VOLUME I: RESEARCH COMPONENT

VIRTUAL REALITY ROUTE LEARNING IN PEOPLE WITH TRAUMATIC BRAIN INJURY

by

Amy Elizabeth Garrood

A THESIS SUBMITTED TO THE UNIVERSITY OF BIRMINGHAM FOR THE DEGREE OF

DOCTOR IN CLINICAL PSYCHOLOGY

Department of Clinical Psychology

School of Psychology

The University of Birmingham

May 2019

Research Thesis Overview

Volume I contains three research papers. Firstly a systematic review was conducted to investigate the effectiveness of rehabilitation strategies for improving navigation in people with acquired brain injuries (ABI). Secondly, a mixed within and between-subjects experiment was conducted to explore differences in Virtual Reality route learning in people with traumatic brain injury (TBI) compared to a matched control group in relation to landmark type (proximal and distal) and navigation strategies (egocentric and allocentric). Finally, a public dissemination document provides an overview of both the systematic review and empirical paper in a manner suitable for dissemination to relevant stakeholders.

The results of the systematic review indicated that there is much variation in the compensatory rehabilitation strategies used to help people with ABI successfully navigate. Compensatory rehabilitation strategies were broadly categorised according to whether they were person-oriented, environmentally-oriented or hybrid approaches. Overall, the conclusions that could be generated was limited by poor methodological quality and thus, more robust research is needed on this topic before an evidence-based navigation rehabilitation strategy can be used in clinical practice. Suggestions as to promising areas for future research are given.

Findings from the experimental Virtual Reality route learning study suggested that people with TBI are impaired at route learning using distal landmarks compared to a neurologically-healthy control group. As navigation using distal landmarks has been associated with an allocentric 'mental map' strategy using the hippocampus, it is possible that the participants with TBI in this study were impaired at route learning using distal landmarks due to an inability to use their hippocampi to build up a mental map of space. However, as both groups of participants reported using an egocentric 'associative' strategy to learn both types of landmark routes, this association needs further exploration.

Acknowledgements

I would like to thank my supervisor Dr Theresa Powell for her time, advice and overall support in managing and negotiating various challenges on the way to completing this thesis.

Thank you to the staff at the brain injury rehabilitation centres for supporting me with this research.

Thank you to everyone who participated, without whom this research would not have been possible. Thank you for your willingness and enthusiasm to take part!

Contents List

List of Illustrations

a) Systematic Review

b) Empirical Paper

c) Public Dissemination Document

List of Tables

a) Systematic Review

b) Empirical Paper

i) Systematic Review

Systematic Review of the Effectiveness of Rehabilitation Strategies for Navigation Difficulties in Adults with an ABI

1. Abstract

Background

Navigation impairment commonly results from an acquired brain injury (ABI) and difficulties navigating are associated with reduced independence in adults with ABI. There is currently no evidence-based guidance for neuro-rehabilitation services for how to rehabilitate navigation impairment and so the purpose of this review was to synthesise and analyse research that has been conducted on this topic.

Method

A systematic search of the PsychInfo, CINAHL, Embase, Medline and PubMed research databases was conducted for studies published up to May 2018 using specific criteria. The papers identified were evaluated for quality using one of two validated quality assessment tools depending on the study design.

Results

Fifteen studies were identified that met the inclusion criteria for the review. These fell into five categories of compensatory rehabilitation strategy type; person-oriented utilising an alternative neurological system, person-oriented unassisted, environmentally-oriented paper-based aids, environmentally-oriented electronic devices and hybrid approaches.

Discussion

Due to limitations of the quality of the research evidence and the lack of research investigating the effectiveness of any particular type of strategy, it is not currently possible to make valid conclusions on which to base guidelines for the rehabilitation of navigation impairment. Further methodologically robust research is needed.

2. Introduction

It is estimated that 1.3 million people in the UK are living with the long-term effects of acquired brain injury (ABI) at a cost of £15 billion per year through health care, social care and loss of earnings (The UK Acquired Brain Injury Forum, June 2018). An ABI is defined as damage to the brain that occurred after birth and includes traumatic brain injury such as through a fall or road traffic accident as well as acquired damage resulting from a brain tumour, stroke, substance misuse, brain infection or deprivation of oxygen (Teasell et al., 2007) (see Appendix I for a glossary of terms).

A long-term effect that often results from an ABI is impairment in cognitive functioning (Cho et al., 2017; Arciniegas, Held & Wagner, 2002). Navigation, defined as the ability to find your way from one location to another, is a complex skill involving the coordination of many cognitive abilities (Claessen, van der Ham, Jagersma & Visser-Miely, 2015; Wolbers & Hegarty, 2010). This cognitive complexity makes navigation particularly vulnerable to brain damage (Ruggiero, Frassinetti, Iavarone, & Iachini, 2014; Van der Ham et al., 2010). The specific cognitive abilities required for successful navigation include attention, working memory, spatial processing, planning, decision-making and error monitoring (Brunsdon, Nickels & Coltheart, 2007; Labate, Pazzaglia, & Hegarty, 2014; Wolbers & Hegarty, 2010).

Impairment in navigation can have debilitating consequences for people with ABI (Rivest, Svoboda, McCarthy & Moscovitch 2016; Whiteneck, Gerhart & Cusick, 2004). For example, not being able to independently navigate may produce a financial cost through the inability to travel to work as well as costs to mental well-being of not being able to independently access social and leisure activities in the community (Rivest et al., 2016; Juengst, Arenth, Raina, McCue & Skidmore, 2014; Anson & Ponsford, 2006).

Despite the importance of navigation on independence and mental wellbeing there does not appear to be evidence-based guidance for neuro-rehabilitation services for how to effectively rehabilitate people with navigation difficulties. It may be that it is particularly difficult to develop effective and generalisable rehabilitation strategies for navigation difficulties because of the heterogeneous nature of brain injury and the cognitive complexity of navigation (Claessen et al., 2015; Incoccia, Magnotti, Iaria, Piccardia & Guariglia, 2009). For example, a rehabilitation strategy for improving navigation in an individual with memory impairment may not be effective for an individual with a spatial orientation difficulty.

In neuro-rehabilitation services it is common practice to use compensatory strategies in the rehabilitation of cognitive impairments (Mazaux & Richer, 1998; Nadar & McDowd, 2010). Compensatory strategies aim to modify behaviour or the environment in order to compensate for an area of deficit (Kirsch, Simpson, Schreckenghost & LoPresti, 2004). Compensatory strategies can be divided into two major categories according to whether they are person-oriented or environmentally-oriented (Kirsch et al. 2004; see Figure 1). Person-oriented strategies are initiated by the person without any modifications of the environment and include strategies such as learning mnemonic techniques to facilitate information retrieval and errorless learning in which a person is supported to learn a task sequence without making errors. Environmentally-oriented strategies include external techniques where the environment is modified to reduce the cognitive demands of a task, such as having a written direction sheet to aid memory or using a phone map as an external cue (Kirsch et al. 2004).

In the model of compensatory strategies proposed by Kirsch et al. (2004), a distinction is made between environmentally-oriented strategies that reduce the cognitive demands of the environment and those which act as external cueing devices. However, in the case of navigation this may not be a valid distinction as aids which act as external cues for directions also reduce the cognitive demands of the environment and vice versa. Electronic navigation systems have

grown in popularity in recent years and so it may instead be useful to distinguish between environmentally-oriented strategies using such technology as opposed to more traditional paper direction-based approaches. Furthermore, Kirsch et al. (2004) describes how a person-oriented technique such as errorless learning may facilitate the use of an environmentally-oriented strategy, calling this a hybrid approach. For example, through errorless learning an individual may be taught how to use a smartphone GPS application to assist navigation.

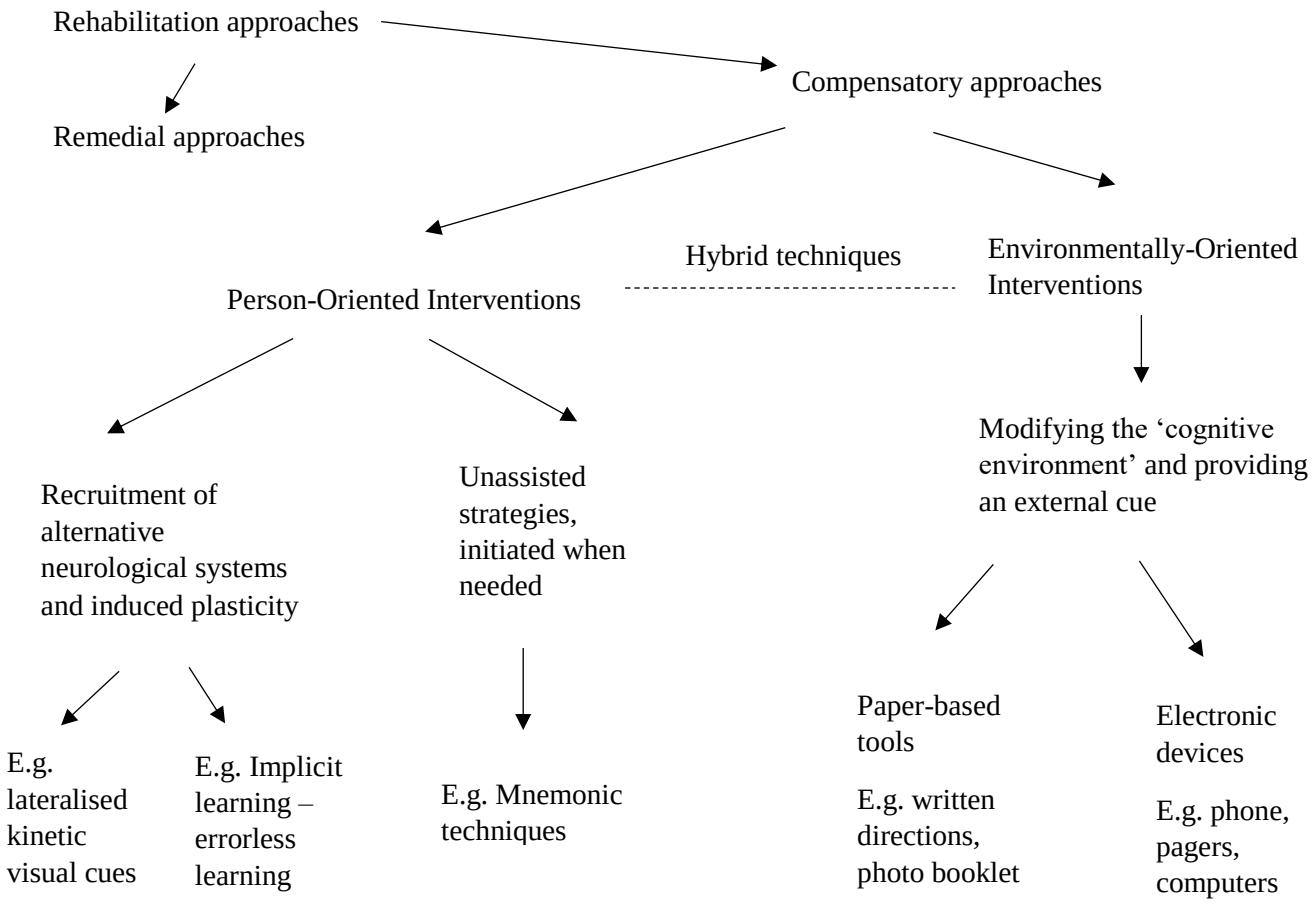


Figure 1. A graphic representation of the categories of compensatory rehabilitation interventions adapted from Kirsch et al. (2004).

