state mindfulness should be positively associated. All three questions are explored in the next chapter.

CHAPTER 4: DETERMINING THE EFFECTS OF SITTING AND WALKING IN GREEN SPACE ON CONNECTEDNESS TO NATURE AND TESTING THE CONCURRENT VALIDITY AND TEST-RETEST RELIABILITY OF A NOVEL MEASURE OF THE CONSTRUCT

4.1 Abstract

INTRODUCTION: It is unclear how brief inactive immersion followed by green exercise affects connectedness to nature. It is also unclear whether 1) the construct can be measured with a one-item scale or 2) how connectedness to nature relates to present-moment, non-judgmental awareness (state mindfulness). This study's primary aim was to determine whether 10 minutes of non-mindful sitting or traditional walking in green space affect connectedness to nature. The secondary aim was to test the concurrent validity of the one-item Visual Analog Scale-Nature (VAS-N) against the 15-item Love and Care of Nature Scale (LCN). The tertiary aim was to assess the test-retest reliability of the scales over approximately 24 hours. The quaternary aim was to determine if connectedness to nature and state mindfulness are related. **METHODS:** On two days separated by approximately 24 hours, participants (22 F, 20 M, 26 ± 9 y, 170 ± 9 cm, 70 ± 16 kg, 24 ± 5 kg·m^{-2}) arrived in green space, consented to participate, and completed the scales. Participants then sat for 10 minutes, completed the scales again, walked for 10 minutes, and completed the scales once more. Two 1 × 3 repeated-measures ANOVAs with post hoc paired-samples *t* tests were conducted to evaluate LCN and VAS-N scores among pre-sit, post-sit, and post-walk. Spearman's (ρ) correlations were conducted to compare the VAS-N scores with the LCN scores. Coefficients of variation and intraclass correlation coefficients were calculated to evaluate the 24-hour test-retest reliability of the LCN and VAS-N. The α-level for all analyses was 0.05. The effect size Cohen's *d* was calculated for all the paired-samples t tests and interpreted as follows: 0.41 = practical effect, 1.15 = moderate, and 2.70 = strong. **RESULTS:** The LCN scores increased from pre-sit to post-sit ($p = 0.003$, $d = 0.28$, not practically significant), from post-sit to post-walk ($p = 0.002$, $d = 0.20$, not practically significant), and from pre-sit to post-walk ($p < 0.001$, $d = 0.48$, practically significant). The VAS-N scores increased from pre-sit to post-sit ($p = 0.011$, $d = 0.37$, not practically significant) but not from post-sit to post-walk ($p = 0.438$, $d = 0.17$, not practically significant). The VAS-N scores also increased from pre-sit to post-walk ($p = 0.004$, $d = 0.50$). There were significant, strong, positive correlations between the VAS-N and LCN scores at each timepoint ($\rho = 0.71$–0.78, $p < 0.001$). Neither the LCN nor the VAS-N met the reliability criteria at any timepoint (CV < 10%, significant ICC > 0.70, and lower-bound of 95% CI of ICC > 0.70). **CONCLUSIONS:** Sitting and walking in green space for 20 minutes cultivated feelings of being connected to nature. There was evidence to support the VAS-N's concurrent validity with the LCN, but the questionable test-retest reliability of both scales warrant using them cautiously. New studies in diverse samples and settings are needed to draw definitive conclusions about the validity and test-retest reliability of the LCN and VAS-N when used in green space.

4.2 Introduction

As mentioned at the end of Chapter 3, few studies have evaluated whether acute inactive immersion or green exercise (GE) affect connectedness to nature. To the author's knowledge, only studies by his lab group have done so (Salatto, 2021; Salatto et al., 2021). In those studies, the inactive immersion was long (45 minutes) and the GE modalities were moderate-to-vigorous (uphill hiking and mountain biking over foothills). The novelty of the present study is that it tested a relatively short inactive immersion (i.e., sitting undisturbed at a trailhead) and light-to-moderate GE modality (self-paced traditional walking). The primary aim was to determine whether the inactive immersion or traditional walking in green space changed people's connectedness to nature. Connectedness to nature was measured via the Love and Care for Nature Scale (LCN) introduced by Perkins (2010). The LCN was chosen because there is published evidence to support its validity and internal consistency, and it measures the "explicitly affective or emotional aspect of the human-nature relationship" (Perkins, 2010). Emotions are both a critical aspect of the human experience and are objects to which a person may be mindful. The latter consideration is an immediate link between the LCN and SMS of Chapter 2. A long-term reason for choosing the LCN is that LCN scores correlate positively with environmentally altruistic behaviors. This associative relationship opens the door to future cause-and-effect research on GE and pro-environmental behaviors. For now, the present study could add value to the literature by revealing whether brief sitting and walking in green space helps people feel emotionally connected to nature.

Measuring connectedness to nature as part of field research on GE is still new and sometimes poses practical problems, namely inconvenience. This situation led the present dissertation's author to the second question of the present study: Could the same construct measured by the LCN be measured more efficiently? At the time the author had this question, he was not aware of any shortened version of the LCN. He created a novel and quick measure, the Visual Analog Scale-Nature (VAS-N). The secondary aim of the present study was to test the concurrent validity of the VAS-N with the LCN. The value of any new measure is determined not only by its validity but its reliability. The tertiary aim was to assess the test-retest reliability of both the LCN as the criterion measure and the VAS-N as the novel measure of connectedness to nature. If the study revealed evidence to support the VAS-N's concurrent validity and test-retest reliability, the scale would expedite the measurement of the LCN's underlying construct in field research. This contribution could benefit and propel future studies on GE.

The overlap between the GE literature and mindfulness literature is the topic of the fourth and final aim of the present study. The specific topic is whether connectedness to nature is associated with state mindfulness. The author's curiosity about a potential relationship stems from an intuitive yet unproven idea he had: to feel connected to nature while in green space, people must generate and maintain a mindful awareness of and attention to their internal experience and external setting. If this supposition is true, then connectedness to nature and state mindfulness should be positively associated. The idea motivated the quaternary aim to determine if connectedness to nature is related to state mindfulness while sitting and walking in green space. The finding in Study 2 that state mindfulness increased after sitting and walking in green space without mindfulness practices was novel and exciting. Such a finding reinforces the value of research on the relationship between mindfulness and GE. Furthermore, the finding justifies rigorous RCTs on mindfulness-based GE interventions (MBGEIs). The present study and its four aims were approved by the author's doctoral advisory committee. The following protocol was developed by the author and his committee chair.

4.3 Methods

This study was completed as per the following methods.

4.3.1 Participants

The sampling method, recruitment strategy, and participants of this study were the same as those explained in Section 3.3.1.

4.3.2 Procedures

This study was completed as per the methods in Section 3.3.2.

4.3.3 Statistical Analysis

The participants' demographic and anthropometric characteristics were summarized as frequencies, arithmetic means, and standard deviations (Google Sheets, Google LLC, Mountain View, CA). Proportions were calculated to determine how many participants had complete data for the LCN and VAS-N at each timepoint at each study site (Calculator, Apple Inc., Cupertino, CA). The null hypotheses for the primary, secondary, and tertiary aims were tested statistically with the appropriate parametric or non-parametric statistical analyses and, when warranted, post hoc analyses. All the statistical analyses for this study were run in IBM SPSS Statistics v28 (IBM, Armonk, NY, United States) with an α-level of 0.05. Unless stated otherwise, all data are presented as arithmetic means ± standard deviations ($\bar{x}$ + SD). The null hypotheses (H_0) and alternative hypotheses (H_1) for the primary aim were:

- H_{0A}: The population arithmetic mean LCN score does not differ among pre-sit, post-sit, and post-walk ($\mu_{pre\text{-}sit} = \mu_{post\text{-}sit} = \mu_{post\text{-}walk}$).
- H_{1A}: The population arithmetic mean LCN score at ≥ 1 timepoint differs from the population arithmetic mean of LCN at ≥ 1 other timepoints (i.e., at least two means differ).
- H_{0B}: The population arithmetic mean VAS-N score does not differ among pre-sit, post-sit, and post-walk ($\mu_{pre\text{-}sit} = \mu_{post\text{-}sit} = \mu_{post\text{-}walk}$).
- H_{1B}: The population arithmetic means VAS-N score at ≥ 1 timepoint differs from the population arithmetic mean of VAS-N at ≥ 1 other timepoints (i.e., at least two means differ).

The null hypotheses and alternative hypotheses for the secondary aim were:

- H_{0C}: The population arithmetic mean VAS-N score is not correlated with the population arithmetic mean LCN score at pre-sit, post-sit, and post-walk.
- H_{1C} The population arithmetic mean VAS-N score is correlated with the population arithmetic mean LCN score at pre-sit, post-sit, and post-walk.

The null hypotheses and alternative hypotheses for the tertiary aim were:

- H_{0D}: LCN scores are not reliable across approximately 24 hours.
- H_{1D}: LCN scores are reliable across approximately 24 hours.
- H_{0E}: VAS-N scores are not reliable across approximately 24 hours.
- H_{1E}: VAS-N scores are reliable across approximately 24 hours.

The null hypotheses and alternative hypotheses for the quaternary aim were:

- H_{0F}: The population arithmetic mean LCN score is not correlated with the population arithmetic mean SMS score at pre-sit, post-sit, and post-walk.
- H_{1F}: The population arithmetic mean LCN score is correlated with the population arithmetic mean SMS score at pre-sit, post-sit, and post-walk.

Before testing the hypotheses, a power analysis was needed to estimate the minimum sample size for adequate statistical power. The power analysis was based on the primary outcome, connectedness to nature (LCN scores). The author used LCN scores published by his research team. Salatto et al. (2021) had participants complete the LCN before and after a non-mindful, self-paced hike (8.6 ± 2.0 minutes) at the Thunderbird Gardens Trailhead (TGT). The effect size was $\eta_p^2 = 0.21$. The value of 0.21 was inputted into the "Effect size f" input parameter in G*Power 3.1 (G*Power 3.1, University of Düsseldorf, Düsseldorf, Germany) (Faul et al., 2007). The test family selected was "*F* tests," and the test selected was "ANOVA: Repeated measures, within factors." The type of power analysis was "A priori: Compute required sample size - given α, power, and effect size.” The “α err prob” inputted was 0.05, and the “Power (1−β err prob)” inputted was 0.80. The number of groups inputted was one with three measurements (for the 1 × 3 RM ANOVA). The default values of 0.5 for “correlation among rep measures” and 1 for “nonsphericity correction ε” were not changed. Based on these settings and the G*Power 3.1 calculations, the current study needed 37 participants for the design to achieve 80% statistical power with an α-level of 0.05.

The null hypotheses of the primary aim were tested statistically via two separate 1 × 3 RM ANOVAs. Before each ANOVA, the data were checked for alignment with the five assumptions of the one-way RM ANOVA (see Section 3.3.3).

The data from the no-music day at the TGT and day one at the Clark County Wetlands Park (WP) were analyzed together. The data met the assumptions for dependent and independent variables and were checked for outliers by inspecting boxplots of pre-sit, post-sit, and post-walk LCN and VAS-N scores. Outliers were considered data points greater than 1.5 box-lengths (interquartile range) from either edge of the box (the top edge was Q_1, and the bottom edge was Q_3). There were no LCN outliers at pre-sit or post-sit, but there was one outlier at post-walk. There were no pre-sit or post-sit VAS-N outliers, but there were two outliers at post-walk. None of the LCN or VAS-N outliers seemed to be errors in measurement or data entry. Rather, the outliers seemed to be genuinely unusual data points from the others in the dataset. Keeping the three outliers was not ideal for the validity of the ANOVAs, but there was no good reason to reject the outliers as invalid data points. After consulting his committee chair, the author of this dissertation kept the outliers for the ANOVAs.

Besides outliers, the data were checked for normality and sphericity. Normality was checked by inspecting histograms and running six separate Shapiro-Wilk tests: one each for pre-sit LCN, post-sit LCN, post-walk LCN, pre-sit VAS-N, post-sit VAS-N, and post-walk VAS-N. Pre-sit LCN was normally distributed ($p = 0.081$), but post-sit and post-walk LCN were not ($p = 0.042$ and $p < 0.001$, respectively). Pre-sit, post-sit, and post-walk VAS-N were not normally distributed ($p = 0.003$, $p = 0.003$, and $p < 0.001$, respectively). Despite the non-normality, the ANOVAs were conducted because they are robust to this assumption. The last assumption, sphericity, was checked via Mauchly's test. Both the LCN and VAS-N data violated the sphericity assumption ($p = 0.013$ and $p = 0.004$, respectively), so the respective ANOVAs were corrected to prevent the risk of a type 1 statistical error from inflating. The epsilon (ε) correction was estimated via the Greenhouse-Geisser method and used to adjust the degrees of freedom used in calculating the *p*-values for the ANOVAs. The effect sizes for the ANOVAs were calculated as ω_p^2 and interpreted in the same manner as in Chapter 3: 0.04 = "recommended minimum effect size representing a 'practically' significant effect [RMPE]," 0.25 = moderate, and 0.64 = strong (Cox et al., 2018; Ferguson, 2009). The ω_p^2 value for each 1 × 3 RM ANOVA was interpreted as the proportion of variance in the dependent variable (i.e., LCN or VAS-N scores) explained by the independent variable in the experimental design (i.e., time). The ANOVAs that yielded significant *F*-statistics were followed by post hoc pairwise comparisons, specifically paired-samples *t* tests that were each corrected by the Bonferroni adjustment. The effect sizes for the paired-samples *t* tests were Cohen's *d* and were interpreted in the same manner as in Chapter 3: 0.41 = RMPE, 1.15 = moderate, and 2.70 = strong (Ferguson, 2009).

The null hypotheses of the secondary aim were initially going to be tested statistically via three separate two-tailed Pearson's product-moment correlations (Pearson's *r*).

1. Pre-sit: VAS-N vs. LCN
2. Post-sit: VAS-N vs. LCN

3. Post-walk: VAS-N vs. LCN

Before each correlation, the data were checked for alignment with the five assumptions of Pearson's product-moment correlations (see Section 3.3.3).

As was done for the primary aim, the data from the no-music day at the TGT and day one at the WP were analyzed together. The LCN and VAS-N scores at pre-sit, post-sit, and post-walk met the assumptions of level of measurement, related pairs, and linear relationship, the latter of which was confirmed by inspecting scatterplots. Shapiro-Wilk tests showed that all the scores violated the normality assumption ($p < 0.05$) except pre-sit and post-sit LCN ($p = 0.492$ and $p = 0.103$, respectively). Boxplots showed that post-walk LCN and post-walk VAS-N violated the outlier assumption. Given how the data violated the assumptions, Pearson's product-moment correlation was not a valid analysis for comparing LCN and VAS-N scores at pre-sit, post-sit, or post-walk. All three correlations were calculated by using the non-parametric alternative to Pearson's product-moment correlation, Spearman's rank-order correlation (Spearman's rho; ρ). Every Spearman's rank-order correlation was two-tailed.

As another test of the concurrent validity of the VAS-N against the criterion LCN, a 1×3 RM ANOVA was run on the VAS-N data. This analysis was chosen to determine whether there were statistically significant differences in pre-sit, post-sit, and post-walk VAS-N scores. The VAS-N scores changing over time in the same direction as the LCN scores would be evidence of concurrent validity. As with the LCN data, the VAS-N data were checked for alignment with the five assumptions of the one-way RM ANOVA. Decisions on the assumptions were made as per the same rules as for the LCN data.

The null hypotheses of the tertiary aim were tested statistically by calculating absolute reliability as the coefficient of variation (CV; %) and relative reliability as the intraclass correlation coefficient (ICC). The CVs and ICCs were calculated from the day one and day two data from the WP. Six CVs and ICCs were calculated for the LCN and VAS-N, one for each scale for pre-sit, post-sit, and post-walk. At each timepoint, a measure was considered test-retest reliable if it met the a priori criteria of a CV < 10%, a significant ICC > 0.70 ($p < 0.05$), and the lower-bound of the 95% confidence interval (CI) of the ICC > 0.70.

The null hypotheses of the quaternary aim were initially going to be tested statistically via three separate Pearson's product-moment correlations.

1. Pre-sit: LCN vs. SMS
2. Post-sit: LCN vs. SMS
3. Post-walk: LCN vs. SMS

Before each correlation, the data were checked for alignment with the five assumptions of Pearson's product-moment correlations (see Section 3.3.3). As was done for the primary and secondary aims, the data from the no-music day at the TGT and day one at the WP were analyzed together. Outliers were observed in the scatterplots of LCN vs. SMS scores at pre-sit, post-sit, and post-walk, violating one of the five assumptions of Pearson's product-moment correlation. Consequently, Spearman's rank-order correlations were calculated (all two-tailed). To determine visually whether the LCN and SMS scores trended together, the results of the two separate 1 × 3 RM ANOVAs for the LCN and SMS scores were plotted together.

4.4 Results

This study extends and complements the second study (Chapter 3) by presenting different analyses of data from the same 42 participants. The proportions of participants who completed the LCN and VAS-N are reported for the overall sample (Table 16) and by study site (Appendix J, Tables 26–27). Participants were not asked why they missed or skipped measures.

Table 16. Proportion of participants with complete LCN and VAS-N data in the overall sample (*N* = 42).

Session	Timepoint	LCN	VAS-N
Session 1	Pre-Sit	41/42 (98%)	42/42 (100%)
	Post-Sit	41/42 (98%)	42/42 (100%)
	Post-Walk	40/42 (95%)	41/42 (98%)
Session 2	Pre-Sit	39/40 (98%)	40/40 (100%)
	Post-Sit	40/40 (100%)	39/40 (98%)
	Post-Walk	40/40 (100%)	40/40 (100%)

LCN: Love and Care for Nature Scale; VAS-N: Visual Analog Scale-Mindfulness. The denominator for Session 2 is 40 instead of 42 because one participant dropped out after Session 1 at each study site.

