

COGNITION AND MOTION: THE RELATIONSHIP BETWEEN COGNITIVE FUNCTION,
PHYSICAL ACTIVITY, AND GAIT DYNAMICS IN YOUNG ADULTS

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Abstract

Physical activity (PA) levels have been declining worldwide, despite the physical health benefits of increased PA having been known and messaged for decades. One possible reason for this is that physical health improvements are too long term, and it may be more effective to promote short-term benefits instead. PA has been shown to improve cognitive function over relatively short timescales and promoting these cognitive benefits may be a better approach for increasing PA levels in the population. What remains unclear is how certain moderators can affect the PA-cognition relationship, and how we can better measure PA in real-world environments. Over three studies, my dissertation aims to examine the effect of moderators like PA intensity and PA time window, as well as develop a novel methodology for measuring low-intensity physical activities, like walking, in outdoor settings. In the first study, I found that PA measured over the week prior to cognitive testing is not predictive of cognitive function, and that this does not depend on PA intensity. In the second study, I found that PA measured over the day prior to testing is associated with improved cognitive performance. In the third study, I examined the bidirectional nature of this relationship, and found that changes in cognition can have a marked impact on outdoor walking behaviour, and that these changes in gait dynamics can be detected using a smartphone-based measurement technique. Overall, my research shows that while PA does appear to be associated with cognitive function, the effects are quite small and dependent on factors like when the PA is measured relative to cognitive testing. In terms of PA promotion, more research needs to be conducted on the limits of this relationship before we can draw any definitive conclusions about the cognitive benefits of PA, as small statistically significant effects don't necessarily translate to useful practical benefits in the real world.

Lay Summary

We're constantly told that doing physical activity (PA) can improve your health, from combating obesity to reducing risk of heart disease. Some also claim that PA can improve how well your brain functions, especially how well you pay attention to things and avoid distractions. However, it's not clear that this is always true, and my research tries to understand when PA provides this benefit and when it doesn't. I found that only PA performed very recently has this benefit. For example, if you have an exam tomorrow, doing PA today can improve your test performance. Before my research, we also didn't have a good way of measuring PA outdoors, so I developed a way to measure activities like walking using smartphones. Overall, my research shows that PA can have some benefits for brain function, but it's important to remember that this might only be true in some specific situations.

Preface

I prepared the content of this dissertation, with minor edits from Todd Handy. The research presented in Chapters 2 to 4 was primarily conducted by myself, and I was responsible for the description of theory and summary of current research described in Chapters 1 and 5. The research studies in Chapters 2 and 4 have been published (see details below).

Chapter 2. A version of Chapter 2 has been published as Ho, S., Gooderham, G. K., & Handy, T. C. (2018). Self-reported free-living physical activity and executive control in young adults. *PLoS ONE*. I was responsible for study conception and design, data analysis and interpretation, and manuscript composition. G. K. Gooderham was responsible for data interpretation and critical review of the manuscript. T. C. Handy was responsible for study conception and design, interpretation, and critical review of the manuscript. This study was approved by the University of British Columbia's Research Ethics Board: H15-02699.

Chapter 3. A version of Chapter 3 is currently being prepared for publication. I was responsible for study conception and design, data analysis, and data interpretation. G. K. Gooderham was responsible for data collection and data interpretation. P. Kozik was responsible for data analysis and data interpretation. T. C. Handy was responsible for study conception and design, and data interpretation. This study was approved by the University of British Columbia's Research Ethics Board: H15-02699 and H15-02429.

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Chapter 5. Minor portions of the concluding chapter were revised and heavily modified from a review paper written for my comprehensive exam completed at the University of British Columbia, of which I was the sole author. Figure 5.1 was created by me for the purpose of this dissertation.

All internet links in this thesis were verified to be active as of April 26, 2020.

Table of Contents

Abstract.....	iii
Lay Summary.....	iv
Preface	v
Table of Contents.....	vii
List of Tables	xii
List of Figures	xiii
Acknowledgements.....	xiv
Dedication.....	xv
1 Introduction.....	1
1.1 Physical activity and cognition	4
1.1.1 Physical activity intensity	7
1.1.2 Physical activity time window	7
1.2 A bidirectional relationship	9
1.2.1 Executive function and gait	11
1.2.2 Outdoor walking.....	13
1.3 Real-world physical activity and cognition.....	15
1.4 Dissertation overview.....	16
2 Weekly physical activity and cognition	18
2.1 Study 1	21
2.1.1 Ethics statement	21

2.1.2	Participants.....	21
2.1.3	Apparatus.....	22
2.1.4	International Physical Activity Questionnaire.....	22
2.1.5	Attention Network Test (ANT).....	25
2.1.6	Data analysis.....	27
2.1.7	Results.....	28
2.1.8	Discussion.....	30
2.2	Study 2.....	31
2.2.1	Participants.....	31
2.2.2	Procedure.....	32
2.2.3	Data analysis.....	33
2.2.4	Results.....	33
2.2.5	Discussion.....	34
2.3	Study 3.....	35
2.3.1	Participants.....	35
2.3.2	Procedure.....	35
2.3.3	Data analysis.....	36
2.3.4	Results.....	36
2.3.5	Discussion.....	39
2.4	General discussion.....	40
2.5	Bridging Summary.....	46
3	24-hour physical activity and cognition.....	47

3.1	Study 1	47
3.1.1	Method.....	49
3.1.2	Results.....	53
3.1.3	Discussion	55
3.2	Study 2	59
3.2.1	Method.....	60
3.2.2	Results.....	63
3.2.3	Discussion	65
3.3	Bridging Summary	69
4	Cognitive load and walking	70
4.1	Study 1	73
4.1.1	Ethics Statement.....	73
4.1.2	Participants.....	73
4.1.3	Apparatus	74
4.1.4	Method.....	75
4.1.5	Calculating Gait Dynamics	75
4.1.6	Results.....	78
4.1.7	Discussion	81
4.2	Study 2	81
4.2.1	Participants.....	82
4.2.2	Method.....	83
4.2.3	Results.....	84

4.2.4	Discussion	85
4.3	Study 3	86
4.3.1	Participants.....	86
4.3.2	Methods.....	87
4.3.3	Results.....	89
4.3.4	Discussion	91
4.4	Study 4	92
4.4.1	Participants.....	92
4.4.2	Methods.....	93
4.4.3	Results.....	94
4.4.4	Discussion	97
4.5	General Discussion.....	97
5	Conclusion.....	102
5.1	Mixed literature on physical activity benefits.....	104
5.2	Neuroimaging versus behaviour.....	107
5.3	Methodological and statistical issues	108
5.4	Public health messaging	110
5.5	Dual task gait	111
5.5.1	Gait prioritization.....	112
5.6	Smartphones in research.....	113
5.6.1	Gait dynamics.....	113
5.6.2	Physical activity	116

5.7	Limitations	117
5.8	Future directions	121
5.9	Summary	124
	References	127
	Appendix A: International Physical Activity Questionnaire	186
	Appendix B: Physical Activity Scale	191

List of Tables

Table 2.1 Participant characteristics for weekly PA studies	24
Table 2.2 ANT mean reaction times for Study 1	28
Table 2.3 Flanker task reaction time means for Study 3.....	37
Table 3.1 Reported PA duration for each 24-hour questionnaire category	52
Table 3.2 Raw mean reaction time and accuracy for 24-hour PA study	54
Table 3.3 Descriptive characteristics of participants for exam study.....	63
Table 3.4 Standardized coefficients for fixed effects in exam study.....	65

List of Figures

Figure 2.1 ANT display sequence	26
Figure 2.2 Flanker display sequence.....	32
Figure 2.3 Task accuracy histograms for weekly PA Study 3	38
Figure 4.1 Example of typical vertical acceleration data from overground walking	76
Figure 4.2 Example autocorrelation of vertical acceleration	78
Figure 4.3 Gait parameters across different levels of cognitive load in Study 1	79
Figure 4.4 Examples of urban and nature photographs presented in Study 2	84
Figure 4.5 Checkpoint photos from Study 3. Map data: Google	88
Figure 4.6 Checkpoint photos from Study 4. Map data: Google	93
Figure 4.7 Fractal dimension values for all participants in both outdoor studies	94
Figure 4.8 Fractal dimension and step time in both outdoor studies	96
Figure 5.1 Example power curves for different effect sizes	109

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Dedication

To Sara, my Eurydice

1 Introduction

Physical activity (PA) levels have been decreasing worldwide, with further decline projected over the next decade (Gordon-Larsen et al., 2004; Ng & Popkin, 2012; Spittaels et al., 2012). Not only will this have negative consequences for physical health (Hamilton et al., 2007; Owen et al., 2010; Soares-Miranda et al., 2016) and emotional health (Bernstein & McNally, 2017; Hogan et al., 2013), it may also be detrimental to long-term brain health (Cabral et al., 2019; Perez et al., 2019) as increased PA has been shown to help maintain gray matter volume in the elderly (Erickson et al., 2010; Raji et al., 2016), and can reduce the risk of Alzheimer's disease and Parkinson's disease (Caciula et al., 2016; Chirles et al., 2017; Hamer & Chida, 2009). Additionally, from an economic perspective, the cost of treating obesity has been increasing and is currently one of the most economically expensive preventable diseases (Spieker & Pyzocha, 2016; Tremmel et al., 2017). As such, it has become increasingly important to promote increased levels of physical activity.

Fortunately, promotion of physical activity has been on the rise and we have seen different methods being used to encourage increased participation in PA. For example, the physical health benefits of PA have been known for decades and these are often used as encouragement by doctors and media outlets (Janssen & LeBlanc, 2010), Michelle Obama's 'Let's Move!' campaign was introduced as a way to tackle childhood obesity (Cappellano, 2011), and ParticipACTION is a Canadian initiative designed to increase overall levels of PA (Spence et al., 2018). We have also seen a rise in public interest in PA with the introduction of reward apps like Carrot (Mitchell et al., 2017; Souvaliotis, 2019) and Sweatcoin (Derlyatka et al., 2019), which provide a monetary incentive to taking part in PA. Sales of PA wristbands and PA-tracking smartwatches are also expected to rise over the next few years (IDC, 2018). These methods, however, have not been entirely effective given the decline in PA that has been projected over the next decade (Ng & Popkin, 2012). This raises the

question of why don't people exercise? The answer to this is not clear, but there is certainly a number of contributing factors such as low self-efficacy and lack of encouragement from peers (Wu et al., 2003), negative post-exercise affect might discourage future PA (P. D. Loprinzi et al., 2020), and the oft-cited physical health benefits might be too long-term as the decision to take part in PA is likely strongest if the reward is more concrete and tangible (Lynch Jr & Zauberman, 2006; Zauberman & Urminsky, 2016).

Researchers have investigated other ways to promote PA participation, and in a lot of cases the most effective methods focus on external motivations and benefits. For example, emphasizing different types of benefits might increase PA adoption, with many older adults reporting that they take part in PA as a way to contribute to research on prevention of Alzheimer's disease (Etnier et al., 2017). PA software applications that incorporate modern technologies, like social sharing (Vandelanotte et al., 2017) and goal-setting features (Jason Fanning et al., 2017), tend to increase participation in PA. Additionally, there is evidence that even non-health focused messaging can increase PA levels, as one of the most effective approaches is to provide financial rewards (Piepmeier et al., 2018). Providing financial incentives, however, is not sustainable in the long-term.

Another approach might be to message the *cognitive* benefits of PA. We already have an intuitive sense of the relationship between cognition and PA when using phrases "going for a run to clear your head". Another common phrase is "let's walk and talk", suggesting that even lower intensity activities like walking (and the gait kinematics associated with walking) might be related to cognition. In fact, we can see this relationship between cognition, PA, and gait behaviour develop across the lifespan through a series of U-shaped (and inverted U-shaped) patterns. As an example, cognitive performance and gait stability tend to follow an inverted U-shaped function across the lifespan, with both being low in children, high/stable in young adults, and decreasing again in older adulthood (Anderson,

2002; Iosa et al., 2014; H. Lee et al., 2012; B. R. Williams et al., 2005, 2007). PA benefits for cognition also shows a similar (albeit inverted) pattern, as the strongest effects are typically observed for children and older adults, and smallest effect sizes for young adults (Sebastian Ludyga et al., 2016; Oberste et al., 2019). Therefore, the promotion of cognitive benefits of PA might be a natural way forward given the underlying relationship that appears to exist between cognition, PA and gait across the lifespan. Before we can test the efficacy of this approach, however, we first need to understand the true relationship between PA and cognition, and currently the literature on the topic is quite divided in terms of findings (Diamond & Ling, 2016; Charles H. Hillman et al., 2018).

Additionally, if the eventual goal is to promote increased PA, it might be prudent to target the age group where PA participation shows the sharpest decline. Studies have shown a drop in PA levels between childhood and young adulthood (Gordon-Larsen et al., 2004; Ku et al., 2006), with young adults showing the lowest levels of PA, as well as the highest prevalence of sedentary behaviour, across multiple countries (Ku et al., 2006; Spittaels et al., 2012). It would seem, then, that curtailing overall PA decline might be best achieved by promoting and increasing PA in young adults.

My dissertation has three main goals. First, to understand the influence of potential moderators on the PA-cognition relationship in young adults, and whether these might be contributing to a divided literature; of primary interest are PA intensity and PA time window. Second, examine the bidirectional nature of the cognition-motion relationship, specifically, do changes in cognitive load lead to changes in low-intensity PA such as walking? Finally, I will be examining these relationships in both laboratory-based and natural real-world contexts, as it's not clear whether behaviour observed in the laboratory necessarily generalizes to more realistic settings. This is particularly important for the goal of PA

promotion as we want to understand how real-world behavior is being impacted, and to ensure the outcomes remain relatable and actionable.

1.1 Physical activity and cognition

There are several reasons why promoting benefits to cognitive function might be a good way to motivate increased physical activity. There is a general interest in cognitive improvement, as demonstrated by the number of brain training games available (Simons et al., 2016). Many people use caffeine as a cognitive performance booster but a recent study showed that acute PA can improve working memory performance to the same level that caffeine does (Morava et al., 2019); and the PA does not need to last long either as even short bouts can improve cognitive function (Gejl et al., 2018). Unlike physical health outcomes, cognitive benefits can be observed relatively quickly, making it less susceptible to temporal discounting effects due to the immediacy of the reward (Lynch Jr & Zauberman, 2006). Finally, improved cognition can also mean better performance at school (Álvarez-Bueno et al., 2020), which is particularly relevant for young adult populations.

In terms of cognitive outcomes, while some have studied neuroimaging effects (Gutmann et al., 2015; Stillman et al., 2019), in this dissertation I focus purely on behavioural outcomes. While neuroimaging effects are no doubt important, they are also less applicable to real-world settings, which makes those results harder to sell when trying to promote PA. Behavioural effects are more direct and actionable. Furthermore, in some cases there is inconsistency between behavioural and neuroimaging results, where no behavioural effect is found while the neuroimaging test is significant (Kamijo & Takeda, 2013; Wagner et al., 2017), so the presence of an imaging effect does not guarantee a behavioural finding (or vice versa). There could be several reasons for this. For example, the PA effect might be too small to detect behaviourally and a more sensitive measure is needed, or it could be that neuroimaging and behaviour are measuring different constructs

(Coen et al., 2011; Diamond & Ling, 2018). Regardless of the exact reason, the evidence suggests that the measurement method used can lead to differing results, so I will examine only behavioural outcomes to keep the findings as relatable as possible.

There are many mechanisms through which PA can improve cognitive function such as improving cerebral oxygenation, increasing concentration of neurotrophins, and increasing availability of neurotransmitters (Mandolesi et al., 2018; Marmeleira, 2013). These changes lead to observable improvements across a variety of outcomes, including improved language skills (Segaert et al., 2018), academic performance (C.H. Hillman et al., 2009), memory (P. D. Loprinzi, 2019), mind wandering (Fenesi et al., 2018), sensory discrimination (Lambourne et al., 2010), and reaction speed (Gejl et al., 2018).

Most of the evidence in the literature, however, comes from the study of executive function (EF). EFs are high-level functions that play an integrative and regulatory role on other basic cognitive functions, and includes the ability to maintain working memory, inhibit task-irrelevant information, make plans, and switch between information sets (Etnier & Chang, 2009). These functions are correlated, uniquely identifiable (Miyake & Friedman, 2012), and develop/mature in stages throughout childhood (Anderson, 2002). Given this, it's possible that immaturity of EFs during childhood is what results in the large PA effects we typically see in children (Álvarez-Bueno et al., 2020; Sebastian Ludyga et al., 2016; Oberste et al., 2019). EF is also the most studied cognitive domain in older adults and shows the largest and most consistent effects (Gomes-Osman et al., 2018). This is likely because advancing age has the greatest impact on frontal brain regions, which is the area we typically associate with EFs (Davis et al., 2009; O'Sullivan et al., 2001). In terms of EF-specific behavioural outcomes, PA has been shown to improve working memory (Hogan et al., 2013; Lo Bue-Estes et al., 2008; Tomporowski et al., 2005), as well as performance on various inhibitory control tasks like the Attention Network Test (Chang, Pesce, et al., 2015;

Pérez et al., 2014), Flanker task (Kamijo et al., 2007; Kao et al., 2018; Kubesch et al., 2009; Sebastian Ludyga et al., 2018), and Stroop task (Chang et al., 2017; Faulkner et al., 2016; Harveson et al., 2016; Park & Etnier, 2019; Salas-Gomez et al., 2020; C.-C. Wang et al., 2019; Yanagisawa et al., 2010).

However, findings in the young adult PA literature are also quite mixed, with several studies showing small or non-significant effects of increased PA. For example, a recent meta-analysis showed that the relationship between aerobic fitness and academic achievement is much smaller in young adults than it is in children (Álvarez-Bueno et al., 2020), there's no difference between active and non-active young adults on any EFs (Boucard et al., 2012), and no relationship observed using either objective or subjective PA measures (P. D. Loprinzi & Kane, 2015; Pindus et al., 2015). The Flanker and Stroop tasks, which have shown significant effects as previously described, sometimes show null results as well (Alderman et al., 2014; Zimmer et al., 2016). One study found no behavioural effects when looking at reaction time or accuracy, but did see an effect when looking at response variability (Kao et al., 2020), which suggests that even if a PA-cognition relationship does exist, it might require looking beyond basic mean reaction times to observe (Gooderham et al., 2020). This mix of findings could be due to a number of moderators that have been identified across multiple age groups, such as participant age itself (Charles H. Hillman et al., 2002, 2006; Leckie et al., 2014; Oberste et al., 2019; Jason R. Themanson et al., 2006), sex of the participant (Coleman et al., 2018; Drollette et al., 2016), cognitive task type (Etnier & Chang, 2009; Sibley et al., 2006; C.-C. Wang et al., 2015), task difficulty (Kamijo & Takeda, 2010, 2013), method of PA assessment (Marques et al., 2018), PA duration (Chang, Chu, et al., 2015), and PA intensity (Oberste et al., 2019; Smith et al., 2016; Vanhelst et al., 2016; Wohlwend et al., 2017). Of particular interest in this dissertation are the potential effects of PA intensity and PA time window on the PA-cognition relationship.

1.1.1 *Physical activity intensity*

In addition to duration, it is also important to measure PA intensity as it affects the amount of energy expended when performing activity. An intuitive example of this is walking and running; you wouldn't expect 30 minutes of walking to necessarily confer the same benefits as 30 minutes of running due to differences in intensities and energy requirements. The benefits of PA on cognition, therefore, may depend on the intensity at which the activity was performed, and not considering this factor may be the cause of some of the mixed results in the literature. We often think of PA intensity and cognitive benefits as having an inverted-U relationship; with the benefit being maximal at moderate PA intensity, and minimal at low and vigorous intensities (Kamijo et al., 2004, 2007). The literature is generally supportive of this pattern (although the intensity boundaries can be a little blurry) as moderate intensity PA tends to result in improved attention capacity (Vanhelst et al., 2016) and inhibitory control (Endo et al., 2013; Kamijo et al., 2004, 2007), whereas vigorous PA tends to impair performance (Labelle et al., 2013, 2014; McMorris et al., 2009; Smith et al., 2016; C.-C. Wang et al., 2013). However, although the evidence provided thus far suggests an inverted-U relationship, this is not always the case as high-intensity PA has also been shown to improve performance on learning and memory-based tasks (Dilley et al., 2019; Etnier et al., 2016; Hötting et al., 2016; Winter et al., 2007), so the moderation may be domain dependent. Given that the majority of the studies described have looked at long-term or acute PA, it's not entirely clear whether this intensity effect holds at more intermediate time windows such as weekly PA. Therefore, Chapter 2 will explore intensity effects over the 1-week period prior to cognitive testing.

1.1.2 *Physical activity time window*

Another potential moderator of the PA-cognition relationship is PA time window, which I define here as the period of time from which PA participation is assessed, relative to

cognitive testing. For example, some researchers assess (or manipulate) PA levels immediately prior to cognitive testing (acute time window), whereas others may be interested in PA levels over the 5-10 years prior to testing (long-term time window). Time window is a relatively understudied moderator (within a single study) as it is a difficult factor to manipulate. The impact of PA on cognition has most frequently been assessed at two separate time windows: acute (Chang et al., 2017; Samani & Heath, 2018; Spitzer & Furtner, 2016) and long-term (Pérez et al., 2014; Polich & Lardon, 1997). Many models have been proposed to explain acute and long-term PA effects: cardiovascular fitness hypothesis, adaptive capacity model, cerebral oxygenation, neurotrophin and neurotransmitter models, energetic models like the inverted-U intensity hypothesis, transient hypofrontality and the reticular-activating hypofrontality models (Dietrich & Audiffren, 2011; Mandolesi et al., 2018; Marmeleira, 2013; Raichlen & Alexander, 2017). The invocation of different models to explain long-term and acute PA effects is suggestive of underlying differences in PA time windows, and there is evidence in the literature to support this. In older adults, for example, long duration PA interventions tend to be associated with structural changes in the brain, whereas shorter interventions tend to result in hemodynamic changes instead (Cabral et al., 2019). Possibly the most compelling evidence in support of time window differences is the transient hypofrontality hypothesis, which shows that concurrent PA is detrimental to executive function as the brain must redistribute finite resources to more important motor control regions during activity (Del Giorgio et al., 2010; Dietrich, 2003, 2006; Dietrich & Sparling, 2004; Smith et al., 2016; C.-C. Wang et al., 2013). However, once you widen the time window and look at acute PA, we start to see the opposite effect where PA is actually beneficial to executive function (Lambourne & Tomporowski, 2010).

Neurochemical studies also support the idea of time window differences. While the focus of this dissertation is not on neurochemical mechanisms or outcomes, a brief

discussion is warranted to highlight neurochemical differences at each time window, which may then give rise to differences in observable behavior. Neurochemical compounds like brain-derived neurotrophic factor (BDNF) and lactate have been linked to executive function (Córdova et al., 2009; Hwang et al., 2017; Leckie et al., 2014). Importantly, the regulation of these neurochemicals can differ at each PA time window. For example, BDNF has been shown to increase following an acute bout of PA, however, the increase is short-lived and is only seen immediately following PA, with the levels decreasing again after 30-minutes post-exercise (Griffin et al., 2011). Central and peripheral lactate levels are also increased in young adults following acute PA (Dennis et al., 2015; Maddock et al., 2011), but this increase is likely to be transient because lactate is most readily observed during, and immediately following, PA as it is the natural product of aerobic and anaerobic glycolysis (Basso & Suzuki, 2017).

Unlike long-term and acute PA, intermediate time windows (such as PA over the past week) are rarely studied (Verburgh et al., 2014), and it's unclear what mechanisms are responsible for PA benefits because existing models don't make any claims about time windows between long-term and acute. Intermediate time windows are particularly valuable from a PA promotion perspective as the benefits may be close enough in time to motivate action, whereas long-term PA benefits might be too susceptible to temporal discounting and acute PA benefits don't allow much time to act (imagine having to go for a run 5 minutes before an exam!). To that end, Chapter 2 will examine PA benefits at the weekly time window, while Chapter 3 will look at PA during the day prior to cognitive testing.

1.2 A bidirectional relationship

The relationship between cognition and PA (and motion more generally) is likely to be bidirectional. In other words, just as PA can affect cognition, changes in cognition can also affect the way we move. Specifically, I'm interested in how changes in cognitive load

can affect the performance of low-intensity physical activities such as walking. From a physiological perspective, central pattern generators, which are a network of neurons in the spine, can cause rhythmic motion of the legs when stimulated (Guertin, 2009). These circuits can give rise to basic gait-like behaviour, and gait adjustments are then introduced as a way to minimize energetic cost during ambulation (Bertram & Ruina, 2001; Holt et al., 1995). However, this is not enough to explain the complexities of gait as energy conservation is not the sole reason for gait and postural changes.

It was originally thought that older adults are susceptible to falls due to deterioration of sensorimotor systems, and while we certainly see gait decline in populations with motor control issues like Parkinson's and Huntington's disease (Hausdorff et al., 2000), this is an overly simplistic view as cognitive function also plays a role in regulating mobility (Lacour et al., 2008). In fact, cognitive impairment is a better predictor of fall frequency than physical impairments (Langeard et al., 2019). As such, higher level cognitive function has been suggested as a moderator of gait and postural changes and there are many examples of this in the wider literature. For example, it has been shown that gait is highly dependent on cognitive function (Hausdorff et al., 2005; Woollacott & Shumway-Cook, 2002), and even simple tasks like standing requires attentional resources (Lajoie et al., 1993). Older adults with cognitive impairment are likely to also have impaired gait (T. Y. Liu-Ambrose et al., 2008; Verghese et al., 2008) and this coupling is often seen as the cause of mobility-related issues such as falling in older adults (Axer et al., 2010; O. Beauchet et al., 2009; Lundin-Olsson et al., 1997; Montero-Odasso et al., 2012, 2012; Nagamatsu et al., 2011, 2016; Perrochon & Kemoun, 2014; Watson et al., 2010; Yogev-Seligmann et al., 2012). Gait speed has also been used as an indicator of future development of mild cognitive impairment, and gait speed decline can even be seen as early as 12 years before onset of any cognitive deficit (Buracchio et al., 2010). This effect is not only seen in older clinical populations, but in

otherwise healthy older adults as well during dual task experiments. Gait speed, step regularity and stride regularity have all been shown to reduce during dual task walking, which imposes a higher level of cognitive load (Alexander et al., 2005; Hausdorff et al., 2008; Hillel et al., 2019). Obstacle avoidance abilities are also worsened under these higher load conditions, with an increased chance of missteps during complex walking tasks (H.-C. Chen et al., 1996; Schaefer et al., 2015).

The negative consequences of increased cognitive load on gait are particularly relevant in the modern world, where cognitive distractions can come in many forms. One prime example of this is concurrent cellphone use while walking, which can lead to altered gait patterns (Magnani et al., 2017; Schabrun et al., 2014). Specifically, texting while walking can decrease gait speed and destabilize gait by increasing gait variability (Crowley et al., 2019; Lopresti-Goodman et al., 2012; Mendelsohn & Zerpa, 2020). High cognitive load while walking can be quite dangerous as concurrent cellphone use has resulted in an increase in pedestrian injuries in recent years, and has exceeded phone-use related driving injuries (Nasar & Troyer, 2013). It can also increase the number of collisions with other pedestrians (Souza Silva et al., 2019) and lead to fewer successful street crossings in a simulated environment (Banducci et al., 2016; Neider et al., 2011). Recent work has shown that these altered gait patterns under cognitive load can be detected using modern machine learning techniques (Alharthi et al., 2019, 2020), and the success of these techniques might provide a way to automatically identify dangerous walking behaviour in the future.

1.2.1 Executive function and gait

Given the vast literature on older adult mobility and falling issues, it would be tempting to think that altered gait is age specific, with advancing age being the primary cause. However, it has been demonstrated that gait alterations are more the result of poor executive function rather than strictly age (Yogev-Seligmann et al., 2008); this is not

particularly surprising given the association between executive function and physical activity, as discussed earlier in the chapter. Neuroimaging evidence supports the idea that gait and executive function are intertwined, showing greater activation of prefrontal regions during avoidance of obstacles while walking (Maidan et al., 2018). In older adults, we can see the correlation between gait and executive function behaviourally as well (Olivier Beauchet et al., 2012; N. P. Gothe et al., 2014). For example, although gait is a rhythmic task, measures of gait dynamics are more correlated with performance on executive function tasks than rhythmic motor tapping tasks (Hausdorff et al., 2005), those with poor executive function performance tend to walk slower under dual task conditions (Hobert et al., 2011), other gait parameters like swing time variability are also correlated with executive function task performance under increased cognitive load (Hausdorff et al., 2008; Springer et al., 2006), and an effective way to treat mobility decline is to improve executive function performance (Montero-Odasso et al., 2012).

Of course, a significant relationship in older adult populations doesn't rule out an age effect, but we can see the same effect in young adults as well. These effects are, again, most prominently seen in dual task walking studies given that most of the cognitive tasks used in these setups are designed to tax executive resources. Young adults show a similar pattern of results as older adults, showing reduced gait speed and stride length under cognitive load (Ebersbach et al., 1995; Nadkarni et al., 2010), as well as increased gait variability (Olivier Beauchet et al., 2005; Szturm et al., 2013). These effects have also been observed using more realistic cognitive manipulations like concurrent cellphone use (Magnani et al., 2017; Mendelsohn & Zerpa, 2020).