Furthermore, the medium by which compensatory strategies are trained and tested may be important in determining the effectiveness of the strategy. Research investigating the rehabilitation of navigation impairment varies as to whether the rehabilitation strategy is taught

in the real-world, using virtual reality, or statically through paper maps and diagrams. Virtual reality (VR) can be defined as an advanced form of computer software that allows the user to interact with a computer-generated version of a real environment (Brooks, McNeil, Rose, Greenwood, Attree & Leadbetter, 1999). It is commonly accepted that real-world rehabilitation is more ecologically valid than paper approaches (Aguirre & D'Esposito, 1997; Kelly & Gibson, 2007). However, there is also a growing body of evidence suggesting that VR can be an effective medium for providing rehabilitation of specific cognitive abilities like memory and navigation (Rose et al., 2005; Schepers, Visser-Meily, Ketelaar, & Lindeman, 2006; Yip & Man, 2013).

To date there has not been a published review of the research investigating the effectiveness of rehabilitation strategies for navigation difficulties in adults with ABI. The synthesis and analysis of any evidence that has been gathered on this topic would be of clinical importance both for rehabilitation services in order to have evidence for clinical practice and for service-users who may benefit from increased independence resulting from being able to navigate in their communities. The objectives of this review are therefore:

1. To investigate whether there is research evidence evaluating the effectiveness of rehabilitation strategies for navigation difficulties in adults with ABI
2. To describe the range of compensatory rehabilitation strategies used to improve navigation in adults with ABI
3. To evaluate the effectiveness of these compensatory rehabilitation strategies as a function of strategy type (person-oriented vs environmentally-oriented)
4. To assess the quality of the research evidence
5. To discuss the implications of the findings and to explore the path for future research

3. Method

3.1 Search Strategy

The electronic databases PsychInfo, CINAHL, Embase, Medline and PubMed were searched for research papers that had been published before May 2018. The key concepts relating to brain injury, navigation and rehabilitation were combined using the following database specific search terms ("brain injur*" OR "stroke" OR "head injur*" OR "TBI" OR "traumatic brain injur*" or "ABI" or "acquired brain injur*" OR "head trauma") AND ("wayfind*" OR "navigat*" OR "spatial memory" OR "route learn*" OR "topographical disorientation") AND ("train*" OR "rehab*" OR "retrain*" or "interven*").

3.2 Study Selection

Firstly, duplicate papers that were identified on multiple databases were removed. Papers were then excluded based on the title and abstract if they were published in a non-English Language, if they were animal studies, if they were not published in a peer-reviewed journal and if they were otherwise clearly not relevant to the topic of the review. Full text papers were then screened and excluded if they were not specifically evaluating the effectiveness of a rehabilitation strategy, if the sample did not have a clearly defined brain injury and if they did not have a direct measure of navigation ability, including wayfinding and route learning. Wayfinding can be defined as knowing ones position in space and using this information to navigate in familiar and unfamiliar locations (Livingstone & Skelton, 2007; Aguirre & D'Esposito, 1999). Route learning is learning a specific path that joins two separate locations (Lloyd, Riley & Powell, 2009b).

Therefore studies were included that met the following criteria:

- Published in English language
- Published in a peer-reviewed journal
- Sample of human adults aged 18 or over

- Sample of adults with an ABI i.e. brain damage acquired after birth resulting from trauma, stroke, brain tumour, brain infection, substance misuse, or deprivation of oxygen
- Studies that implemented a rehabilitation strategy for improving an aspect of navigation, including wayfinding and route learning
- Studies that reported quantitative data measuring an aspect of navigation including wayfinding and route learning

As there was a relatively limited number of studies meeting these criteria it was decided not to exclude based on design. Consequently, this review included case studies, single-case experimental designs as well as experimental group studies.

3.3 Data Extraction

The data extracted from each research paper are displayed in Table 2.

3.4 Quality Assessment

The quality of the research papers selected in terms of risk of bias was evaluated using two validated quality assessment tools depending on the study design. For N-of-1 studies including case reports, pre/post-interventions and single-case experimental designs, the Risk of Bias in N-of-1 Trials (RoBiNT) Scale was used (Tate et al., 2015) (see Appendix E). This scale rates N-of-1 studies according to 15 items. Seven items are combined to create an internal validity score and eight items are combined to create an external validity score. Studies were given 0, 1 or 2 points for each item according to how well they met the criteria for that item.

The quality of the group studies was evaluated using the National Institute for Health and Care Excellence (NICE) quality appraisal checklist for quantitative studies (NICE, 2012) (see Appendix F). This tool was designed to critically appraise the internal and external validity

of any research study that reports the effect of an intervention on a quantitative outcome. Similarly to the RoBiNT Scale, the NICE checklist produces an external and internal validity score for each study based on certain items. Items are scored a (++) if the study has been designed and conducted in a way to minimise the risk of bias, a (+) if not all risks of bias have been accounted for, a (–) if there is likely to be a significant source of bias, a (NR) if the risk is not reported and a (NA) if the particular item is not applicable to the study design.

Neither the RoBiNT Scale nor the NICE quality appraisal checklist for quantitative studies included an item evaluating the quality of the rationale. It was felt that this would be an important item to include in order to ascertain whether researchers had a clear rationale for the rehabilitation strategies used based on previous research evidence or theory. Therefore, an item to evaluate the rationale for the rehabilitation strategy used was added to the quality assessment of the studies in this review.

4. Results

4.1 Search Strategy

The search strategy yielded 901 papers. A systematic process of removing duplicates and excluding studies that did not meet the inclusion criteria was followed and 13 papers were identified. Two additional papers were identified from a reference list search (see Figure 2 for a diagrammatic representation of the search process).

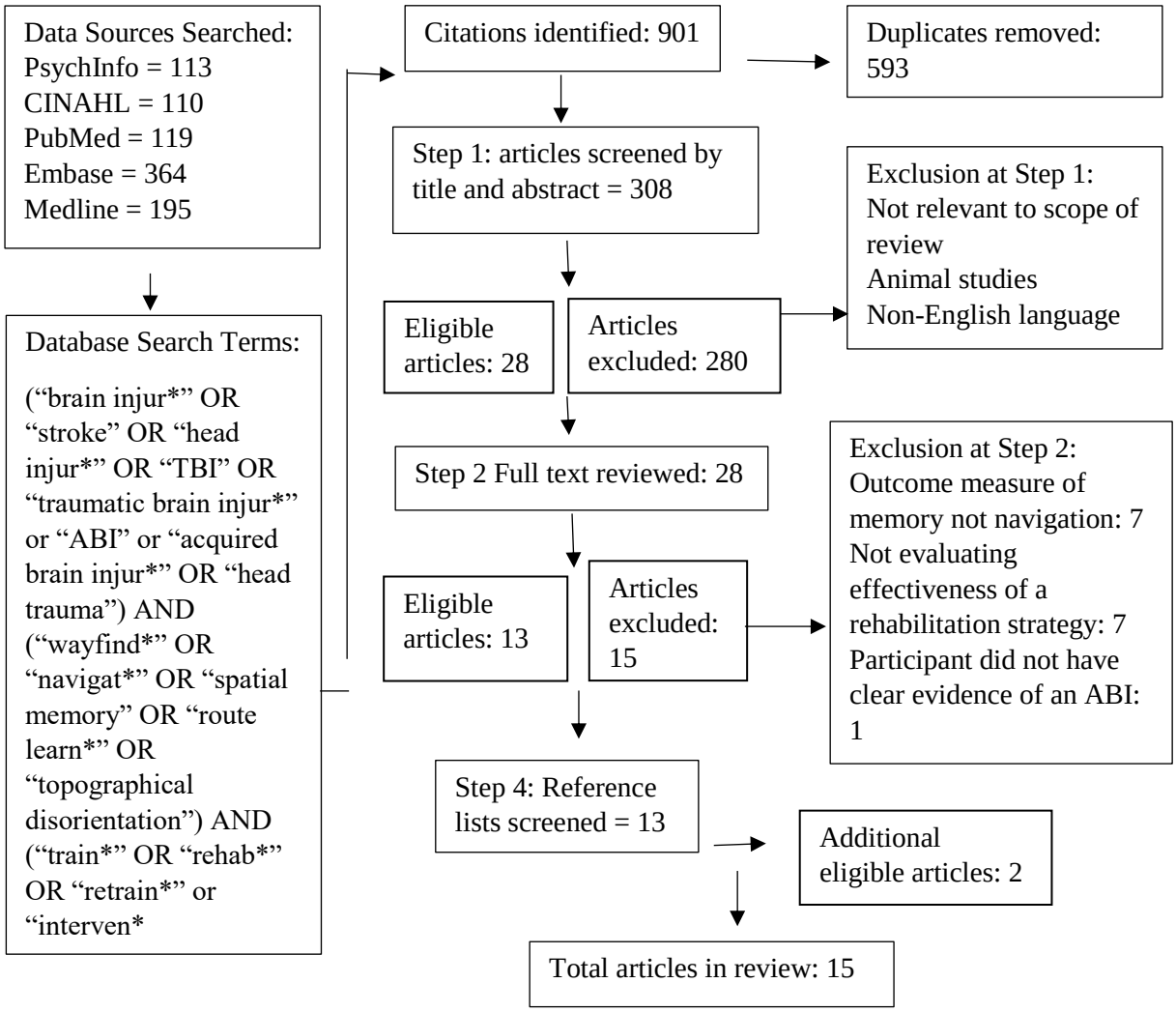


Figure 2. Flow diagram representing the systematic search process

4.2 Study Design and Risk of Bias

Nine of the research studies used N-of-1 methodology and were rated for quality using the RoBiNT Scale, including four pre/post-intervention studies, four single-case experimental designs and one case report (Cho & Sohlberg, 2015; Cho et al., 2017; Claessen et al., 2015; Incoccia et al., 2009; Bouwmeester, van der Wege, Haaxma & Snoek, 2014; Brooks et al., 1999; Rivest et al., 2016; Kirsch et al., 2004; Newbigging & Laskey, 1996). The quality of the six group studies was evaluated using the NICE checklist and included four within-subjects experimental designs and two mixed within and between-subjects experimental designs

(Evans, Wilson, Andrade & Green, 2000; Lloyd et al., 2009; Kessels, van Loon & Wester, 2007; Lemoncello, Sohlberg & Fickas, 2010a; Lemoncello, Sohlberg & Fickas, 2010b; Sohlberg, Fickas, Hung & Fortier, 2007). A second rater independently scored four of the studies (27%), which included three N-of-1 studies and one group study. Ratings were compared and discrepancies were discussed until consensus was reached. Table 1 presents a summary of the ratings given for each study for internal and external validity ordered by compensatory strategy type (see Appendices E and F for the full quality assessment ratings).