The 1 × 3 RM ANOVA warranted rejecting the null hypothesis that the population arithmetic means of LCN scores are equal at pre-sit, post-sit, and post-walk (Figure 17). Not all three of the arithmetic means were equal. At least one of the arithmetic means was statistically significantly different from at least one other arithmetic mean; $F(1.66, 62.90) = 21.36$, $p < 0.001$, $\omega_p^2 = 0.26$, a moderate effect. The post hoc pairwise comparisons showed that sitting and walking in green space increased participants' LCN scores from their baseline upon arriving at the trailhead or park. The mean LCN scores increased by 4.8 AU from pre-sit to post-sit (95% CI [1.5, 8.1], $p = 0.003$, $d = 0.28$, no practically significant effect) and by 8.2 AU from pre-sit to post-walk ([4.6, 11.9], $p < 0.001$, $d = 0.48$, a practically significant effect). The mean LCN scores also increased by 3.5 AU from post-sit to post-walk ([1.1, 5.8], $p = 0.002$, $d = 0.20$, no practically significant effect). These results show that sitting undisturbed near a trailhead or in a park for 10 minutes and then walking there for 10 minutes increased LCN scores.

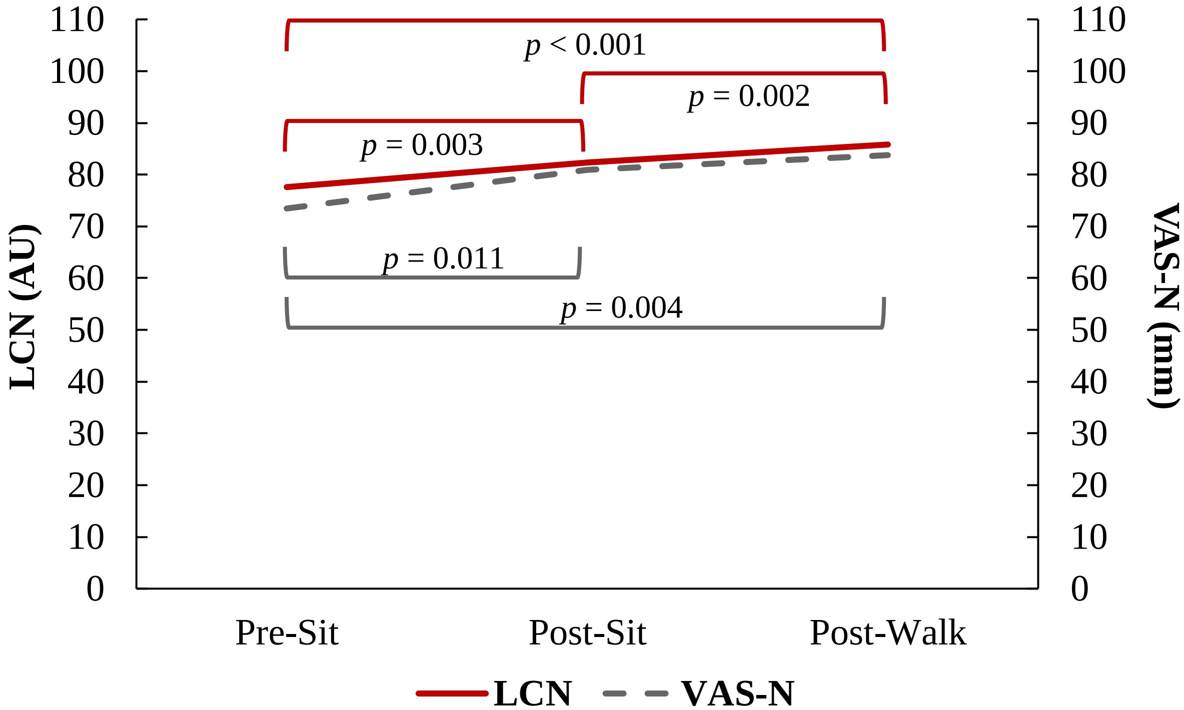

Figure 17. LCN and VAS-N scores from pre-sit to post-sit to post-walk. LCN: Love and Care for Nature Scale; VAS-N: Visual Analog Scale-Nature; AU: arbitrary units; mm: millimeters.

The VAS-N scores responded similarly to the LCN scores from pre-sit to post-sit to post-walk (Figure 17). The 1 × 3 RM ANOVA warranted rejecting the null hypothesis that the population arithmetic means of VAS-N scores are equal at pre-sit, post-sit, and post-walk. Not all three of the arithmetic means were equal. At least one of the arithmetic means was statistically significantly different from at least one other arithmetic mean; $F(1.59, 60.34) = 9.306$, $p < 0.001$, $\omega_p^2 = 0.12$, a practically significant effect). The post hoc pairwise comparisons showed that sitting and walking in green space increased participants' VAS-N scores from their baseline upon arriving at the trailhead or park. The mean VAS-N scores increased by 7.5 mm from pre-sit to post-sit (95% CI [1.5, 13.6], $p = 0.011$, $d = 0.37$, no practically significant effect) and by 10.3 mm from pre-sit to post-walk ([2.8, 17.8], $p = 0.004$, *d* = 0.50, a practically significant effect). The mean VAS-N score did not increase from post-sit to post-walk (*p* = 0.438, *d* = 0.17, no practically significant effect). These results show that the VAS-N scores track the LCN scores upon arrival at a trailhead or park and after sitting there undisturbed for 10 minutes. Though the mean LCN score significantly increased from post-sit to post-walk while the mean VAS-N score did not, the mean scores of both measures moved in the same direction (Figure 17).

The correlations between VAS-N and LCN at pre-sit, post-sit, and post-walk warranted rejecting the null hypotheses that the VAS-N and LCN are not correlated (Table 17 and Figures 18–20).

Table 17. Correlations between VAS-N and LCN at pre-sit, post-sit, and post-walk.

Timepoint	Coefficient(df) (95% CI)	Direction, Strength	*p*-value
Pre-Sit	$\rho(40) = 0.71$ (0.51, 0.84)	Positive, Strong	< 0.001
Post-Sit	$\rho(39) = 0.73$ (0.55, 0.85)	Positive, Strong	< 0.001
Post-Walk	$\rho(38) = 0.78$ (0.61, 0.88)	Positive, Strong	< 0.001

df: degrees of freedom; CI: confidence interval. All three correlations are two-tailed Spearman's rank-order correlations (Spearman's rho; ρ). The 95% CIs were estimated based on Fisher's *r*-to-*z* transformation, and the standard error was estimated by using Fieller, Hartley, and Pearson's formula in IBM SPSS Statistics v28.

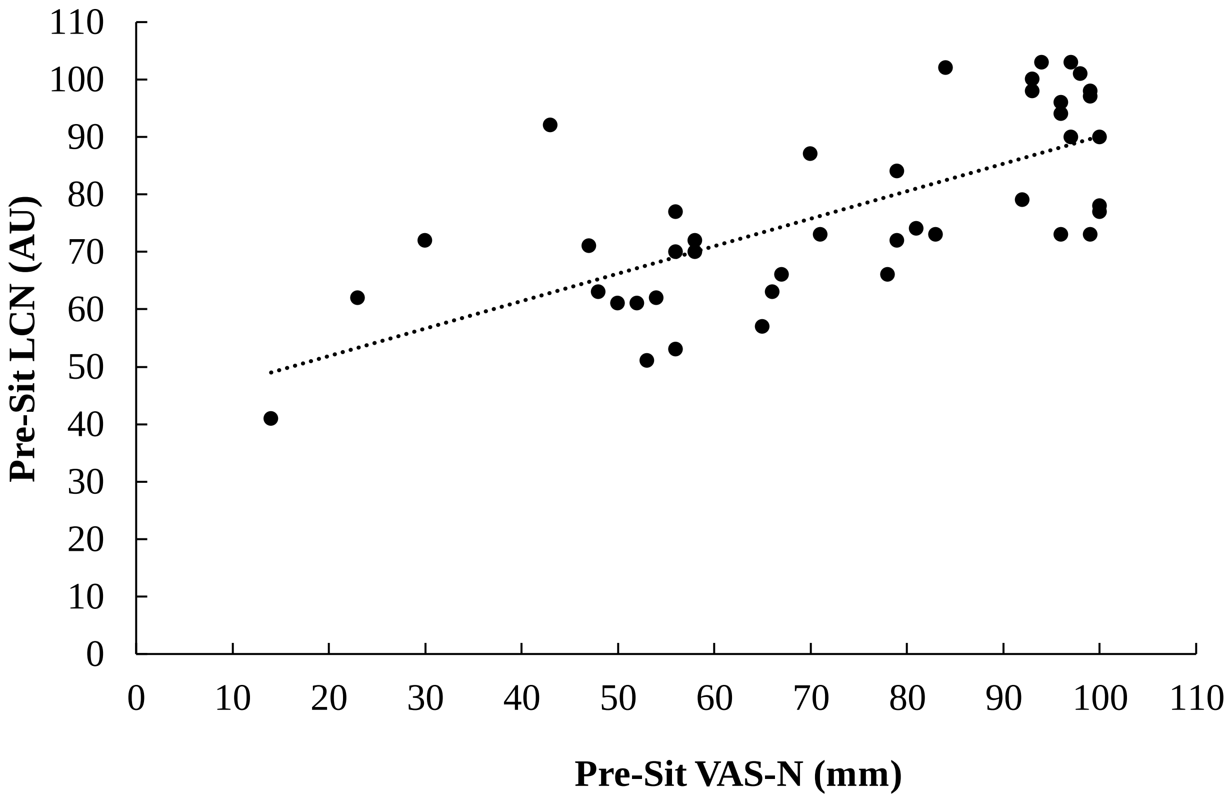

Figure 18. Scatterplot of pre-sit VAS-N and pre-sit LCN scores (N = 42). Maximum VAS-N and LCN scores are 100 mm and 105 AU, respectively. Line is the linear trendline. Neither the regression equation nor the coefficient of determination is shown because the data violated at least one assumption of Pearson's product-moment correlation. VAS-N: Visual Analog Scale-Nature; LCN: Love and Care for Nature Scale; millimeters; AU: arbitrary units.

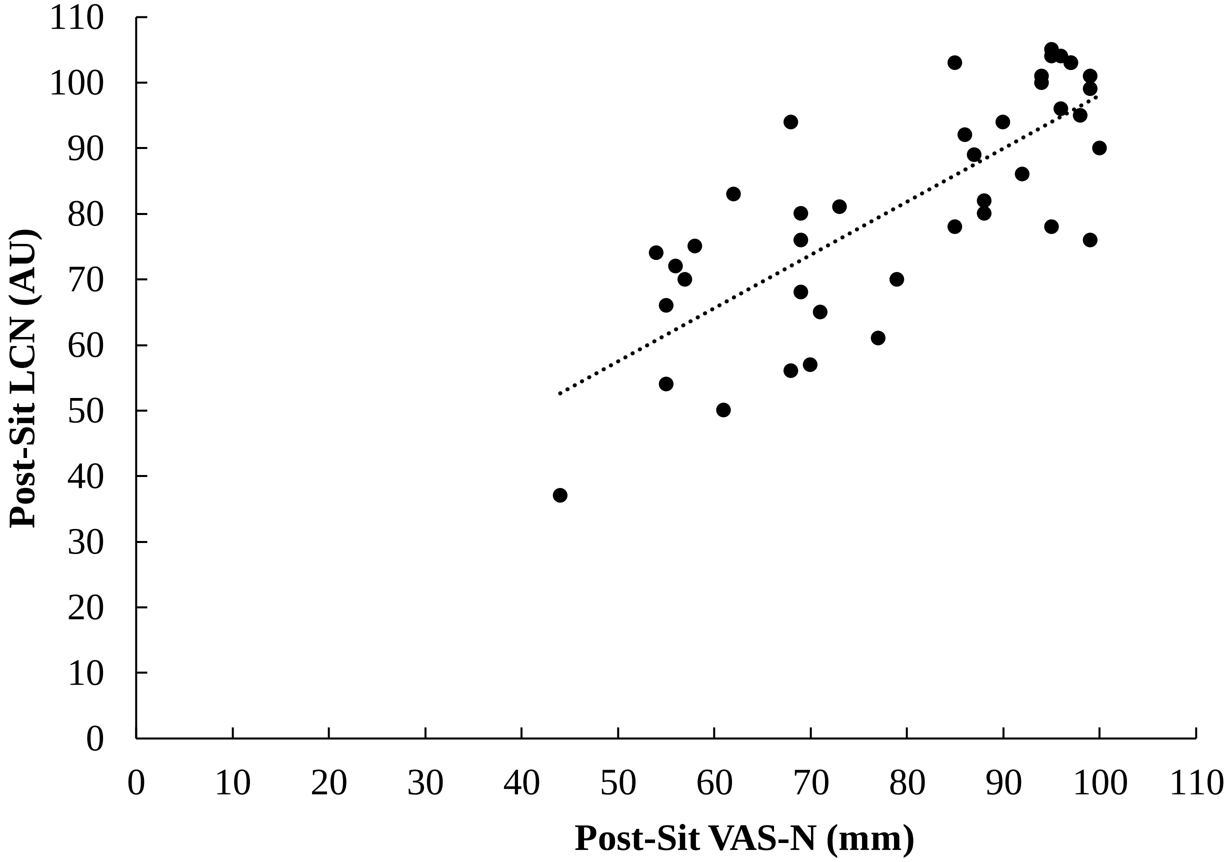

Figure 19. Scatterplot of post-sit VAS-N and post-sit LCN scores (*n* = 41). Maximum VAS-N and LCN scores are 100 mm and 105 AU, respectively. Line is the linear trendline. Neither the regression equation nor the coefficient of determination is shown because the data violated at least one assumption of Pearson's product-moment correlation. VAS-N: Visual Analog Scale-Nature; LCN: Love and Care for Nature Scale; mm: millimeters; AU: arbitrary units.

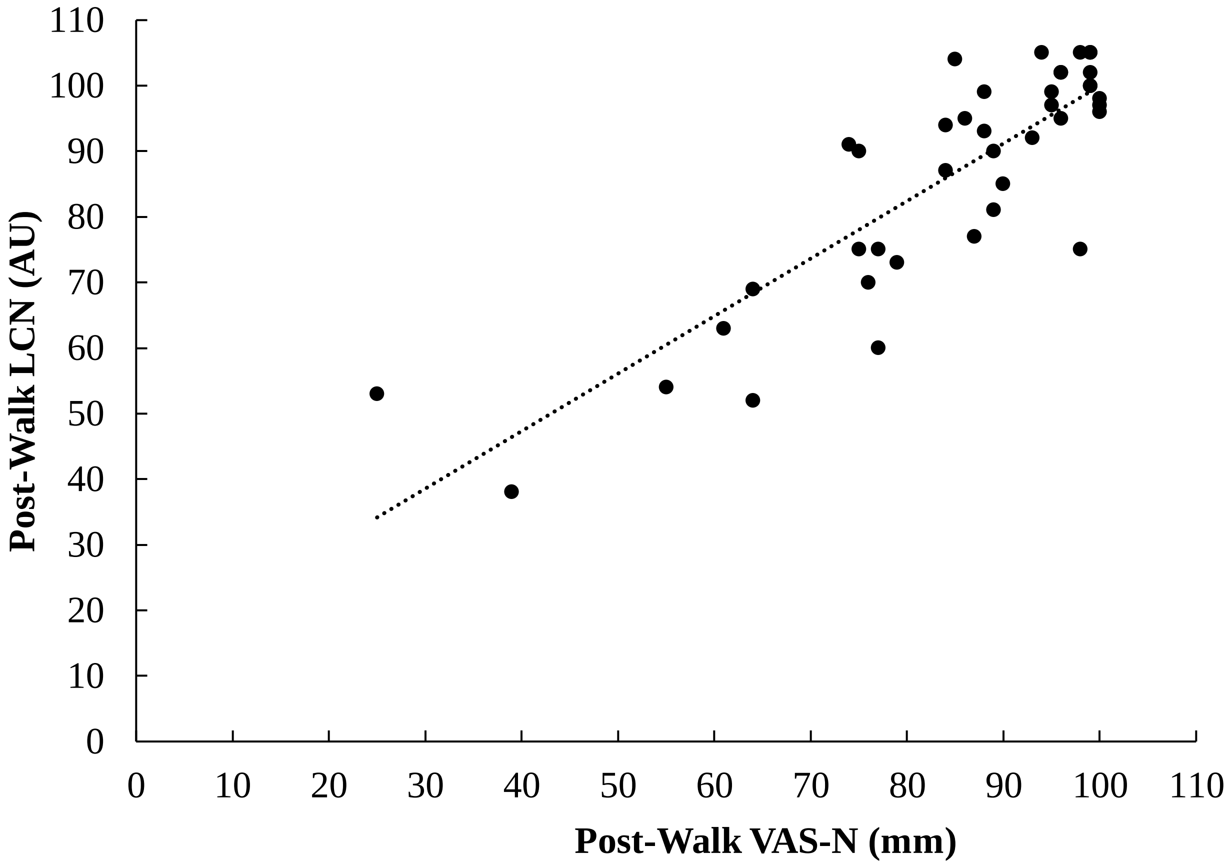

Figure 20. Scatterplot of post-walk VAS-N and post-walk LCN scores (*n* = 40). Maximum VAS-N and LCN scores are 100 mm and 105 AU, respectively. Line is the linear trendline. Neither the regression equation nor the coefficient of determination is shown because the data violated at least one assumption of Pearson's product-moment correlation. VAS-N: Visual Analog Scale-Nature; LCN: Love and Care for Nature Scale; mm: millimeters; AU: arbitrary units.

Based on the reliability criteria, there was no evidence to support the test-retest reliability of the LCN or VAS-N at any timepoint (Table 18).

Table 18. CVs and ICCs for the LCN and VAS-N.

