Attention Training following Moderate to Severe Traumatic Brain Injury in Adults: A Systematic Review

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COMPULSORY DECLARATION

This work has not been previously submitted in whole, or in part, for the award of any degree. It is my own work. Each significant contribution to, and quotation in, this dissertation from the work, or works, of other people has been attributed, and has been cited and referenced.

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Abstract

Traumatic brain injury (TBI) is a significant contributor to trauma-related mortality and morbidity globally. The high burden of disease for TBI is not only attributable to high incidence rates, but also due to the extensive and often severe post-TBI multi-domain sequelae, of which attentional deficits are particularly prevalent and persistent. Such deficits have far-reaching consequences, given the gateway function of this domain. The high prevalence rate of post-TBI attentional deficits holds true particularly for moderate to severe TBI populations, as more severe injuries are associated with poorer outcomes. Thus exists a clear need from a personal and economic perspective to establish reliable and valid rehabilitation efforts to aid in the recovery of those directly affected by TBI and their families, especially for deficits in attention. The current study aimed to determine the extent and efficacy of attentional training as a form of neuropsychological rehabilitation to ameliorate such deficits following moderate to severe TBI in adults, through a systematic review of relevant literature in this area. Methodology was based on PRISMA guidelines. Databases searched included CINAHL, Cochrane Library, PubMed, PsycINFO, Scopus, and Web of Science. Eligible studies included a combination of RCTs, observational studies and single case designs. All final included studies were assessed for methodological quality and analysed in accordance with the objectives of this review. A total of 7 314 articles were retrieved and of those, 4 845 articles were screened for eligibility. 21 articles met eligibility criteria and were included in this review. Findings of this review suggest that attentional gains can be made in a moderate to severe adult TBI sample, irrespective of time since injury, age, and injury severity. A growing interest in investigating the role that technology (e.g., computerised training) plays in this rehabilitation setting is ever present and needs to be further explored. Further, while findings from this review are suggestive that attentional gains are seen across techniques, it is unclear whether these gains are reflective in the patients' daily life or maintained over time. As such, it is recommended that future research adopts a more holistic assessment approach in which aspects like self-report questionnaires in conjunction with standardised neuropsychological assessments are administered with longer term follow-up assessments.

Keywords: attention, cognitive rehabilitation, neuropsychology, TBI

List of Abbreviations

ABI	Acquired brain injury
APT	Attention Process Training
GCS	Glasgow Coma Scale
MARS	Moss Attention Rating Scale
NFT	Neurofeedback training
PRISMA	Preferred Reporting Items for Systematic Reviews and Meta-Analysis
PROSPERO	International Prospective Register of Systematic Reviews
PASAT	Paced Auditory Serial Addition Task
PTA	Post-traumatic amnesia
RCT	Randomised control trial
RSAB	Rating Scale of Attentional Behavior
SCED	Single case experimental design
TAP	Test for Attentional Performance
TBI	Traumatic brain injury
TEA	Test of Everyday Attention
TMT	Trail Making Test

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Attention Training following Moderate to Severe Traumatic Brain Injury in Adults: A Systematic Review

Traumatic brain injury (TBI) is a significant contributor to trauma-related mortality and morbidity globally (Rubiano et al., 2015). With estimates of 69 million annual cases, TBI is a growing public health concern. However, this may be an underestimate, given the scarcity and accuracy of TBI databases, particularly within low- to middle-income countries (LMICs) (Dewan et al., 2018; Li et al., 2016). The high burden of disease for TBI is not only attributable to such high prevalence rates, but also due to the extensive and often severe post-TBI sequelae (Mar et al., 2011). Such outcomes not only lead to negative emotional, financial, and physical effects on patients and their families, but also to greater social impacts, including increased burden on healthcare facilities and decreased productivity or job loss, resulting in personal and global economic strain (Rubiano et al., 2015). This holds particular importance in the case of cognitive deficits, which are said to impact daily functioning most (Barman et al., 2016).

One such important deficit to consider is attention, namely due to its high prevalence following TBI. Due to widespread brain networks of attention, this cognitive domain is often disrupted following TBI. Further, most other cognitive functions are directly impacted by attention, thereby increasing the burden of disease for TBI. The high prevalence of post-TBI attentional deficits holds true particularly for moderate to severe TBI populations, as more severe injuries are associated with poorer outcomes (Svingos et al., 2019). Thus exists a clear need from a personal and economic perspective to establish reliable and valid rehabilitation efforts to aid in the recovery of those directly affected by TBI and their families (Oberholzer & Müri, 2019), especially for deficits in attention. Although there are several systematic reviews on attentional training, many of these exist within the broader context of comprehensive, multi-domain cognitive rehabilitation (see e.g., Cicerone et al., 2011). Additionally, acquired brain injury (ABI)[1] samples are generally utilised, and those that include TBI, do so in terms of mild TBI in particular. As such, this paper aims to review studies on attentional training following moderate to severe TBI to provide relevant neuropsychological intervention decision makers with updated evidence-based practical guidelines for ameliorating attentional deficits in this population.

[1] Acquired brain injury denotes any classification of brain injury acquired after birth that is not degenerative or hereditary in nature, including TBI (Acquired Brain Injury Outreach Service (ABIOS), 2020).

Definition of Traumatic Brain Injury

TBI is a type of ABI wherein an external force alters and impacts brain function (Pavlovic et al., 2019). The associated injuries are classified on two levels: primary and secondary injuries. The primary injury refers to the focal impact on the brain as a direct result of the external force acting upon it. Secondary injuries refer to the chemical, molecular and inflammatory changes that occur secondarily to the external impact on the brain, resulting in further brain damage (Galgano et al., 2017). The external impact of such an injury acts upon the brain through a combination of acceleration, deceleration, and rotational forces (Frost et al., 2013). Such forces usually occur in the context of closed TBIs whereby the skull and dura remain intact. This contrasts with open or penetrating TBIs, in which the skull and dura mater are breached by an external object (Blennow et al., 2016; Pavlovic et al., 2019). Although less prevalent than closed injuries, penetrating injuries are often more severe in nature and have higher risk of infection and hematomas (Kazim et al., 2011). Additionally, open TBIs are more susceptible to focal deficits, compared to more common closed TBIs in which diffuse deficits are usually evident post-injury, particularly with more severe injuries (Ginsburg & Huff, 2022).

Depending on the extent of injury, TBI can vary significantly in terms of severity. Classification includes mild, moderate and severe TBIs. Severity is most often classified by the period and degree of loss of or impaired consciousness, as well as a period of memory loss and confusion post-injury known as post-traumatic amnesia (PTA; Hawryluk & Manley, 2015; O'Neil et al., 2013). Severity is frequently measured by the Glasgow Coma Scale (GCS), which is considered the gold standard in TBI severity classification. GCS determines the level of consciousness out of 15, with a score of 13-15 indicating mild severity, 9-12 indicating moderate severity and a score of 8 or less indicating severe TBI (National Academies of Sciences, Engineering, and Medicine, 2019; Saika et al., 2015).

The severity of TBI is particularly important to consider, as there is a dose-response relationship which exists between severity and TBI sequelae, suggesting more severe injuries are correlated with poorer outcomes (Baum et al., 2016; Olsen et al., 2015). Mild TBI is often amenable to spontaneous recovery and frequently full cognitive functioning returns within three months post-injury. However, this is not the case in moderate to severe cases of TBI, where cognitive impairments remain and markedly impair daily functioning for many years following injury (Ruet et al., 2019). It has been reported that 80-90% of TBI cases are diagnosed as mild

(Skandsen et al., 2019). As such, mild TBI outcomes are often overrepresented in literature, compared to those reported for moderate to severe TBIs. However, even in the instance of mild TBI, and even more so in moderate to severe injuries, the outcomes resultant of TBI can be vast and far-reaching.

Common Sequelae Following TBI

Given the heterogeneity of TBI, determined by location, extent, and severity of injury, it is easy to understand that sequelae can also vary in type and severity. As such, mild to severe deficits following TBI can be seen in multiple functional domains, including physical, cognitive, emotional, behavioural, and psychological (Mar et al., 2011; Rabinowitz & Levin, 2014; Stocchetti & Zanier, 2016). Among all domains affected, cognitive deficits are often the most debilitating for patients and their families, as they can greatly impact on daily functioning (Barman et al., 2016). Frequent impairments in cognition post-TBI include deficits in attention, executive functioning, and memory (Zimmermann et al., 2017). Attention, however, plays a key role in both executive functions and memory, as higher order attentional functions are executive in nature and attention impacts how effectively information is encoded in memory (Aly & Turk-Browne, 2016; Chun & Turk-Browne, 2007).

Attention

Attentional impairments are frequently seen as an outcome following TBIs of various classifications and severity (Ahmed et al., 2017). While the neuropathological presentation of patients with TBI are highly diverse in nature, basic attentional networks are widespread throughout the brain, both anteriorly and posteriorly (Petersen & Posner, 2012; Posner & Petersen, 1990). As such, attention is highly susceptible to damage following TBIs even in the context of the heterogeneity in pathology. Literature suggests that cognitive impairments post-TBI are a direct result of axonal shearing and focal lesions found particularly within the frontal lobes and related circuitry (McDonald et al., 2002). It is within these regions where higher order attentional processes are housed. Neuroanatomically, given the anterior location of the frontal lobes, these lobes are more susceptible to damage in a TBI (Levin et al., 1987), especially in the context of falls and road traffic accidents which are the leading causes of TBI globally (Iaccarino et al., 2018). Further TBI can also exacerbate existing attentional deficits, which are rife in the general population (Chung et al., 2019), where attentional disorders, such as attention-deficit/hyperactivity disorder, may be precursor risk factors to sustaining a TBI later in life

(Biederman et al., 2015). Therefore, it is relatively unsurprising that attentional deficits are particularly prevalent and persistent in patients with TBI (Shah et al., 2017). However, to understand the far-reaching extent to which attentional impairments can disrupt daily functioning, the concept of attention needs to be defined.

The construct of attention has no consensual definition and varies according to the author, topic, and context. Essentially, attention does not refer to a single definable entity, but rather a complex construct which acts as an umbrella term for a variety of related and distinct psychological phenomena (Styles, 2006). Attention refers broadly to complex cognitive processes in which a person registers and processes external and internal stimuli (Lezak et al., 2004). There is some consensus that attention involves processing information within a limited capacity and broadly, can be intentionally and consciously controlled (Styles, 2006).

Given the complexity and lack of formal definition of attention, several attentional models have been proposed in an attempt to explain the cognitive and biological underpinnings of this concept – all of which lend evidence towards the multidimensionality of attention.

Posner's Neuroanatomical Model of Attention. An early and influential model is Posner and Petersen's (1990) neuroanatomical model of attention, which has subsequently been reviewed and updated (Petersen & Posner, 2012; Posner & Petersen, 1990; Posner & Rothbart, 2007). The underlying notion, which remains the same across the models, suggests that attention is subserved by three networks: the alerting network, orienting network, and the executive attention network. The alerting network is also described as the sustained attention or vigilance system in which a high state of responsivity towards incoming stimuli is maintained, and the alertness required to attend to such stimuli. This system is housed within thalamic, frontal and parietal cortical regions to name a few, which is primarily associated with the modulation of norepinephrine (Posner & Rothbart, 2007). More recent literature suggests that vigilance is strongly lateralised to the right hemisphere (Petersen & Posner, 2012).

The second attentional component is referred to as the orienting network, or posterior attentional network, and refers generally to the process of selective attention. Orienting involves the utilisation of sensory systems to allow for the direction of attention to specific incoming sensory stimuli, particularly to the suspected location of a stimulus. This network is modulated by acetylcholine (Posner & Rothbart, 2007). The orienting system for visual direction involves posterior brain regions, including the temporal-parietal junction and superior parietal lobe (Corbetta & Shulman, 2002).

The last component is that of the executive attention network, also referred to as the anterior attentional system. This network initially receives information from the posterior attentional system. The anterior attentional system then oversees the process of conflict monitoring. Conflict monitoring is required when the detection of a specific target creates interference with another target (Petersen & Posner, 2012). This interference thereby requires attentional control to detect and be made consciously aware of such targets. This process is modulated primarily by dopaminergic connections. Further functions include intended responses by means of the moderation of sensory, affective and cognitive systems in general. Such processes show connectivity and enhancement within the medial frontal cortex, basal ganglia, and anterior cingulate (Posner & Rothbart, 2007).

Sohlberg and Mateer's Clinical Model of Attention. Amongst several clinical frameworks that aim to explain attention and its cognitive underpinnings, a commonly used model is Sohlberg and Mateer's (2001) clinical model of attention. This model subdivides attention into three basic categories, namely focused, sustained and selective attention, in addition to further higher-order categories of attention including divided and alternating. This model of attention is particularly useful as it provides a clear clinical framework by which attentional impairments can be assessed, and therefore rehabilitated (Sohlberg & Mateer, 1989, 2010). Focused, sustained and selective attention are regarded as basic components of attention. Focused attention is defined as the basic ability to respond to and direct attention towards incoming stimuli across auditory, tactile, visual and cognitive modalities. Sustained attention refers to the process in which a person maintains awareness towards stimuli over a prolonged time period (vigilance) and it includes the mental control required to hold information and manipulate it (working memory). Selective attention is the last basic component and refers to the ability to select specific stimuli amongst an array of competing stimuli, and then maintain an attentional set towards the chosen stimuli free from distraction (Sohlberg & Mateer, 2010). The final two components of attention within this model are referred to as the higher order attentional functions, namely divided and alternating attention. Divided attention refers to the process in which one is able to cognitively produce multiple simultaneous competing cognitive outputs. In comparison, alternating attention refers to the mental control required to allow one to switch between different cognitive tasks (Sohlberg & Mateer, 1989, 2010).

Attention is considered a gateway function as it allows someone to orientate, select and maintain focus on various incoming stimuli and information – which is essential before any higher order cognitive functioning can occur (Zillmer et al., 2008). Irrespective of the model, attention broadly encompasses many daily activities including the ability to concentrate for periods of time, freedom from distractibility, orientation towards stimuli, and shifting attention between stimuli, to name a few (Tsaousides & Gordon, 2009). As such, it is understandable that attentional processes are utilised during most mental activity to complete activities of daily living (Shah et al., 2017). Therefore, an impairment in attention can have far-reaching consequences on all cognitive functioning (Glisky, 2007). Hence there lies a great need to remediate deficits in this cognitive domain as best as possible (Barman et al., 2016; Glisky, 2007). This remediation is of particular importance in moderate to severe TBI cases in which attentional deficits are frequently experienced and tend to persist over time (Beaulieu-Bonneau et al., 2017; Kinsella, 1998).

Cognitive Processes Associated with Attention. Given that attentional processing is widely distributed within the brain and serves as a gateway function to cognition, it is understandable that attentional processing is closely associated with many other distinct and overlapping forms of cognition. Notably, as seen above, whilst there are many subcategories of attentional processing, there are distinct cognitive processes which are excluded under the broader category of attentional training for the purpose of this review. An example of such related cognitive processes is unilateral/spatial neglect, which is predominantly seen following right hemispheric lesions (Posner & Petersen, 1990). Spatial neglect can be characterised as a disorder of spatial attention in which an individual lacks the ability to fully orientate and attend to stimuli on the contralateral side of space to the lesion (Heilman et al., 2000). In terms of cognitive processing, neglect can be understood as a disorder of visual scanning (Cicerone et al., 2019). As such, there is a long-established trend of remediating spatial neglect as a visuospatial and perceptual disorder, which contrasts distinctly to methods of remediation of other attentional processes (Robertson, 1990). Although literature suggests an association between spatial neglect and global attentional processes, this is likely due to brain damage typically occurring in regions that control both processes (Van Vleet & DeGutis, 2013). As such, in accordance with literature posed by Virk et al. (2015), and Park and Ingles (2001), studies solely targeting spatial neglect rehabilitation were excluded from this paper as the clinical presentation, pathology and methods remediation are significantly different from the forms of inattention as explained above.

Similarly, a concept that is frequently associated with attentional processing is working memory. Working memory can be defined as the ability to hold and manipulate maintained information in mind (Eriksson et al., 2015). It is understandable that in order to manipulate information, one must first attend to such information. As such, it is clear that these two cognitive processes are inexplicably linked. However, the distinction between them lies within the function of each – namely attention allowing for selecting and prioritising certain incoming stimuli, thereby determining the capacity in which such stimuli can be held in mind and further manipulated (working memory). While there is literature to suggest that working memory is essentially a form of controlled attention (Oberauer, 2019), this is up for debate. Based on the mechanistic underpinnings of working memory, in conjunction with the primary role of attention as a gateway function, studies solely targeting remediation of working memory were excluded in this review.

Neuropsychological Rehabilitation

Definition

Neuropsychological rehabilitation involves interventions designed to treat sequelae of brain pathology, including cognitive, behavioural, emotional, and psychosocial functioning (Yi & Belkonen, 2011). The main objective is to improve functioning of these various impaired domains to the highest level possible (i.e., as close to premorbid functioning as possible; Wilson, 2008). Neuropsychological rehabilitation can be targeted at patients with various ABI-related conditions (Yi & Belkonen, 2011). An abundance of literature reports on effective interventions post-TBI targeting cognitive deficits, thereby improving the process of recovery, as well as reducing functional disability (Barman et al., 2016). Such interventions can be categorised under different approaches.

Approaches

Neuropsychological rehabilitation can be classified under two main approaches, namely restorative and compensatory. Compensatory approaches utilise methods that bypass impairments and rely on intact functional systems or cognitive abilities to achieve certain tasks or activities (Koehler et al., 2011; McDonald et al., 2017). In contrast, restorative, or cognitive retraining, is a process by which neural pathways are targeted and is aimed at restoring premorbid functioning (Gilmore et al., 2021; Wilson, 1997). This is often achieved by targeting a particular cognitive domain, like attention, and utilising a graded-attentional approach using repetitive, practice-oriented activities (Sohlberg et al., 2003). Attention, in particular, lends itself towards a more restorative approach (Koehler, et al., 2011).

A growing body of evidence suggests that utilising a combination of both restorative and compensatory techniques (Gordon et al., 2006), as well as introducing meta-cognitive approaches, is most effective in improving patients' capabilities to complete tasks of daily living (McDonald et al., 2017). A systematic review suggested that the inclusion of metacognitive strategies, including self-monitoring and clinical feedback from a clinician, is more effective than attentional training on its own (Cicerone et al., 2011). Such findings are supported in recent guidelines for the management and treatment of attentional processes post-TBI (Ponsford et al., 2014).

Attentional Training

Attentional training is a long-standing form of cognitive rehabilitation technique. Amongst the previous and current literature, there are several attention-specific training programmes used by clinicians, including Attention Process Training (APT-3; Sohlberg & Mateer, 2010), and the paediatric version thereof Pay Attention! (Thomson et al., 1994, 2005), Attention Training Technique (Wells, 1990), Time Pressure Management (Fasotti et al., 2000), and Attentional Control Training (McMillan et al., 2002) to name a few. Such programmes often rely on restorative rehabilitative techniques by including a variety of activities and can thereby target and train several attentional domains. Models upon which attention programmes are based, often include a hierarchy of activities which increase in difficulty as the patient progresses. A cessation of activities may occur when a certain target score is achieved and maintained (Sohlberg & Mateer, 2010). Alternatively, activities are continuously replicated with the intent of reducing the number of errors or time across the intervention period. Attentional programmes can be paper-and-pencil based (e.g., APT-I), however, there is a growing interest in technological advances for treatment.

Computerised training is particularly favourable as clinicians can monitor progress remotely, as well as including objective recording of scores for each activity over time. A particularly well-known computerised attentional training programme that utilises both restorative and compensatory approaches is APT-3 (Sohlberg & Mateer, 2010). However, computerised training is restricted in a resource-limited setting and requires highly experienced personnel to conduct the training (Cicerone et al., 2011; Ferreira-Correia et al., 2018). Furthermore, some literature argues that computer-based attention tasks do not have sufficient evidence to generalise improvements into tasks of daily living (Ponsford et al., 2014; Roitsch et al., 2019; SIGN, 2013). That said, there are studies that have reported on computerised programmes yielding significantly better functional improvements compared to traditional rehabilitation efforts (Fernandez et al., 2018). This debate regarding the ecological validity of computerised training interventions within the evidence-based practical guidelines and extant literature, highlights the need for further research into the efficacy of attentional training, specifically in relation to daily functional abilities.

Many neuropsychological interventions that target attention, do so in combination with targeting another functional domain, such as executive functioning or memory (see e.g., Bogdanova et al., 2016; Leśniak et al., 2020). As attention and executive functions have neural correlates within the frontal lobe circuitry, it is evident that these functional domains are connected and may benefit from a combined approach (Azouvi et al., 2004). However, by targeting more than one domain simultaneously, it makes it difficult to tease apart exactly which domains are targeted by each component of the intervention and the subsequent efficacy of each component of the training programme. This holds particularly true in the context of attention, which has an impact on all other mental processes. As such, a need to establish the efficacy of interventions that target attention individually, is paramount. One such way to achieve this is by establishing clinician-focused and ecologically valid guidelines on attentional training, such as those provided in systematic reviews.

Systematic Reviews on Attention

Within the past decade, there have been several systematic reviews on attentional training with brain-impaired individuals. However, the majority look at comprehensive, multi-domain cognitive rehabilitation in general, and not specifically at attention (see e.g., Barman et al., 2016; Cicerone et al., 2019). In review of the existing literature, only four systematic reviews within the last two decades target attention specifically through cognitive rehabilitation (see Appendix A; Jabalera et al., 2012; Knowles et al., 2016; Loetscher et al., 2019; Virk et al., 2015). Furthermore, although many cognitive domains overlap, attention appears to be particularly amenable to methods of restoration, as opposed to domains such as executive functioning and memory, for which remediation is more effective when using compensatory strategies (Koehler et al., 2011). As such, reviews including studies targeting multiple domains simultaneously challenge the validity of assessing the efficacy of rehabilitation on attention alone (see e.g., Bogdanova et al., 2016; Ponsford et al., 2014). Herewith lies the importance of exploring neuropsychological rehabilitation that targets attention specifically. Especially when considering the significant role that attention plays in mediating all other cognitive processes, there exists a clear need to review the extent to which research suggests it is amenable to treatment.

A further consideration regarding existing literature is that many of the systematic reviews conducted include ABI samples when reviewing cognitive rehabilitation rather than TBI-specific samples (see e.g., Marshall et al., 2018; van Heugten et al., 2012). Similar findings are present when reviewing attention-specific rehabilitation, where patients with stroke pathology demonstrated significant improvement in attentional gains whereas the TBI sample did not (see e.g., Virk et al., 2015; see Appendix A). ABI samples include many aetiologies including strokes, TBI, inflammatory responses, tumours, or any other acquired brain pathology (Lanzillo et al., 2019).

However, different pathologies are associated with different pathological mechanisms of insult within the brain. For example, comparing TBI to stroke, although similar in terms of mechanisms of cell damage, there are several pathophysiological differences between the two. TBI, especially due to blunt forces, results in damage to the cell body membranes in neurons, as well as shearing impact on white matter, which is important for processing speed, resulting in more diffuse damage across the brain (Bramlett & Dietrich, 2004). This is particularly true in the context of closed TBIs, which are more common and result in more diffuse brain damage (Ginsburg & Huff, 2022). In strokes, however, more chemical outcomes are present, such as abnormal metabolic activity and ion perturbations, and damage is frequently more focal in nature (Bramlett & Dietrich, 2004). Differing brain pathologies result in different types of outcomes, as well as potential for recovery (REF). As such, the type of injury is important to classify in rehabilitation literature and consideration of specific types of ABIs and related outcomes, is necessary.

In addition to the differing pathological mechanisms of injury, an important factor in rehabilitation literature is the severity of injury. As explained above, mild TBI, which is overrepresented in literature in terms of TBI-related research, is much more amenable to spontaneous recovery. This is directly compared to moderate-to-severe injuries in which sequelae may persist for many years following injury (Ruet et al., 2019; Schretlen & Shapiro, 2003). Of the recent systematic reviews on cognitive rehabilitation and attentional training, all three reviews include mixed severity samples (Knowles et al., 2016; Loetscher et al., 2019; Virk et al., 2015), and the last review does not state severity (Jabalera et al., 2012). Given the importance of severity in determining the efficacy of rehabilitative techniques, there is a clear paucity in examining the lesser-examined moderate-severe TBI sample – which this review aims to address.

The aim of this review is in line with the overall need to evaluate the evidence base of rehabilitative strategies in clinical samples. It is essential that clinicians remain up to date with current evidence-based practice to inform diagnostics, sequelae, and treatment for varying medical conditions (Abbas et al., 2008). This is particularly true in the context of neuropsychological rehabilitation in which technological advances and novel techniques are constantly evolving (Barman et al., 2016). As such, the need for systematic reviews highlighting recent developments within neuropsychological rehabilitation is clear.

Rationale, Aim, and Objective

The majority of existing reviews on the efficacy of attentional training, do so within the context of comprehensive, multi-domain cognitive rehabilitation in general, thereby making it difficult to ascertain the contribution of attention training alone. That said, there have been a select few recent reviews published on cognitive rehabilitation in ABI samples in which attentional training is targeted and analysed independently (see Appendix A). However, within these reviews there exists a gap regarding the type (TBI) and severity of injury (moderate to severe). As noted, the type of injury not only influences the neuropathology and subsequent sequelae post-injury, but also the potential for recovery. TBI severity is also important to consider as more severe TBI classification is correlated with worse prognostication of functional outcomes (Baum et al., 2016). However, many reviews include TBI without specifying severity, or make causal inferences based on findings of varied severity. Differences in severity are important to distinguish as treatment plans and success of rehabilitation will vary based on severity classification (Bogdanova et al., 2016). To the best of my knowledge, there are currently no systematic reviews that look specifically at a moderate to severe TBI sample, regarding methods of attentional training. It is the hope that this systematic review will contribute to the growing evidence-based practical guidelines which inform neuropsychological rehabilitation efforts.

Aim and Objective

The primary aim of this research was to conduct a systematic review on studies on attention training following moderate to severe TBI in adults, with the intent of summarising the efficacy of remediation strategies. To this end, the objective of this study was to determine the extent and efficacy of attentional training as a form of neuropsychological rehabilitation to ameliorate attention deficits following moderate to severe TBI in adults.

Method

Procedure

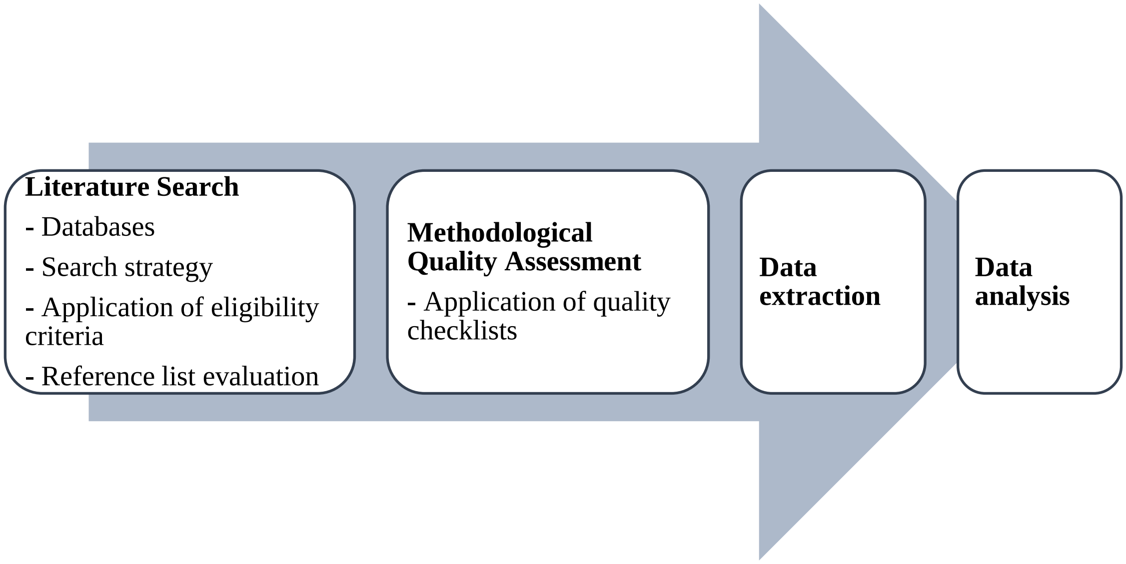
The protocol for this systematic review was developed in accordance with the guidelines outlined in the Preferred Reporting Items for Systematic Reviews and Meta-Analysis (PRISMA) statement. As such, this review was structured according to the PRISMA 2020 Checklist (Page et al., 2021; see Appendix B). The protocol of this review was registered with the International Prospective Register of Systematic Reviews (PROSPERO; registration ID: CRD42021223666). Notably, due to previous prioritisation of Covid-19-related protocols by PROSPERO, this protocol was automatically published, and the PROSPERO team had not checked this review for eligibility.