Table 1. Risk of Bias Summary

Research Study	**Study Design**	**Internal Validity Score**	**External Validity Score**
Person-Oriented – Alternative Neurological Systems			
Brooks et al. (1999)	N-of-1	2/16	8/16
Lloyd, Riley & Powell (2009)	Group	+	+
Evans et al. (2000)	Group	-	-
Person-Oriented – Unassisted Internal Strategies			
Cho & Sohlberg (2015)	N-of-1	3/16	9/16
Cho et al. (2017)	N-of-1	8/16	14/16
Environmentally-Oriented – Paper-based Aids			
Newbigging & Laskey (1996)	N-of-1	0/16	8/16
Bouwmeester et al. (2014)	N-of-1	2/16	8/16
Incoccia et al. (2009)	N-of-1	2/16	5/16
Claessen et al. (2015)	N-of-1	2/16	7/16
Lemoncello et al. (2010)	Group	-	-
Environmentally-Oriented – Electronic Devices			
Kirsch et al. (2004)	N-of-1	3/16	11/16
Sohlberg et al. (2007)	Group	+	+
Lemoncello et al. (2010b)	Group	-	-
Hybrid Techniques			
Rivest et al. (2016)	N-of-1	5/16	11/16
Kessels et al. (2007)	Group	-	-

4.3 Description of Studies

The majority of the studies were conducted in North America or Europe, including six in the USA (Cho & Sohlberg, 2015; Cho et al., 2017; Lemoncello et al., 2010a & 2010b; Sohlberg et al., 2007; Kirsch et al., 2004), three in the Netherlands (Bouwmeester et al., 2014; Kessels et al., 2007; Claessen et al., 2015), two in the UK (Brooks et al., 1999; Lloyd et al., 2009), two in Canada (Newbigging & Laskey, 1996; Rivest et al., 2016) and one in Italy (Incoccia et al., 2009). However, the study by Evans et al. (2000) only states that the research took place across Europe and Argentina. In terms of the setting, nine of the studies trained participants to use a navigation strategy in a real town, university campus or hospital location (Cho & Sohlberg, 2015; Cho et al., 2017; Newbigging & Laskey, 1996; Bouwmeester et al., 2014; Incoccia et al., 2009; Lemoncello et al., 2010a & 2010b; Kirsch et al., 2004; Sohlberg et al., 2007; Rivest et al., 2016; Kessels et al., 2007). One study trained participants to use a navigation strategy using a virtual simulation of a real town (Lloyd et al., 2009). Two studies trained route learning using a combination of VR and the real-world (Brooks et al., 1999; Claessen et al., 2015). Evans et al. (2000) tested route learning using paper diagrams.

In terms of compensatory rehabilitation strategy type, three of the studies used errorless learning, a person-oriented compensatory strategy utilising alternative neurological systems (Brooks et al., 1999; Lloyd et al., 2009; Evans et al. 2000). Two of the studies used help-seeking as an unassisted person-oriented strategy (Cho & Sohlberg, 2015; Cho et al., 2017). Five studies used a paper environmentally-oriented strategy such as written directions with landmark cues (Newbigging & Laskey, 1996; Bouwmeester et al., 2014; Incoccia et al., 2009; Claessen et al., 2015; Lemoncello et al., 2010a). Three papers studied the effectiveness of an electronic environmentally-oriented strategy (Kirsch et al., 2004; Sohlberg et al., 2007; Lemoncello et al., 2010b). Finally, two studies used a hybrid approach combining a person-oriented and an environmentally-oriented strategy (Kessels et al., 2007; Rivest et al., 2016).

Furthermore, all of the studies used a quantitative measure to determine effectiveness. Five studies used formal measures such as the Executive Function Route Finding Task (EFRT) or the Virtual Tubingen Test (Cho & Sohlberg, 2015; Cho et al., 2017; Bouwmeester et al., 2014; Incoccia et al., 2009; Claessen et al., 2015). The other studies used a simple measure of route accuracy such as the number of errors made when performing a route pre/post-training or the number of trials taken for a participant to correctly perform a route (Brooks et al., 1999; Lloyd et al., 2009; Evans et al., 2000; Newbigging & Laskey, 1996; Rivest et al., 2016; Kessels et al., 2007; Lemoncello et al., 2010a & 2010b; Sohlberg et al., 2007; Kirsch et al., 2004).

The sample sizes of the studies varied widely, from case studies to experimental group studies of 18 participants with ABI and 18 controls. Participants in all of the studies were adults aged between 19 to 70 years old. In terms of the type of ABI, seven studies included only TBI (Cho & Sohlberg, 2015; Newbigging & Laskey, 1996; Kirsch et al., 2004; Rivest et al., 2016) or only stroke participants (Brooks et al., 1999; Bouwmeester et al., 2014; Claessen et al., 2015). Furthermore, Kessels et al. (2010) only included participants with Korsakoffs syndrome and the case study by Incoccia et al. (2009) was of a young woman who suffered meningitis as a baby. The other six studies included a sample of participants with ABI of mixed aetiology (Lloyd et al., 2009; Evans et al., 2000; Cho et al., 2017; Lemoncello et al., 2010a & 2010b; Sohlberg et al., 2007).

The study findings and quality assessment will be discussed according to the categories of compensatory rehabilitation strategies outlined in Figure 1. Table 2 provides the detail of each study.

Table 2. Summary of the characteristics and findings of the research studies

Author and Date	Participant Characteristics	Study setting	Study Aims and Design	Rehabilitation Method and Outcome Measure	Key Findings
Person-Oriented: Alternative neurological systems strategies					
Brooks, McNeil et al. (1999) Route learning in a case of amnesia. A preliminary investigation into the efficacy of training in a virtual environment.	**Sample size:** One participant **Age and gender:** 53 year old woman **BI type:** Subarachnoid haemorrhage leading to memory and executive function difficulties	**Country:** UK **Setting:** Hospital Rehabilitation Unit (Indoor environment) **Recruitment:** Unclear	To see whether routes learned in a 3D virtual environment generalise to the real world in a woman with amnesia. To see if learning is quicker when routes are taught in the real world or virtual environment. **Design:** Single-case design (AB)	The patient's ability to perform 10 simple routes around the hospital unit was initially tested twice in a **baseline phase**, once by the experimenter and once by the Clinical Psychologist working on the unit. Performance order of the 10 routes was randomised. **Rehabilitation Strategy:** Phase 1: The patient was taught two routes randomly selected from the 10 routes in a **virtual simulation of the real hospital** unit using a **backwards chaining errorless learning strategy.** The patient was trained on both routes for 15 minutes each weekday for 3 weeks. At the end of each week, the patient was instructed by the Clinical Psychologist to perform each of the 10 routes in the real hospital unit with the order of the routes presented at random. The patient was tested for two more weekly sessions once this first phase of training was completed to test maintenance of learning resulting in 5 testing sessions for the intervention phase. Phase 2: The patient was taught two more of the original 10 routes using backwards chaining selected from the remaining routes. The two routes that were selected were deliberately chosen as they were roughly equal in terms of complexity and the paths of the two routes did not cross in the real unit and so they were independent routes. One of the routes was randomly selected to be taught in the virtual unit and the other to be taught in the real unit. The patient was trained on each route for 15-minutes on each	Prior to training, the patient was unable to walk any of the 10 routes around the hospital unit. Phase 1: After two weeks of training, the patient could walk the two routes in the real unit that she had been taught using backwards chaining in VR. She could not walk the untrained routes around the unit. She was able to walk these routes correctly for a further two weeks after the training had ended. Phase 2: After two weeks of training on two more of the 10 routes, the patient was able to walk the route trained in VR in the real unit but not the route trained in the real unit. She had maintained her ability to walk the routes taught in phase one. The patient remained unaware that she was able to walk the routes taught and had to be encouraged by the clinical psychologist who tested the patient on the routes to 'just have a go' (implicit memory). **Clinical Implications:** Errorless learning to teach routes in a virtual environment may be an effective strategy to help patients with amnesia learn relatively simple routes.

				weekday for two weeks. After each week the Clinical Psychologist tested the patient's performance on each of the 10 routes in the real unit. A final testing session was completed one week after training had ended resulting in three testing sessions for phase two. **Outcome Measure(s):** Number of correct routes (no errors) out of 10 performed each week.	Errorless learning taught in the real-world may not be effective due to distractions/difficulty to control obstacles in the real environment. When practicing the route in the real unit in phase two, the patient would often stop to talk to other patients or staff and peered into rooms as she walked past. However the route training does not generalise to other routes as the patient continued to be unable to perform the untrained routes and the patient needed encouragement in the real unit to try – did not initiate the route herself due to lack of explicit memory of being able to perform the routes. Nevertheless, VR may be a useful medium to train specific simple routes in individuals with severe memory impairments as VR removes factors like noise which act as distractors when training in the real-world.
Lloyd et al., (2009) Errorless learning of novel routes through a virtual town in people with ABI.	**Sample size:** 20 participants **Age and gender:** Mean age = 42.5 years old No gender data reported	**Country:** UK **Setting:** Participants attended outpatient rehabilitation services/day centres	To compare errorless and errorful learning of novel routes in a non-immersive 3D VR environment. **Design:**	**Rehabilitation Strategy:** Comparing **errorless and errorful learning** techniques to learn two equivalent routes in a **virtual simulation** of a town. The order of presentation of the routes and allocation of routes to errorless or errorful conditions was counterbalanced. In the errorless condition, participants watched the researcher complete and verbally describe the turnings made at each junction for a demonstration trial and two learning trials. For the test trial, participants were asked to	A paired samples t-test revealed a significant difference between number of errors made under errorful and errorless conditions. There were significantly higher mean errors in the errorful condition. 14 out of 20 participants showed an advantage for route learning using an errorless learning strategy. **Clinical Implications:** Errorless learning using VR may be effective to help people with ABI learn routes. However, this was

	BI type: 8 TBI 6 Vascular 6 brain tumour and cortical cysts	(Learning routes in a VR outdoor environment) **Recruitment:** Recruited from the outpatient rehabilitation centres	Repeated-measures, within-subjects design	direct the researcher around the route and were corrected within 5 seconds of a wrong turning to prevent the participant from getting lost. In the errorful condition, participants watched the researcher complete and call out the directions in the demonstration trial. However, the two learning trials took the format of the test trial in the errorless condition where participants called out the directions and were corrected within 5 seconds if they made a wrong turning (as such they were allowed to make errors). The test trial was the same as in the errorless condition. **Outcome Measure**: Number of errors made recalling the route in the errorless and errorful conditions	not an effective strategy for all participants. This study showed that participants' with ABI who were better at route learning in errorful conditions showed improvement over the learning trials thus demonstrating that they had been able to explicitly remember the errors they made and self-correct, learning from their mistakes. However, the participants who performed better at route learning in errorless conditions did not show this improvement. Thus, errorless learning of routes may only benefit certain people with ABI who have impaired explicit memory. However, there was no delayed recall trial to test if learning was maintained and there was no generalisation measure to test transfer of learning to the real-world. Also, participants did not move themselves through the routes and the routes did not have cues from motion and head movements like route learning from real life which may have affected participants' route performance.
Evans et al. (2000) A comparison of errorless and trial and error learning methods for teaching individuals	**Sample size:** Phase 1: 18 Phase 2: 16 Phase 3: 20	**Country:** Europe and Argentina **Setting:** Unclear	To compared trial and-error learning with different types of errorless learning methods for acquiring	**Rehabilitation Strategy:** Phase 1 – participants learned a 10 step route around a 2D paper diagram room in three conditions: **trial and error, errorless with the steps written down to follow and errorless using backward chaining** (completing last step first, then the last two steps, the last three steps etc). Phase 2 – participants learned an eight step route around a paper diagram room in two conditions; **trial and error vs**	No difference was found between participants' trial and error test scores and errorless learning test scores in any of the route learning or stepping stone experiments. There was not a beneficial effect of any errorless learning method (forward chaining, backwards chaining, chunking) over trial and error learning.