Most of the studies described thus far have been laboratory-based experiments, but it is also extremely common for dual tasking to occur in daily life: walking while talking to a friend, walking and talking on the phone, or walking while listening to music. Given the

prevalence of real-world dual tasking, it would be important to study this relationship in more natural environments outside of the laboratory. The ability to do so could lead to some interesting research questions and potential applications. For example, given the association between gait and cognitive load, we might be able to use measures of gait dynamics as a proxy for the cognitive demand of an outdoor environment, which may help us understand why certain environments are more distracting or cognitively challenging. Additionally, the destabilization of gait while under increased cognitive load can make walking in certain environments more dangerous. Unstable gait at a crosswalk or near an intersection can be very costly, but it is currently unclear how load can affect gait in real-world situations like these due to a lack of adequate outdoor measurement tools.

1.2.2 Outdoor walking

While there is a lot of evidence to show the impact of cognitive load on walking in young adults, the vast majority of those studies have been conducted in artificial environments, and the results may not generalize to the real world. Walking in a laboratory is not the same as walking in natural outdoor environments as there are many factors outdoors that can affect gait dynamics.

Treadmill walking has been found to be different from overground walking, and can produce differences in gait speed, smoothness, and rhythmicity (Carpinella et al., 2010; Dingwell et al., 2001; Row Lazzarini & Kataras, 2016; J.G. Wrightson & Smeeton, 2017). During dual tasks, young adults tend to prioritize performance on the cognitive task rather than the walking task, but this effect is dependent on walking modality, which can lead to situations where a dual task effect is found during overground walking but not during treadmill walking (Simoni et al., 2013; James G. Wrightson et al., 2019; J.G. Wrightson & Smeeton, 2017). These differences likely arise due to the fixed speed of treadmill walking, which places an upper limit on gait variability as you risk falling if your gait is too different

from the constant speed of the treadmill. Gait variability is also best measured over long distances (over 20 meters), which can be problematic in indoor walking studies where a treadmill is not used (Lord et al., 2011).

Differences in gait speed and gait regularity have been observed between laboratory walking and normal day-to-day walking in older adults (Hillel et al., 2019), supporting the idea that laboratory-based walking studies may not generalize to the real world. Outdoor environments also contain different sources of cognitive load and different types of distracting stimuli. The visual environment itself contains many elements that can affect the availability of attentional resources, stress levels, and overall cognitive performance: natural versus urban environments (Berman et al., 2008; Stevenson et al., 2019), presence of greenery (Wells, 2000), differences in biodiversity (Carrus et al., 2015). These factors are inherent to natural environments, but typically not manipulated in laboratory environments. In fact, even simple eye movements can affect gait and balance (Hunter & Hoffman, 2001), and gaze shifts are used to guide foot placement while walking (Matthis et al., 2018). Natural, outdoor environments demand an increased need to overtly shift gaze due to the presence of obstacles and hazards (compared to the relative safety of the laboratory), and this alone might be enough to cause differences between indoor and outdoor walking. Many have attempted to use virtual reality to mimic natural environments, but this is not quite the same as being at a real intersection where unstable gait can actually be quite dangerous (Banducci et al., 2016; Chaddock et al., 2012; Nagamatsu et al., 2011; Neider et al., 2011; Souza Silva et al., 2019).

Due to the limited research on outdoor walking, it remains unclear how cognitive load affects gait outdoors, or how the environment itself might influence the way we walk. This raises the question of whether the cognitive load effects observed in young (and older) adults can generalize to outdoor walking, or in what ways the results might change. The

impact of load in outdoor environments is clearly important to study as, for example, it would allow us to monitor gait in dangerous places like roads and intersections, but we currently lack adequate measurement tools to study gait in the real world. Some have implemented advanced exoskeletons for measuring outdoor gait, but this is cost-prohibitive and difficult to scale (Matthis et al., 2018). Cheaper methods involve the use of depth sensors, but these techniques only work over small capture volumes (Dawe et al., 2019). The simplest approach is to use dedicated accelerometers to identify step parameters directly from acceleration data, but these devices are still considered specialty hardware (Samà et al., 2011). In an ideal world we could use technology that is cheap, portable, and widely available, as it would reduce the barrier to entry for studying outdoor gait. Fortunately, modern smartphones are essentially portable accelerometers; they also have the benefit of being widely available and can be used for other tasks like taking photos of the walking environment or presenting a concurrent cognitive task while walking. Development of such a tool would allow us to directly compare indoor and outdoor walking. Chapter 4 focuses on the intersection between cognitive load, gait dynamics, and technology, and aims to find a solution to outdoor gait assessment in young adults.

1.3 Real-world physical activity and cognition

Naturally occurring physical activity and real-world cognitive outcomes are important for promoting increased PA as the contexts are relatable and findings are likely more applicable. PA that has been manipulated in a laboratory for the purpose of an experiment, however, is likely not the same as PA that occurs more naturally, as in the real world we may walk at variable speeds, switch between running intensities, or we might take breaks every few minutes. Additionally, computer tasks are not the same as real-world tests of cognition (Paap et al., 2020), although it should be noted that some have shown correlations between laboratory neuroimaging measures and real-world academic achievement (Charles

H. Hillman et al., 2012). In the same way that neuroimaging effects aren't directly actionable, or even necessarily understandable by the general public, typical computerized tasks also have less applicability due to the narrower scope of what they're measuring. In the literature we often run into the issue where both sides of the equation are manipulated, artificial, or have low ecological validity, as the PA performed and cognitive performance measured don't necessarily represent human behaviour as it occurs in the real world. Although, it is entirely understandable why this is the norm, as laboratory-based manipulations provide a level of control that allowed causality to be inferred and statistical noise to be reduced. These methodological choices lead to the following questions: is PA still associated with cognition if the PA occurs naturally rather than manipulated? And do PA benefits extend to a real-world measure of cognitive function? To address these questions, Chapters 2, 3, and 4 examines PA that occurs in the real world (natural unstructured PA and outdoor walking), while Chapter 3 also assesses cognitive performance using a real university exam.

1.4 Dissertation overview

My dissertation examines the relationship between cognition and motion from the perspectives of physical activity and physical mobility, with a focus on the following questions:

1. How does physical activity affect cognitive function (specifically executive function), and are there any moderators of that relationship?
2. How does cognitive load affect low-intensity physical activities like walking?
3. Are these effects observable when measured in more natural environments? For example, real world cognitive performance and naturally occurring physical activity outside of the laboratory.

To address these questions, Chapters 2 and 3 discuss potential moderators of the PA-cognition relationship, specifically PA intensity and PA time window. Chapter 4 demonstrates a smartphone-based methodology for gait assessment and examines the impact of increased cognitive load on indoor and outdoor walking. From the perspective of PA promotion, it is important to understand these effects in terms of real-world settings and outcomes to maintain relatability and applicability. To that end, the PA measured in Chapters 2 and 3 are naturally occurring (i.e., free-living PA) rather than manipulated. Additionally, Chapter 3 examines a real-world cognitive outcome that is of direct relevance to the target population of young adults, namely a university examination. Finally, the smartphone-based gait assessment method discussed in Chapter 4 is designed primarily with the goal of measuring gait outdoors, in the real world, and outside of the laboratory.

In terms of research design, a correlational and cross-sectional approach will be the most appropriate for the PA studies in Chapters 2 and 3. Although traditional experiments would allow us to infer the direction of causality due to the presence of a manipulation, manipulating PA in that fashion is directly counter to the goal of understanding PA as it occurs naturally. A mix of experimental and correlational approaches will be used in Chapter 4. An experiment is ideal for indoor walking where cognitive load levels can be directly manipulated and would give a clear sense of how changing levels of load can affect gait. In outdoor environments where load levels cannot be tightly controlled, we will need to turn to correlations once again. It could be argued that a quasi-experimental approach could be used, where we first categorize environments by their level of cognitive load, but artificial grouping in this manner is not ideal as it reduces variance in the measure and makes it more difficult to find true effects, especially when load can be measured on a continuous scale instead.

2 Weekly physical activity and cognition

In general, research studying the relationship between physical activity (PA) and cognitive function in healthy young adults falls into two broad categories: the study of near-term or single/acute effects of PA, and the effects of more long-term or intervention-style PA. While these categories capture the timing of PA relative to cognitive testing, the dichotomy begins to break down when considering more intermediate timescales that are longer than single bouts but that do not necessarily qualify as long-term. This is an important issue because the impact of PA on cognition has been shown to be quite varied depending on when the PA occurred (Lambourne & Tomporowski, 2010). For example, acute PA that occurred immediately prior to cognitive assessment has been shown to improve cognitive function (Chang et al., 2017; Samani & Heath, 2018; Spitzer & Furtner, 2016), but when cognition is assessed *during* an acute bout of PA, the impacts appear to be negative (Dietrich, 2003; Dietrich & Sparling, 2004; Soga et al., 2015). At longer timescales, while increased voluntary PA over the past 3 and 10 years has a positive impact on cognitive function (Pérez et al., 2014; Polich & Lardon, 1997), findings from shorter PA interventions in young adults (3 weeks to 2 months) have produced more equivocal results (Gourgouvelis et al., 2018; Griffin et al., 2011; Stroth, Hille, et al., 2009). Whereas mixed results can be found within each temporal category, these findings highlight the importance of examining PA timescales separately as they may have different effects on cognitive function.

From a public health and physical activity promotion perspective, PA over the past 7 days may be a useful timescale to study. Whereas studies have shown a cognitive benefit of short-term (acute) PA (Chang et al., 2017; Samani & Heath, 2018; Spitzer & Furtner, 2016), it is not always possible to exercise immediately prior to a cognitive task. Similarly, promoting the cognitive benefits of long-term PA may not be an effective way to increase overall activity levels either, as it can be difficult to remain disciplined enough to exercise

consistently for months prior to a cognitive task. 7-day PA is a middle ground between long-term and acute timescales, where individuals have control over their PA levels and general lifestyle choices and may still be motivated enough to exercise consistently throughout the week. However, recent meta-analyses show that the majority of young adult studies examining the relationship between PA and cognition have looked at acute timescales, with few assessing the impact of long-term PA, and even fewer looking at 7-day PA (Roig et al., 2013; Verburgh et al., 2014). In the present study we will be focusing specifically on free-living (voluntary and self-initiated) activities over the past 7-days, which we will refer to simply as free-living PA.

The impact of free-living PA on cognition has been studied objectively using accelerometers, showing improved attention capacity to be associated with longer time spent in moderate-intensity PA (Vanhelst et al., 2016), however, free-living PA can also be assessed via subjective self-report. Questionnaires have benefits over objective methods as they can capture activities that cannot typically be measured using accelerometry, such as cycling and swimming. A number of self-report studies have shown that increased PA time over the past week is associated with improved executive control (Kamijo & Takeda, 2009, 2010) and response monitoring (Kamijo & Takeda, 2013). It is important to note, however, that many studies using self-report free-living measures only assess the frequency of activity, or amount of time spent being active (Berchicci et al., 2013; Guiney et al., 2015; Kamijo et al., 2011; Kamijo & Takeda, 2009, 2010, 2013; O'Connor et al., 2015). While PA duration is important, it is unlikely to fully capture the intricacies of recent activity. For example, 30 minutes of light walking is likely to have a different impact on cognition than 30 minutes of high-intensity interval training due to differences in PA intensity and energy expenditure. Research on acute and concurrent PA supports this idea, for example, cognitive performance has been shown to change in an inverted-U fashion as a function of

PA intensity, with attentional resource allocation increased following medium-intensity PA, and decreased after high-intensity PA (Kamijo et al., 2004, 2007). Similar findings have been observed during concurrent PA, with high-intensity PA resulting in longer reaction times and higher error rates on a variety of executive control tasks (Labelle et al., 2013, 2014; Smith et al., 2016; C.-C. Wang et al., 2013). That PA intensities have differential effects on cognition underscores the importance of considering both duration and intensity when examining free-living PA. Furthermore, whereas free-living PA studies in young adults have examined a variety of cognitive domains, such as implicit learning (Stillman et al., 2016) and memory (P. D. Loprinzi & Kane, 2015; Stillman et al., 2016), the majority of work has focused on executive control. Behavioral and neuroelectric indices of improved executive control, defined as the ability to purposefully inhibit automatic responses (Miyake et al., 2000), have been positively correlated with higher levels of weekly PA (Guiney et al., 2015; Kamijo & Takeda, 2010, 2013; Stillman et al., 2016). However, as most free-living PA studies generally do not examine the impact of PA intensity, it remains unclear whether these benefits remain once the intensity of PA has been accounted for.

The goal of the present study is to examine the relationship between free-living PA over the past 7 days and executive control in young adults, while taking into consideration the energy requirements of the various activities performed throughout the week. Research on free-living effects and PA intensity lead to the prediction that increased moderate-intensity PA should have a positive association with performance, with little-to-no effect of low or vigorous intensity PA. Tasks requiring executive control are an ideal starting point for examining free-living effects as they have been associated with PA levels at other timescales (Charles H. Hillman et al., 2003; Sebastian Ludyga et al., 2018; Pontifex & Hillman, 2007), and PA has been found to benefit executive function more than other cognitive domains (Colcombe & Kramer, 2003). To that end, free-living PA was assessed

across three studies using self-report questionnaires, and PA intensities were then tested for their association with performance on executive control tasks.

2.1 Study 1

We began by assessing the relationship between free-living PA and cognition using the Attention Network Test, which is designed to examine three facets of attention: alerting, orienting, and executive control. The goal was to understand which form of attention was most associated with weekly PA at different intensities.

2.1.1 Ethics statement

Ethical approval was received through the University of British Columbia's Behavioural Research Ethics Board, and written informed consent was obtained from each participant prior to the start of the study.

2.1.2 Participants

We conducted a two-tail power analysis, using a small effect size estimate of 0.20, to determine minimum sample size for the study. Only a handful of studies have examined the relationship between free-living PA and behavioral measures of executive control in young adults, and they estimate the effect size to be in the range of 0.34-0.38 (Galioto Wiedemann et al., 2014; Guiney et al., 2015; Stillman et al., 2016). Due to the paucity of research on this topic, we also turned to a couple of key meta-analyses to determine our effect size target for power analysis. Those meta-analyses show that the relationship between acute PA and cognitive performance (e.g. executive control, working memory) has a mean effect size of 0.20 in young adults (Lambourne & Tomporowski, 2010), but can be as high as 1.24 for cross-sectional PA studies depending on the cognitive test used (Etnier et al., 1997). Therefore, our choice of 0.20 should be considered a conservative lower bound estimate of the population effect size, chosen to minimize the likelihood of being underpowered. The

analysis indicated that a minimum of 193 participants would be needed to achieve 80% power. A total of 267 participants were recruited from undergraduate psychology courses at the University of British Columbia and received course credit for their time. Participants were eligible to take part in the study if they were young adults (under 45 years of age) and physically able to take part in PA. Three participants were excluded due to computer problems that prevented data collection, and two participants were excluded for vision-related issues (e.g. recent surgery). Our final sample consisted of 262 participants (mean age = 20.44, $SD = 2.65$, 138 male).

2.1.3 Apparatus

All tasks and questionnaires were displayed using a 19" LCD monitor with a resolution of 1280x1024. Data collection for the computer tasks was conducted using the open-source Cognitive Battery 3.2 software package (Ho, 2017), which utilizes Python 3.6.4 and Pygame 1.9.3 for stimulus display. The primary operating system was Windows 7. The details of the individual tasks are described for each study separately.

2.1.4 International Physical Activity Questionnaire

At the start of the session, participants completed the self-administered long-form of the International Physical Activity Questionnaire (IPAQ; Appendix A), which measured self-reported physical activity (PA) over the past week. The IPAQ has high reliability and validity when monitoring physical activity across diverse populations (Bauman et al., 2009; Craig et al., 2003; Vandelanotte et al., 2005), and has been used to study a wide range of cognitive outcomes such as academic achievement (Marques et al., 2017; Pellicer-Chenoll et al., 2015), response monitoring (Kamijo & Takeda, 2013), spatial priming (Kamijo & Takeda, 2009), task switching (Kamijo & Takeda, 2010), and functional and structural brain connectivity (Kamijo et al., 2011; Kim et al., 2016). The long-form was chosen because the

short-form of the IPAQ shows low correlation with objective measures and typically overestimates activity levels (P. H. Lee et al., 2011).

The IPAQ long-form assesses physical activity undertaken in the past week across several domains, including leisure time PA, domestic activities, work-related PA, and transportation-related PA. Activities of different intensities are reported for each domain. Weekly duration estimates are calculated by multiplying time spent in a typical day by number of days spent in the past week performing that activity. Weekly durations are then multiplied by MET values for the different activities (Ainsworth et al., 2000) to calculate MET-minutes per week for each intensity level. METs are multiples of resting metabolic rate, and multiplying activity duration by a MET value effectively weights the activity by the energy required to perform it. After data aggregation, the IPAQ reports MET-minutes per week for three intensities: low, moderate, and vigorous, which are estimates of energy expended over the past week performing activities at those intensity levels. Additionally, the IPAQ provides a total PA measure, which captures MET-minutes per week regardless of intensity. For a detailed explanation of the scoring process, see the official IPAQ scoring protocol (The IPAQ Group, 2005). Some researchers have used an alternate scoring method for the IPAQ that categorizes individuals as sedentary or active based on recommended PA levels by the American College of Sports Medicine (ACSM). Active participants were those that had ≥ 5 days/week of moderate-intensity PA or ≥ 3 days/week of vigorous-intensity PA, whereas sedentary participants had ≤ 2 days/week of moderate- or vigorous-intensity PA (American College of Sports Medicine, 2017; Kamijo et al., 2011; Kamijo & Takeda, 2010, 2013). Our analyses include this alternate scoring method for comparison purposes. Overall participant PA information can be found in Table 2.1.

Table 2.1 Participant characteristics for weekly PA studies

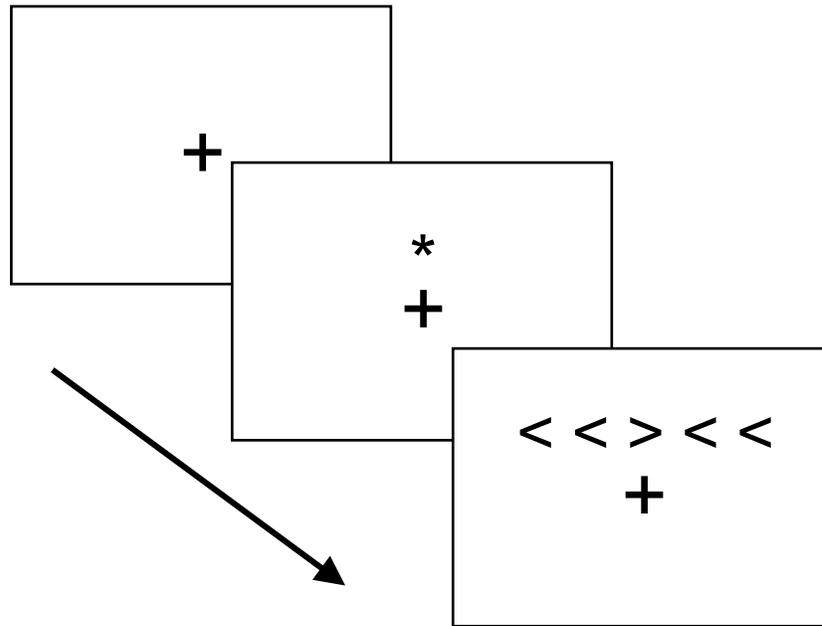
	Study 1	Study 2	Study 3
Sample size	262	218	206
Age mean (SD)	20.44 (2.65)	20.11 (1.87)	20.33 (2.71)
Sex (M/F)	138 / 124	48 / 170	51 / 155
Transportation PA means (SD)			
Raw minutes	241.84 (325.15)	372.01 (331.02)	394.72 (374.44)
MET-mins	832.81 (1087.96)	1273.95 (1121.32)	1344.11 (1245.65)
Domestic PA means (SD)			
Raw minutes	146.52 (259.52)	164.23 (338.40)	170.49 (255.84)
MET-mins	561.67 (1017.97)	604.82 (1183.62)	639.19 (990.99)
Work PA means (SD)			
Raw minutes	163.06 (330.83)	247.57 (513.81)	266.64 (539.62)
MET-mins	699.16 (1441.33)	1087.67 (2310.58)	1139.95 (2349.81)
Leisure PA means (SD)			
Raw minutes	276.78 (315.77)	284.11 (309.91)	314.21 (491.92)
MET-mins	1635.05 (1943.48)	1534.79 (1834.05)	1820.48 (3015.97)
Low Intensity PA means (SD)			
Raw minutes	388.72 (413.90)	617.52 (538.32)	629.13 (606.08)
MET-mins	1282.78 (1365.87)	2037.83 (1776.44)	2076.12 (2000.06)
Moderate Intensity PA means (SD)			
Raw minutes	267.80 (350.67)	280.49 (391.24)	313.95 (385.02)
MET-mins	1072.53 (1411.67)	1104.16 (1456.33)	1243.81 (1551.68)
Vigorous Intensity PA means (SD)			
Raw minutes	171.67 (222.78)	169.90 (257.26)	202.97 (341.64)
MET-mins	1373.39 (1782.20)	1359.24 (2058.08)	1623.80 (2733.12)
Total PA means (SD)			
Raw minutes	828.19 (663.88)	1067.91 (802.69)	1146.05 (936.11)
MET-mins	3728.70 (3045.97)	4501.22 (3437.42)	4943.73 (4399.06)

2.1.5 Attention Network Test (ANT)

After the IPAQ, participants completed the Attention Network Test (Fan et al., 2002), which is designed to measure three aspects of attention: the alerting task measures the ability to maintain a vigilant and alert state during continuous performance; the orienting task measures the ability to select information at different spatial locations; and the executive control task measures conflict resolution and the ability to inhibit task-irrelevant information (Petersen & Posner, 2012; Posner & Petersen, 1990). Each trial began with a fixation cross in the center of the screen, displayed for 400-1600ms. Next, a cue (an asterisk) was displayed for 100ms. Four cue types were utilized to capture different aspects of the attentional function: 1) no cue, where the fixation cross remained unchanged and thus the participant was not warned about stimulus onset; 2) central cue, where the fixation cross was replaced by the asterisk; 3) spatial cue, where an asterisk appeared at the location of the upcoming stimuli (either above or below fixation); and 4) double cue, where asterisks appeared both above and below fixation, informing the participant of upcoming stimuli but providing no information about the location. Flanker arrows (five horizontal lines with arrowheads) were shown 400ms after the cue and remained on screen until the participant made a response or 1700ms had elapsed. Participants were simply asked to use the left and right arrows keys on the keyboard to indicate the pointing direction of the central arrow. Arrow directions could either be leftward-congruent (all arrows pointing to the left), leftward-incongruent (center arrow pointing left, flanking arrows pointing right), rightward-congruent (all arrows pointing to the right), or rightward-incongruent (center arrow pointing right, flanking arrows pointing left). A neutral condition was also shown, where the central arrow was flanked by plain horizontal lines (no arrowheads). All cue and arrow conditions were equiprobable and presentation order was randomized. A dynamic fixation period (intertrial interval) followed the keyboard response, the duration of which was calculated as 3500ms

minus the response time minus the duration of the initial fixation, as per the design of the original ANT task (Fan et al., 2002). See Figure 2.1 for an illustration of the ANT display sequence.

Figure 2.1 ANT display sequence



Participants began with a practice block of 24 trials, and the main task consisted of three blocks, each with 96 trials. Alerting performance was calculated as the difference between the double cue and no cue conditions, orienting performance was the difference between spatial and center cue conditions, and finally, the executive control performance was the difference between congruent and incongruent arrow conditions. These difference scores were normalized by dividing the difference by the faster of the two conditions, for example, the executive control score was divided by the individual's mean congruent reaction time. This process places the reaction time in the metric of, in this executive control example, the congruent condition and makes the value more interpretable. The normalized conflict score is interpreted as the magnitude to which the person's incongruent responses

were slower relative to their baseline congruent performance. More details of this task can be found in the original ANT development paper (Fan et al., 2002).

2.1.6 Data analysis

We hypothesized that moderate-intensity PA in the past week would be related to cognitive function. Specifically, higher levels of moderate-intensity PA would be associated with improved performance on the executive control task. Therefore, multiple regression was used to determine the relationship between self-reported PA at each intensity level (low, moderate, vigorous) and performance on the alerting, orienting, and executive control tasks. Participant age was used as a covariate as it has been shown to impact cognitive performance (H. Lee et al., 2012; West et al., 2002; Ziegler et al., 2012). Specifically, the model (Model 1) predicted alerting/orienting/executive control performance from age, low-intensity MET-mins/week, moderate-intensity MET-mins/week, and vigorous-intensity MET-mins/week.

Previous studies have looked at the effect of long-term PA on ANT performance (Pérez et al., 2014), however, their PA measures did not include intensity categories. The IPAQ also provides a total PA measure that is collapsed across intensity, so we included a second model (Model 2) predicting ANT performance from age and total MET-mins/week. This is to maximize comparability between studies to ensure observed effects are due to the timespan of the PA measure (long-term vs. free-living), rather than being caused by looking at different intensities of PA. Finally, some studies have used an alternate coding method for the IPAQ (Kamijo & Takeda, 2009, 2013) that classifies individuals as sedentary or active based on ACSM recommendations (described in the previous section). For each of the ANT outcome measures, we included a third model (Model 3) predicting performance from age and ACSM category to ensure the IPAQ scoring method does not change observed relationships. Regression assumptions were checked by visual inspection of quantile-

quantile and normality plots, and no violations were indicated. Furthermore, due to potential issues with collinear predictors, variance inflation factor (VIF) was calculated for all predictors in each of the models. All VIF values were within an acceptable range (< 1.5).

2.1.7 Results

The mean task reaction time (and standard deviation) for each condition can be seen in Table 2.2.

Table 2.2 ANT mean reaction times for Study 1

Alerting		Orienting		Executive Control	
Double Cue	No Cue	Spatial Cue	Center Cue	Congruent	Incongruent
611.5ms (87.63)	662.57ms (96.02)	575.94ms (91.92)	612.4ms (88.82)	592.89ms (90.5)	694.75ms (102.82)

2.1.7.1 Alerting task

We tested the relationship between alerting performance and PA intensity using Model 1. None of the predictors were significantly related to alerting performance. Age: $\beta = 0.04$, 95% CI [-0.08, 0.17], $t(243) = 0.65$, $p = .52$, low-intensity: $\beta = 0.02$, 95% CI [-0.11, 0.14], $t(243) = 0.24$, $p = .81$, moderate-intensity: $\beta = 0.10$, 95% CI [-0.03, 0.24], $t(243) = 1.51$, $p = .13$, vigorous-intensity: $\beta = -0.05$, 95% CI [-0.18, 0.09], $t(243) = -0.68$, $p = .50$. Using Model 2, we tested the relationship between total PA and alerting. However, none of the predictors were significantly related to alerting performance. Age: $\beta = 0.04$, 95% CI [-0.08, 0.17], $t(245) = 0.67$, $p = .51$, total PA: $\beta = 0.04$, 95% CI [-0.08, 0.17], $t(245) = 0.68$, $p = .50$. Finally, we tested the relationship between ACSM category and alerting using Model 3. Again, none of the predictors were significantly related to alerting performance. Age: $\beta =$

0.05, 95% CI [-0.08, 0.17], $t(245) = 0.75$, $p = .46$, ACSM: $\beta = -0.23$, 95% CI [-0.50, 0.05], $t(245) = -1.61$, $p = .11$.

2.1.7.2 Orienting task

We tested the relationship between orienting performance and PA intensity using Model 1. None of the predictors were significantly related to orienting performance. Age: $\beta = 0.02$, 95% CI [-0.11, 0.14], $t(243) = 0.24$, $p = .81$, low-intensity: $\beta = 0.04$, 95% CI [-0.09, 0.17], $t(243) = 0.62$, $p = .54$, moderate-intensity: $\beta = 0.06$, 95% CI [-0.08, 0.19], $t(243) = 0.85$, $p = .40$, vigorous-intensity: $\beta = 0.03$, 95% CI [-0.10, 0.16], $t(243) = 0.44$, $p = .66$. Using Model 2, we tested the relationship between total PA and orienting. However, none of the predictors were significantly related to orienting performance. Age: $\beta = 0.02$, 95% CI [-0.11, 0.14], $t(245) = 0.25$, $p = .80$, total PA: $\beta = 0.08$, 95% CI [-0.04, 0.21], $t(245) = 1.30$, $p = .20$. Finally, we tested the relationship between ACSM category and orienting using Model 3. Again, none of the predictors were significantly related to orienting performance. Age: $\beta = 0.01$, 95% CI [-0.11, 0.14], $t(245) = 0.22$, $p = .83$, ACSM: $\beta = 0.13$, 95% CI [-0.15, 0.40], $t(245) = 0.92$, $p = .36$.