	Pre-Sit		Post-Sit		Post-Walk	
	CV (%)	ICC	CV (%)	ICC	CV (%)	ICC
LCN	6.6	0.84 (0.65, 0.93) $p < 0.001$ Good	5.5	0.85 (0.68, 0.94) $p < 0.001$ Good	10.1	0.73 (0.44, 0.88) $p < 0.001$ Moderate
VAS-N	14.0	0.67 (0.37, 0.85) $p < 0.001$ Moderate	10.9	0.59 (0.23, 0.81) $p = 0.002$ Moderate	9.3	0.76 (0.50, 0.90) $p < 0.001$ Good

LCN: Love and Care for Nature Scale; VAS-N: Visual Analog Scale-Nature; CV: coefficient of variation; ICC: intraclass correlation coefficient (95% confidence interval [CI]). Interpretation of ICC based on 95% CIs per the guidance of Koo and Li (2016). Red, yellow, and green shading indicate, respectively, that the CV or ICC did not meet, was close to, or met the thresholds for test-retest reliability. The threshold for reliability per the CV was < 10% and per the ICC was a significant coefficient > 0.70 ($p < 0.05$) with the lower-bound of the 95% CI > 0.70.

The data warranted rejecting the null hypothesis of the quaternary aim that the population arithmetic mean LCN score is not correlated with the population arithmetic mean SMS score at pre-sit, post-sit, and post-walk (Table 19 and Figures 21–23). The SMS scores responded similarly to the LCN scores from pre-sit to post-sit to post-walk (Figure 24).

Table 19. Correlations between the LCN and SMS at pre-sit, post-sit, and post-walk.

Timepoint	Coefficient(df) (95% CI)	Direction, Strength	*p*-value
Pre-Sit	$\rho(31) = 0.21$ (−0.16, 0.52)	No Significant Correlation	0.246
Post-Sit	$\rho(32) = 0.53$ (0.22, 0.74)	Positive, Moderate	0.001
Post-Walk	$\rho(32) = 0.59$ (0.31, 0.78)	Positive, Moderate	< 0.001

LCN: Love and Care for Nature Scale; SMS: State Mindfulness Scale; df: degrees of freedom; CI: confidence interval. All three correlations are two-tailed Spearman's rank-order correlations (Spearman's rho; ρ). The 95% CIs were estimated based on Fisher's *r*-to-*z* transformation, and the standard error was estimated by using Fieller, Hartley, and Pearson's formula in IBM SPSS Statistics v28.

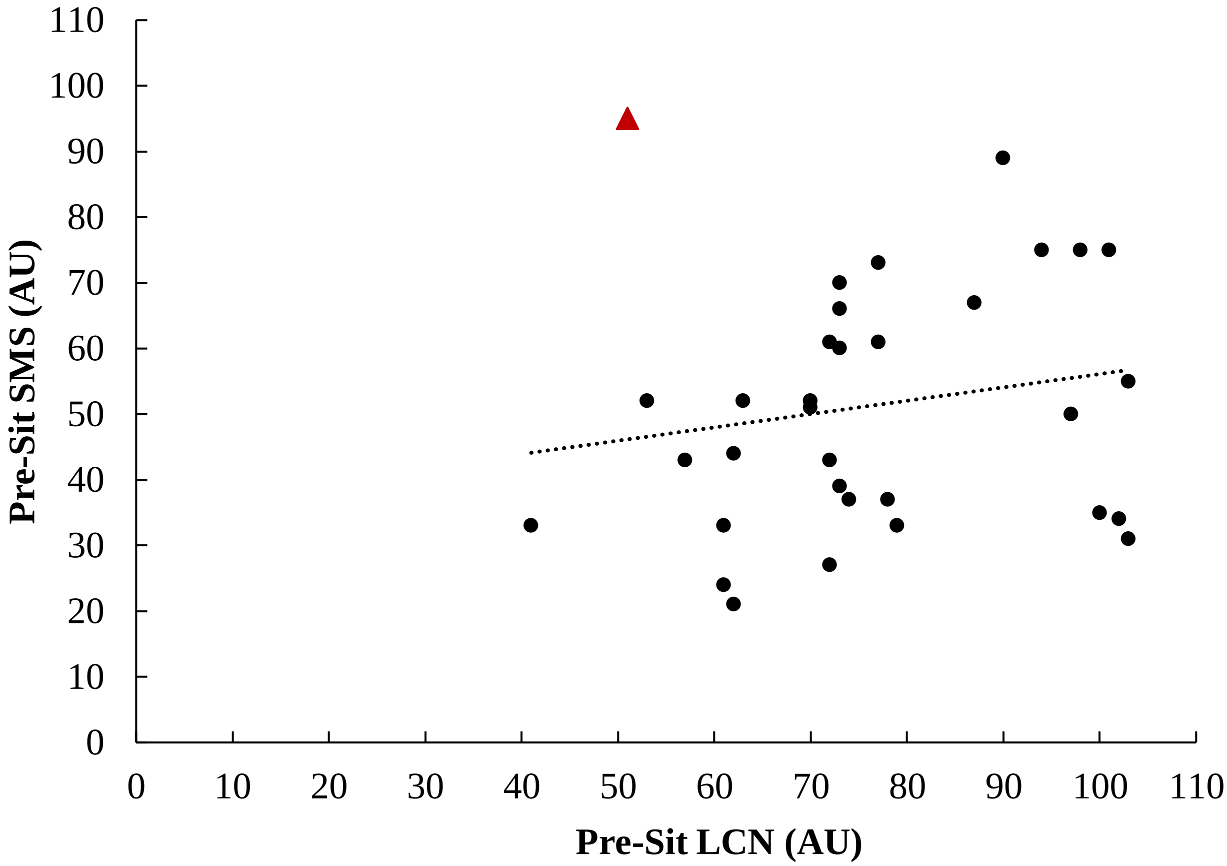

Figure 21. Scatterplot of pre-sit LCN and pre-sit SMS scores (n = 33). Maximum LCN and SMS scores are both 105 AU. Line is the linear trendline. Neither the regression equation nor the coefficient of determination is shown because the data violated at least one assumption of Pearson's product-moment correlation. The red triangle is the outlier that warranted calculating Spearman's ρ instead of Pearson's r. LCN: Love and Care for Nature Scale; SMS: State Mindfulness Scale; AU: arbitrary units.

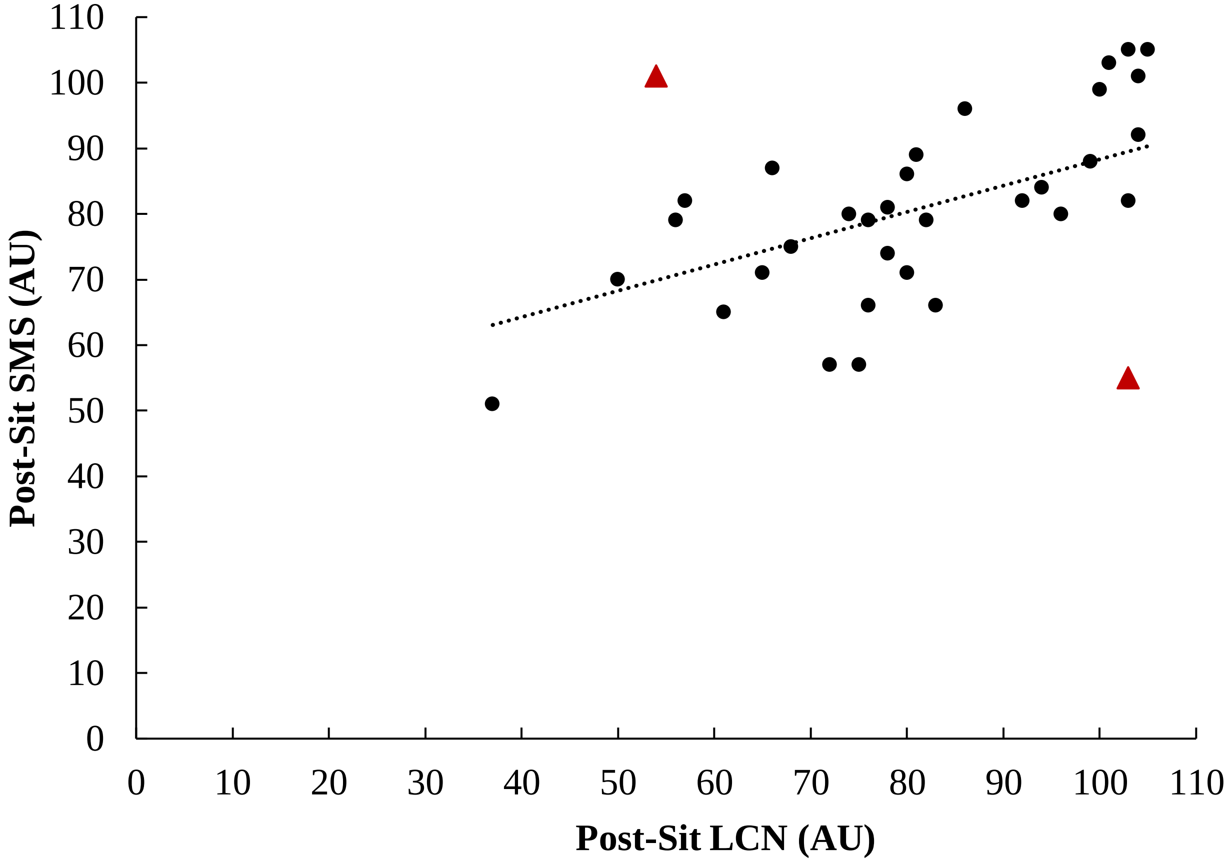

Figure 22. Scatterplot of post-sit LCN and post-sit SMS scores (*n* = 34). Maximum LCN and SMS scores are both 105 AU. Line is the linear trendline. Neither the regression equation nor the coefficient of determination is shown because the data violated at least one assumption of Pearson's product-moment correlation The two red triangles are the outliers that warranted calculating Spearman's ρ instead of Pearson's r. LCN: Love and Care for Nature Scale; SMS: State Mindfulness Scale; AU: arbitrary units.

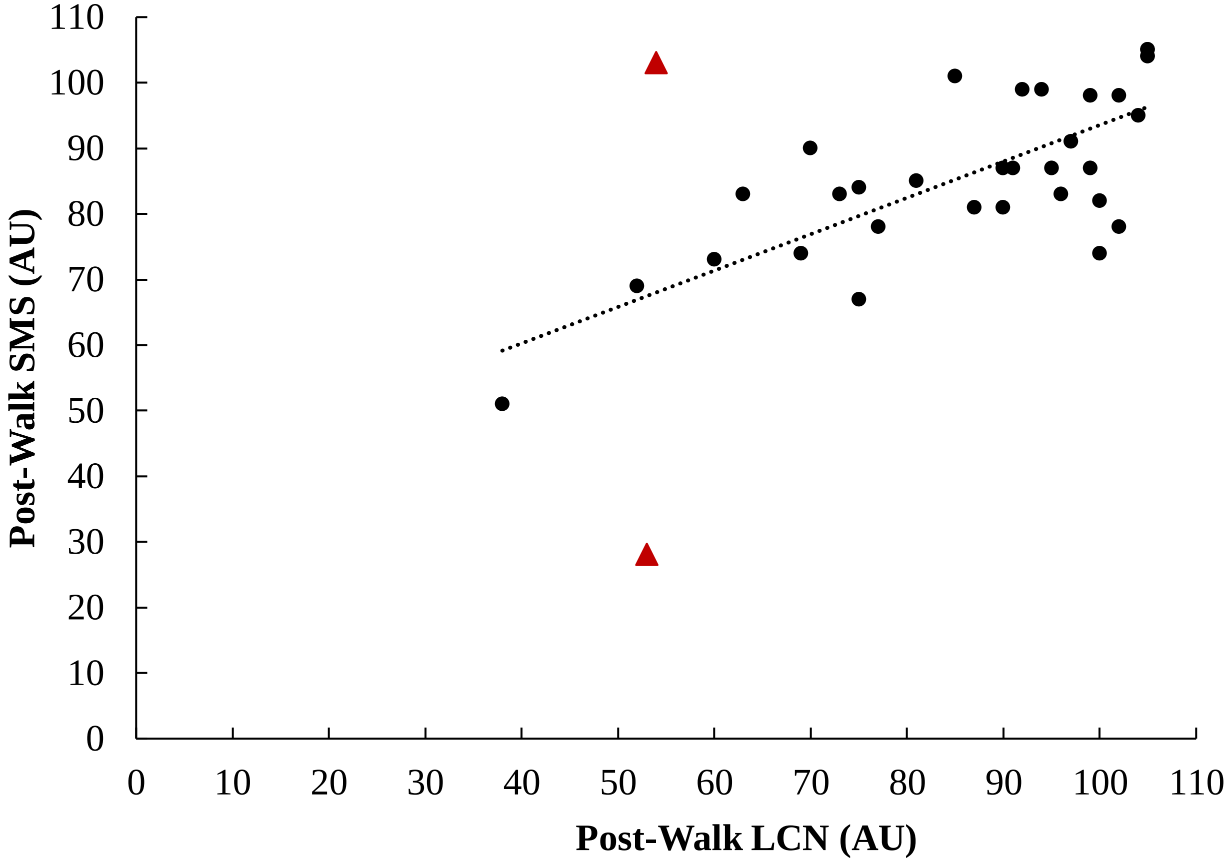

Figure 23. Scatterplot of post-walk LCN and post-walk SMS scores (*n* = 34). Maximum LCN and SMS scores are both 105 AU. Line is the linear trendline. Neither the regression equation nor the coefficient of determination is shown because the data violated at least one assumption of Pearson's product-moment correlation. The two red triangles are the outliers that warranted calculating Spearman's ρ instead of Pearson's r. LCN: Love and Care for Nature Scale; SMS: State Mindfulness Scale; AU: arbitrary units.

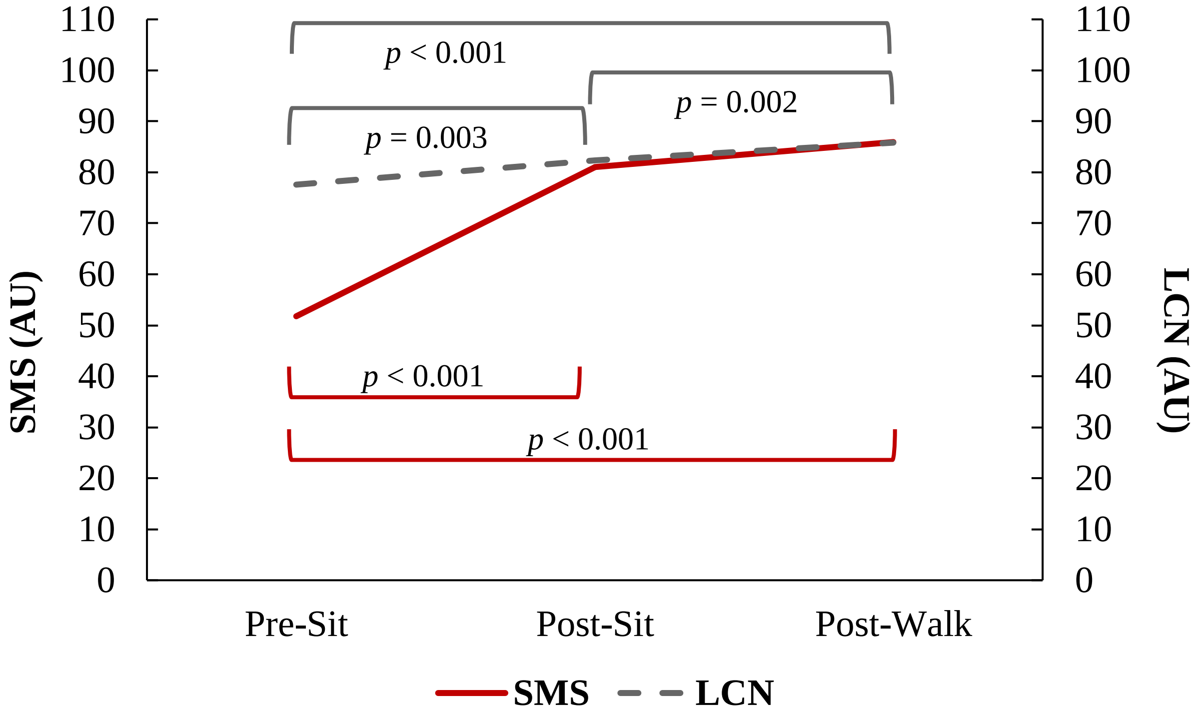

Figure 24. SMS and LCN scores at pre-sit, post-sit, and post-walk. SMS: State Mindfulness Scale; LCN: Love and Care for Nature Scale; AU: arbitrary units.

4.5 Discussion

This study had four aims. The primary aim was to determine whether sitting or walking in green space changed people's connectedness to nature. The secondary aim was to test the concurrent validity of a quicker measure of connectedness to nature, the VAS-N, against the criterion measure (LCN). The tertiary aim was to assess the test-retest reliability of the LCN and VAS-N, and the quaternary aim was to determine if connectedness to nature was related to state mindfulness while sitting and walking in green space.

The proportions of participants who responded completely to the LCN and VAS-N were nearly perfect. This outcome was likely supported by the same action that promoted high proportions of complete responses on the SMS and SMS-PA. The author provided the LCN and

VAS-N in tables in which the rows alternated white and gray. The near-full set of LCN and VAS-N data allowed the present study to achieve its four aims.