Overview

Figure 1 depicts a summary of the procedure followed in this review.

Figure 1

Summary of Systematic Review Procedure



Literature Search

Databases. A search of literature was conducted through multiple online databases including Cumulative Index to Nursing and Allied Health Literature (CINAHL), Cochrane Library, PubMed, PsycINFO, Scopus, and Web of Science.

Search Strategy. Boolean logic was combined with individual database index phrases. Combinations of search phrases were selected by two reviewers (A.S. and T.C.T.) in accordance with the aim of assessing the efficacy of attentional training in adults who had sustained moderate to severe TBI. Table 1 gives a summary of the Boolean phrases used. According to Boolean logic, these were combined with the “OR” operator when combining Boolean phrases with index terms for each database that allowed for index phrase searching (CINAHL, Cochrane Library, PsycINFO, PubMed). Each key term (Boolean phrase and index term) was combined with “AND” Boolean logic for a final search in each database. For detailed search strategies for each database, see Appendix C. Searches were conducted on 27 January 2021 by both reviewers independently. A comparison of total searches was made to ensure that the same articles were retrieved for each database.

Table 1

Boolean Phrases for Literature Search

Key Term	Phrase
TBI	traumatic brain injury OR traumatic brain injuries OR TBI
Attention	attention*
Intervention	Intervention OR interven* OR rehabilitation OR rehab OR rehab* OR rehabilitate OR program OR programme OR train* OR training OR remediation OR remediat* or treat*
Type of intervention	cognit* OR neuro* OR psycholog*

Note. *denotes suffix including, but not limited to, "y", "ion", "ive", "al", "ing", "ed"

All articles retrieved from database searching were then exported to EndNote – a reference managing software package (The EndNote Team, 2013). Using EndNote's inbuilt software, duplicates were reviewed and deleted. The remaining citations were uploaded onto Rayyan - a free systematic review software package (Ouzzani et al., 2016). Once again, using Rayyan's inbuilt software, duplicates were detected, reviewed and removed.

Application of Eligibility Criteria. Each of the articles uploaded to Rayyan were then assessed according to their eligibility. All types of studies were considered for this review, including but not limited to case studies, single case experimental designs (SCEDs), cross-sectional data, and experimental designs. As this review does not include a meta-analysis, qualitative studies were included. Additionally, only peer-reviewed articles were included to ensure that studies included in this review were methodologically sound and had gone through an appropriate ethical process. As such, dissertations and non-peer reviewed conference proceedings were excluded. Studies written or translated into English only were included, as both reviewers are only fluent in English, thereby reducing the risk of misinterpretation of data.

In accordance with the aim of this review, any type of neuropsychological intervention that targeted attention was considered. This includes any form of intervention aimed to improve cognitive, behavioural, affective and/or psychosocial functioning by means of cognitive rehabilitative, psychoeducational, or psychotherapeutic strategies either at an individual or group level (Yi & Belkonen, 2011). This excludes any medical interventions, for example but not limited to psychopharmacological or brain stimulation techniques. If used in combination with neuropsychological intervention, and reported on separately, data pertaining to the neuropsychological intervention was extracted. An additional consideration, as demonstrated by literature, was that many interventions target several cognitive domains. As such, more generalised cognitive rehabilitation interventions or programmes were considered, provided that attention training was present, as well as reported on and conducted distinctly from other cognitive domains. Furthermore, studies reporting on the various subdomains of attention (e.g., sustained, focused, divided, selective, and alternating) were considered, and subsequently reported on in this review. Notably, interventions targeting hemineglect were not considered as these are clinically and pathologically distinct from other forms of inattention (Park & Ingles, 2001).

Eligibility was also determined based on sample characteristics used within articles. In cases where ABI samples were reported on, data for the TBI sample was extracted if reported on separately. Additionally, as on the focus for this review was moderate to severe TBI, only those studies that focused specifically on samples of individuals with moderate to severe TBI, and those in which data for participants with moderate to severe TBI could be extracted, were included. As discussed in the literature review, post-TBI sequelae and potential for recovery differ based on TBI severity – as such, valid findings for this review could only be made if TBI severity was clearly defined. Most commonly, TBI severity is classified using the GCS, where a score of 3 to 8 indicates a severe injury and a score of 9 to 12, a moderate injury (Teasdale et al., 2014). Other methods of TBI classification, such as PTA defined as the time taken to return to premorbid memory functioning and awareness after sustaining a TBI (Parker et al., 2022), loss of consciousness, and brain imaging, are also appropriate measures of severity, and as such were considered in accordance with the report issued by Centers for Disease Control and Prevention (2015)[2]. Notably, some studies did not report severity, and in such cases, were excluded. For

[2] This report classified TBI severity as follows: period of PTA between one and seven days (moderate) and above seven days (severe); loss of consciousness between 30 minutes and 24 hours is moderate, and any time above this. threshold is severe; AIS score of 3 and 4 to 6 is moderate and severe respectively; imaging can provide severity indication based upon clinical judgment of imaging results.

studies including participants with mixed levels of severity and including mild TBI, data for moderate and severe TBI diagnoses were extracted if reported on separately.

This review only included adult participants, i.e., those who were aged 18 and over. This decision is based upon the ongoing brain development in paediatric samples, the common sequelae following TBI, as well as potential for recovery and remediation which are clinically distinct from adult samples (Figaji, 2017; Greenwald, 2011). In the case of studies which included combined adult and paediatric samples, the adult data was extracted if reported on independently. Last, research was only included if conducted on a human sample.

Following the above-mentioned process, both reviewers applied the exclusion and inclusion criteria to each of the final articles uploaded to Rayyan. By utilising Rayyan, each reviewer evaluated each article and decided if each article met the inclusion criteria with reasoning for each decision. This process was conducted independently by each reviewer.

Once both reviewers had completed their independent selection, decisions were unblinded. Rayyan allows for a filtering of all the articles that were not agreed upon for inclusion. In this instance, the two reviewers discussed each article until a consensus was reached. Associate Professor Leigh Schrieff-Brown was consulted in the event of any disagreement, to make the final decision.

Reference List Evaluation. To ensure that all possible studies were considered, the reference list of each included article was reviewed to see if any cited articles met the inclusion criteria to qualify for selection in this systematic review.

Methodological Quality Assessment

In line with the notion that systematic reviews are considered the gold standard when reporting on the efficacy, benefits, and potential harms of various interventions or approaches (Ernst & Pittler, 2001; Higgins et al., 2019), it is logical that the articles included within the review need to be of a high methodological standard. As such, all articles included up to this point were assessed for risk of potential bias by their assessing methodological quality.

A quality review of selected studies was assessed using either Cicerone et al.'s (2009) Randomised Control Trial (RCT) and Observational Study Checklist (see Appendix D), or Tate et al.'s (2008) SCED Scale (see Appendix E), depending on the study design of each article. The RCT and Observational Study Checklist was developed specifically for evaluating cognitive rehabilitation research following TBI. The initial development of the scale reported a moderate to substantial interrater reliability (*ICC* = .59–.64) amongst raters and is based upon long-standing methodological quality literature for rehabilitation research (Cicerone et al., 2009). The SCED Scale was developed as the first validated scale to measure the methodological quality of single-subject designs. This checklist also demonstrated high interrater reliability for the total score (*ICC* = .83-.88) demonstrating its utility in systematic review research (Tate et al., 2008).

According to Cherney et al. (2013), methodological quality tools must not only be valid and reliable but must be sensitive to the research design in question. As such, two checklists were appropriate to use, rather than adjusting one checklist for multiple designs. Both checklists apply a numeric rating scale, where one point is given for meeting the criterion, and zero is given if the criterion is not met. As such, higher scores on both scales represent higher methodological quality. The total score was adjusted by removing items that were not applicable for the RCT and Observational Study Checklist as is stipulated in the development of the tool (Cicerone et al., 2009). As such, the overall score was lower, and a percentage rating could be ascertained rather than a total point score. This was done in order to distinguish clearly those criteria that were not met (i.e., received a score of 0 for that item) compared to items that were inherently not applicable in the design, and therefore is not representative of poor quality The application of checklists was conducted independently by both reviewers, followed by a discussion until 100% agreement was reached.

Notably, some literature suggests that using a numeric rating scale, like those used above to assess quality, can result in invalid judgments. For example, a high score on certain categories may result in the appearance of an unbiased study as those criteria will be met. However, there may be very clear biases in other categories which do not change the total rating score significantly overall, but indeed pose a methodological flaw (O'Connor et al., 2015). Notably, for both scales used, each criterion has an equivalent rating of one point. However, based on the individual researcher and the focus of the research, some criteria could be argued as inherently more important in assessing methodological quality. Therefore, quality ratings should rather be interpreted regarding overall direction towards a higher or lower score, correlating to higher and lower quality respectively (Cherney et al., 2013).

By using two independent scales of methodological quality, it was important to establish a method of comparing results, as each scale has a different total score. As such, a percentage rating of each article's score against the total score of the scale was calculated. A percentage rating was calculated by taking the sum of the items that had been awarded credit for, dividing this by the sum of the total *applicable* items, and multiplying this by 100 (For example, for Kim et al., 2009: 6/12*100 = 50%). A comparison of differing quality scales with differing categories and total scores is important as causal inferences can only be reliably drawn from methodologically sound articles. While the use of numerical rating scales like those used in the checklists used here have been argued as delivering invalid judgements on overall quality (O'Connor et al., 2015), such scales can still demonstrate very clear biases with particularly low scores per category (O'Connor et al., 2015). As such, no article was excluded from this review purely based on low quality, but rather such articles with scores lower than 40% were interpreted with caution. Furthermore, as there were no cut-off scores applied to the SCED checklist (Tate et al., 2008), and the RCT checklist (Cicerone et al., 2009) cut-off scores were chosen arbitrarily by the tool developers, no percentage cut-off score was applied to the methodological quality assessment in this study. As such, data analysis and recommendations were made in this review based upon the relative strength of evidence presented in each article (Cicerone et al., 2019). For example, when assessing the relative efficacy of the results of each study, studies with poor methodological quality findings on the efficacy of attentional training were made against the background of how reliable and valid the findings are. This decision to not exclude any studies based on poor methodological quality is further supported by the National Centre for Dissemination of Disability Research (NCDRR) which states that the best *available* evidence should be presented in systematic reviews, rather than the best possible evidence, to ensure that there are sufficient recommendations presented (Dijkers, 2009).

Data Extraction

Once a finalised list of studies was agreed upon, relevant data from each article was extracted. Data extraction was carried out using an Excel spreadsheet with predetermined headings, namely: citation, type of study, sample size, sample characteristics, measures used, summary of intervention, measures used to assess attention, statistical outcomes and results, effect size, conclusion, and limitations. Notably, in the case of multiple cognitive domains targeted and measured, only data relevant to attention was extracted.

Data Analysis

Once all relevant information was extracted, a descriptive evaluation was created on both methodological quality and efficacy of attention training across studies included in this review.

Information including sample details, measures, findings based on the intervention, and limitations of each study, was reviewed and synthesised. Based on the data extracted, a descriptive trend analysis was used to determine if there were distinct patterns of findings across studies.

Ethical Considerations

The current study was approved by Research Ethics Committee in the Department of Psychology at the University of Cape Town – reference number is PSY2020-030 (see Appendix F).

Transparency

This review was conducted with complete transparency by disclosing all funding or any other conflicts of interest. Both reviewers aimed to report without bias to ensure ethical results were achieved. Furthermore, this review was registered with PROSPERO in an effort to increase transparency.

Bias

There is a risk concerning systematic reviews regarding bias within reporting. Some bias is inherent and cannot be controlled for (e.g., studies with statistically significant results have a higher probability of being published than those which do not contain statistically significant results; Drucker et al., 2016). As such, it is essential to reduce bias as best as possible when conducting a systematic review. This was achieved by adding an additional reviewer to the article selection and review process, having a third reviewer to mediate any disagreements, and by following a strict and standardised protocol in which all steps undertaken are reported to ensure transparency and replicability.

Results

The presentation of results below follows the same structure of information as presented in the method section above.

Literature Search

Database and Search Strategy

Searches of all aforementioned databases with key terms were conducted on 27 January 2021 by both reviewers, independently. A total of 7,314 articles were retrieved from all databases. These articles were then exported to EndNote (The EndNote Team, 2013) where duplicates were reviewed and deleted, which resulted in a total of 4,845 articles. The remaining citations were uploaded onto Rayyan (Ouzzani et al., 2016) where duplicates were once again detected, reviewed, and removed, resulting in a total of 4,325 articles to be assessed for inclusion based on eligibility criteria (see Figure 2 below).

Application of Eligibility Criteria

All articles uploaded to Rayyan were assessed independently by both reviewers as to whether eligibility criteria were met or not, and therefore whether the article should be included in this review or not. Cohen's kappa (κ) was run to determine the level of agreement between both raters on the articles to be included (N = 4,325). There was substantial agreement between the two raters, κ = .644, *SE* -.048 (95% CI, .550 to .739), $p < .001$. Based on the guidelines from Altman (1990), and adapted from Landis and Koch (1977), a κ of .495 represents a substantial strength of agreement.

Once both reviewers had completed their independent selection, decisions were unblinded. Articles for which reviewers reached different outcomes for inclusion were discussed until a consensus was reached. A total of 4,197 articles were excluded purely based on their title and/or abstract, applying the eligibility criteria described in the method section above. This resulted in 128 articles that needed to have their full text reviewed for a decision on inclusion. 107 of these articles were excluded (see reasons in Figure 2 below). Of these, 21 articles were selected for inclusion and thus for methodological review. A summary of the citation information for each article is given in Appendix G.

Reference List Evaluation

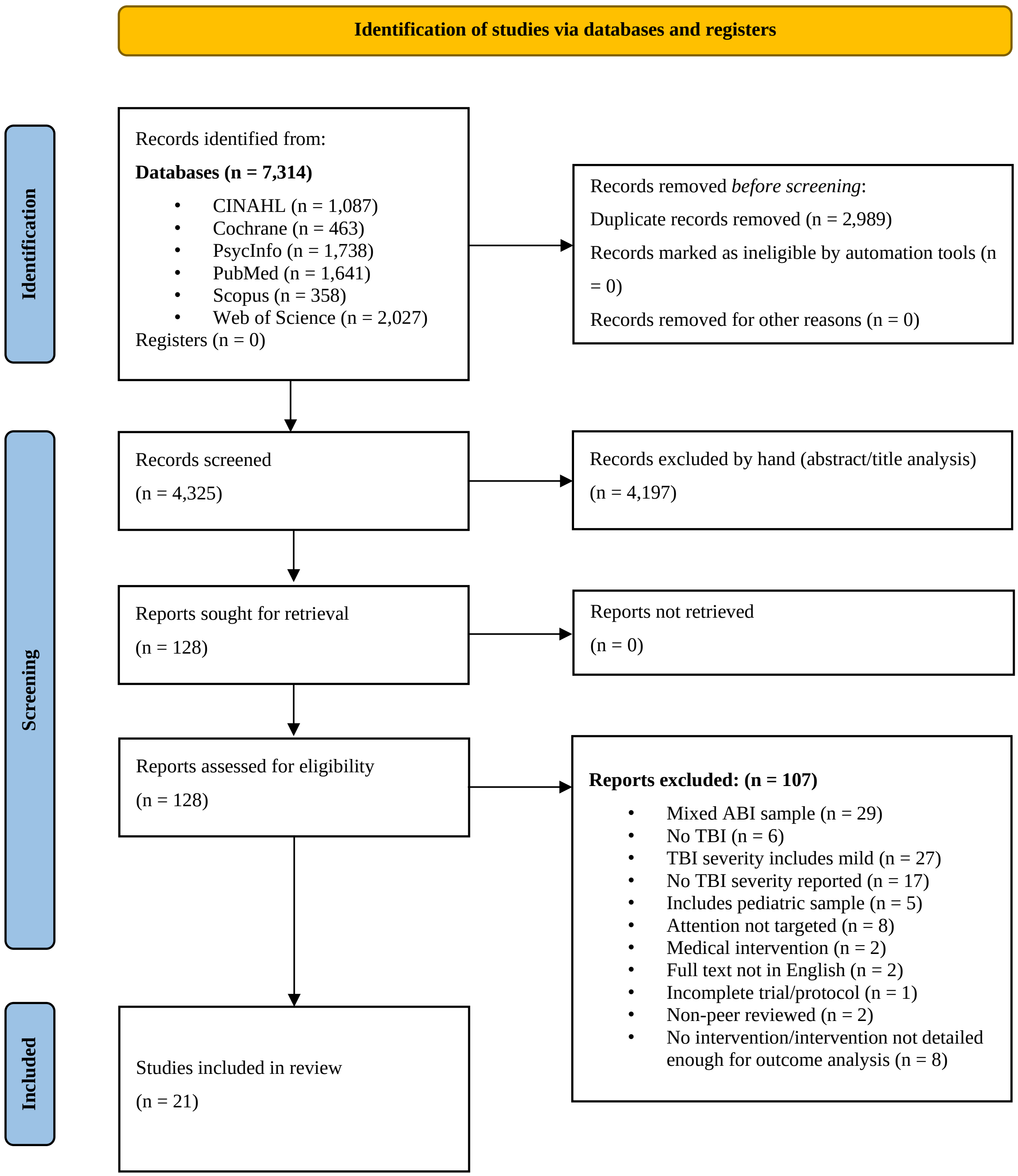
Each of the 21 articles had their reference lists evaluated to check for any other studies that may have been suitable for inclusion according to eligibility criteria. No other studies were found to be suitable for inclusion.

Summary of Article Selection Process: PRISMA Flow Diagram

The PRISMA 2020 flow diagram is shown below in Figure 2 as per the guidelines outlined in the PRISMA 2020 Checklist and statement (Page et al., 2021). The PRISMA 2020 flow diagram illustrates in detail the quantitative features of each stage within the review (i.e., how many articles were identified, screened, excluded, and finally included with reasons).

Figure 1

PRISMA 2020 Flow Diagram



Methodological Quality Assessment

Each of the final 21 articles chosen was assessed by the appropriate checklist, independently by each reviewer. Eight of these articles were RCT-type or observational designs. The remaining 13 articles are classified as SCEDs (*N* = 1). Intraclass correlation (ICC) was run on total scores for the SCED checklist. A high degree of reliability was found between both raters for total scores of the SCED checklist. The average measure ICC based on absolute agreement was .884 with a 95% confidence interval from .636 to .964 ($F(12, 12) = 8.854$, $p < .001$). However, ICC could not be run for RCT checklist due to it being underpowered, as per recommendations by Bujang and Baharum (2017). As such, point-to-point inter-rater reliability between both raters was calculated for each article. A summary of significant results is reported in table 2 below.

Table 2

Summary of Point-to-Point Inter-Rater Reliability

Rating Scale	Percentage agreement range	Average unweighted kappa coefficient
RCT and Observational Studies Checklist (Cicerone et al., 2009)	56.25 – 100.00%	.672 (*N = 5)* [a]
SCED Checklist (Tate et al., 2008)	60.00 – 100.00%	.820 (*N* = 8) [a]

Note. [a] Only significant results were included in average weighted kappa calculation.

Following this, both reviewers discussed the results of each article until 100% agreement was reached. The final agreed upon ratings for each article is demonstrated in Tables 3 and 4. Table 3 includes the eight studies that were measured for quality by the RCT and observational design checklist by Cicerone et al. (2009). Table 4 denotes the quality checklist of the remaining 13 studies assessed using the SCED checklist by Tate et al. (2008). A percentage rating of each article’s score against the total score of each of the two scales utilised was calculated (see table 3 for RCT and observational quality scale and table 4 for SCED quality scale below). No article was excluded from this review purely based on low quality, but articles with scores lower than 40% were interpreted with caution. Such studies include those by Mohanty and Gupta (2013), and Wall et al. (2013).

RCT and Observational Design Methodological Quality Analysis

Total Quality Score Across Studies. As noted, the total number of items awarded credit for each study was calculated as a percentage in order to draw comparisons across studies (see Table 3 below). Of note, across all eight studies, at least 50% of the 13 quality criteria was met. However, the highest total quality score was only at 68.75%, suggesting a lack of fundamental design characteristics across the board, although a very similar standard of quality with little variability was seen across all eight studies (*M* = 55.47, *SD* = 7.79). That is to say, whilst RCT and observational studies across the board were all similar in terms of methodological quality, the overall mean total score is lower than expected. As such, a low average score across all studies was indicative of poor methodological design or suggests that criteria specified in the checklist may be too stringent.

Quality Score for Each Independent Criterion Across Studies. Each subcategory within the checklist (i.e., internal validity, descriptive criteria, and statistical criteria) was reviewed across the eight studies to identify the relative prevalence of meeting within-subcategory-specific criteria (see Figure 3 below). The first category reviewed in the checklist was that of *internal validity* which refers to the extent to which a valid cause-effect conclusion could be drawn from the results without the influence of confounding factors (Patino & Ferreira, 2018). Of the *internal validity* category, the following criteria were consistently met within the study designs: specifying eligibility criteria, description of similarity of baseline characteristics, both treatment and control intervention described, and containing relevant outcomes measures. These criteria speak to strong internal validity and sufficient detail for replication. However, there were some criteria of internal validity that were not included in many of the study designs, including detailed description of randomisation method, the concealment of treatment allocation, co-interventions avoided or equivalent, and the blinding of outcome measures. Although many of these criteria are dependent on a strict RCT design, which some of these studies did not follow, it does highlight a cause for concern in terms of replicability.

The second category reviewed is *descriptive criteria* which broadly requires a satisfactory description of the various basic components which comprise the design of the study. Most criteria within the *descriptive criteria* category were met consistently across all eight studies, including the reporting of withdrawal and drop-out rates, timing of outcome assessments and sample size, and the presence of short-term outcome assessments. However, there was one criterion of particular concern, namely that studies lacked the presence of long-term outcome measures within their design. A study was required to have an outcome measurement conducted at least three months post-intervention completion to be awarded credit for this item. From all studies reviewed, only one study (Leśniak et al., 2020) out of eight, was awarded credit for this item.

The last category reviewed was the *statistical* category which aimed to evaluate the suitability and adequacy of the statistical analyses used within the study. In this category, two out of three criteria were particularly concerning in the lack of consistency across studies, namely the presence of ITT analysis and the statistical comparison of treatment effects. ITT analysis requires that all randomised participants are reported and analysed, apart from missing values. Many of the studies reviewed did not include those participants that completed the baseline interventions but could not complete the outcome measures in their pre-post intervention analyses, and therefore did not meet the ITT analysis criterion. The statistical comparison of treatment effect criterion requires between-group analyses, in conjunction with within-group analyses, to receive full credit. Both criteria were not consistently met across the eight studies reviewed.

Table 3

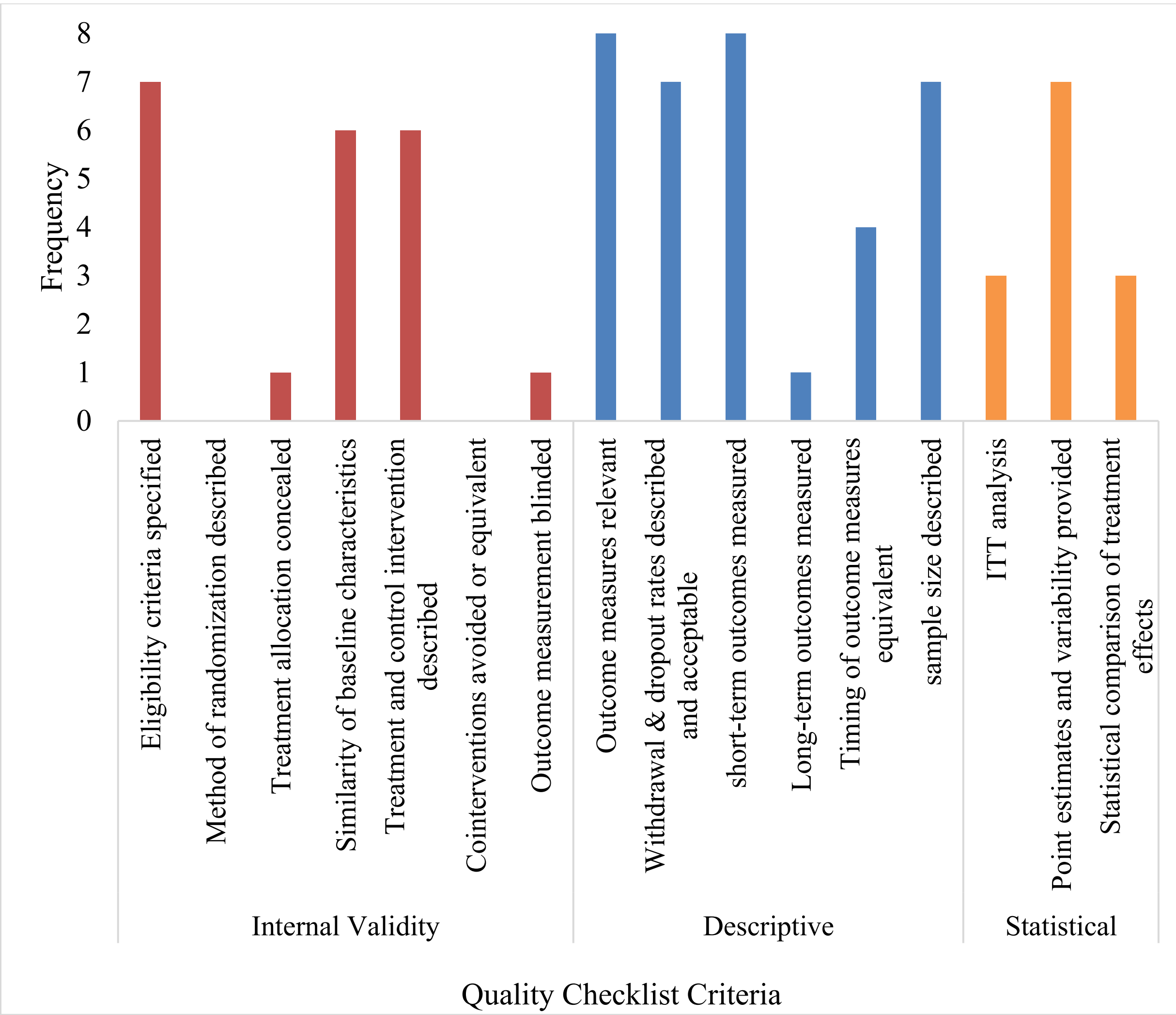
Summary of Modified Methodological Quality Checklist: RCTs and Observational Studies (Cicerone et al., 2009) Across Studies (N = 8)

	Internal Validity							Descriptive						Statistical			Totals	
Authors	Eligibility criteria specified	Method of randomization described	Treatment allocation concealed	Similarity of baseline characteristics	Treatment and control intervention described	Cointerventions avoided or equivalent	Outcome measurement blinded	Outcome measures relevant	Withdrawal & dropout rates described and acceptable	short-term outcomes measured	Long-term outcomes measured	Timing of outcome measures equivalent	sample size described	ITT analysis	Point estimates and variability provided	Statistical comparison of treatment effects	Total Score	Percentage of Total Score
Dvorkin et al.	X				X		X	X	X	X			X		X		8	50.00
Couillet et al.	X		X	X	X			X	X	X		X	X		X	X	11	68.75
Grealy et al.	X			X				X	X	X			X	X		X	8	50.00
Keller					X			X	X	X		X	X	X	X		8	50.00
Kim et al.	X	N/A	N/A	X				X	N/A	X			X	N/A	X		6	50.00
Leśniak et al.	X			X	X			X	X	X	X	X	X		X		10	62.50
Niemann et al.	X			X	X			X	X	X		X	X		X	X	10	62.50
Park et al.	X			X	X			X	X	X				X	X		8	50.00
Total per item	7/8	0/7	1/7	6/8	6/8	0/8	1/8	8/8	7/7	8/8	1/8	4/8	7/8	3/7	7/8	3/8		

Note. X denotes a criterion being met. N/A represents a criterion not being applicable given observation study design, as per guidelines in Cicerone et al. (2009).