with acquired memory deficits.	**Age and gender:** Phase 1: mean age 43.9 years Phase 2: mean age 41.4 years Phase 3: mean age 37.9 years **BI type:** Phase 1 and 2: ABI of mixed aetiologies (stroke, hypoxia, TBI, encephalitis, chronic alcoholics) Phase 3: additionally included brain tumour, myotonic dystrophy with cerebral atrophy	**Recruitment:** Unclear	practically relevant skills (only four of the nine experiments were specific to route learning and will be described in this review) **Design:** Within-subjects, repeated measures	**forward chaining.** In the forward chaining condition participants learned to correctly complete the first step, then the first and second step, then the first, second and third step and so on. In a separate experiment, participants also learned a nine step route in a stepping stone maze in a trial and error and errorless condition using a guided route to directly follow. Phase 3 – participants completed a 13 step stepping stone maze route in two conditions; trial and error and an errorless condition where the 13 steps were chunked into 5, 4, 4 step sequences and participants were guided to learn the sequences without making errors. There was at least a week gap between each condition in each experiment and condition order was counterbalanced. **Outcome Measures:** Each condition was followed by three blocks of three test trials in which participants were asked to recall the route leading to a short delay test score of total number of correct moves made across test trials. Another test of participants route recall was completed an hour after each condition in each experiment to produce a long delay test score of number of correct moves made.	**Clinical Implications:** Errorless learning may not be an effective rehabilitation strategy for improving route learning in people with ABI. Evans et al. (2000) suggest that ceiling effects may have not allowed differences between conditions to be detected (i.e. the tasks may have been too easy). It may also be that route recall on paper diagram tasks may have used explicit and not implicit memory and so was not actually utilising an alternative neurological system (implicit memory). The cognitive mechanisms involved in spatial learning processed implicitly/procedurally may need movement through space in a large-scale 3D environment. Thus, these were paper-based experiments with very little ecological validity to real-world route learning.
Person-Oriented: Unassisted strategies					

Cho & Sohlberg (2015). Training adults with brain injury how to help seek when lost	**Sample size:** Three participants **Age and gender:** 51 year old male 24 year old male 52 year old male **BI type:** TBI	**Country:** USA **Setting:** University of Oregon campus (outdoor environment) **Recruitment:** Convenience sampling	To describe the development and initial piloting of a therapy protocol specifically targeting help seeking when adults with brain injury are lost. **Design:** Pre/post intervention	**Rehabilitation Strategy:** Participants' completed the **NICE help-seeking treatment protocol (Noticing you have a problem, Identifying the information you need for help, Compensatory strategies, Evaluating progress).** Manualised group treatment protocol, 6 sessions teaching skills through role plays, video feedback. **Outcome Measure:** The Executive Function Route Finding task was administered pre and post training on the NICE group protocol. The examiner observed the participant navigating to an unfamiliar destination and rated performance on a four-point scale in (a) task understanding (b) information seeking (c) retaining directions (d) error detection (e) error correction and (f) on-task behaviour. The ratings result in the EFRT Total Score. Higher scores on the EFRT indicate more independent wayfinding. An EFRT footstep ratio was also measured to examine efficiency of wayfinding pre and post training (the participant's footsteps divided by fewest number of footsteps taken to complete the route).	All three participants successfully completed the 6-week NICE group treatment. Post-treatment measures suggested they were more independent and efficient in their wayfinding after being taught help-seeking skills. Specifically, there was a reduction in their EFRT total score and footstep ratio. **Clinical Implications:** Adults with TBI will most likely need to seek help on occasions when they do not have the internal resources to solve problems while navigating/route finding in the community. The benefit of this person-oriented strategy in navigation rehabilitation is that it could generalise to different routes, as it is not limited to learning set routes.
Cho et al. (2017). Training adults how to help seek when wayfinding: an understudied	**Sample size:** Seven participants **Age and gender:** Range between 20	**Country:** USA **Setting:**	To experimentally evaluate the potential impact of the NICE group programme on people with ABI.	**Rehabilitation Strategy and Outcome Measure:** **Participants completed a six-week NICE manualised group protocol to train help-seeking when wayfinding** (same as above). Participants were divided into two groups of two participants and one group of three participants to complete the NICE group intervention. Prior to the study, 10 routes were equated for distance and complexity.	Visual analysis and Tau-U non-parametric statistical analysis. All three groups showed improved help-seeking as measured by the social behaviour rating scale. Rapid immediacy effect of treatment, tau-u scores of 1 indicated strong effects of treatment. Wayfinding: All participants showed a reduction in EFRT footstep ratio score indicating fewer steps being taken to reach

critical life skill.	and 64 years old, mean age =50.4 years All female **BI type:** 4 TBI 2 stroke 1 brain tumour	University of Oregon campus (outdoor environment) **Recruitment:** Recruited from a major metropolitan medical centre	**Design:** Single subject combined non-concurrent and multiple probe multiple baseline across participant cohort design.	Participants completed the 10 routes in a random order counterbalanced to make up five baseline and five intervention data points. Participants were observed navigating to the goal destination and help-seeking was measured on a 21 point social behaviour rating scale. Inter-observer agreement was 95.2% for 60% of baseline and intervention probes. Wayfinding was measured using the EFRT total score and footstep ratio. 90.5% inter-observer agreement for baseline and 95.2% for treatment. In the intervention phase, participants completed the route 60 minutes before the next scheduled group session, at the same time and day of the week. The first group of three participants completed the five baseline sessions over a two-week period. After a two week gap five intervention data points were collected from the start of the NICE group intervention. The second group of two participants completed the five baseline sessions over four consecutive weeks after the first group had completed the study. They then completed the NICE group intervention after a two week gap and five intervention data points were collected. The last group of two participants completed the five baseline sessions over a six week period after the second group had completed the study. They then completed the NICE group intervention after a two week gap and five intervention data points were collected. (Participants were divided into three groups and within each group participants completed five baseline and five intervention probes of three target behaviours; help-seeking, wayfinding independence and wayfinding efficiency, concurrently. The three groups completed the study non-concurrently. The time period over which the three groups completed the baseline phase was systematically increased by two-week increments).	the goal destination. Total EFRT score improved for six out of seven participants. **Clinical Implications:** The findings suggest the NICE group treatment has potential to improve help-seeking as applied to wayfinding. This suggests efficacy of a group treatment which addresses social problem solving to improve functional behaviours for help seeking when wayfinding for adults with ABI. As above, this intervention may allow people with ABI to navigate not limited to a set of learned routes.

Environmentally-Oriented Strategies: Paper based aids					
Newbigging & Laskey (1996) Riding the bus: teaching an adult with brain injury to use a transit system to travel independently to and from work	**Sample size:** One participant **Age and gender:** 28 year old male **BI type:** TBI – severe Frontal/parietal lesions	**Country:** Canada **Setting:** Ontario city bus routes (Outdoor environment) **Recruitment:** Not clear	To teach independent bus travel to a man with memory problems and no previous experience of urban bus routes. **Design:** Case study	**Rehabilitation Strategy:** Three bus routes were taught. Each route was first preceded by a **planning phase (tracing the routes on a map with the therapist and developing a help sheet with the steps to follow)**. The participant then completed each bus route **by using a sheet with the task steps and road names on. Each turn was ticked off by the participant along the route.** **Outcome Measure:** Number of trials required to master each bus route	The participant mastered all three bus routes within five trials. The direction sheets were eventually reduced to laminated cards with prompts. Same technique was used successfully later to teach other routes. **Clinical Implications:** In vivo specific route learning tailored to an individual's needs can be effective to help patients with TBI access their community. Limitations are that this approach is limited to specific routes, cannot spontaneously complete a new route without engaging in learning with assistance from a direction sheet/person.
Bouwmeester et al. (2014) Rehabilitation in a complex case of topographical disorientation	**Sample size:** One participant **Age and gender:** 35 year old man **BI type:** Stroke	**Country:** The Netherlands **Setting:** Community-based learning of meaningful routes in the patients home town (outdoor environment) **Recruitment:**	To describe the rehabilitation process of a patient with severe topographical disorientation over a 12 year period **Design:** Pre/post intervention	**Rehabilitation Strategy:** The participant's Occupational Therapist (OT) agreed with him six meaningful routes that he would like to learn. The OT made use of the participant's intact reading ability to **develop a direction booklet for each route which contained pictures of his chosen landmarks** along each route **and all with concise, written directions and some additional pictures of the features he was using.** He learned each route one at a time over two training periods due to him moving house. The two training periods taken together took an average of 10 weekly two-hour sessions to learn a short route and 18 weekly two-hour sessions to learn a long route. **Outcome Measure(s):**	The participant learned a set of new routes using the landmark direction booklets and could walk them without cues after 12 years. He was able to identify new landmarks to use in the learning of new routes but relied on others to help him develop the written instructions. The participant gained in independence and in quality of life, but only within the limits of the learned routes. Number of sessions needed to use a single landmark decreased over time. This carried over from the first to second training period despite a 6 year training gap. His score on the RLT had also improved from his wife's estimation of his abilities prior to training.

		From a centre to rehabilitate his topographical disorientation		Number of sessions needed to be able to use a single landmark in a route. The Route Learning task (RLT) was also used to measure training outcomes as a pre and post measure. The RLT consists of seven tasks including a route retracing task in the forwards direction, a route retracing task in the backwards direction, a dead reckoning task where he was asked to point to the starting point of the route, a landmark location and recognition test, an order memory and map drawing task. The participant completed this RLT for a route he had previously been guided by the therapist without a direction sheet in the post outcome measure. However, the participant's wife was asked to score how he would have performed pre route training retrospectively as the pre outcome measure as the RLT had not yet been developed at this time.	Not clear how long it took the patient to learn each route as the measure of learning a single landmark is just one step along each route. **Clinical Implications:** Extremely lengthy process but time was needed to describe the precise nature of the topographic deficit and to design a tailor-made intervention. Routes can be taught and learned but need to be individualised to the goals of the patient. Routes taught from a street-level perspective using landmarks worked well in this case. But length of time needed for this rehabilitation to be effective is unlikely to be cost-effective or feasible to rehabilitation services. The OT had to start the training process again when the participant moved to a new home.
Incoccia et al. (2009) Topographical disorientation in a patient who never developed navigation skills	**Sample size:** One participant **Age and gender:** 20 year old female **BI type:** ABI – meningitis at six months old	**Country:** Italy **Setting:** Hospital setting, paper map based exercises as well as real navigation in the hospital (indoor and outdoor environments) **Recruitment:**	To describe the rehabilitation of a case study with topographical disorientation who had never learned to navigate **Design:** Pre/post intervention	**Rehabilitation Strategy and Outcome Measure:** The patient first underwent a battery of neuropsychological tests to assess her navigational impairment in experimental (paper-based) and ecological (hospital and community) environments. This included a mental rotation test, map drawing test, road map test, distance replication test, place learning test, Semmes test (translate a paper map into movement), map-based wayfinding task, route-based wayfinding task, landmark-based wayfinding task, landmark identification test. The patient was then trained on basic visuo-spatial abilities outside of route training over 10 sessions. She was then trained to use written instructions to follow a path using landmarks and to create those instructions herself. She then completed all of the navigation tests that she	The patient was able to navigate and orientate herself by using the trained strategies at the end of training (not sure how long this was). This result was maintained at the one year follow-up, at which time the patient was also able to reach locations in her community that she had never been to alone before. Her scores on the road map test, Semmes test and wayfinding map and route based tasks had improved. **Clinical Implications:** Patients' who have never developed the ability to navigate are able to learn and apply cognitive strategies to real world wayfinding with tailored