The study achieved the primary aim and showed that connectedness to nature increased after sitting and walking for 10 minutes each in green space. This finding extends the findings of an earlier study by the author's research team. Salatto et al. (2021) reported that sitting and hiking in green space increases connectedness to nature. The group had participants complete the LCN, sit for 45 minutes near the TGT, and then hike at a self-selected pace for 0.8 km (0.5 mi). The mean hike duration was 8.5 minutes. Despite the hike being uphill and vigorous exercise, LCN scores significantly increased from before sitting to after hiking (p = 0.035, η_p^2 = 0.21). In a separate study, Salatto (2021) had participants complete two out-and-back 1.6-km (0.99-mi) rides separated by 10 minutes. The participants rode over desert foothills on a trail in the Mojave Desert, so there was an uphill portion (44 m of elevation gain) and downhill portion. Given that the participants were novice mountain bikers, the rides were vigorous exercise. There was no significant interaction effect between trial (rides one and two) and time (pre- and post-ride; p = 0.171) or significant main effect for trial (p = 0.781), but there was a significant main effect for time (p = 0.014, η_p^2 = 0.29). The mean LCN score increased from 82.3 ± 2.1 pre-ride to 84.2 ± 2.1 post-ride (p = 0.021). The present dissertation corroborates the finding that acute GE increases connectedness to nature and adds new findings: seated immersion need not be long and GE intensity need not be vigorous to increase connectedness to nature. Connection increased after sitting in green space for 10 minutes (vs. 45 minutes) and walking in green space (vs. hiking uphill and mountain biking).

The ability to connect to nature quickly via brief seated immersion and light-intensity GE is a fresh finding. The finding is good news because it lowers two barriers to doing GE. The first barrier is the misconception that connecting with nature requires grandiose gestures and getting away for long excursions, but this is not true (Loewe, 2022). Connecting with nature requires only a small investment of time, such as 10 minutes of sitting, which may encourage people to visit green space more often. The TGT is only five minutes by car from Cedar City, UT. The WP is part of the surrounding city of Las Vegas, NV, and combines human-made structures with a natural landscape. For the people who are fortunate to have access to these areas and others like them, getting away for 10–20 minutes could help them feel connected to nature. The second barrier is that people who hear of GE research may assume that one can benefit only from hiking, mountaineering, mountain biking, rock climbing, and trail running. The present dissertation shows that light-intensity walking is enough to feel more connected to nature. People who are new to GE may find brief light walking on one day doable.

The present dissertation did not test for the possible effects of several days of immersion and exercise in green space, but a recent study did. Garza-Teran et al. (2022) had Mexican adults complete a two-day excursion at the Pinacate y Gran Desierto de Altar Biosphere Reserve in the Sonoran Desert in Mexico. The excursion involved hiking, watching wildlife, stargazing, and completing other activities across two days. Connectedness to nature was measured via a scale with 32 items, 15 of which were the 15 questions of the LCN. The mean per-item score out of seven on the LCN significantly increased from 6.05 to 6.23 arbitrary units ($p = 0.035$, $d = 0.62$). This finding aligns with the present dissertation’s finding that sitting and observing nature for 10 minutes increased LCN scores. The present dissertation took place in and just north of the Mojave Desert where the southwestern part of the Great Basin Desert begins. While the Sonoran, Mojave, and Great Basin Deserts differ in climate, ecology, and topography, each has brown and green colors. Collectively, the present dissertation and work of Salatto et al. (2021), Salatto

(2021), and Garza-Teran et al. (2022) show that connectedness to nature increases after small amounts of time immersed and exercising in desert green space.

Despite the statistical significance and moderate-to-large effect sizes, skeptical readers may question the practical meaning of the increases in connectedness to nature. After all, the mentioned studies showed that GE increased the mean LCN score by only 2.3% (Salatto, 2021) and 3.0% (Garza-Teran et al., 2022). The present dissertation showed a modestly larger increase of 10.6% from pre-sit to post-walk. The practical meaning of rising LCN scores may be revealed in innovative studies that explore the relationships among connectedness to nature and pro-environmental sentiments and behaviors. Nearly 12 years ago, the LCN's founder did this. Perkins (2010) analyzed whether LCN scores predict people's willingness to make pro-environmental sacrifices to lifestyle (e.g., "willingness to pay much higher prices for goods and services to protect the environment"). He also analyzed correlations between LCN scores and the frequency of behaviors (e.g., "How often do you vote for a candidate in an election at least in part because he or she is in favour of strong environmental protection/conservation?"). Scores on the LCN significantly predicted willingness to sacrifice for the environment ($p < 0.001$) and were significantly positively correlated with all seven of the pro-environmental behaviors explored ($r = 0.37$–0.51, $p < 0.001$). Correlation does not equal causation, so it cannot be argued that increasing LCN scores increases the frequency of pro-environmental behaviors. However, it is worth exploring whether GE is a gateway to being aware of and engaged in environmental protection.

It is also worth exploring if GE improves stress, anxiety, and depression at clinical and non-clinical levels. These conditions can arise and be exacerbated by feelings of disconnection and isolation. Connecting with nature may help. While the quality of evidence needs improvement, interacting with nature is a promising therapy for mental health conditions, particularly anxiety (Kotera et al., 2022; Lackey et al., 2021; Pretty et al., 2007; Tillmann et al., 2018). The literature would benefit from RCTs on GE that are rigorous, have long follow-up periods, and report clear outcomes of connectedness to nature and mental health.

Clear outcomes depend on valid and efficient measures. Testing whether the efficient VAS-N was concurrently valid with the LCN was the secondary aim achieved by the present study. The study offers superb initial evidence for the validity of the VAS-N. The VAS-N scores correlated significantly, strongly, and positively with the LCN scores and responded similarly to the LCN scores across the study. Researchers seeking quick field measurements of connectedness to nature may use the VAS-N in lieu of the LCN. A caveat is that the VAS-N is not granular like the LCN. Both the VAS-N and LCN reflect overall connectedness to nature, but only the LCN explains the aspects of that connectedness revealed by the LCN's 15 separate but related items. Another caveat is that, for a scale to be valid, it must also be reliable. Recall that the tertiary aim of the present dissertation was to assess the test-retest reliability of the LCN and VAS-N. Neither scale showed test-retest reliability in the present study. The LCN was the closest to meeting the reliability criteria at pre-sit and post-sit because the lower-bounds of the 95% CIs were about 0.02–0.05 below the cut-off.

As mentioned in the third chapter about the SMS and VAS-M, the CV and ICC may be too conservative and stringent for the LCN and VAS-N. Nonetheless, the lack of evidence to support the LCN's test-retest reliability per the CV and ICC criteria was surprising because Salatto (2021) reported the LCN had excellent test-retest reliability across two mountain biking rides separated by 10 minutes (CV = 2.3%, ICC [95% CI] = 0.94 [0.85, 0.98]). It is possible the LCN is reliable across repeated measures within 30–60 minutes but not approximately 24 hours.

Multi-day GE studies should use and interpret the LCN cautiously until researchers reach a consensus on the criteria for its test-retest reliability. The present dissertation is the first instance of assessing the VAS-N's test-retest reliability. The VAS-N did not provide a stable measure of connectedness to nature across approximately 24 hours.

Upon not finding evidence to support the LCN and VAS-N being test-retest reliable, it was hypothesized that this was caused by participants reporting greater connectedness to nature on day two than on day one. If true, this hypothesis would indicate that being in green space on day one explains some of the connectedness to nature reported on day two (i.e., carryover effects). To test the hypothesis, the day one LCN and VAS-N scores were compared with the day two scores. The hypothesis was unsupported because the LCN scores between the two days were similar at pre-sit (paired-samples *t* test; $p = 0.435$), post-sit (paired-samples *t*-test; $p = 0.523$), and post-walk (Wilcoxon signed-rank test; $p = 0.638$). The VAS-N scores between the two days were also similar at pre-sit (Wilcoxon signed-rank test; $p = 0.091$) and post-walk (Wilcoxon signed-rank test; $p = 0.070$) but were higher at post-sit on the second day (Wilcoxon signed-rank test; $p = 0.029$). Collectively, these data show that approximately 24 hours washed out transient increases in connectedness to nature before the second session started. The unreliability of the scales was not caused by carryover effects. As research on GE grows and longer interventions are explored (e.g., chronic exposure to nature via GE), studies should assess the test-retest reliability of the LCN and VAS-N across longer periods. It is also important to discern the washout period needed between measurements.

The quaternary aim achieved by the present dissertation was determining if connectedness to nature is related to state mindfulness while sitting and walking in green space. Connectedness to nature and mindfulness have been described as interconnected (Van Gordon et al., 2018). A field that explores this interconnection is ecopsychology (Wolsko & Lindberg, 2013). In the field, research has focused predominantly on the statistical relationship between connectedness to nature and trait mindfulness. A meta-analysis reported the weighted effect size (*r*) of the relationship between the two constructs as 0.25 (Schutte & Malouff, 2018). The present dissertation is novel because it focuses on the relationship between connectedness to nature and state mindfulness. Unlike trait mindfulness, state mindfulness is considered temporary and modifiable. The novel finding of the present dissertation was that connectedness to nature and state mindfulness were correlated after sitting and walking in green space. The participants who reported the greatest connectedness to nature tended to report the greatest state mindfulness. The point-estimates of the coefficients (ρ = 0.53 and ρ = 0.59) were nearly double the weighted effect size reported for connectedness to nature and trait mindfulness (Schutte & Malouff, 2018). A good next step is determining if people connect more to nature after beginning a mindfulness practice. Mindfulness practices that increase trait mindfulness and the frequency and duration of periods of state mindfulness may be shown to help connect with nature.

It should be noted that cause and effect cannot be inferred from correlations, but the correlations suggest that being in nature increases state mindfulness. This speculative conclusion is drawn rather than its inverse (state mindfulness increases connectedness to nature) because participants in the present dissertation were given a nature intervention, not a mindfulness intervention. Additionally, across the intervention, LCN and VAS-N scores increased only modestly while SMS and VAS-M scores increased considerably. Being in nature thus seems to induce state mindfulness, a hypothesis that was confirmed in Study 2. That finding aligns with a meta-analysis that reported the effects of nature-based mindfulness interventions on physical, psychological, and social functioning (Djernis et al., 2019). Across 21 studies, the pooled effect of the interventions on state mindfulness compared to the control conditions was medium; Hedges' $g = 0.62$, 95% CI [0.41, 0.83], $p < 0.001$. However, the studies' interventions varied by duration (from one session to weeks) and were mixed bags of solo and group work, GEs, and informal and formal mindfulness practices. The present dissertation is a unique contribution to the literature because, unlike most of the past studies, it measured the effects of a delineated bout of acute GE: 10 minutes of sitting and 10 minutes of walking while only using a cellphone to manage timers. The intervention's other parameters were also well-defined. The participants completed the intervention alone, and the intervention omitted mindfulness practices. These parameters kept the intervention free of extra elements that could confound the relationship between connectedness to nature and state mindfulness.

The author of the present dissertation encourages new studies on green immersion and GE that test narrow, well-defined interventions. Well-defined, narrow interventions are not a waste of resources because they allow researchers to identify the effects of specific activities. Combining mindfulness practices with GE may create a synergistic activity (mindful GE). When testing mindful GE, it is difficult to ascertain the effects of the GE and mindfulness components. High-quality RCTs can ameliorate this issue if the trial has one arm each for mindful GE, GE, non-exercise mindfulness practice, and waitlist. In addition to the types of studies already described, other studies should compare GE with other mindfulness interventions to determine their relative capacities to increase state mindfulness. Sitting and walking in green space could be compared to verbal instructions, written scripts, audio recordings, and guided meditations. Also worth exploring is the relative duration that state mindfulness remains elevated after each intervention. Studies on these topics would reveal which interventions increase state mindfulness the most by magnitude and duration. Such information would help people compare the interventions and choose those that fit their goals, lifestyle, and mindfulness practice.

The findings and ideas for future studies have been discussed, so it is important to turn to the present study's limitations. The study's design shares many of the same limitations as Study 2. However, Study 3 was better because the responses from day one were removed before day two. This action likely reduced the risk of recall bias in the test-retest design by keeping participants from viewing their earlier responses when giving later responses (e.g., looking at day-one responses when responding on day two). Despite improvements such as this one, elements of this study may be critiqued. Only elements of this study are discussed here. Limitations shared among Studies 2 and 3 and the limitations of the overall dissertation are discussed in Chapter 5.

The first element that may be critiqued is the risk of response bias, for the same reasons discussed in Chapter 3. Additionally, the high baseline connectedness to nature reported by the overall sample, especially by the TGT subsample, may have predisposed them to report that sitting and walking in nature made them feel more connected to it. Several participants at the TGT verbally expressed their love of nature and that they hiked and ran on trails regularly. This study cannot explain whether or to what degree sitting and walking in nature affects connectedness to nature in people who would not volunteer for a study about passing time in nature. In this population, the effects may be similar or attenuated. The effects may be greater in a population that has a lower baseline connectedness to nature. Determining which of these alternatives is true will require studies that recruit people who would not ordinarily sit and walk in nature. Nonetheless, this study reports the data of 42 participants who completed the protocol.

The increases in LCN scores from pre-sit to post-sit to post-walk justify continuing to explore the effects of short periods in nature on connectedness to it.

The second element that may be critiqued is the practical meaning of the increases in LCN and VAS-N scores beyond their statistical significance and effect sizes. The reader may ask if a 5-AU increase on the LCN or a 7.5-millimeter increase on the VAS-N from pre-sit to post-sit matters, or whether the smaller increment of 3.5 AU on the LCN from post-sit to post-walk matters. Admittedly, this study by design could not clarify the perceptual or physiological importance of the increases to the person. This information could be captured better with studies that collect more outcomes and employ a mixed-methods research design. Ideally, these studies would measure acute changes in physiological markers of cardiovascular health and relaxation, such as resting and recovery respiratory rate, heart rate, blood pressure, and heart-rate variability. The studies would also employ a wider range of questionnaires that include open-ended questions about acute changes in feelings of stress, anxiety, and depression and sentiments toward returning to nature or engaging in environmental activism. The open-ended questions could ask participants to describe in their own words their feelings about nature and how their mental state changed across the time passed in green space. All these ideas are exciting opportunities for future studies, but that was not the purpose of the present dissertation. The best response for now to the critical question of what the increases mean is this: people reported feeling more connected to nature and ought to be trusted that they reported their genuine feelings. No participant reported an adverse outcome. Collectively, these observations suggest at the least that sitting and walking in nature has a net benefit on people (i.e., increased state mindfulness) even if that benefit is not perceived by the participants or intricately understood by the researchers.

The third element that readers may critique is whether the VAS-N is valuable, given that the LCN is well-cited in the literature and that 95–100% of participants completed it across both sessions. Though the LCN is used often and had excellent completion rates in the present dissertation, repeating the LCN alongside other measures three successive times in 30 minutes is cumbersome. Participants can mark a vertical dash on the VAS-N more simply than they can answer 15 questions on the LCN. The author of this dissertation speculates that a greater ease of answering may keep participants engaged and less likely to answer carelessly just to finish a mountain of measures. If this claim is true, the VAS-N may lead to valid and considered responses in cases where the participant may have answered the longer LCN mindlessly. Aside from easing the burden on participants, quicker measures are convenient for researchers to administer, quantify, and analyze. In the settings of field research, such as a trailhead or park, the VAS-N is easier and faster to explain than the LCN. Measuring the score is simpler too, requiring only a quick check via a ruler instead of summing 15 items with scores ranging from one to seven. This action, multiplied by hundreds of times is burdensome (e.g., 42 participants × 2 sessions × 3 timepoints = 252 summations for the LCN alone). One could argue for administering a digital version of the LCN via Qualtrics on participants’ phones or iPads issued by the researchers. Yet cellular and internet connections are notoriously unreliable on remote trails. Furthermore, obtaining enough iPads for studies with large samples is a financial hurdle not surmountable by many student and faculty researchers alike. Considering these ideas, the VAS-N is a practical measure of connectedness to nature that may prove more useful than the LCN in field studies with repeated-measures designs.

CHAPTER 5: OVERALL DISCUSSION AND CONCLUSIONS

The findings of each study were discussed in detail in the earlier chapters. This chapter offers a broader discussion about lessons learned and the studies' commonalities. The chief lesson learned is the value of conducting a systematic review before planning an experimental study. Unlike an unsystematic literature review, a systematic review is structured, transparent, and repeatable. These defining characteristics curb bias and provide a clear understanding of what is known and unknown about a given intervention in a specific population. Put simply, a systematic review enables researchers to identify and focus their attention on gaps in the literature. A desirable bonus is that the systematic review uncovers hidden side paths to unexplored areas. The systematic review in Chapter 2 did just this. Its purpose was to summarize the effects of meditative and mindful walking on mental and cardiovascular health in any population. The review was not limited to a specific population because of the nascency of the literature on meditative and mindful walking. Along the main path, the systematic review illuminated a fledgling field of research and a methodological issue affecting it. The field of research is mindful green exercise (MGE)—the overlap of mindfulness, exercise in nature, and connectedness to nature. The methodological issue affecting it is the uncertainty and variability in measuring state mindfulness and connectedness to nature. Investigating MGE and the measurement of state mindfulness and connectedness to nature became the focus of the experimental studies in Chapters 3 and 4. The systematic review in Chapter 2 not only answered a research question but led to new pertinent questions. Systematic reviews are valuable because they generate findings and thrust toward new studies.

The experimental studies in Chapters 3 and 4 may not have been planned or completed had the systematic review not been completed first. Both experimental studies achieved their respective aims. It is worth restating that neither study provided participants with a mindfulness intervention. Participants were not given instructions or audio scripts to guide them through a meditation or mindfulness experience. The purpose of this approach was to isolate the effects of sitting and walking in green space on state mindfulness and connectedness to nature. Had a mindfulness intervention been provided, parsing the separate effects of that intervention and being in green space would have been difficult. The author chose not to create a separate study arm to receive a mindfulness intervention because of his limited resources, namely expertise, money, and time. The author is not a trained mindfulness instructor. Paying for live instruction or audio scripts was a financial barrier. Time was a barrier because data were collected at one of the study sites with a partnering university during an academic semester. That timing limited the collaboration to a single weekend. The author is interested in the effects of mindfulness-based exercise interventions in green space. He encourages researchers to explore this potentially fruitful path.