2.1.7.3 Executive control task

We tested the relationship between executive control performance and PA intensity using Model 1. None of the predictors were significantly related to executive control. Age: $\beta = 0.03$, 95% CI [-0.10, 0.15], $t(243) = 0.41$, $p = .68$, low-intensity: $\beta = 0.06$, 95% CI [-0.07, 0.19], $t(243) = 0.89$, $p = .37$, moderate-intensity: $\beta = -0.07$, 95% CI [-0.20, 0.07], $t(243) = -0.98$, $p = .33$, vigorous-intensity: $\beta = -0.003$, 95% CI [-0.14, 0.13], $t(243) = -0.05$, $p = .96$. Using Model 2, we tested the relationship between total PA and executive control. However, none of the predictors were significantly related to performance. Age: $\beta = 0.03$, 95% CI [-0.09, 0.15], $t(245) = 0.47$, $p = .64$, total PA: $\beta = -0.02$, 95% CI [-0.14, 0.11], $t(245) = -0.25$, $p = .81$. Finally, we tested the relationship between ACSM category and executive

control using Model 3. Again, none of the predictors were significantly related to executive control. Age: $\beta = 0.03$, 95% CI [-0.09, 0.16], $t(245) = 0.54$, $p = .59$, ACSM: $\beta = -0.21$, 95% CI [-0.49, 0.07], $t(245) = -1.50$, $p = .14$.

2.1.8 Discussion

Overall, our results fail to support the hypothesis that moderate-intensity free-living PA is related to executive control. Previous studies have shown a relationship between long-term PA and executive control (Pérez et al., 2014), however, our models utilizing the IPAQ's total PA measure (Model 2) failed to show a relationship with any of the outcome measures. This difference in results suggests that the timescale of PA assessment may be important for PA and cognition effects. Furthermore, other studies have utilized different methods of coding the IPAQ based on ACSM recommendations (Kamijo & Takeda, 2009, 2013). Although those researchers focused on different cognitive outcomes, our results using Model 3 suggest that IPAQ coding method does not play a major role in identifying relationships between free-living PA and executive control.

The ANT is complex owing to its multiple cue and congruency conditions, and this complexity necessitates the use of many trials to establish stable condition means, resulting in long testing sessions (lasting almost 2 hours) and possibly fatigue effects. We examined the linear trend in participant reaction times across their 288 ANT trials by regressing reaction time on trial number, and calculating a regression coefficient for each participant, which represents the degree to which the individual's responses were slowing down over time. The mean coefficient value suggested that, on average, participant responses were getting slower by 0.09ms for each successive trial. When these coefficient values were submitted to a one sample t-test (tested against 0), we found a significant degree of response slowing over time, $t(261) = 3.82$, $p < .001$. While not conclusive, this suggests the

task length may be causing fatigue and a general slowing of performance, and possibly masking the impact of PA on executive control.

2.2 Study 2

The results from Study 1 showed no relationship between free-living PA and executive control, however, there may be a fatigue effect due to the complexity and length of the ANT task. Previous studies have shown that long-term PA impacts only the executive control task in young adults (Pérez et al., 2014), and similar results have been shown for acute bouts of PA using the Eriksen Flanker task (Charles H. Hillman et al., 2003). The Eriksen Flanker task is one of the most commonly used measures of executive control in both the young adult PA literature (N. Gothe et al., 2013; Sebastian Ludyga et al., 2018; Schmit et al., 2015; Soga et al., 2015; Spitzer & Furtner, 2016), and PA intervention studies involving older clinical populations (Hsu et al., 2018; T. Liu-Ambrose et al., 2012; ten Brinke et al., 2018). Our goal in Study 2 was to shorten the testing duration and focus only on executive control performance using an Eriksen Flanker task, thus allowing us to rule out complexity and fatigue as alternate explanations for the null results found in Study 1.

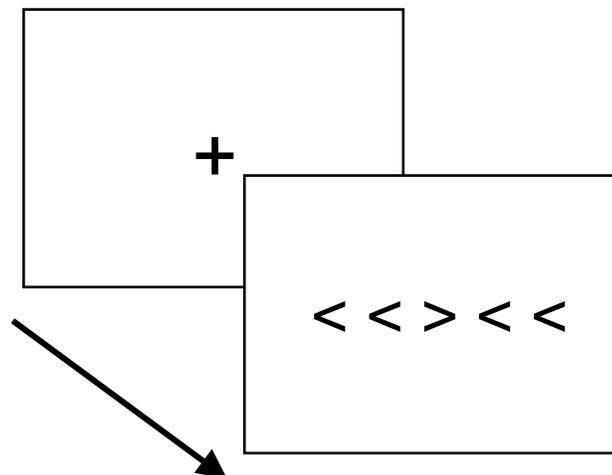
2.2.1 Participants

A total of 220 participants were recruited from undergraduate psychology courses at the University of British Columbia and received course credit for their time. Participants were eligible to take part in the study if they were young adults (under 45 years of age) and physically able to take part in PA. Two participants were excluded due to computer problems that prevented data collection. Our final sample consisted of 218 participants (mean age = 20.11, $SD = 1.87$, 48 male). Participant demographic information can be found in Table 2.1.

2.2.2 Procedure

Free-living PA over the past week was assessed using the IPAQ, and executive control was measured using a modified Eriksen Flanker task. Each trial began with a fixation cross in the center of the screen, displayed for 1000ms. Next, flanker arrows (five horizontal arrowheads) were shown for 200ms at the center of the screen, and participants had a maximum of 1500ms (from stimulus onset) to make a response (Figure 2.2). Participants were asked to use the left and right arrows keys on the keyboard to indicate the pointing direction of the central arrow. Like Study 1, arrow directions could either be leftward-congruent (all arrows pointing to the left), leftward-incongruent (center arrow pointing left, flanking arrows pointing right), rightward-congruent (all arrows pointing to the right), or rightward-incongruent (center arrow pointing right, flanking arrows pointing left). All stimulus conditions were equiprobable and presentation order was randomized. Feedback was displayed after each trial with the words “correct”, “incorrect”, or “too slow” depending on the response. At the end of the trial, the fixation cross was shown again for 1500ms (intertrial interval) before the next trial began. Participants began with 12 practice trials, and the main task consisted of 100 trials. Performance was calculated as the difference in reaction time between incongruent and congruent trials, which was then normalized like in Study 1.

Figure 2.2 Flanker display sequence



2.2.3 Data analysis

We began by confirming that the congruency manipulation during the flanker task was effective by checking that incongruent trials resulted in longer reaction times than congruent trials using a t-test. The remaining analyses followed the same plan as Study 1, where each of the multiple regression models (models 1-3) were used to test the relationship between weekly PA and flanker performance. Regression assumptions were checked by visual inspection of quantile-quantile and normality plots, and no violations were indicated. Furthermore, due to potential issues with collinear predictors, variance inflation factor (VIF) was calculated for all predictors in each of the models. All VIF values were within an acceptable range (< 1.5).

2.2.4 Results

A paired samples t-test showed that participants took significantly longer to respond to the incongruent trials (mean: 528.4ms, SD: 63.83) than the congruent trials (mean: 470.61ms, SD: 56.38), suggesting that the flanker congruency manipulation was effective, $t(217) = 25.11, p < .001$. To test our main hypothesis, we tested Models 1-3 using the normalized flanker difference score as the dependent variable. Model 1 showed that none of the predictors were significantly related to flanker performance. Age: $\beta = 0.05$, 95% CI [-0.10, 0.19], $t(194) = 0.66, p = .51$, low-intensity: $\beta = 0.02$, 95% CI [-0.12, 0.17], $t(194) = 0.35, p = .73$, moderate-intensity: $\beta = -0.09$, 95% CI [-0.23, 0.05], $t(194) = -1.27, p = .21$, vigorous-intensity: $\beta = 0.04$, 95% CI [-0.10, 0.17], $t(194) = 0.53, p = .60$. Using Model 2, we tested the relationship between total PA and flanker performance. However, none of the predictors were significantly related to performance. Age: $\beta = 0.04$, 95% CI [-0.10, 0.19], $t(196) = 0.56, p = .58$, total PA: $\beta = -0.01$, 95% CI [-0.15, 0.13], $t(196) = -0.14, p = .89$. Finally, we tested the relationship between ACSM category and executive control using Model 3. Again, none of the predictors were significantly related to flanker performance.

Age: $\beta = 0.04$, 95% CI [-0.10, 0.19], $t(196) = 0.55$, $p = .58$, ACSM: $\beta = 0.06$, 95% CI [-0.22, 0.34], $t(196) = 0.44$, $p = .66$.

It is possible that PA effects are only observable for the more difficult task conditions, so Model 1 was also tested on congruent and incongruent reaction times separately.

However, none of the predictors were significantly related to congruent trial performance.

Age: $\beta = 0.11$, 95% CI [-0.04, 0.26], $t(194) = 1.43$, $p = .16$, low-intensity: $\beta = 0.06$, 95% CI [-0.08, 0.21], $t(194) = 0.87$, $p = .39$, moderate-intensity: $\beta = 0.07$, 95% CI [-0.07, 0.21], $t(194) = 0.96$, $p = .34$, vigorous-intensity: $\beta = -0.002$, 95% CI [-0.14, 0.14], $t(194) = -0.02$, $p = .98$.

Additionally, incongruent trial performance also failed to show a relationship with PA. Age: $\beta = 0.13$, 95% CI [-0.02, 0.28], $t(194) = 1.73$, $p = .09$, low-intensity: $\beta = 0.08$, 95% CI [-0.06, 0.23], $t(194) = 1.17$, $p = .25$, moderate-intensity: $\beta = 0.02$, 95% CI [-0.12, 0.16], $t(194) = 0.26$, $p = .79$, vigorous-intensity: $\beta = 0.02$, 95% CI [-0.12, 0.16], $t(194) = 0.29$, $p = .77$.

2.2.5 Discussion

Overall, our results failed to support the hypothesis that free-living PA is related to executive control, and the null results found in Study 1 were unlikely to be due to task complexity and fatigue effects. It has been suggested that executive control in young adults is highly efficient (Friedman et al., 2009) and it is possible that the benefits of PA do not emerge unless the cognitive task is sufficiently demanding (Chang, Pesce, et al., 2015; Kamijo et al., 2007; Scisco et al., 2008). This idea is supported by studies examining the impact of aerobic fitness on executive control in pre-adolescent populations, showing that PA benefits are most strongly observed for higher difficulty versions of the flanker task (Moore et al., 2013; Pontifex et al., 2011). Although we are not studying pre-adolescents in the present set of studies, the PA benefit during more difficult tasks may also extend to young adult populations. Our analyses attempted to distinguish high and low difficulty conditions by examining congruent and incongruent trial performance separately, finding no

difference between the trial types, however, it is possible that the incongruent condition was still not demanding enough for a PA benefit to be observed.

2.3 Study 3

The goal of Study 3 to test whether the null results observed in the previous studies may be due to the task being too easy. The difficulty of the flanker task can be manipulated by introducing an “incompatible” condition, where the participant is asked to respond in the *opposite* direction of the central arrow (Friedman et al., 2009). This manipulation has been successfully used to show an association between aerobic fitness and executive control, at least in pre-adolescent populations (Moore et al., 2013; Pontifex et al., 2011).

2.3.1 Participants

A total of 210 participants were recruited from undergraduate psychology courses at the University of British Columbia and received course credit for their time. Participants were eligible to take part in the study if they were young adults (under 45 years of age) and physically able to take part in PA. Four participants were excluded due to computer problems, declined to report their age, was wheelchair-bound and not physically active, and reported IPAQ activity durations using different (unknown) timescales. Our final sample consisted of 206 participants (mean age = 20.33, $SD = 2.71$, 51 male). Participant demographic information can be found in Table 2.1.

2.3.2 Procedure

The study setup was identical to Study 2 barring the addition of a within-subject compatibility manipulation. During compatible trials, participants were instructed to respond to the pointing direction of the central arrow (same as Study 2; Figure 2.2), and during incompatible trials they were asked to respond in the *opposite* direction of the central arrow using the arrow keys on the keyboard. Compatible and incompatible trials were blocked, and

block order was counterbalanced between participants. Each compatibility block contained 100 trials. The congruency manipulation was the same as the previous two studies, where arrow directions could either be leftward-congruent (all arrows pointing to the left), leftward-incongruent (center arrow pointing left, flanking arrows pointing right), rightward-congruent (all arrows pointing to the right), or rightward-incongruent (center arrow pointing right, flanking arrows pointing left).

2.3.3 Data analysis

We began by confirming that the congruency and compatibility manipulations were effective using a 2-way ANOVA. The remaining analyses followed the same plan as the previous studies, where the multiple regression models (models 1-3) were used to test the relationship between weekly PA and flanker performance for each of the compatibility conditions. Regression assumptions were checked by visual inspection of quantile-quantile and normality plots, and no violations were indicated. Furthermore, due to potential issues with collinear predictors, variance inflation factor (VIF) was calculated for all predictors in each of the models. All VIF values were within an acceptable range (< 1.5).

2.3.4 Results

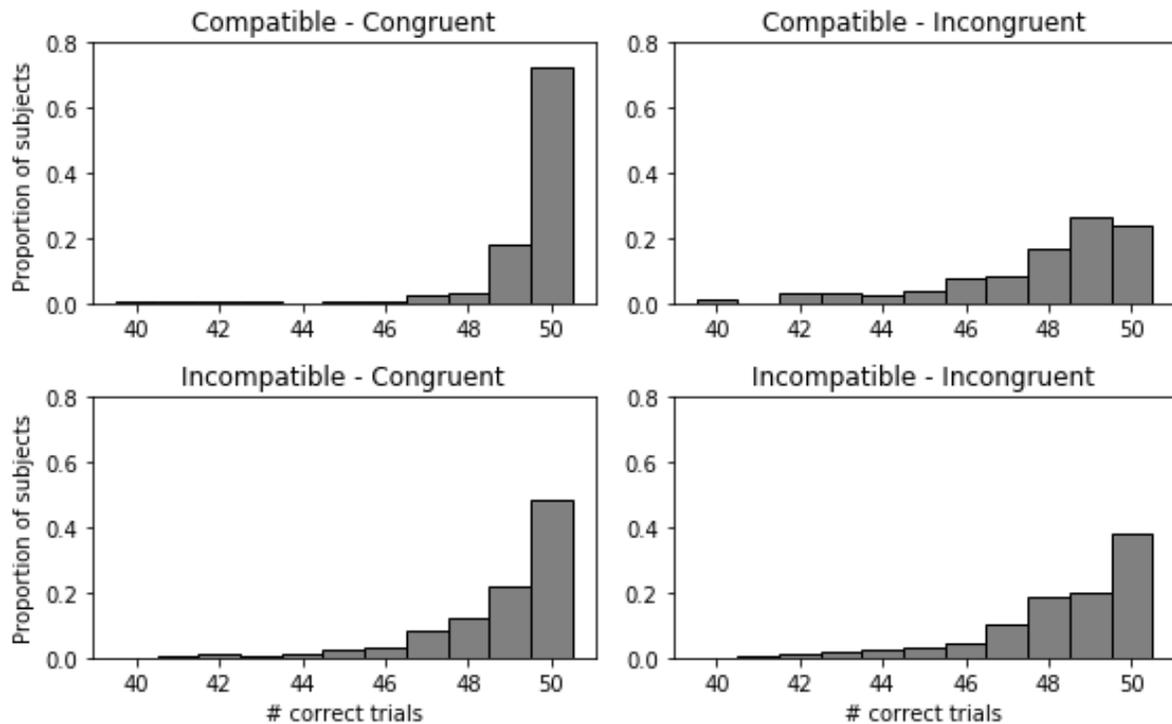
As a manipulation check, reaction time was submitted to a 2 (compatibility: compatible, incompatible) x 2 (congruency: congruent, incongruent) within-subjects ANOVA to ensure that 1) incongruent trials were more difficult than congruent trials, and 2) that incompatible trials were more difficult than compatible trials. The raw reaction times (and standard deviations) can be seen in Table 2.3. A significant interaction between compatibility and congruency was found, $F(1, 205) = 88.44, p < .001, \eta_p^2 = 0.30$. There was a significant main effect of compatibility, $F(1, 205) = 32.71, p < .001, \eta_p^2 = 0.14$, as well as a significant main effect of congruency, $F(1, 205) = 469.94, p < .001, \eta_p^2 = 0.70$. The observed data

pattern is as predicted, with congruent trials faster than their incongruent counterparts, and compatible trials faster than incompatible trials, suggesting a successful difficulty manipulation. However, simple main effects analysis of the interaction, with Bonferroni adjustment for multiple comparisons, showed a significant difference between compatible and incompatible conditions for the congruent trials, $t(248.63) = 8.37, p < .001$, but not for the incongruent trials, $t(248.63) = 2.50, p = .08$. This is likely due to a ceiling effect in task performance, rather than the difficulty manipulation only working for congruent trials, and is supported by looking at histograms of task accuracy for each condition (Figure 2.3). The graphs show that the vast majority of participants achieved near perfect accuracy in all conditions. Perfect performance can place an upper bound on reaction time. In other words, due to the ease of the overall Flanker task, participants needed no more than 531.52ms (on average) to complete even the most difficult trials (incompatible-incongruent), and this cap on reaction time likely prevented a significant simple main effect from being observed in the incongruent trials. The overall data pattern suggests that the difficulty manipulation was effective, but the Flanker task itself may not be difficult enough, thus causing a ceiling effect that masks the true difficulty of the incompatible condition.

Table 2.3 Flanker task reaction time means for Study 3

	Compatible	Incompatible
Congruent	470.67ms (65.36)	506.29ms (75.0)
Incongruent	520.86ms (68.01)	531.52ms (87.89)

Figure 2.3 Task accuracy histograms for weekly PA Study 3



For the compatible condition, Models 1-3 were tested using the normalized flanker difference score as the dependent variable, which should replicate our findings from Study 2. Model 1 showed that none of the predictors were significantly related to compatible flanker performance. Age: $\beta = -0.03$, 95% CI [-0.17, 0.12], $t(190) = -0.37$, $p = .71$, low-intensity: $\beta = 0.03$, 95% CI [-0.12, 0.18], $t(190) = 0.45$, $p = .65$, moderate-intensity: $\beta = 0.01$, 95% CI [-0.16, 0.17], $t(190) = 0.08$, $p = .94$, vigorous-intensity: $\beta = 0.001$, 95% CI [-0.16, 0.16], $t(190) = 0.01$, $p = .99$. Using Model 2, we tested the relationship between total PA and compatible flanker performance. However, none of the predictors were significantly related to performance. Age: $\beta = -0.03$, 95% CI [-0.17, 0.12], $t(192) = -0.37$, $p = .71$, total PA: $\beta = 0.02$, 95% CI [-0.12, 0.17], $t(192) = 0.34$, $p = .74$. Finally, we tested the relationship between ACSM category and executive control using Model 3. Again, none of the predictors were significantly related to compatible flanker performance. Age: $\beta = -0.03$, 95% CI [-0.17, 0.11], $t(192) = -0.39$, $p = .70$, ACSM: $\beta = 0.02$, 95% CI [-0.28, 0.33], $t(192) = 0.16$, $p = .88$.

For the incompatible condition, Models 1-3 were tested using the normalized flanker difference score as the dependent variable. Model 1 showed that none of the predictors were significantly related to incompatible flanker performance. Age: $\beta = 0.09$, 95% CI [-0.06, 0.23], $t(190) = 1.20$, $p = .23$, low-intensity: $\beta = -0.01$, 95% CI [-0.16, 0.14], $t(190) = -0.14$, $p = .89$, moderate-intensity: $\beta = -0.05$, 95% CI [-0.21, 0.12], $t(190) = -0.57$, $p = .57$, vigorous-intensity: $\beta = 0.08$, 95% CI [-0.07, 0.24], $t(190) = 1.05$, $p = .30$. Using Model 2, we tested the relationship between total PA and incompatible flanker performance. However, none of the predictors were significantly related to performance. Age: $\beta = 0.08$, 95% CI [-0.06, 0.22], $t(192) = 1.10$, $p = .27$, total PA: $\beta = 0.03$, 95% CI [-0.12, 0.17], $t(192) = 0.36$, $p = .72$. Finally, we tested the relationship between ACSM category and executive control using Model 3. Again, none of the predictors were significantly related to incompatible flanker performance. Age: $\beta = 0.08$, 95% CI [-0.06, 0.22], $t(192) = 1.10$, $p = .26$, ACSM: $\beta = 0.07$, 95% CI [-0.23, 0.37], $t(192) = 0.48$, $p = .63$.

2.3.5 Discussion

Our results failed to show a relationship between free-living PA and executive control, even after introducing a more difficult task condition. Furthermore, no difference was observed when looking at separate PA intensity levels versus total PA, nor was there a difference between different IPAQ scoring methods. One potential issue is the use of the Flanker task. The reaction time data pattern suggests that the difficulty manipulation was effective, with incompatible trials resulting in slower overall responses than compatible trials. However, task accuracy suggests a ceiling effect, capping both accuracy and reaction time during the task. That is to say that while a more difficult task condition was introduced, the Flanker task itself may still not be difficult enough for PA effects to be observed. Furthermore, overall cognitive function and processing speed are generally better in young adults than other age groups (H. Lee et al., 2012), and some studies have shown that PA

benefits executive control only in older adults (Berchicci et al., 2013; Boucard et al., 2012). The ceiling effect may be due to studying a young adult age group where executive control functions well enough that either no benefit arises from increased PA, or an extremely demanding cognitive task is required for the effects to be observed.

2.4 General discussion

Overall, our results show that self-reported PA over the past week, at any intensity level, does not appear to have a substantive impact on executive control. Our control studies (Studies 2 and 3) show that the null findings were not due to fatigue and task complexity, or low cognitive demands of the task. Although it is certainly possible that there is no relationship between free-living PA and executive control in younger adults, there are several important factors that need to be considered with regard to our null findings.

Studies of long-term PA have shown improvements to executive control (Cox et al., 2016; Pérez et al., 2014) and similarly very short-term acute PA has also been shown to be beneficial (Chang et al., 2017; Samani & Heath, 2018). Those studies exist at opposite ends of the temporal continuum and it is possible that mid-term PA, such as the type studied here, may be too recent to see the long-term benefits from improved aerobic fitness and cardiovascular health, and at the same time too distal to see improvements due to acute physiological arousal. However, we did not control for the effects of longer-term habitual PA or aerobic fitness, which may have a larger impact on cognition than PA at short timescales (Kwak et al., 2009; Pindus et al., 2015). Other research in our laboratory has demonstrated that increased levels of free-living PA over the past two days is associated with improved performance on an academic exam, suggesting that PA benefits may be observable only when looking at PA that occurred very close to the time of cognitive assessment, making it more similar to acutely measured activity. In other words, just like there is an inverted-U relationship between PA intensity and cognition (Kamijo et al., 2007; Labelle et al., 2014;

Smith et al., 2016; C.-C. Wang et al., 2013), a similar relationship may exist with PA recency, with effects only observable for very recent or very long-term activity.

There may be factors that mediate the relationship between free-living PA and executive control. A recent study showed that frequency of PA is related to improved executive control, but that this relationship is mediated by efficiency of cerebral blood-flow regulation (Guiney et al., 2015). Aerobic fitness may be a better predictor of executive control than measures of moderate-to-vigorous physical activity (Pindus et al., 2015), which may explain why long-term PA studies have found associations with executive control given that increased levels of habitual long-term activity would likely increase overall aerobic fitness. Sleep efficiency has also been shown to mediate the relationship between objectively measured PA and executive control (Wilckens et al., 2016). Finally, some research has shown that self-reported PA is related to executive control only in lean, but not obese, individuals (Galioto Wiedemann et al., 2014). While these studies have assessed different timescales, it would be worth investigating the mediating effects of health-related metrics such as sleep, fitness, and obesity.

Throughout the set of three studies, we used the ANT and Flanker tasks as measures of executive function. Executive function is not a unitary construct and contains a number of correlated domains (Etnier & Chang, 2009; Miyake & Friedman, 2012), and of specific interest in this set of studies is executive (or inhibitory) control. In the PA literature, the Flanker task is often seen as a measure of executive control that can be improved through increased PA participation (Gejl et al., 2018; Oberste et al., 2019). However, this is only one stage of processing and we often ignore the early attentional mechanisms involved in Flanker task performance. From early work on the Flanker task, it has been observed that increasing the spacing between flanking stimuli can eliminate the interference effect (Eriksen & Eriksen, 1974). This is a perceptual-level phenomenon and has been ascribed to

improved discriminability between stimuli (Eriksen & Eriksen, 1974) or due to the distribution of visual attention being graded and non-linear (Handy et al., 1996). This raises the question of whether increased PA is affecting late-stage response inhibition or improving performance at earlier levels of perception. The useful field of view (UFOV) is described as the attentional window from which visual information can be obtained and attended to, the size of which has been shown to change as a function of multiple factors like cognitive demand and age (Ball et al., 1988; L. J. Williams, 1982). It is possible that, rather than being a response inhibition effect, increased PA participation might be causing a reduction the size of the UFOV, thus allowing participants to narrow their attention on the target stimuli and effectively ignoring the flanking task-irrelevant information, in which case an inhibitory process may not be required.

We used an effect size estimate of 0.20 when conducting the power analysis to determine required sample size for the studies. There is little research studying the impact of self-reported free-living PA on executive control in young adults, however, a review of the literature, using different PA and cognitive measures, generally shows larger effect sizes than the conservative 0.20 that we used for the power analysis. For example, studies have found that the association between PA and executive control ranges from 0.22 to 0.41 (Galioto Wiedemann et al., 2014; Guiney et al., 2015; Stillman et al., 2016; Wilckens et al., 2016), and moderate-intensity PA and attention capacity to be 0.30 (Vanhelst et al., 2016). Meta-analyses show the relationship between PA interventions and academic achievement in high school aged adults to be 0.24 (Sibley & Etnier, 2003), acute PA and cognitive function to be 0.20 (Lambourne & Tomporowski, 2010), and fitness and cognitive function in college-aged adults to be 0.64 (Etnier et al., 1997). Furthermore, Studies 2 and 3 had a large majority of female participants, and some studies suggest that females show larger effect sizes than males (Barha et al., 2017; Kwak et al., 2009). Granted that some studies

have shown smaller relationships between PA and executive control, with effect sizes around 0.10 (P. D. Loprinzi & Kane, 2015; Pindus et al., 2015), we remained conservative in our estimated effect size relative to the majority of the PA literature to avoid being underpowered. However, our observed effect sizes for executive control were as low as 0.001 in some models, suggesting that we may be underpowered for detecting such small effects. It should be noted that many of the confidence intervals around our point estimates were wide enough to contain our estimated effect of 0.20, leaving open the possibility of an effect of that size in the population. If, however, the population effect sizes are actually as small as observed, this raises a question of practical significance, even if statistical significance was not found. For example, our models for the incompatible flanker task in Study 3 showed effect sizes of 0.01, 0.05, and 0.08 on a standardized metric for low, moderate and vigorous intensity PA. In the raw score metric, each unit increase in MET-minutes/week would correspond to reaction differences (between congruent and incongruent trials) of 4.56×10^{-5} , 1.15×10^{-3} , and 8.77×10^{-4} milliseconds, which are too small to be of any meaningful importance.

Various PA measurement-related issues may contribute to the differences between our null findings and the significant results reported in the PA literature. A number of studies have used the IPAQ, but only reported PA duration, rather than duration weighted by intensity (Guiney et al., 2015; Kamijo et al., 2011; Kamijo & Takeda, 2009, 2010, 2013). Not only does this fail to account for the importance of PA intensity, but it may also alter the psychometric properties of the measurement instrument. The IPAQ was originally designed and validated as a measure of MET-minutes per week (Craig et al., 2003), and it is unknown how reliability and validity are affected when only the time-based PA metrics are utilized. We did not observe significant relationships when using the ACSM time-based coding method for the IPAQ, however, this may be due to differences in cognitive task used, timescale

differences, or not considering potential mediators as previously discussed. Furthermore, some IPAQ studies have only used the short-form of the questionnaire (Galioto Wiedemann et al., 2014; Guiney et al., 2015; P. D. Loprinzi & Kane, 2015; Pellicer-Chenoll et al., 2015), which has been shown to have relatively weak psychometric properties, with low correlations against objective measures and a tendency to overestimate activity levels (P. H. Lee et al., 2011). Overall, there is no single standard for collecting and handling self-reported PA data, and this can lead to differences in the accuracy, reliability, and validity of the measures used, resulting in some studies reporting significant associations, and others demonstrating null relationships.

Finally, research has shown that self-reported PA may be inaccurate, have low reliability and low validity compared to more direct measures of PA (Corder et al., 2009; Falck et al., 2016; Gråstén et al., 2012; Helmerhorst et al., 2012; Prince et al., 2008; van Poppel et al., 2010), while objective PA measures show stronger correlations with anthropometric variables, such as BMI, than self-report questionnaires (Belcher et al., 2015). In terms of the validity range, self-report has been estimated to have a validity between 0.08-0.40, while accelerometry-based techniques are between 0.20-0.47 when compared to direct measures like $VO_2\text{max}$ (Helmerhorst et al., 2012; Ottevaere et al., 2011). Therefore, inaccuracies stemming from subjective reporting may explain our lack of significant findings. The choice between subjective and hardware-based PA recording is not straightforward, however, as objective methods have environmental limitations (for example, they cannot measure water-based activities, or activities where the body remains stationary such as cycling), and require high wear-time to be accurate (Troost et al., 2005). Furthermore, many studies have found significant associations between self-reported PA and cognitive function (Galioto Wiedemann et al., 2014; Guiney et al., 2015; Kamijo & Takeda, 2010, 2013; O'Connor et al., 2015; Padilla et al., 2013, 2014; Pérez et al., 2014), suggesting our null

results are not solely due to the use of subjective reporting. One possibility is that our method of cognitive assessment (behavioral performance on the ANT and Flanker tasks) may not be sensitive enough to show significant relationships with free-living PA. Across a number of PA time windows (acute, long-term, free-living), many studies have shown no relationship between PA and behavioral measures of cognitive performance, whereas a significant relationship is seen when using neuroimaging techniques to assess cognition (Herting & Nagel, 2013; Charles H. Hillman et al., 2003; Kamijo et al., 2004; Magnié et al., 2000; Jason R. Themanson et al., 2006; Wagner et al., 2017). This difference between behavioral and neuroimaging results suggests that a more sensitive measure of cognitive performance may be needed to observe PA benefits in young adult populations. Additionally, the Flanker task is a commonly used measure of executive control and other studies have demonstrated improved performance due to increased levels of PA (Charles H. Hillman et al., 2003; Kamijo et al., 2009; Stroth, Kubesch, et al., 2009). While these significant findings differ from the null results presented here, the discrepancy can again be explained methodologically, as those studies found neuroelectric changes during the Flanker task, while our focus was on behavioral Flanker performance.