		Was referred to the researchers' rehabilitation centre for evaluation of her topographical disorder		completed pre-training as an outcome measure to test change in navigational abilities. Each test produced a pre/post score based on number of errors or number of correct responses.	rehabilitation programmes. The patient was able to develop her own instructions and follow them to learn new routes in the community – clinically meaningful in giving her independence.
Claessen et al. (2015). Navigation strategy training using virtual reality in six chronic stroke patients: a novel and explorative training to the rehab of navigation impairment.	**Sample size:** Six participants **Age and gender:** Four male, two female Mean age = 57 years old **BI type:** Chronic stroke	**Country:** The Netherlands **Setting:** Rehabilitation centre (Virtual outdoor environment and real outdoor environment for 3 participants) **Recruitment:** Recruited from a sample of 77 participants participating in another stroke study	To investigate the feasibility of a VR navigation training tool to instruct chronic stroke patients to adopt an alternative navigation strategy. **Design:** Pre/post intervention – performance on Virtual Tubingen Test	**Rehabilitation Strategy and Outcome Measure:** The navigation abilities of all six participants was assessed through a wayfinding questionnaire and scores on the Virtual Tubingen test battery. In the Virtual Tubingen test participants watched a short film of a route through the German city of Tubingen twice and were then tested on 10 tasks related to their knowledge of the route. The first four tasks tested aspects of route knowledge (knowledge from a street-level perspective) and six tasks test aspects of survey knowledge (knowledge from a bird's eye perspective). Participants completed a parallel equivalent route on the Virtual Tubingen test after training as a pre/post measure of navigation performance. Each participant's performance on the Virtual Tubingen test prior to training was interpreted by the researchers and translated into an individual profile of navigational strengths and weaknesses. Participants then completed four one hour training sessions which included feedback on their test performance and exercises to improve specific navigation abilities identified from their individual profiles. The exercises were completed in an interactive version of the Virtual Tubingen test where	Participant 6 had improved navigation abilities in general as measured by post Virtual Tubingen test scores. Participants 1, 3, 4 and 5 were in part successful in using their trained navigation strategy and improved on some navigation abilities. However, participants 1 to 5 performed worse on some of the Virtual Tubingen navigation tasks which included some that were targeted during training. **Clinical Implications:** The authors conclude that training a specific tailored navigation strategy to participant's deficits can be effective in helping them learn particular types of navigation abilities. However, the findings are very mixed as all participants other than participant 6 performed worse after training on some of the tasks that measured route knowledge and survey knowledge regardless of the strategy they had been trained in. If 5 out of 6 patients performed worse on some aspects of virtual navigation after training, is this a useful approach?

		participants could explore or follow routes in the virtual environment using a joystick. The profiles for participant's 1, 4 and 5 identified that they had impaired route knowledge and so they were trained to use a survey-based strategy to utilise their relatively good bird's-eye view knowledge. Firstly they were given a map with a route to follow and secondly they planned and drew a route onto the map themselves to follow. In the pointing exercises the participants were instructed to regularly point to the starting location as they progressed along the routes. Participant 1 also completed a training session using a map-based strategy in a real-world route. Participant 2's profile indicated that he only had impaired scene recognition and so he was taught a strategy to point back to the starting location as he progressed along the route (a survey strategy) to improve his sense of direction. A real-life route pointing exercise was also practiced. Participant 3 had impaired route knowledge as indicated by low scores on the scene recognition and route progression tasks. She was trained to adopt a survey map-based strategy where she would prepare routes on maps before following them in the virtual environment. Participant 6 was impaired at the route sequence and pointing to start tasks. She was trained to use a survey map-based strategy where she used a map to follow a route in the virtual environment and constructed a route watched in the virtual environment onto a map to follow. She was also trained on a real-life route where the start and end point had been given to her where she was asked to plan the route using a map focussing on landmarks along the way (using both route and survey knowledge).	It is not clear why some participants were trained in VR and the real-world. Although the authors attempt to explain the rationale for teaching each participant a specific strategy, it is not that clear or easy to follow. For example, the author's state that they taught participants 1, 3, 4 and 5 survey-based strategies due to impaired performance on the route knowledge tasks, however, results show that participants 1, 3 and 4 were impaired at survey tasks as well given poor performance on one or both of the map drawing or map recognition tasks. Only participant 6 performed well overall post training and she was the only participant who had been trained to develop a map in the real-world using landmarks she noticed along the way (a combined approach).

Lemoncello et al. (2010a). How best to orient travellers with ABI. A comparison of three directional prompts.	**Sample size:** 18 adults with ABI and 18 controls matched for gender, age and education. **Age and gender:** ABI mean age = 44.8 years, 6 females, 12 males Controls mean age = 43.6 years, 6 females, 12 males **BI type:** 9 TBI 3 stroke 3 tumour 2 anoxia 1 seizure disorder	**Country:** USA **Setting:** Downtown city location (outdoor environment) **Recruitment:** Recruited from local assisted living facilities	To compare the effects of written landmark, cardinal and left/right street directions on navigational success at the beginning of a walking route. **Design:** Mixed within and between subjects design	**Rehabilitation Strategy:** Participants completed three trials of each of four routes in a downtown city location. The three trials required participants to follow **written directions with either landmark, cardinal or left/ right directions**. Researchers positioned the participants at the start of each route. Participants wore a camera to record their performance for reliability purposes. A researcher also followed the participants at approximately 6 feet behind them to make observation notes. After each set of three direction prompts, the researcher provided the participant with the three written direction cards from that location and asked participants to rank them in order from the easiest to the hardest to follow. **Outcome Measure(s):** Dependent measures included route accuracy (number of errors), directness (level of hesitation), self-reported confidence and preference.	Participants with ABI produced more route following errors than controls when using cardinal and left/right directions. Both groups performed equally well with landmark-based directions. All participants preferred the landmark-based directions. **Clinical Implications:** Written landmark directions may be an effective strategy for teaching people with ABI relatively simple routes. Despite the potential equalizing benefits of landmark directions for improving orientation at the origin of a route, a major barrier to wide-spread implementation of landmark orienting cues is that of scale. Step by step landmark directions are time intensive. Most care providers do not have time to preview each novel route for an individual with ABI, determine relevant and salient landmarks and program these into an assistive navigational device or incorporate them into a list of written instructions. Each route in this study was only 3 turnings – very simple.
Environmentally-oriented strategies: Electronic devices					

Kirsch et al. (2004). Web based assistive technology interventions for cognitive impairments after TBI: a selective review and two case studies.	**Sample size:** One participant **Age and gender:** 19 year old man **BI type:** Severe topographical disorientation due to a TBI sustained in an RTA. Severe memory and EF deficits.	**Country:** USA **Setting:** Rehabilitation Unit (indoor environment) **Recruitment:** Part of his rehabilitation programme	To present two case studies using assistive technology interventions to address moderately complex sequential tasks (only one of these case studies is on navigation and will be included in the review) **Design:** ABA single-case design	**Rehabilitation Strategy:** A **web interactive assistant** was installed on the walls in the participant's rehabilitation facility. There were 30 possible routes through the facility. Each route had four or five choice points along the route. At each choice point, the participant was presented with verbal instructions to find the next coloured circle and press the button on the software when this was found. Each trial was a single day, consisting of those routes that comprised the participant's therapy schedule for that day. In the baseline phase, the researcher recorded the number of errors the participant made getting to each of his appointments and intervened to help him only after 5 minutes of unsuccessfully getting to his appointment. In the intervention phase, the researcher instructed the participant to follow the instructions on the iPaq screen (web interactive assistant). **Outcome Measure:** Number of errors made performing each route – making a wrong turning or reversing back	There was a significant reduction in route errors during the intervention phase with the assistive technology. There was evidence that procedural learning had taken place with reintroduction of baseline phase as his route errors did not return to baseline when the assistive technology was withdrawn. **Clinical Implications:** Web-based assistive technology may be useful to help teach relatively short simple routes around a rehabilitation facility where the technology can be applied to the walls. Not applicable to community route learning. Not generalisable to other routes. The participant was unaware of the aim of finding coloured circles to route learn – probably only useful for individuals with most severe cognitive impairments in inpatient facilities.
Sohlberg et al. (2007) A comparison of four prompt modes for route finding for community travellers with severe	**Sample size:** 20 participants **Age and gender:** Age ranged from 24-67 years, mean = 46.9 years, 15	**Country:** USA **Setting:** Downtown (outdoor environment) **Recruitment:**	To explore the relative merit of four prompt modes on a wrist-worn electronic assistive device. **Design:**	**Rehabilitation Strategy:** Wayfinding was tested along four equivalent 300m unfamiliar real life routes. Each route consisted of seven decision points. **Prompts were presented on a wrist-worn electronic device in the format of either: aerial map (bird's-eye view), point of view map (street-level perspective), written text, or auditory prompt.** Order of prompts and routes was counterbalanced.	Performance was better with auditory prompts than point of view and aerial map prompts. Other pairwise comparisons were not significant. The auditory prompts were most preferred by participants. **Clinical Implications:** Evidence that an auditory prompt may improve wayfinding performance. Need to consider whether strategies compete with task for cognitive resources. It appears that speech-based prompts may be the most effective form of

cognitive impairments	males, five females **BI type:** 18 TBI 2 stroke	Convenience sample – referred by service providers of local assisted living facilities	Within-subjects, repeated-measures	**Outcome Measure:** Measure was performance accuracy on each route. Five points were assigned for direct navigation to each choice point without error, extra cues or hesitation. The highest possible navigation score was 35. Participants were also asked about their preference for prompt mode.	navigation guidance for the majority of people with spatial cognitive impairments. The findings suggest that the effectiveness of prompt mode may be intricately linked to the cognitive requirements for an individual task.
Lemoncello et al. (2010b) When directions fail: investigation of getting lost behaviour in adults with acquired brain injury.	**Sample size:** 18 adults with ABI and 18 matched control participants (same participants as Lemoncello et al. 2010a) **Age and gender:** (see Lemoncello et al. 2010a) **BI type:** (see Lemoncello et al. 2010a)	**Country:** USA **Setting:** Downtown city location **Recruitment:** Recruited from local assisted living facilities	**Aim:** To investigate and describe getting lost behaviour and wayfinding strategies among acquired brain injury (ABI) participants and matched controls. **Design:** Matched control group comparison design (within and between subjects design accuracy score at each decision point treated as a within-	**Rehabilitation Strategy:** **Using a phone to request assistance if lost (electronic environmental strategy)** Both groups of participants were provided with written directions and a map to complete an eight turning route in a real town location. The written directions contained left/right turnings and landmark information such as street signs some of which were deliberately wrong to induce the participants to get lost. Participants' progress along the route was videoed and each participant was given a phone connected to a researcher who was unable to see the participant from whom they could request navigational assistance. The phone helper followed a script to provide assistance and gather information from participants who requested assistance. Participants were told to use the phone to request assistance if they became lost. They just needed to speak into the headset which was already connected. **Outcome Measure:** A researcher watched the videos and scored each participant's route performance for accuracy, wayfinding strategy and directness on a scale. A second-rater independently scored 50% of the videos with 88% agreement.	**Findings:** Participants with ABI demonstrated significantly more errors and hesitation than controls. The ABI group requested assistance over the phone more frequently than controls and required more attempts at re-orientation with concrete, salient directions. Participants in the control group anticipated errors with greater frequency than those with ABI. The phone assistance that was most helpful in re-orienting participants with ABI were specific instructions utilising explicit landmarks. **Clinical Implications:** Participants with ABI are able to request assistance on the phone to help them navigate with the assistance providing salient landmark information. However, the phones in this study were already connected so participants did not need to press buttons or navigate the phone in order to get help. Requires the person with ABI to pass on accurate information about the environment in order for the phone helper to give accurate information. There is also safety concerns as one participant in this study when on the phone requesting assistance had to be stopped by the researcher observing behind from walking into traffic.