Figure 3

Frequency of Criteria Met on Modified Methodological Quality Checklist: RCTs and Observational Studies (Cicerone et al., 2009) Across Studies (N = 8)



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SCED Methodological Quality Analysis

Total Quality Score Across Studies. Similar to above, the total number of items awarded credit was calculated as a percentage for all studies (see Table 4 below). There was large variation of quality across studies, with achievement of quality credit ranging from 10.00% to 100.00%, with a mean percentage of 58.46% (*SD* = 25.44). Although not all studies claim to be true SCEDs, it speaks to a large variation in the standard of presenting studies with such designs.

Quality Score for Each Independent Criterion Across Studies. Again, each criterion was reviewed to establish the prevalence across the literature for each criterion (see Figure 4 below). There are three criteria which were predominantly met across all studies as discussed below. The first being that target behaviours were identified and defined. All 13 studies included operationally defined, repeatable behaviours and outcomes that were then used to assess the intervention's success. The second criterion, a design with a cause-and-effect analysis to adequately establish the efficacy of the intervention, was met by 12 studies. The last criterion which was consistently met was the inclusion of a baseline description. 11 studies were awarded merit for this criterion by successfully sampling sufficient outcome measures and behaviour prior to the intervention to provide a reliable and sufficient set of baseline measurements.

Notably, there were several criteria of concern that were not consistently met across studies as discussed below. The presence of some form of inter-rater reliability was required for merit on the item related to inter-rater reliability. Only two studies reported inter-rater reliability. Many of the studies only had one primary researcher implementing the outcomes measures, hence the lack of inter-rater reliability. Only two studies met the criterion for the independence of assessors. This criterion required a blind assessor for the evaluation of outcomes measures for each participant, in an attempt to reduce bias. However, in many of the studies included in this review, the primary researcher implemented the intervention and completed the outcome measures, thereby making it impossible to blind the assessment. A further criterion that was not consistently met across studies included generalisation of treatment. To be awarded credit on this criterion, the study was required to demonstrate the functionality of the intervention effects outside of the intervention setting in the participants' daily life. Six studies met this criterion, thereby demonstrating a lack in the generalisability of conclusions that could be drawn by the remaining studies that did not satisfy this criterion. The remaining criteria were moderately met across studies.

Table 4

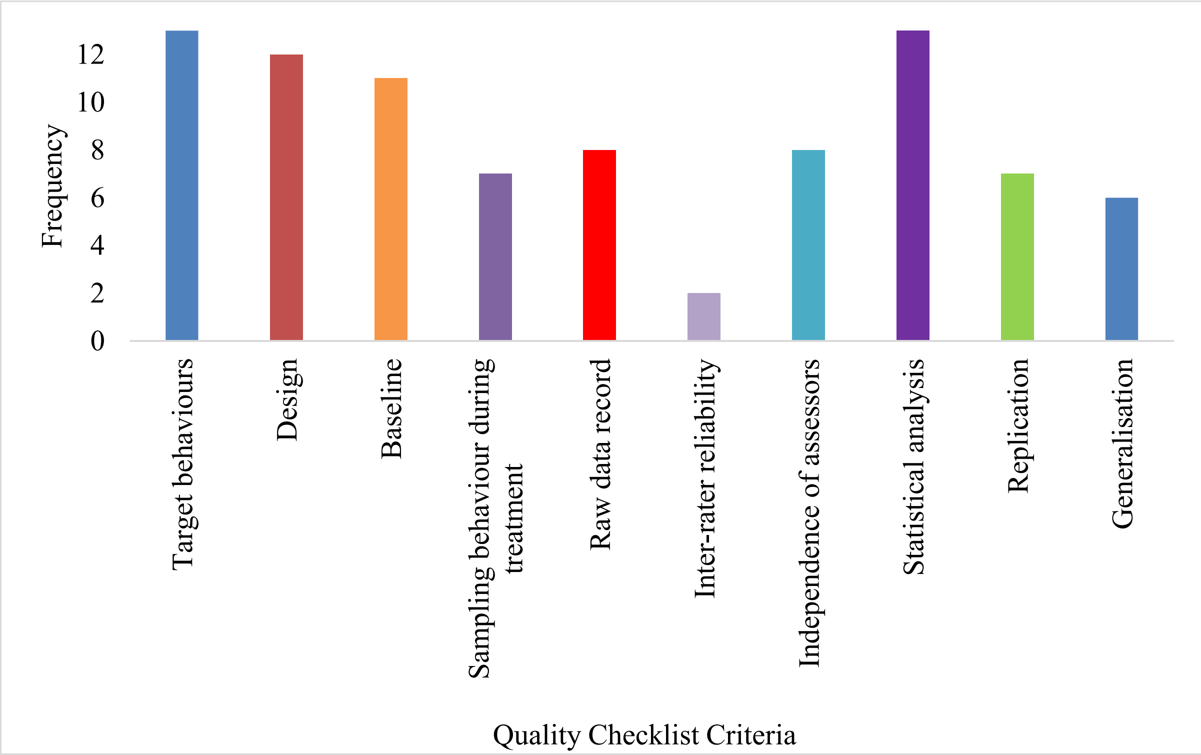
Summary of Modified Methodological Quality Checklist: SCEDs (Tate et al., 2008) Across Studies (N = 13)

	Target behaviours	Design	Baseline	Sampling behaviour during treatment	Raw data record	Inter-rater reliability	Independence of assessors	Statistical analysis	Replication	Generalisation	Total Score	Percentage of Total Score
Connor & Shaw	X	X							X	X	4	40.00
Dymowski et al.	X	X	X	X	X	X	X	X	X	X	10	100.00
Fritz & Basso	X	X	X		X						4	40.00
Gamito et al.	X	X	X					X			4	40.00
Gray & Robertson	X	X	X	X	X			X	X		7	70.00
Mohanty & Gupta.	X	X	X								3	30.00
Penkman & Mateer	X	X	X	X			X		X		6	60.00
Pero et al.	X	X	X	X	X			X	X		7	70.00
Ramanathan et al.	X	X	X	X	X			X		X	7	70.00
Wall et al.	X										1	10.00
Wilson et al.	X	X	X		X			X		X	6	60.00
Yoshida et al.	X	X	X	X	X			X	X	X	8	80.00
Zickefoose et al.	X	X	X	X	X	X		X	X	X	9	90.00
Total per item	13/13	12/13	11/13	7/13	8/13	2/13	2/13	8/13	7/13	6/13		

Note. X denotes a criterion being met.

Figure 4

Frequency of Criteria Met on Modified Methodological Quality Checklist: SCEDs (Tate et al., 2008) Across Studies (N = 13)



Data Extraction

Design characteristics for each of the 21 studies are summarised below (see Appendix H for further information including a detailed summary of each reviewed study).

Design Characteristics

As noted, 13 of the 21 studies utilised a single case study design. Notably, for two out of 13 studies, data for only one participant was extracted from their sample, as the remaining participant(s) did not meet the inclusion criteria of the current study. For example, in the study conducted by Connor and Shaw (2016), data was extracted for Patient B from the three-participant sample. Patient A had no severity of TBI reported, and Patient C had surgery due to a brain malformation and not a TBI, and therefore could not be included in this review. In Pero et al.’s (2006) study, data was extracted for patient MG, and patient LUG was not included in this review, as patient LUG was 17 years of age at the time of injury and therefore did not meet the 18-year cut-off age inclusion criteria for this review. Seven of the 13 single case studies had more than one participant, but still reported a single case study design (Connor & Shaw, 2016;

Dymowski et al., 2016; Gray & Robertson, 1989; Penkman & Mateer, 2004; Pero et al., 2006; Yoshida et al., 2014; Zickefoose et al., 2013). The remaining six single case studies only contained one participant within their design (Fritz & Basso, 2013; Gamito et al., 2011; Mohanty & Gupta, 2013; Ramanathan et al., 2019; Wall et al., 2013; Wilson & Manly, 2003). Last, 12 of the single case studies contained both pre- and post-intervention assessment. The only study that did not include a baseline was by Wall et al. (2013) in which assessment was first conducted 12 months into the rehabilitation programme.

Seven studies utilised a RCT or variant thereof in their study design. Five of these studies utilised both a within and between-subjects design (Couillet et al., 2010; Grealy et al., 1999; Keller, 2001; Niemann et al., 1990; Park et al., 1999). Of note, Keller (2001) aimed to compare TBI neurofeedback training (NFT) to a TBI control group who received computerised attentional training. As NFT does not fall within the aim of this review, being a medical intervention, data for the control group was extracted as to ascertain the efficacy of the computerised attention training. As such, the within-subjects results for the control group are extracted for this review. Of note, six of these studies utilised a TBI sample for both groups either by random assignment (Couillet et al., 2010; Keller, 2001; Niemann et al., 1990) or using TBI matched controls (Grealy et al., 1999). Similar to the observational study described above (Kim et al., 2009), one study utilised a healthy control group instead of a TBI control group (Park et al., 1999).

The remaining study utilised an observational design, at least in part (Kim et al., 2009). For the initial part of the study, a prospective fMRI study was conducted to compare TBI patients, who had undergone four weeks of cognitive training, with healthy controls, in order to characterise the reorganisation of neural attentional networks following cognitive training. As the fMRI findings are predominantly medical in nature and not related to neuropsychological outcomes as defined within the aims of this review, only data for pre-post neuropsychological attention-based outcomes for the TBI group was extracted.

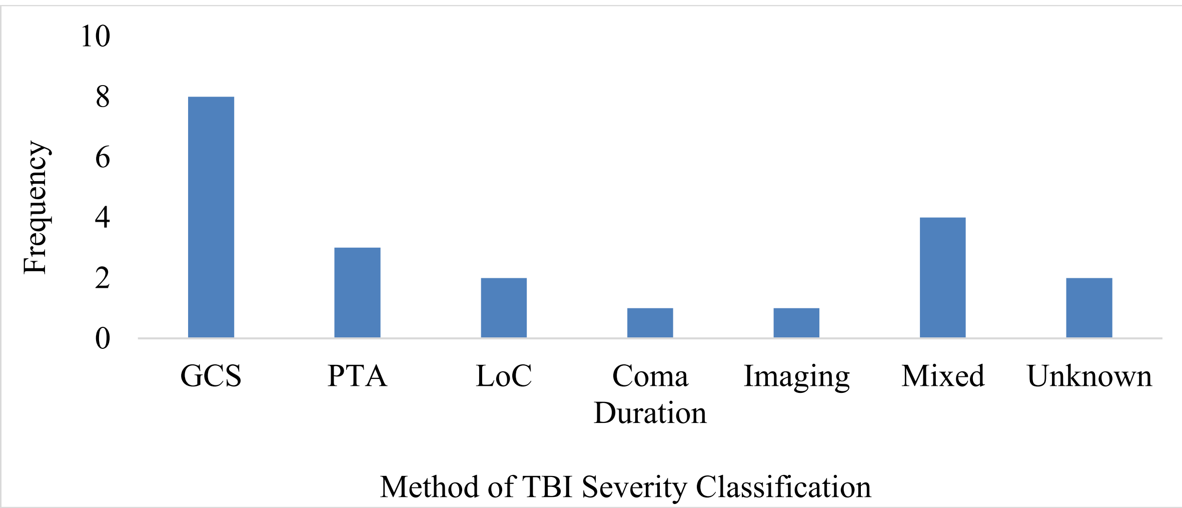
Study Sample Characteristics

Descriptive Statistics. A total sample of N = 238 individuals were initially identified as part of the final 21 studies included in this review. However, as mentioned above, only data relevant to the aim of this review was extracted, and as such, a total of N = 223 individuals were included in the analyses of this review. The average age across all studies was 33.16 years, with a wide range of ages from 18 to 64 years. Of all of these individuals, 185 sustained a moderate to severe TBI and the remaining 38 individuals were included as healthy matched controls.

TBI Severity Classification. Regarding the TBI participants, the method of determining injury severity included the GCS, duration of PTA, loss of consciousness, duration of coma, and neuroimaging (see Figure 5 below). In two of the studies, the GCS was used to determine severity; however, the values were not reported (Gamito et al., 2011; Kim et al., 2009). Furthermore, two studies reported severity as being severe but did not include a method in which this severity was determined (Connor & Shaw, 2016; Dvorkin et al., 2013). It could be argued that these studies should be excluded on this basis, however, given the paucity of literature on this topic, these studies are retained for the analyses.

Figure 5

Summary of Methods of TBI Severity Classification Across Studies (N=21)

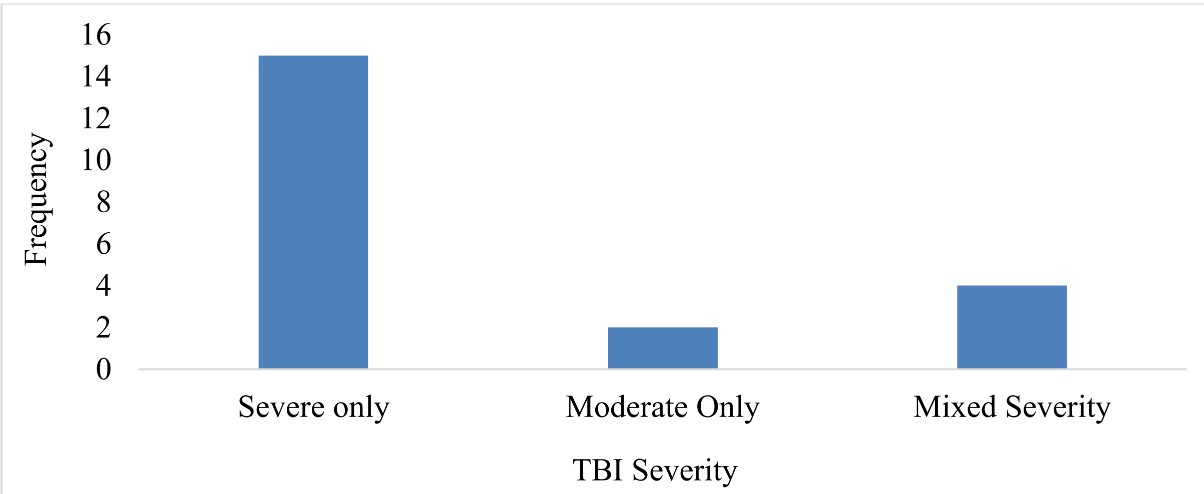


Note. GCS = Glasgow Coma Scale; PTA = post-traumatic amnesia; LoC = loss of consciousness.

TBI Severity. 15 studies contained a sample of participants with severe TBI only (Connor & Shaw, 2016; Couillet et al., 2010; Dvorkin et al., 2013; Dymowski et al., 2016; Fritz & Basso, 2013; Gamito et al., 2011; Gray & Robertson., 1989; Grealy et al., 1999; Mohanty & Gupta, 2013; Park et al., 1999; Pero et al., 2006; Ramanathan et al., 2019; Wall et al., 2013; Wilson & Manly, 2003; Zickefoose et al., 2013), 2 studies that included a sample with moderate TBI only (Kim et al., 2009; Yoshida et al., 2014), and 4 studies included samples of participants with a mix of moderate to severe TBI (Keller, 2001; Leśniak et al., 2020; Niemann et al., 1990; Penkman & Mateer, 2004).

Figure 6

Summary of TBI Severity Across Studies (N=21)



Time Since Injury. The time from when the injury occurred and the rehabilitation was administered was another important factor to determine across studies. The reason for this consideration was based on literature suggesting that spontaneous recovery is greatest within the first few first years following injury (Ruttan et al., 2008; Sandhaug et al., 2015). As such, treatment outcomes needed to be differentiated from spontaneous recovery to avoid the potential confounding effects on the reported efficacy. Across all 21 studies, the average time since injury was 1.8 years, with a range of 51 days to 35 years.

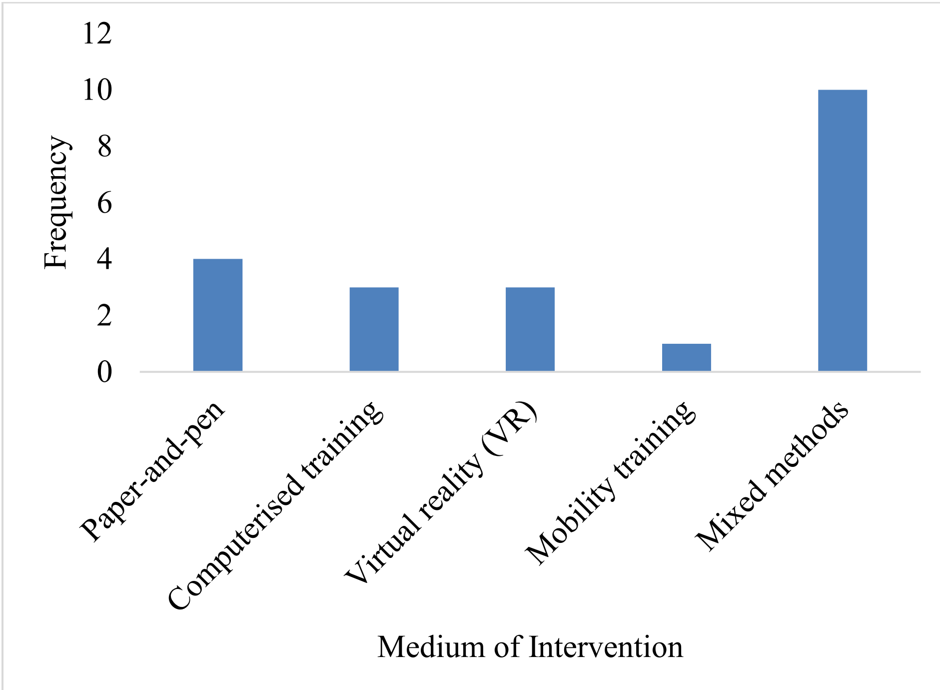
Intervention/Training Design

Below is a summary of the intervention/training used in each study (see Appendix I for further information including a detailed summary of each study's intervention/training programme). The first aspect reviewed was the medium of administration of the interventions used across all studies (see Figure 7). Many studies utilised a computerised attentional training programme within their intervention. Six studies used computerised training only (Keller, 2001; Kim et al., 2009; Zickefoose et al., 2013), and the remaining three used virtual reality (VR) as a training method (Dvorkin et al., 2013; Gamito et al., 2011; Grealy et al., 1999). Of note, the study conducted by Keller (2001) utilised NFT as their main experimental condition, whereas the control group engaged in computerised attentional training served. However, in line with aims of this review, the computerised training group data was extracted as it was reported on separately as neurofeedback training does not fall within the definition of neuropsychological intervention as outlined in the aim of this review. In the studies that utilised computerised interventions in some form, commonly used software programmes used to deliver training included Attention Process Training (APT), Lumosity, ComCog, RehaCom and CogniPlus. The most commonly used of these was APT-III, utilised by three studies (Dymowski et al., Ramanathan et al., 2019; Zickefoose et al., 2013). Two studies used Lumosity training, although both times this was used in conjunction with other training (Connor & Shaw, 2016; Zickefoose et al., 2013). Two studies incorporated physical activity as part of their training regime: Fritz and Basso (2013) utilised mobility training, and Grealy et al. (1999) utilised a cycle ergometer during VR training.

The most popular method of intervention across studies was a combination of computerised training and pen-and-pencil/physical training methods to ameliorate deficits (Connor & Shaw, 2016; Dymowski et al., 2016; Leśniak et al., 2020; Mohanty & Gupta, 2013; Ramanathan et al., 2019; Wall et al., 2013; Wilson & Manly, 2003; Yoshida et al., 2014). For example, Connor and Shaw (2016) utilised metacognitive training, group discussions and a computerised cognitive training programme (Lumosity). Zickefoose et al. (2013) utilised a two-group crossover design with two separate computerised training schemes, namely APT-III and Lumosity. Both Couillet et al. (2010) and Penkman and Mateer (2004) utilised a two-group crossover design (AB x BA) with the alternate non-attentional training consisting of a control condition. Niemann et al. (1990) compared attention training to memory training but did not utilise a crossover design, but rather a between-group comparison where participants only took part in one intervention method. Yoshida et al. (2014) utilised a between group comparison, of differing control conditions (control versus occupational therapy) but both groups participated in the attentional flow inducing training.

Figure 7

Summary of Differing Mediums of Interventions Utilised Across Studies (N=21)



Several studies targeted multiple cognitive domains within their intervention. In accordance with the aim of this review, only the relevant data pertaining to attentional training and attentional outcomes were extracted. In addition to attention, domains targeted across studies included memory, working memory, executive functioning, cognitive-mobility functioning, information processing, learning, reaction and movement speed, verbal memory, expressive speech, verbal fluency, reading and spatial neglect. Wall et al. (2013) consisted of holistic training including mood, cognition, and daily functioning. Of note, Niemann et al. (1990) included a control group who received memory training as a control measure, which was then compared to the attentional experimental training group.

Timing of Assessments

The timing of assessments is essential in establishing the efficacy of outcome changes over time, thereby informing the validity of treatment effects on attention (see Appendix I for a more detailed summary of the timing of assessment outcomes). Six of the 21 studies utilised a simple pre-intervention versus post-intervention comparison (Connor & Shaw, 2016; Grealy et al., 1999; Keller, 2001; Kim et al., 2009; Park et al., 1999; Wilson & Manly, 2003). Of note, two studies made use of a baseline assessment followed by a pre-intervention assessment to establish a reliable pre-treatment baseline and control for any spontaneous recovery effects (Fritz & Basso, 2013; Leśniak et al., 2020). For some studies that used a randomised cross-over design, a pre-intervention assessment was completed, along with post-intervention assessments after both intervention phases (Couillet et al., 2010; Yoshida et al., 2014; Zickefoose et al., 2013). Three studies utilised continuous assessment for all attentional measures (Dvorkin et al., 2013; Penkman & Mateer, 2004; Ramanathan et al., 2019). Only one study had a pre-post intervention comparison (session one and session nine respectively), as well as an intermediate assessment during session five (Gamito et al., 2011).

Several studies made use of a combination approach, with some measures being continuously assessed, and others being pre-post intervention comparison scores (Dymowski et al., 2016; Gray & Robertson., 1989; Niemann et al., 1990; Pero et al., 2006; Zickefoose et al., 2013). Notably, three of the studies already mentioned made use of a follow-up assessment in conjunction with the measures mentioned above to establish long-term validity of outcomes (Couillet et al., 2010; Dymowski et al., 2016; Leśniak et al., 2020). Mohanty and Gupta (2013) conducted a one-and-a-half-month pre-intervention assessment and then a post-intervention assessment at nine months post-intervention completion. Wall et al. (2013) only assessed attentional measures at 12 months post holistic rehabilitation completion and then again at 24 months post-rehabilitation (six months post-completion of relationship and consolidation-based therapy). Notably there was a pre-intervention assessment, however, this did not include attentional measures.

Measures of Attention Utilised

A variety of cognitive measures used to assess attention can be seen across all studies (see Figure 8 below; see Appendix J for a detailed summary of attentional measures used by each study). The most commonly used measure was the Test of Everyday Attention (TEA; Robertson et al., 1994, 1996) used by six of the studies (Dymowski et al., 2016; Penkman & Mateer, 2004; Pero et al., 2006; Wall et al., 2013; Wilson & Manly, 2003; Zickefoose et al., 2013). The TEA (Robertson et al., 1996) is a broad-based measure of several attentional domains (sustained, selective and attentional switching) using everyday materials to establish the generalisation of attentional gains in an everyday setting. Another measure used to try to establish ecological validity of attentional gains is The Rating Scale of Attentional Behavior (RSAB; Ponsford &

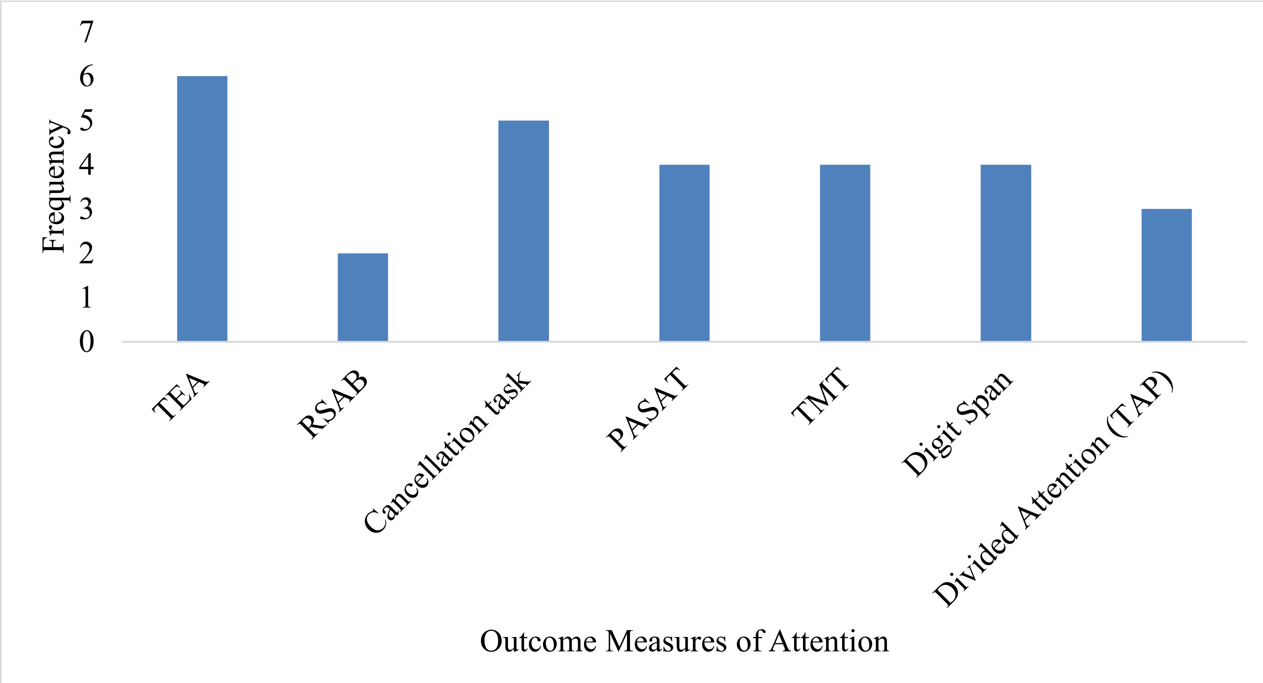
Kinsella, 1991). Two studies utilised the RSAB (Couillet et al., 2010; Dymowski et al., 2016), which aims to assess divided attention in everyday life.

Objective pre-post attentional measures seem to be the most common across studies. The second most commonly used measure of attention was some form of cancellation task, used by five of the studies (Connor & Shaw, 2016; Dymowski et al., 2016; Keller, 2001; Niemann et al., 1990; Wilson & Manly, 2003). The Paced Auditory Serial Addition Task (PASAT; Gronwall, 1977) was used by four studies (Gamito et al., 2011; Leśniak et al., 2020; Niemann et al., 1990; Park et al., 1999). The PASAT comprises of multiple subtests mainly aimed at assessing sustained and selective attention. The Trail Making Test (TMT; Reitan, 1958) was also used in four studies (Couillet et al., 2010; Fritz & Basso, 2013; Niemann et al., 1990; Yoshida et al., 2014) aimed as assessing sustained attention in Trail Making A and divided attention in Trail Making B. Some form of digit span assessment was similarly utilised in four studies (Couillet et al., 2010; Gray & Robertson, 1989; Grealy et al., 1999; Mohanty & Gupta, 2013) to assess short term memory. The forward span assesses attention capacity and backwards span assesses working memory, which is heavily reliant on basic attentional capacity (Hale et al., 2002). Three studies utilised subtests from The Divided Attention subtest of the Test for Attentional Performance (TAP; Zimmermann & Fimm, 1995, 2002) which primarily assessed divided attention (Couillet et al., 2010; Keller, 2001; Pero et al., 2006).