Overall, there is a paucity of research on the effects of self-reported free-living PA, and it is difficult to make direct comparison between the present study and much of the PA literature. This is, by and large, due to published studies not making a clear distinction between long-term and free-living timescales (Berchicci et al., 2013, 2014; T. M. C. Lee et al., 2014; O'Connor et al., 2015), and even combining multiple timescales into a single equally-weighted composite measure (Boucard et al., 2012). Moving forward, we suggest that the distinction is vital for understanding the impact of PA on executive control, as the timescale of assessment can have a markedly different impact on strength and direction of association.

2.5 Bridging Summary

The results in this chapter showed that PA performed over the past week is not related to performance on an executive function task, and that the relationship (or lack thereof) is not affected by PA intensity. This raises the question of why no PA effect was observed, when the wider literature has shown a beneficial effect of increased PA. One possibility is that the PA time window in this set of studies is too wide and, based on the evidence presented in the Introduction, a more acute time window may be needed to see significant effects. To that end, the next chapter examines a more acute time window (24-hours prior to cognitive testing), to see whether PA time window might be affecting the ability to see PA benefits.

3 24-hour physical activity and cognition

3.1 Study 1

Physical activity (PA) levels have been shown to decline during young adulthood (Ku et al., 2006; Spittaels et al., 2012), with further decline predicted over the next decade (Ng & Popkin, 2012). The physical health benefits of PA have been known and messaged for decades (Hamilton et al., 2007; Owen et al., 2010; Soares-Miranda et al., 2016), and yet PA levels have continued to decline, suggesting that a different approach to PA promotion may be needed. Young adults are particularly important to study as PA begins to decline during adolescence and young adulthood (Ku et al., 2006; Spittaels et al., 2012). Young adults also show the lowest levels of PA participation in Asian countries (Ku et al., 2006) and have the highest prevalence of sedentary behavior (Spittaels et al., 2012). If we want to promote increased PA participation, then it would make sense to target the age group where physical activity participation levels are lowest.

Although it remains an empirical question, one alternative approach to promoting increased PA adoption is to message the benefits to cognitive function. A large body of work has demonstrated a positive relationship between acute PA and cognition function in young adults, with most of the work focused primarily on executive processes. For example, acute PA, performed just prior to cognitive testing, has been shown to improve working memory performance (Hogan et al., 2013; Lo Bue-Estes et al., 2008; Tomporowski et al., 2005) and executive control (inhibitory control) performance as measured by the Eriksen Flanker (Chang, Pesce, et al., 2015; Kamiyo et al., 2007; Kubesch et al., 2009; Sebastian Ludyga et al., 2018) and Stroop tasks (Byun et al., 2014; Chang et al., 2017; Faulkner et al., 2016; Harveson et al., 2016; Yanagisawa et al., 2010). However, when assessing time windows longer than acute PA the benefits of increased activity are less clear. When measuring PA

over the week prior to cognitive testing, some studies have shown a beneficial effect of increased PA on executive control (Guiney et al., 2015; Kamijo & Takeda, 2010; Stillman et al., 2016), memory (Stillman et al., 2016) and performance variability (Gooderham et al., 2020), whereas others have reported non-significant relationships between weekly PA and behavioral measures of executive control (Boucard et al., 2012; P. D. Loprinzi & Kane, 2015; Pindus et al., 2015).

The inconsistent findings between acute and weekly PA time windows suggests there may be a temporal threshold at which the positive benefits of PA begin to emerge. To our knowledge there is no research examining time windows between acute and weekly PA, such as 24-hour PA, and its relation to behavioral measures of executive control in young adults. In terms of underlying mechanisms, at longer time windows, the cardiovascular fitness hypothesis suggests that PA benefits arise due to improvements in aerobic function, and that it is these cardiovascular improvements that mediate the relationship between PA and cognition (Marmeleira, 2013). Other work shows that PA over multiple months and years can also lead to structural brain changes (Cabral et al., 2019). These physiological changes, however, take place over longer periods of time, and are unlikely to contribute to changes at the daily (or even weekly) level. At more acute time windows, the literature is generally supportive of neurochemical effects, where neurotrophins like BDNF act as a moderator (or mediator) between acute PA and cognitive function (Basso & Suzuki, 2017; Mandolesi et al., 2018; Piepmeier & Etnier, 2015). While no weekly PA effects were observed in the previous chapter, it is possible that at even more acute time windows (e.g., 24-hour PA) these neurotrophic changes may persist long enough to allow a daily PA effect to be observed.

Intermediate time windows are understudied but are an important period to examine for the purposes of PA promotion. Messaging the benefits of acute PA may not be effective

because even if it is shown to benefit executive control, the time window is too close to cognitive testing for sufficient amounts of PA to occur. A more reasonable approach might be to promote increased PA levels over the 24-hours prior to cognitive testing, as there remains ample time to participate in physical activity. In the present article we examined whether increased PA levels over the previous 24-hours is predictive of performance on an executive control task. In terms of temporal proximity, 24-hour PA is more similar to acute PA than weekly PA and, given the generally positive associations observed in the acute PA literature, we predict that increased levels of 24-hour PA would also be positively associated with executive control performance (specifically inhibitory control). Additionally, task difficulty has been found to moderate the relationship between PA and executive control (Kubesch et al., 2009; Padilla et al., 2013, 2014; Pérez et al., 2014; Pontifex & Hillman, 2007; Samani & Heath, 2018). Therefore, the positive association of 24-hour PA is predicted for only more difficult task conditions.

3.1.1 Method

3.1.1.1 Ethics Statement

Ethical approval was received through the University of British Columbia's Behavioural Research Ethics Board, written informed consent was obtained from each participant prior to the start of the study, and research was performed in accordance with ethics board guidelines.

3.1.1.2 Participants

We conducted a two-tail power analysis to determine minimum sample size for the study. Previous studies have shown that the relationship between PA and cognitive performance ranges from 0.20 to 0.64, depending on the specific factors involved (Etnier et al., 1997; Galioto Wiedemann et al., 2014; Lambourne & Tomporowski, 2010; Wilckens et

al., 2016). Therefore, to avoid being underpowered, we used the lower bound estimate of 0.20 for the effect size. The analysis indicated that a minimum of 193 participants would be needed to achieve 80% power. A total of 201 volunteers were recruited from undergraduate psychology courses and received course credit for their time. Four participants were excluded due to computer problems that prevented data collection. Our final sample consisted of 197 participants (mean age = 20.60, SD = 2.66, 74 male).

3.1.1.3 Apparatus

All tasks and questionnaires were displayed using a 19" LCD monitor with a resolution of 1280x1024. Data collection for the computer task was conducted using the open-source Cognitive Battery 3.2 software package (<https://github.com/sho-87/cognitive-battery>), which utilizes Python 3.6.4 and Pygame 1.9.3 for stimulus display. The primary operating system was Windows 10.

3.1.1.4 Executive control task

The Eriksen Flanker task is a commonly used measure of executive control in young adults (N. Gothe et al., 2013; Sebastian Ludyga et al., 2018; Schmit et al., 2015; Soga et al., 2015; Spitzer & Furtner, 2016). However, some research suggests that the relationship between PA and executive control may only be observable if the cognitive task is sufficiently demanding (Chang, Pesce, et al., 2015; Kamijo et al., 2007; Scisco et al., 2008). We therefore used a modified version of the Flanker task that contains a difficulty manipulation to test whether difficulty has a moderating effect on the relationship. The overall task design and timings have previously been used to assess executive control in multiple age groups (Friedman et al., 2009). Each trial began with a fixation cross in the center of the screen, displayed for 1000ms. Next, flanker arrows (five horizontal arrowheads) were shown for 200ms at the center of the screen, and participants had a maximum of 1500ms (from stimulus onset) to make a response. The task contained a within-subjects congruency

manipulation (congruent/incongruent), where arrow directions could either be leftward-congruent (all arrows pointing to the left), leftward-incongruent (center arrow pointing left, flanking arrows pointing right), rightward-congruent (all arrows pointing to the right), or rightward-incongruent (center arrow pointing right, flanking arrows pointing left). Feedback was displayed after each trial with the words “correct”, “incorrect”, or “too slow” depending on the response (Charles H. Hillman et al., 2006). At the end of the trial, the fixation cross was shown again for 1500ms (intertrial interval) before the next trial began. A within-subjects difficulty manipulation (compatible/incompatible) was conducted using two blocks of 100 trials. During the compatible (easy) block, participants were simply asked to indicate the pointing direction of the central arrow, and during the incompatible (hard) block of trials, participants were asked to indicate the *opposite* direction of the central arrow. Compatible and incompatible blocks were counterbalanced between participants, stimulus conditions within each block were equiprobable, and presentation order was randomized. Task performance was calculated as the reaction time difference between congruent and incongruent trials, which was then normalized by dividing the difference by the mean congruent reaction time. The normalized reaction time is interpreted as the time difference between conditions expressed as a percentage of congruent reaction time.

3.1.1.5 Physical activity scale

Many self-report questionnaires have been developed to measure PA over multiple days or multiple months (Craig et al., 2003; Helmerhorst et al., 2012; van Poppel et al., 2010), however, few are designed to measure PA over the past 24-hours. While it's possible to re-word one of the more commonly used questionnaires to measure a 24-hour period, this approach could potentially violate the psychometric properties of the measurement instrument. Instead, we employed a questionnaire that is both designed and validated for measuring PA over a single day (Aadahl & Jørgensen, 2003). The questionnaire presents

the participant with nine activity categories of increasing intensity and asks them to report the amount of time spent performing those activities in the previous day (Appendix B). The time spent is then multiplied by the MET (metabolic equivalents) value for the category (Ainsworth et al., 2000) as duration alone does not take into consideration the intensity of the activity. MET values are multiples of resting metabolic rate and are used to weight the time spent performing an activity by its energy requirement. The MET-times are summed across all intensity categories, resulting in a single 24-hour MET-time value, which is interpreted as the time spent performing PA in the past 24-hours weighted by the energy requirements of the activities performed. Across all participants, the mean time spent performing PA was 19.16 hours (SD = 4.41), with mean MET-hours of 31.12 (SD = 11.72). A breakdown of mean reported duration for each intensity category can be found in Table 3.1.

Table 3.1 Reported PA duration for each 24-hour questionnaire category

Category	Mean Duration (hours)	SD
A	7.17	1.25
B	3.31	2.18
C	4.76	2.49
D	1.42	1.25
E	0.81	1.04
F	0.62	0.98
G	0.33	0.66
H	0.35	0.63
I	0.38	0.72

3.1.2 Results

We hypothesized that the performance penalty associated with incongruent Flanker trials would be smaller as a result of increased levels of 24-hour PA in young adults, and that the relationship would be moderated by task difficulty. Specifically, increased levels of 24-hour PA would be associated with decreased normalized reaction time difference scores between congruent and incongruent trials during the more difficult task trials. Due to the within-subjects nature of the computer task conditions, linear mixed-effects models were used to avoid data independence violations (Freeberg & Lucas, 2009; Lazic, 2010). Visual inspection of residual plots did not reveal any violations of homoscedasticity or normality. Age was used as a covariate in the models as it has been shown to covary with cognitive function (H. Lee et al., 2012; Ziegler et al., 2012). Additionally, we asked participants if they took part in regular structured exercise as a rough control of long-term exercise and included this as a binary covariate in all models. All analyses were conducted using R (3.4.3) and lme4 (1.1-17), with model parameters estimated using maximum likelihood.

We began by testing the hypothesis that the relationship between PA and executive control would only be observed during the more demanding incompatible task trials. In other words, we expected an interaction between PA and task difficulty when predicting task performance. A full interaction model (Model 1) was constructed, with executive control as the outcome. As fixed effects, we entered age, regular exercise, 24-hour PA, task difficulty, and the interaction between 24-hour PA and difficulty into the model. Additionally, random intercepts were modelled for each participant to maintain independence of data points. A reduced model (Model 2) was constructed for comparison, which predicted executive control performance from age, regular exercise, 24-hour PA, and task difficulty (with no interaction term). Random intercepts were again modelled for each participant. To test for the presence of an interaction, a likelihood ratio test was used to compare Model 1 (with interaction) and

Model 2 (no interaction). The results revealed no significant moderation by task difficulty, $\chi^2(1) = 0.61, p = .43$, suggesting that the association between 24-hour PA and executive control did not differ between the compatible and incompatible task conditions. See Table 3.2 for a summary of raw task performance data.

Table 3.2 Raw mean reaction time and accuracy for 24-hour PA study

	Mean Reaction Time (SD)	Mean Accuracy /50 (SD)
Compatible Congruent	488.03ms (84.85)	49.35 (2.87)
Compatible Incongruent	543.89ms (81.76)	47.43 (3.57)
Incompatible Congruent	512.84ms (79.95)	48.78 (2.15)
Incompatible Incongruent	540.17ms (97.55)	48.30 (2.29)

Due to the lack of a significant interaction, Model 2 was used to test the hypothesis that increased 24-hour PA is related to improved executive control. This model was compared against a reduced model that had 24-hour PA removed. Specifically, Model 3 predicted executive control from only age, regular exercise, and task difficulty, with random intercepts for each participant. The likelihood ratio test showed a significant difference between Models 2 (with 24-hour PA) and 3 (without 24-hour PA), $\chi^2(1) = 6.06, p = .01$, which suggests that 24-hour PA is a significant contributor to executive control performance. Overall, Model 2 predicted 42.04% of the variance in executive control: marginal $R^2 = 0.21$, conditional $R^2 = 0.42$ (Nakagawa & Schielzeth, 2013). As expected, participants showed a significant amount of variability in baseline executive control performance, $SD = 0.03$, 95% CI [0.02, 0.04]. Age was not found to be significantly related to executive control, $\beta = 1.13 \times 10^{-3}$, 95% CI [-0.002, 0.004], $t(196) = 0.79, p = .43$, and neither was regular exercise, $\beta = -8.01 \times 10^{-4}$, 95% CI [-0.02, 0.02], $t(196) = -0.10, p = .92$. However, both task difficulty, $\beta = -6.75 \times 10^{-2}$, 95% CI [-0.08, -0.06], $t(196) = -11.50, p < .001$, and 24-hour PA, $\beta = -8.26 \times 10^{-4}$, 95% CI [-0.0015, -0.0002], $t(196) = -2.48, p = .01$, were significant predictors of task

performance. Specifically, each additional MET-hour was associated with a reduction of 0.08% in the reaction time difference between congruent and incongruent trials. In other words, although the effect size is small, the significant effect of PA suggests that increased levels of PA are associated with better executive control.

3.1.3 Discussion

Our results showed that increased levels of PA over the past 24-hours is predictive of improved executive control in young adults and that this beneficial effect is not dependent on task difficulty. In this light, messaging the cognitive benefits of increased 24-hour PA may be a useful approach for promoting increased PA participation, although this remains an empirical question. Given our findings, several theoretical questions and issues follow.

The young adult PA literature generally shows a less consistent benefit of PA as the time window of PA assessment becomes more distal from the time of cognitive testing, which suggests a critical threshold for PA effects on executive control. For example, acute PA (administered immediately prior to testing) has shown beneficial effects on cognition (Chang, Pesce, et al., 2015; Kamijo et al., 2007; Kubesch et al., 2009; Sebastian Ludyga et al., 2018). A handful of studies have also examined the delayed impact of acute PA and show improvements to executive function for up to two hours following exercise (Basso et al., 2015; Pontifex et al., 2009), although this may depend on PA intensity and type of cognitive task (Brush et al., 2016). Others have examined longer PA time windows, however, the benefits of increased PA measured over those periods are less consistent. For example, PA measured over the week prior to cognitive testing has been shown to be both beneficial (Kamijo & Takeda, 2010, 2013; Stillman et al., 2016; Vanhelst et al., 2016) and unrelated to behavioral indices of executive control in young adults (Boucard et al., 2012; Ho et al., 2018; P. D. Loprinzi & Kane, 2015; Pindus et al., 2015). Similar inconsistencies have been observed for long-term PA measured over multiple months and years prior to cognitive

testing (Berchicci et al., 2013; Boucard et al., 2012; Charles H. Hillman et al., 2002; T. M. C. Lee et al., 2014; Padilla et al., 2014; Pérez et al., 2014; Jason R. Themanson et al., 2006). Overall, the PA literature suggests that the benefits of increased PA are most consistent if it occurs close to the time of cognitive testing. As a clear example of this, we can contrast the findings from the present study to one studying the effects of weekly PA (Ho et al., 2018). Both studies used the same Flanker task, data handling methods, and were similarly powered, with the primary difference being the time window of PA assessment. Weekly PA was shown to be unrelated to executive control (Ho et al., 2018), but here we showed a significant association with 24-hour PA. As such, PA time window appears to be an important determinant of executive control performance and there may be a critical period during which increased PA confers maximal cognitive benefits. Unfortunately, intermediate time windows are understudied and while our findings provide preliminary evidence that the consistent benefits of acute PA may also extend to the day prior to cognitive testing, further research is required to determine whether a critical PA threshold exists in young adults.

We chose a self-report questionnaire that was designed and validated for use at the 24-hour time window (Aadahl & Jørgensen, 2003). However, it should be noted that while the questionnaire has high validity against diary methods, it has relatively low validity compared to accelerometry (Aadahl & Jørgensen, 2003). Unfortunately, there are very few questionnaires designed to assess a 24-hour period in young adult populations (Helmerhorst et al., 2012; van Poppel et al., 2010), and self-report questionnaires typically have low validity compared to more direct measures of PA (Dowd et al., 2018; Helmerhorst et al., 2012; Prince et al., 2008). So, while our chosen questionnaire had low correlations with accelerometer-derived PA, this was difficult to avoid given the lack of high-validity questionnaires designed for our target population and PA time window. Moving forward, we

recommend the development of PA questionnaires that have higher criterion-validity, as well as replication of the current findings using objective methods.

We hypothesized an interaction with task difficulty such that the benefit of increased PA would only be observed during more difficult task conditions, but our results failed to support this hypothesis. Some work has shown a moderating effect of task difficulty, for example, increased PA only benefits working memory performance during more demanding task conditions (Martins et al., 2013; Pesce & Audiffren, 2011; Pontifex et al., 2009; Sibley & Beilock, 2007). A similar effect has also been observed for tasks involving executive control (Kubesch et al., 2009; Padilla et al., 2013, 2014; Pérez et al., 2014; Pontifex & Hillman, 2007; Samani & Heath, 2018). However, in the present study we found the benefit of increased PA to be more general as it was observed for both the compatible and incompatible task conditions. This discrepancy could be due to differences in statistical power between studies. The aforementioned executive control studies have much smaller sample sizes than the sample used in the present study, and the non-significant findings reported for easier task conditions could be due to insufficient power to detect small effects. Therefore, it could well be the case that task difficulty doesn't actually moderate the relationship between PA and executive control, but only appears to act as a moderator due to low power. Relatedly, the effect size we observed was extremely small as increased PA improved performance by only fractions of a millisecond, and such small effect sizes coupled with low statistical power may explain the inconsistent findings reported in the literature.

While we interpreted our results as PA being predictive of executive control, a couple of alternate interpretations are possible due to our study design. As the study was correlational, the direction of causality could be reversed and it's possible that individuals with better executive control practice PA more regularly and are therefore more likely to

have participated in PA in the previous 24-hour period. Relatedly, individuals with high levels of 24-hour PA may also have high levels of long-term PA and it is possible that we are observing the effects of increased long-term PA rather than increased 24-hour PA. We included a measure of long-term exercise, which showed no relationship with executive control, but the measure was not specific, and the lack of a detailed long-term PA measure may represent a potential confound in our study. It should be emphasized, however, that the inclusion of long-term PA as a measure is predicated on the idea that long-term/weekly PA covaries with executive function in young adults. While there is support for this idea (T. M. C. Lee et al., 2014; Padilla et al., 2013, 2014; Pérez et al., 2014), there are also many studies showing no such relationship with behavioural performance measures (Berchicci et al., 2013; Boucard et al., 2012; Charles H. Hillman et al., 2002, 2006; Ho et al., 2018; Jason R. Themanson et al., 2006). Therefore, it's not clear that the lack of a long-term PA measure necessarily confounds the results. In fact, including long-term PA may cause more problems than it solves in this particular age group. Studies have shown that PA levels decline in young adult populations (Ku et al., 2006; Spittaels et al., 2012), so any aggregated measure of long-term PA would have reduced validity as it would likely overestimate activity levels over the long term. One possible solution would be to measure yearly PA levels (over multiple consecutive years) and introduce the trend coefficient as a model covariate, but we're not aware of any studies that have adopted this approach for young adults.

PA levels are predicted to decline over the next decade (Ng & Popkin, 2012), which can have negative consequences for physical health (Hamilton et al., 2007; Owen et al., 2010). As such, it has become increasingly important to promote increased participation in PA. However, the majority of research, present article included, has focused on computer-based measures of cognitive performance, which are relatively abstract and have little relation to real-world performance. This disconnect between the measures used in the PA

literature and the outcomes that are of direct interest to the general public could make PA promotion more difficult. Therefore, we suggest an increased focus on real-world measures that are more directly relevant to the young adult population, such as academic performance. While we understand that real-world performance is multi-faceted and not directly analogous to laboratory-based measures, the idea of time window differences between weekly, 24-hour, and acute PA may extend to academic contexts as well. A handful of studies have examined the relationship between PA and academic performance in young adults, however, most either assess acute PA (Fenesi et al., 2018) or PA over longer time windows (Flueckiger et al., 2014; Kwak et al., 2009; Pellicer-Chenoll et al., 2015; Slade & Kies, 2015). To our knowledge, there have been no studies assessing 24-hour PA and young adult academic performance, and this may be a fruitful avenue for future research.

3.2 Study 2

In a manner similar to how regular PA leads to improvements in physical and mental health, engaging in regular bouts of PA has been shown to improve a range of cognitive functions in young adults, including attentional capacities (Charles H. Hillman et al., 2003; Kubesch et al., 2009), memory capacities (Martins et al., 2013; Stroth, Hille, et al., 2009), and performance monitoring processes (J.R. Themanson et al., 2008). In addition to these longer-term "pro-cognitive" effects of PA, even single bouts of PA have been found to produce improvements in cognitive function, as assessed in the minutes and hours immediately following the PA (Basso et al., 2015; Pontifex et al., 2009; Scudder et al., 2012). Given the positive relationship between daily PA and executive function shown in the previous study, one logical next step would be to see whether we converge on the same result using a real-world cognitive outcome.

For the present study, we turned to measures of student academic performance as an ideal focus for operationalizing cognitive performance in our study. For one, academic

performance is already known to improve with longer-term increases in PA levels. For example, children in the healthy fitness zone of the FITNESSGRAM test are more likely to perform better on academic math and reading tests than those not in the healthy fitness zone (Bass et al., 2013). This relationship extends to high school and university aged students, with studies showing that regular participation in sports and recreational exercise are positively associated with improved overall academic performance measures (Sigfúsdóttir et al., 2006), as well as performance on specific academic exams (Slade & Kies, 2015). However, what remains unknown is whether positive effects on academic performance are also observed following recent bouts of PA. That is, do the "pro-cognitive" effects of recent bouts of PA on cognitive function as measured in laboratory studies (Basso et al., 2015; Pontifex et al., 2009; Scudder et al., 2012) generalize to discrete, real-world measures of student academic performance?

As such, we designed a study that used university students' grades on final exams taken in their courses as our cognitive performance outcome measure. At issue was whether exam grades would positively correlate with students' self-reported levels of PA on the day immediately preceding the exam.

3.2.1 Method

3.2.1.1 Ethics Statement

Ethical approval was received through the University of British Columbia's Behavioural Research Ethics Board. A consent form was provided to each participant prior to the start of the study. We received ethics board approval to waive written consent due to time constraints in the exam environment, and as such, the consent form simply provided an explanation of the study and did not require a signature from the participant. The research was performed in accordance with ethics board guidelines.

3.2.1.2 Participants

During the recruitment process we identified psychology instructors teaching upper-level undergraduate courses at the University of British Columbia and asked if they would be willing to administer surveys during the final examination session of their respective courses. If receptive, each instructor was provided with a survey and was briefed on the study design, research question, and procedure. Courses spanned multiple fields of psychology (Cognitive, Social, Developmental, Behavioural Neuroscience, and Forensic). A total of seven courses were eventually registered in the study, six with their final exam in December and one with its final in April, with a total of 902 participants (mean age: 21.5 years, $SD = 2.9$, 715 female). Students enrolled in the courses provided their consent at the completion of their examination, and participants received no benefit or compensation for study participation. However, because it was possible for a student to be sampled multiple times by being enrolled in multiple courses, we only used data from the first exam for each of those students to avoid independence assumption violations. The first survey was selected to ensure responses were not biased by prior exposure to the survey items; in total, 106 responses were removed due to registration in multiple courses, leaving an effective sample size of 796 participants (mean age: 21.5 years, $SD = 3.0$, 625 female).

3.2.1.3 Materials and Procedure

Our goal was to design a short survey with low participation time due to the time-constrained environment. Research suggests that different types of PA may have diverse effects on cognition (Pontifex et al., 2009), therefore, we decomposed PA into three categories asking about participation in aerobic exercise, weight (or resistance) training, and stretching-related activities. Specifically, this consisted of asking participants to report the total time, in minutes, they spent engaging in each category of activity on (1) the day of and (2) the day before their exam, from which a summed composite was then created for each

category. Furthermore, we asked participants to report whether they regularly took part in exercise activities as an analogue for overall fitness. While our primary measure of interest was physical activity, we also included questions on sleep quality and amount of time spent studying, two additional factors we reasoned might impact exam performance (see Discussion below) and which were thus later used as covariates in our model. Sleep quality was reported using a 7-point Likert scale, with higher values indicating better sleep quality. We also asked for participant age and gender, and for their individualized student number, which was used during data analysis to anonymously match each students' survey to her/his exam score as per described below.

Interested students were provided with both the study consent form and survey form. At the beginning of the examination session, the research team provided information on the consent procedure and instructions for completing the survey via a general announcement to the class. Students were informed that participation was optional, would not affect their course standing, and that their course instructor would be blind to student enrollment in the study. To help minimize impact on the examination itself, students—if interested in participating—were instructed to read the consent form and complete the survey once they had finished their examination but before returning it to the instructor. Several weeks later, the instructors from each of the participating courses were contacted to obtain their courses' final exam scores listed by student number only (i.e., without names), which were then matched to the completed surveys during data coding/analysis. The marks themselves were not letter grades, but rather, percentages of total possible points on each exam. Because the Psychology Department has a grade scaling policy in place, we asked that instructors submit only unscaled marks for data analysis.

3.2.2 Results

The descriptive statistics of our sample are shown in Table 3.3. As expected, we found that mean grade varied between classes, owing to likely differences in content difficulty, type, form and length of the exam taken, and grading styles of the different instructors. Consistent with this, the intra-class correlation was calculated on exam grade grouped by the different classes, and was estimated to be significant (0.11, with 95% CI [0.05, 0.40]). Due to this significant intra-class correlation, we performed a linear mixed-effects regression with class as a random effects variable to account for differences in exam grade. Specifically, our model predicted exam grade from our PA measures (aerobic, resistance, stretching), regularity of exercise, sleep quality, and hours spent studying for the exam, with class as a random effects variable allowing for varying intercepts. All variables were standardized, parameters were then estimated using maximum likelihood, and Satterthwaite approximations of the degrees of freedom were used for t-tests. Regression assumptions were checked by visual inspection of quantile-quantile and normality plots, and no violations were indicated. All analyses were conducted using R 3.4.1.