			subjects measure).		
Hybrid compensatory approaches					
Rivest et al. (2016). A case study of topographical disorientation: behavioural intervention for achieving independent navigation.	**Sample size:** One participant **Age and gender:** 70 year old male **BI type:** TBI following a car accident	**Country:** Canada **Setting:** Local town (outdoor environment) **Recruitment:** Was attending a rehabilitation facility	To evaluate the effectiveness of an intervention to help a man with topographical disorientation to travel independently without fear of getting lost. **Design:** ABAB single-case design	**Rehabilitation Strategy:** **Hybrid approach**. There were two phases to learning to use the navigation app, a skill acquisition phase and a generalisation phase. In the skill acquisition phase, the participant was given step-by-step instructions in use of the smartphone navigation app guided by errorless-fading-of-cues. The software app was broken down into its individual steps and the trainer gradually faded guidance (verbal prompts and physical pointing) as steps were learned and entered independently by the patient, thereby reducing errors in using the app. It took 10 training sessions – two one-hour sessions a week for the participant to acquire and be able to apply the learned skills without prompting. In the generalisation phase the participant was asked to use the navigation app for upcoming outings, typically including a mixture of walking and taking public transport. 20 training sessions – two one-hour sessions a week – were necessary during the generalisation phase for the participant to confidently and independently use the smartphone navigation app. A single-case design (A1-B1-A2-B2) was used to assess the participant's navigation skills directly, without (A) and with (B) the **smartphone navigation app intervention** in several locations	The intervention enabled the patient to efficiently and confidently use his navigation smartphone app to cope with various way-finding challenges. Route efficiency improved from the baseline to intervention phases – fewer unnecessary changes and less looking at the app to check the way in the second intervention phase **Clinical Implications:** smartphone technology is effective for individuals who function at a high level in domains other than the one related to the impairment. It remains an open question whether individuals who are functioning at a lower level could also benefit from the programme. Despite these limitations, many individuals should benefit from this rehabilitation approach. In terms of the hybrid approach, the participant initially learned to use the app through errorless learning – not clear whether he would have been able to use the external smartphone app strategy if he had not first learned to use it through errorless learning. This study was time intensive (took a total of 5 ½ months to train the participant to use the navigation app). He had a google map printout in the non-intervention phases (survey/bird's-eye view strategy) but was

				within his city of residence. Phase A1 was the baseline phase and took place before the participant learned the navigation app. Phase B1 occurred after the skill acquisition and generalisation phases. In phase A2 the app intervention was withdrawn and then reinstated in phase B2. During each experimental phase, the participant was required to walk to three unknown locations and walk back to his starting location using a google printout map of the area. In phases B1 and B2 he had his smartphone navigation app which he could consult and in phases A1 and A2 he just had the printout map. All routes in all phases were matched for difficulty. Video and GPS technology was used to record the participant's navigation behaviour and accuracy and was independently coded by two raters. **Outcome Measure:** Pre/post Canadian Occupational Performance Measure Confidence in dealing with various navigational scenarios questionnaire Navigating in locations of varying familiarity questionnaire Route efficiency measured by number of surplus direction changes and number of times checking the app (coded by two independent-raters from video recordings – 94% agreement)	not able to use this strategy successfully. Needed the dynamic smartphone map which would move as he moved and show which direction he was headed (advantage of electronic devices over paper-based aids).
Kessels et al. (2007) Route learning in amnesia: a comparison of	**Sample size:** 10 patients	**Country:** The Netherlands	To test whether errorless learning is more effective than trial and error	**Rehabilitation Strategy:** **Hybrid approach**: Two routes were created in the hospital grounds equivalent for complexity and turnings (18). Photographs were made of landmarks	There was no significant difference in performance of the routes using errorless learning compared to trial and error. Four patients showed an advantage (fewer errors) in the trial and error condition. Two patients

trial and error and errorless learning in patients with Korsakoff's syndrome.	**Age and gender:** Mean age = 56.8 years (42-70) No gender data **BI type:** Korsakoff's syndrome	**Setting:** Clinic for Korsakoff's patients in a psychiatric hospital (outdoor environment within the hospital grounds) **Recruitment:** Convenience sample	learning for route learning. **Design:** Within-subjects	at each decision point and included in a booklet. Order of the two routes was counterbalanced. Each participant completed four learning trials of a route in errorless conditions followed by a test and four learning trials of a route in trial and error conditions followed by a test. In both conditions the researcher told the participants the end goal of the route and presented them with the photo booklet pointing out the landmark in the booklet and the real environment to make sure the participant's attention was focussed at the decision point. In the errorless condition participants were told at each decision point which way to go. In the errorful condition, participants were asked at each decision point to guess which way to go and were only allowed to continue moving once they had guessed correctly. The procedure in the test laps was the same for both conditions. The participant was told the end goal of the route, after which the patient had to walk the route. At each decision point, the experimenter presented the photographs of the decision point as a cue and asked the patient which way to go. If given an incorrect answer, the patient was corrected and the route was continued. **Outcome Measure:** Number of errors (incorrect direction answers) made when performing routes taught in errorful vs errorless conditions	showed an advantage for the errorless condition. Four patients showed equal performance. **Clinical Implications:** Errorless learning does not appear to be any more effective as a rehabilitation strategy for route learning than trial and error. However, is the errorful condition truly trial and error? Participants were not allowed to move forward until they guessed the correct way to go at a decision point so in a sense they were prevented from physically making errors in this condition. Instead it might be that participants were able to use an associative learning strategy using the photo landmark booklet at each decision point in both conditions. Patients with Korsakoff's syndrome may not be representative of the wider ABI population.

4.4 Person-Oriented Strategies – Alternative Neurological Systems

Three of the studies investigated the effectiveness of errorless learning as a rehabilitation strategy to improve route learning in people with ABI (Brooks et al., 1999; Evans et al., 2000; Lloyd et al., 2009). In errorless learning an individual acquires a skill through repeated exposure to the correct information without making errors (Baddeley & Wilson, 1994). Errorless learning is thought to facilitate the acquisition of information through engaging implicit procedural memory systems which are often relatively intact after an ABI (Evans et al., 2000; Maxwell, Masters, Kerr & Weedon, 2001). This is compared to explicit memory which is often impaired after ABI and is thought to be necessary for an individual to learn through making errors (Evans et al., 2000; Baddeley and Wilson, 1994). Thus, errorless learning is a person-oriented rehabilitation strategy that may help an individual with ABI route learn through engaging an alternative neurological system, namely implicit memory.

Brooks et al. (1999) conducted a single-case experimental study which demonstrated that a woman with severe amnesia was able to perform routes in her real hospital unit that she had been trained to perform using an errorless learning technique in VR. A virtual learning environment is free of competing demands on cognitive resources like noise that are often present in the real-world and so VR may more easily allow for the implicit errorless learning of routes (Lloyd et al., 2009b). Furthermore, the finding that the patient was able to transfer her learning of the routes trained in VR to the real unit and that learning was maintained two weeks after training suggests that errorless learning through VR is a promising strategy for learning relatively simple routes in people with severe memory deficits.

Building on this research, Lloyd et al. (2009b) demonstrated an advantage for errorless learning of routes in VR compared to errorful learning in an experimental group study. Fourteen of the 20 participants with ABI showed a route recall advantage in the errorless

condition and six participants showed an advantage in the errorful condition. Lloyd et al. (2009b) noted that participants who benefitted from errorful learning showed improvements across learning trials, displaying explicit memory of errors and an ability to self-correct. However, participants who benefitted from errorless learning did not show explicit recall of errors. Thus, the authors concluded that errorless learning may be an effective rehabilitation strategy for individuals with ABI who have impaired explicit memory and who need to use an alternative neurological system to route learn i.e. implicit learning through not making errors.

Conversely, in a series of paper room diagram and stepping stone maze experiments, Evans et al. (2000) did not find an advantage for errorless over errorful route learning. Nevertheless, questions have been raised about the ecological validity of these paper-based experiments. Specifically, route learning on paper may not use implicit, procedural memory systems as implicit spatial learning may require participants to see movement through a 3D environment as would be the case in VR and the real-world (Hegarty, Montello, Richardson, Ishikawa, & Lovelace, 2006). Real-life wayfinding impairments do not necessarily correspond to impairments on paper tests of spatial memory (Aguirre & D'Esposito, 1997). Thus, these paper-based experiments may have relied on explicit memory with little ecological validity to real-world route learning, weakening the conclusions in this study.

Of these three studies, only the study by Lloyd et al. (2009b) achieved a positive rating for both internal and external validity (see Table 1 and Appendices E and F). As well as poor external validity, the Evans et al. (2000) study is weakened by the poor internal validity of the ABI sample. Specifically, each experiment had a different sample size and the nature of cognitive impairments in each participant sample was not stated. As there is research to suggest that errorless learning may be more or less effective depending on the nature and severity of cognitive impairment (Clare & Jones, 2008), differences in the characteristics of the sample of participants who took part in each experiment may confound the results of this study.

The Brooks et al. (1999) study scored particularly poorly for internal validity as assessed by the RoBiNT Scale and this is due to limitations in the experimental control of the single-case design (see Table 1 and Appendix E). This study implemented a within-subjects comparison where performance on routes taught using errorless learning in VR was compared to performance of routes that were not taught following a baseline phase which demonstrated that the patient had been unable to perform all routes prior to training. The addition of control routes that had not been taught and a baseline phase improves the internal validity of the conclusion that errorless learning using VR was an effective route learning strategy. However, the study scores poorly due to lack of data points per phase, lack of randomisation and lack of an independent second rater, all factors of single-case experimental designs which improve experimental control and thus the ability to determine cause and effect (Tate, Perdices, McDonald, Togher & Rosenkoetter, 2014).

In summary there was evidence both for and against errorless learning as an effective route learning strategy and this may be due to differences in whether routes are taught using paper diagrams, VR or the real-world. Nevertheless, the most robust evidence is provided by Lloyd et al. (2009b) which suggests that errorless learning using VR may be a particularly effective strategy. Nevertheless, the conclusions of this study are also limited by the lack of a delayed recall route or a generalisation measure of route learning to show that people with ABI can recall the routes at a later point in the real-world. Thus, further research is needed before errorless learning using VR can be considered a clinically effective strategy.