In reflecting on the present dissertation, one finding stands out as a takeaway for a general, non-academic audience. No participants in either study reported adverse outcomes from sitting and walking in green space. While larger and more diverse samples are needed to clarify how these activities affect state mindfulness and connectedness to nature, one fact is already clear. Heading to a trail or park to sit or walk for 20 minutes or fewer is likely safe for people like the present dissertation's sample. These people are mostly young adults who are free from cardiovascular, metabolic, and renal diseases.

A central part of reflecting on the present dissertation is pondering its limitations. This process is critical to the author's growth as a professional researcher. The following paragraphs explain the dissertation's shortcomings and considerations to make the next study better. The first limitation of the overall dissertation is that the author conducted the data analyses and wrote Studies 1–3 independently with minimal peer-review of his work by his colleagues. This structure is required as part of the doctoral program to demonstrate the author's abilities to research, think critically, and communicate effectively via scientific writing. However, the lack of co-writing and peer-review threatens the validity of the dissertation because one person working in isolation is prone to errors, oversights, and typos. The probability of catching and correcting these mistakes would have been higher if the writing were collaborative. The author perceived the risk of these sorts of mistakes and made every effort to identify them via fact-checking, revising, editing, and proofreading. Nonetheless, some mistakes may have been missed. Readers should exercise caution when interpreting each study's results and discussion. The author will happily field any questions and requests for clarification he receives after the dissertation has been published.

The second limitation of the overall dissertation is that the author did not consult a questionnaire expert when developing the Visual Analog Scale-Mindfulness (VAS-M) or Visual Analog Scale-Nature (VAS-N). Doing so would have been a judicious move, given the expertise required and the complexities of developing valid and reliable questionnaires. The author did not recruit a questionnaire expert because of the speed with which he had to choose a new path during his doctoral program. After three semesters in the program, he had to change his doctoral advisor and thus the advisory committee chair, committee members, and focus of his dissertation. The author pivoted as quickly as possible to recover lost time and remain on track to graduate in four years. As he shifted his research focus to the measurement of state mindfulness and connectedness to nature, the author considered published visual analog scales. He also consulted his committee chair, who does not identify as an expert in questionnaire development, to build the first versions of the VAS-M and VAS-N tested in Studies 2 and 3. If the VAS-M and VAS-N are revised for future studies, the author will consult an expert in the creation of psychometric measures as well as psychometric data collection and analysis. The author will also give clearer instructions to participants prior to them completing the VAS-M or VAS-N. When responding to either VAS, a substantial proportion of participants (≥ 25%) marked circles or Xs instead of a single vertical dash on the horizontal line. Such responses created a dilemma among the present dissertation's author and his committee chair. The two individuals agreed on the imperfect solution of measuring such responses from the left anchor to the center of the circle and the intersection of the two lines creating the X. By not giving better instructions for completing the VAS, the author inadvertently introduced respondent error. The chosen method of measurement introduced slight measurement error. Fortunately, the typical distance from the center of a circle or X to where the corresponding vertical dash would have gone is probably small. To minimize the risk of the same error occurring in his next study, the author will provide better instructions for the VAS-M and VAS-N.

In addition to the other limitations discussed, there are several factors shared by Studies 2 and 3 that readers may critique. The first factor was how the author collected data on participants' race and categorized those participants accordingly. Asking participants self-reported race without asking for ethnicity or providing a list of standard choices was problematic and short-sighted. The approach was supposed to promote equity and inclusion in this dissertation and avoid boxing people into categories to which they did not feel they belonged. The problems became apparent when 1) some participants reported races not officially recognized as race categories and 2) some participants reported ethnicity as race. The accurate and officially recognized race and ethnicity categories to which these participants belong are unknown. Consequently, this dissertation is likely misreporting some participants' race as per published guidance (*Race and National Origin*, 2022). Another problem with the author's approach is that he grouped participants who reported White and Caucasian. It is unclear if these participants viewed themselves as belonging to the same race, and published guidance recommends avoiding the term *Caucasian* to refer to White people. The author also grouped participants who reported Hispanic and participants who reported Latino, but these terms do not represent identical categories. The terms are vague and not specific enough to distinguish people from different geographic origins (e.g., Brazil, Columbia, Honduras, Mexico, Spain, etc.). The author regrets the ignorance he had at the outset of the dissertation and will follow the National Institutes of Health's published guidance in his future studies.

The second factor shared by Studies 2 and 3 is the researchers' effort to minimize response bias. The author of the dissertation considered requesting IRB approval for a deception study that would have misdirected participants' beliefs about the purpose of the study. The protocol would have required participants to complete many more measures than just the measures about state mindfulness and connectedness to nature. The extraneous measures may have clouded the researchers' intentions and reduced the risk that response bias made participants report more state mindfulness and connectedness to nature over time. The author ultimately did not request approval for or run a deception study. He perceived a high probability of the application getting delayed during review by his institution's IRB. Another option the author considered was randomizing the order of every question of every measure. However, this feature was not included in the protocols of Studies 2 or 3 because it would have been excessively laborious for paper measures. The author used paper measures instead of Qualtrics or another survey software because Internet access was inconsistent at the Thunderbird Gardens

Trailhead (TGT) and Clark County Wetlands Park (WP). Given the study's sample sizes and 4–5 measures per timepoint per day per participant, hundreds to thousands of unique versions of the paper measures would have been needed. Future studies would benefit from using survey software that quickly and automatically randomizes the order of questions on each questionnaire for each participant at each timepoint. Research cell phones or iPads with access to a hotspot or satellite Internet would be needed, especially in mountainous areas such the TGT. Issuing these devices for the study would also create more equity by enabling financially disadvantaged students without their own devices to participate. Besides these changes, the author recommends against randomizing the questionnaire order and measuring two or more constructs of interest simultaneously. These aspects of the present dissertation likely introduced an order effect and confounding into the dataset.

Another factor shared by Studies 2 and 3 that readers may critique is that data from the TGT were combined with data from the WP. Readers may argue that the data should not be combined for two reasons: 1) the settings were different and 2) the TGT data were from participants' no-music day, which was day two for some participants. In contrast, all the WP data were from participants' day one. Combining TGT day-two data with WP day-one data does not invalidate the statistical analyses. Despite the different settings and days (one vs. two), the interventions at each study site were identical and occurred in green space. Moreover, approximately 24 hours elapsed between day one and day two at the TGT. This duration was enough to wash out the carryover effects of baseline connectedness to nature from pre-sit on day one to pre-sit on day two at the WP. Carryover effects of baseline state mindfulness from pre-sit on day one to pre-sit on day two are not a concern. State mindfulness is a construct that represents mindfulness in the present moment rather than a general tendency to be mindful. If a moment is conceptualized as a second, the number of new moments between day one and day two was 86,400 (24 hours × 60 minutes × 60 seconds). A final consideration regarding both the Love and Care for Nature Scale (LCN) and State Mindfulness Scale (SMS) data is that participants at the TGT were randomly allocated. The random allocation ensured half of the participants completed the no-music day first and the other half of participants completed the music day first. The proportions of TGT data from day one and day two were balanced.

A third factor shared by Studies 2 and 3 that readers may critique is the duration between the repeated measures for evaluating their test-retest reliability. Only approximately 24 hours elapsed between day one and day two. Test-retest reliability can be measured over longer durations, including several weeks. While this approach may be better than the approach taken in Studies 2 and 3, time constraints limited the duration to approximately 24 hours. The data collection at the TGT was a collaborative effort between two universities over one weekend in October during the Fall 2021 semester. Conducting the study then was a double-edged sword. On one edge, it being a weekend during the semester meant more students were around and able to complete both days of the study. On the other edge, the study had to fit inside 48 hours because the UNLV faculty and student researchers traveled from Las Vegas, NV, to Cedar City, UT, and needed to return in time to teach on Monday. Because of student availability and the variability of students' Monday–Friday schedules, the data collection at the WP was also held on a weekend in November of the same semester. Participants arrived at the same time on Saturday and Sunday. Given the designs of the data collections at the TGT and WP, Studies 2 and 3 can only clarify 24-hour test-retest reliability of the SMS, VAS-M, LCN, VAS-N, and State Mindfulness Scale for Physical Activity. To assess the test-retest reliability of these measures over longer durations, new studies will need longer-term access to a sample of participants.

Having discussed the studies' commonalities and limitations, it is time for the dissertation's final appraisal. The present dissertation can be appraised with a ground-level and high-level view. The purpose of the ground-level view is to check whether the dissertation achieved its overall purpose and specific aims. The present dissertation achieved its overall purpose to expand what is known about mindful exercise and green exercise (GE) and how state mindfulness and connectedness to nature are measured. The three specific aims were also achieved. First, Study 1 summarized the effects of meditative and mindful walking, irrespective of location, on mental and cardiovascular health. Studies 2 and 3 determined the effects of sitting and walking in green space on state mindfulness and connectedness to nature, respectively. In contrast to the ground-level view, appraising the work with a high-level view is more philosophical and concerns the long-term impacts of the study. Simply, "so what, and what now?" The "so what" has both inward-facing and outward-facing elements. The inward-facing element is that the dissertation challenged the author to think critically, face obstacles, and persevere despite those obstacles and self-criticism. As his largest professional endeavor to date, the dissertation forced the author to face his own ignorance on many topics, endure the resulting discomfort, and work harder than ever to learn what he did not know. The outward-facing element of the "so what" is that the dissertation revealed a stark reality: Studies on mindful exercise and GE are often poorly planned and executed, and they need to be better. The studies are burdened by severe methodological limitations and risks of bias that all but nullify their value and preclude meaningful inferences from the findings.

The chain of flawed studies must be broken by researchers convening and designing medical-grade RCTs. This means testing the effects of mindful exercise, GE, and MGE in trials adhering to the strictest requirements used for trials on medications and healthcare procedures.

Populations, interventions, comparators, outcomes, settings, and study designs must be decided and defined clearly before participant recruitment begins. Interventions should be delineated according to frequency, intensity, time, type, location, and emphasis (the FITTLE principle). Both passive and active control conditions are required. Participants should be blinded to their interventions and the interventions of other participants to the greatest extent feasible. The researchers responsible for collecting, analyzing, and interpreting data should be blinded to the interventions the participants receive. Every outcome that researchers state they will measure should be reported fully and transparently, leaving nothing out. Finally, the limitations of convenience sampling should be acknowledged when making inferences about interventions tested among a convenience sample. Studies that lack these elements only bloat the literature. Only studies that adhere to the methodological pillars of RCTs will benefit and propel the literature.

The literature on mindful exercise, GE, and MGE would also benefit from using mixed methods in the RCTs. Qualitative and quantitative researchers are notorious for viewing each other as belonging to separate camps. This view, besides stifling the best research, keeps researchers bitterly divided and uninterested in collaboration. The author hopes that researchers from each camp will realize they are not in camps after all. Rather, the researchers are on one of two sides of the same coin. Open conversations will cultivate good will and, hopefully, lead to collaboration to address current obstacles. A difficult obstacle is the validity and reliability of quantitative, Likert-type scales for measuring state mindfulness and connectedness to nature. The present dissertation revealed that both the criterion measures and novel measures pose methodological problems that call the data into question. Along with refining quantitative measures, it is worth asking participants to explain how they think and feel in their own words.

Structured and unstructured interviews give participants this opportunity. What is learned about participants' mindfulness, connectedness to nature, and health may be far more valuable than the time invested during the interviews. The point is not to replace all quantitative measures with qualitative ones. Rather, the point is that mixed methods RCTs can be more than the sum of their quantitative and qualitative parts. Mixed methods RCTs can help researchers learn as much as possible about interventions. Maximizing what is learned is a worthy cause of any study aimed at improving people's health.

Wanting to improve people's health is why the author of the present dissertation began the doctoral program. The program has culminated with the finished dissertation, which is also a steppingstone to the author's future research on MGE. The finished dissertation is extensive yet imperfect. In this way, it is like the typical human mind at any given moment. Both are vast yet limited by constraints known and unknown. Both are products of many moments strung together and people, places, and things that came before. And both are a trying to leave a good mark on the world.

APPENDIX A

This appendix contains the State Mindfulness Scale (SMS) and the published guidelines for its use.

State Mindfulness Scale

Usually participants are asked to describe what they were doing during this 15-minute period of time.
Depending on the study design and context of interest, researchers can change the instructions to probe a different period of time (e.g., 5, 10, 20 minutes).

Items are aggregated into two subscales (see table) or one total score.

Higher scores = higher level of mindfulness
Numeric responses: from *1 = not at all* to *5 = very well*

Instruction to be used:
Below is a list of statements. Please use the rating scale to indicate how well each statement describes your experiences in the past 15 minutes

		"mind" subscale	"body" subscale
1.	I was aware of different emotions that arose in me	×	
2.	I tried to pay attention to pleasant and unpleasant sensations	×	
3.	I found some of my experiences interesting	×	
4.	I noticed many small details of my experience	×	
5.	I felt aware of what was happening inside of me	×	
6.	I noticed pleasant and unpleasant emotions	×	
7.	I actively explored my experience in the moment	×	
8.	I clearly physically felt what was going on in my body		×
9.	I changed my body posture and paid attention to the physical process of moving		×
10.	I felt that I was experiencing the present moment fully	×	
11.	I noticed pleasant and unpleasant thoughts	×	
12.	I noticed emotions come and go	×	
13.	I noticed various sensations caused by my surroundings (e.g., heat, coolness, the wind on my face)		×
14.	I noticed physical sensations come and go		×
15.	I had moments when I felt alert and aware	×	
16.	I felt closely connected to the present moment	×	
17.	I noticed thoughts come and go	×	
18.	I felt in contact with my body		×
19.	I was aware of what was going on in my mind	×	
20.	It was interesting to see the patterns of my thinking	×	
21.	I noticed some pleasant and unpleasant physical sensations		×

Below is a list of 21 statements. Please use the rating scale (1-5) to show how well each statement describes your experiences in the past 10 minutes. Check only one box per statement.

	1 **Not at all**	**2**	**3**	**4**	**5** **Very well**
1. I was aware of different emotions that arose in me					
2. I tried to pay attention to pleasant and unpleasant sensations					
3. I found some of my experiences interesting					
4. I noticed many small details of my experience					
5. I felt aware of what was happening inside of me					
6. I noticed pleasant and unpleasant emotions					
7. I actively explored my experience in the moment					
8. I clearly physically felt what was going on in my body					
9. I changed my body posture and paid attention to the physical process of moving					
10. I felt that I was experiencing the present moment fully					
11. I noticed pleasant and unpleasant thoughts					
12. I noticed emotions come and go					
13. I noticed various sensations caused by my surroundings (e.g., heat, coolness, the wind on my face)					
14. I noticed physical sensations come and go					
15. I had moments when I felt alert and aware					
16. I felt closely connected to the present moment					
17. I noticed thoughts come and go					
18. I felt in contact with my body.					
19. I was aware of what was going on in my mind					
20. It was interesting to see the patterns of my thinking					
21. I noticed some pleasant and unpleasant physical sensations					

APPENDIX B

This appendix shows the Love and Care for Nature Scale (LCN).

Below is a list of 15 statements. Please use the rating scale (1-7) to show how much you agree or disagree with each statement. Check only one box per statement.

	1 Very strongly disagree	**2 Strongly disagree**	**3 Disagree**	**4 Neutral**	**5 Agree**	**6 Strongly Agree**	**7 Very strongly agree**
1. I feel joy just being in nature.							
2. I feel that closeness to nature is important for my wellbeing.							
3. When I am close to nature, I feel a real sense of oneness with nature.							
4. I feel content and somehow at home when I am in unspoiled nature.							
5. I feel a deep love for nature.							
6. I often feel emotionally close to nature.							
7. When I spend time in unspoiled nature, I feel that my day-to-day worries seem to dwindle away in the face of the wonder of nature.							
8. Protecting the wellbeing of nature for its own sake is important to me.							
9. I feel spiritually bound to the rest of nature.							
10. I feel a personal sense of interconnectedness with the rest of nature.							
11. I often feel a sense of awe and wonder when I am in unspoiled nature.							
12. I often feel a strong sense of care towards the natural environment.							
13. I need to have as much of the natural environment around me as possible.							
14. When in natural settings, I feel emotionally close to nature.							
15. I enjoy learning about nature.							

APPENDIX C

This appendix shows the State Mindfulness Scale for Physical Activity (SMS-PA) and the published guidelines for its use.

State Mindfulness Scale for Physical Activity (SMS-PA)

The State Mindfulness Scale for Physical Activity (SMS-PA) was developed based on Tanay and Berstein's (2013) State Mindfulness Scale. Tanay and Bernstein (2013) conceptualized mindfulness as both the self-regulation of attention to the present and an attitude of openness, acceptance and curiosity about mental and physical experiences. However, the items on physical experiences did not adequately capture the breath of physical experience during physical activity. In order to more accurately capture the physical experience, items were developed and tested to measure the extent to which a person attends to their physical exertion, muscular engagement or bodily movements. The SMS-PA was developed to include both the objects of mindfulness (i.e., physical and mental events) and the qualities of mindfulness (e.g., attention, awareness, openness). The SMS-PA is intended to assess the specific experience of mindfulness and is ideally completed immediately following participation in a physical activity.

The SMS-PA is a 12 item measure with six items assessing state mindfulness of the mind (i.e., thoughts and emotions) and six items assessing state mindfulness of the body (i.e., movement, body sensations, muscle engagement). The response scale is 0 – 4, with anchors of Not at all (0) and Very much (4). Initial evidence supports a bi-factor structure of the measure in which both a general state mindfulness factor and two specific factors (mind and body) are supported. The bi-factor structure supports the use of using a single score to capture overall state mindfulness, but also the use of two scores capture mindfulness of the mind and mindfulness of the body. See Cox, Ullrich-French, and French (2016).