Table 3.3 Descriptive characteristics of participants for exam study

Course	#1	#2	#3	#4	#5	#6	#7
n	113	64	121	128	180	142	48
Grade (%)							
<i>M</i>	68.95	69.97	78.31	64.28	67.32	69.92	64.97
<i>SD</i>	13.24	18.47	10.73	12.56	11.82	10.83	15.37
Age (years)							
<i>M</i>	21.17	21.43	21.34	21.71	21.28	22.12	22.00
<i>SD</i>	1.77	2.66	2.20	3.95	1.85	4.67	1.87

Course	#1	#2	#3	#4	#5	#6	#7
Time spent studying (hours)							
<i>M</i>	7.31	6.83	5.45	6.07	6.08	6.12	8.26
<i>SD</i>	3.39	4.16	3.03	3.66	3.89	3.48	3.52
Aerobic PA (minutes)							
<i>M</i>	11.55	12.06	14.15	17.09	11.67	21.28	14.46
<i>SD</i>	19.96	22.92	21.85	48.76	25.74	44.67	23.80
Resistance PA (minutes)							
<i>M</i>	4.29	3.59	5.08	4.89	5.40	8.24	6.71
<i>SD</i>	17.20	17.12	17.90	16.57	18.59	22.68	17.92
Stretching (minutes)							
<i>M</i>	5.59	3.39	5.59	3.91	4.99	7.83	4.27
<i>SD</i>	13.03	8.60	13.56	11.62	14.84	19.86	11.16
Sleep quality (7pt-Likert)							
<i>M</i>	4.19	3.38	4.84	4.31	3.50	4.23	4.09
<i>SD</i>	1.34	1.56	1.31	1.44	1.50	1.56	1.36

Overall, the model predicted 11.28% of the variance in exam grade: marginal $R^2 = 0.02$, conditional $R^2 = 0.11$ (Nakagawa & Schielzeth, 2013). The standard deviation around the mean intercept was found to be significant, $SD = 0.30$, 95% CI [0.18, 0.58], suggesting a significant degree of variation in the mean exam grade between the different classes. Minutes of aerobic physical activity significantly predicted exam grade, $\beta = 0.08$, 95% CI [0.01, 0.14], $t(749) = 2.19$, $p = 0.03$, suggesting that higher amounts of aerobic PA over the

past day was associated with an increase in exam performance. Resistance PA was negatively associated with grade, $\beta = -0.07$, 95% CI [-0.14, -0.004], $t(748.60) = -2.07$, $p = 0.04$, suggesting that higher resistance PA over the past day associated with a decrease in exam performance. Finally, sleep quality was also significantly predictive of exam performance, $\beta = 0.10$, 95% CI [0.03, 0.17], $t(754.80) = 2.68$, $p = 0.01$, suggesting that an increase in self-perceived quality of sleep was associated with improved exam grade. None of the other covariates in the model significantly predicted exam performance (see Table 3.4).

Table 3.4 Standardized coefficients for fixed effects in exam study

	β	$SE \beta$	df	t	p	.95CI
Time spent studying	0.00	0.04	753.10	0.12	.91	[-0.07, 0.07]
Aerobic PA	0.08	0.03	749.00	2.19	.03	[0.01, 0.14]
Resistance PA	-0.07	0.03	748.60	-2.07	.04	[-0.14, -0.004]
Stretching	-0.03	0.03	749.20	-0.94	.35	[-0.10, 0.03]
Regular exercise	0.02	0.07	750.20	0.21	.83	[-0.13, 0.16]
Sleep quality	0.10	0.04	754.80	2.68	.01	[0.03, 0.17]

Note. Regular exercise was coded as participated = 1, and no participation = 0.

3.2.3 Discussion

The aim of our study was to examine the short-term relationship between PA levels and cognitive performance, on the idea that the tight temporal coupling between PA and cognitive functioning as documented in laboratory studies (Basso et al., 2015; Charles H. Hillman et al., 2003; Kubesch et al., 2009; Martins et al., 2013; Pontifex et al., 2009; Scudder et al., 2012; Stroth, Hille, et al., 2009; J.R. Themanson et al., 2008) might provide a promising focus for public health messaging less susceptible to temporal discounting biases known to negatively impact peoples' propensity to engage in health-promoting behaviors

(Chapman, 1996; Critchfield & Kollins, 2001; Lynch Jr & Zauberman, 2006; Zauberman & Urminsky, 2016). In this regard, we found that students' self-reported levels of aerobic PA in the day prior to taking a university final exam was positively correlated with their performance on the exam. We suggest that this provides novel preliminary evidential support for messaging the relatively immediate positive benefits of PA to young, student-aged adults in a direct, non-abstracted manner that speaks to an outcome of high personal relevance to many—*Want to do better on an upcoming test or exam? Try being a bit more aerobically active in the day before.*

Although from a public health perspective our findings should be taken as preliminary, we suggest that their validity is supported in two complimentary ways. First, as noted above single bouts of PA have been shown to have significant "pro-cognitive" effects in the minutes to hours following a workout (Basso et al., 2015; Pontifex et al., 2009; Scudder et al., 2012), while at the same time, academic performance has been reported to improve with longer-term increases in PA levels (Bass et al., 2013; Sigfúsdóttir et al., 2006; Slade & Kies, 2015). Taken as such, our study results are not only consistent with these two distinct lines of research, but more importantly, in finding a positive association between exam performance and short-term self-reported PA levels, our study results provide novel empirical evidence that helps to unify our understanding on this front. In particular, if short-term PA levels can immediately improve cognitive function, and if longer-term PA levels can improve academic performance, we now have data to suggest that short-term PA levels may be capable of immediately improving academic performance as well.

Second, although our study was focused on the relationship between PA and cognitive performance, a large body of research has shown that sleep is another significant factor affecting cognitive function (de Bruin et al., 2017; Killgore, 2010). For example, lower amounts of total sleep time hinders executive function (Sadeh et al., 2002; Vriend et al.,

2013) and academic performance (Dimitriou et al., 2015; Titova et al., 2015). However, when the distinction is made between sleep duration and sleep quality, sleep quality has been shown to be not only a better predictor of academic performance than sleep duration (Dewald et al., 2010), but as well, it is predictive of better academic grades (Gilbert & Weaver, 2010; Gomes et al., 2011), lower probability of academic failure (Titova et al., 2015), and improved school functioning (Meijer et al., 2000). While our study intent wasn't to examine issues of sleep quality per se, we consider it here because not only did we find self-reported PA to positively impact exam performance, but we found that self-reported sleep quality did as well. Given that the literature on sleep quality and academic performance predict that the former should positively correlate with the latter, this additional finding in our study provides key normative checks on both the validity of our academic performance measure (a university final exam) and the self-reports of the study participants themselves. Moreover, by including sleep quality as a factor in our statistical model, it mitigates possible concerns that our main finding—short-term PA positively correlated with exam performance—was actually due to aerobic PA indirectly improving performance by way of sleep quality acting as a mediator (Driver & Taylor, 2000; King et al., 1997; Kredlow et al., 2015; Passos et al., 2011; Reid et al., 2010; Wilckens et al., 2016; Youngstedt et al., 1997).

In arguing for the validity of our findings, however, we also acknowledge important limitations of our study and its design. First, our sample size was range-restricted with respect to the sampled student age and academic discipline of examination. Whether our findings generalize to younger, high school-aged students or older, more mature students, and whether they generalize to academic performance in disciplines beyond psychology both remain to be empirically demonstrated. Second, the majority of our data were collected during the month of December, a time of year that is notoriously cold, dark, and rainy in

Vancouver and thus not optimal for engaging in out-of-the-home exercise, be it running outdoors or travelling to a gym. Likewise, many students may simply not prioritize PA (or exercising) during final examination periods, a time when student culture places a strong emphasis on studying course materials over other time investments. Consistent with this, the distributions of our PA related variables were somewhat skewed, with many participants reporting zero overall minutes of any kind of PA on their survey. As such, although we found a significant impact of aerobic PA on exam performance, it is unclear whether this relationship would hold if higher PA levels were better represented in the data set. In a similar vein, we found a negative relationship between resistance PA and exam performance. While aerobic vs. resistance PA are known to differentially impact behavioural performance and functional brain activity (Pontifex et al., 2009; Voelcker-Rehage et al., 2011; Voss et al., 2011), our sample may have been particularly range-restricted with regards to endorsement of resistance PA: over 88% of participants reported zero minutes in total. So again, whether the relationship between exam performance and resistance PA would hold if participants report higher activity levels remains to be tested. Finally, we kept our study survey short in order to minimize participation time and student stress during exams. However, both PA and sleep are rich and complex constructs that likely cannot be fully appreciated through a small handful of questions. Whether our current pattern of findings would thus hold if more established/validated questionnaires like the International Physical Activity Questionnaire (Craig et al., 2003) and the Pittsburgh Sleep Quality Index (Buysse et al., 1989) were employed remains uncertain.

PA is recognized as a vitally important target for public health messaging (Heath et al., 2012; Warburton & Bredin, 2016) owing to its broad spectrum of positive physical and mental health benefits (Buchman et al., 2012; Carnethon, 2009; Soares-Miranda et al., 2016). By providing preliminary evidence showing that recent aerobic PA levels positively

correlate with performance on university exams, our findings highlight the potential promise of promoting PA in young adults based not on its longer-term consequences for improved physical health in general, but on its more immediate, tangible, short-term "pro-cognitive" effects.

3.3 Bridging Summary

Chapter 2 showed no relationship between weekly PA and executive function, but in this chapter, we see the PA effect emerge after narrowing the PA time window to the day prior to cognitive testing. Furthermore, the relationship can also be seen using a real-world measure of cognitive function, namely a university exam. This set of findings suggests that PA time window may be an important factor to consider when looking at young adult PA effects, although no definitive conclusion can be drawn as moderation was not explicitly tested. So far, I have looked at how PA can affect cognition, and in the next chapter I will examine the bidirectional nature of that relationship by looking at how changes in cognition (specifically, cognitive load) can impact low-intensity PA like walking.

4 Cognitive load and walking

Traditionally, dynamic adjustment of gait is seen as a way to minimize energy cost during ambulation (Bertram & Ruina, 2001; Holt et al., 1995), and in fact, humans continuously optimize energetic cost in real-time, even if the savings are small (Selinger et al., 2015). However, energy conservation is only part of the reason for changing gait dynamics. For example, visual information from the external environment is often used to guide foot placement (Matthis & Fajen, 2013, 2014), and in turn environmental complexity changes where we look when walking (Matthis et al., 2018). The interaction between gait and environment suggests that some level of cognitive processing is required for effective ambulation. Consistent with this, the maintenance of postural and gait control both require attentional resources, and are highly dependent on cognitive function (Hausdorff et al., 2005; Woollacott & Shumway-Cook, 2002). Furthermore, availability of cognitive resources affects our ability to handle environmental challenges (e.g. avoiding obstacles) while walking (H.-C. Chen et al., 1996; Hausdorff, 2005). The external environment also imposes demands on our cognitive systems (Berman et al., 2008), and fluctuations in environmental load should be reflected in real-time gait dynamics during outdoor walking. However, gait is difficult to measure in outdoor environments owing to a lack of portable measurement tools, but wearable technology may provide a way to measure gait dynamics in more natural contexts. The present study therefore set out to examine whether the availability of cognitive resources affects gait dynamics during outdoor walking, using a methodology that is more appropriate for real-world gait assessment.

In the laboratory, the relationship between gait dynamics and cognition is often studied with dual task paradigms: if walking requires cognitive resources, then increasing the difficulty of a concurrent secondary task should result in changes to the dynamics of gait (e.g. speed, step time variability, regularity). This effect has been well documented; for

example, research has shown that walking under high cognitive load is associated with worse performance on a stepping stone task (Alexander et al., 2005), reduced ability to avoid obstacles (H.-C. Chen et al., 1996), reduced gait speed and leg swing time (Hausdorff et al., 2008), or increased number of missteps in complex walking environments (Schaefer et al., 2015).

There are a couple of methodological issues in the gait literature that our study aims to address. The first concerns the difference between treadmill and overground walking. Many studies examining the relationship between gait dynamics and cognitive function have used treadmill walking, finding, for example, that variability in gait parameters increases under higher levels of cognitive load (Grabiner & Troy, 2005; Magnani et al., 2017; Szturm et al., 2013). However, treadmill walking has been shown to be different from overground walking (Carpinella et al., 2010; Row Lazzarini & Kataras, 2016) and can produce significantly reduced variability in stride time and upper and lower body acceleration, even at self-selected speeds (Dingwell et al., 2001). Therefore, findings from treadmill-based studies cannot reliably be generalized to overground walking.

The second methodological issue concerns indoor vs. outdoor walking. Many cognition and gait studies have been conducted indoors due to the use of treadmills or 3D motion capture systems, but these studies don't usually test the impact of the environment itself on gait, and findings therefore do not necessarily generalize to environments outside of the laboratory. Walking in a laboratory is likely to be different from outdoor walking as cognitive load can originate from different sources. For example, visual stimuli in urban environments might impose a high demand on cognition, while natural environments may have a restorative effect on attention (Berman et al., 2008). In other words, explicitly manipulated load in the laboratory via a cognitive dual task may not have the same impact on gait as load that is inherent to an outdoor environment.

The ability to study outdoor gait dynamics would provide an opportunity to assess ambulation in cognitively demanding environments where it has traditionally been difficult to perform gait assessment. For example, crossing the street while under cognitive load has been studied in virtual reality (Nagamatsu et al., 2011), but gait dynamics are likely to be different at a real intersection where the consequences of unstable gait are more severe. Being able to study environmental effects on gait in the real world could help inform the design of public spaces, such as reducing levels of load at crosswalks to reduce mobility-related traffic accidents.

The competition for cognitive resources between cognition and gait may also contribute to increased fall risk in the elderly (especially in cognitively demanding environments), with divided attention resulting in reduced obstacle avoidance abilities and an increase in missteps (H.-C. Chen et al., 1996; Schaefer et al., 2015). Gait dynamics can be used as a proxy for the cognitive demand of an outdoor environment and may help us understand whether certain environments are more cognitively challenging and thus contribute to increased fall risk.

The goal of the present article is to determine whether cognitive load from environmental and task-related sources impacts gait during indoor and outdoor walking, and whether findings from indoor studies generalize to outdoor environments. Findings from previous studies lead to the prediction that gait should become slower and more variable as levels of cognitive load increase, and that this relationship should be observable both indoors and outdoors, regardless of the source of the cognitive load (internal task demands vs. external/environmentally induced). We tested this hypothesis across four studies. First, we explicitly manipulated levels of cognitive load using a verbal dual task to determine whether the relationship is detectable using a smartphone during an indoor walk (Study 1). Next, we validated the use of fractal dimension from outdoor photos as an operational

definition of environmental load (Study 2). Finally, we tested whether the load imposed by natural environments, as measured by fractal dimension, impacts gait dynamics during outdoor walking (Studies 3 and 4).

4.1 Study 1

First, we tested the relationship between cognitive load and gait dynamics in a controlled environment as a validation of our smartphone methodology. We predicted that smartphone-based accelerometry, during indoor overground walking, should be sensitive enough to detect a relationship between load and gait, specifically that increased levels of cognitive load would result in slower and more variable gait. To test this, participants were asked to walk down a hallway while vertical acceleration was measured using a smartphone accelerometer. Participants were placed under varying degrees of cognitive load using a verbal dual task.

4.1.1 Ethics Statement

Ethical approval was received through the University of British Columbia's Behavioural Research Ethics Board, written informed consent was obtained from each participant prior to the start of the study, and research was performed in accordance with ethics board guidelines.

4.1.2 Participants

A power analysis was conducted to determine minimum sample size for the study. We used a conservative effect size estimate of 0.1 (Cohen's d) as we were unsure of the magnitude of the relationship between gait and our chosen cognitive task, especially when measured using smartphone accelerometry. We also expected a violation of the homogeneity of variance assumption owing to differences in step variability across conditions; thus, a non-sphericity correction was applied conservatively at 0.6. The analysis

indicated that at least 140 participants would be required to achieve 80% power. A total of 154 participants (mean age = 20.50, $SD = 3.25$, 35 male) were recruited for the study. Participants were undergraduate student volunteers and received course credit for their time.

4.1.3 Apparatus

An LG Nexus 5X smartphone (<http://www.lg.com>) was used for data collection. This particular model was chosen due to its wide range of integrated sensors; however, any smartphone with a built-in accelerometer and gyroscope would be suitable. The smartphone was mounted in front of the participant's sternum, using an adjustable Action Mount chest harness (<http://action-mount.com>), with the accelerometer's positive X-axis pointing upwards. A second smartphone, of the same model, was used concurrently by the researcher to start and stop recording, and to set the current trial and condition number.

The phones were running Android 7.0 and paired via Bluetooth 4.0. An application was created to record sensor data from the participant's phone. Of primary interest was vertical acceleration, sampled at a frequency of 100 Hz. The Android API also allows for the collection of linear acceleration data (acceleration minus the effect of gravity); however, we found it to have lower precision than manually calculating the value. Therefore, we also recorded gravity sensor values, which used the built-in gyroscope, and subtracted them from the acceleration vector for the calculation of linear acceleration. For devices without a gyroscope or gravity sensor, the tilt of the device (and thus the gravity component) can be isolated using traditional techniques such as applying a low-pass filter to the acceleration signal (Rosenberger et al., 2013).

4.1.4 Method

After providing signed consent, participants wore a chest harness with the phone secured directly in front of their sternum. They were asked to walk, at a self-selected pace, down a 30m hallway while performing a concurrent verbal task. The oral version of the trail making test (OTMT) was used for the low and high load conditions (Abraham et al., 1996; Ricker & Axelrod, 1994). In the low load condition (OTMT-A), participants were asked to count numbers (e.g., 1, 2, 3) while walking. In the high load condition (OTMT-B), participants were asked to alternate between numbers and letters (e.g., 1, A, 2, B, 3, C) while walking. We also included a control condition where no verbal task was performed and simply involved walking from one end of the hallway to the other. Each trial type was repeated 3 times, with the load order counterbalanced between participants, giving us a total of 9 trials per participant. Participants were asked to continue walking even if they committed a counting error. Two seconds were removed from the start and end of each trial as we were interested in the period where participants were walking at their self-selected speed, rather than the periods where they were accelerating from (or decelerating to) a standing position.

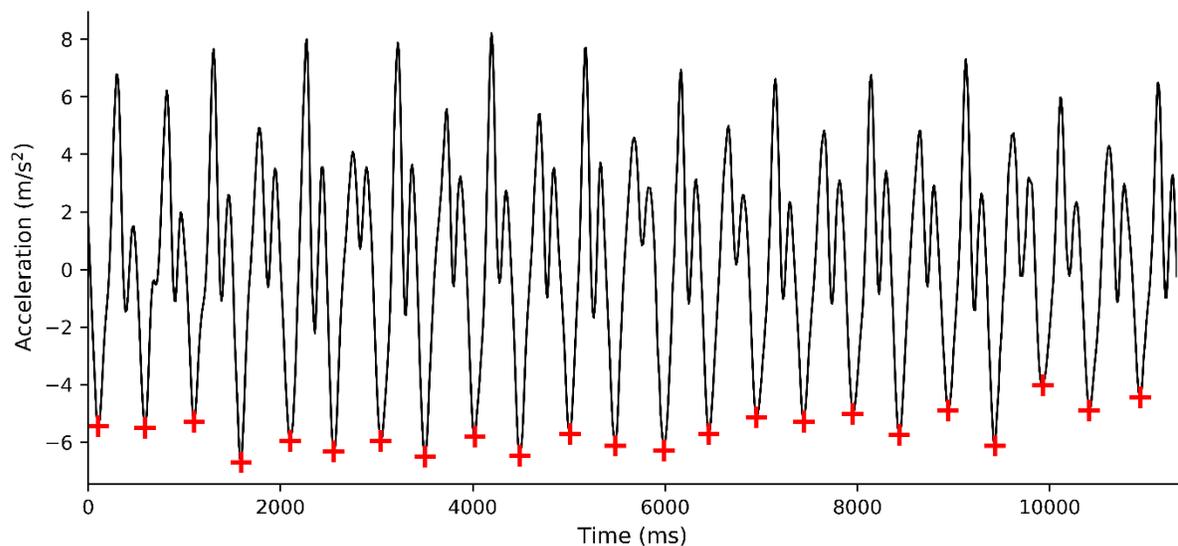
Two research assistants coded the individual steps as per the procedure described in the following section. Trials were excluded from analysis if there was a lack of agreement regarding step placement between the coders, resulting in 28 excluded trials (2% of total data). Participants with over 50% excluded trials were removed from analysis entirely, resulting in the removal of two subjects.

4.1.5 Calculating Gait Dynamics

First, linear acceleration was calculated by subtracting the gravity component from the acceleration vector. Previous work has shown that gait cycle frequency is typically below 8 Hz (K. Y. Chen & Bassett, 2005; Lan & Shih, 2013), thus filtering below that frequency

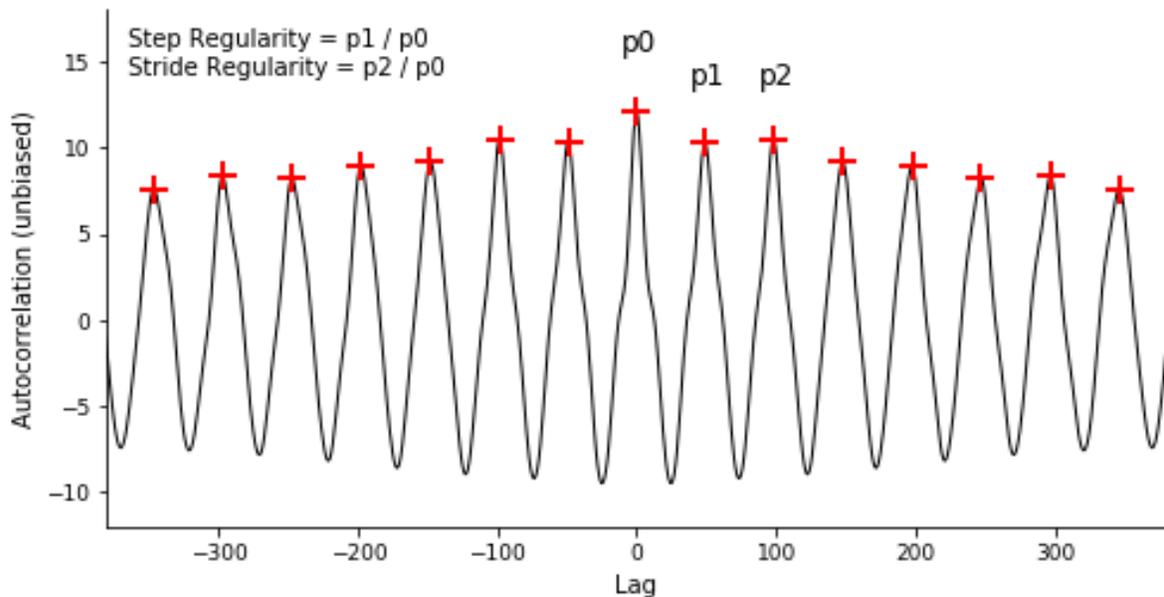
would remove valuable gait information. Therefore, to provide additional headroom in the filtering process, a low-pass zero-lag Butterworth filter was applied at 10 Hz to remove high-frequency noise from the linear acceleration signal. The gait cycle in vertical acceleration data typically consists of valleys occurring when the heel strikes the ground and the first peak occurring when entering the stance phase, forming a rough “M” shaped pattern in the signal (Lan & Shih, 2013). The acceleration signal can be quite different for each participant due to individual differences in gait dynamics, and automated extraction of the valleys could lead to false positive identification. Research assistants blind to the hypothesis manually coded step events by identifying the valleys at the start and end of each gait cycle. A few guidelines were followed to ensure coding consistency: acceleration between steps should follow a rough “M” shaped pattern, individual differences in gait can cause deflections in the signal before or after the “M” shape, the tallest peak in the cycle usually follows the initial step of the gait cycle, and the steps are not always the lowest valleys in the cycle. See Figure 4.1 for an example of typical vertical acceleration data during overground walking, where crosses in the main valleys indicate individual steps.

Figure 4.1 Example of typical vertical acceleration data from overground walking



From the times of the valleys we calculated step time (time between each step), and step time variability (standard deviation of step time). The use of acceleration data also allowed us to examine two measures of gait regularity: step regularity and stride regularity. Step regularity is the similarity in accelerative force between one step and the subsequent (contralateral) step (e.g., left foot and right foot steps) and stride regularity is the similarity between a step and the next ipsilateral step (e.g., left foot and next left foot step). These were calculated by examining the unbiased autocorrelation of the vertical acceleration signal (Moe-Nilssen & Helbostad, 2004). The autocorrelation is a time-lagged correlation between a signal and itself. Given the periodic nature of gait, the time-lag allows one to examine the similarity between different steps in the gait cycle. The largest peak (p_0) of an autocorrelation occurs at zero-lag as it is simply the correlation between a signal and itself. The next peak (p_1) occurs when one signal is time-shifted such that the correlation will be between a step and the subsequent step. Peak p_2 measures the correlation between a step and the next ipsilateral step (i.e. one stride). Therefore, step regularity is calculated as the ratio of p_1 and p_0 , and stride regularity is the ratio of the p_2 and p_0 (Figure 4.2).

Figure 4.2 Example autocorrelation of vertical acceleration



4.1.6 Results

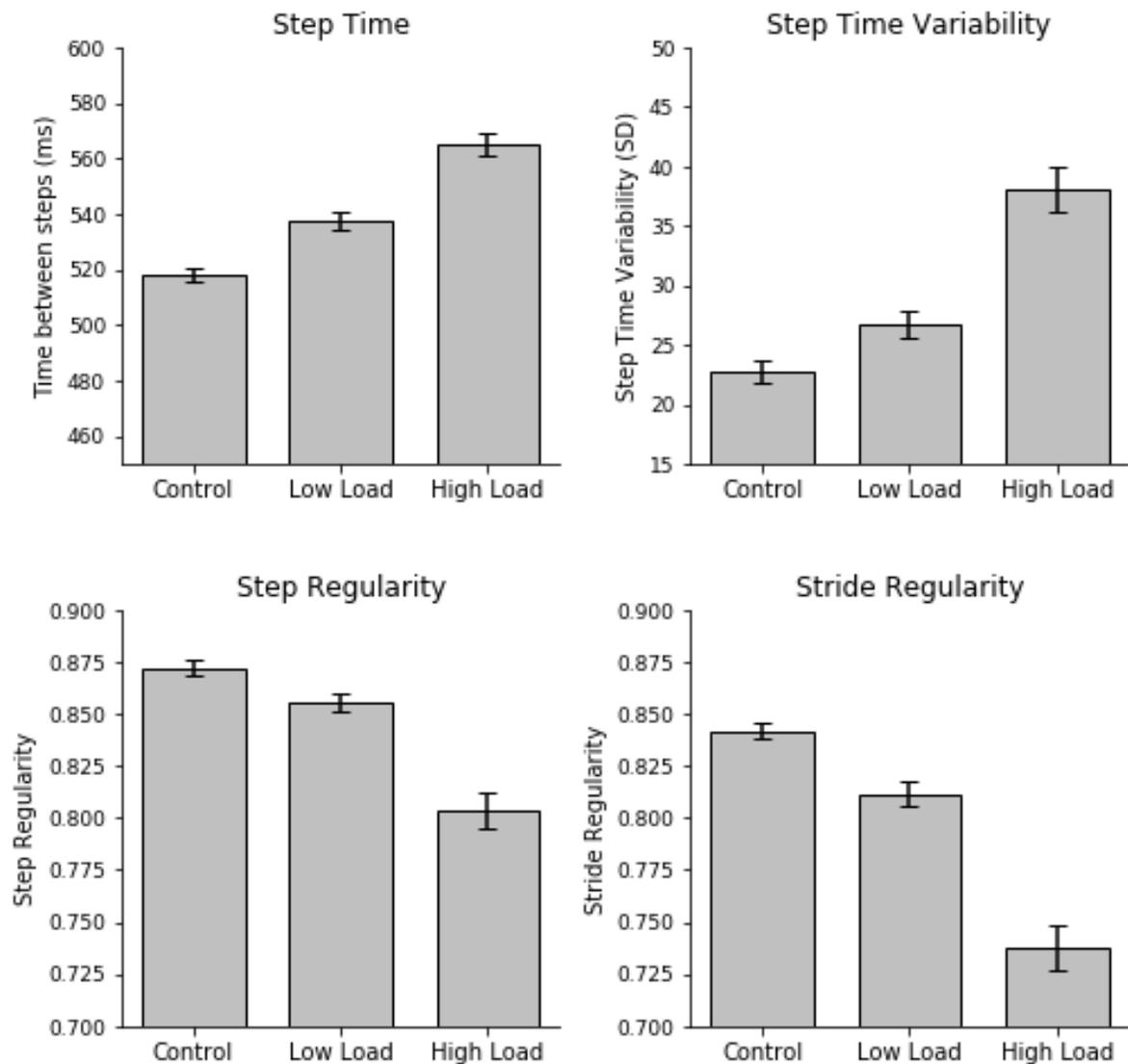
We hypothesized that increasing cognitive demand would cause gait to become slower and more variable during indoor walking. Therefore, we tested whether there was a difference between the control, low, and high load conditions with regard to the previously mentioned gait parameters.

4.1.6.1 Step Time

A one-way repeated measures ANOVA was conducted to determine the effect of cognitive load on step time (time between individual steps) (Figure 4.3). The mean (and standard deviation) of step time for the control, low, and high load conditions were 518.38 (31.28), 537.39 (37.91), and 565.20 (51.56) milliseconds, respectively. Mauchly's test indicated a sphericity violation, $\chi^2(2) = 109.91$, $p < .001$, and Greenhouse-Geisser adjustments were therefore used for the following tests ($\epsilon = .66$). A significant main effect of cognitive load was observed, $F(1.31, 197.14) = 195.49$, $p < .001$, $\eta_p^2 = .566$. Multiple

comparisons with Bonferroni adjustment indicated significant differences for all pairwise comparisons ($p < .001$).