4.5 Person-Oriented Strategies – Unassisted

Two research studies in this review sought to investigate the effectiveness of a person-oriented help-seeking strategy to improve wayfinding in people with ABI (Cho & Sohlberg, 2015; Cho et al. 2017). Cho and Sohlberg (2015) piloted the effectiveness of a group therapy

protocol to improve help-seeking behaviour when people with TBI have lost their way whilst navigating. After completing the manualised NICE social skills group protocol, three participants with TBI demonstrated improved wayfinding independence and efficiency as measured by improved performance on the EFRT task. Cho et al. (2017) then repeated this research in seven participants with ABI using single-case experimental design methodology and found that participants improved in help-seeking, wayfinding independence and efficiency after completing the NICE social skills group intervention. The authors suggest that a group therapy programme targeting help-seeking can therefore be an effective rehabilitation strategy for improving wayfinding in people with ABI.

However, the study by Cho and Sohlberg (2015) scores poorly on the RoBiNT Scale for internal validity due to lack of experimental control (see Table 1 and Appendix E). As a pre/post design, it makes it hard to rule out the effect of other confounding factors on the improvement in help-seeking and wayfinding such as the passing of time or increased exposure to peers who share similar experiences. Nevertheless, the strength of the conclusion that the NICE social skills group was an effective wayfinding intervention was improved by replication of the findings in a more robust single-case experimental design by Cho et al. (2017). This study achieved a score of eight out of 16 for internal validity on the RoBiNT Scale due to the application of a multiple-baseline design in which it was demonstrated across participants that scores for the three behaviours of help-seeking, wayfinding independence and wayfinding efficiency significantly increased from the baseline to the NICE group intervention phase. Furthermore, this finding was replicated across three groups of participants strengthening its reliability. Nevertheless, this study still falls short on the RoBiNT Scale due to a lack of randomisation and blinding of phases.

In terms of external validity, both studies scored highly due to their detailed descriptions of the participant characteristics, the setting, the NICE group intervention and the

standardised measurement of help-seeking and wayfinding. This is a strength of this research as it allows for the study to be replicated in a larger sample of people with ABI, which is necessary given the small sample sizes. Furthermore, the EFRT measure assessed wayfinding in the real-world and the help-seeking strategy is not limited to the learning of a set route, and so the research has good ecological validity and generalisability. Nevertheless, a concern regarding the external validity of the Cho et al. (2017) study is the all-female sample. Men have been found to have greater impairments in social skills following an ABI than women (Turkstra, 2008) and thus, it may be that the women with ABI in this study were better able to learn help-seeking to improve wayfinding than a typical male population with ABI. Overall therefore, these studies provide preliminary evidence that wayfinding in people with ABI may be improved by increasing internally-generated help-seeking behaviours as taught through a manualised social skills group intervention. However, the strength of this evidence would benefit from replication in a larger sample, particularly in men with ABI.

4.6 Environmentally-Oriented Strategies – Paper-based Aids

People often use paper-based aids to help them navigate, such as, having a map of the environment to help them build a mental map of their position in space in order to plan how to get to a target location (Claessen et al., 2015). A map navigation tool requires survey (bird's-eye view) knowledge of a spatial environment (Latini-Corazzini et al., 2010). Alternatively, people may follow written directions utilising left/right, cardinal or landmark information helping them to navigate from a route (street-level) perspective.

Three of the N-of-1 studies investigated the effectiveness of a written direction sheet to help people with ABI learn personally-meaningful routes in their communities (Newbigging & Laskey, 1996; Bouwmeester et al., 2014; Incoccia et al., 2009). In the case study by Inoccia et al. (2009) a young woman who had never been able to navigate learned to use written

landmark-based directions to accurately navigate routes in her community, increasing her independence. Furthermore, Bouwmeester et al. (2014) supported a man with severe navigation impairment to learn routes in his community by teaching him to associate landmarks with written directions. These studies appear to provide promising evidence that people with ABI can learn routes when supported to use a specific written landmark-based direction strategy.

Nevertheless, the strength of this conclusion is limited by the lack of experimental control in pre/post and case report designs and as a result these studies scored poorly for internal validity on the RoBiNT Scale (see Table 1 and Appendix E). However, ecological validity is a strength of this research as the participants were taught personally-meaningful routes in their communities. Yet, the practicality of neuro-rehabilitation services being able to dedicate the time to train specific paper-based strategies is questionable. For example, in Bouwmeester et al. (2014), an Occupational Therapist spent an average of 10 weekly two-hour sessions to teach a short route and 18 weekly two-hour sessions to teach a long route for a total of six routes, placing great demand on clinical time.

Building on this N-of-1 research, Lemoncello et al. (2010a) conducted an experimental group study comparing the effectiveness of written left/right directions, cardinal directions and landmark-based directions on route learning in a sample of adults with ABI and matched controls. The authors found that the group with ABI performed worse on the routes learned using cardinal and left/right directions but equally as well as the controls using landmark directions. As this study used a between-subjects experimental design with a matched control comparison group, it appears to add weight to the conclusion that written landmark directions can be an effective route learning strategy. Nevertheless, this study achieved a negative rating for both internal and external validity on the NICE checklist which diminishes this conclusion (see Table 1 and Appendix F). For example, 16 of the 18 participants with ABI had co-morbid psychiatric diagnoses including depression and schizophrenia and these diagnoses have been

associated with cognitive deficits (Tripathi, Kumar Kar & Shukla, 2018; Solé et al., 2017; Lam, Kennedy, McIntyre & Khullar, 2017). Therefore it is difficult to infer a relationship between ABI and impaired performance in route learning using cardinal and left/right directions as the deficits may have been linked to the cognitive effects of the psychiatric conditions.

Only Claessen et al. (2015) trained participants to use survey strategies to navigate as opposed to a strategy that supports route knowledge from a street-level perspective. In this series of pre/post case studies, six participants' route and survey navigation abilities was tested pre/post-training using the Virtual Tubingen Test. Participants were taught a variety of strategies designed to support the acquisition of survey knowledge including following a route on a map and pointing back to the start location as they progressed along the route. The specific strategies were chosen depending on each participant's profile of navigation strengths and weaknesses. Despite the tailored training approach used, participants' performance on the Virtual Tubingen Test post-training was mixed. For example, participant five performed worse on measures of 'map drawing' and 'pointing to start' post-training and participant three performed worse on measures of 'map recognition' and 'pointing to start' despite being trained in these abilities. Participant six was the only participant who was taught to develop a map using landmarks she noticed as she practiced a real-world route and she was also the only participant who performed better post-training on the Virtual Tubingen tasks measuring both route and survey knowledge. It is possible that only participant six improved because she was trained to use a strategy combining survey map knowledge with route landmark knowledge. It can therefore be questioned as to whether teaching people with ABI to solely use survey navigation strategies would be an effective rehabilitation approach.

Nevertheless, any conclusions that can be made on the basis of the results of the Claessen et al. (2015) study is limited by poor internal and external validity (see Table 1 and Appendix E). This study is a series of pre/post cases which implemented different strategies

making comparison and generalisability difficult. Furthermore, although the authors provide a rationale for the strategy taught to each participant it is not clear why three of the participants were given training in the real-world in addition to VR and three were not. This lack of clarity makes replication difficult.

Overall, although there is evidence that training people with ABI to use a paper-based landmark strategy to route learn may be effective, the evidence to date is not of sufficient quality to derive conclusions on which to base the rehabilitation of navigation impairment.

4.7 Environmentally-Oriented Strategies - Electronic Devices

Three of the studies investigated the use of electronic devices to improve navigation (Kirsch et al., 2004; Sohlberg et al., 2007; Lemoncello et al., 2010b). Similar to paper-based navigation strategies, an electronic device acts as an external cue and reduces the cognitive demands of the navigation task. Kirsch et al. (2004) found that an electronic assistive technology intervention helped a man with severe navigation impairments learn routes around his rehabilitation unit. Moreover, Lemoncello et al. (2010b) found that participants with ABI were able to use a phone to request assistance when they had become lost whilst navigating. The participants with ABI in this study were most successful at re-orienting themselves in the route when the phone helpers provided landmark cues. Furthermore, in Sohlberg et al. (2007) participants with ABI made fewer errors route learning using auditory prompts than survey map (bird's-eye view) and route map (street-level) prompts. All modalities of prompts were presented via a wrist-worn electronic device. Sohlberg et al. (2007) suggested that the auditory prompts were more effective than visual prompts as they compete less for cognitive resources during navigation which is primarily a visual task. The authors of these three studies conclude that electronic devices may be effective in improving navigation in people with ABI.

Nevertheless, the Kirsch et al. (2004) study scored poorly for internal validity on the RoBiNT Scale (see Table 1 and Appendix E). This study implemented a single-case design with three phases (ABA). Kirsch et al. (2004) reported that the purpose of the second 'A' phase was to establish a return to baseline in order to demonstrate an experimental effect of the electronic intervention on route learning. However, Tate et al. (2015) argue that ABA designs may not be valid in neuro-rehabilitation research as it is often not possible to withdraw learning from a rehabilitation intervention. Thus, as the participant's route performance did not return to baseline when the electronic device was withdrawn, this suggests that learning had taken place and makes it impossible to differentiate the cause of change from other confounding variables such as the passing of time. However, this study scored well for external validity due to the detailed descriptions of the setting and intervention, allowing for replication which would be necessary given the low internal validity. Moreover, the electronic device was applied to the walls of the participant's rehabilitation unit and as such, this particular electronic intervention may only be generalisable to learning relatively simple routes in indoor environments.

The internal and external validity of the Lemoncello et al. (2010b) study can also be questioned (see Table 2 and Appendix F). Participants in this study were deliberately given wrong directions which leads one to question whether participants with ABI would have needed to externally phone for assistance had they been given accurate directions initially and thus to the validity of whether help-seeking through phoning another person would be a useful strategy. Furthermore, the effectiveness of this strategy as with the help-seeking strategies described by Cho and Sohlberg (2015) and Cho et al. (2017) relies on the availability, route knowledge and willingness to help of the people who are asked to provide assistance. Thus, this could be a limiting factor of relying on this approach for navigation rehabilitation purposes.

On the other hand, the study by Sohlberg et al. (2007) appears to be more methodologically robust in demonstrating the effectiveness of electronic auditory navigation

prompts specifically. This study used a within-subjects design comparing route performance across three conditions where the order of route and prompts was counterbalanced to control for confounding variables such as practice and fatigue. Furthermore, the routes were learned in the real-world giving the study good ecological validity. The navigation prompts were presented to participants by a wrist-worn electronic device which is portable and practical for learning routes in outdoor environments. Thus, this study scored well for internal and external validity on the NICE checklist (see Table 1 and Appendix F).

In summary, three studies investigated the effectiveness of an electronic device to improve navigation, including an assistive handheld computer device (Kirsch et al., 2004), a phone (Lemoncello et al., 2010b) and a wrist-worn computer device (Sohlberg et al., 2007). However, as these electronic devices were very different they may only be applicable to certain environments and phases of rehabilitation (i.e. inpatient vs community). Overall, research investigating electronic devices for navigation rehabilitation is lacking and the research that has been conducted is of questionable quality. The most robust evidence was provided by Sohlberg et al. (2007) and therefore the effectiveness of electronic devices providing auditory prompts for navigation may warrant further investigation.

4.8 Hybrid Compensatory Strategies

Two studies investigated the effectiveness of an errorless learning strategy to support participants to learn to use either an electronic or paper-based external cue when navigating, a hybrid approach to navigation rehabilitation (Rivest et al., 2016; Kessels et al., 2007).