Of note, there are several studies that assessed attention through the intervention itself. For example, three studies used APT (Dymowski et al., 2016; Ramanathan et al., 2019; Zickefoose et al., 2013) and two studies used Lumosity brain training (Connor & Shaw., 2016; Zickefoose et al., 2013) – both of which have inbuilt measures of training tasks that target attention within them. For example, Connor and Shaw (2016) used Brain Performance Test from the Lumosity programme, which is a composite score across tasks to establish cognitive functioning across targeted domains. Yoshida et al. (2014) utilised video game tasks within the intervention, which could measure various aspects of attention based on the performance within each task. Physical measurements of responding to stimuli was utilised to measure attention in both Dvorkin et al. (2013) and Kim et al. (2009).

Figure 8

Summary of Differing Measures of Attention Utilised Across Studies (N=21)



Note. Some studies utilised several methods of assessing attention. TEA = Test of Everyday Attention; RSAB = Rating Scale of Attentional Behavior; PASAT = Paced Auditory Serial Addition Task; TAP = Test for Attentional Performance.

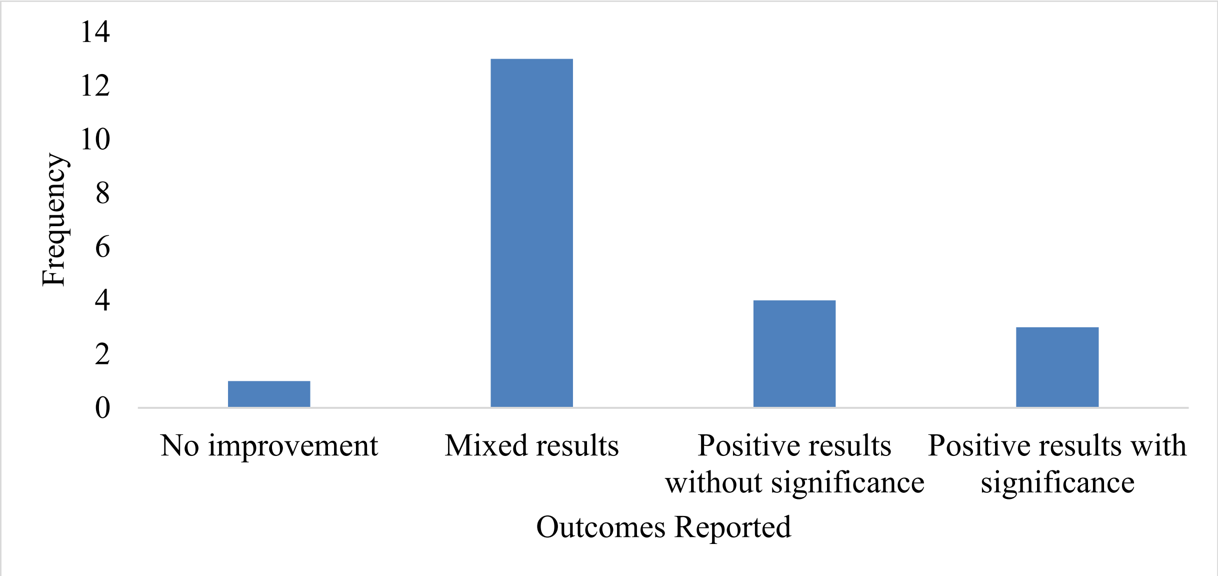
Other measures of attention included the Stroop test (measuring executive control of attention by the use of inhibitive cognitive processes; Stroop, 1935), Walking While Talking Test (measuring cognitive-mobility dual-task functioning; Verghese et al., 2002), Choice Reaction Task (measuring reaction speed; Bukasa & Wenninger, 1986), Digit Vigilance Test (measuring sustained attention; Lewis & Rennick, 1979), Digit Symbol Substitution Test (measuring processing speed; Wechsler, 1955), Moss Attention Rating Scale (observational rating of attention-related behaviour; Whyte et al., 2003), and Continuous Performance Test "X" Task (measuring sustained attention; Rosvold et al., 1956).

Outcomes Reported

Outcomes reported across all 21 studies is summarised below in order to determine the efficacy of the intervention (see figure 9 below). Please see Appendix J which includes a summary of reported results and interpretation of results by each study in more detail.

Figure 9

Summary of Attentional Outcomes Across Studies (N = 21)



No Improvement Reported. All outcomes reported here were related to the cognitive function of attention only, and as such, data was extracted for each study relating to the assessment of attention. Notably, there was only one study that reported no improvement in attentional measures across the intervention (Mohanty & Gupta, 2013). Both the digit span test and the attention and concentration subtest from the PGI Memory Scale had no significant change from pre- to post-intervention. The Digit Vigilance Test (DVT) improved slightly, however scores remained in the severe impairment range. Notably, this study scored particularly low in the quality appraisal above, and therefore these results are interpreted cautiously.

Mixed Results Regarding Improvement. The majority of studies reviewed had mixed results regarding attentional improvement post-intervention (Couillet et al., 2010; Dymowski et al., 2016; Grealy et al., 1999; Keller, 2001; Niemann et al., 1990; Park et al., 1999; Penkman & Mateer, 2004; Pero et al., 2006; Ramanathan et al., 2019; Wall et al., 2013; Wilson & Manly, 2003; Yoshida et al., 2014; Zickefoose et al., 2013). Couillet et al. (2010) found significant improvements in divided attention, but other attentional measures did not improve significantly on standardised testing. In support of such findings, the same outcomes were seen on behavioural measures, where divided attention improved, and other attentional measures did not. Dymowski et al. (2016) saw significant improvement in APT tasks, and three subtests of the TEA.

Performance on cancellation tasks varied, and scores on the remaining four subtests of the TEA decreased indicating poorer performance. A ceiling effect was demonstrated across participants on the Elevator Counting subtest of the TEA. Notably, RSAB ratings (measuring the impact of attentional processes on the activities of daily living) for the patients themselves did not improve, whilst significant other ratings did improve. Grealy et al. (1999) reported that there was significant improvement across all three attentional measures, and significant improvement was seen in the digit symbol task measuring processing speed. However, there was no significant improvement seen in both Trails tasks.

Keller (2001) also reported mixed results. For example, the cancellation and choice reaction task had significant improvement in one of the subcomponents assessed (i.e., number of targets acquired and time to complete respectively), but number of errors did not show significant improvement, nor did the sustained attention task of the TAP. Niemann et al. (1990) found that the attention group had significantly better scores than the memory group on the Attention d2 and TMT-B measures. The remaining tests did not significantly improve, and the Selected test of the San Diego Neuropsychological Test Battery (SDNTB) demonstrated the treatment effects that did not generalise. Park et al. (1999) demonstrated that while the TBI group did improve on the PASAT, similar findings were seen with the control group. Notably, no between-group comparisons were made. Penkman and Mateer (2004) demonstrated promising results where the APT and computerised task scores improved for the experimental group, and no improvement was seen in the active control condition. However, significance was not reported. Further, some improvement was demonstrated on the TEA, but a ceiling was reached early on in several subtests and therefore further improvement could not be assessed.

Pero et al. (2006) demonstrated improved vigilance, selective attention and divided attention which improved beyond a pathological range. Five out of seven measures on the TEA improved, placing the participant within 1 standard deviation of the control mean on those measures. The TAP test demonstrated improvement in some subtests whilst others, like the Alertness test, remained in the pathological range. Ramanathan et al. (2019) reported an improvement in divided attention but the same was not seen for alternating attention (which may be a function of a high pre-intervention result). Additionally, no significant values were reported, and therefore interpretations of such results are limited. Wall et al. (2013) demonstrated that visual attention was the only improvement seen relative to premorbid levels, with the remainder of the TEA subtests yielding no change. Wilson and Manly (2003) reported improved sustained attention by the reduced need for prompting in the self-care activities of daily living (ADL) programme. However, both the Behavioral Inattention Test and the TEA remained in the impaired range. Findings from Yoshida et al. (2014) demonstrated most improvement regarding the Moss Attention Rating Scale (MARS), but other measures had variable results. Lastly, Zickefoose et al. (2013) demonstrated significant improvement in progressing through the intervention, but there was variation in change of scores for the probe measures and the TEA.

Positive Results Reported. Four of the studies reviewed reported positive results regarding attentional measures, however, due to the associated effect size values reported, these findings must be interpreted with caution (Connor & Shaw, 2016; Dvorkin et al., 2013; Fritz & Basso, 2013; Gray & Robertson, 1989). Connor and Shaw (2016) reported modest improvement seen in attentional measures (Pair Cancellation subtest and Brain Performance Test from the Lumosity programme). However, qualitative analysis demonstrated positive changes in daily living but none of these were related to attention specifically. Dvorkin et al. (2013) reported improvements in both spatial and temporal kinematic parameters, with an increase in the number of targets, and the reduced need for haptic prompts as the study progressed. Fritz and Basso (2013) had positive findings with improvements seen for both Walking While Talking Test (WWTT) and the TMT, with fewer cues, fewer errors, and faster completion times. Lastly, Gray and Robertson (1989) showed overall improvement for all three cases, although only one subtest for case 2 and two subtests from case 3 improved by more than 1 standard deviation.

The remaining three studies reviewed had mostly positive and statistically significant improvements reported (Gamito et al., 2011; Kim et al., 2009; Leśniak et al., 2020). Gamito et al. (2011) reported a significant increase in the percentage of correct responses across intervention trials in the PASAT. Kim et al. (2009) reported that the TBI group had significantly lower accuracy scores and lower response times compared to the controls. Both of these measures significantly improved for the TBI group post-intervention. However, it is worth noting that no between-group comparison was made post-intervention as the control group did not complete the measures post-intervention. Lastly, Leśniak et al. (2020) demonstrated significant improvement in the PASAT and Rapid Visual Information Processing scores from pre- to post-intervention.

Ecological Validity: Generalisation to Daily Lives. There were eight studies that utilised some form of measure aimed at assessing the generalisability of attentional gains in daily life. Measures included the TEA, RSAB and MARS. Three of the studies reported improvements post-intervention (Couillet et al., 2010; Pero et al., 2006; Yoshida et al., 2014) compared to one study which reported no changes following the intervention (Wilson & Manly, 2003). The remaining four studies had mixed results with some improvements seen, some decline seen, and also no real-world changes reported following the intervention (Dymowski et al., 2016; Penkman & Mateer, 2004; Wall et al., 2013; Zickefoose et al., 2013).

Follow-Up. Only three of the studies included utilised a follow-up assessment in addition to post-intervention assessments (Couillet et al., 2010; Dymowski et al., 2016; Leśniak et al., 2020). Notably, all three of these studies reported significant improvement on attentional measures which were maintained at follow-up, suggesting a stable performance post-intervention completion. However, it is of note that two of these studies had short-term follow-up durations of 1 month and 3 weeks (Couillet et al., 2010; Dymowski et al., 2016 respectively). Leśniak et al. (2020) had a longer follow up duration of 4 months. Mohanty and Gupta (2013) did not conduct a post-intervention assessment apart from the 9-month follow-up which demonstrated little to no attentional gains. Similar findings were seen by Wall et al. (2013) with 24-month post-rehabilitation outcome measures remaining in the impaired range.

Limitations Reported by Studies

A summary of the limitations reported by each of the 21 studies is described below (see Appendix K for a more detailed summary of reported limitation and recommendations across studies). The most common limitation, reported by nine studies, was the inability to determine if the intervention outcomes would generalise to the patients' daily lives following the completion of the intervention period (Couillet et al., 2010; Keller, 2001; Mohanty & Gupta, 2013; Niemann et al., 1990; Park et al., 1999; Penkman & Mateer, 2004; Ramanathan et al., 2019; Wall et al., Yoshida et al., 2014; Zickefoose et al., 2013). This was not only due to limitations of the study design itself, but also the lack of ecologically valid measures that could accurately ascertain attentional gains in tasks of daily living.

Along similar lines was the question of whether functional gains were sustained in the long-term post-intervention, as reported by six studies (Dvorkin et al., 2013; Grealy et al., 1999; Kim et al., 2009; Mohanty & Gupta, 2013; Penkman & Mateer, 2004; Ramanathan et al., 2019). In the cases where outcome measures did not improve as expected, limitations have been reported regarding the ecological validity of assessment measures, suggesting that gains made through the intervention may be seen in everyday life, but may not be reflected on formal assessment (Niemann et al., 1990; Wall et al., 2013).

Another commonly reported limitation was a small sample size (Couillet et al., 2010; Dymowski et al., 2016; Gamito et al., 2011; Leśniak et al., 2020; Park et al., 1999; Penkman & Mateer, 2004; Pero et al., 2006; Ramanathan et al., 2019; Yoshida et al., 2014), which was not surprising given the number of single case studies that comprise the sample. A small sample size can have several implications for a study's findings, the most obvious of which is the lack of generalisability. Four studies reported being unable to determine if their findings could be generalised to a wider population, even those within a moderate to severe TBI sample (Connor & Shaw, 2016; Couillet et al., 2010; Pero et al., 2006; Ramanathan et al., 2019).

Several studies report that the use of multiple rehabilitation strategies in conjunction with the desired intervention make it difficult to infer the contributions of each to the outcomes seen, and similarly, which domains are targeted by which aspect of the intervention (Dymowski et al., 2016; Fritz & Basso, 2013; Leśniak et al., 2020; Penkman & Mateer, 2004; Ramanathan et al., 2019; Yoshida et al., 2014).

The efficacy of the intervention itself having a direct influence on outcome measures was a question of concern. For example, several studies reported on the influence of external factors on outcomes measures, including but not limited to, familial support and psychosocial support (Dymowski et al., 2016; Kim et al., 2009; Leśniak et al., 2020; Penkman & Mateer, 2004; Wall et al., 2013; Zickefoose et al., 2013). For example, both Fritz and Basso (2013) and Kim et al. (2009) comment on the effect that medication may have had on attentional outcome measures. Along the same lines, three studies reported the possibility of spontaneous recovery as a contributing factor to the improvement seen on outcome measures (Keller, 2001; Mohanty & Gupta, 2013; Wall et al., 2013). Other studies questioned the internal validity of the intervention and if attentional cognitive processes themselves were changed during the intervention (Grealy et al., 1999; Ramanathan et al., 2019). One possible explanation was that outcome measures had improved due to repetition throughout the intervention (i.e., through practice effects). Considerations around practice effects are important to consider regarding outcome efficacy, as reported in two studies (Park et al., 1999; Yoshida et al., 2014).

A further limitation reported was the brevity of training which may not be long enough to efficiently ameliorate attention deficits (Leśniak et al., 2020; Park et al., 1999; Penkman &

Mateer, 2004; Zickefoose et al., 2013). Of note, the study conducted by Wilson and Manly (2003) did not mention any limitations in their writeup.

Discussion

This systematic review aimed to evaluate studies on attentional training in adults following moderate to severe TBI. The objective was to summarise the extent and efficacy of such training programmes, thereby informing the scope of best practice within the field of neuropsychological rehabilitation strategies. A total of 21 studies were included in this review and evaluated accordingly. Below is a discussion of the included studies with regards to 1) the methodological quality of these studies (which informs the reliability and validity of their findings), 2) the factors that may have impacted the intervention outcomes (such as design, age, severity of injury and time since injury), and consequently, 3) the attentional outcomes are reviewed to assess the efficacy of such training programmes. The limitations of each study included in this review will be examined in conjunction with the implications that they have on the reported outcomes. Lastly, the limitations of this review will be discussed.

Methodological Quality

The validity of the conclusions that can be drawn from a systematic review are highly dependent on the quality of the articles that are included for review (Negarandeh & Beykmirza, 2020). Studies that contain issues with methodological quality, typically result in biases within the reported results (Higgins et al., 2019). The reported results and findings from studies with poor methodological quality need to be interpreted with caution. As such, it speaks to the need to review the quality of each study included in this review, to assess the confidence with which their findings can be accurately interpreted and analysed.

RCT and Observational Design Methodological Quality Analysis

Overall, the quality of the studies utilising an RCT or observational design fared relatively well, particularly within the descriptive domain, which assessed the presence of several descriptive components within particular study designs. However, there were several items concerning internal validity that were not upheld upon methodological quality review across the studies. The most lacking internal validity criteria included a detailed randomisation procedure, concealment of treatment allocation, the presence of co-interventions, and lastly, the blinding of the outcome measures.

Detailed Randomisation and Concealment of Treatment Allocations. Several studies utilised a randomisation procedure but did not describe this in sufficient detail to be awarded credit on the quality scale utilised in this review. The need for randomisation is to increase the likelihood of subject variables being equally distributed across groups (Kim & Shin, 2014). Along similar lines, the need for treatment allocation to be concealed is particularly important to reduce selection bias (Aggarwal & Ranganathan, 2019). This is distinct from blinding which occurs after randomisation has occurred – which is not always possible given the specific design (Altman & Schulz, 2001; Moustgaard et al., 2020). While there are many methods of randomisation to ensure treatment allocation is concealed, many of these methods are expensive and require a methodologist to implement, which can be difficult to achieve in the context of limited funding (Doig & Simpson, 2005).

Given the number of methods possible for randomisation, a detailed randomisation method should be described to further demonstrate consistent internal validity. This is particularly important in the context of TBI samples, as there are many individual factors which can influence recovery, like age, severity, time since injury as discussed below (Svingos et al., 2019; Walker et al., 2018). Ideally, these factors are controlled for to some degree by exclusionary methods, but it is not always possible for many reasons (e.g., access to clinical samples, the high variability, etc.). A recent systematic review considered factors such as age, sex, time since injury and other injury-specific factors and their influence on cognitive outcomes (Mollayeva et al., 2017). The review findings were inconsistent, and the prognostication of cognitive outcome appear to be dependent on intra-variability for each person. As such, it is evident that randomisation and the concealment of treatment are indeed important in studies utilising a TBI sample in an attempt to distribute such subject variables across groups.

Presence of Cointerventions. Another important consideration when determining the efficacy of a rehabilitation programme is the presence of co-interventions. Cointerventions are defined as any form of additional advice, intervention or treatment that the participant may be receiving at the same time as the target intervention (Aggarwal & Ranganathan, 2019). The methodological concern around the presence of cointerventions is that these interventions act as confounding variables that may then influence or obscure the changes in the outcome variables being measured. As such, the validity of the conclusions drawn about the target intervention efficacy may inaccurately be attributed to the target intervention or missed (Aggarwal &

Ranganathan, 2019; Moutzouri et al., 2020). For example, in the context of this review, the validity of positive treatment outcomes on attention may be not due the intervention itself, but rather if the participant was on medication or partaking in another form of intervention. It may be these alternate interventions had a greater influence in terms of the positive outcomes seen, and therefore valid and reliable conclusions cannot accurately be drawn from the target intervention.

Notably, cointerventions cannot always be avoided, for example, in the context of a patient receiving multidisciplinary treatment within a rehabilitation hospital. In this case, it is essential that cointerventions are reported upon to ensure that outcomes are interpreted with caution regarding the influence of the cointervention on the outcome variables in question (Aggarwal & Ranganathan, 2019). Across the studies reviewed, there was no evidence of cointerventions being reported upon. Given the need for multiple interdisciplinary therapy in remediating the far-reaching consequences that follow TBI (Gómez-de-Regil et al., 2019), it is understandable and expected that participants in the recovery stages following TBI will be involved in multiple forms of therapy. As such, the need for reporting on the presence of co-interventions is great, and as demonstrated within this review, is lacking as basic practice.

Blinded Outcome Measures. The last criterion that fell short across RCT and observational studies to improve internal validity, was the paucity with which outcome measures were blinded. A recent review conducted suggested that the blinding of outcome measures is poorly reported upon and rarely utilised in published trials (Kahan et al., 2015), thereby suggesting a broader issue that is not limited to rehabilitation literature nor the type of study utilised. The importance of assessor blinding lies within potentially biased conclusions regarding treatment effects (Aggarwal & Ranganathan, 2019; Kahan et al., 2015).

In the context of blinding being impossible, research suggests using objective and standardised outcome variables that require little in terms of subjective interpretation (Karanicolas et al., 2010) and many of the studies included in this review did indeed utilise what is considered to be objective measures of attentional functioning. However, without the application of a double-blind procedure, even if the scoring is ‘objective’, there is still the presence of experimenter expectancy effects. Expectancy effects refers to the phenomenon in which participants behave in a manner congruent with the group they are allocated to, based on their expectation of the study aims and objectives which may bias the outcomes towards a favourable result (Hawryluk & Bullock, 2015). This leads to potential confounds which cannot be removed purely by choosing outcome measures that can be scored objectively. Furthermore, many of the outcomes used across studies do indeed include some form of subjective interpretation, and there is always the risk of experimenter bias present when administering and scoring such measures (Fernández & Abe, 2018).

SCED Methodological Quality Analysis

Overall, the methodological quality of the single case studies included in this review fared well. For example, items like describing the design in enough detail, including baseline assessments, and having a record of the raw data and statistical analyses – were consistently met by the reviewed studies across the board. However, there were some criteria which were lacking across the board thereby reducing the methodological quality of the studies.

Presence of Multiple Assessors. One such criterion was the presence of utilising multiple assessors in the study design, a criterion which was lacking in almost all the studies reviewed. While one may not consider this relevant for objective and standardised outcome measures, there still exists a certain level of clinical interpretation required when interpreting the results of neuropsychological assessment scores (Fernández & Abe, 2018). Given the detailed nature of single case studies in particular, where subjective interpretation is often utilised, the need to include multiple assessors in the study design is even more pronounced in an effort to improve the consistency of results of the outcome measures utilised in each study. The use of multiple raters thereby allows for more accurate and reliable findings (Howard, 2016). Therefore, it is understandable that it would be considered best practice to utilise multiple raters, even on ‘objective’ outcome measures (McHugh, 2012).

Inter-Rater Reliability. By not including multiple assessors within the study design, one cannot determine inter-rater reliability – which was lacking across many of the studies reviewed. Due to high clinical variability in the subjective interpretation of neuropsychological assessments, it is best practice to establish the degree to which all reviewers agree regarding the scores of outcome measures. This will strengthen the confidence in which results reported upon are consistent and are as ‘objectively’ measured as possible (McHugh, 2012). Further, it reduces the risk of human error when scoring outcome measures.

Generalisability of Findings to Daily Functioning. A further consideration that is reiterated in single case methodological quality is the extent to which findings are translate or extend to changes in everyday functioning (i.e., the ecological validity of the intervention). An intervention may aim to remediate specific functions, like attention, but the success of the intervention arguably rests upon whether positive outcomes can be translated to and improve upon daily functioning (Dawson & Marcotte, 2017). While this criterion was only assessed in the SCED-based checklist, I believe that this should apply to all types of designs including RCTs and observational designs. The concept of measuring generalisation and the findings for this review are discussed in more detail below. However, relating purely to the design of the study it seems fitting to include a measure of how the treatment effects observed are reflected within the participants' activities of daily living. While some of the studies included in this review attempted to include such measures (e.g., TEA; RSAB), there are many studies that did not include these measures. Literature suggests that standardised assessment measures, while useful for gathering information regarding patients' cognitive changes, may not reflect their real-life functioning (Wilson, 2017), and thus may not reflect if any daily functional gains have been achieved.

Factors Influencing the Validity and Reliability of Findings

Results of this systematic review provide encouraging evidence that gains in attentional functioning can be made in those who have sustained moderate to severe TBI. However, there are several factors that need to be considered when evaluating the overall efficacy of attentional interventions that may indeed influence the validity and reliability of such findings. The first factor explored below relates to the study design and how this has implications for the reported outcomes. Then, subject variables such as age, severity of TBI and time since injury are evaluated and compared across studies. Furthermore, the specific attentional outcomes explored, the use of technology, and reporting of long-term outcomes at follow-up are discussed. Lastly, the limitations that each study reports, over and above the aforementioned factors are addressed. Such factors are highlighted in an effort to evaluate the weight of evidence when concluding the findings of this systematic review.

Study Design

More than half of the studies included in this review utilised a SCED. Single case designs have several limitations, the most commonly reported being low external validity with findings not necessarily generalisable to a broader population (Graham et al., 2012; Lobo et al., 2017). As such, it is recommended that single case designs should be used in rehabilitation to establish proof of concept that can then later be used within larger clinical trials (Hart & Bagiella, 2012).

That said, there is growing resurgence in the use of SCEDs within research (Tate et al., 2013), and there is growing literature that suggests that such designs are much more useful than previously thought, particularly regarding the efficacy of rehabilitation research (Leelarungrayub et al., 2020; Sexton-Radek, 2014). For example, SCEDs are considered Level 1 evidence regarding the treatment decisions for individuals who require treatment or rehabilitation, even within the context of RCTs (Howick et al., 2011). This is particularly useful in the context of TBI rehabilitation as it allows for high quality and detailed research to be conducted within a small clinical sample. Given the relatively limited sample of moderate to severe injuries, where majority of TBIs are considered mild in classification, SCEDs can be useful in investigating this under-reported sample (Skandsen et al., 2019). Furthermore, the efficacy of a rehabilitative programme is often considered within the context of tailoring the programme to suit the individual needs of the patient (Wilson, 2017). As such, unique and tailored programmes that may not be effective as a one-size-fits-all approach, can be used as a valid research tool to inform evidence of best practice (Krasny-Pacini & Evans, 2018).

That said, there is a large variety in single case designs which may prove challenging to generalise such findings to a larger population outside of the study cohort. As such, the need to include specific aspects to be considered a true SCED is ever more important, as outlined by the SCED quality checklist utilised within this review. For example, having repeatable and valid outcome measures are essential when establishing an intervention’s efficacy (Krasny-Pacini & Evans, 2018). These target measures must not only be relevant to the target behaviour being measured, but must be specific and observable (Tate et al., 2016). Other important design characteristics include the inclusion of control measures, measures of generalisability, and the use of a cause-and-effect design with pre-post assessments in addition to multiple sampling of behaviours (Krasny-Pacini & Evans, 2018; Tate et al., 2008). As discussed above, these criteria were not sufficiently met in the studies included in this review, and as such, poses a challenge to determining the efficacy of such studies in improving attention. The limitation of using single case designs is not necessarily a problem if rigorous methodology is utilised. That said, given the aim of this review to establish if attentional training is efficacious in moderate to severe TBI samples, single case designs provide limited insights into the overall efficacy of such measures to a broader sample.

Age

Age is considered a prognostic factor regarding outcomes following TBI. A recent study reviewing the extent of disability and global neurological outcomes following severe TBI over a 10-year period suggest that there is frequently improved global functioning over time in younger adult TBI participants (Forslund et al., 2019). These findings are consistent with a large body of literature of similar findings (see e.g., Dhandapani et al., 2012; Fraser et al., 2019; Hukkelhoven et al., 2003; Walker et al., 2018), the outcomes of which can likely be attributed to the brain's decreased propensity for recovery with reduced functionality of neurons with age, in conjunction with the increased likelihood of subclinical insults in adults (Carlsson et al., 1968; Senathi-Raja et al., 2010). However, there is literature to suggest that age alone is not a significant predictor of recovery from TBI (Sobuwa et al., 2014). Ponsford et al. (2014) reported no notable differences in terms of functional outcome for the various age groups 10 years post-injury, suggesting that age may not be as significant of a predictor as discussed above.

Due to the conflicting evidence on the importance of age as a predictor of long-term recovery following TBI, more evidence on long-term follow-up post-intervention studies is required. Within the context of this review, the age of participants varied from young adults (aged 18) to mid-60's. Notably, positive attentional gains were seen across various age groups. For example, Connor and Shaw (2016) demonstrated positive improvements in attention in their single participant, aged 60. Therefore, age may not be as significant a predictor in TBI recovery as previously noted (Sobuwa et al., 2014). There may be several extenuating factors that may influence the notion that older adults are less likely to recover from injury. A recent study reported that elderly patients typically receive less intensive management of treatment during hospitalisation post-TBI compared to younger patients (Skaansar et al., 2020). As such, the urgency in which elderly patients are treated, and subsequently rehabilitated, post-injury may be a confound to the existing literature which states that as age increases, the propensity for recovery decreases. This is important to note because if the notion is that elderly patients are not as amenable to treatment, they may not be offered or receive the same level of rehabilitative care compared to a younger sample.