Figure 4.3 Gait parameters across different levels of cognitive load in Study 1



4.1.6.2 Step Time Variability

A one-way repeated measures ANOVA was conducted to determine the effect of cognitive load on the variability (SD) of step time (Figure 4.3). The mean (and standard deviation) of step time variability for the control, low, and high load conditions were 22.71

(11.62), 26.63 (14.07), and 38.08 (24.29) milliseconds, respectively. Mauchly's test indicated a sphericity violation, $\chi^2(2) = 150.85$, $p < .001$, and Greenhouse-Geisser adjustments were therefore used for the following tests ($\epsilon = .61$). A significant main effect of cognitive load was observed, $F(1.22, 183.30) = 63.58$, $p < .001$, $\eta_p^2 = .298$. Multiple comparisons with Bonferroni adjustment indicated significant differences for all pairwise comparisons ($ps < .001$).

4.1.6.3 Step Regularity

A one-way repeated measures ANOVA was conducted to determine the effect of cognitive load on step regularity (Figure 4.3). The mean (and standard deviation) of step regularity for the control, low, and high conditions were .87 (.05), .86 (.05), and .80 (.11), respectively. Mauchly's test indicated a sphericity violation, $\chi^2(2) = 167.02$, $p < .001$, and Greenhouse-Geisser adjustments were therefore used for the following tests ($\epsilon = .60$). A significant main effect of cognitive load was observed, $F(1.20, 179.21) = 65.19$, $p < .001$, $\eta_p^2 = .303$. Multiple comparisons with Bonferroni adjustment indicated significant differences for all pairwise comparisons ($ps < .001$).

4.1.6.4 Stride Regularity

A one-way repeated measures ANOVA was conducted to determine the effect of cognitive load on stride regularity (Figure 4.3). The mean (and standard deviation) of stride regularity for the control, low, and high conditions were .84 (.04), .81 (.07), and .74 (.13), respectively. Mauchly's test indicated a sphericity violation, $\chi^2(2) = 113.94$, $p < .001$, and Greenhouse-Geisser adjustments were therefore used for the following tests ($\epsilon = .65$). A significant main effect of cognitive load was observed, $F(1.30, 195.50) = 81.11$, $p < .001$, $\eta_p^2 = .351$. Multiple comparisons with Bonferroni adjustment indicated significant differences for all pairwise comparisons ($ps < .001$).

4.1.7 Discussion

Overall, our results show strong support for our hypothesis that cognitive load is related to gait during indoor walking in our young participant cohort. Specifically, gait became less stable under high levels of cognitive load, resulting in increases in step time and step time variability, as well as decreases in step and stride regularity. Importantly, these differences were detectable using smartphone accelerometry. Research on postural control suggests that verbal articulation may be a large contributor to dual task deficits and postural instability (Dault et al., 2003). We included a control condition that didn't require articulation and found that even the low load condition was enough to destabilize gait, which may provide some support for the verbal articulation hypothesis. However, our results also show significant differences between our low and high load conditions (both of which contain a verbal component), across multiple gait parameters, suggesting that cognitive load may act as an additional contributor to young adult gait control, over and above any effect verbal articulation may have.

4.2 Study 2

While it has traditionally been challenging to measure gait outdoors, it is also difficult to quantify the cognitive demand of an outdoor environment. Therefore, before we can study the relationship between environmentally induced cognitive load and gait in outdoor environments, we first need an operationalization of load. One operational definition of cognitive demand is the fractal dimension (FD) of a visual scene. Fractals are shapes that have some level of self-similarity at different scales, and fractal dimension is a measure of that complexity or self-similarity (Mandelbrot, 1982). FD of a 2D image is bounded between 1 and 2 (for topological reasons) and is expressed as the number of copies of the original shape needed to fill space at a certain scale. For example, a high-fractal shape retains its complexity and self-similarity at increasingly smaller levels of scale, while a low-fractal

shape becomes simpler when scale is reduced. As such, high-fractal images contain repetitive information, and their repetitive nature results in increased perceptual fluency and confers lower cognitive demand than low-fractal counterparts (Joye et al., 2016). Furthermore, fractals are often found in natural environments (Mandelbrot, 1982; Spehar et al., 2003) and it is therefore not surprising that images of nature are found to have a restorative effect on attention (Berman et al., 2008). While there is some evidence to support the relationship between FD and cognitive load in computer-generated imagery (Joye et al., 2016), we ran a validation study to test whether FD of real outdoor photographs was related to cognitive load. The primary objective was to determine whether 1) fractal dimension can distinguish between different types of outdoor environments, and 2) outdoor images with low fractal dimension are more cognitively demanding to process than high-fractal images. To test this, we showed participants images of urban and natural scenes and timed their responses to a question about the image. Fractal dimension was then calculated for each image and correlated with their response time. The goal was to validate the relationship between FD and cognitive load, with the idea of using FD as a measure of cognitive demand in outdoor walking studies.

4.2.1 Participants

A power analysis was conducted to determine minimum sample size for this study. An effect size of 0.3 was used, which was estimated from a study examining the relationship between fractal dimension and cognitive function (Joye et al., 2016). This effect size is also in line with the smallest effect observed in Study 1. The power analysis showed that a minimum of 37 participants would be required to achieve 80% power. A total of 52 undergraduate student volunteers (mean age = 20.86, $SD = 2.33$, 9 male) were recruited from the Greater Vancouver area and received course credit for their time.

4.2.2 Method

Participants saw 50 photos of urban scenes and 50 photos of natural scenes in randomized order (Figure 4.4). Each image had pixel dimensions of 1280x800, was shown for 5 seconds, and presentation order was randomized between participants. Upon seeing each image, participants were asked “how uncomfortable is this image to view?”, which was rated using a 7-point Likert scale. This question was asked primarily to focus the participant’s attention on the image. Their image rating was recorded along with their response time, and the Minkowski–Bouligand fractal dimension was calculated for each image (Schroeder, 1991). The colour images were normalized, converted to grayscale, and finally binarized using the mean image value, before the box counting algorithm was run over a range of box sizes to calculate fractal dimension.

Figure 4.4 Examples of urban and nature photographs presented in Study 2



4.2.3 Results

We predicted that 1) nature scenes would have a higher fractal dimension than urban scenes, and more importantly 2) participants would respond faster to images with higher fractal dimension owing to their reduced cognitive demand. First, we conducted Welch's t-test to determine whether there was a difference in FD between urban and nature images. The mean (and standard deviation) of FD for the urban and nature images were 1.62 (0.09) and 1.74 (0.09), respectively. A significant difference between the image types

was observed, $t(97.84) = 6.97, p < .001$. This result suggests that images of nature have a higher fractal dimension than urban images.

Next, we employed a linear mixed-effects model to determine the relationship between FD, self-reported visual discomfort, and response time. All variables were first standardized, then we predicted response time from FD and visual discomfort, modelling random intercepts and random slopes for both relationships. The random effects were grouped by subject as we expected the relationships to be clustered. FD was found to be significantly negatively related to response time, $\beta = -0.12, t(51.34) = -7.56, p < .001$. This finding suggests that as fractal dimension increases, response time to the image decreases (i.e. becomes faster). Additionally, self-reported visual discomfort was positively related to response time, $\beta = 0.18, t(48.12) = 6.16, p < .001$, showing that participants responded slower to images they found more uncomfortable.

4.2.4 Discussion

Outdoor environments naturally impose demands on cognition; for example, urban environments have high attentional demands while images of nature are thought to have a restorative effect on attention (Berman et al., 2008). Additionally, research suggests that high-fractal imagery imposes lower cognitive demands, thus freeing resources for concurrent tasks (Joye et al., 2016). Our results are in line with these findings, showing that 1) images of nature have a higher fractal dimension than urban images, and 2) high-fractal images are associated with faster responses. That high-fractal dimension was associated with faster responses suggests that high-fractal scenes may be easier to process and less cognitively demanding. Furthermore, FD was predictive of response time independent of any contribution from self-reported visual discomfort. It is possible that participants simply preferred nature scenes, resulting in faster responses, and our results cannot exclude this possibility as we did not measure image preference. However, research using computer-

generated fractal imagery shows that fractal dimension is related to cognitive function (Joye et al., 2016), and this is independent of factors such as preference for nature scenes (as only computer-generated shapes were used). Overall, these findings present fractal dimension as a good candidate for quantifying environmentally induced cognitive load in outdoor walking environments.

4.3 Study 3

To assess the relationship between gait dynamics and cognitive load during outdoor walking we used a smartphone to record vertical acceleration during a walk along a pre-chosen outdoor route. Cognitive load along the route was operationalized as fractal dimension (FD) of photos captured at a series of locations along the walk. The results from Study 2, and from studies using computer-generated fractal images (Joye et al., 2016), show that high fractal dimension confers low cognitive demand. We therefore used the phone's camera to take photographs at preselected points along the walking route, calculated fractal dimension for each photograph, and correlated it with gait parameters from the same time point as the image.

4.3.1 Participants

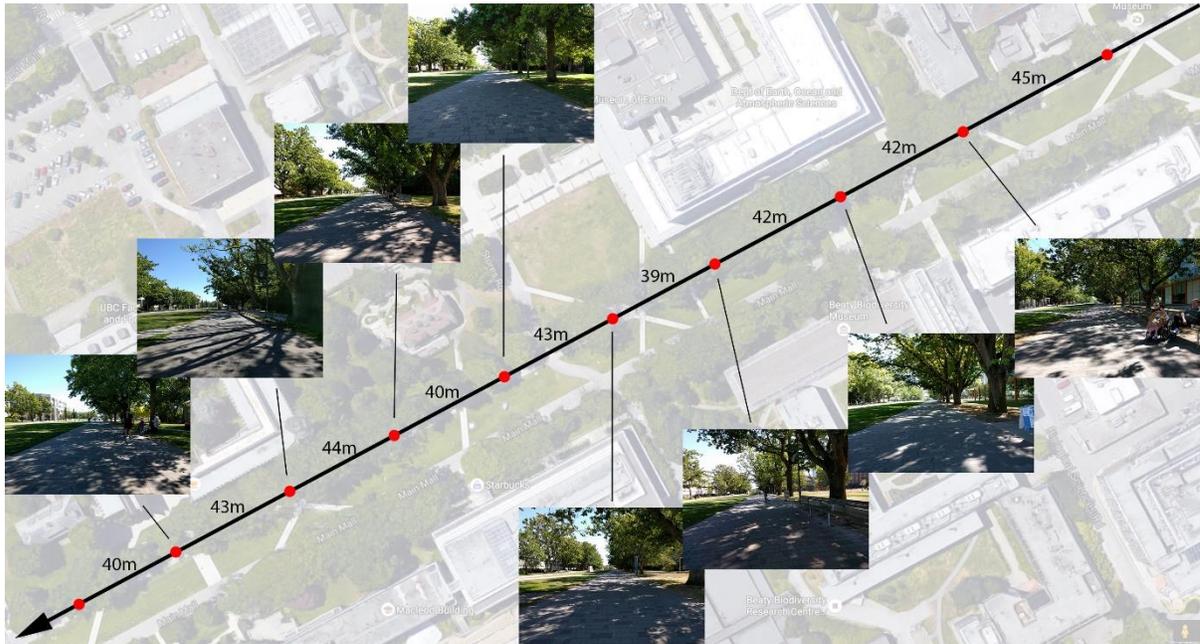
To determine minimum sample size, we conducted a Monte Carlo simulation for a mixed-effects model using the simR software package (Green & MacLeod, 2016). Results from Study 1 showed the smallest effect size for the relationship between cognitive load and gait to be $< .30$; however, walking outdoors introduces a lot of noise due to the uncontrolled environment (e.g. other pedestrians, variability in visual stimuli, weather conditions). Therefore, to avoid being underpowered, we used a conservative effect size estimate of 0.05 in case outdoor effect sizes are considerably smaller than those found in controlled settings. The simulation indicated that 228 individual data points would be needed to

achieve 80% power, which, for our study design, amounts to a minimum of 29 subjects. Participants were recruited from the University of British Columbia and received monetary compensation for their time. Participants were excluded if they were over 60 years of age as cognitive function declines with age and will likely confound any observed relationships (H. Lee et al., 2012). Furthermore, some participants had an incorrectly positioned smartphone, which would negatively impact the calculation of fractal dimension (described in more detail below). Our final sample consisted of 38 participants (mean age = 26.04, $SD = 6.83$, 13 male).

4.3.2 Methods

After providing signed consent, participants wore a chest harness with a smartphone phone secured directly in front of their sternum, and their height was measured. A research assistant used a second smartphone, connected via Bluetooth, to instruct the *participant's* phone to take photographs at various points along the walking route. This provided photos of the walking environment from the perspective of the participant at the time of the actual walk. A 520m walking route was used for the study. The route was selected due to having variance in the visual scene, few physical obstructions, and consistency in the walking surface (concrete, with no major bumps or changes in inclination). Additionally, during the location scouting stage of the project, fractal dimension was calculated along the entire route to ensure there was variability in the measure. Participants were only tested on days with no rain to ensure that gait dynamics were not affected by a change in the conditions of the walking surface. Ten approximately equidistant checkpoints were selected along the route. The first checkpoint was excluded from analysis as the participant was accelerating up to their normal walking speed. Similarly, the final checkpoint was excluded due to deceleration to standing at the end of the route. Ultimately, only the middle eight checkpoints were analyzed (Figure 4.5).

Figure 4.5 Checkpoint photos from Study 3. Map data: Google



Participants started at the first checkpoint and were asked to walk at a self-selected speed to the end of the route at the final checkpoint. They were instructed to look directly ahead while walking, not to interact with other pedestrians, and to maintain as straight a path as possible while avoiding oncoming traffic (e.g. bicycles, pedestrians). Participants were told that the research assistant would follow them at a distance of 5m. The research assistant used their smartphone to take a photograph from the participant's phone at each of the checkpoints.

After data collection, the Minkowski–Bouligand fractal dimension was calculated using the box-counting technique for each photograph (Schroeder, 1991). Gait dynamics were calculated from the 10-seconds following each timestamped photo. For correct calculation of fractal dimension, the participant's phone must be positioned correctly on the harness. Participants were excluded if the environment's horizon line was not within the middle third of the image. Additionally, individual checkpoints were excluded if the participant deviated from the path (e.g. walked on grass), or if the photo was accidentally

taken too early. These exclusions were already considered in our sample description in the previous section. Finally, we coded the number of other pedestrians on the walking path as an additional source of cognitive load, which was used as a covariate in the following statistical models.

4.3.3 Results

We hypothesized that fractal dimension (FD) of an image would be associated with the dynamics of gait. Specifically, as fractal dimension increases (and thus cognitive load decreases), step time and step time variability would decrease, while step and stride regularity would increase. These predictions stem from our findings in Study 1.

We employed linear mixed-effects models to formally test for the presence of each predicted relationship. All variables were first standardized at the raw score level using the grand mean. In predicting each of the gait parameters, we began by including participant height and number of other pedestrians (counted from the photos) as covariates, as they would likely impact gait dynamics. Next, we disaggregated fractal dimension into two components to disentangle between and within-subject fluctuations in FD (Enders & Tofighi, 2007; L. P. Wang & Maxwell, 2015), both of which were used as separate predictors in the model. This was accomplished by calculating mean FD for each participant from their set of eight photos, which captures between-subject fluctuations in FD that may arise from changes in environmental conditions such as weather and differences in lighting/shadows. Next, for our primary predictor of interest, we calculated each photo's deviation from that individual's previously calculated mean FD, which captures the within-subject changes in fractal dimension for that individual. In sum, each gait parameter was predicted by height, number of pedestrians, mean FD, and deviations in FD. Random effect components were added for both the intercept and slope of the FD deviation values, clustered by subject. For the following models, parameters were estimated using restricted maximum likelihood;

Satterthwaite approximations were used for the degrees of freedom, and confidence intervals were estimated using percentile bootstrap.

4.3.3.1 Step Time

For the random effects, the standard deviation around the mean intercept was found to be significant, $SD = 0.87$, 95% CI [0.66, 1.09], suggesting a significant degree of variation in step time between subjects. Additionally, the standard deviation around the FD deviation slope values was significant, $SD = 0.004$, 95% CI [0.001, 0.06], suggesting that the relationship between FD and step time is different for each subject. For the fixed effects, height was significantly positively associated with step time, $\beta = 0.64$, 95% CI [0.35, 0.94], $t(34.88) = 4.31$, $p < .001$. However, none of the other predictors showed a significant relationship with step time (pedestrian count: $\beta = -0.003$, 95% CI [-0.03, 0.03], $t(261.40) = -0.18$, $p = .86$. FD mean: $\beta = 0.35$, 95% CI [-0.36, 1.13], $t(34.94) = 0.92$, $p = .37$. FD deviation: $\beta = -0.004$, 95% CI [-0.03, 0.03], $t(246.80) = -0.31$, $p = .76$).

4.3.3.2 Step Time Variability

For the random effects, the standard deviation around the mean intercept was found to be significant, $SD = 0.97$, 95% CI [0.73, 1.20], suggesting a significant degree of variation in step time variability between subjects. Additionally, the standard deviation around the FD deviation slope values was significant, $SD = 0.02$, 95% CI [0.002, 0.16], suggesting that the relationship between FD and step time variability is different for each subject. For the fixed effects, none of the predictors were significantly associated with step time variability (height: $\beta = 0.10$, 95% CI [-0.23, 0.43], $t(34.90) = 0.60$, $p = .55$. pedestrian count: $\beta = 0.03$, 95% CI [-0.04, 0.10], $t(266.30) = 0.78$, $p = .44$. FD mean: $\beta = -0.32$, 95% CI [-1.20, 0.45], $t(35.13) = -0.73$, $p = .47$. FD deviation: $\beta = -0.004$, 95% CI [-0.07, 0.07], $t(224.43) = -0.10$, $p = .92$).

4.3.3.3 Step Regularity

For the random effects, the standard deviation around the mean intercept was found to be significant, $SD = 1.19$, 95% CI [0.91, 1.48], suggesting a significant degree of variation in step regularity between subjects. Additionally, the standard deviation around the FD deviation slope values was significant, $SD = 0.04$, 95% CI [0.006, 0.12], suggesting that the relationship between FD and step regularity is different for each subject. For the fixed effects, none of the predictors were significantly associated with step regularity (height: $\beta = -0.02$, 95% CI [-0.38, 0.40], $t(34.75) = -0.10$, $p = .92$. pedestrian count: $\beta = -0.01$, 95% CI [-0.07, 0.04], $t(262.57) = -0.51$, $p = .61$. FD mean: $\beta = -0.25$, 95% CI [-1.37, 0.79], $t(35.12) = -0.49$, $p = .63$. FD deviation: $\beta = 0.02$, 95% CI [-0.03, 0.07], $t(136) = 0.83$, $p = .41$).

4.3.3.4 Stride Regularity

For the random effects, the standard deviation around the mean intercept was found to be significant, $SD = 0.66$, 95% CI [0.47, 0.85], suggesting a significant degree of variation in stride regularity between subjects. Additionally, the standard deviation around the FD deviation slope values was significant, $SD = 0.08$, 95% CI [0.01, 0.23], suggesting that the relationship between FD and stride regularity is different for each subject. For the fixed effects, none of the predictors were significantly associated with stride regularity (height: $\beta = -0.01$, 95% CI [-0.22, 0.22], $t(33.44) = -0.06$, $p = .95$. pedestrian count: $\beta = -0.04$, 95% CI [-0.13, 0.06], $t(281.38) = -0.75$, $p = .46$. FD mean: $\beta = -0.30$, 95% CI [-0.94, 0.24], $t(34.40) = -1.03$, $p = .31$. FD deviation: $\beta = 0.07$, 95% CI [-0.02, 0.17], $t(96.93) = 1.37$, $p = .18$).

4.3.4 Discussion

Our results failed to find support for our hypothesis that fractal dimension of outdoor photos is related to gait dynamics. The significant random effects (intercept and slope) across the models are not surprising as we would expect gait to vary between individuals.

Additionally, the only significant fixed effect predictor was for height in the relationship between FD and step time. Specifically, we found that as participant height increased, their time between steps also increased. This is expected as taller individuals are likely to have a longer stride length, thus longer time between each step.

One possible reason for the null results is a lack of sufficient variance in fractal dimension across the checkpoints. Figure 4.5 shows that the visual scene along the route is quite similar, with all images containing high amounts of foliage, constant shadowing on the ground, and little to distinguish one checkpoint from another. In Study 1 we showed that explicit manipulation of cognitive load can lead to changes in gait. However, when load is inherent to the environment the effect may not be as strong, and thus larger variability in load may be required to see an effect in a correlational real-world design.

4.4 Study 4

To test whether our null results from Study 3 may have been due to low variability in our predictors, a new walking route was chosen that contained more variability in fractal dimension across checkpoints.

4.4.1 Participants

Participants were again recruited from the University of British Columbia and received monetary compensation for their time. Participants were excluded for the following reasons: hardware issues (data saving problems due to Bluetooth disconnections), one participant was constantly adjusting the harness which led to a lot of noise in the signal, and incorrectly positioned phone (as described in the previous study). Our final sample consisted of 38 participants (mean age = 24.79, $SD = 6.04$, 15 male).

4.4.2 Methods

The procedure was identical to Study 3, with the only difference being a new walking route. During location scouting for the new route, a research assistant took photos along each of our candidate routes. Variability in fractal dimension was then calculated and compared against the values in Study 3. The chosen route (Figure 4.6) was approximately 480m in length and was selected for increased variability in the visual scene at each checkpoint (e.g., differences in amount of foliage, ground shadows, visible sky, larger variability in fractal dimension induced by higher amounts of built environment), while still maintaining consistency in the walking surface (concrete, with no major bumps or changes in inclination).

Figure 4.6 Checkpoint photos from Study 4. Map data: Google

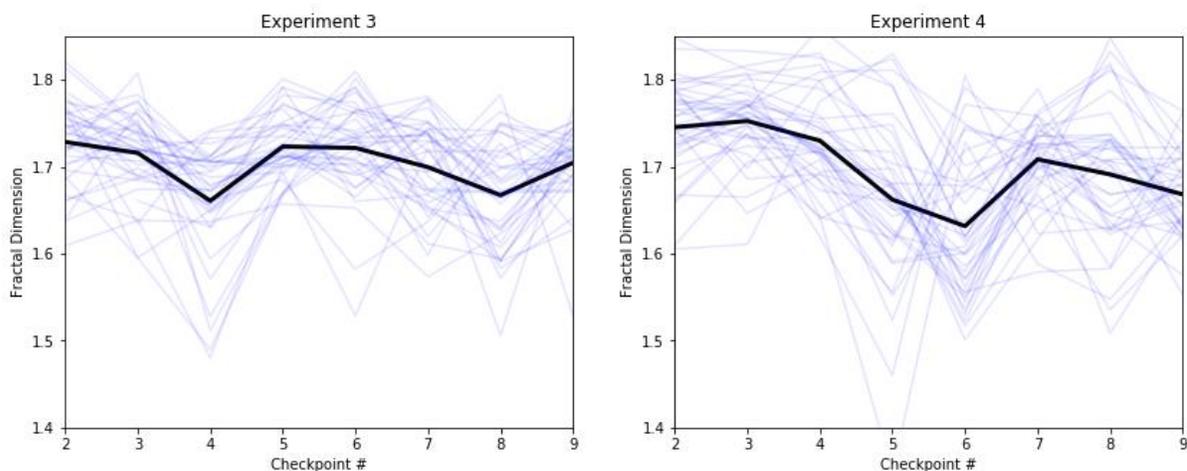


4.4.3 Results

4.4.3.1 Fractal Dimension Variability

To show that the new route has more variability in fractal dimension compared to Study 3, we graphed the mean FD value for each checkpoint, along with the trace for each individual subject (Figure 4.7). The graphs show that the new route has a wider range of FD values, as well as a larger amount of variance, across the checkpoints. This was confirmed using Levene's test for homogeneity of variance between the two studies. The test shows a significant difference in variance of FD between Studies 3 and 4, $F(1, 599) = 23.29, p < .001$. The difference in mean FD between the two studies was not found to be significant, $t(554.3) = 0.68, p = 0.50$, suggesting that average load between the two studies was similar.

Figure 4.7 Fractal dimension values for all participants in both outdoor studies



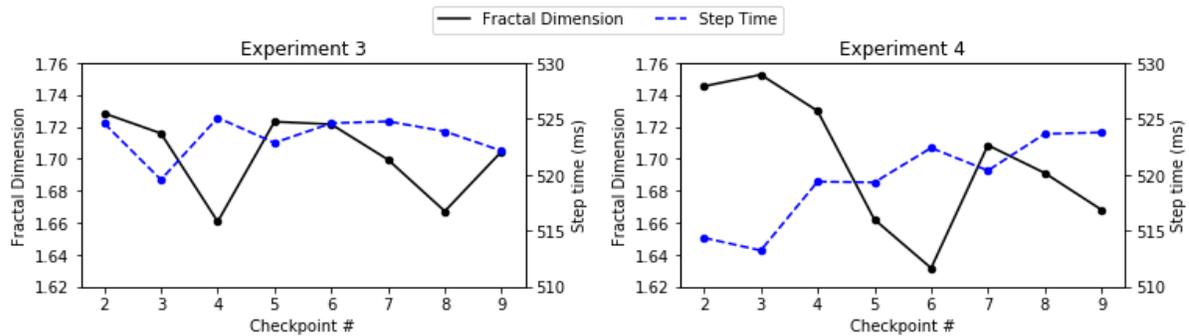
4.4.3.2 Step Time

For the random effects, the standard deviation around the mean intercept was found to be significant, $SD = 0.78, 95\% CI [0.60, 0.98]$, suggesting a significant degree of variation in step time between subjects. The standard deviation around the FD deviation slope values was also significant, $SD = 0.02, 95\% CI [0.002, 0.06]$, suggesting that the relationship

between FD and step time is different for each subject. For the fixed effects, height was significantly positively associated with step time, $\beta = 0.49$, 95% CI [0.24, 0.74], $t(35.13) = 3.91$, $p < .001$. Importantly, FD deviation was significantly negatively associated with step time, $\beta = -0.04$, 95% CI [-0.06, -0.02], $t(25.53) = -3.77$, $p < .001$, suggesting that time between steps becomes shorter as fractal dimension increases (i.e. cognitive load decreases). However, none of the other predictors showed a significant relationship with step time (pedestrian count: $\beta = -0.01$, 95% CI [-0.04, 0.01], $t(259.84) = -1.14$, $p = .26$. FD mean: $\beta = 0.01$, 95% CI [-0.62, 0.56], $t(34.48) = 0.05$, $p = .96$).

The significant effect of step time differs from the finding in Study 3, where no such relationship was observed. The differences between Studies 3 and 4 can be further highlighted by plotting mean FD and step time across all checkpoints for each study (Figure 4.8). The plots show two notable data patterns. First, the FD and step time values in Study 3 span a narrower range compared to Study 4, suggesting, again, less variability in parameter values across the walk. Second, Study 4 shows a clear inverse relationship between FD and step time: low step time values are seen when FD is high (e.g. checkpoints 2 to 3), and when FD begins to decline we see a rise in step time (e.g. checkpoints 3 to 6). This relationship is not as pronounced in Study 3, likely due to reduced variability in parameter values. In addition to looking at the effect at individual checkpoints, we can also assess the overall linear trend for step time and fractal dimension in both studies. In Study 4, step time shows a significant positive trend over time ($\beta = 0.03$, $p < .001$), while fractal dimension shows a negative trend ($\beta = -0.04$, $p < .001$), showing that even at the aggregate level, the two measures are inversely related. However, neither of these trends are significant in Study 3 (step time: $\beta = -0.004$, $p = .61$; fractal dimension: $\beta = -0.004$, $p = .77$).

Figure 4.8 Fractal dimension and step time in both outdoor studies



4.4.3.3 Step Time Variability

For the random effects, the standard deviation around the mean intercept was found to be significant, $SD = 0.83$, 95% CI [0.65, 1.04], suggesting a significant degree of variation in step time variability between subjects. Additionally, the standard deviation around the FD deviation slope values was significant, $SD = 0.10$, 95% CI [0.03, 0.16], suggesting that the relationship between FD and step time variability is different for each subject. For the fixed effects, none of the predictors were significantly associated with step time variability (height: $\beta = 0.10$, 95% CI [-0.17, 0.32], $t(36.73) = 0.79$, $p = .44$. pedestrian count: $\beta = 0.01$, 95% CI [-0.04, 0.05], $t(260.77) = 0.26$, $p = .80$. FD mean: $\beta = 0.28$, 95% CI [-0.25, 0.80], $t(34.12) = 0.98$, $p = .33$. FD deviation: $\beta = -0.02$, 95% CI [-0.07, 0.03], $t(38.57) = -0.70$, $p = .49$).

4.4.3.4 Step Regularity

For the random effects, the standard deviation around the mean intercept was found to be significant, $SD = 0.75$, 95% CI [0.57, 0.94], suggesting a significant degree of variation in step regularity between subjects. Additionally, the standard deviation around the FD deviation slope values was significant, $SD = 0.01$, 95% CI [0.002, 0.08], suggesting that the relationship between FD and step regularity is different for each subject. For the fixed effects, none of the predictors were significantly associated with step regularity (height: $\beta = -0.04$, 95% CI [-0.27, 0.22], $t(35.09) = -0.32$, $p = .75$. pedestrian count: $\beta = -0.01$, 95% CI [-

0.05, 0.03], $t(266.39) = -0.50$, $p = .62$. FD mean: $\beta = 0.13$, 95% CI [-0.41, 0.68], $t(34.83) = 0.47$, $p = .64$. FD deviation: $\beta = 0.02$, 95% CI [-0.02, 0.06], $t(215.37) = 0.98$, $p = .33$.