Initial evidence also supports the use of the measure with youth ages ten and older (Ullrich-French, Cox, Cole, Cooper, & Gotch, 2017). The SMS-PA has also been adapted in Spanish (Ullrich-French, González Hernández, Hildago Montesinos, 2017).

Scoring:

State Mindfulness of the Mind - Items 1 – 6
State Mindfulness of the Body – Items 7-12

Citations for the SMS-PA

Cox, A., Ullrich-French, S., & French, B. (2016). Validity evidence for state mindfulness scale scores in a physical activity context. *Measurement in Physical Education and Exercise Science, 20*, 38-49. doi:10.1080/1091367X.2015.1089404

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Contact Anne Cox (anne.cox@wsu.edu) or Sarah Ullrich-French (sullrich@wsu.edu) for more information about the SMS-PA. Please use appropriate citations for the measure and we appreciate any updates or information about the use of the SMS-PA.

We are interested in what you just experienced during [insert activity]. Please indicate how much you experienced each of the following by circling one number.						
		Not at all	**A little**	**Moderately**	**Quite a bit**	**Very much**
1.	I was aware of different emotions that arose in me.	0	1	2	3	4
2.	I noticed pleasant and unpleasant emotions.	0	1	2	3	4
3.	I noticed pleasant and unpleasant thoughts.	0	1	2	3	4
4.	I noticed emotions come and go.	0	1	2	3	4
5.	I noticed thoughts come and go.	0	1	2	3	4
6.	It was interesting to see the patterns of my thinking.	0	1	2	3	4
7.	I focused on the movement of my body.	0	1	2	3	4
8.	I felt present in my body.	0	1	2	3	4
9.	I listened to what my body was telling me.	0	1	2	3	4
10.	I was aware of how my body felt.	0	1	2	3	4
11.	I noticed the sensations in my body.	0	1	2	3	4
12.	I was in tune with how hard my muscles were working.	0	1	2	3	4

We are interested in what you just experienced during walking. Below is a list of 12 statements. Please use the rating scale (1-5) to show how much you agree or disagree with each statement. Check only one box per statement.

	1 Not at all	**2 A little**	**3 Moderately**	**4 Quite a bit**	**5 Very much**
1. I was aware of different emotions that arose in me.					
2. I noticed pleasant and unpleasant emotions.					
3. I noticed pleasant and unpleasant thoughts.					
4. I noticed emotions come and go.					
5. I noticed thoughts come and go.					
6. It was interesting to see my patterns of thinking.					
7. I focused on the movement of my body.					
8. I felt present in my body.					
9. I listened to what my body was telling me.					
10. I was aware of how my body felt.					
11. I noticed the sensations in my body.					
12. I was in tune with how hard my muscles were working.					

APPENDIX D

This appendix contains the Visual Analog Scale-Mindfulness (VAS-M).

Please read the sentence:

> From moment-to-moment, I noticed and accepted my thoughts, feelings, bodily sensations, and environment without judging them.

How well does the sentence describe your experience over the last 10 minutes?

Not at all like my experience	Exactly like my experience

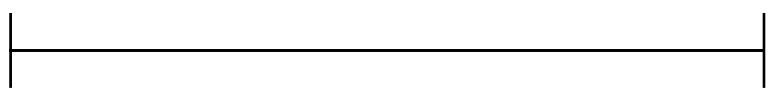
APPENDIX E

This appendix contains the Visual Analog Scale-Nature (VAS-N).

Please read the sentence:

> Interacting with nature brings me joy and makes me feel a sense of personal connection to and care for nature.

How well does the sentence describe your present feeling?



Very strongly disagree	Very strongly agree

APPENDIX F

Instructions for Researcher: Ask the participant the following questions and circle Yes or No.

1. Have you consumed any caffeine in the past six hours, such as from coffee, dark chocolate, energy drinks or tea?

    Yes/<u>No</u> (To participate, the answer must be No).

Ask questions 2-3 <u>ONLY</u> if this session is the session with music

2. Did you bring a phone that can play music and earbuds today?

    <u>Yes</u>/No (To participate, the participant must have a phone that can play music. If the participant does not have earbuds, lend them a pair.)

3. What music genre will you listen to while sitting and walking today?

    ____

4. For the <u>session without music</u>, instruct the participants:

    1) For the duration of the session, do not use your phone other than to set and turn off alarms
    2) Go off and choose a spot to sit alone away from the trailhead
    3) Once seated, set a 10-minute timer on your phone
    4) Once the timer goes off, fill out the questionnaires immediately (before walking back to the trailhead)
    5) After filling out the questionnaires, walk back to the trailhead to get further instructions
    6) Set a 5-minute timer for the walk away from the trailhead
    7) Walk away from the trailhead until the 5-minute timer goes off

8) Once the timer goes off, stop walking and set another 5-minute timer for the walk back toward the trailhead
9) Walk back toward the trailhead until the 5-minute timer goes off
10) Once the timer goes off, fill out the questionnaires immediately (even if not back to the trailhead yet)
11) After filling out the questionnaires, walk back to the trailhead to get further instructions

5. For the <u>session with music</u>, instruct the participants:
    1) For the duration of the session, do not use your phone other than to start your music and set and turn off alarms
    2) Do not change the music genre or skip songs during the study
    3) Go off and choose a spot to sit alone away from the trailhead
    4) Once seated, start listening to music, and set a 10-minute timer on your phone
    5) Once the timer goes off, fill out the questionnaires immediately (before walking back to the trailhead)
    6) After filling out the questionnaires, walk back to the trailhead to get further instructions
    7) Set a 5-minute timer for the walk away from the trailhead
    8) Walk away from the trailhead until the 5-minute timer goes off
    9) Once the timer goes off, stop walking and set another 5-minute timer for the walk back toward the trailhead
    10) Walk back toward the trailhead until the 5-minute timer goes off

11) Once the timer goes off, fill out the questionnaires immediately (even if not back to the trailhead yet)

12) After filling out the questionnaires, walk back to the trailhead to get further instructions

APPENDIX G

Instruct the participants:

1) For the duration of the session, do not use your phone other than to set and turn off alarms
2) Go off and choose a spot to sit alone away from the trailhead
3) Once seated, set a 10-minute timer on your phone
4) Once the timer goes off, fill out the questionnaires immediately (before walking back to the trailhead)
5) After filling out the questionnaires, walk back to the trailhead to get further instructions
6) Set a 5-minute timer for the walk away from the trailhead
7) Walk away from the trailhead until the 5-minute timer goes off
8) Once the timer goes off, stop walking and set another 5-minute timer for the walk back toward the trailhead
9) Walk back toward the trailhead until the 5-minute timer goes off
10) Once the timer goes off, fill out the questionnaires immediately (even if not back to the trailhead yet)
11) After filling out the questionnaires, walk back to the trailhead to get further instructions

APPENDIX H

Table 20. Self-reported biological sex and race among the TGT subsample (*n* = 19).

Classification	Count (*n*)	Percent (%)
Total	19	100
Females	6	32
Males	13	68
Intersex	0	0
African American or Black	1	5
Caucasian or White	15	79
Hispanic or Latino	2	11
Polynesian	1	5

TGT: Thunderbird Gardens Trailhead

Table 21. Self-reported biological sex and race among the WP subsample (*n* = 23).

Classification	Count (*n*)	Percent (%)
Total	23	100
Females	16	70
Males	7	30
Intersex	0	0
African American or Black	3	13
Asian	4	17
Caucasian or White	4	17
Hispanic or Latino	7	30
Mediterranean	1	4
Middle Eastern	1	4
Multi-Racial	3	13

WP: Clark County Wetlands Park. The percentages for the self-reported races do not sum to precisely 100% because of rounding.

Table 22. Age, height, mass, and body mass index at the TGT (*n* = 19).

Age (y)	Height (cm)	Mass (kg)	BMI ($kg \cdot m^{-2}$)
29.0 ± 11.7	173.4 ± 9.3*	74.8 ± 16.7*	24.6 ± 4.7*

TGT: Thunderbird Gardens Trailhead; y: years; cm: centimeters; kg: kilograms; BMI: body mass index; m: meters. Arithmetic mean ± standard deviation. *One participant's data were missed and not included in the calculation of the arithmetic mean and standard deviation.

Table 23. Age, height, mass, and body mass index at the WP (*n* = 23).

Age (y)	Height (cm)	Mass (kg)	BMI ($kg \cdot m^{-2}$)
23.5 ± 5.0	166.6 ± 7.0	65.3 ± 14.3	23.5 ± 4.4

WP: Clark County Wetlands Park; y: years; cm: centimeters; kg: kilograms; BMI: body mass index; m: meters. Arithmetic mean ± standard deviation.

APPENDIX I

Table 24. Proportion of participants with complete SMS, VAS-M, and SMS-PA data at the TGT (n = 19).

Session	Timepoint	SMS*	VAS-M	SMS-PA
	Pre-Sit	10/19 (53%)	19/19 (100%)	-
Session 1	Post-Sit	10/19 (53%)	19/19 (100%)	-
	Post-Walk	9/19 (47%)	18/19 (95%)	19/19 (100%)
	Pre-Sit	17/18 (94%)	18/18 (100%)	-
Session 2	Post-Sit	18/18 (100%)	18/18 (100%)	-
	Post-Walk	18/18 (100%)	18/18 (100%)	17/18 (94%)

SMS: State Mindfulness Scale; VAS-M: Visual Analog Scale-Mindfulness; SMS-PA: State Mindfulness Scale for Physical Activity; TGT: Thunderbird Gardens Trailhead. The denominator for Session 2 is 18 instead of 19 because one participant dropped out after Session 1. *Without the author's error, the Session 1 SMS proportions may have been pre-sit (17/19, 89%), post-sit (18/19, 95%), and post-walk (18/19, 95%). The author's error at the TGT was the only SMS question missed during Session 1 by seven participants at pre-sit, eight participants at post-sit, and nine participants at post-walk.

Table 25. Proportion of participants with complete SMS, VAS-M, and SMS-PA data at the WP (n = 23).

Session	Timepoint	SMS	VAS-M	SMS-PA
	Pre-Sit	22/23 (96%)	23/23 (100%)	-
Session 1	Post-Sit	23/23 (100%)	23/23 (100%)	-
	Post-Walk	22/23 (96%)	22/23 (96%)	21/23 (91%)
	Pre-Sit	21/22 (95%)	21/22 (95%)	-
Session 2	Post-Sit	22/22 (100%)	21/22 (95%)	-
	Post-Walk	22/22 (100%)	22/22 (100%)	21/22 (95%)

SMS: State Mindfulness Scale; VAS-M: Visual Analog Scale-Mindfulness; SMS-PA: State Mindfulness Scale for Physical Activity; WP: Clark County Wetlands Park. The denominator for Session 2 is 22 instead of 23 because one participant dropped out after Session 1.

APPENDIX J

Table 26. Proportion of participants with complete LCN and VAS-N data at the TGT (*n* = 19).

Session	Timepoint	LCN	VAS-N
	Pre-Sit	18/19 (95%)	19/19 (100%)
Session 1	Post-Sit	18/19 (95%)	19/19 (100%)
	Post-Walk	18/19 (95%)	19/19 (100%)
	Pre-Sit	18/18 (100%)	18/18 (100%)
Session 2	Post-Sit	18/18 (100%)	18/18 (100%)
	Post-Walk	18/18 (100%)	18/18 (100%)

LCN: Love and Care for Nature Scale; VAS-N: Visual Analog Scale-Mindfulness; TGT: Thunderbird Gardens Trailhead. The denominator for Session 2 is 18 instead of 19 because one participant dropped out after Session 1.

Table 27. Proportion of participants with complete LCN and VAS-N data at the WP (*n* = 23).

Session	Timepoint	LCN	VAS-N
	Pre-Sit	23/23 (100%)	23/23 (100%)
Session 1	Post-Sit	23/23 (100%)	23/23 (100%)
	Post-Walk	22/23 (96%)	22/23 (96%)
	Pre-Sit	21/22 (95%)	22/22 (100%)
Session 2	Post-Sit	22/22 (100%)	21/22 (95%)
	Post-Walk	22/22 (100%)	22/22 (100%)

LCN: Love and Care for Nature Scale; VAS-N: Visual Analog Scale-Mindfulness; WP: Clark County Wetlands Park. The denominator for Session 2 is 22 instead of 23 because one participant dropped out after Session 1.

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CURRICULUM VITAE

Dustin Wyatt Davis

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Education

Postdoc — Awarded by the University of Nevada, Las Vegas (UNLV). Doctoral Graduate to Postdoctoral Scholar Program.
Advisor: Dr. James Navalta — *Anticipated start in May 2023*

Ph.D. — In Progress at UNLV. Interdisciplinary Health Sciences.
Advisor: Dr. James Navalta — *Will graduate in May 2023*

M.S. — University of Central Missouri (UCM): Kinesiology
Advisor: Dr. Matthew Garver — *Graduated in May 2019*

B.S. — UCM: Physical Education (Emphasis in Exercise Science and Sports Nutrition) — *Graduated in May 2017*

Teaching Experience

UNLV

Graduate Teaching Assistant (Senior Lab Instructor), Kinesiology and Nutrition Sciences, 2019–2022
- KIN 223 Anatomy and Physiology I Lab (Fall 2019–Spring 2020, Fall 2021–Fall 2022)

Part-Time Instructor
- KIN 223 Anatomy and Physiology I Lab (Summer 2020, Summer 2022)
- KIN 224 Anatomy and Physiology II Lab (Summer 2022)
- KIN 391 Exercise Physiology Lecture (Summer 2021)

Guest Lectures
- KINE 4200 Seminar in Kinesiology, California State University, San Bernardino, "My Path in Higher Ed" (May 2022)

UCM

Graduate Teaching Assistant; Nutrition, Kinesiology, and Psychological Science, 2017–2019
- KIN 1206 Fitness for a Global Community (taught 1 semester)
- KIN 2900 Essentials of Personal Training (taught 4 semesters)
- KIN 4850 Assessment and Evaluation of Fitness/Wellness (taught 3 semesters)
- KIN 4870 Clinical Exercise Physiology (taught 2 supervised guest lectures)

Undergraduate Learning Assistant, Anatomy, 2016–2017

Tutor for Student Athletes, Athletics Program Development and Retention Services, 2015

<u>Research Experience</u>

UNLV

Advanced Doctoral Graduate Assistantship Completion Program Scholar, 2023–Present

Student Researcher, Exercise Physiology Laboratory, 2019–Present

Skills: evaluating body composition, exercise testing, collecting data outdoors, operating a metabolic cart, analyzing data quantitatively in Microsoft Excel and SPSS

Top Tier Doctoral Graduate Research Assistant, Kinesiology and Nutrition Sciences, 2020–2021

UCM

Student Researcher, Human Performance Laboratory, 2015–2019

Skills (beyond the skills applied at UNLV): electrocardiography, peripheral arterial tonometry, spirometry

<u>Professional Experience</u>

UNLV

Graduate Assistant, Graduate College (Summer 2021, Summer 2022)

1. Summer 2021: Collaborated with financial professionals to develop top-tier professional development content for graduate students. Compiled qualitative and quantitative data about UNLV's aspirational peer institutions. Reported the data and takeaways succinctly to my supervisors. Applied my understanding of the data to create the Graduate College's Financial Literacy & Wellness Graduate Badges.
2. Summer 2022: Built The Grad Academy's top-tier career, leadership, and professional development programs into UNLV's learning management system, hosted workshops, and made digital documents accessible.

<u>Textbook Chapters</u>

1. **Davis, D.**, Helm, M., Izuora, K., & Basu, A. (2022). Chapter 10: The role of berry bioactive compounds in diabetes mellitus. In Dorothy Klimis-Zacas & Ana Rodriguez-Mateos (Eds.), *Berries and berry bioactive compounds in promoting health* (1st ed., pp. 275-305). Royal Society of Chemistry.

<u>Peer-Reviewed Journal Articles (Published or In Press)</u>

1. Garver, M., Navalta, J., Heijnen, M., **Davis, D.**, Reece, J., Stone, W., Siegel, S., & Lyons, T. (2023). IJES self-study on participants' sex in exercise science: Sex-data gap and corresponding author survey. *International Journal of Exercise Science*, *16*(6), 364-376.

2. **Davis, D.**, Carrier, B., Cruz, K., Barrios, B., Landers, M., & Navalta, J. (2022). A systematic review of the effects of meditative and mindful walking on mental and cardiovascular health. *International Journal of Exercise Science*, *15*(2), 1692-1734.

3. **Davis, D.**, Garver, M., Stone, W., Penumetcha, M., Hair, J., & Philipp, N. (2022). Endothelial function and arterial stiffness in young adults with histories of chronic resistance activity. *Journal of Human Sport and Exercise*, *17*(2), 358-369. https://doi.org/10.14198/jhse.2022.172.11

4. Philipp, N., Garver, M., Crawford, D., **Davis, D.**, & Hair, J. (2022). Interlimb asymmetry in collegiate American football players: Effects on combine-related performance. *Journal of Human Sport and Exercise*, *17*(3), 708-718. https://doi.org/10.14198/jhse.2022.173.20

5. Navalta, J., **Davis, D.**, Carrier, B., Sertic, J., & Cater, P. (2021). Teaching applied exercise physiology using a prototype energy expenditure measurement device. *The Interdisciplinary Journal of Problem-based Learning*, *15*(2). https://doi.org/10.14434/ijpbl.v15i2.31525

6. **Davis, D.**, Carrier, B., Cruz, K., Barrios, B., & Navalta, J. (2021). A protocol and novel tool for systematically reviewing the effects of mindful walking on mental and cardiovascular health. *PLOS ONE*, *16*(10): e0258424. https://doi.org/10.1371/journal.pone.0258424

7. Salatto, R., McGinnis, G., **Davis., D.**, Carrier, B., Manning, J., DeBeliso, M., & Navalta, J. (2021). Effects of acute beta-alanine ingestion and immersion-plus-exercise on connectedness to nature and perceived pain. *International Journal of Environmental Research and Public Health*, *18*(15), 8134. https://doi.org/10.3390/ijerph18158134

8. **Davis, D.** (2021). A literature review on the physiological and psychological effects of labyrinth walking. *International Journal of Yogic, Human Movement, and Sports Sciences*, *6*(1): 167-175.