Severity

In accordance with the aim of this review, the severity of injury informed one of the predominant exclusion criteria. The reason for this criterion was because of the high prevalence of mild TBIs in the general population, and subsequently in the literature base (Skandsen et al., 2019). In terms of TBI rehabilitation literature, many study samples contain a mixed severity sample with a small proportion of those being moderate to severe injury. As such, a study may report successful rehabilitative techniques for the majority of their sample, without acknowledging the injury severity as an extraneous variable. In other words, successful study outcomes may not necessarily apply across all severity groups. Given the dose-response relationship between injury severity and outcome, with moderate-severe injuries associated with poorer outcomes (Svingos et al., 2019), such claims need to be challenged. As such, the initial step of diagnosing injury severity is essential towards establishing prognosis and which rehabilitative techniques would be most beneficial to the individual (Saatman et al., 2008; Tenovuo et al., 2021).

However, there is a growing debate suggesting that the initial assessment of injury severity may not necessarily successfully predict the prognostication of outcomes and extent of disability in TBIs – which can result in the under-treatment of patients who may have more severe injuries than initially assessed (Tenovuo et al., 2021). Criticisms include that the concept of severity is not only poorly defined, but is also subjective and influenced by more factors that are not included within the severity classification tools – suggesting that conventional severity ratings be replaced with a wide range of descriptors (Tenovuo et al., 2021). The GCS is considered the gold standard regarding injury severity classification (National Academies of Sciences, Engineering, and Medicine, 2019; Saika et al., 2015), as seen in this review with 10 out of 21 studies utilising the GCS as a severity classification tool. However, the inter-rater agreement of GCS has been documented as unreliable (Reith et al., 2016) with 55–74% of clinicians obtaining the same score on the same patient, and with 6–17% of cases being more than 2 points apart (Gill et al., 2004). Such findings are particularly concerning given that a single point difference can represent a change within severity classifications (e.g., mild (GCS score of 13) versus moderate (GCS score of 12)).

Other research has demonstrated that tools such as PTA are more reliable regarding prediction of functional independence post-discharge, even when controlling for extraneous variables like age, sex, cause of injury, surgical intervention, and previous substance use (Perrin et al., 2015). However, PTA is not without issue. A measurement of PTA primarily considers the period in which attention and memory functioning is disrupted, but does not take into account fluctuations seen in consciousness, psychomotor functioning and sleep-wake cycles – all of which are considered to be disrupted following injury and serve as prognostic factors (Sherer et al., 2008; Stuss et al., 1999). As such, the subjective nature of a seemingly objective measure of severity is brought into question, with questions raised regarding its clinical utility, particularly in cases where only one measure of severity is included. Such is seen within this review with only four studies utilising more than one method of injury severity classification (Pero et al., 2006; Wall et al., 2013). That is not to say that such methods do not have clinical utility, but rather that measures of severity should be determined from multiple methods of assessment including but not limited to, brain biomarkers, duration of hospital stay and the associated level of functioning upon discharge, independency as an independent outcome measure, quality of life and intensity of treatment needed (Tenovuo et al., 2021).

Time Since Injury

A longstanding theory in TBI literature is that the efficacy of treatment decreases as time since injury increases (Penkman & Mateer, 2004). However, in order to determine the efficacy of interventions, the notion spontaneous recovery occurring during treatment needs to be considered. For example, two studies included in this review utilised a sample that was in the acute stage of recovery, and therefore amenable to spontaneous recovery (Penkman & Mateer, 2004; Pero et al., 2006). There are differing opinions on the length of time following moderate to severe TBI in which spontaneous recovery is possible. In a study on a series of severe TBI patients, no spontaneous recovery was seen eight months post-injury (León Carrión et al., 2001). A consensus exists that most of the recovery that one would see following TBI occurs within the first 24 months post-injury (Ruttan et al., 2008; Sandhaug et al., 2015).

That said, there is such individual variability following this period as improvements have been reported up to five to 10 years post-injury in some cases (Fleminger & Ponsford, 2005). In a longitudinal study of moderate to severe TBI patients, findings demonstrated that there are many persisting deficits that are still present even 10 years after injury (Ponsford et al., 2014). The same study reported that deficits present at two years post-injury were present at 10 years post-injury, with frequency of deficits being related to injury severity. Such findings would suggest that apart from the period of spontaneous recovery, which may confound the validity of conclusions drawn on efficacy of treatment, that more improvement is typically seen in those with a shorter duration of time since injury. That said, results by Penkman and Mateer (2004) demonstrated improvement by all patients with a range in time since injury from 15 months to 12 years. Such evidence is suggestive that once natural recovery has reached its maximum, time since injury may not affect the acquisition of treatment effects.

While the average time since injury across all 21 studies was under 24 months, there was evidence from specific studies that time since injury was not a significant factor that determined the success of attentional training. For example, in Zickefoose et al. (2013), all participants demonstrated significant attentional gains irrespective of time since injury which ranged from 3 years to 35 years. Further, Ramanathan et al. (2019) saw divided attentional gains in a participant that was 7.5 years post-injury, and Penkman and Mateer (2004) included a participant that was 12 years post-injury who also demonstrated positive attentional gains post-intervention. Notably these studies included the longest duration of time since injury, and such findings are positive in suggesting that attentional gains, and therefore the efficacy of rehabilitation can be made many years post-injury. Although most spontaneous recovery and functional improvements are seen in the first two years post-injury, the notion that minimal gains can be made following 24-months post-injury therefore could be challenged based on the findings of this review. The implication thereof is that it could not only afford those individuals the opportunity for further improvement albeit years later, but also challenge the way in which state and medical aid funders assist long-term survivors of TBI.

Attentional Outcomes of Intervention

Across the studies reviewed, there was large variation in the types of measures utilised to assess attentional gains. Each of these measures have varying reliability and validity statistics, and therefore have different clinical utility. For example, some measures by design are more sensitive when measuring changes across time, and therefore would be more likely to detect statistically significant results. This would include objective measures like standardised neuropsychological assessments. However, other measures may be more relevant at detecting changes in daily functioning, such as self-report measures which are by nature more subjective in nature (Bogdanova et al., 2016). In this review, there was not only variation in the types of measures used (e.g., objective versus subjective measures), but also variation within these measures (i.e., differing neuropsychological measures used across studies). Each measure, although aiming to measure a similar construct (i.e., attention) does have subtle variations in the underlying cognitive processes that are being tapped into. Given the varied measures of attention utilised across studies, it is difficult to compare functional outcomes. In part, this may be related to the lack of consensus in literature on how attention can be defined, modelled, and assessed. As such, future research efforts should be directed towards developing more clearly defined methods of assessing attentional impairments, such as developing a composite score of various subdomains of attention that can be used consistently in neuropsychological practice (Loetscher et al., 2019).

A further consideration is placed upon the use of traditional objective neuropsychological assessments as tools used to measure accurate changes in attentional functioning. The use of such objective measures is considered as the gold standard in rehabilitative research, rather than relying purely on subjective ratings or clinical judgment, as evidenced by the requirement in both quality checklists utilised within this review. Such findings within this review would suggest that attentional gains are achievable through neuropsychological methods of remediation in adults with moderate to severe TBI irrespective of age, methods of injury severity classification, and time since injury. However, such standardised assessment tools have been criticised as not directly reflecting changes in real-life functioning (Wilson, 2017). If the purpose of a rehabilitative programme is to create and maintain functional changes in cognition that directly impacts the individual's daily life (Tenovuo et al., 2021), such standardised and *objective* measures are called into question. Therefore, the importance of utilising multiple methods of assessing changes in cognition should be emphasized – a theme that was lacking across the studies included in this review.

An additional, and more objective manner of determining changes in cognition following rehabilitation is to consider biological data by utilising neuroimaging techniques. For example, both Kim et al. (2009) and Ramanathan et al. (2019) utilised fMRI data in conjunction with neuropsychological assessments to assess functional changes. Kim et al. (2009) demonstrated that not only were there significant improvements in task performance, but also that increased activation was seen in the anterior cingulate cortex, cerebellum and precuneus, and decreased activation within the frontal cortex, suggestive of neuroplastic changes within attentional networks following rehabilitation. Similar findings were seen by Ramanathan et al. (2019) where behavioural functional gains in attention were further evidence by both structural and functional neuroplastic changes post-intervention. While these changes can be viewed as an objective way to measure the efficacy of attentional training, the question regarding how these impact on the participant's daily living is still not adequately answered.

While the use of traditional objective measures of cognitive functioning has a need and place within assessing the efficacy of intervention, the use of subjective measures is arguably just as important. Given the widespread and far-reaching consequences that TBI has for the individual, the need for multidimensional assessments is apparent (Maas et al., 2017; Sarajuuri et al., 2018). Despite the obvious need to include subjective ratings of improvement, less than half of the selected articles in this review included such measures. This demonstrates that although objective measures are standard practice, the use of subjective measures are not.

Use of Technology

There is growing interest in the use of computerised cognitive training programmes in ameliorating deficits following ABI (Bogdanova et al., 2016). Many studies have found improved cognitive outcomes following computerised training (see e.g., Brehmer et al., 2012; Nouchi et al., 2013). Of the six studies that utilised computerised attention training only in the current review, four reported overall significant improvement in attentional outcomes (Dvorkin et al., 2013; Gamito et al., 2011; Kim et al., 2009; Zickefoose et al., 2013). Keller (2001) demonstrated improvement in attentional functioning following a hierarchical based computerised selective attention training intervention (using COGPACK software). However, in this study, the computerised attentional training group acted as a control group as compared to neurofeedback-therapy (NFT) as the target intervention. Although their findings demonstrated improved attention in the computerised control group, the NFT group improved significantly more than the computerised attentional training group. Of the computerised training interventions, only one study reported no improvement in attention following a non-immersive computer-based VR intervention (Grealy et al., 1999). Such results are promising on the clinical utility and efficacy of computerised attentional training post moderate to severe TBI.

The growing use of technology within rehabilitation literature is ever increasing. The utilisation of technology in rehabilitation has many advantages, including increasing the accessibility of training, increasing engagement within the intervention, and improving the ecological validity of training programmes in some cases (Wade et al., 2018). Depending on the design, participants may perceive the tasks as games which draws interest and acts as external motivator to sustain their attention (Dvorkin et al., 2013). However, previous research has highlighted the importance of familiarity with technology as a factor which may impact on the efficacy of computerised attentional training. For example, certain age groups may be more familiar with technological training programmes, and therefore the rate of engagement and subsequent success of training may vary based on the sample population (Bogdanova et al., 2016).

Computerised cognitive training is not without controversy, with varied clinical views on its efficacy across clinical groups. The gold standard of determining such efficacy is by utilising neuropsychological tests to demonstrate near and far transfer effects (i.e., seeing improved cognition on nontrained tasks and cognitively demanding functional tasks respectively; Harvey et al., 2018). However, there is again debate as to the clinical utility of such computerised intervention methods. Training in a particular cognitive domain and having improvements reflected in neuropsychological outcome measures assessing the same domain does not necessarily reflect real transfer. Rather, there may be limited training benefits demonstrated through intervention outcomes instead of general cognitive improvement (Simons et al., 2016). That said, in agreement with Harvey et al. (2018), such logic dismisses potentially substantial improvements in functioning which may hold real-world applicability for patients and their families. Two of the five computerised studies that reported improvements in attention also used neuropsychological assessment measures in conjunction with inbuilt training measures to assess improvement, and similar findings of improvement were seen. As such, while the findings of this review are in line with previous research (Bogdanova et al., 2016) suggesting that computerised training is efficacious, the clinical utility of computerised rehabilitative programmes indeed needs to be further explored across varying age and demographic groups.

Virtual Reality. Notably, three of the six technology-based studies mentioned made use of virtual reality as a training mechanism. Although Grealy et al. (1999) did not report attentional improvements, both Dvorkin et al. (2013) and Gamito et al. (2011) reported significant improvement in attentional outcomes. The customisable nature of using VR is particularly promising in terms of clinical utility. For example, Dvorkin et al. (2013) utilised a virtually minimal approach whereby participants see a dark background with only the target and cursor visible, thereby reducing any distractor variables which may not be possible within a typical clinical environment. Furthermore, by using haptic cues (i.e., a gentle pulse of force felt by the participant), participants are further engaged with visual, tactile, and proprioceptive cues which may further facilitate attentional cueing (Dvorkin et al., 2013). Similarly, in Grealy et al.'s (1999) study, participants were able to physically steer their way around a map. This increased tactile stimulation may have improved engagement with the task and intervention overall. Furthermore, the device utilised by Dvorkin et al. (2013) has clinical utility as it can safely administer the task to participants over a long period of time with minimal supervision required by the clinician.

Combination Approaches. Ten of the remaining studies included in this review utilised a combination of computerised and in-person training methods (Connor & Shaw, 2016; Couillet et al., 2010; Gray & Robertson, 1989; Leśniak et al., 2020; Niemann et al., 1990; Park et al., 1999; Penkman & Mateer, 2004; Pero et al., 2006; Ramanathan et al., 2019; Yoshida et al., 2014), all of which demonstrated some form of attentional improvement following the intervention completion. While computerised training has many benefits, as does pen-and-paper administration, the combination of both mediums might provide a mechanism in which the therapist can adequately tailor the intervention to suit the participant, based on their unique impairments, access to resources, and individual goals for the rehabilitation programme.

Follow-up of Long-Term Functional Gains

While the overall results from the studies in this review are promising regarding the long-term follow-up of sustained treatment effects, it is important to note that only three studies contained follow-up measures in their design (Couillet et al., 2010; Dymowski et al., 2016; Leśniak et al., 2020). All three of these studies reported significant improvement on attentional measures that were maintained at follow-up. While this is promising in terms of concluding that treatment effects can be sustained long-term there are several points to consider. Firstly, a very small sample of studies actually utilised a follow-up design, and as such, one cannot conclude whether follow-up results are consistently seen after efficacious attentional treatment. Secondly, two of the studies had a one month or less duration of follow-up. This finding of a paucity in long-term functional gains reported within literature of attentional rehabilitation is documented in previous ABI literature (Bogdanova et al., 2016; Virk et al., 2015). As per the quality assessment of the RCT design criterion of a long-term outcome measure included in the design, a minimum of three-month follow-up is required for full credit (Cicerone et al., 2009), of which only one study meets this criterion. The lack of long-term outcomes measures seems is consistent with previous similar literature (see e.g., Bogdanova et al., 2016), and therefore future studies should aim to include these measures in the study design.

Limitations of the Studies Reviewed and the Implications

Ecological Validity: Generalisation to Daily Lives

The importance of ecological transfer effects from intervention training to aspects of daily living has already been touched upon. Being the most commonly reported limitation across all 21 studies, it is safe to say that it is an area of concern across intervention types (Couillet et al., 2010; Keller, 2001; Mohanty & Gupta, 2013; Niemann et al., 1990; Penkman & Mateer, 2004; Ramanathan et al., 2019; Wall et al., Yoshida et al., 2014; Zickefoose et al., 2013).

Eight studies utilised a measure of generalised attentional gains in order to assess the ecological validity of their findings, like subjective self-report measures (e.g., RSAB, TEA, MARS). Findings across these eight studies were mixed. Three studies reported some improvements, four reported mixed results (some subtest scores improved, while others remained the same or worsened), and one study reported no changes post-intervention. Based on the evidence presented in this review, it is impossible to state conclusively state whether generalisation of outcomes can be achieved following attentional training.

One explanation previously posited by Niemann et al. (1990) is that frontal lobe deficits, which are frequently present in severe TBI, may directly interfere with the participants' ability to generalise such treatment effects into aspects of their daily living – a feature which is distinct from other ABI samples, like patients with CVAs. In support of such findings is evidence from the Bleiberg et al. (1985) study, which looked at the ability of stroke versus TBI participants in learning avoidance and escape behaviour. Their findings suggested that although both groups were similar in terms of learning, the TBI group was unable to acknowledge and use the information that would have allowed them to avoid an unfavourable noise. As such, Niemann et al. (1990) report that even with the addition of verbally informing patients with feedback on their training performance and the real-world applicability of the intervention, this did not improve generalisation of such abilities obtained during the intervention for TBI patients. The efficacy of training may therefore be directly influenced by the limited lack of self-awareness, self-monitoring, and insight, which are frequently disrupted in more severe cases of TBI (Robertson & Schmitter-Edgecombe, 2015).

That said, time since injury may play a significant role regarding whether TBI patients are able to adequately generalise treatment effects into their daily lives. Literature suggests that TBI patients demonstrate an increased awareness regarding cognitive and behavioural difficulties over time (Hart et al., 2009; Ponsford et al., 2014). This was particularly evident when comparing the subacute versus post-acute time since injury. Impaired self-awareness of cognitive difficulties will not only impact the engagement within the intervention itself, but can have negative influences on the rehabilitation outcomes (Rigon et al., 2017). Such findings may indicate greater generalisability of attentional gains with increased time since injury. As such, future research efforts should be directed towards investigating the awareness of deficits comparing those in the acute stage versus long-stage recovery as a function of ecological transfer of treatment effects.

The transfer of treatment effects post-intervention should arguably extend to all aspects of daily living. The generalisability of results is particularly important to consider when concluding if attentional training is efficacious – if there are no real-world improvements beyond the intervention itself, one could question what the purpose of such training is. Practice effects may explain improvement on neuropsychological assessment measures, but it is unclear if such findings will be reflected in tasks of daily living thereby improving quality of life for participants and their families. The limited reporting of the transfer of treatment effects is evident in this review. While longitudinal studies do report increased independence in tasks of daily living across 10 years post-injury, such as those returning to work or driving, findings suggest that there is continued need for support for those following moderate to severe TBI in the years following injury (Ponsford et al., 2014).

Small Sample Size

A small sample size was the second most common reported limitation of the studies included in this review. Notably, six of the SCED studies reviewed reported limitations with a small sample size (Dymowski et al., 2016; Gamito et al., 2011; Penkman & Mateer, 2004; Pero et al., 2006; Ramanathan et al., 2019; Yoshida et al., 2014). Given intraindividual variability, generally, and not only between those with TBI pathology, means that it is difficult to then generalise such findings to a larger population. Even within group studies, due to the inability to obtain a sample with homogenous functional lesions with differing and often nuanced cognitive deficits, true generalisability to a broader population cannot be obtained (Margevičiūtė, 2012).

Four studies included in this review reported being unable to determine if their findings can be generalised to a wider population, even those within a moderate to severe TBI sample (Connor & Shaw, 2016; Couillet et al., 2010; Pero et al., 2006; Ramanathan et al., 2019). Given the low proportion of studies claiming ecological validity of their results, it is difficult to conclude that the studies presented here with positive attentional outcomes, would be applicable to a larger population.

Paucity of Literature

I believe that it is important to consider that only 21 studies met inclusion criteria in this review from 4,325 articles. While many of these articles were off topic, there were many articles that were excluded based on two major principles: (1) TBI sample of mixed severity without clear delineation of severity groups, and (2) ABI sample used without separate reporting for TBI participants.

As examined above, the importance of identifying injury severity is essential within a rehabilitative setting as it serves as a relatively stable prognostic factor. There exists a dose-response relationship between the severity of injury and the severity of cognitive outcomes (Baum et al., 2016; Olsen et al., 2015). The majority of TBIs diagnosed are mild with estimates of up to 90% (Skandsen et al., 2019), and as such, is often overrepresented in literature. Literature suggests that in terms of rehabilitation efforts, mild TBI is most amenable to spontaneous recovery with deficits being less severe than those from moderate to severe TBI which often persist for years (Ruet et al., 2019; Schretlen & Shapiro, 2003). Best evidence synthesis suggests that the majority of patients with mild TBI will make a good recovery (Turner-Stokes et al., 2015).

Many studies reporting on the efficacy of rehabilitative programmes includes a mixed TBI sample with the majority being mild TBI participants. Therefore, stating such findings of efficacious treatment may not be applicable to severe injuries. Even within a moderate to severe TBI range, the extent of the associated outcomes varies widely. Penkman and Mateer (2004) found that out of their four participants, the two with the least severe injury demonstrated the most improvement on both ERP and behavioural measures. As such, it is important to reflect on rehabilitative techniques specifically for moderate to severe TBI populations which may not respond to treatment efforts in the same way that a mild TBI population might.

The second point is that many studies published that investigate cognitive remediation of attention deficits include a mixed ABI sample. While it is understandable as many conditions may overlap with each other or may result in similar pathologies neurologically, the underlying mechanism of damage is distinct. For example, for a study by Shah et al. (2004) who compared an ABI sample to a TBI sample who were functionally similar upon admission to rehabilitation, findings suggest that the TBI sample required longer admission, with a higher cost of care. Further, the Bleiberg et al. (1985) study mentioned above demonstrates the possibility of patients with TBI-related frontal lobe damage unable to generalise treatment effects to their daily life in comparison to patients with CVAs. As such, research should be aimed at establishing the differences between the two sample groups following rehabilitation, in an effort to determine if the groups can be combined and valid conclusions regarding efficacy can be drawn.

Limitations of the Current Study

There are several notable limitations of this systematic review. This review only considered published literature that was accessible in an electronic form. This means that all print publications were not considered, nor were any form of grey literature (i.e., unpublished or not academically traditional literature). This approach was adopted on the basis of time and resource constraints and in an attempt to maintain rigor and produce peer-reviewed evidence-based recommendations. Obtaining print publications was next to impossible given that this review was conducted in South Africa during the Covid-19 pandemic and several lockdown restrictions impacted on the accessibility of obtaining material in person. By not making use of print publications, it is acknowledged that there may be significant clinical research that therefore has not been reviewed, as online accessible articles are more frequently cited due to the increased ease of accessibility (Lawrence, 2001).

Similarly, the notion of excluding grey literature from this review was made in an attempt to increase the reliability of the findings and recommendations made in this review by assuming the peer-reviewed literature would be of a higher methodological quality and therefore more likely to present reliable findings. However, this is not without fault given the high prevalence of publication bias that exists in which statistically significant results are more likely to be published (Joober et al., 2012). This is particularly important to consider within the context of the aim of this review, which was to determine the efficacy of attentional rehabilitation strategies. As such, there is a concern that there may have been several studies conducted that did not yield statistically significant findings, and as such, were not published. However, given the stringent, methodological nature of conducting a systematic review effectively, and given the importance of such research when informing evidence-based clinical guidelines, priority was given to the methodological quality of peer-reviewed published literature.

A further limitation to note is that only English-published or English-translated texts were considered. This decision was made on the basis that both reviewers were only fluent in English. However, this created a bias towards English publications and therefore, there is a large body of multi-language articles particularly from developing nations on cognitive remediation that unfortunately could not be reviewed for selection and inclusion in this study (Cleaver & Nixon, 2014).

Conclusion

In summary, the aim of this review was to assess the efficacy of attentional training in adults who had experienced a moderate to severe TBI. Despite the limitations mentioned above, this systematic review provides useful information regarding the efficacy of such training programmes within this understudied population group. Such findings suggest that irrespective of time since injury and age, attentional training can be efficacious within this sample. A growing interest in investigating the role that technology, and computerised training in particular, plays in this rehabilitation setting, is ever present and needs to be further explored. That is not to say that paper-and-pen, in-person rehabilitation is not successful as it remains an important aspect of the rehabilitative practice. However, given the high clinical labour demand associated with rehabilitation, not to mention the costly nature thereof, technological advancements include benefits to address such demands that needs to be explored in more detail.

Further, while findings from this review are suggestive that attentional gains are seen across techniques, it is unclear whether these gains are reflective in the patients' daily life. As such, it is recommended that future research adopt a more holistic assessment process in which aspects like self-report and quality of life questionnaires are administered in conjunction with standardised neuropsychological assessment to determine the ecological validity of attentional gains. Such importance is emphasized within the definition of neuropsychological rehabilitation post-TBI which is ultimately to improve the daily living experience of those who have sustained such injuries. It is unclear whether attentional gains seen in this review are maintained over time given the lack of long-term follow-up. As such, follow-up assessment should be conducted as standard practice in any rehabilitative research.

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Appendix A

Table Summarising Previous Systematic Reviews Targeting Attention Through Cognitive Rehabilitation

Table 5

Summary of Systematic Reviews Targeting Attention Through Cognitive Rehabilitation

Authors	Rehabilitation strategy	Injury type	Injury severity	Sample characteristics	Specific details
Virk et al., 2015	Cognitive rehabilitation	ABI: stroke; TBI; CNS-impacting malignancy	Mixed	Paediatric and adult sample	Attention subdomains: sustained; selective; alternating; inhibition; divided
Loetscher et al., 2019	Cognitive rehabilitation	Stroke	Mixed	Mean age of all studies included classified as adults	Global attention. Attention subdomains: divided; alertness; selective; sustained. Additional findings on functional abilities; mood; quality of life.
Knowles et al., 2016	Attention Training Technique (ATT)	Clinical sample mainly psychiatric conditions	Mixed	Clinical and non-clinical samples	Focus on self-regulation following psychological disorders
Jabalera et al., 2012*	Combination of stimulants and cognitive rehabilitation	TBI	Unknown	Unknown	Additional findings on mood changes

Appendix B

PRISMA 2020 Checklist

Section and Topic	Item #	Checklist item	Location where item is reported
TITLE			
Title	1	Identify the report as a systematic review.	
ABSTRACT			
Abstract	2	See the PRISMA 2020 for Abstracts checklist.	
INTRODUCTION			
Rationale	3	Describe the rationale for the review in the context of existing knowledge.	
Objectives	4	Provide an explicit statement of the objective(s) or question(s) the review addresses.	
METHODS			
Eligibility criteria	5	Specify the inclusion and exclusion criteria for the review and how studies were grouped for the syntheses.	
Information sources	6	Specify all databases; registers; websites; organisations; reference lists and other sources searched or consulted to identify studies. Specify the date when each source was last searched or consulted.	
Search strategy	7	Present the full search strategies for all databases; registers and websites; including any filters and limits used.	
Selection process	8	Specify the methods used to decide whether a study met the inclusion criteria of the review; including how many reviewers screened each record and each report retrieved; whether they worked independently; and if applicable; details of automation tools used in the process.	
Data collection process	9	Specify the methods used to collect data from reports; including how many reviewers collected data from each report; whether they worked independently; any processes for obtaining or confirming data from study investigators; and if applicable; details of automation tools used in the process.	
Data items	10a	List and define all outcomes for which data were sought. Specify whether all results that were compatible with each outcome domain in each study were sought (e.g. for all measures; time points; analyses); and if not; the methods used to decide which results to collect.	
	10b	List and define all other variables for which data were sought (e.g. participant and intervention characteristics; funding sources). Describe any assumptions made about any missing or unclear information.	
Study risk of bias assessment	11	Specify the methods used to assess risk of bias in the included studies; including details of the tool(s) used; how many reviewers assessed each study and whether they worked independently; and if applicable; details of automation tools used in the process.	
Effect measures	12	Specify for each outcome the effect measure(s) (e.g. risk ratio; mean difference) used in the synthesis or presentation of results.	
Synthesis methods	13a	Describe the processes used to decide which studies were eligible for each synthesis (e.g. tabulating the study intervention characteristics and comparing against the planned groups for each synthesis (item #5)).	
	13b	Describe any methods required to prepare the data for presentation or synthesis; such as handling of missing summary statistics; or data conversions.	
	13c	Describe any methods used to tabulate or visually display results of individual studies and syntheses.	
	13d	Describe any methods used to synthesize results and provide a rationale for the choice(s). If meta-analysis was performed; describe the model(s); method(s) to identify the presence and extent of statistical heterogeneity; and software package(s)	

Section and Topic	Item #	Checklist item	Location where item is reported
		used.	
	13e	Describe any methods used to explore possible causes of heterogeneity among study results (e.g. subgroup analysis; meta-regression).	
	13f	Describe any sensitivity analyses conducted to assess robustness of the synthesized results.	
Reporting bias assessment	14	Describe any methods used to assess risk of bias due to missing results in a synthesis (arising from reporting biases).	
Certainty assessment	15	Describe any methods used to assess certainty (or confidence) in the body of evidence for an outcome.	
RESULTS			
Study selection	16a	Describe the results of the search and selection process; from the number of records identified in the search to the number of studies included in the review; ideally using a flow diagram.	
	16b	Cite studies that might appear to meet the inclusion criteria; but which were excluded; and explain why they were excluded.	
Study characteristics	17	Cite each included study and present its characteristics.	
Risk of bias in studies	18	Present assessments of risk of bias for each included study.	
Results of individual studies	19	For all outcomes; present; for each study: (a) summary statistics for each group (where appropriate) and (b) an effect estimate and its precision (e.g. confidence/credible interval); ideally using structured tables or plots.	
Results of syntheses	20a	For each synthesis; briefly summarise the characteristics and risk of bias among contributing studies.	
	20b	Present results of all statistical syntheses conducted. If meta-analysis was done; present for each the summary estimate and its precision (e.g. confidence/credible interval) and measures of statistical heterogeneity. If comparing groups; describe the direction of the effect.	
	20c	Present results of all investigations of possible causes of heterogeneity among study results.	
	20d	Present results of all sensitivity analyses conducted to assess the robustness of the synthesized results.	
Reporting biases	21	Present assessments of risk of bias due to missing results (arising from reporting biases) for each synthesis assessed.	
Certainty of evidence	22	Present assessments of certainty (or confidence) in the body of evidence for each outcome assessed.	
DISCUSSION			
Discussion	23a	Provide a general interpretation of the results in the context of other evidence.	
	23b	Discuss any limitations of the evidence included in the review.	
	23c	Discuss any limitations of the review processes used.	
	23d	Discuss implications of the results for practice; policy; and future research.	
OTHER INFORMATION			
Registration and protocol	24a	Provide registration information for the review; including register name and registration number; or state that the review was not registered.	
	24b	Indicate where the review protocol can be accessed; or state that a protocol was not prepared.	