4.4.3.5 Stride Regularity

For the random effects, the standard deviation around the mean intercept was found to be significant, $SD = 0.47$, 95% CI [0.33, 0.62], suggesting a significant degree of variation in stride regularity between subjects. Additionally, the standard deviation around the FD deviation slope values was significant, $SD = 0.01$, 95% CI [0.002, 0.20], suggesting that the relationship between FD and stride regularity is different for each subject. For the fixed effects, none of the predictors were significantly associated with stride regularity (height: $\beta = 0.08$, 95% CI [-0.08, 0.27], $t(34.98) = 0.93$, $p = .36$. pedestrian count: $\beta = -0.04$, 95% CI [-0.13, 0.07], $t(295.63) = -0.70$, $p = .49$. FD mean: $\beta = 0.09$, 95% CI [-0.30, 0.48], $t(35.44) = 0.43$, $p = .67$. FD deviation: $\beta = 0.02$, 95% CI [-0.07, 0.11], $t(260.86) = 0.52$, $p = .60$).

4.4.4 Discussion

We hypothesized that the null results in Study 3 were due to low variability in the fractal dimension measure across participants and checkpoints. After increasing FD variability in the present study, our results showed that step time became shorter when FD increased (i.e. cognitive load decreased). This supports the step time finding in Study 1; however, the other gait parameters were still not related to fractal dimension. The overall data pattern will be discussed in more detail in the general discussion section.

4.5 General Discussion

Research has demonstrated that an increase in cognitive load can result in increased gait variability and slower overall walking speed (Hausdorff et al., 2008). The external environment also imposes load on our cognitive systems; however, most gait research has been conducted in a laboratory setting and little work has demonstrated how

load imposed by real-world environments impacts gait dynamics during outdoor walking. Overall, our results suggest that, during indoor walking, task-induced increased cognitive load impacted a range of gait parameters such as step time and step time variability. The impact of environmental cognitive load on gait, however, was not as pronounced, with increased load associated only with step time increase during outdoor walking. This suggests that the intensity of experienced load moderates the degree to which gait dynamics are affected. Overall, the present work shows that cognitive load is related to young adult gait during both indoor and outdoor walking, and importantly, that smartphones can be used as gait assessment tools in environments where gait dynamics have traditionally been difficult to measure. Given our findings, several theoretical questions and issues follow.

Cognitive load induced by everyday environments is, potentially, of lower intensity than load imposed by an explicit dual task, and it is possible that changes in cognitive demand induced by everyday man-made outdoor environments are too low to bring about changes in all measured gait parameters. Our results show that, compared to task-related cognitive load changes in indoor walking, the impact of environmental cognitive load on outdoor gait is less pronounced, with only step time being affected by increased environmental load. There could be a couple of reasons for these smaller effect sizes. First, the low load levels in outdoor environments, coupled with generally better cognitive function in young adults relative to older adults (Charles H. Hillman et al., 2004; H. Lee et al., 2012), could result in young adults only needing to modulate step time to compensate for changes in environmental load. Future studies can test this hypothesis by exposing individuals to a dual task while walking outdoors and seeing whether fractal dimension impacts gait after baseline levels of experienced load have increased or testing individuals in more cognitively demanding environments such as urban city centers or street intersections. Second, the

walking routes were located on the same university campus where participant recruitment took place, and it is possible that familiarity with the environment was causing a reduced cognitive demand while walking. Given the central location of the route on the campus, students would likely be familiar with the environment and may not need to devote as many cognitive resources to route planning as a naïve walker. Additionally, students familiar with the route might pay less attention to the visual environment, reducing the potential impact of fractal dimension and ultimately resulting in smaller effects. We predict that a replication of the study using a novel environment would result in larger effect sizes, and possibly increased involvement of the other gait parameters.

While fractal dimension has been linked to cognitive load and perceptual fluency (Joye et al., 2016), and the relationship is supported by our findings in Study 2, it remains unclear whether a photo from chest level is a good representation of where participants were actually looking while walking through the environment. Although we instructed participants to look directly ahead during their walk, we can't guarantee that this instruction was followed for the entire route, and any head turns or changes in gaze location may result in reduced validity of our fractal dimension measure. Some work has shown that gaze is typically directed towards the ground while walking outdoors over rough terrain, but is less focused on the ground plane when walking over flat terrain (Matthis et al., 2018). As both Studies 3 and 4 contained flat ground walking, it may be prudent for future studies to utilize head-mounted cameras or eye-trackers for more precise localization of gaze during the walk.

Traditional methods of gait assessment can be cost-prohibitive, and the present study demonstrates the use of smartphone-based techniques for this task, confirming that smartphones can be used as relatively cheap, accessible alternatives for examining the relationship between gait and cognition in outdoor environments. While some have studied

outdoor gait using state-of-the-art mobile motion capture systems (Matthis et al., 2018), smartphones offer a more robust methodology that is both relatively inexpensive and more readily available. Modern smartphones have built-in accelerometers and accelerometry is commonly used in the study of gait and postural balance, which has been shown to have accuracy similar to force platform-based techniques (Mayagoitia et al., 2002). Analysis of gait patterns from accelerometers can provide the ability to differentiate between habitual fallers and healthy adults (Kamen et al., 1998), and can be used as a diagnostic tool for rheumatoid arthritis (Yamada et al., 2012). Although it should be noted that data accuracy and consistency depends on placement of the accelerometer (Carcreff et al., 2018; Mansour et al., 2015; Rispens et al., 2014). Smartphones have only been used to study gait in a small handful of studies (Yamada et al., 2012); however, smartphone-based methods could allow researchers to ask new research questions, and have some additional benefits over traditional methods. For example, while they can be used to examine gait in natural, outdoor environments, they can also be employed for monitoring gait in home environments owing to their portability. Additionally, smartphones can be used as portable testing computers, allowing for remote administration of questionnaires or cognitive tasks, which can then be correlated with gait measures during the same time-period. This approach could be extended using Ecological Momentary Assessment, which would allow for direct examination of within-person variability in gait dynamics and cognitive function (Genevieve F. Dunton et al., 2011; Genevieve Fridlund Dunton, 2017). Finally, smartphone applications have the potential for wide deployment on application stores (e.g., Apple App Store, Google Play Store), allowing for 1) faster acquisition of data, and 2) access to a larger, more diverse population; thus, allowing for cross-cultural comparison studies. There are some drawbacks compared to traditional methods (e.g. 3D motion capture), however, as distance-based metrics such as stride length are difficult to obtain from acceleration data. While some techniques exist to estimate distance metrics (Caldas et al., 2017; Samà et al., 2011), the

direct transformation of acceleration requires double numerical integration of the signal, which may be problematic given the noisy nature of acceleration data. As such, the validity and accuracy of distance-based gait measures derived from acceleration data through integration techniques remains an empirical question.

In conclusion, our results suggest that in young adults both dual task induced as well as sensory environment induced cognitive load are related to changes in gait dynamics, although the effect was attenuated in the latter, possibly owing to reduced levels of cognitive load changes when walking outdoors. Furthermore, our findings show that smartphone-based accelerometry is sensitive enough to detect these relationships, thus allowing for future gait studies to be conducted in more natural environments where gait dynamics have traditionally been difficult to measure.

5 Conclusion

The three primary questions addressed in this dissertation were:

1. How does physical activity affect cognitive function (specifically executive function), and are there any moderators of that relationship?
2. How does cognitive load affect low-intensity physical activities like walking?
3. Are these effects observable when measured in more natural environments? For example, real world cognitive performance and naturally occurring physical activity outside of the laboratory

Chapters 2 and 3 examined the effects of PA intensity and PA time window. Chapter 2 showed a non-significant relationship between PA and cognition when measured at the weekly time window, and this was the same for all PA intensity levels. However, when we make the PA time window more acute in Chapter 3 the effect of increased PA started to emerge, and this result was seen using both laboratory and real-world measures. This suggests that the effect of PA might be easier to see when it is measured closer to the time of cognitive testing, and could be an important factor to consider when studying young adult PA. Additionally, while the wider literature has shown an effect of PA intensity (Kamijo et al., 2004, 2007), Chapter 2 did not show the same result, and there could be a few reasons for this. One possibility is that the effect sizes seen in Chapters 2 and 3 are generally very small, which might make it even more difficult to detect if looking at a time window that is too distal from cognitive testing. Another possibility is that the operationalization of PA intensity might be different between my studies and other studies in the literature. PA intensity is typically manipulated in acute PA studies by asking participants to perform activities at some percentage of their maximum heart rate or VO₂ max. The International Physical Activity Questionnaire, on the other hand, provides participants with examples of activities that are

performed at different intensities, and the task is to categorize their own activity by picking the closest matching example category. So, differences in findings could be due to some combination of assessing different PA time windows, self-report versus objective measurement, increased noise from subjective questionnaires, or differences in the latent construct that is being measured.

Chapter 4 showed that the relationship between cognition and motion is bidirectional. Specifically, increased cognitive load can affect low-intensity physical activities like walking, making gait slower during both indoor and outdoor walking. This effect is detectable with both explicit (task-based) and implicit (environment-based) changes in cognitive load and is detectable using a new smartphone-based methodology. Not only does Chapter 4 replicate some of the cognitive load effects found in the literature, it also shows how these effects change when examined in a real-world outdoor environment. This is especially important as we have thus far lacked adequate tools for measuring outdoor gait, and this methodology makes outdoor gait research more accessible by employing a relatively cheap and widely available technology.

Throughout each of the chapters I examined PA as it occurs in the real world, outside of the laboratory, in the form of either free-living PA or outdoor walking. Additionally, Chapter 3 assessed performance on a real-world outcome, namely a university exam. The examination of real-world contexts and outcomes is particularly important for the goal of PA promotion as it keeps the findings applicable and relatable. The increased tangibility of the results, I hope, will make increased PA easier to adopt.

In sum, the relationship between cognition and motion is found to be bidirectional and detectable in young adults, but these effects are generally quite small and can only be observed under certain circumstances. Specifically, in terms of PA, the effect is easier to see when measured closer to the time of cognitive testing and, in terms of outdoor walking,

there must a sufficiently high level of variability in cognitive load in the walking environment. The specificity of these findings might explain why the young adult PA literature often shows mixed behavioural results; in the following sections I will discuss a few factors that might be contributing to a mixed literature.

5.1 Mixed literature on physical activity benefits

Many reviews and meta-analyses have concluded that increased aerobic PA and aerobic fitness are associated with improved cognitive function. This result has been shown in children (Álvarez-Bueno et al., 2017; Booth et al., 2014; Chu et al., 2019; Drollette et al., 2018; Drollette & Hillman, 2020; Raine et al., 2018; Sibley & Etnier, 2003; Syväoja et al., 2018; Vazou et al., 2016; Westfall et al., 2018), young adults (Chang, Labban, et al., 2012; Christmas et al., 2019; Gejl et al., 2018; Johnson et al., 2019; Labban & Etnier, 2018; P. Loprinzi et al., 2019; S. Ludyga et al., 2019; Luque-Casado et al., 2016; Park & Etnier, 2019; C.-C. Wang et al., 2019), as well as older adults (Chang, Ku, et al., 2012; Colcombe & Kramer, 2003; Falck et al., 2018; J. Fanning et al., 2017; Charles H. Hillman et al., 2018; Northey et al., 2017; Stillman et al., 2019). The effects in children are likely due to later maturation of frontal brain regions, allowing PA the opportunity to help improve executive function (Anderson, 2002; Etnier & Chang, 2009). A similar line of reasoning applies to older adults, where executive functions like inhibitory control are one of the earliest to decline with advancing age (Sweeney et al., 2001).

Many others, however, have shown little to no impact of PA on cognition (Blough & Loprinzi, 2019; Diamond & Ling, 2016, 2018; Ehmann et al., 2017; Etnier et al., 2006; Farris & Loprinzi, 2019; Felez-Nobrega et al., 2018; Lambourne et al., 2010; C.-C. Wang et al., 2015; Young et al., 2015). For example, only 48% of children intervention studies found a significant benefit of PA on cognition, and only 60% found a benefit on academic achievement (Singh et al., 2018). The benefits in older adults have also been shown to be

controversial (Coen et al., 2011; Diamond & Ling, 2016, 2018; Charles H. Hillman et al., 2018; Young et al., 2015), with less than 60% of intervention studies showing a benefit (Gomes-Osman et al., 2018).

While children and older adults are important populations to study, I should reiterate that I am focusing on young adults for PA promotion reasons, as that is the age at which PA begins to decline (Ku et al., 2006; Spittaels et al., 2012). However, there are reasons to believe that young adult populations may produce the smallest effect sizes out of any age group. Recent meta-analyses show that the PA benefit in young adults is typically smaller than for children and older adults (Sebastian Ludyga et al., 2016; Oberste et al., 2019); this is also true when looking at academic achievement, where children tend to show the largest effect (Álvarez-Bueno et al., 2020). Another study showed that aerobic fitness is associated with better executive function in older adults, but not in young adults (Hayes et al., 2016). The turning point for these effects appears to be around 35 years of age, as it is only after this point that PA benefits begin to emerge (Berchicci et al., 2013). Executive function performance tends to follow a U-shaped pattern across the lifespan, with young adults showing the best, and most stable, performance (Zelazo et al., 2004). So, it is possible that the high consistency of young adult performance (B. R. Williams et al., 2005, 2007; Ziegler et al., 2012) places them at ceiling levels, leaving little room for improvement by PA and resulting in small effect sizes.

The inconsistent nature of young adult findings is shown in this dissertation, where half of the PA studies showed non-significant effects (Chapter 2), and the other half showing a PA benefit (Chapter 3). This 50-50 split is not unique to this dissertation, however, as we can see a similar situation in the wider behavioural literature. Let's take acute PA and executive function as an example. Studying executive function broadly using a battery of different tasks, some researchers have found a performance increase as a result of acute

PA (Basso et al., 2015), whereas others have found non-significant (P. D. Loprinzi & Kane, 2015) or barely significant (Edwards & Loprinzi, 2018) effects. We can see the same split when looking at specific functions, where working memory, set-shifting and inhibitory control have been shown to either benefit from acute PA (Budde et al., 2008; Hogan et al., 2013; Jaffery et al., 2018; Lo Bue-Estes et al., 2008; Tomporowski et al., 2005), or show no relationship with PA (Audiffren et al., 2009; Coles & Tomporowski, 2008; Griffin et al., 2011; Lambourne et al., 2010; Tomporowski & Ganio, 2006; Zimmer et al., 2016). In terms of inhibitory control, specifically the Flanker task which I used in Chapters 2 and 3, we can once again see a divide in the literature with both significant (Chang, Pesce, et al., 2015; Kamijo et al., 2007; Kubesch et al., 2009; Sebastian Ludyga et al., 2018) and non-significant results (Charles H. Hillman et al., 2003; Stroth, Kubesch, et al., 2009; J.R. Themanson & Hillman, 2006).

As mentioned in the introduction, many theoretical models have been proposed to explain inconsistencies in acute and long-term PA results. However, the fact that so much of the literature shows non-significant findings, and that the plethora of models can't fully reconcile the disparate results, might suggest a more systemic problem. Furthermore, the divided nature of these findings is particularly problematic and alarming given that a large majority of researchers in the field (as well as the general public) believe PA to have an overall beneficial effect (Nazlieva et al., 2019), despite the evidence being quite inconsistent. Overall, this is not to say that PA does not influence behavioural measures of cognition. Rather, the inconsistent findings are likely due to a combination of various methodological and statistical issues in the literature. These issues are discussed in the following sections and need to be addressed before we can draw any firm conclusions about the impact of PA on young adult cognition.

5.2 Neuroimaging versus behaviour

Although I'm primarily interested in behavioural effects as they're directly observable and more relatable for the purposes for PA promotion, it's important to note that many of the reviewed PA studies have also used neuroimaging techniques (e.g., EEG, fMRI) to study changes in cognition. In many cases, the behavioural results are in line with the neuroimaging findings, for example, they may both show a significant benefit of PA or both show a non-significant effect (Chang et al., 2017; Chang, Pesce, et al., 2015; Herting & Nagel, 2012; O'Leary et al., 2011; Pontifex & Hillman, 2007; J.R. Themanson et al., 2008; Tsai et al., 2014; C.-H. Wang et al., 2015, 2016; Yanagisawa et al., 2010). At the same time, however, there are many studies where the behavioural and neuroimaging results don't fully agree and it is often the behavioural finding that shows a non-significant relationship (Herold et al., 2020). This has been shown across multiple cognitive functions like memory (Herting & Nagel, 2013; Wagner et al., 2017), task switching (Kamijo & Takeda, 2010; Jason R. Themanson et al., 2006), and inhibitory control (Charles H. Hillman et al., 2003; Kamijo et al., 2004, 2009; Magnié et al., 2000; Stroth, Kubesch, et al., 2009).

These cases, where a non-significant behavioural effect is seen, but a significant neuroimaging effect is present, suggests an underlying difference between these two techniques. In older adults, changes in brain function can be detected years before any behavioural symptoms of cognitive impairment (Beason-Held et al., 2013), so it could be that neuroimaging is simply more sensitive than behavioural measures, hence the difference in significant and non-significant findings between the two methods. So, it may not be that behaviour isn't changing as a result of PA, but rather that it is more difficult to observe. Another possibility is that neuroimaging and behaviour are measuring different latent constructs; physical activity affecting brain activity is not the same thing as affecting observable behavior. This raises questions about the value of neuroimaging in the context of

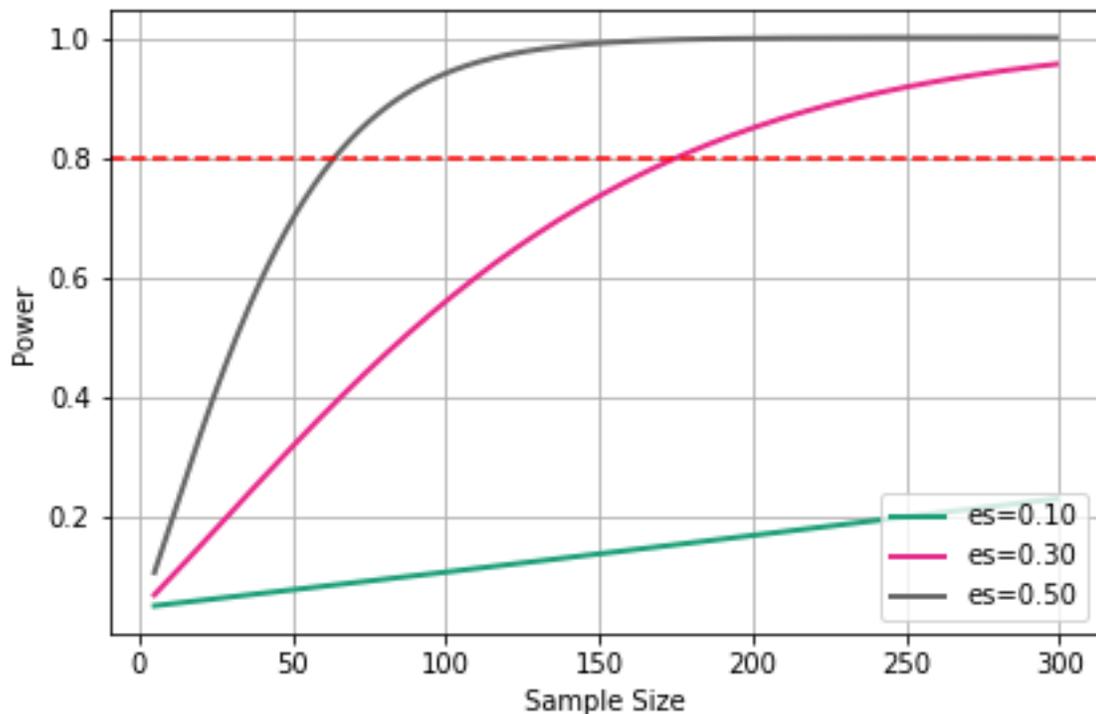
PA promotion. Are significant neuroimaging measures relevant if they are difficult for the general public to relate to? And if such a sensitive measure is required to see the impact of increased PA, would this translate to any meaningful performance changes on real world tasks? There is certainly value in neuroimaging studies for understanding the theoretical basis of PA-based improvements, but I think in terms of promoting PA to the general public behavioural outcomes are a much easier result to sell. It is just unfortunate that much of the behaviour literature shows inconsistent findings, as previously discussed.

5.3 Methodological and statistical issues

While some of the mixed results in the literature could be due to lack of consideration for moderators like PA intensity or PA time window, these issues are relatively easy to address by simply designing experiments with those moderators in mind. However, there are other methodological and statistical issues that make it difficult (sometimes impossible) to discern the true impact of PA on cognition. One of the most pressing issues is low statistical power. Due to the recent concerns with the reproducibility of results in psychological science (Open Science Collaboration, 2015), it is more important than ever to conduct well-powered studies with sufficient sample sizes. Low power is simple to address moving forward, but the problem it presents is that it calls into question the reliability of past results. Although this has been improving, few studies in the PA literature conduct power analyses and we can't reject the possibility that the non-significant results observed in past studies are simply due to being underpowered. What's more concerning is that insufficiently powered studies make it harder to detect true effects, resulting in non-significant results that are less likely to be published. Dubbed the "file-draw problem", the bias towards only publishing significant findings means that the 50-50 split we see in the PA literature may actually be conservative; there could be many more non-significant PA studies that we don't know about.

If we look at recent meta-analyses on PA effects, we see that the standardized effect size (es) can be quite varied, ranging from 0.1 to 0.5 depending on factors like PA time window, age, study design, and cognitive function examined (Álvarez-Bueno et al., 2020; Chang, Labban, et al., 2012; Sebastian Ludyga et al., 2020; Oberste et al., 2019; Verburch et al., 2014). The power analyses I conducted in Chapters 2 and 3 showed that, in the case of correlational study designs, a sample size of around 200 participants would be needed to achieve 80% power for detecting a true effect. However, we often see sample sizes smaller than 100 in the PA literature. More common are between-groups designs with manipulated PA (e.g., acute PA versus control), for which an independent groups t-test would typically be used. In Figure 5.1, I simulated power curves for an independent t-test across a range of effect sizes. As you can see, even a relatively common effect size of 0.30 would still require over 150 subjects to achieve 80% power. The sub-100 sample sizes in the literature are concerning and likely a large part of the reason why PA results appear to be mixed.

Figure 5.1 Example power curves for different effect sizes



Another issue concerns the psychometric properties of PA measures. For example, the International Physical Activity Questionnaire is validated as a measure of MET-minutes per week (Craig et al., 2003; The IPAQ Group, 2010), but some studies take those values and recategorize them by an external classification scheme (Kamijo & Takeda, 2009, 2010, 2013). This process effectively takes a validated instrument and creates a new measurement instrument where validity and reliability are only assumed, as it's unknown how this process affects the psychometric properties of the original questionnaire. Additionally, in one study aerobic fitness scores were treated as a continuous variable and then *also* recategorized into tertiles, and these two measures (which came from the same original fitness measure) led to different results regarding the impact of aerobic fitness on cognition (Hwang et al., 2017). This raises questions about how we should be handling PA data, as the lack of standardization in data handling techniques can lead to divergent findings, even if the data comes from the same source.

5.4 Public health messaging

The daily PA results shown in Chapter 3, combined with acute PA studies in the literature (Chang et al., 2017; Samani & Heath, 2018; Spitzer & Furtner, 2016), suggest a tight temporal coupling between recent bouts of PA and improved cognitive performance. There are a couple of reasons why this may form a useful basis for future messaging and promotion of increased PA in young adults.

First, temporal discounting, or the tendency to devalue future rewards of current decisions, is a potent and well-known bias negatively impacting peoples' propensity to adopt health behaviours (Chapman, 1996; Critchfield & Kollins, 2001; Lynch Jr & Zauberman, 2006; Zauberman & Urminsky, 2016). In other words, people are often more prone to seek the immediate, tangible rewards of an unhealthy decision or choice, such as eating a fatty dessert or smoking a cigarette, than they are to guide their behavior based on the delayed,

longer-term promise or anticipation of health benefits, as when foregoing the immediate enjoyment of the fatty dessert or cigarette for the chance of a longer life spent with fewer chronic diseases (Bradford, 2010; Daugherty & Brase, 2010). Importantly, this temporal discounting bias extends to PA decisions as well, which are often construed through a lens of delayed physical health benefits (Tate et al., 2015). In Chapter 3, I targeted the relationship between daily PA and cognitive function for study on the idea that the tight temporal coupling between PA and cognitive benefits might ultimately provide a focus for public health messaging that is less susceptible to temporal discounting biases.

Second, I wanted to assess the potential cognitive benefits of PA using a measure that had a direct, immediate translation into possible public health messaging. The reasoning was that research on temporal discounting has shown that the impact of an immediate decision reward is strongest when that reward is concrete or tangible rather than abstracted in some way (Lynch Jr & Zauberman, 2006; Zauberman & Urminsky, 2016). This suggests that assessing cognitive function via a standard cognitive measure (e.g., digit span, Trail Making A & B, Flanker/Stroop interference) would be less ideal for public health messaging, relative to an assessment of cognitive performance as measured in a real-world task of high personal and/or professional importance to people. If it can be shown that recent PA leads to improved performance on such a task, the measure itself—how well someone did on the task—could provide a potent basis for public health messaging.

5.5 Dual task gait

There are a few models of dual tasking while standing and walking (Lacour et al., 2008; Pashler, 1994). The cross-domain competition model suggests that posture/gait and cognitive function compete for the same set of attentional resources, but this model falls apart when we realize that sometimes a dual task can actually improve balance. A U-shaped interaction model posits that balance control can either be improved or diminished as a

function of the cognitive demands of the secondary task. Both of these models, however, are most commonly used to explain posture control rather than walking, so they may not necessarily generalize to gait behaviour (Swan et al., 2004; Vuillerme et al., 2000). Finally, the task prioritization model might provide some insight into why gait stability can actually improve under dual task conditions, as it suggests that gait is improved if it is given higher priority than the concurrent cognitive task.

5.5.1 Gait prioritization

Some people have a tendency to stop talking while walking, and others do the opposite where they stop walking when talking (Lundin-Olsson et al., 1997). This has traditionally been explained as a choice of strategy. Posture-first strategy is the prioritization of gait stability over concurrent cognitive performance when there are no specific prioritization instructions (Shumway-Cook et al., 1997; Yogev-Seligmann et al., 2012). We often see this strategy in older adults, as they tend to prioritize gait when dual tasking to improve stability (Brown et al., 2002). Prioritization instructions can have a marked impact on gait and cognition, as gait speed is increased when participants are told to prioritize gait, but reduced when told to prioritize performance on the concurrent cognitive task (Yogev-Seligmann et al., 2010).

In Chapter 4, I didn't measure cognitive performance during the indoor walking study, so it is impossible to tell whether participants were simply placing higher priority on the cognitive task at the expense of gait stability. There is evidence to suggest that young adults place higher priority on cognitive tasks (Yogev-Seligmann et al., 2010), however, there is also evidence to the contrary, where they prioritize postural control under dual task conditions (Resch et al., 2011). From the indoor walking study alone, it is unclear whether the observed effect was due to resource competition or simply prioritization of cognitive performance over gait. However, when looking at those results in conjunction with the

outdoor walking results, the task prioritization model seems an unlikely explanation. During the outdoor walking study, there was no explicit cognitive task to perform as load levels were inherent to the external environment. In other words, there was nothing for the participants to prioritize, and yet we still saw reduced gait performance. It could be argued that there was an implied cognitive task (i.e., avoiding other pedestrians), but if this were prioritized then we would see a significant relationship between gait dynamics and number of pedestrians, which was not the case.

The task prioritization model and posture-first strategies are likely incomplete, as strategy is not dichotomous; gait is affected by a multitude of other factors such as postural reserve (availability of resources that can be allocated to posture control) and ability to predict environmental hazards. It has been suggested that subjects focus on the cognitive task when postural reserve is high and the threat to posture is low, but when the environment becomes more complex, focus shifts to maintenance of gait instead (Yogev-Seligmann et al., 2012). In Chapter 4, gait effects were smaller when walking outdoors compared to indoors, and this was explained as lack of sufficient variability in cognitive load in the outdoor environment. But from the postural reserve perspective, another explanation could be that because the outdoor environment is more complex and focus was shifted to gait maintenance, thus reducing the overall effect size as gait became more stable.

5.6 Smartphones in research

5.6.1 *Gait dynamics*

One of the main goals of Chapter 4 was to develop a methodology that would allow for objective assessment of low-intensity PA, such as walking, in outdoor environments. The motivation behind this was two-fold. First, there is interest in real-world gait assessment given the prevalence of simulated and virtual reality gait experiments (Banducci et al., 2016;

Chaddock et al., 2012; Nagamatsu et al., 2011; Neider et al., 2011; Souza Silva et al., 2019). Second, concurrent cellphone (and other cognitive distractions) have become increasingly common, and research has shown a detrimental impact on gait dynamics in scenarios like simulated street cross (Banducci et al., 2016; Neider et al., 2011; Souza Silva et al., 2019). The latter point can have particularly dangerous consequences, so it is important to understand these relationships in real-world contexts. Simulated environments don't necessarily translate to the real world. It's possible that the real world has higher levels of load because the consequences of unstable gait are more extreme than in a laboratory. Or the opposite could be true, where virtual reality headsets are so novel that they increase cognitive demand just because the technology is more foreign. It is unclear which scenario is more likely because we have yet to test these effects in natural environments.