9. Philipp, N., Crawford, D., Garver, M., **Davis, D.,** & Hair, J. (2021). Interlimb asymmetry thresholds that negatively affect change of direction performance in collegiate American football players. *International Journal of Exercise Science*, *14*(4): 606-612.

10. **Davis. D.**, Crew, J., Planinic, P., Alexander, J., & Basu, A. (2020). Associations of dietary bioactive compounds with maternal inflammation and adiposity in gestational diabetes: An update of observational and clinical studies. *International Journal of Environmental Research and Public Health*, *17*(20): 7528. https://doi.org/10.3390/ijerph17207528

11. **Davis, D.**, Tallent, R., Navalta, J., Salazar, A., Lyons, T., & Basu, A. (2020). Effects of acute cocoa supplementation on postprandial apolipoproteins, lipoprotein subclasses, and inflammatory biomarkers in adults with type 2 diabetes after a high-fat meal. *Nutrients*, *12*(7): 1902. https://doi.org/10.3390/nu12071902

12. **Davis, D.**, Navalta, J., McGinnis, G., Serafica, R., Izuora, K., & Basu, A. (2020). Effects of acute dietary polyphenols and post-meal physical activity on postprandial metabolism in adults with features of the metabolic syndrome. *Nutrients*, *12*(4): 1120. https://doi.org/10.3390/nu12041120

13. Barrios, B., Carrier, B., Jolley, B., **Davis, D.**, Sertic, J., & Navalta, J. (2020). Establishing a methodology for conducting a rapid review on wearable technology reliability and validity in applied settings. *Topics in Exercise Science and Kinesiology*, *1*(2), Article 8.

14. Salatto, R., **Davis, D.**, Carrier, B., Barrios, B., Sertic, J., Cater, P., & Navalta, J. (2020). Efficient method of delivery for powdered supplement or placebo for an outdoor exercise investigation. *Topics in Exercise Science and Kinesiology*, *1*(2), Article 5.

15. Carducci, J., Garver, M., Stone, W., Penumetcha, M., **Davis, D.**, Philipp, N., & Hair, J. (2020). Early-morning and late-night maximal runs: Metabolic and perceived exertion outcomes. *Topics in Exercise Science and Kinesiology*, *1*(2), Article 2.

Peer-Reviewed Journal Articles (Under Development or Peer-Review)

1. Carrier, B., Salatto, R., **Davis, D.,** Sertic, J., Cater, P., Barrios, B., Girouard, T., McGinnis, G., Burroughs, B., & Navalta. J. (2022). Assessing the validity and reliability of several heart rate monitors in wearable technology while mountain biking. Submitted to *Journal for the Measurement of Physical Behaviour*.

2. Salatto, R., **Davis, D.**, Carrier, B., Barrios, B., Sertic, J., Cater, P., Liang, J., Basu, A., Burroughs, B., & Navalta, J. (2022). Test-retest reliability of four perceptual scales after the task of mountain biking. Submitted to *Measurement in Physical Education and Exercise Science*.

Abstracts (Published)

1. **Davis, D.**, Malek, E., Salatto, R., Montes, J., Aguilar, C., Bodell, N., Manning, J., DeBeliso, M., Lawrence, M., & Navalta, J. (2022). The effects of sitting and walking in green space on state mindfulness and connectedness to nature. *International Journal of Exercise Science: Conference Proceedings*, *14*(2), Article 26. Southwest American College of Sports Medicine 2022 Annual Meeting.

2. Carlos, K., **Davis, D.**, Carrier, B., Perdomo Rodriguez, J., Vargas, N., Malek, E., Weyers, B., & Navalta, J. (2022). The validity of bicep located heart rate monitors during running. *International Journal of Exercise Science: Conference Proceedings*, *14*(2), Article 108. Southwest American College of Sports Medicine 2022 Annual Meeting.

3. Cox, C., **Davis, D.**, Malek, E., Vargas, N., Dial, M., Weyers, B., Ziegler, W., Fullmer, W., Gil, D., Torres, M., Bodell, N., Manning, J., DeBeliso, M., Navalta, J., & Cowley, J. (2022). Evaluation and comparison of wearable technology device data between devices during trail running. *International Journal of Exercise Science: Conference Proceedings*, *14*(2), Article 112. Southwest American College of Sports Medicine 2022 Annual Meeting.

4. Peck, M., **Davis, D.**, Malek, E., Vargas, N., Dial, M., Weyers, B., Fullmer, W., Ziegler, W., Gil, D., Torres, M., Cox, C., Bodell, N., Manning, J., Cowley, J., Funk, M., Lawrence, M., DeBeliso, M., & Navalta, J. (2022). Evaluation of average and maximum heart rate of wrist-worn wearable technology devices during trail running. *International Journal of Exercise Science: Conference Proceedings*, *14*(2), Article 68. Southwest American College of Sports Medicine 2022 Annual Meeting.

5. Perdomo Rodriguez, J., **Davis, D.**, Vargas, N., Malek, E., Carrier, B., Carlos, K., Weyers, B., & Navalta, J. (2022). Comparing exercise intensity as a percentage of the age-estimated heart rate max among walking, jogging, and skipping. *International Journal of Exercise Science: Conference Proceedings*, *14*(2), Article 69. Southwest American College of Sports Medicine 2022 Annual Meeting.

6. Weyers, B., **Davis, D.**, Vargas, N., Malek, E., Carrier, B., Carlos, K., Perdomo Rodriguez, J., & Navalta, J. (2022). Determining validity and reliability of caloric expenditure recorded by wearable technology while walking and running. *International Journal of Exercise Science: Conference Proceedings*, *14*(2), Article 183. Southwest American College of Sports Medicine 2022 Annual Meeting.

7. Ziegler, W., **Davis, D.**, Malek, E., Vargas, N., Dial, M., Weyers, B., Fullmer, W., Gil, D., Torres, M., Cox, C., Bodell, N., Manning, J., DeBeliso, M., Navalta, J., & Cowley, J. (2022). Reliability of the Stryd accelerometer on an incline and decline. *International Journal of Exercise Science: Conference Proceedings*, *14*(2), Article 97. Southwest American College of Sports Medicine 2022 Annual Meeting.

8. Garcia, A., Strehlow, M., **Davis, D.**, Miguel, J., Montes, J., & Navalta, J. (2022). Validity of Garmin devices while ascending and descending flights of stairs. *International Journal of Exercise Science: Conference Proceedings*, *14*(2), Article 118. Southwest American College of Sports Medicine 2022 Annual Meeting.

9. Vargas, N., Carrier, B., **Davis, D.**, Perdomo Rodriguez, J., Malek, E., Weyers, B., Carlos, K., & Navalta, J. (2022). The validity and reliability of the Garmin Instinct in measuring heart rate, energy expenditure, and steps during skipping. *International Journal of Exercise Science: Conference Proceedings*, *14*(2), Article 87. Southwest American College of Sports Medicine 2022 Annual Meeting.

10. Pearce, D., Graffius, J., Ellingford, B., Carrier, B., Aguilar, C., Gil, D., Torres, M., **Davis, D.**, Ziegler, W., Fullmer, W., Bodell, N., Manning, J., Navalta, J., DeBeliso, M., Funk, M., & Lawrence, M. (2022). Concurrent validity and reliability of average heart rate and energy expenditure of identical Garmin Instinct watches during low intensity resistance training. *International Journal of Exercise Science: Conference Proceedings*, *14*(2), Article 159. Southwest American College of Sports Medicine 2022 Annual Meeting.

11. Graffius, J., Pearce, D., Ellingford, B., Carrier, B., Aguilar, C., Fullmer, W., Ziegler, W., Gil, D., Torres, M., **Davis, D.**, Bodell, N., Manning, J., DeBeliso, M., Navalta, J., Funk, M., & Lawrence, M. (2022). Average heart rate and energy expenditure validity of Garmin vívoactive 3 and fēnix 6 wrist watches during light circuit resistance training. *International Journal of Exercise Science: Conference Proceedings*, *14*(2), Article 129. Southwest American College of Sports Medicine 2022 Annual Meeting.

12. Whaley, V., & **Davis, D.** (2022). Remote learning. *166.* https://digitalscholarship.unlv.edu/btp_expo/166/. UNLV Best Teaching Practices Expo, 2022.

13. **Davis, D.**, Carrier, B., Cruz, K., Barrios, B., & Navalta, J. (2021). The effects of meditative and mindful walking on mental and cardiovascular health. *International Journal of Exercise Science: Conference Proceedings*, *14*(1), Article 8. Southwest American College of Sports Medicine 2021 Annual Meeting.

14. Bodell, N., Carrier, B., Gil, D., Fullmer, W., Cruz, K., Aguilar, C., **Davis, D.**, Malek, E., Montes, J., Manning, J., DeBeliso, M., & Lawrence, M. (2021). Validity of average heart rate and energy expenditure in Polar OH1 and Verity Sense while self-paced walking. *International Journal of Exercise Science: Conference Proceedings*, *14*(1), Article 69. Southwest American College of Sports Medicine 2021 Annual Meeting.

15. Carrier, B., Salatto, R., **Davis, D.**, Sertic, J., Barrios, B., Cater, P., & Navalta, J. (2021). Assessing the validity of several heart rate monitors in wearable technology while mountain biking. *International Journal of Exercise Science: Conference Proceedings*, *14*(1), Article 18. Southwest American College of Sports Medicine 2021 Annual Meeting.

16. Cruz, K., Navalta, J., **Davis, D.**, & Carrier, B. (2021). Validity of the K5 wearable metabolic system during the YMCA Bench Press Test - A pilot study. *International Journal of Exercise Science: Conference Proceedings*, *14*(1), Article 22. Southwest American College of Sports Medicine 2021 Annual Meeting.

17. Fullmer, W., Carrier, B., Gil, D., Cruz, K., Aguilar, C., **Davis, D.**, Malek, E., Bodell, N., Montes, J., Manning, J., DeBeliso, M., Navalta, J., & Lawrence, M. (2021). Validity of average heart rate and energy expenditure in Polar armband devices while self-paced biking. *International Journal of Exercise Science: Conference Proceedings*, *14*(1), Article 26. Southwest American College of Sports Medicine 2021 Annual Meeting.

18. Garcia, A., Strehlow, M., **Davis, D.**, Miguel, J., Montes, J., & Navalta, J. (2021). Validity of Garmin devices while ascending and descending flights of stairs. *International Journal of Exercise Science: Conference Proceedings*, *2*(14), Article 1. Southwest American College of Sports Medicine 2021 Annual Meeting.

19. Gil, D., Carrier, B., Fullmer, W., Cruz, K., Aguilar, C., **Davis, D.**, Malek, E., Bodell, N., Montes, J., Manning, J., DeBeliso, M., & Lawrence, M. Validity of average heart rate and energy expenditure in Polar OH1 and Verity Sense while self-paced running. *International Journal of Exercise Science: Conference Proceedings*, *14*(1), Article 27. Southwest American College of Sports Medicine 2021 Annual Meeting.

20. Helm, M., Carrier, B., **Davis, D.**, Cruz, K., Barrios, B., & Navalta, J. (2021). Validation of the Garmin fēnix 6S maximal oxygen consumption (VO_{2max}) estimate. *International Journal of Exercise Science: Conference Proceedings*, *14*(1), Article 29. Southwest American College of Sports Medicine 2021 Annual Meeting.

21. Miguel, J., Garcia, A., Strehlow, M., **Davis, D.**, Montes, J., & Navalta, J. (2021). Comparison of flights climbed between Garmin and Fitbit devices. *International Journal of Exercise Science: Conference Proceedings*, *2*(14), Article 22. Southwest American College of Sports Medicine 2021 Annual Meeting.

22. Salatto, R., **Davis, D.**, Navalta, J., Montes, J., Bodell, N., Carrier, B., Manning, J., & DeBeliso, M. (2021). Evaluating the validity of the Salatto-Love and Care of Nature Direct Indication Scale against the Love and Care of Nature Scale during rest and after self-paced hiking. *International Journal of Exercise Science: Conference Proceedings*, *2*(14), Article 30. Southwest American College of Sports Medicine 2021 Annual Meeting.

23. Strehlow, M., Garcia, A., **Davis, D.**, Miguel, J., Montes, J., & Navalta, J. (2021). Validity of Fitbit devices while ascending and descending flights of stairs. *International Journal of Exercise Science: Conference Proceedings*, *14*(1), Article 57. Southwest American College of Sports Medicine 2021 Annual Meeting.

24. Salatto, R., McGinnis, G., **Davis, D.**, Carrier, B., Manning, J., DeBeliso, M., & Navalta, J. (2021). Exercise increases connectedness to nature regardless of perceived pain. *Medicine & Science in Sports and Exercise*, *53*(8 Suppl), 319. American College of Sports Medicine 2021 Annual Meeting.

25. **Davis, D.**, Scofield, H., Betts, N., Izuora, K., & Basu, A. (2020). Dietary strawberries improve insulin resistance in adults with the metabolic syndrome. *Current Developments in Nutrition*, *4*(2 Suppl), 388. American Society for Nutrition 2020 Nutrition 2020 Live Online.

26. **Davis, D.**, Garver, M., Stone, W., Penumetcha, M., Hair, J., & Philipp, N. (2020). Endothelial function in young adults reporting histories of chronic resistance activity. *Medicine & Science in Sports and Exercise*, *52*(7 Suppl), 898. American College of Sports Medicine 2020 Annual Meeting.

27. Carducci, J., Garver, M., Stone, W., Penumetcha, M., **Davis, D.**, Hair, J., & Philipp, N. (2020). Metabolic and perceived exertion outcomes during maximal runs at ends of the day. *Medicine & Science in Sports and Exercise*, *52*(7 Suppl), 1041. American College of Sports Medicine 2020 Annual Meeting.

28. Garver, M., Stone, W., Wakeman, A., Hair, J., **Davis, D.**, Grinde, D., Allan, S., & Nehlsen, E. (2020). Somatotype of female and male field athletes: Comparing between sexes and among select events. *Medicine & Science in Sports and Exercise*, *52*(7 Suppl), 874. American College of Sports Medicine 2020 Annual Meeting.

29. Hair, J., Garver, M., Stone, W., Wakeman, A., **Davis, D.**, Grinde, D., Allan, S., & Nehlsen, E. (2020). Somatotyping male and female sprinters and endurance sprinters. *Medicine & Science in Sports and Exercise*, *52*(7 Suppl), 874. American College of Sports Medicine 2020 Annual Meeting.

30. Navalta, J., McGinnis, G., Manning, J., Salatto, R., Carrier, B., **Davis, D.**, Sertic, J., Cater, P., Barrios, B., Malek, E., Reynolds, C., & DeBeliso, M. (2020). Acute beta-alanine supplementation and pain perception before and after hiking. *Medicine & Science in Sports and Exercise*, *52*(7 Suppl), 622. American College of Sports Medicine 2020 Annual Meeting.

31. Stone, W., Wakeman, A., Garver, M., Hair, J., **Davis, D.**, Nehlsen, E., Grinde, D., & Allan, S. (2020). Somatotyping in college track and field athletes: Evaluating change across a competitive season. *Medicine & Science in Sports and Exercise*, *52*(7 Suppl), 873. American College of Sports Medicine 2020 Annual Meeting.

32. Salatto, R., Navalta, J., Montes, J., Bodell, N., Carrier, B., Sertic, J., Barrios, B., Cater, P., **Davis, D.**, Manning, J., & DeBeliso, M. (2020). Evaluating the validity of heart rate measured by the Suunto Spartan sport watch during trail running. *Medicine & Science in Sports and Exercise*, *52*(7 Suppl), 220. American College of Sports Medicine 2020 Annual Meeting.

33. **Davis, D.,** Carducci, J., Garver, M., Stone, W., Penumetcha, M., Philipp, N., Hair, J., Elledge, J., Williams, H., Oliphant, M., & Hopkins, Z. Ability to predict impending volitional exhaustion based on aerobic capacity. *Medicine & Science in Sports & Exercise*, *51*(5 Suppl), 947. Central States American College of Sports Medicine 2018 Annual Meeting. American College of Sports Medicine 2019 Annual Meeting.

34. Garver, M., **Davis, D.**, Jennings, M., Dinyer, T., Rickard, A., Burns, S., Hughes, B., & Burnett, D. (2019). Asthma and EIB testing among collegiate athletes in indoor winter sports. *Medicine & Science in Sports & Exercise*, *51*(5 Suppl). American College of Sports Medicine 2019 Annual Meeting.

35. Wakeman, A., Stone, W., Younkin, B., **Davis, D.**, Garver, M., & Strohmeyer, H.S. (2019). Case-control investigation of speed and gait after an incomplete spinal cord injury. *Medicine & Science in Sports & Exercise*, *51*(5 Suppl). American College of Sports Medicine 2019 Annual Meeting.

36. Stone, W., Wakeman, A., Younkin, B., **Davis, D.**, Garver, M., & Strohmeyer, H.S. (2019). Should we stick with step counts after incomplete spinal cord injury? A case-control investigation. *Medicine & Science in Sports & Exercise*, *51*(5 Suppl). American College of Sports Medicine 2019 Annual Meeting.

37. Carducci, J., Garver, M., Stone, W., Penumetcha, M., **Davis, D.**, McMillin, A., Hair, J., Philipp, N., Elledge, J., Sheck, E., & Scherry, K. (2019). Maximal VO_2 at extreme ends of the day following glucose provision. *International Journal of Exercise Science: Conference Proceedings*, *11*(6), Article 14. Central States American College of Sports Medicine 2019 Annual Meeting.