Section and Topic	**Item #**	**Checklist item**	**Location where item is reported**
	24c	Describe and explain any amendments to information provided at registration or in the protocol.	
Support	25	Describe sources of financial or non-financial support for the review; and the role of the funders or sponsors in the review.	
Competing interests	26	Declare any competing interests of review authors.	
Availability of data; code and other materials	27	Report which of the following are publicly available and where they can be found: template data collection forms; data extracted from included studies; data used for all analyses; analytic code; any other materials used in the review.	

Appendix C

Search Strategies for Databases

Table 6

CINAHL Search Strategy (N = 1,087)

Set	Boolean logic	Combination logic	Index terms
#1 (TBI)	"Traumatic brain injury" OR "traumatic brain injuries" or TBI	AND	(MH "Brain Injuries+")
#2 (Attention)	attention*	AND	(MH "Attention") OR (MH "Selective Attention") OR (MH "Divided Attention")
#3 (Intervention)	Intervention OR intervene* OR rehabilitation OR rehab OR rehab* OR rehabilitate OR program OR programme OR training OR train* OR remediation OR remediate* OR treat*	AND	(MH "Rehabilitation; Cognitive") OR (MH "Therapy; Computer Assisted") OR (MH "Rehabilitation+")
#4 (Type of intervention)	N/A		
#5	#1 AND #2 AND #3		

Note. N/A denotes not being included as it limited search results due to indexing within database

Table 7

Cochrane Library Search Strategy (N = 463)

Set	Boolean logic	Combination logic	Index terms
#1 (TBI)	“Traumatic brain injury” OR “traumatic brain injuries” or TBI	AND	Brain injuries+
#2 (Attention)	attention*	AND	Attention+
#3 (Intervention)	Intervention OR intervene* OR rehabilitation OR rehab OR rehab* OR rehabilitate OR program OR programme OR training OR train* OR remediation OR remediat* OR treat*	AND	Rehabilitation+ OR rehabilitation research+ OR treatment outcome+
#4 (Type of intervention)	Cognit* OR neuro* OR psycholog*	AND	Cognitive remediation+
#5	#1 AND #2 AND #3 AND #4		

Note. + denotes exploding all trees.

Table 8

PsycINFO Search Strategy (N = 1,738)

Set	Boolean logic	Set	Index terms
#1 (TBI)	“Traumatic brain injury” OR “traumatic brain injuries” or TBI	#1 (TBI)	DE "Traumatic Brain Injury"
#2 (Attention)	attention*	#2 (Attention)	DE "Attention" OR DE "Attentional Capture" OR DE "Distraction" OR DE "Divided Attention" OR DE "Focused Attention" OR DE "Monitoring" OR DE "Selective Attention" OR DE "Sustained Attention" OR DE "Vigilance" OR DE "Visual Attention" OR DE "Attention Span" OR DE "Concentration" OR DE "Set Shifting" OR DE "Task Switching"
#3 (Intervention)	Intervention OR intervene* OR rehabilitation OR rehab OR rehab* OR rehabilitate OR program OR programme OR training OR train* OR remediation OR remediat* OR treat*	#3 (Intervention)	DE "Cognitive Rehabilitation" OR DE "Neuropsychological Rehabilitation" OR DE "Neurorehabilitation" OR DE "Intervention"
#4 (Type of intervention)	N/A		
#5	#1 AND #2 AND #3		

Note. N/A denotes not being included as it limited search results due to indexing within database

Table 9

PubMed Search Strategy (N = 1,641)

Set	Boolean logic	Combination logic	Index terms
#1 (TBI)	“Traumatic brain injury” OR “traumatic brain injuries” or TBI	OR	Brain Injuries; traumatic
#2 (Attention)	attention*	OR	Attention
#3 (Intervention)	Intervention OR intervene* OR rehabilitation OR rehab OR rehab* OR rehabilitate OR program OR programme OR training OR train* OR remediation OR remediat* OR treat*	**OR**	Rehabilitation OR Rehabilitation Research OR Treatment outcome
#4 (Type of intervention)	Cognit* OR neuro* OR psycholog*	**OR**	Cognitive remediation
#5	#1 AND #2 AND #3 AND #4		

Table 10

Scopus Search Strategy (N = 358)

Set	Boolean logic
#1 (TBI)	TITLE-ABS-KEY("Traumatic brain injury" OR "traumatic brain injuries" or TBI)
#2 (Attention)	TITLE-ABS-KEY(Attention*)
#3 (Intervention)	TITLE-ABS-KEY(intervention OR interven* OR rehabilitation OR rehab OR rehab* OR rehabilitate OR program OR programme OR training OR train* OR remediation OR remediat* OR treat*)
#4 (Type of intervention)	TITLE-ABS-KEY(Cognit* OR neuro* OR psycholog*)
#5	#1 AND #2 AND #3 AND #4

Table 11

Web of Science Search Strategy (*N = 2,027)*

Set	Boolean logic
#1 (TBI)	TS=("Traumatic brain injury" OR "traumatic brain injuries" or TBI)
#2 (Attention)	TS=(attention*)
#3 (Intervention)	TS=(Intervention OR interven* OR rehabilitation OR rehab OR rehab* OR rehabilitate OR program OR programme OR training OR train* OR remediation OR remediat* OR treat*)
#4 (Type of intervention)	TS=(Cognit* OR neuro* OR psycholog*)
#5	#1 AND #2 AND #3 AND #4

Appendix D

Quality Criteria for Rating Studies of Cognitive **Rehabilitation** (Cicerone et al.; 2009)

Internal Validity

A. Inclusion and exclusion criteria were explicitly stated.

Bi. Randomization: An unpredictable; random sequence was used to assign participants to treatment condition. The method of randomization was adequately specified. Quasi-random methods (eg; alternating admissions) do not receive credit.

Bii. Allocation of participants to condition was concealed from the investigators; achieved through 1 of the following methods of assignment to treatment:

- An independent person who is not responsible for determining the eligibility of participants; and who has no information about the person participating in the trial.
- A centralized randomization scheme; eg; a computer system providing allocations in a locked unreadable file that could be assessed only after inputting the characteristics of an enrolled participant.
- Randomization order is predetermined and individual assignments are maintained in sequentially numbered or coded sealed opaque containers until after the participant is enrolled.

C. Baseline characteristics: The participants in different treatment conditions should be comparable at start of treatment on important characteristics; such as demographic variables (age; sex; education); injury severity; time since injury; severity of impairment; and value of the primary outcome measure. Characteristics of both the experimental and control groups must be described to receive credit. The reviewer may need to determine the relative importance of various baseline characteristics; and may elect to give full credit on this item even if some characteristics are not equivalent at baseline. In cases in which the outcomes between groups differ on a variable; and this variable was not equivalent at baseline; a negative rating should typically be given even if other baseline characteristics are equivalent. If statistical comparisons are not conducted on baseline variables; the reviewer determines whether the information provided is adequate to consider the groups similar on important baseline characteristics.

D. Description of interventions: Adequate information is provided describing both the experimental and control interventions; allowing the reader to understand the rationale both for the intervention and for the comparison of experimental and control conditions. To receive credit for this item; all of the following criteria must be met:

- Experimental intervention
    (1) The nature of the intervention is described in sufficient detail to understand how the interventions were provided (eg; individual or group) and the methods used to promote change (eg; repetitive practice of exercises; development of compensatory strategies).
    (2) The total duration of treatment is provided; either in terms of length of treatment or termination criteria.
    (3) The intensity of treatment is provided; in terms of hours; number of sessions; frequency of sessions; and so forth.
- Control intervention
    (1) For no-treatment conditions (including wait-list controls); duration of the nontreatment or wait-list condition should be equivalent to duration of the experimental treatment.
    (2) For alternative treatment conditions; the nature of the control intervention is described as outlined for “Experimental intervention.”

E. Cointerventions: Adequate information is provided as to possible exposure to alternative treatments or cointerventions (outside of the study design); both for the treatment and for the no-treatment control conditions (if any). If cointerventions could not be avoided in the study design (eg; memory retraining in the context of patients receiving multidisciplinary treatments); indication should be given regarding the equivalence of cointerventions between treatment conditions.

F. Outcome assessor blinded: In order to receive credit; both

(1) the person conducting the outcome assessment should be unaware of the participant’s treatment condition; and
(2) objective outcome measures are used; including objective neuropsychologic measures; standardized structured interviews; or standardized clinical rating. If only self-report by the participant is used; and the participant is aware of his/her assignment to treatment condition; no credit is given.

G. Outcome measures should be congruent with the intended effects of the intervention. For cognitive rehabilitation; such measures might include

(1) measures of cognitive impairment; including standardized neuropsychologic assessment or other standardized or experimental measures of cognitive-linguistic functioning;
(2) neurobehavioral or psychosocial symptoms;
(3) assessment of activity limitations;
(4) measures of participation; community integration; or employment; and
(5) quality of life and subjective well being.

Descriptive Criteria

H. Withdrawal and dropout rates: Participants included in the study but who did not complete the observation period or were not included in the analyses must be described; and reasons for withdrawal should be provided. If the percentage of withdrawals and dropouts does not exceed 20% for short-term outcome and 30% for long-term outcome and does not lead to substantial bias; a "yes" is scored (these percentages are arbitrary and not supported by the literature28).

I. Short-term outcome assessment is conducted at the end of the intervention period28 or within 3 months of the end of treatment and is reported and analyzed within the article.

J. Long-term outcome measurement was conducted more than 3 months after completion of treatment28 and is reported and analyzed within the article.

K. Timing of outcome assessment should be identical for all intervention groups and for all important outcome assessments.

L. Sample size should be stated for each group at randomization and/or at the beginning of the intervention. There is no pre-set cut-off point to determine whether sample size is sufficient.

Statistical Criteria

M. ITT analysis: All randomized patients are reported and analyzed; other than missing values. Patients who withdrew after randomization but prior to baseline observations should be identified; all patients who received the baseline evaluation should be included in the pre-post treatment analyses to receive credit for this item. Alternative analyses may also be conducted;

particularly if dropout is greater than 20%. Observational studies do not receive credit for this item because of the inability to assess withdrawals from treatment accurately.

N. Both point estimates and measures of variability should be presented for 1 or more relevant outcome measures. Dichotomous data or frequency data (eg; number of participants who return to work) should typically include ranges or confidence intervals to receive credit for this item.

O. Statistical comparison of treatment effect: The statistical analyses must include a direct comparison between treatment conditions and not just report change for each treatment group (within-group effects). Between-group analyses of outcome that do not include a group time interaction effect are acceptable; as long as there is evidence of no difference between groups at baseline. If multiple outcome measures are used; and not all are statistically analyzed; credit for this item is based on statistical analysis of the primary outcome measure. If no primary outcome measure is identified; statistical comparisons must be reported for all outcome measures that are identified a priori as relevant to the intended effects of treatment. In cases where no treatment differences are claimed with regard to a specific measure; and the frequency or value of the outcome measure is too low to allow meaningful statistical comparison; credit may be given.

*Adapted from the following: van Tulder MW; Assendelft WJ; Koes BW; Bouter LM. Method guidelines for systematic reviews in the Cochrane Collaboration Back Review Group for spinal disorders. Spine 1997;22:2323-30.28 van Tulder MW; AFurlan A; Bombardier C; Bouter LM. Updated method guidelines for systematic reviews in the Cochrane Collaboration Back Review Group. Spine 2003;28:1290-9.29 Turner-Stokes
L; Disler PB; Nair A; Wade DT. Multi-disciplinary rehabilitation for acquired brain injury in adults of working age. Cochrane Database Syst Rev 2005;3:CD004170.31

Appendix E

Quality Criteria for Rating SCEDs (Tate et al.; 2008)

Descriptions of Items in the Single Case Experimental Design (SCED) Scale		
Item	**Aim and brief definition**	**Examples meeting the criterion**
1. Clinical history	The study provides critical information regarding demographic and injury characteristics of the research subject that allows the reader to determine the applicability of the treatment to another individual.	"S1 was a 38-year old woman with a TBI of moderate severity (GCS ¼ 9)."
2. Target behaviours	The paper identifies a precise; repeatable and operationally defined target behaviour that can be used to measure treatment success.	"The participant exhibited a specific problem behaviour defined as walking repeatedly around the rest home unit in which she resided with no apparent aim. The identified problem behaviour was operationally defined as the number of minutes during 1-hour observation periods that the participant walked around the unit."
3. Design	The study design allows the for the examination of cause and effect relationships to demonstrate treatment efficacy.	"A multiple baseline design across communication behaviours was employed to examine the effects of memory books on communication aspects of individuals with dementia."
4. Baseline	To establish that sufficient sampling of behaviour had occurred during the pre-treatment period to provide an adequate baseline measure.	"The subject was observed twice a day during the study. He underwent the control condition for 3 consecutive days; and then the treatment condition for 10 consecutive days; producing 3 control data points and 10 treatment data points."
5. Sampling behaviour during treatment	To establish that sufficient sampling of behaviour during the treatment phase has occurred to differentiate a	"Testing was undertaken daily throughout all study phases. A minimum of 10 data points per

	treatment response from fluctuations in behaviour that may have occurred at baseline.	phase were collected for all three tests of neglect. Intervention always took place during the morning; for a minimum of 10 sessions."
6. Raw data record	To provide an accurate representation of the variability of the target behaviour.	Provides the individual data from pre-treatment; treatment; and post-treatment phases; either in graphed or tabular form.
7. Inter-rater reliability	To determine if the target behaviour measure is reliable and collected in a consistent manner.	"Inter-rater reliability for the spelling accuracy and identification of facts was calculated by having both authors analyse all data. Inter-rater agreement was 93% for spelling accuracy and 90% for reporting accuracy."
8. Independence of assessors	To reduce assessment bias by employing a person who is otherwise uninvolved in the study; to provide an evaluation of the patients.	"To reduce the possibility of observer bias; all testing sessions for subjects were videotaped and later independently analysed. Testing and training were carried out by two different individuals; and the assessor was masked to which phase of the single-subject design was in effect in each test session."
9. Statistical analysis	To demonstrate the effectiveness of the treatment of interest by statistically comparing the results over the study phases.	"Interrupted time-series analysis was used to examine the effect of treatment"; if the t statistic and associated p value were provided
10. Replication	To demonstrate that the application and results of the therapy are not limited to a specific individual or situation (i.e.; that the results are reproduced in other circumstances – replicated across subjects; therapists or settings).	"Five patients underwent the treatment protocol."
11. Generalisation	To demonstrate the functional utility of the treatment in extending beyond the target behaviours or therapy environment into other areas of the individual's life.	"The extent to which patients gained in task relearning was quantified by comparing the performance of the trained tasks at baseline with the

	performance at the end of the training session. Upon completion of the programme the additional five untrained tasks assessed at baseline were readministered to the patients."

*Adapted from Robyn L Tate; Skye Mcdonald; Michael Perdices; Leanne Togher; Regina Schultz & Sharon Savage. (2008). Rating the methodological quality of single-subject designs and *n*-of-1 trials: Introducing the Single-Case Experimental Design (SCED) Scale. *Neuropsychological Rehabilitation*, *18*(4), 385-401. https://doi.org10.1080/09602010802009201

Appendix F

Ethical Approval

UNIVERSITY OF CAPE TOWN

Department of Psychology

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Telephone (021) 650 3417
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14 July 2020

Alexa Soule
Department of Psychology
University of Cape Town
Rondebosch 7701

Dear Alexa

I am pleased to inform you that ethical clearance has been given by an Ethics Review Committee of the Faculty of Humanities for your study, *Attention Training following Moderate to Severe Traumatic Brain Injury in Adults: A Systematic Review* The reference number is PSY2020-030.

I wish you all the best for your study.

Yours sincerely

Catherine Ward
Professor
Chair: Ethics Review Committee

Appendix G

Citation Information Extracted from Articles Presented in Alphabetical Order by Author(s)

Table 12

Citation Information Extracted from Articles Presented in Alphabetical Order by Author(s)

Authors	Year	Journal	Title
Connor & Shaw	2016	Journal of Pain Management	Case study series using brain-training games to treat attention and memory following brain injury
Couillet et al.	2010	Neuropsychological Rehabilitation	Rehabilitation of divided attention after severe traumatic brain injury: A randomized trial.
Dvorkin et al.	2013	Journal of Neuroengineering and Rehabilitation	A “virtually minimal” visuo-haptic training of attention in severe traumatic brain injury
Dymowski et al.	2016	Neuropsychological Rehabilitation	Cognitive training approaches to remediate attention and executive dysfunction after traumatic brain injury: A single-case series.
Fritz & Basso	2013	Journal of Neurologic Physical Therapy	Dual-task training for balance and mobility in a person with severe traumatic brain injury: a case study.
Gamito et al.	2011	International Journal on Disability and Human Development	Traumatic brain injury memory training: A virtual reality online solution.
Gray & Robertson	1989	Brain Injury	Remediation of attentional difficulties following brain injury: three experimental single case studies.
Grealy et al.	1999	Archives of Physical Medicine and Rehabilitation	Improving cognitive function after brain injury: the use of exercise and virtual reality.
Keller	2001	Journal of Neurotherapy	Neurofeedback therapy of attention deficits in patients with traumatic brain injury.
Kim et al.	2009	Neurorehabilitation and Neural Repair	Plasticity of the attentional network after brain injury and cognitive rehabilitation

Leśniak et al.	2020	Applied Neuropsychology	Comprehensive cognitive training improves attention and memory in patients with severe or moderate traumatic brain injury.
Mohanty & Gupta	2013	Annals of Neurosciences	Home based neuropsychological rehabilitation in severe traumatic brain injury: A case report
Niemann et al.	1990	Journal of Consulting and Clinical Psychology	Computer-assisted attention retraining in head-injured individuals: a controlled efficacy study of an outpatient program.
Park & Ingles	1999	Neuropsychological Rehabilitation	Evaluation of the attention process training programme.
Penkman & Mateer	2004	Journal of Cognitive Rehabilitation	The specificity of attention retraining in traumatic brain injury.
Pero et al.	2009	Brain injury	Rehabilitation of attention in two patients with traumatic brain injury by means of 'attention process training'.
Ramanathan et al.	2019	Aphasiology	Intensive cognitive rehabilitation therapy for chronic traumatic brain injury: A case study of neural correlates of functional improvement
Wall et al.	2013	Neurocase	Evaluation of neuropsychological rehabilitation following severe traumatic brain injury: A case report
Wilson & Manly	2003	Neuropsychological Rehabilitation	Sustained attention training and errorless learning facilitates self-care functioning in chronic ipsilesional neglect following severe traumatic brain injury.
Yoshida et al.	2014	Hong Kong Journal of Occupational Therapy	Flow experience during attentional training improves cognitive functions in patients with traumatic brain injury: An exploratory case study.
Zickefoose et al.	2013	Brain Injury	Let the games begin: A preliminary study using attention process training-3 and Lumosity™ brain games to remediate attention deficits following traumatic brain injury

Appendix H

Table of Detailed Intervention/Training of Studies Presented in Author Alphabetical Order

Table 13

Table of Detailed Intervention/Training of Studies Presented in Author Alphabetical Order

Authors	Intervention description
Connor & Shaw [a]	Training occurred twice per week over 12 weeks (60 min per session) in a group setting using Lumosity and other psychological techniques to remediate attention and memory. Training sessions included 1) metacognitive training to assist with compensatory strategies and promote real-world generalisation, 2) psychosocial group discussions, and 3) Lumosity Training Program and free play on participant chosen Lumosity games.
Couillet et al.	Training consisted of four 60 min sessions over six weeks targeting divide attention remediation. A randomised ABxBA crossover design was used. In the experimental group, each participant was trained in two tasks individually which was later combined simultaneously using progressive difficulty hierarchy. The control group consisted of non-specific cognitive training.
Dvorkin et al.	Training consisted of 12 four-minute blocks over two days in an effort to remediate sustained attention to visuo-motor tasks. Training was conducted using VROOM (Virtual Reality and Robotics Optical Operations Machine) in which participants needed to direct the robot handle towards targets within a visuo-haptic virtual environment. Blocks consisted of several conditions of feedback which were randomised in order.
Dymowski et al.	Training consisted of 18 sessions of assessment and intervention, with each phase lasting 3-4 weeks over a total of 12-16 weeks. Phases consisted of APT-3 and strategy training. APT-3 training consisted of nine one-hour sessions in which 72 computer tasks were completed which trained sustained attention, selective attention, working memory, suppression and alternating attention.
Fritz & Basso	A single case design included a seven-day baseline phase and a seven-day dual task intervention. Training including mobility training paired with secondary motor and cognitive tasks in order to remediate sustained and divided attention.

Gamito et al.	The single case design included 10 online VR sessions aimed at training visuospatial orientation, working memory and selective attention. VR training consisted of VR ADL tasks set in a small town which progressively increased in complexity.
Gray & Robertson	**Case 1:** Computerised training took place over two months aimed at remediating attention in the form of visual scanning and time-constrained information processing. Tasks included Rapid Number Comparison (RNC) and Digit Symbol Transfer (DST). **Case 2:** Computerised training consisted of three sessions aimed at alternating attention. Most of the programme consisted of A large proportion of his training consisted o the Alternating Stroop programme with inbuilt verbal regulation. **Case 3:** Training \consisted of a combination of Alternating Stroop, Digit Symbol Transfer and a simple arcade type game (Breakout) in an effort to train speed of psychomotor processing and visuo-motor coordination
Grealy et al.	1) Training consisted of four-week non-immersive VR environment linked to a cycle ergometer in an effort to train attention, information processing, memory, learning and reaction and movement times. Participants were requ8ired to steer through a virtual course or race against other virtual riders with performance feedback provided during the session. Performance was compared to a control TBI population. 2) Participants participated in a single intervention session in a non-immersive VR environment linked to a cycle ergometer aimed to assess reaction and movement time.
Keller	Participants in the experimental group received feedback of beta-activity to treat attention. The control group consisted of a computerised attention training program (COGPACK) to remediate cognitive impairments such as selective attention and information processing. Training consisted of 10 30-minute sessions over two weeks. A progressive hierarchical difficulty approach was utilised with feedback provided after each task.
Kim et al.[b]	Training consisted of three 30-minute sessions per week over four weeks with the intent of assessing and remediating attentional networks within the brain. The computerised training used ComCog software consisting of tasks of progressive hierarchical difficulty targeting visual and auditory attention, vigilance, divided attention, and persistence.
Leśniak et al.	Training consisted of 15 45-min group and individual (15 each) sessions 5 days per week. Group sessions included discussions on previous sessions, presentations and internal memory strategies. Individual sessions consisted of computerised training using RehaCom and CogniPlus software with progressive hierarchical difficulty aimed at memory strategy implementation and training.
Mohanty & Gupta	A home-based intervention was conducted with a family member. Tasks were given with saturated cueing, increasing in difficulty and complexity as time progressed. The aim was to train attention, verbal learning and memory, verbal fluency ,

	naming and description. Performance was reviewed and adjusted weekly, providing feedback and counselling for a total of 18 sessions over 10 months.
Niemann et al.	Training consisted of randomised two group repeated measurement design with two treatment conditions. The attention training group completed tasks aimed at remediating visual, auditory, divided, focused and alternating attention, including feedback and strategy training. This took place in six two-hour sessions conducted twice a week. The control group consisted of memory training focused on internal and external memory aids including both paper and computerised training.
Park & Ingles	Training to remediate attention deficits (sustained, selective, alternating, or divided) totaled 40 hours over seven months, with two-hour sessions. The training consisted of the APT programme with tasks of progressive hierarchical difficulty.
Penkman & Mateer	Training consisted of three phases: 1) six-week baseline, 2) six-week APT training, and 3) six-week active control condition, with the order counterbalanced. Training duration was one hour per session twice per week. APT training aimed to remediate attention deficits and consisted of tasks of progressive hierarchical difficulty. The active control phase consisted of psychoeducation and emotional adjustment to TBI and related sequelae.
Pero et al.[c]	Training varied in duration based on individual response and utilised APT programme with tasks tasks of progressive hierarchical difficulty aimed at remediating sustained, selective, alternating, and divided attention.
Ramanathan et al.	Training was conducted after four-week baseline, consisting of 30 hours over three weeks with a follow-up assessment at one-month post-intervention. The training aimed to remediate prospective memory and executive functions and included attention training (APT-III), metacognitive strategy instruction (MSI), and prospective memory training.
Wall et al.	The participant participated in holistic rehabilitation over 18 months which included tasks of daily living, cognitive, affective and behavioural interventions. The first year intervention included self-monitoring and self-awareness, general psychotherapy, cognitive rehabilitation, behavioural techniques, and adapted cognitive behavioral therapy. The last six-months focused on future goals and interpersonal relationship intervention.
Wilson & Manly	Training utilised an ABA withdrawal design which included sustained attention training (SAT) and errorless learning in order to improve spatial awareness and attention to tasks of daily living. A 10-day baseline phased was followed by SAT training consisted of 10 days of two-minute 30 second sessions with feedback and cueing prior to starting their daily care routine.

Yoshida et al.	Training consisted of two video game tasks (a flow and control task) aimed at remediating attention. Patient A performed the flow task for 14 days after receiving general occupational therapy (OT) for 11 days. Patient B performed the flow task for 15 days after performing the control task for 10 days.
Zickefoose et al.	Training consisted of eight weeks of two intervention types, namely APT-3 and Lumosity Brain Games, with repeated measures counterbalanced order.

Note. [a] Data is extracted for patient B. [b] Data is extracted for the computerised control group. [c] Data is extracted for participant MG.