How can we enable assessment of motion in the real world? As shown in Chapter 4, accelerometers might provide the answer. Patterns of acceleration for both treadmill and overground walking are well defined (Lan & Shih, 2013; Zijlstra & Hof, 2003), and various techniques have been developed to analyse both gait (Cui et al., 2014; Moe-Nilssen & Helbostad, 2004; Samà et al., 2011) and postural data (Lacour et al., 2008). Although the accuracy of gait estimation can be somewhat dependent on where the sensors are placed, modern analysis techniques can be used to increase the reliability between different sensor locations, rendering sensor positioning less of a factor, however, this works best for regions that are located in the spinal region (Rispen et al., 2014). One study showed that acceleration patterns are similar even between waist and head mounted accelerometers, but this was only in the context of a simple sit-to-stand task, and more research needs to be conducted to validate this in the context of walking (Sun et al., 2019). Overall, accelerometers have been well validated against traditional footswitches and force platforms (Hennig & Lafortune, 1991; Kobsar, Olson, Paranjape, & Barden, 2014; Mayagoitia et al.,

2002), making them a portable alternative to traditional methods of gait assessment like 3D motion capture.

Accelerometers have a number of real-world clinical applications. For example, they have been used to detect differences in gait pattern between young and old walkers (Kobsar, Olson, Paranjape, Hadjistavropoulos, et al., 2014), differentiate between frequent fallers and healthy older adults (Kamen et al., 1998), detect falls in real-world settings (Bagalà et al., 2012), identify rheumatoid arthritis from gait parameters (Yamada et al., 2012), and detect bradykinesia (slow and impaired movement) in patients with Parkinson's disease (Teshuva et al., 2019). Although not accelerometry-based, some recent work has applied deep learning to the automatic classification of gait patterns from images and videos (Alharthi et al., 2019, 2020), and similar techniques could potentially be applied to acceleration data for real-time detection of unsafe gait behaviours.

The potential of accelerometer-based gait assessment has really increased in recent years with smartphones becoming more commonplace; they are ideal tools for measuring low-intensity physical activity and offer the ability to take research on PA and cognition in new directions. For example, Chapter 4 showed that embedded accelerometers can detect step events in complex outdoor walking environments. Smartphones also allow us to quantify additional metrics in a single device, as variables like pedestrian count, environment type, and weather can now be recorded at the same time as the gait data. Gait has been shown to have high consistency across testing sessions (Howell et al., 2016), and the availability of smartphones makes it easier to conduct multi-session testing over much longer periods of time. Traditional methods of gait assessment are cost-prohibitive, and this dissertation confirms that smartphones offer a relatively cheap and accessible solution to the problem of real-world gait assessment.

5.6.2 Physical activity

Subjective assessments of PA are heavily dependent on the memory of the respondent, and one alternative might be to adopt more objective PA measures. To that end, many have turned to accelerometers for PA measurement (Booth et al., 2014; Felez-Nobrega et al., 2018; Pindus et al., 2016). However, subjective and objective PA measures are not perfectly correlated, as they sometimes lead to differences in findings. For example, subjective PA has been found to be associated with academic achievement, whereas objective PA was not significantly related (Marques et al., 2018; Syväoja et al., 2018). This is not to say that objective measurement of PA is necessarily better or worse than subjective assessment, rather that they're suited for different purposes. Objective PA measurement can have a difficult time accurately measuring energy expenditure during some activities. A good example of this is cycling, which will tend to give small acceleration values because the rider is essentially sitting, however, the energy expenditure of that activity is quite high, so subjective assessment might work better in that scenario. Some new devices like the Fibion try to get around this problem by first classifying the activity being performed and then weighting the acceleration data depending on what activity has been detected (Yang et al., 2018).

Despite these drawbacks of objective PA assessment, smartphone-based accelerometry offers some key advantages over regular accelerometers. Smartphones are much more accessible than traditional dedicated accelerometers, simply because most people already own one. Additionally, smartphones are essentially portable computers, so we can easily combine PA measurement with cognitive testing. This can be done in a research laboratory, at home with the participant's own device, over long periods of time by incorporating methods like ecological momentary assessment (Genevieve Fridlund Dunton, 2017; Maher et al., 2018), and they allow for faster data acquisition from much larger and

more diverse populations by distributing the software using online application stores.

Combining acceleration and cognitive measurement using smartphones is a relatively novel approach, and there are some significant hurdles that need to be overcome (e.g., heavy battery usage), but I think it would allow us to take research questions that have traditionally been confined to the laboratory out into the real world.

5.7 Limitations

There are several limitations that should be noted when considering the findings of this dissertation. First, the PA studies were purely correlational, so no clear sense of causality can be determined. We do have temporal precedence as we measured PA that occurred before the cognitive testing took place, making it more likely that PA caused the cognitive performance rather than the other way around. However, it could easily be argued that low levels of cognitive function in the participants might have caused them to be more sedentary and take part in less PA. In which case, we should turn to the wider literature; Chapters 2 and 3 don't exist in a vacuum. Many of the theoretical causal and mediation models predict (with supporting evidence) that the direction of causality is from PA to cognitive function. Additionally, there are many acute PA studies where the PA has been manipulated using control groups. These two points suggest that, even though the set of studies in this dissertation don't show direct causality themselves, when placed within the wider literature it becomes clearer that PA is the cause of cognitive changes rather than the other way around.

Second, self-reported PA may be inaccurate and have low validity compared to more direct PA measures (Dowd et al., 2018; Gråstén et al., 2012; Helmerhorst et al., 2012; Prince et al., 2008). Self-report questionnaires are useful in that they are quick, cheap, and can measure PA that cannot be captured using physical devices (such as swimming). However, they tend to show lower correlations with anthropometric variables, such as BMI,

than more objective measures of activity (Belcher et al., 2015). This potentially lower measurement validity may be contributing to the small effect sizes seen in Chapters 2 and 3. One alternative approach might be to take the smartphone-based measurement technique developed in Chapter 4 and extend it to measure PA over longer periods of time, but this still can't measure certain activities and would require the user to remember to wear the device at all times. The ideal approach would be to use both self-report and objective measurement concurrently to make up for weaknesses inherent to each methodology (Dowd et al., 2018; Troiano et al., 2012).

Third, in Chapters 2 and 3 I measured PA at different time windows (over the past week, and past day), but maybe by narrowing the time window I'm simply increasing the precision of measurement, and that could be why there were significant effects at the more acute window of time. In other words, it is easier for someone to remember the amount of PA they did yesterday than for them to remember their activity from one week ago. So, the significant effects in Chapter 3 might simply be a function of better memory and less measurement noise. This could also explain why neuroimaging effects are occasionally found, even when no behavioural effect is observed (Charles H. Hillman et al., 2003; Kamijo et al., 2004, 2009; Magnié et al., 2000; Stroth, Kubesch, et al., 2009), because it can offer higher levels of precision. However, this problem is the result of imprecise self-report questionnaires at long time windows, and the only way to get around it is to develop a new questionnaire. Borrowing from ecological momentary assessment techniques, the ideal solution might be to develop a short questionnaire that asks about the amount of PA performed over the past hour but is administered every hour over the course of a week. This would provide us with weekly PA data that is more precise in its measurement and less reliant on memory.

Fourth, asking about PA over short time windows doesn't tell us anything about how much PA occurred over long periods of time (i.e., long-term PA). I attempted to address this by asking about exercise regularity in Chapter 3, but this is admittedly not enough to fully capture long-term PA behaviour. So, the significant effects observed may be the result of increased long-term PA, but this was not measured or controlled for. It's not clear, however, how to best measure long-term PA in young adults. Long-term PA is typically measured over multiple years, and young adults don't have many years to draw upon before they start recalling childhood PA, where the PA benefit has been shown to be different (Sebastian Ludyga et al., 2016; Oberste et al., 2019). Furthermore, this problem is exacerbated by the fact that young adulthood is when PA levels begin to decline (Gordon-Larsen et al., 2004; Ku et al., 2006), which poses an issue for measurement validity. Specifically, if you measure long-term PA in young adults, but you know that their PA levels have possibly been declining over the past few years, your measurement (on the aggregate) would be an overestimate of their true PA levels. If this error was systematic and consistent for all participants then this wouldn't be an issue, but it is more likely that levels of PA decline would be different for each person. In an ideal world we would accurately control for PA at all time windows and, although difficult, this is likely where the combined methodologies of self-report and objective measurement would be invaluable.

Fifth, participants for many of the studies in this dissertation were recruited from undergraduate psychology courses and the majority of that population consists of females, resulting in a sex imbalance. The role of sex as a moderating factor is not well understood. There is some evidence from rodent studies that the upregulation of BDNF due to exercise (which has been associated with neurogenesis and improved cognitive function) is strongest in an estrogen-rich environment (Berchtold et al., 2001), suggesting that females may see a larger cognitive benefit after increased PA. This female-specific benefit can be seen in some

studies (Barha et al., 2017; Coleman et al., 2018), but there is also conflicting evidence suggesting no sex differences or even a benefit favoring males (Johnson et al., 2019; Sebastian Ludyga et al., 2020). If I reanalyze the data from Study 1 of Chapter 2 (as it has the largest sample size and most balanced sex distribution), we can see that the effect of weekly PA on executive control performance is non-significant for both males (moderate-intensity PA: $\beta = -0.17$, $t(127) = -1.90$, $p = .06$) and females (moderate-intensity PA: $\beta = 0.04$, $t(111) = 0.38$, $p = .70$), suggesting that sex is not moderating the effect and that the sex imbalance in these studies may not be an issue. However, the most appropriate way to test for this moderation is by testing for an interaction by sex, but interaction tests are notoriously power hungry, and my sample sizes are too small for such an analysis. A replication with a larger sample size would be needed to determine the role of sex as a moderating factor.

Finally, gait detection using smartphone accelerometry is somewhat limited compared to traditional methods like 3D motion capture. There are key gait variables, such as step length and stride length variability, that I did not capture in Chapter 4. This is because pure accelerometry doesn't provide any spatial information like position or distance travelled without some form of transformation. One way to transform acceleration to velocity and position is to use numerical integration, but this method is extremely susceptible to noise and typically renders the resulting signal unusable. Some recent work uses support vector regression to improve position estimates from acceleration data, but these techniques are still in their infancy (Yan et al., 2017). Another approach might be to simultaneously record GPS data from the phone, but this is also imprecise and only useful if walking over very large distances. Overall, while outdoor gait assessment using accelerometry shows promise, it should be used with the understanding that some key aspects of gait dynamics can't be measured.

5.8 Future directions

This dissertation examined the effects of increased physical activity (PA), as well as how gait dynamics are impacted by cognition, and the natural extension of this work is to look at whether increased PA can lead to more stable gait, or whether increased PA can protect against the effects of increased cognitive load when walking. Gait dynamics and total daily PA have been shown to be correlated (Dawe et al., 2018) and PA interventions can also improve gait stability in older adults (Falbo et al., 2016). This relationship has also been observed in children, where increased aerobic fitness can reduce the dual task interference effect when stress crossing in virtual environments (Chaddock et al., 2012). One possible explanation is mediation, as PA has been associated with improved executive function in those age groups, and executive function is correlated with gait performance. This raises a few interesting questions. Is that mediation model correct? The prediction would be that increased PA causes improvements in executive function, which in turn causes an improvement in gait stability. Furthermore, given the tenuous link between PA and executive function in young adults, would that model hold for that age group?

In Chapter 4 I showed that more gait parameters become affected when cognitive load variability in the environment is increased. The natural next step would be to determine whether other gait parameters are impacted when load levels are increased further, or whether there is a point where the effect plateaus, indicating a fundamental difference between indoor and outdoor walking. One way to test this would simply be to run the same study in a more cognitively demanding environment such as urban areas with more pedestrians and distractions, but it would be difficult to control or manipulate levels of cognitive load with this approach. An alternative would be to use the same environment as the current studies but introduce a dual task to increase baseline levels of load. The prediction would be that higher levels of baseline load would make the brain more

susceptible to the impact of natural environmental load, resulting in other gait parameters being detrimentally affected. The Paced Auditory Serial Addition Test (PASAT) would be a good candidate for this; the task simply presents participants with a number every few seconds (aurally) and asks them to add the number they hear to the number that preceded it. The PASAT assesses executive function (Nikraves et al., 2017; Tombaugh, 2006), taxes the same resources as the fractal dimension measure (Joye et al., 2016), and has been shown to correlate with trail-making performance (Tombaugh, 2006), which was used in the indoor walking study. Additionally, it is easier to measure performance on the PASAT than on the verbal trail-making task, so it would also allow us to look at task prioritization and trade-off effects during the walk.

Some PA models explain the impact of physical activity in terms of resource capacity. For example, the strength model of self-control posits that the ability to inhibit responses and ignore distractors has a finite capacity, which is increased by participation in PA (Audiffren & André, 2015). The studies in Chapters 2 and 3 can't rule out increased capacity as the main driver of the effect, but there is one way to test this hypothesis. Instead of looking at performance aggregated across trials, we can instead look at the performance curve across the entire session to see whether high PA and low PA participants perform differently. If PA increases capacity, then both low and high PA participants should show similar accuracy and reaction times on the first few trials, but at some point, the capacity limit will be reached by low PA participants and we will see a performance drop earlier in these subjects. So instead of looking at average mean effects, we would be looking at performance as a function of time. Looking at aggregate effects, rather than trial-by-trial changes, might also explain the small PA effects seen in Chapters 2 and 3.

As far as I know, there are currently no self-report questionnaires that measure PA over a 24-hour period and that also measures PA intensity. In Chapter 2 I predicted a PA intensity effect, but that was not something that I could test in Chapter 3. I had to make a decision about how to best measure daily PA, and for a natural extension of Chapter 2 the simplest approach would be to use the same IPAQ questionnaire but reword/reframe the questions to ask about a 24-hour period instead of a 1-week period. However, this would ultimately change the psychometric properties of the questionnaire and it would be unclear whether previous validation and reliability studies on the survey would hold after the rewording. If we were to use this approach, we would gain a clean transition from weekly to 24-hour PA in terms of storytelling, but we would sacrifice the validity of results. I chose not to go with this option. Instead, I opted for a different 24-hour PA questionnaire that has been validated but does not measure intensity (or at least is not designed to create separate intensity categories). If intensity is of interest at the daily time window, then it would be useful to create and validate a new daily PA questionnaire with intensity categories included in its design.

Throughout the chapters I have been discussing real world behaviour and developing a smartphone methodology to measure activity in natural environments. The natural progression of this set of studies is to measure both free-living PA and cognitive function in the real world simultaneously. This idea is based on ecological momentary assessment, where recordings are made multiple times a day over a long time period so we can look at longitudinal correlations between PA and cognitive function. One way to do this would be to expand the smartphone methodology developed in Chapter 4. Acceleration can be recorded throughout the day using the smartphone, but at random intervals a cognitive task would be displayed on the screen. This would provide data with high temporal resolution, and we could track whether changes in PA levels are correlated with cognitive

performance at that point in time. In addition to a cognitive task, we could also display a short self-report PA questionnaire at the same time, which would then allow us to compare reliability and validity between subjective and objective PA assessment.

Finally, what are the next steps for the field as a whole? I think the most pressing issue is the mixed nature of the PA literature. Any field of study will no doubt have conflicting findings, but when the ratio of significant to non-significant behavioural effects is almost 50-50, there is cause for concern. In terms of outcome measures, I think executive function is the correct place to look as there is a lot of evidence to suggest its importance in relation to PA (Davis et al., 2009; Dietrich, 2006; Dietrich & Audiffren, 2011; Gomes-Osman et al., 2018; O'Sullivan et al., 2001). It would appear that the problem is more related to methodology, and specifically low statistical power. The first logical step, then, would be to replicate some of the key findings in the field using larger sample sizes, as this will tell us whether there is even an effect worth chasing. Related to this goal is to determine whether the effects we are currently seeing in the literature are the result of publication bias, which can be achieved by performing a P-curve analysis on existing findings (Simonsohn et al., 2014).

5.9 Summary

In sum, this dissertation showed that weekly PA is not associated with executive function, and that intensity does not affect the relationship, but a PA benefit begins to emerge when we make the PA time window more acute. I converged on the same result using both a laboratory-based measure and real-world university exam. The relationship is also bidirectional, as changes in cognitive load can impact low-intensity PA like walking, which can be observed in natural outdoor environments using a novel smartphone-based methodology. Overall, although there is tentative evidence that PA is beneficial in young

adults, the effect sizes are generally pretty small, and significant effects are marred by moderating factors like PA time window.

So, what does this mean for PA promotion? Most effect sizes in this dissertation were really small in real-world terms (on the order of millisecond differences). While I did find statistical significance (at some time windows), the small effect sizes suggest low practical benefit, and it would be prudent to investigate other moderators and mediators before claiming any real-world benefits. It is important not to overstate these findings given the small impact PA seems to have on behavioural performance. Brain training games have fallen into this trap, as the transfer effects from such games are not robust and benefits not generalizable (Boot & Kramer, 2014; Simonet et al., 2019; Simons et al., 2016).

If there is no cognitive benefit to increased PA (or only a really small one), it would be disingenuous to promote PA for cognitive reasons, especially given the opportunity costs associated with spending time on PA (e.g., there might be other ways to improve cognitive performance, such as getting better quality sleep as shown in Chapter 3). Furthermore, there are certain populations that probably shouldn't spend time on increased PA for such small benefits. Chronic pain sufferers are a good example here. Those that suffer from chronic pain often report reduced executive function (Berryman et al., 2014; Schultz et al., 2018) and this is likely due to decreased activation and reduced volume of regions within the prefrontal cortex (Apkarian, 2008; Cauda et al., 2014). So, this population, in the hopes of improving their executive function, may be more susceptible to false claims about PA benefits, and if the practical benefit is trivial or non-existent, they would be putting themselves through more pain for little-to-no gain.

Overall, when looking at the PA and cognition literature, it would be tempting to conclude that increased PA can improve cognitive function, but when you break the results down into narrower categories like young adult behavioural performance on executive

function tasks, the benefit becomes less clear. The work presented in this dissertation offers a more conservative conclusion, that increased PA is associated with benefits to cognitive function under select circumstances (namely shorter time windows). Furthermore, while the effect may be bidirectional, the effect sizes are generally quite small, calling into question the practical significance of such a relationship. From a theoretical and academic perspective, practical significance may not be all that important, but from a physical activity promotion perspective, it is arguably of the utmost importance.

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Appendix A: International Physical Activity Questionnaire

(The IPAQ Group, 2010)

INTERNATIONAL PHYSICAL ACTIVITY QUESTIONNAIRE

We are interested in finding out about the kinds of physical activities that people do as part of their everyday lives. The questions will ask you about the time you spent being physically active in the **last 7 days**. Please answer each question even if you do not consider yourself to be an active person. Please think about the activities you do at work, as part of your house and yard work, to get from place to place, and in your spare time for recreation, exercise or sport.

Think about all the **vigorous** and **moderate** activities that you did in the **last 7 days**. **Vigorous** physical activities refer to activities that take hard physical effort and make you breathe much harder than normal. **Moderate** activities refer to activities that take moderate physical effort and make you breathe somewhat harder than normal.

PART 1: JOB-RELATED PHYSICAL ACTIVITY

The first section is about your work. This includes paid jobs, farming, volunteer work, course work, and any other unpaid work that you did outside your home. Do not include unpaid work you might do around your home, like housework, yard work, general maintenance, and caring for your family. These are asked in Part 3.

1. Do you currently have a job or do any unpaid work outside your home?

Yes

No →

Skip to PART 2: TRANSPORTATION

The next questions are about all the physical activity you did in the **last 7 days** as part of your paid or unpaid work. This does not include traveling to and from work.

2. During the **last 7 days**, on how many days did you do **vigorous** physical activities like heavy lifting, digging, heavy construction, or climbing up stairs **as part of your work**? Think about only those physical activities that you did for at least 10 minutes at a time.

_____ **days per week**

No vigorous job-related physical activity



Skip to question 4

3. How much time did you usually spend on one of those days doing **vigorous** physical activities as part of your work?

_____ **hours per day**

_____ **minutes per day**

4. Again, think about only those physical activities that you did for at least 10 minutes at a time. During the **last 7 days**, on how many days did you do **moderate** physical activities like carrying light loads **as part of your work**? Please do not include walking.

_____ **days per week**

No moderate job-related physical activity



Skip to question 6

LONG LAST 7 DAYS SELF-ADMINISTERED version of the IPAQ. Revised October 2002.

5. How much time did you usually spend on one of those days doing **moderate** physical activities as part of your work?

_____ **hours per day**
_____ **minutes per day**

6. During the **last 7 days**, on how many days did you **walk** for at least 10 minutes at a time **as part of your work**? Please do not count any walking you did to travel to or from work.

_____ **days per week**

No job-related walking → **Skip to PART 2: TRANSPORTATION**

7. How much time did you usually spend on one of those days **walking** as part of your work?

_____ **hours per day**
_____ **minutes per day**

PART 2: TRANSPORTATION PHYSICAL ACTIVITY

These questions are about how you traveled from place to place, including to places like work, stores, movies, and so on.

8. During the **last 7 days**, on how many days did you **travel in a motor vehicle** like a train, bus, car, or tram?

_____ **days per week**

No traveling in a motor vehicle → **Skip to question 10**

9. How much time did you usually spend on one of those days **traveling** in a train, bus, car, tram, or other kind of motor vehicle?

_____ **hours per day**
_____ **minutes per day**

Now think only about the **bicycling** and **walking** you might have done to travel to and from work, to do errands, or to go from place to place.

10. During the **last 7 days**, on how many days did you **bicycle** for at least 10 minutes at a time to go **from place to place**?

_____ **days per week**

No bicycling from place to place → **Skip to question 12**

11. How much time did you usually spend on one of those days to **bicycle** from place to place?

_____ **hours per day**
_____ **minutes per day**

12. During the **last 7 days**, on how many days did you **walk** for at least 10 minutes at a time to go **from place to place**?

_____ **days per week**

No walking from place to place



**Skip to PART 3: HOUSEWORK,
HOUSE MAINTENANCE, AND
CARING FOR FAMILY**

13. How much time did you usually spend on one of those days **walking** from place to place?

_____ **hours per day**
_____ **minutes per day**

PART 3: HOUSEWORK, HOUSE MAINTENANCE, AND CARING FOR FAMILY

This section is about some of the physical activities you might have done in the **last 7 days** in and around your home, like housework, gardening, yard work, general maintenance work, and caring for your family.

14. Think about only those physical activities that you did for at least 10 minutes at a time. During the **last 7 days**, on how many days did you do **vigorous** physical activities like heavy lifting, chopping wood, shoveling snow, or digging **in the garden or yard**?

_____ **days per week**

No vigorous activity in garden or yard



Skip to question 16

15. How much time did you usually spend on one of those days doing **vigorous** physical activities in the garden or yard?

_____ **hours per day**
_____ **minutes per day**

16. Again, think about only those physical activities that you did for at least 10 minutes at a time. During the **last 7 days**, on how many days did you do **moderate** activities like carrying light loads, sweeping, washing windows, and raking **in the garden or yard**?

_____ **days per week**

No moderate activity in garden or yard



Skip to question 18

LONG LAST 7 DAYS SELF-ADMINISTERED version of the IPAQ. Revised October 2002.

17. How much time did you usually spend on one of those days doing **moderate** physical activities in the garden or yard?

_____ **hours per day**
_____ **minutes per day**

18. Once again, think about only those physical activities that you did for at least 10 minutes at a time. During the **last 7 days**, on how many days did you do **moderate** activities like carrying light loads, washing windows, scrubbing floors and sweeping **inside your home**?

_____ **days per week**

No moderate activity inside home



***Skip to PART 4: RECREATION,
SPORT AND LEISURE-TIME
PHYSICAL ACTIVITY***

19. How much time did you usually spend on one of those days doing **moderate** physical activities inside your home?

_____ **hours per day**
_____ **minutes per day**

PART 4: RECREATION, SPORT, AND LEISURE-TIME PHYSICAL ACTIVITY

This section is about all the physical activities that you did in the **last 7 days** solely for recreation, sport, exercise or leisure. Please do not include any activities you have already mentioned.

20. Not counting any walking you have already mentioned, during the **last 7 days**, on how many days did you **walk** for at least 10 minutes at a time **in your leisure time**?

_____ **days per week**

No walking in leisure time



Skip to question 22

21. How much time did you usually spend on one of those days **walking** in your leisure time?

_____ **hours per day**
_____ **minutes per day**

22. Think about only those physical activities that you did for at least 10 minutes at a time. During the **last 7 days**, on how many days did you do **vigorous** physical activities like aerobics, running, fast bicycling, or fast swimming **in your leisure time**?

_____ **days per week**

No vigorous activity in leisure time



Skip to question 24

LONG LAST 7 DAYS SELF-ADMINISTERED version of the IPAQ. Revised October 2002.

23. How much time did you usually spend on one of those days doing **vigorous** physical activities in your leisure time?

_____ **hours per day**
_____ **minutes per day**

24. Again, think about only those physical activities that you did for at least 10 minutes at a time. During the **last 7 days**, on how many days did you do **moderate** physical activities like bicycling at a regular pace, swimming at a regular pace, and doubles tennis **in your leisure time**?

_____ **days per week**

No moderate activity in leisure time



Skip to PART 5: TIME SPENT SITTING

25. How much time did you usually spend on one of those days doing **moderate** physical activities in your leisure time?

_____ **hours per day**
_____ **minutes per day**

PART 5: TIME SPENT SITTING

The last questions are about the time you spend sitting while at work, at home, while doing course work and during leisure time. This may include time spent sitting at a desk, visiting friends, reading or sitting or lying down to watch television. Do not include any time spent sitting in a motor vehicle that you have already told me about.

26. During the **last 7 days**, how much time did you usually spend **sitting** on a **weekday**?

_____ **hours per day**
_____ **minutes per day**

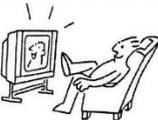
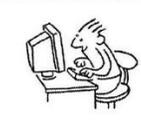
27. During the **last 7 days**, how much time did you usually spend **sitting** on a **weekend day**?

_____ **hours per day**
_____ **minutes per day**

This is the end of the questionnaire, thank you for participating.

Appendix B: Physical Activity Scale

(Aadahl & Jørgensen, 2003)

	Examples	Minutes	Hours	Time:
A	 <p>Sleep, rest</p>	<input type="checkbox"/> <input type="checkbox"/> <input type="checkbox"/> 15 30 45	<input type="checkbox"/> <input type="checkbox"/> 1 2 3 4 5 6 7 8 9 10	
B	 <p>Sitting quietly, watching television, listening to music or reading</p>	<input type="checkbox"/> <input type="checkbox"/> <input type="checkbox"/> 15 30 45	<input type="checkbox"/> <input type="checkbox"/> 1 2 3 4 5 6 7 8 9 10	
C	 <p>Working at a computer or desk, sitting in a meeting, eating</p>	<input type="checkbox"/> <input type="checkbox"/> <input type="checkbox"/> 15 30 45	<input type="checkbox"/> <input type="checkbox"/> 1 2 3 4 5 6 7 8 9 10	
D	 <p>Standing, washing dishes or cooking, driving a car or truck</p>	<input type="checkbox"/> <input type="checkbox"/> <input type="checkbox"/> 15 30 45	<input type="checkbox"/> <input type="checkbox"/> 1 2 3 4 5 6 7 8 9 10	
E	 <p>Light cleaning, sweeping floors, food shopping with grocery cart, slow dancing or walking downstairs</p>	<input type="checkbox"/> <input type="checkbox"/> <input type="checkbox"/> 15 30 45	<input type="checkbox"/> <input type="checkbox"/> 1 2 3 4 5 6 7 8 9 10	
F	 <p>Bicycling to work or for pleasure, brisk walking, painting or plastering</p>	<input type="checkbox"/> <input type="checkbox"/> <input type="checkbox"/> 15 30 45	<input type="checkbox"/> <input type="checkbox"/> 1 2 3 4 5 6 7 8 9 10	
G	 <p>Gardening, carrying, loading or stacking wood, carrying light object upstairs</p>	<input type="checkbox"/> <input type="checkbox"/> <input type="checkbox"/> 15 30 45	<input type="checkbox"/> <input type="checkbox"/> 1 2 3 4 5 6 7 8 9 10	
H	 <p>Aerobics, health club exercise, chopping wood or shoveling snow</p>	<input type="checkbox"/> <input type="checkbox"/> <input type="checkbox"/> 15 30 45	<input type="checkbox"/> <input type="checkbox"/> 1 2 3 4 5 6 7 8 9 10	
I	 <p>More effort than level H: Running, racing on bicycle, playing soccer, handball or tennis</p>	<input type="checkbox"/> <input type="checkbox"/> <input type="checkbox"/> 15 30 45	<input type="checkbox"/> <input type="checkbox"/> 1 2 3 4 5 6 7 8 9 10	