38. Philipp, N., Carducci, J., Garver, M., Stone, W., Penumetcha, M., **Davis, D.**, McMillin, A., Hair, J., Elledge, J., Sheck, E., & Scherry, K. (2019). Maximal perceived effort during maximal running at extreme ends of the day. *International Journal of Exercise Science: Conference Proceedings*, *11*(6), Article 61. Central States American College of Sports Medicine 2019 Annual Meeting.

39. **Davis, D.**, Garver, M., Jennings, M., Hughes, B., Burns, S., Dinyer, T., Rickard, A., Colf, J., Wilson, L., Carducci, J., & Blazer, A. (2018). Comparison of overall and segmental body composition in collegiate track athletes using BIA and DXA. *Medicine & Science in Sports & Exercise*, *50*(5 Suppl), 167. American College of Sports Medicine 2018 Annual Meeting.

40. Garver, M., Jennings, M., **Davis, D.**, Hughes, B., Burns, S., Dinyer, T., Rickard, A., Colf, J., Carducci, J., Blazer, A., Wilson, L., & Burnett, D. (2018). Pulmonary testing and exercise-induced bronchoconstriction in collegiate baseball players. *Medicine & Science in Sports & Exercise*, *50*(5 Suppl), 835. American College of Sports Medicine 2018 Annual Meeting.

41. Burns, S., Hughes, B., Garver, M., Glover, D., Dinyer, T., Rickard, A., Jennings, M., **Davis, D.**, Blazer, A., Carducci, J., Miller, L., Colf, J., Burnett, D., & Godard, M. (2017). Prevalence of asthma and exercise-induced bronchoconstriction in college wrestlers. *Medicine & Science in Sports and Exercise*, *49*(5 Suppl), 1044. American College of Sports Medicine 2017 Annual Meeting.

42. **Davis, D.**, Harden, N., Adams, Z., Ingram, J., Low, J., & Burns, S. (2016). Effects of pre-workout nutrition on peak anaerobic power output on the Wingate anaerobic test. *International Journal of Exercise Science: Conference Proceedings*, *11*(4), Article 38. Central States American College of Sports Medicine 2016 Annual Meeting.

43. Burns, S., Calcote, A., Dinyer, T., **Davis, D.**, Pearson, R., Rickard, A., Blazer, A., Seymour, M., Burnett, D., & Godard, M. (2016). Investigating 3-min step test compared to treadmill test for provocation of exercise induced bronchoconstriction (EIB). *Medicine & Science in Sports & Exercise*, *48*(5 Suppl), 281. American College of Sports Medicine 2016 Annual Meeting.

Abstracts (Unpublished but Presented at Conferences)

1. **Davis, D.**, Malek, E., Salatto, R., Montes, J., Manning, J., DeBeliso, M., & Navalta, J. (2022). Validity of measuring state mindfulness with a visual analog scale. 24th Annual Graduate and Professional Student Research Forum.

2. Cruz, K., Salatto, R., **Davis, D.**, Carrier, B., Barrios, B., Cater, P., Farmer. H., & Navalta, J. (2020). Evaluation of Rating of Perceived Exertion during mountain biking. Southwest American College of Sports Medicine 2020 Annual Meeting.

3. Farmer, H., Salatto, R., **Davis, D.**, Carrier, B., Barrios, B., Cater, P., Cruz, K., & Navalta, J. (2020). Evaluation of the Felt Arousal Scale during mountain biking. Southwest American College of Sports Medicine 2020 Annual Meeting.

4. **Davis, D.**, Barrios, B., Carrier, B., Salatto, R., Sertic, J., Cater, P., Montes, J., Bodell, N., Manning, J., DeBeliso, M., & Navalta, J. (2019). Evaluating the validity of heart rate measured by the Garmin fēnix 5 during trail running. Southwest American College of Sports Medicine 2019 Annual Meeting.

5. Barrios, B., Sertic, J., Cater, P., **Davis, D.**, Carrier, B., Salatto, R., Montes, J., Bodell, N., Manning, J., DeBeliso, M., & Navalta, J. (2019). Evaluating the validity of heart rate measured by the Jabra Elite during trail running. Southwest American College of Sports Medicine 2019 Annual Meeting.

6. Cater, P., Sertic, J., **Davis, D.**, Barrios, B., Carrier, B., Salatto, R., Montes, J., Bodell, N., Manning, J., DeBeliso, M., & Navalta, J. (2019). Evaluating the validity of heart rate measured by the Rhythm during trail running. Southwest American College of Sports Medicine 2019 Annual Meeting.

7. Navalta, J., Salatto, R., Montes, J., Bodell, N., Bryson, C., Sertic, J., Barrios, B., Cater, P., **Davis, D.**, Manning, J., & DeBeliso, M. (2019). Wearable device price is correlated with the limits of agreement range as a measure of heart rate validity during trail running. Southwest American College of Sports Medicine 2019 Annual Meeting.

8. Sertic, J., Cater, P., **Davis, D.**, Barrios, B., Carrier, B., Salatto, R., Montes, J., Bodell, N., Manning, J., DeBeliso, M., & Navalta. J. (2019). Validating the heart rate feature of the Motiv Ring on outside graded terrain. Southwest American College of Sports Medicine 2019 Annual Meeting.

Presentations

1. **Davis, D.**, & Navalta, J. (2023). Deep work: Minimizing distractions and prioritizing important tasks. Virtual workshop hosted for the UNLV Graduate College (Las Vegas, NV).

2. **Davis, D.** (2022). Outside looking in: Connecting to oneself and nature through green exercise. Preliminaries, semi-finals, and finals of the 9th Annual Rebel Grad Slam (Las Vegas, NV).

3. **Davis, D.**, Malek, E. Salatto, R., Montes, J., Aguilar, C., Bodell, N., Manning, J., DeBeliso, M., Lawrence, M., & Navalta, J. (2022). The effects of sitting and walking in green space on state mindfulness and connectedness to nature. Southwest American College of Sports Medicine 2022 Annual Meeting (Costa Mesa, CA).

4. **Davis, D.**, Malek, E., Salatto, R., Montes, J., Manning, J., DeBeliso, M., & Navalta, J. (2022). Validity of measuring state mindfulness with a visual analog scale. 1st Place, In-Person Poster Session 4, 24th Annual Graduate and Professional Student Research Forum (Las Vegas, NV).

5. **Davis, D.** (2022). This place, this moment. 7th Annual Inspiration, Innovation, Impact (Las Vegas, NV).

6. **Davis, D.**, & Navalta, J. (2022). Publishing your research in graduate school. Virtual workshop hosted for the UNLV Graduate College (Las Vegas, NV).

7. **Davis, D.** (2021). Let your mind wander… and follow it (literally). Preliminaries and semi-finals of the 8th Annual Rebel Grad Slam (Las Vegas, NV).

8. **Davis, D.**, Carrier, B., Cruz, K., Barrios, B., & Navalta, J. (2021). The effects of meditative and mindful walking on mental and cardiovascular health. Graduate Student Research Award Competition. Southwest American College of Sports Medicine 2021 Annual Meeting (Costa Mesa, CA).

9. **Davis, D.**, Scofield, H., Betts. N., Izuora, K., & Basu, A. (2020). Effects of strawberries on LDL cholesterol and insulin resistance in adults with the metabolic syndrome. 22nd Annual Graduate and Professional Student Research Forum (Las Vegas, NV).

10. Whaley, V., & **Davis, D.** (2020). Hands-on, eyes-off learning. *120.* https://digitalscholarship.unlv.edu/btp_expo/120. UNLV Best Teaching Practices Expo (Las Vegas, NV).

11. **Davis, D.** (2020). Using plant-based foods and physical activity to lower blood sugar after eating. Preliminaries, semi-finals, and finals of the 7th Annual Rebel Grad Slam (Las Vegas, NV).

12. **Davis, D.**, Barrios, B., Carrier, B., Salatto, R., Sertic, J., Cater, P., Montes, J., Bodell, N., Manning, J., DeBeliso, M., & Navalta, J. (2019). Evaluating the validity of heart rate measured by the Garmin fēnix 5 during trail running. 23rd Annual Graduate and Professional Student Research Forum (Las Vegas, NV) and Southwest American College of Sports Medicine 2019 Annual Meeting (Costa Mesa, CA).

13. **Davis, D.,** Carducci, J., Garver, M., Stone, W., Penumetcha, M., Philipp, N., Hair, J., Elledge, J., Williams, H., Oliphant, M., & Hopkins, Z. (2019). Ability to predict impending volitional exhaustion based on aerobic capacity. American College of Sports Medicine 2019 Annual Meeting (Orlando, FL); 2019 Graduate Scholars Symposium (Warrensburg, MO); Central States American College of Sports Medicine 2018 Annual Meeting (Kansas City, MO).

14. **Davis, D.**, Garver, M., Jennings, M., Hughes, B., Burns, S., Dinyer, T., Rickard, A., Colf, J., Wilson, L., Carducci, J., & Blazer, A. (2018). Comparison of overall and segmental body composition in collegiate track athletes using BIA and DXA. American College of Sports Medicine 2018 Annual Meeting (Minneapolis, MN) and 2018 Graduate Scholars Symposium (Warrensburg, MO).

15. **Davis, D.**, Harden, N., Adams, Z., Ingram, J., Low, J., & Burns, S. (2016). Effects of pre-workout nutrition on peak anaerobic power output on the Wingate anaerobic test. 2016 Undergraduate Scholars Symposium (Warrensburg, MO); Central States American College of Sports Medicine 2016 Annual Meeting (Fayetteville, AR); Missouri Association for Health, Physical Education, Recreation, and Dance 2016 Annual Convention (Lake Ozark, MO).

16. **Davis, D.** Examining the relationships between skeletal muscle mass, fiber type, and obesity: A systematic review of the literature. 2017 Undergraduate Scholars Symposium (Warrensburg, MO).

Supporting Roles in Mentees' Projects

1. Helm, M., Carrier, B., **Davis, D.**, Cruz, K., Barrios, B., & Navalta, J. (2021). Validation of the Garmin fēnix 6S maximal oxygen consumption (VO_{2max}) estimate. Fall 2021 Virtual Undergraduate Research Symposium, Lightning Talk (Las Vegas, NV).

2. Miguel, J., Garcia, A., Strehlow, M., **Davis, D.**, Montes, J., & Navalta, J. (2021). Comparison of flights climbed between Garmin and Fitbit devices. Fall 2021 Virtual Undergraduate Research Symposium, Poster Presentation (Las Vegas, NV).

Funding Sought (Awarded)

UNLV

1. Kinesiology and Nutrition Sciences Graduate Student Travel Funding, 2019, 2022

UCM

1. Graduate Student Travel Fund Recipient, 2018, 2019

2. Graduate Research Award Recipient, 2018

Funding Sought (Not Awarded)

UNLV

1. Feasibility of Chronic Mindful Walking in Undergraduate Students at Two Different U.S. Universities (R34 Clinical Trial - PAR-21-240 - $651,626): A Feasibility Clinical Trial of Mind and Body Interventions for National Center for Complementary and Integrative Health High Priority Research Topics — submitted June 2022

2. Feasibility of Heart Rate Variability-Guided Aerobic Exercise in People with Chronic Migraine (R34 Clinical Trial - PAR-21-240 - $660,299): A Feasibility Clinical Trial of Mind and Body Interventions for National Center for Complementary and Integrative Health High Priority Research Topics — submitted February 2022

3. Feasibility of Heart Rate Variability-Guided Exercise and Mindful Walking in Migraineurs (R34 Clinical Trial - PAR-21-240 - $672,750): A Feasibility Clinical Trial of Mind and Body Interventions for National Center for Complementary and Integrative Health High Priority Research Topics — submitted October 2021

4. Southwest American College of Sports Medicine Student Travel Grant, 2021

5. Southwest Travel Award (UNLV Philanthropy and Southwest Airlines), Fall 2019, Spring 2020

Professional Service

UNLV

1. Graduate Rebel Ambassador, 2021–Present
    a. Spring 2023 The Grad Academy Promo Video
    b. Fall 2022 Involvement Fair
    c. Grad Rebel Ambassador Tips for Success
    d. Finals of the 1st Annual President's Innovation Challenge
    e. Finals of the 8th Annual Rebel Grad Slam (3-Minute Thesis)
    f. Teaching-In Person Training Series: Tips & Tricks from Graduate Teaching Assistants
    g. Etiquette Training
    h. Fall 2021 Open House of the Graduate College and Graduate and Professional Student Association
    i. Fall 2021 New Graduate Assistant Orientation Panel

2. Student Leader, Southern Nevada Leadership Summit: The Future of UNLV Talent Pipelines, 2022

3. Member, Health + Wellness Advisory Board, 2021–Present

4. Member, Graduate Student Advisory Board, 2021–Present

5. Member, The Grad Academy Advisory Board Member, 2021–Present

6. Member, Ph.D. Social Committee, 2022

7. At-Large Representative, Graduate and Professional Student Association, 2019–2020
    a. Awards Committee, 2019–2020
    b. Professional Development Committee, 2019–2020

UCM

1. President, Graduate Student Association, 2017–2019

2. Graduate Student Representative, Graduate Council, 2017–2019

3. Graduate Student Representative, Center for Teaching and Learning Advisory Group, 2018–2019

4. Graduate Student Representative, Exercise Science Faculty Search Committee, 2019

5. Graduate Student Representative, Health Studies Faculty Search Committee, 2017–2018

6. Graduate Student Representative, Faculty Senate Distance Education Committee, 2017–2018

7. President, Health Professions Association, 2016–2017

8. Vice President, Health Professions Association, 2016

9. Member, Honors College Student Association, 2015–2017

Peer-Reviewed Academic Journals

1. *International Journal of Exercise Science* (IJES)
    a. Member, IJES West Executive Team, 2022–Present
    b. Associate Editor, 2022–Present
    c. Managing Editor, 2017–Present
    d. Peer-Reviewer, 2017–Present (seven manuscripts)

2. *Topics in Exercise Science and Kinesiology*
    a. Peer-Reviewer, 2020–Present (two manuscripts)

3. *International Journal of Environmental Research and Public Health*
    a. Peer-Reviewer, 2022–Present (one manuscript)

4. *Nutrients*
    a. Peer-Reviewer, 2020 (one manuscript)

5. *Complementary Therapies in Clinical Practice*
    a. One collaborative peer-review with Dr. James Navalta

Professional Organizations

1. Southwest Chapter of the American College of Sports Medicine
    a. Quiz Bowl Judge, 2022

Professional Memberships

1. National Student Member, American College of Sports Medicine, 2018–Present

2. Regional Student Member, Southwest Chapter of the American College of Sports Medicine, 2019–Present

3. Student Member, International Community of Scholars in Kinesiology, 2020–Present

4. National Student Member, American Society for Nutrition, 2019

5. Regional Student Member, Central States Chapter of the American College of Sports Medicine, 2016–2019

6. Student Member, Missouri Association for Health, Physical Education, Recreation, and Dance Student Member, 2016–2018

Scholastic Achievements and Awards

UNLV

1. Outstanding Graduate Student Teaching Award, University-Wide 1st-Place Winner, 2021

2. Finalist, Rebel Grad Slam (3-Minute Thesis), 2020, 2022
    a. Recipient, Audience's Choice Award in the semi-finals and finals, 2022

3. Semi-Finalist, Rebel Grad Slam (3-Minute Thesis), 2021

UCM

1. Recipient, Reid Hemphill Outstanding Graduate Student Scholarship, University-Wide 1st-Place Winner, 2019

2. Nominee, Learning to a Greater Degree Award, 2017

3. Nominee, Who's Who Award, 2017

4. Nominee, George H. Charno Outstanding Undergraduate Award, 2017

5. Recipient, Missouri Association for Health, Physical Education, Recreation, and Dance's Major of the Year Award (Exercise Science), 2016

6. Recipient, Shelby and Marjorie Brightwell Scholarship, 2015–2017

7. Recipient, Dr. Robert Tompkins Memorial Scholarship, 2015–2017

8. Recipient, William & Dorothy Brewster & Nancy Saunders-Cromer Scholarship Award, 2015–2017

9. Recipient, Transfer Scholarship, 2015–2017

10. Dean's List, 2015–2017

<u>Volunteer Work</u>

1. Volunteer, Kappa Delta Chi, Springs Preserve Día de Los Muertos, the National Association of Latino Fraternal Organizations' National Day of Service, 2021 (Las Vegas, NV)

2. Student Representative, UNLV's Mid Cycle Accreditation Visit with the Northwest Commission on Colleges and Universities, 2020

3. Graduate Representative for Kinesiology, Showcase for the College of Health, Science, and Technology, UCM, 2019

4. Speaker, Graduate Webinars hosted by The Office of Graduate and International Student Services
    a. Choosing a Graduate Program at UCM
    b. Preparing for the GMAT/GRE

5. Interviewee, University Promotional Materials, Office of Integrated Marketing and Communication
    a. Class of 2019 Graduate Commencement video, 2019
    b. National First-Generation Student video, 2018
    c. Opportunity in Action campaign, 2018
    d. Transfer Student video, 2018
    e. Class of 2017 Undergraduate Commencement video, 2017

6. Judge, Missouri Region IV Science Olympiad Tournament, 2017

<u>Non-Academic Work Experience</u>

1. Cardiac and Pulmonary Rehabilitation Intern, Western Missouri Medical Center, 2017

2. Personal Trainer, UCM Student Recreation and Wellness Center, 2016–2017

<u>Certifications</u>

1. CITI Program (Collaborative Institutional Training Initiative), 2015–Present

2. Certified Publons Academy Peer Reviewer, 2021

3. Financial Fundamentals for Students, Magna Publications, 2021

4. Core Strategies for Teaching in Higher Ed, LinkedIn Learning, 2020

5. Learning to Teach Online, LinkedIn Learning, 2020

6. CPR/AED for Pro Rescuers, 2015–2020

UNLV Professional Development Programs

1. Grad Rebel Writing Bootcamp, 2020

2. Graduate Assistant Online Teaching Essentials, 2020