Appendix I

Table of Detailed Design Characteristics of Studies Presented in Author Alphabetical Order

Table 14

Table of Design and Sample Characteristics of Studies Presented in Author Alphabetical Order

Authors	Study Type	Sample Size	Sample Characteristics	Exclusion Criteria
Connor & Shaw	Single case A-B-A design	3 (Extract data for Patient B)	Patient B: *Age*: 60 *Sex*: male *TBI severity*: severe (no GCS reported) *Cause of TBI*: MVA *Time since injury*: 8 years (2008)	1) had cognitive impairments such that they were unable to understand instructions regarding computer or tablet use; /training instruction; 2) were physically unable to view a screen for up to 60 minutes 2 times per week for 12 weeks; 3) were physically unable to use a computer/tablet keyboard; 4) were unable to commit to training 2 times per week; 60 minutes per session; for 12 weeks.
Couillet et al.	A randomised AB vs. BA cross-over design (between & within-subjects design – TBI only)	12	*Age*: age range 18-37 *Sex*: 9 males; 3 females *TBI severity*: severe (GCS ≤ 8; average = 4.8) *Cause of TBI*: not reported *Time since injury*: subacute/chronic stage range 5-38 months	1) previous psychiatric or neurological diseases; 2) substance abuse; 3) severe behavioural modifications; 4) motor or visual impairments; 5) severe cognitive deficits that would affect completion of intervention programme.
Dvorkin et al.	Within-subjects multiple baseline measures design (TBI only)	21	*Age*: 37.8 ± 17.9 *Sex*: 17 males; 4 females *TBI severity*: severe (no GCS reported) *Cause of TBI*: not reported *Time since injury*: 10.3 ± 15.6 weeks	1) no visual field defects or hemispatial neglect that prevented perception of test stimuli.
Dymowski et al.	Single case ABCA design	3	*Age*: 21–53 years *Sex*: not reported *TBI severity*: severe (PTA 11-88 days) *Cause of TBI*: 1 cycling accident; 2 car accidents *Time since injury*: 1–7 years	1) could not speak English as first language; 2) cognitive impairment affecting engagement in therapy and assessment tasks; 3) history of previous psychiatric or neurological illness

Fritz & Basso	Single case ABACA design	1	*Age*: 26 *Sex*: female *TBI severity*: severe (LoC on scene) *Cause of TBI*: MVA *Time since injury*: 51 days	Not reported
Gamito et al.	Single case ABABA design	1	*Age*: 20 *Sex*: male *TBI severity*: severe (GCS used although no value reported) *Cause of TBI*: MVA *Time since injury*: not reported	1) previous psychiatric disorders that may have an impact on memory and attention; 2) neurological diseases; namely dementia of any type
Gray & Robertson	A multiple baseline across function single case experimental design	3	3 participants: Case 1; 2; 3 *Age*: 20; 30; 19 *Sex*: all male *TBI severity*: severe (1) PTA> 8 weeks *Cause of TBI*: (1) RTA; (2 & 3) MVA *Time since injury*: (1) > 3 years; (2) 3 years; (3) 6 months	Not reported
Grealy et al.	(A) A random allocation crossover between-subjects ABA design with two conditions (exercise and no-exercise control both TBI) (B) Within-subjects multiple baseline design (TBI only)	(A) 38 (13 TBI and 25 matched TBI controls) (B) 13	Experimental TBI group (*N=13)*: *Age*: 19-64 *Sex*: 8 males; 5 females *TBI severity*: severe (GCS 3-7) *Cause of TBI*: not reported *Time since injury*: 1.7-178.6 weeks Control group (*N=25)*: Age: matched ± 2 years TBI severity: matched GCS ± 1 Time since injury: matched ± 2 weeks	1) unable to score on the Digit Span (forward and backward) or Digit Symbol (WAIS-R) tests were excluded; 2) those who were unable to carry out simple instructions; 3) those who had insufficient language function to allow verbal learning capacity to be measured
Keller	ABA between-subjects design (experimental NFT vs control computerised attention training)	21 (12 NFT; 9 control) - *Extract data control group*	*Age*: 21-42 *Sex*: not reported *TBI severity*: moderate to severe (GCS 7-12) *Cause of TBI*: not reported *Time since injury*: 1-8 months	Not reported

Kim et al.	(A) Prospective controlled fMRI study between-subjects (TBI vs health control group) (B) Observation within subjects ABA design (TBI only)	17 TBI and 15 healthy control subjects	Experimental TBI group (*N=17*): *Age*: 27.8 ± 9.8 *Sex*: 12 males; 5 females *TBI severity*: moderate (GCS used but values not reported) *Cause of TBI*: not reported *Time since injury*: 3-57 months Control group (*N=15*) *Age*: 25.1 ± 3.1 *Sex*: 10 males; 5 females	1) Patients with a history of other neuropsychiatric disorders or significant systemic medical illnesses (for both groups); 2) Patients who had intracerebral space occupying lesions
Leśniak et al.	Within subjects AABAA design	15	*Age*: 18-45 *Sex*: 11 males; 4 females *TBI severity*: moderate to severe (LoC 4-90 hours) *Cause of TBI*: not reported *Time since injury*: 3-25 months	1) history of previous neurological or psychiatric disease; or substance abuse; 2) inability to participate in both individual and group therapy (agitated behavior; depression; aphasia; and no severely impaired alertness); 3) ages below 18 or above 50
Mohanty & Gupta	Single case ABA design	1	*Age*: 24 *Sex*: female *TBI severity*: severe (GCS 8) *Cause of TBI*: RTA *Time since injury*: 1.5 months	Not reported
Niemann et al.	A Group x Trial repeated measurement design with 2 treatment conditions using a multiple baseline procedure for set 1 measures and pre-post group comparison for set 2 measures (between and within-subjects design)	26 (assigned to 2 groups: experimental and control)	Experimental group (*N=13*): *Age*: *M*=28.9; *SD*=8.2 *Sex*: not reported *TBI severity*: moderate to severe (coma duration *M*=15 days) *Cause of TBI*: not reported *Time since injury*: *M*=41; *SD*=21.5 months Control group (*N=13*): *Age*: *M*=34.3; *SD*=12.0 *Sex*: not reported *TBI severity*: moderate to severe (coma duration *M*=20 days) *Cause of TBI*: not reported *Time since injury*: *M*=37.1; *SD*=20.1 months	1) below 16 and above 60 years; 2) evidence of severe disorientation and confusion; 3) insufficient cognitive functioning (Dementia Rating Scale under 100); 4) severe aphasia; 5) insufficient vision to read text on a screen; 6) no functional hands; 7) substance abuse since injury; 8) premorbid history of psychiatric disorders resulting in hospitalizations

Park & Ingles	ABA repeated measures design with 2 conditions (TBI with treatment and matched controls without treatment - between and within-subjects design)	46 (23 TBI and 23 matched healthy controls)	Experimental TBI group: *Age*: *M*=37.3; *SE*=2.66 *Sex*: 6 males; 17 females *TBI severity*: severe (PTA *M*=8.3; *SE*=3.0) *Cause of TBI*: not reported *Time since injury*: *M*=51; *SE*=9.93 months Control group: *Age*: *M*=37.9; *SE*=2.55 *Sex*: not reported	1) outstanding medical problems; 2) profile of test results not suggestive of an attention deficit; 3) clinical judgement that the participant would not benefit from this type of training programme; 4) unwillingness to participate
Penkman & Mateer	A modified single case multiple baseline cross-over design	4	4 participants: Participant 1; 2; 3; 4 *Age*: 58; 22; 34; 40 *Sex*: female; male; male; male *TBI severity*: moderate to severe: 9-day coma; 3-day PTA; 8-week coma; 2-day LoC *Cause of TBI*: (1) fall from horse and bicycle accident; (2 & 3) MVA; (4) assault *Time since injury*: 26 months & 40 years; 45 months; 12 years; 15 months	1) under one-year post-injury; 2) not currently abusing alcohol or street drugs
Pero et al.	Single case ABABA design	2 (extract data for MG)	Patient MG: *Age*: 36 *Sex*: male *TBI severity*: severe (GCS<8; PTA 29 days) *Cause of TBI*: RTA *Time since injury*: at least 1 year	1) below 15 or above 35 years; 2) GCS above 8; 3) time since injury less than 1 year; 4) previous traumas or other neurological or psychiatric conditions; 5) Residual perceptual and motor impairments that would interfere with treatment evaluation; 6) no prevalence of attentional deficits
Ramanathan et al.	AB single case quasi-experimental design with 1-month post-treatment follow-up	1	*Age*: 54 *Sex*: male *TBI severity*: severe (confirmed by imaging) *Cause of TBI*: MVA *Time since injury*: 7.5 years	1) history of brain pathology unrelated to the TBI; 2) mobility issues restricting participation; 3) uncorrected visual impairment; 4) hearing impairment; 5) aphasia; 6) apraxia of speech; 7) reading impairment; 8) amnesia; 9) developmental learning disability; 10) classified as gifted in primary or secondary school; 11) history of drug or alcohol abuse requiring hospitalisation or chemical detoxification; 12) any psychiatric diagnosis; 13) other cognitive rehabilitation services within the

				previous 6 months; 14) any condition preventing 3T MRI scan
Wall et al.	Single case BABA design	1	*Age*: 35 *Sex*: female *TBI severity*: severe (GCS 8; PTA > 4 months) *Cause of TBI*: assault *Time since injury*: 1 year	Not reported
Wilson & Manly	Single case ABA time series design	1	*Age*: 40 *Sex*: female *TBI severity*: severe (GCS 5) *Cause of TBI*: not reported *Time since injury*: 2.5 years	Not reported
Yoshida et al.	Single case ABACA design	2	2 participants: Patient A; Patient B *Age*: 47; 41 *Sex*: female; male *TBI severity*: moderate (GCS 11; 9) *Cause of TBI*: not reported *Time since injury*: 948; 228 days	1) history of developmental disorder; psychiatric disorder; or other neurological diseases other than TBI; 2) comprehension deficits or severe aphasia; 3) receiving medication that impacts cognition
Zickefoose et al.	Single case ABACA design	4	4 participants: OE; NG; KX; KS *Age*: 36; 50; 36; 49 *Sex*: male *TBI severity*: severe (1) coma 2 days; (2) coma 115 days; (3) coma 84 days; (4) PTA>1 month *Cause of TBI*: (1) penetrating blow; (2) fall; (3 & 4) car accident *Time since injury*: 3; 35; 9; 23 years	1) Period of coma less than 1 day or a period of PTA less than 1 week, 2) a maximum of 3 years postinjury, 3) did not speak English as a primary language, 4) impaired or uncorrected hearing and vision 5) history of learning disabilities or neurological damage other than that associated with TBI, 6) presence of an aphasia based on attainment of an Aphasia Quotient score of less than 93.8 on the Western Aphasia Battery–Revised (WAB-R), 7) unable to pass a motor and mathematical computation screening demonstrating inadequate skills to perform the experimental tasks.

Note. In studies where data was extracted; as per methods; only extracted data is reported. Regarding design; A = assessment; B and C = intervention phase. MVA = motor vehicle accident. RTA = road traffic accident. GCS = Glasgow Coma Scale. PTA = post-traumatic amnesia. LoC = loss of consciousness. All Ages reported in years.

Appendix J

Table of Detailed Intervention/Training Details and Timing of Assessments of Studies Presented in Author Alphabetical Order

Table 6

Table of Intervention/Training Details and Timing of Assessments Presented in Author Alphabetical Order

Authors	Domains targeted	Attention training/intervention description	Timing of assessments
Connor & Shaw.[a]	Memory and attention	60 minutes, twice per week for 12 weeks. Includes metacognitive training, group discussion, Lumosity Training Program (15 min), free play on Lumosity (15 min). Training on computer on tablet using Lumosity website games for attention and memory.	Pre-post training (12 weeks)
Couillet et al.	Working memory, executive functioning and attention	1 hour, 4 times per week for 6 weeks. Experimental treatment aimed at dual-task training to perform 2 tasks simultaneously, of progressive hierarchical difficulty. Control consisted of non-specific cognitive training. AB vs BA – A is control; B is intervention	Assessment at baseline, 6-weeks, 12-weeks and follow-up (1-month post training completion)
Dvorkin et al.	Attention only	2 days, with 6 blocks of trails each 4 minutes per day with unlimited trials. Patients used the VROOM (Virtual Reality and Robotics Optical Operations Machine) – use handle of robot to move the handle toward targets that appeared within the visuo-haptic virtual environment consisting of 3 conditions: no haptic feedback, a break-through force, or a gentle pulse of force. The aim was to assess if haptic cues remediate attention, as hypothesised that haptic cues would redirect and help sustain attention.	Continuous assessment throughout intervention
Dymowski et al.	Attention only	APT-3 and strategy training phases comprised 18 sessions each (9 assessments and 9 intervention sessions). Each phase lasted 3–4 weeks and the study ran for 12–16 weeks in total. During the APT-3 phase participants completed 9 1-hour treatment sessions. Participants completed 72 computer tasks in total across domains.	Across task continuous assessment – APT-3 assessment Assessment pre-strategy training, post-strategy training and follow-up. Baseline and follow-up phase was 3-4 weeks. Intervention was 6-8 weeks (strategy and APT-3 training).

Fritz & Basso	Cognitive-mobility functioning	This case study included 2 phases: a 7-day baseline period (phase A) and a 7-day dual-task intervention period (phase B). Interventions included mobility tasks such as walking, balancing, and stair climbing. Mobility training was paired with specific secondary cognitive and motor tasks.	Assessment as start of Phase A (day 58), start of Phase B (day 65), and end of Phase B (day 71)
Gamito et al.	Working memory and attention	The training consisted of 10 online VR sessions. The VR platform consisted of a small town complete with buildings, where the participant was able to move freely around. Each session comprised of a variety of ADL tasks increasing in complexity. Tasks were aimed at targeting specific cognitive functions such as visuospatial orientation, working memory and selective attention.	The neuropsychological assessment for this experiment was divided in three phases: PTA – Pre-Treatment Assessment (session 1); ITA – Intermediate Treatment Assessment (session 5) and POSTA – Post-Treatment Assessment (session 9)
Gray & Robertson	Attention only	Case 1: Two computerised tasks, the Rapid Number Comparison (RNC) and Digit Symbol Transfer (DST) over 2 months. Case 2: Majority of training included computerised modification of Alternating Colour Stroop task with verbal regulation. Case 3: Combination of Alternating Stroop, Digit Symbol Transfer and a simple arcade type game (Breakout).	Target vs control functions measured at almost every session Measures of attention (e.g. PASAT) measured pre-training and post-training
Grealy et al.	Attention, information processing, learning, memory, reaction and movement times	4-week training using three virtual non-immersive VR environments linked to a cycle ergometer, in which the subject had to steer a course around a virtual world or participate in a race against other virtual riders. A map of the virtual world and summary statistics provided the subject with performance feedback during the exercise session.	Pre and post intervention (4 weeks)
Keller[b]	Attention only	Control group utilised a computerised attention training program (COGPACK) consisting of 10 different tasks selected for the training of speed of information processing and selective attention. Feedback concerning speed and accuracy was immediately provided during the task and hierarchical progression applied. Ten 30-minute training sessions were conducted in two weeks.	Pre and post intervention (2 weeks)
Kim et al.	Attention only	Cognitive training was performed using ComCog software for 30 minutes per session and 3 times per week for 4 consecutive weeks. ComCog is a computer-assisted cognitive training program consisting of 10 different attention tasks to train various domains of attention.	Pre and post intervention (4 weeks)

		Control group provided baseline comparison to TBI group and did not partake in any training.	
Leśniak et al.	Attention and memory	Consisted of 15 group therapy sessions and 15 individual therapy sessions (each 45 minutes), on a daily basis, five times a week. Group therapy consisted of multiple aspects of recovery, including training in internal and external memory aids. Individual therapy program consisted of cognitive computerised training conducted RehaCom and CogniPlus.	Pre-intervention, post-intervention (3 weeks), and follow-up (4 months).
Mohanty & Gupta	Cognitive functioning including verbal memory, expressive speech, verbal fluency, attention, information processing and reading	A home-based neuropsychological remediation plan was developed including the patient's father as co-therapist. The cognitive retraining tasks included tasks of enhancing attention, temporal encoding tasks to enhance verbal learning and memory, tasks to improve verbal fluency and naming and describing things to improve expressive speech. The goals were set as per the patients need with hierarchical progression. This was used in conjunction with counselling and feedback sessions.	1½ months (pre-assessment) and 9 months (post-intervention)
Niemann et al.	Attention only (memory as control condition)	The 29 subjects were randomly assigned to one of two treatment conditions: **Attention training**. The training was structured into three major components: visual, auditory, and divided attention. Task durations varied between 5-10 min. All visual tasks were computerized, and all but one set of auditory tasks were recorded on cassette. Six 2-hour sessions were allocated for each component, with alternating tasks within session. Minimum training time on the tasks was 30-40 min. The remaining time was allocated for feedback and strategy teaching. The patients were seen twice per week individually. **Memory training**. Behavioural treatment approaches for memory deficits were utilised, focusing on internal aids. included internal and external memory aids. Both pen-and-paper and computerised tasks were used.	A multiple baseline design was juxtaposed with a pretreatment and posttreatment (9 weeks) comparison design. Multiple baseline measures were administered at intervals three times prior to, six times during, and two times after completion of the training. In contrast with the baseline measures, the tests of the SDNTB were administered only before and after the training (9 weeks).
Park & Ingles	Attention only	APT programme was administered. Training lasted approximately 40 hours, and typically consisted of 2-hour sessions over seven months. The APT programme consists of a series of progressively more difficult exercises, designed to provide practice in at least one of the following subtypes of attention: sustained, selective, alternating, or divided.	Pre-post intervention (7 months)

Penkman & Mateer	Attention only	Each participant underwent a six-week baseline phase, six weeks participation in Attention Process Training (APT) and six weeks participation in a group-based Active Control (AC) condition - twice per week for one hour each session for both conditions. The APT materials consisted of hierarchically organised treatment tasks for each of the five subtypes of attention: focused, sustained, selective, alternating, and divided attention.	Behavioral dependent measures were collected at two-week intervals throughout the study for a total of nine data points per phase (x3 baseline, x3 APT, x3 Active Control).
Pero et al.[c]	Attention only	Training consisted of 75 sessions of APT. The programme is directed toward four levels or components of attention: sustained attention, selective attention, alternated attention, and divided attention. For each of these four components a hierarchy of specific tasks was developed.	TAP: administered twice before therapy (with a 1-month interval) and after each therapy stage. TEA: pre-post intervention
Ramanathan et al.	Attention, prospective memory and executive functioning	After an initial 4-week pre-treatment baseline phase, 30 hours of therapy was provided over 3 weeks, targeting attention, PM, and EFs. Treatments included APT, metacognitive strategy instruction, and PM training. A modified version of the APT-III was used to target simultaneous attention to two different tasks (i.e., divided attention). Other training included EF training (using step-by-step Metacognitive Strategy Instruction (MSI)) and prospective memory training.	Modified APT-III: administered weekly for ongoing repeated measures
Wall et al.	Mood, cognition and daily functioning	The first 12 months consisted of holistic rehabilitation of daily activities. A structure-based routine training with compensatory aids was utilised. The next 6 months focused on building up her relationships with others, considering future accommodation needs, finding meaningful activities and consolidating previous rehabilitation goals.	TEA: measured 12 months post-rehab and again at 24 months post-rehab.
Wilson & Manly	Sustained attention, spatial neglect and daily functioning	A 25-min training session using the Baking Tray Task was employed to raise awareness of sustained and spatial attentional difficulties following the initial 10-day baseline period. The intervention phase consisted of a brief sustained attention training (SAT) routine lasting no more than 2min 30-sec delivered everyday for 10 days just prior to the commencement of the self-care programme. This intervention was used in conjunction with errorless learning.	Pre-post intervention
Yoshida et al.	Attention only	Two types of video game tasks for attention training were created, one inducing flow (flow task) and the other not (control task). Patient A performed the flow task for 14 days after receiving general occupational therapy (OT) for 11 days. Patient B performed the flow task for 15 days after performing the control task for 10 days.	Measures admisitered during pre-intervention, post general OT phase/control phase, and post flow phase.

Zickefoose et al.	Attention only	Participants engaged in 8 weeks of intervention using both APT-3 and Lumosity Brain Games. Two participants received APT-3 treatment first, while the other two received Lumosity treatment first. All participants received both treatments throughout the course of two, 1-month intervention phases.	TEA: Pre-intervention, post-intervention 1, and post-intervention 2. NAB: Pre-post intervention as well as weekly during intervention phases

Note. [a] Data is extracted for patient B. [b] Data is extracted for the computerised control group. [c] Data is extracted for participant MG. APT = Attention Process Training. EF = executive functioning. PM = prospective memory. * Includes composite scores that include attention assessment within them.

Appendix K

Table of Attentional Measures, Results Reported and Interpretation of such Results Presented in Author Alphabetical Order

Table 6

Table of Attentional Measures, Results Reported and Interpretation of such Results Presented in Author Alphabetical Order

Authors	Measures used to assess attention	Results reported	Interpretation of results
Connor & Shaw [a]	• Pair Cancellation subtest from Woodcock Johnson Cognitive Abilities (WJ-III) –visual sustained attention • *Brain Performance Test (BPT) in the Lumosity programme • Qualitative analysis using semi-structured interviews	• Participant B demonstrated modest improvement on W-J Pair Cancellation (22% to 39%) and Lumosity BPT composite (10% to 16%) regarding age-related percentile scores. No significance values reported or effect size. • Qualitative analysis – no reported attentional changes. That said, cognitive processing efficiency was reduced according to participant B by spending too much time on the computer or playing difficult games causing headaches.	Modest improvement seen on attentional measures. Qualitative analysis reflected positive changes in daily living but not related to attention specifically.
Couillet et al.	• The Divided attention subtest of TAP (reaction times & omissions) – divided attention • Combination of Go-no-go and Digit Span tests (reaction times and omissions in go-no-go, and percent of hits in digit span task)– divided attention	• Divided attention subtest (TAP): Both main effect of time and interaction of group x time was significant for both median reaction time and omissions. Post-hoc significant difference between groups at week 6, as the control group remained stable during control phase and only improved after training had started. The experimental group had a large effect size (*d*>1.5) and minor effect size for control group (*d*<.02). Improvement remained stable at follow-up. • Go-no-go and digit span dual task: Both main effect of time and interaction of group x time was significant for reaction times and digit span dual task. Regarding omissions, the main effect of time was significant, but the interaction of group x time was not – most likely due to ceiling effect reached. Post-hoc demonstrated significant difference between groups at 6 weeks for digit span dual task, but not omissions or reaction times. Experimental group had a large effect size (*d*>.8) for all	Results showed a training-related improvement of the two target tasks of divided attention. Performance improved both in terms of reaction time and accuracy. As expected, the group who received treatment first (BA) performed significantly better than the other group at the 6-week assessment time point for 3/5 measures that were used across the two dual-tasks, suggesting that attentional training was more effective than control training. A non-significant trend was found for the two remaining measures. Effect-sizes

	• Flexibility sub-test of the TAP – alternating attention • TMT – sustained attention • Stroop test – sustained and alternating attention • RSAB	three measures, and minor effect size for control group. Graphical representation demonstrated stable improvement at follow-up. • Both main effect of time and interaction of group x time was significant for interference score of Stroop and number of errors in flexibility subtest of TAP. No significant difference between groups at 6 weeks. • TMT: Main effect of time was non-significant and no significant difference between groups at 6 weeks. Moderate to large effect size for experimental group (*d*>.05) and minor after control training (*d*>.02). Findings remained at similar levels at follow-up. • RSAB: ○ Mean score of divided attention item: Both main effect of time and interaction of group x time was significant. Significant between group differences at 6 weeks, with stable ratings during control training which decreased after experimental training. Effect size of experimental group was large and close to 0 for control training. Scores remained similar at follow-up. ○ Mean scores of other 13 items: no significant difference seen after either type of training.	revealed that experimental training had a large effect on all five divided attention measures, while control training demonstrated negligible changes. Similar findings were found with the divided attention question of the behavioural scale (while other attention-related failures in everyday life were not significantly modified by the treatment). Most findings demonstrated stable scores at follow-up suggesting maintenance of treatment effects.
Dvorkin et al.	• Spatial and temporal kinematic parameters of the reach movement were calculated for each trial. These included total trial time (s), from target appearance to trial completion, as well as hand path, velocity (m/s), and distance from target (m). The duration (s) of any pauses that occurred during the movement were also calculated. The number of targets acquired in a block was also calculated.	• Attention loss: Kinematic analysis of arm movements revealed attention loss both before and during a reaching movement. Investigation of the arm velocity and distance from target indicated that some movements contained prolonged movement initiation and/or a pause (or multiple pauses) occurring during the course of the movement. This was present on both visits, but on the second visit, there was significantly less pauses and a significantly short pause duration. • A visit x block number comparison revealed significant increase in number of targets acquired across blocks. Post-hoc revealed first two blocks had significantly lowest targets acquired. The number of targets acquired in the last block on the first visit and in the first block on the second visit approached significance but was not significant. Visit x force type • Main effect of visit: Patients acquired significantly more targets on the second visit for all 3 force conditions. • Main effect of force type: Significant main effect without interaction effect. • Post-hoc: On first visit *break-through* and *no force* significantly different to *nudge* – resulting in fewer targets acquired. Compared to second visit	Results indicated benefits in most patients from the haptic nudge both before and during a movement. These patients regained concentration on the task and were able to complete the reaching movement and acquire the target. The beneficial effect of the haptic nudges compared to the other two conditions however was obvious mostly on the first visit, as patients needed less assistance from nudges as the study progressed. The increase in the number of targets acquired as the study progressed suggested that patients benefited from practice across blocks. Comparing targets acquired in last block on first day and first block on second day, although not significant (due to large variability in the data),

		where *break-through* was significantly different to *no force* and *nudge* – resulting in more targets acquired. • Significant decrease in the number of nudged required by patients over the two visits.	findings suggest that patients improved overnight between visits.
Dymowski et al.	• APT-3 inbuilt exercises • Pencil and paper cancellation tasks - speed and selective attention. One condition consisted of the Ruff 2 and 7 Selective Attention Test and the other - number of digits correctly identified and accuracy were recorded for each condition – Automatic Speed Raw Score (ASRS) and Controlled Speed Raw Score (CSRS). • TEA • RSAB • Qualitative feedback semi-structured interviews on perceptions of intervention	• APT-3: Statistical analyses significant difference all domains of attention. • Cancellation tasks: ASRS, 2 participants improved significantly from baseline to APT-3, all improved significantly from APT-3 to strategy training, and 1 participant had significant improvement from strategy training to follow up. Regarding CSRS, improvements were seen throughout although none were significant. • TEA: across participants there was significant improvement on three subtests and deterioration on four subtests after APT-3, and significant improvement on three subtests and deterioration on three subtests after individualised strategy training • RSAB: There was minimal change on self-RSAB ratings after either cognitive training intervention. Compared to significant other (SO) ratings were mixed during APT-3 and generally improved during strategy training. • Qualitative feedback: Both interventions increased participants' confidence in managing attentional difficulties and minimised their impact in everyday life, although there was limited change in everyday activities. • Participant goals achieved after strategy training were generally maintained at follow-up	• APT-3 analysis suggests that all participants progressed to more challenging levels in each domain. • At the individual level, significant improvements were seen across stages although this varied on tasks and subjects. These results suggest minimal change after either intervention on near-transfer measures. • On the TEA, there was considerable variability across tasks with generally equivocal results following both APT-3 and individualised strategy training. No domain of attention demonstrated generalisation effects over another. These findings suggest that neither APT-3 nor individualised strategy training generalised consistently to near-transfer neuropsychological tasks. • Results from RSAB depict the importance of asking both the participant and an SO to rate everyday attentional behaviour as insight may be impaired in severe injury. • In general, findings at follow-up remained stable with the exception of one participant

			improving significantly on cancellation tasks.

Fritz & Basso	• Walking While Talking Test (WWTT) – divided attention
	• TMT A and B
		• WWTT:
		  ○ Simple scores (walking while reciting alphabet)/Complex scores (walking while reciting every other letter in the alphabet) revealed 10% improvement between the start of Phase A and the start of Phase B, further there is 13% improvement from the start to end of Phase B.
		• TMT
		  ○ A – Score at start of Phase A was 41 with 3 errors, 38 with 2 errors at start of Phase B (7% improvement) and 28 with no errors at end of Phase B (26% improvement).
		  ○ B – Score at start of Phase A was 173 and was unable to complete the task, a score of 119 with 2 errors at start of Phase B (31% improvement) and 71 with 3 errors at end of Phase B (40% improvement).
			Performance on divided attention tests (WWTT and Trail Making Test) in phase B indicated modest improvement when compared that in phase A. The participant required fewer cues and had fewer errors and faster completion times.

Gamito et al.	• PASAT	For the both first and second trials, data showed a significant increase in the percentage of correct responses between PTA and ITA, and between ITA and POSTA.	Attention exercises conducted on the VR world led to an improvement of attention of this patient.
Gray & Robertson	• Case 1, 2, 3: compound measure of attention/working memory (Digit Span DS), Backwards Digit Span and Arithmetic) vs control function • Case 2, 3: WAIS Digit Span, Modified Wisconsin Card Sorting Test, PASAT	• Case 1: there was a stable baseline for both control and compound measure, followed by a gradual significant improvement during the intervention phase for the compound measure only • Case 2: there was a stable baseline for both control and compound measure, followed by a significant improvement during the intervention phase for the compound measure only. WAIS digit span, WCST errors and PASAT all improved pre-post training, although only WCST improved by more than 1*SD*. • Case 3: No scores increase during the baseline period for either the control measure or the compound measure. There was no significant rise in scores within the intervention phase for the control measure or for the target measure, but there was a sudden improvement in the target function with onset of training, but no subsequent improvement. WAIS digit span, WCST errors and PASAT all improved pre-post training, although only DS and PASAT improved by more than 1*SD*.	In all three cases, a stable baseline was reported, and attentional measures reflected positive gains, although some improvements in Case 2 were not significant. . In no case was there any improvement during the baseline period or in the control function.

Grealy et al.	• Digit Span (forward and backward) • Digit Symbol (WAIS-R) • TMT A and B	• Pre-post intervention scores demonstrated significant improvements for the digit symbol task. • Preintervention and postintervention scores (with respect to the control mean) on the three tests of attention revealed a significant overall improvement after the intervention. There was also a significant main effect for test and post hoc paired t tests revealed significant improvements on the digit symbol test following interactive exercise, but not for TMT A or B. On the digit symbol test two patients scored close to the control population mean before the intervention but improved their performance to a postintervention score beyond the range of the control population.	Overall, some positive improvement reported. Poor performance on digit span test indicated that the deficits in rehearsal, chunking, and organization of material had not improved beyond the expected, and the inability to switch between tasks (as assessed by TMT B) suggested that the allocation of attentional resources for effortful processing was at a similar level of impairment after intervention.

Keller[b]	• Pen-paper letter cancellation task
	• Choice reaction task from DR2
	• Sustained attention task from TAP
		• Cancellation task:
		  ○ Number of errors decreased for both groups but was only significant for the NFT group.
		  ○ Both groups improved significantly regarding the number of targets crossed out.
		• Choice reaction task:
		  ○ Number of errors decreased but this was not significant for either group.
		  ○ Reaction time decreased for significantly for both groups.
		• Sustained attention task from TAP:
		  ○ Number of errors decreased but this was not significant for either group.
		  ○ Reaction time decreased for both groups, but only the NFT group was significant.
			Both accuracy and speed of processing seemed to improve for both groups, although the NFT group seemed to have the greatest improvements. Importantly, no between group comparisons were made.

Kim et al.	• Attentional task based on modification of Posner paradigm – endogenous VATs aimed to elicit an attentional shift towards the target	At baseline, the accuracy of VAT was significantly lower in the TBI patients than in the controls. The response time was significantly slower in the TBI patients than in the controls. After 4 weeks of cognitive training, the TBI patients had a significant improvement in both task accuracy and response time in the VAT.	While it seems that the TBI group significantly improved on attentional measures, notably, no between group comparison was made as controls did not complete measures post-intervention.

Leśniak et al.	• Rapid Visual Information Processing (RVP) from CANTAB– selective and sustained attention.
	• PASAT
		• RVP
		  ○ Minimal to no change from baseline to pre-intervention (*d*=.20).
		  ○ Significant improvement from preintervention to postintervention (*d*=.66).
		  ○ Non-significant minimal decline from postintervention to 4-month follow-up (*d*=.22).
		• PASAT
		  ○ Significant improvement from baseline to pre-intervention (*d*=.20).
		  ○ Significant improvement from preintervention to postintervention (*d*=.21).
		  ○ Non-significant improvement from postintervention to follow-up (*d*=.22).
			RVP and PASAT scores did improve significantly from baseline to pre-intervention. In general, measures of attention improved from pre to postintervention, without any significant change at 4-month follow up indicating a stable performance with no significant decline.

Mohanty & Gupta	• Digit span from Verbal Adult Intelligence Scale • Attention & concentration from PGI Memory Scale • Digit Vigilance Test – sustained attention	• Digit span: Scores remained the same pre-post intervention • Attention & concentration subtest: Scores remained in the same percentile (90th). • DVT: Scores improved but remained in severe impairment range.	Measures of attention in general showed little to no improvement comparing pre to post intervention scores. The pre-assessment revealed severe impairment on the tests of sustained attention (visual). After the intervention, improvement was observed in performance on tests of sustained visual attention but not yet reaching normal level of functioning.

Niemann et al.	• Attention Test d2: cancellation test – visual selective & sustained attention
	• PASAT – auditory selective & sustained attention
	• Divided attention test (combine visual-digit matching with auditory target selection) – visual & auditory divided attention
		The attention group improved significantly more than the memory group on four measures of attention
		• Attention d2
		  ○ Within-groups comparison: Both groups improved but neither were significant for pre-post intervention.
		  ○ Repeated measures across intervention: A significant positive trend was obtained across 11 testing points. Most significant changes were observed during the three baseline testings, which then plateaued. The significant difference between Testings 4 and 5 reflected the influence of the visual attention training component, which carried over into the auditory attention training (Testing 5 vs. 6) and levelled out until the divided attention training was started. The effect of the latter component was evident with significant differences between Testings 8 and
			Findings suggest improvements seen within Attention d2 Test and TMT B only regarding within groups comparison. However, the attention group overall seemed to improve significantly more than the control memory group on all 4 attention measures, while the reverse was not seen on memory measures. As such the specificity of attentional training seems promising. However, pre-post treatment scores on SDNTB suggests that none of the treatment effects of either the attention or memory

	• TMT B – complex attention
	• Ruff 2 & 7 Test from the San Diego Neuropsychological Test Battery (SDNTB) – visual selective & sustained attention
		9 and continued after termination of the training (Testing 9 vs. 10) until a plateau was finally reached.
		• PASAT
		  ○ Within-groups comparison: Both groups improved but neither were significant for pre-post intervention.
		  ○ Repeated measures across intervention: non-significant findings regarding trend.
		• Divided attention test:
		  ○ Within-groups comparison: Both groups improved but neither were significant for pre-post intervention.
		• TMT B
		  ○ Within-groups comparison: Both groups improved significantly for pre-post intervention.
		  ○ Univariate test revealed a significant difference between the attention group and the memory group on the TMT-B.
		• SDNTB
		  ○ Within-groups comparison for Ruff 2 & 7 Test: Both groups decreased slightly but not significantly for pre-post intervention.
		  ○ Overall SDNTB scores demonstrated a nonsignificant group effect, trial effect and group x trial interaction effect.
			training carried over to other tests/generalised.

Park & Ingles	• PASAT	• No significant differences between TBI and control group preintervention.
		• Within group comparison of correct responses on PASAT:
		  ○ TBI group: PASAT scores were significantly higher after training compared to before. Performance significantly declined at faster rates of presentation and rate interacted significantly with test order. No other factors were significant.
		  ○ The same findings were seen for the control group.
		• Within group comparison of consecutive correct responses until first error measuring sustained attention:
		  ○ Performance on this measure increased substantially after training for the TBI group. Performance was significantly higher after training, performance declined significantly at faster rates of presentation, and these two factors interacted significantly
		  ○ The same findings were seen for the control group.
		• Within group comparison of dependent on rate of presentation:
		  ○ TBI group: Improvement seen, particularly at the slower rates, the differences were not statistically significant.
		  ○ Control group: significant improvement was seen.
			Performance appears to improve for TBI group after intervention regarding various attentional processes. However, the control group demonstrated very similar findings. Of note, there are no between-group comparisons.

		• Within group comparison of mean run length of consecutive correct responses measuring sustained attention: o TBI: significantly higher performance after training than before, a significant decrease in the mean run length of consecutively correct responses at faster rates of presentation, and a significant interaction between these two factors. In addition, there was a significant interaction between length of the list studied and rate. o The same findings were seen for the control group. o Post-hoc: For the TBI group, performance improved significantly on all but the fastest rate of presentation. The improvement in performance on test 2 versus test 1 for the control sample was significant at all rates of presentation. • Within group comparison of mean run length of consecutive incorrect responses measuring ability to regain set: o TBI: performance improved significantly after training, and that the mean run length of consecutively incorrect responses increased significantly at faster rates of presentation. o The same findings were seen for the control group.	
Penkman & Mateer	• Computerised working memory task (reaction time and error rates) • Subtests from TEA: Lottery – sustained attention; Map Search – visual selective attention; Auditory Elevator with Distraction – selective attention & resistance to distraction	• Computerised task: All participants were able to work at four levels of difficult for the spatial and verbal computerised tasks. All participants were able to achieve ceiling performance for the four item trials. Three of the four participants showed some improvement in error rates following participation in APT. All participants demonstrated improvement on the cognitive tasks following participation in APT only. For the two participants who received APT prior to the active control (AC) condition, changes were maintained during AC. For those who received AC first, changes were not noted until attention retraining was initiated. • Two participants did demonstrate improvement on TEA tasks. The remained reached a ceiling performance, with the least sensitive task being the Lottery task. • Participant 2 showed improvement on the Auditory Elevator with Distraction task after APT training and improvement on the Map Search task after participation in the AC condition delivered following APT. • Participant 4 demonstrated improvement on the Lottery subtest after participation in the APT condition. Improvement on the Auditory Elevator Task was noted during the AC condition delivered prior to APT. This participant’s performance for the Map Search task showed	The APT training seemed to improve computerised scores of attention, further evidenced by the lack of improvement following the AC condition. The tasks selected from the TEA were not as sensitive as expected resulting in a ceiling level performance. Two participants did demonstrate improvement on TEA tasks.

		considerable variability in terms of scores. The baseline was stable and improvements and decreases in performance were observed during the Active Control condition. Considerable improvement was noted for the APT phase. However, there was a decrease in scores for the last test session. This corresponds with the drop in scores also observed for the Auditory Elevator with Distraction task.	
Pero et al.[c]	• Four sub-tests from TAP: Alertness test, Optical Vigilance test, Go–No go test and Divided Attention test. • APT progression • TEA (8 subtests): (1) Map Search (visual selective attention), (2) Elevator Counting (sustained attention), (3) Elevator Counting with Distraction (auditory verbal working memory/auditory selective attention), (4) Visual Elevator (attentional switching), (5) Elevator Counting with Reversal (auditory verbal working memory), (6) Telephone Search (selective attention-visual search capacity), (7) Telephone Search while Counting (sustained attention and divided attention)	Patient MG: • Baseline measures of the TAP demonstrated MG performed below the 5th percentile for most attentional measures. In both the alertness test and reaction times in the Go-No go test were lower than other TBI patients. He made the highest number of omissions on the optical vigilance test. His performance on the divided attention test was very erratic. • APT training: In cycle 1, his speed increased on visual tasks but as more difficult exercises were introduced, his performance plateaued. The auditory trials of arithmetic computation demonstrated inconsistent performance. Over 13 sessions, his performance on cancellation exercises improved in a linear and notable fashion, even with complexity. In cycle 2 and 3, performance was good at the start and became optimal with few repetitions. • TAP: ○ In the Alertness test, MG showed no change in responding in the no-warning condition, with the performance on the post-test still in the pathological range. In the warning condition, responding on the post-test is faster and places MG just above the pathological range. ○ In the Optical Vigilance test, performance in the post-test indicates a decrease in omissions, placing MG above the pathological range; however, his performance is still low in comparison to TBI patients. ○ In the Go–No go test, an improvement in performance is present at both test 1 and post-test for the RT measure. ○ In the Divided Attention test, the pattern of performance changes considerably with time. At the first and second sessions, omissions are extremely frequent. In testing sessions 2 and 3 and in the post-test, omissions decrease and MG was within the normal limits by the last testing. • At the post-test, improvements of performance are present for most TEA sub-tests. MG's performance is now within 1 SD of the control mean in	APT training: shows progress in the execution of cancellation tasks following training in somewhat different trials, such as the auditory recognition exercises. Improvement was maintained and increased slightly in the final stages. Postintervention, MG improved in several areas of performance including vigilance, selective attention and divided attention. This improvement places MG above the pathological range although his performance is clearly still at the low end of the distribution. These improvements were present also in the functional battery where a considerable increase in performance was present across the various sub-tests.

	and (8) Lottery (sustained attention).	five measures. A large improvement is present for the Telephone Search while Counting sub-test: the additional time required for the dual task is now comparable to that of control subjects. Marked improvements are present also in the Map Search sub-test and in the Elevator Counting with Distraction sub-test.	
Ramanathan et al.	• Modified version of APT-III - sustained attention tasks and included a second (cancellation) task performed simultaneously (divided attention)	• Divided attention: Overall performance remained stable and flat over the baseline period, averaging 59.7%, then began rising immediately upon introducing treatment and continued rising over the course of treatment to 81.3% by the end of treatment, with the rate of increase beginning to plateau at 89.7% at 1-month follow-up. • Alternating attention: JD's performance during the baseline phase was already quite good (averaging 91.6%) and this remained stable over the course of the study.	It appears that divided attention was particularly amenable to changes following intervention. However, alternating attention did not improve, although notably it was at a relatively good level at baseline.
Wall et al.	• TEA – assessing auditory and visual attention, dual task, divided and focused attention and visual search and information processing, along with speed • Qualitative observations	• TEA: Map search scaled scores improved from 5 to 8, and telephone scores from 5 to 9, and visual elevator timing from 4 to 5, comparing 12 to 24 months post Oakwood rehabilitation. Visual elevator accuracy remained at a scaled score of 14. No other consistent, beyond practice effects, improvements were seen for other executive tests with most improvements stopping short of expected performance or remaining at the impaired level, including processing speed. This was with the exception of a visual attention search tasks which improved to pre-morbid levels. • From a qualitative perspective, the patient could still be distracted at times, with a deleterious effect on performance. This lends support to the proposition that some structure and routine assisted her to achieve optimal performance in this domain.	Although some improvement was made as represented by cognitive test scores, on-going cognitive difficulties were observed at 24 months post injury (both at below expected and impaired levels).
Wilson & Manly	• Verbal and physical prompt count • Behavioural Inattention Test (BIT) subtests of Line Cancellation, Letter Cancellation, Star Cancellation, Figure and Shape Copying, Line Bisection, and Representational	• The number of verbal, physical and overall prompts required to complete self-care ADL programme was significantly lower during sustained attention training (SAT) compared to baseline. Fewer prompts were required post-intervention compared to during SAT, however this was not significant. • A comparison of the individual BIT sub-test scores, pre- and post-intervention, indicated that PK's performance had remained stable on three sub-tests, improved above the clinical cut-off on two sub-tests (Line and Letter Cancellation) and sharply declined on another sub-test (Star Cancellation). Overall BIT performance remained in the impaired range postintervention.	A decrease in the number of prompts suggests an increase in sustained attention improved during SAT and continued to improve post-intervention. However, similar results were not seen in BIT and TEA where performance remained within the impaired range postintervention.

	Drawing sum to create measure of visual inattention • TEA – Elevator counting (sustained attention); Elevator counting with distraction (divided auditory attention); Lottery task (auditory sustained attention)	• In terms of non-lateralised auditory attention, PK's performance on all three sub-tests of the TEA, namely, Elevator Counting, Elevator Counting with Distraction, and Lottery were markedly impaired during both pre- and post-intervention assessment.	
Yoshida et al.	• Intervention Video game tasks: Square (sustained, selective and divided attention); Click number (sustained and selective attention); Tower (sustained, selective and alternating attention). • MARS (Moss Attention Rating Scale) • TMT • Symbol Digit Modalities Test (SDMT) • Continuous Performance Test "X" Task (CPT-X)	• Patient A showed slight improvement in the reaction time (RT) of CPT-X and MARS after general OT. However, SDMT and TMT deteriorated, and no changes were seen in the other neuropsychological assessments. After the flow phase, Patient A showed substantial improvement in both the RT and the SD of RT of CPT-X, SDMT, and MARS compared with the earlier phases. • Patient B showed improvement in both the RT and the SD of RT of CPT-X, SDMT, TMT-A and TMT-B, and MARS. No changes were seen after the control phase in the other neuropsychological assessments. • The SDMT and MARS of both patients showed two consecutive scores above the mean ± 2 SD of the general OT or control phase during the flow phase.	Both patients showed improvement in attentional function, but not in other measures of cognition; therefore, the video game tasks appear to be specific to attentional function. The flow experience was more effective than either general OT or the control task. Furthermore, both patients showed improvement on MARS,. In other words, the intervention inducing flow experience may improve patients' daily behavioural problems that result from attention deficits.
Zickefoose et al.	• APT-3 and Lumosity inbuilt measures • The researcher generated probe measures were	• All four participants demonstrated statistically significant progress in reaching new levels of difficulty on intervention tasks over the course of the APT-3 and Lumosity intervention phases. Specifically, participant KX improved significantly on all APT-3 and Lumosity activities and the other	Individual results showed participants made significant improvements in progressing through both interventions. However, limited generalization occurred: one

	adapted from the Neurological Assessment Battery (NAB) Numbers and Letters Test Parts B, C and D • TEA - visual selective attention/speed (i.e. map search and telephone search without distraction sub-tests), attentional switching (i.e. visual elevator sub-test), sustained attention (i.e. lottery and telephone search while counting sub-tests) and auditory-verbal working memory (i.e. elevator counting with reversal and elevator counting with distraction sub-tests).	three participants improved significantly on all but the Rotation Matrix game of Lumosity. • Generalization of improved attention evidenced through probe measure performance changes across participants appeared predominantly during the first intervention period, regardless of which intervention programme a given participant received during that phase. Specifically, early generalization was evident for one aspect of the probe measure for participant KX; the performances of two other participants (i.e. OE and KS) approached significance either for one or two aspects of the probe task. OE's high-performance accuracy at the time of pre-testing suggests that the probe task may have been inadequate for measuring generalization of improvement in his attention functioning. • Generalization of improved attention based on repeated performances of the TEA yielded mixed findings. Participant OE appeared to generalize his improved attending behaviour attained over the course of both intervention programmes to several TEA subtests; the TEA sub-test scores of the remaining three participants were too inconsistent to warrant any such claim associated with the separate or combined intervention programmes.	participant demonstrated significantly improved performance on one of five probe measures and one other participant showed improved performance on some sub-tests of the TEA; no other significant generalization results emerged.

Note. [a] Data is extracted for patient B. [b] Data is extracted for the computerised control group. [c] Data is extracted for participant MG. * Includes composite scores that include attention assessment within them. TAP = The Divided attention subtest of the Test for Attentional Performance; TMT = Trail Making Test; RSAB = Rating Scale of Attentional Behaviour; TEA = Test of Everyday Attention; PASAT = Paced Auditory Serial Addition Task; APT = Attention Process Training. EF = executive functioning. PM = prospective memory.

Appendix L

Table of Limitations and Recommendations Reported Presented in Author Alphabetical Order

Table 7

Table of Limitations and Recommendations Reported Presented in Author Alphabetical Order

Authors	Limitations and Recommendations
Connor & Shaw [a]	• Rehabilitation research that is conducted in treatment settings may not generalised to a larger population and as such, difficulty in assessing the efficacy of the intervention in clinical practice.
Couillet et al.	• Small sample size (due to intensity and timeousness of training). But due to representative sample of severe TBI in subacute phase, reported to be able to generalise to similar settings. • Lack of blinding of assessment measures resulting in potential examiner bias. Although, the intention of the research was to conduct the study blinded but became impossible during the study due to practical reasons. • No measure of everyday divided attention, apart from the RSAB was utilised, limiting ecological validity conclusions.
Dvorkin et al.	• The use of haptic nudges during post-acute stage of recovery needs further research - ideally in the form of a protracted, repeat-measurement design to determine if such an intervention is efficacious post the initial stages of recovery. • More research should be conducted on if tactile stimulation can be distracting to participants. • More research should be conducted on using his technique with progressive hierarchy of attentional training, to avoid the ceiling effect reached in this study and removed the aspect of boredom that some participants reported.
Dymowski et al.	• Small sample size (3 cases) thereby impacting on the validity of drawing conclusions on the efficacy of APT-3. • Presence of individualised training utilising a client-centred approach can be regarded as a confounding variable and may have influenced attentional gains. Unable to, from the current design, infer which portions of the intervention influenced the reported findings. • For patient CC, the presence of motor weakness and diplopia may have influenced outcome measures negatively. • More research required into the use of metacognitive training delivered online or in a group setting required to investigate cost-effectiveness and success of training in these contexts.
Fritz & Basso	• Improvements in dual task measures and increased walking speed may be attributable to the intensive multidisciplinary therapy provided to the participant. • All treadmill training occurred during phase B of the study and may have contributed to the observed gains in walking speed.

	• For participant TK, amantadine (a medication used to improve attention and reduce impulsivity) was started at the beginning of phase B and may have influenced on outcome measures. • Participant TK, 2 days of Phase A was during the PTA phase which may be an explanation to the slower recovery rate seen during this phase.
Gamito et al.	• Small sample utilised, and therefore require a larger sample to fully assess efficacy of training in VR online setting. • With increasing use of PC and online games with the younger generation, familiarity with such forms of technology will improve with time.
Gray & Robertson	• The study design does not allow direct conclusions to be drawn regarding the specific effects seen (e.g., Stroop training particularly improving executive functioning). The specificity of particular procedures and assessements used needs further research to investigate such claims.
Grealy et al.	• Lack of long-term follow-up measures, along with previous literature, makes it unclear whether results will be maintained or gains will continue to be made in the long-term recovery phase post-injury.
Keller[b]	• Regarding the improvements in attention seen, it is impossible to rule out the confound of spontaneous recovery. • Training effects may be limited in terms of ecological validity, as participants of the computer based training group, improved mainly in computerized attention tests requiring them to focus their attention for short periods of time. This indicates that the training effects seems to be limited to tasks (i.e., computer-based tasks) that were directly trained during the intervention.
Kim et al.	• Lack of repeat fMRI scans in the TBI control group (i.e., no cognitive training). • No long-term measures assessing treatment effects on attentional neural networks. More research is required on the long-term effect of different training techniques on attentional neural networks utilising an RCT design to establish causality of training effects. • Confounds, such as medications, impaired sleep, etc. were not accounted for.
Leśniak et al.	• Small sample size limits the statistical power with which conclusions can be effectively drawn. Utilising two condition groups rather than cross-over design, due to literature stating that cognitive training has long-lasting effects that exceed the therapy period. Role that the involvement in activities within a rehabilitation programme can positively impact on outcome measures. Although all participants prior to the programme were involved in some form of rehabilitation which could mitigate these effects. Future research could use an improved control condition which includes combined group and individual treatment that does not include attentional training to rule out such effects. • Improved outcome measures, as some were computerised, may be reflective of familiarity with computerised tasks rather than improved attention. Relevance of findings are questions given the high intensity of treatment and lower than expected attentional gains. Future research should be directed towards matching the training programme to the participant's individual needs. Future research, specifically with longer duration, should investigate the role that initial improvement in sustained attention may benefit longer term gains in other more complex domains of cognition.

Mohanty & Gupta	• Limited attentional gains may be a result of decreased contact with patient and their family in the later stages of the intervention. Future research should include more closely monitored reflection on progression. • Limited ability to infer whether treatment effects generalised to aspects of daily living in current design. However, qualitative feedback from the participant remarked on changes in ADLs and resuming her previous occupation are reflective of such changes. • Regarding the improvements in attention seen, it is impossible to rule out the confound of natural or spontaneous recovery due to single case study design. That said, research suggests such training should begin as early as possible in the recovery stages for long-term success. • Efficacy needs to be assessed in larger population.
Niemann et al.	• Although positive findings, the relevance of such findings is limited by the lack of treatment effects demonstrated on the second set of neuropsychological assessment measures. • Perhaps explained that frontal lobe dysfunction can impact on participant's ability to generalise treatment findings to their everyday life, as verbal instruction of the relevance of such training to the participant was ineffective. • Inability to determine generalisability of findings due to the lack of enrolment within comprehensive rehabilitation programmes which are evidenced as being effective in the transfer of treatment effects.
Park & Ingles	• The role of practice effects on the PASAT for the TBI group may be a confound to the improvement seen. • Lack of ability to demonstrate positive treatment effects may be attributable to study design (e.g., not enough training, small sample size, etc.) or that the training was not beneficial. • Substantial difference in time between PASAT measurements of control group vs TBI group. • Future research should be directed towards investigating more clearly the relationship between training tasks and outcome measures. • Future research should look to the degree to which training can generalise across tasks, settings and time.
Penkman & Mateer	• Small sample size and previous limited literature establishes need for replication and extension of reported findings. The presence of the AC condition (containing psychotherapeutic intervention) demonstrated improvement in outcome measures and may play a role in the efficacy of cognitive training programmes. Senstivity of testing impacts on ability to determine changes on outcome measures (e.g., ceiling effects seen on TEA). • Specificity of training specific attentional networks (e.g., selective vs resistance to distraction) impossible to determine given that only two participants showed improvements on specific attentional tasks. • Duration of training was short, and longer more intensive training may demonstrate stronger treatment effects, particularly for more severe injuries. • Lack of long-term follow-up data limits the ability to draw conclusions on maintenance of treatment effects. • Lacking in terms of assessing generalisation of treatment effects to daily living, apart from self-reported feedback post-intervention, particularly given inability to control the influence of the participant's psychosocial environment outside of the training programme.
Pero et al.[c]	• Small sample size limits ability to generalise findings with regards to long-term community integration, where larger samples would be more beneficial.

	• Future research should look into developing similar interventions which are less demanding in terms of clinical and financial resources and that are shorted in duration thereby increasing access to treatment. • Focus of research should also look into the secondary gains in social reintergration and productivity of such training.
Ramanathan et al.	• Single case (A-B) design provides challenges to internal validity due to a lack of inferential statistics, challenges to test–retest reliability, etc. Although rigourous design and testing was employed to mitigate such effects, the lack of independent/blinded assessors of pre-post treatment outcomes and the potential for speed/accuracy trade-offs in some tests were limitations. • Participant did not consent to 1-month follow-up, thereby impacting on assessment of maintenance of treatment effects. • The complex and multimodal training design and inter-relatedness of cognitive domains limits the ability to determine specific treatment effects on the targeted domains (i.e., attention, executive function and memory). • Lack of sensitivity of alternating attention APT-III task which remained above 90% throughout the study makes it difficult to rule out the potential involvement of alternating attention. • Difficulty in terms of determining the ecological validity of training, as due to the disciplined attendance and participation of the training design, the reported increased capacity to work, may be attributable to the design rather than improved attention. • 1-month follow-up data follows a similar train of thought in that effects may be an indicator of short-term maintenance rather than long-term, enduring acquisition. • Small sample size, and only two-point comparison of imaging, treatment effects/neural changes may not be generalisable to a broader population. Future research should use multiple SCEDs or multiple within-subject measurements.
Wall et al.	• Limitation of ruling out the confounding variables that may have impacted on treatment effects (e.g., spontaneous recovery; family support; pre-morbid functioning; rehabilitation (specific interventions and/or milieu); or a combination of these). Due to lack of rigorous SCED methodology, limitations on the assumption that the changes in outcomes measures is purely attributable to the intervention. • Poor ecological validity of neuropsychological assessments reflecting changes in daily living. • Finally, during the rehabilitation period, researchers were concurrently being trained on the process of recording formal procedures and the recording behavioural outcomes.
Wilson & Manly	• Not reported
Yoshida et al.	• The study design, including within-subject (A-B) design and small sample size posed limitations to findings. Longer hospital stay of participants would have allowed an improved design, such as an ABC or ABAB design. • The confound of practice effects on outcome measures cannot be ruled out as alternate versions of assessment were not available for all assessment measures. • As the flow phases were the second condition administered, there may be carry over effects from the initial condition. To counteract this, differences in task difficulty were adopted to suit the participant's individual level of treatment which may have subsequently impacted on the results. • Inability to generalise findings to the participants' daily living, as the functional independence measure did not show significant improvement.

Zickefoose et al.	• The limitation of the wide range across participants post-injury, and future research should be directed towards a more homogeneous sample regarding time post-injury is needed to evaluate the efficacy of computer-based interventions during specific stages of recovery. • Subjective clinical judgement and logic-based assumptions were used in order to determine task selection, inclusion and the level of difficulty for participants. Whereas, a different order of presentation and differing task inclusion may have provided better results. • Lack of generalisation of treatment effects may related to specific aspects of the intervention programme or may be a factor of duration and intensity of training, where longer and more intense training may have improved the ecological validity of the intervention.

Note. [a] Data is extracted for patient B. [b] Data is extracted for the computerised control group. [c] Data is extracted for participant MG. APT = Attention Process Training. EF = executive functioning. PM = prospective memory. * Includes composite scores that include attention assessment within them.