Rivest et al. (2016) used an ABAB single-case design to demonstrate that a participant with ABI was more efficient at wayfinding using a dynamic smartphone navigation app as an external aid than in the baseline phases when only a static paper map of the environment was available. Before the intervention phases commenced, the participant had been taught how to

use the navigation app through errorless learning. The authors conclude that smartphone technology can be used to improve navigation in people with ABI. The particular finding that the participant was more efficient at wayfinding using the navigation app which tracked his movement through the environment than using paper maps suggests there may be an advantage for dynamic electronic devices over static paper maps. However, it is not clear whether the participant would have been able to use the navigation app independently if it were not for the errorless learning strategy. Thus, the effectiveness of the electronic strategy may rely first on errorless teaching of people with ABI to use electronic devices using implicit memory.

Nevertheless, the Rivest et al. (2016) study is weakened by low internal validity in the study design (see Table 1 and Appendix E). Although a four phase design (ABAB) appears to improve experimental control there was only one data point per phase. This reduces the reliability of the conclusion that wayfinding improved due to the smartphone navigation app as it is not possible to demonstrate a pattern from one data point per phase. Furthermore, Rivest et al. (2016) do not give a clear rationale for why they implemented a smartphone navigation app strategy specifically. Without stating previous research or theory for why a particular strategy was chosen, it is hard to comment on the validity of the conclusions in this study.

The other study to use a hybrid navigation rehabilitation approach was by Kessels et al. (2007). In this within-subjects experiment, no difference was found in route performance when participants with Korsakoff's syndrome were taught two equivalent routes, one using an errorless strategy and the other using trial and error. In both conditions participants were presented with a photograph of a landmark at each decision point which corresponded to a landmark in the real route. In the errorless condition participants were told which way to go after looking at the landmark at each decision point and in the trial and error condition participants were asked to guess which way to go after looking at the landmarks and were not

allowed to progress to the next decision point until they had guessed correctly. Kessels et al. (2007) argue that errorless learning does not appear to be an effective route learning strategy.

Nevertheless, this study scores poorly for both internal and external validity (see Table 1 and Appendix F). Whether the errorful condition is truly trial and error can be questioned. Specifically, participants were prevented from physically trying a turning and thus making an error in the errorful condition as participants stayed with the researcher at each decision point until they had verbally guessed the correct direction. Moreover, the findings in this study may only be generalisable to patients with Korsakoff's syndrome and not to other people with ABI. Korsakoff's syndrome is associated with severe impairment in episodic memory and lack of insight into deficits which may not be generalisable to people with ABI of other aetiologies (Arts, Walvoort & Kessels, 2017).

Overall, it is not possible to draw conclusions as to the effectiveness of a hybrid rehabilitation approach for navigation impairment due to the lack of research and concerns over the quality of the evidence presented by Rivest et al. (2016) and Kessels et al. (2007). Furthermore, a limitation of the hybrid research is that it is not possible to know the relative contribution of each strategy on navigation improvement i.e. whether the participants would have improved with the implementation of just one of the strategies or whether a combined approach was needed.

5. Discussion

The first two objectives of this review were to investigate whether there is research evaluating the effectiveness of rehabilitation strategies for navigation impairment in adults with ABI and to describe the range of compensatory strategies used. In answer, a total of 15 studies were found that met the inclusion criteria across the five categories of compensatory rehabilitation strategy (see Table 2 for a description of the studies).

The aim was to then discuss the study findings and evaluate the quality of the evidence in order to ascertain whether it is possible to reach a conclusion which may inform guidelines for the rehabilitation of navigation impairment. Overall, it was found that the studies differed greatly in terms of the strategy used, the sample, the medium of strategy training and testing, the methodology and the quality, not only between but within each category of compensatory strategy (see Table 2). This makes it difficult to compare and combine the research into a meaningful conclusion. For example, although five studies evaluated the effectiveness of a paper-based environmentally-oriented strategy and appeared to show that a landmark-based strategy may be an effective navigation rehabilitation approach, the five studies varied in methodology and were of questionable quality, limiting the strength of this conclusion.

The majority of the research studies scored poorly on the RoBiNT Scale and the NICE checklist for internal and external validity (see Table 1 and Appendices E and F). The methodological limitations in terms of internal validity included not having a clear rationale for the rehabilitation strategy used, samples with co-morbid psychiatric diagnoses, and poor experimental control (e.g. Lemoncello et al., 2010a; Bouwmeester et al., 2014; Rivest et al., 2016). These limitations cast doubt on whether improvement in navigation ability is due to the rehabilitation strategies studied or a confounding factor like the passing of time or particular characteristics of the specific sample selected. Furthermore, the external validity of many of the studies can be questioned in terms of the ecological validity of the navigation tasks (e.g. Evans et al., 2000) as well as the generalisability of the study findings to all people with ABI (e.g. Kessels et al., 2007).

Nevertheless, there were three studies which achieved a reasonable rating for both internal and external validity (see Table 1) and from which it may be appropriate to draw specific conclusions. The findings from the experimental group study by Lloyd et al. (2009b) suggests that errorless learning through the medium of VR may be effective for improving

route learning in people with more severe impairments who are unable to route learn using explicit memory. Furthermore, Cho et al. (2017) demonstrated that women with ABI may be able to use an internally-generated help-seeking strategy to help them navigate when lost. Lastly, Sohlberg et al. (2007) found effectiveness for auditory direction prompts delivered through an electronic wrist-worn device to improve route learning.

As stated in the introduction, it may be that it is particularly difficult to find a navigation strategy that will be effective for all adults with ABI due to the complexity of navigation and heterogeneity of ABI. For example, it may not be valid to compare and combine research that has evaluated the effectiveness of a strategy to rehabilitate navigation impairment in indoor environments such as a rehabilitation/residential facility to a strategy designed to help people navigate outdoors once they are living back in the community (e.g. Kirsch et al., 2004; Rivest et al., 2016). Furthermore, the effectiveness of a particular rehabilitation strategy may depend upon the severity of impairment and may therefore only be applicable to certain groups of people with ABI. For example, an errorless learning strategy may be suitable for people who cannot learn explicitly through making errors but not to people with milder impairments who are relatively spared in this ability (Evans et al., 2000; Lloyd et al., 2009b). Furthermore, use of an electronic navigation app may only be appropriate for people who have less severe impairments due to the need to manage the app whilst navigating in an environment with real dangers such as moving traffic (Rivest et al., 2016). These differences highlight the difficulties in generating a 'one size fits all' strategy to navigation rehabilitation and indicates the need for services to have a range of evidence-based strategies in their toolbox so that navigation rehabilitation can be tailored to the differing needs of service-users with ABI.

Another factor limiting the effectiveness of navigation rehabilitation strategies is the individuals own strategy preference. There is evidence to show that there is variation in the navigation strategies that people choose including differences related to sex and most studies

do not take this into account (Bohbot, Iaria & Petrides, 2004; Picucci, Caffo & Bosco, 2011). In Sohlberg et al. (2007), participants rated prompts delivered from an aerial map (survey) perspective as their least preferred modality as they found it difficult to identify how the survey pictures related to where they were walking and route performance was worse in this modality. Participants in the Lemoncello et al. (2010a) study performed well in the landmark strategy condition and rated this as their preferred strategy compared to left/right or cardinal written directions. Thus, preference needs to be considered when deciding upon the particular navigation strategy that might be effective for each service-user.

Moreover, the low experimental control in the N-of-1 studies may reflect an overall weakness of neuro-rehabilitation research as opposed to being a specific criticism of the navigation studies. For example, Tate et al. (2015) found that only 44% of a randomly selected sample of 235 published neuro-rehabilitation papers used a single-case design with experimental control. Tate et al. (2015) argue that it is possible to improve the experimental control of neuro-rehabilitation single-case designs but recognise that to do so would require the recruitment of significant time and resources at a financial cost to services. The standard for an intervention evaluated through single-case methodology to be considered 'evidence-based' is five separate single-case experiments with at least three phases, five data points per phase, independence and blinding of assessors and raters, conducted by at least three separate research groups in at least three geographical areas totalling at least 20 participants (Kratochwill et al., 2010; 2013). Nevertheless, Tate et al. (2015) argue that as robust N-of-1 designs have been added to level one in the hierarchy of evidence (OCEBM Levels of Evidence Working Group, 2011, see Appendix H), it would be worthwhile for neuro-rehabilitation services to conduct research using this methodology in order to develop a meaningful evidence-base for clinical practice.

5.1 Future Research

It may be appropriate to conduct future research specifically building on the findings of the three research studies evaluated in this review as being of good quality. For example, further research could explore the effectiveness of the NICE group intervention to facilitate navigation through a help-seeking strategy in a larger ABI sample of mixed sex (Cho et al., 2017). Furthermore, the studies by Lloyd et al. (2009b) and Sohlberg et al. (2007) suggest that strategies which reduce competition for cognitive resources during navigation may warrant further investigation. For example, errorless learning of routes through VR could be further investigated to test whether route learning in an environment free of distractions like noise generalises to the real-world and is maintained over time (Lloyd et al., 2009b). Moreover, the electronic auditory prompts used in the Sohlberg et al. (2007) study may have been successful for route learning due to auditory stimuli not competing for visual resources during spatial learning and this study would therefore benefit from replication in a larger sample of people with ABI.

5.2 Limitations

There are also limitations of this systematic review to acknowledge. A combined search was conducted of specific headings and key words in four different databases; however, it is possible that some studies were missed, particularly those published in a non-English language. Furthermore, there is a risk of publication bias due to the inclusion of only published papers. Studies with statistically significant results are more likely to be published which could skew the results (Easterbrook, Gopalan, Berlin, & Matthews, 1991).

5.3 Conclusion

Overall, more research is needed into the effectiveness of compensatory rehabilitation strategies for navigation impairments in people with ABI. Research needs to be conducted that has greater experimental control through the use of group and robust single-case experimental design methodologies. Until there is more robust research evidence it will not be possible to

develop clear clinical guidance for services on how best to rehabilitate navigation impairments in people with ABI. Consequently, the independence and quality of life of people with ABI will continue to be limited by navigation impairments.

6. References

Aguirre, G. K., & D'Esposito, M. (1997). Environmental knowledge is subserved by separable dorsal/ventral neural areas. *Journal of Neuroscience, 17,* 2512–2518

Aguirre, G. K., & D'Esposito, M. (1999). TD: a synthesis and taxonomy. *Brain, 122(9),* 1613–1628. doi: 10. 1093/brain/122.9.1613

Anson, K., & Ponsford, J. (2006). Evaluation of a coping skills group following traumatic brain injury. *Brain Injury, 20(2),* 167-178.

Arciniegas, D. M., Held, K., & Wagner, P. (2002). Cognitive impairment following traumatic brain injury. *Current Treatment Options in Neurology, 4(1),* 43-57.

Arts, N. J. M., Walvoort, S. J. W., & Kessels, R. P. C. (2017). Korsakoff's syndrome: a critical review. *Neuropsychiatric Disease and Treatment, 13,* 2875-2890.

Baddeley, A., & Wilson, B. (1994). When implicit learning fails: Amnesia and the problem of error elimination. *Neuropsychologia, 32,* 53–68.

Barrash, J., Damasio, H., Adolphs, R., & Tranel, D. (2000). The neuroanatomical correlates of route learning impairment. *Neuropsychologia, 38,* 820–836.

Bohbot, V., Iaria, G. & Petrides, M. (2004). Hippocampal function and spatial memory: evidence from functional neuroimaging in healthy participants and performance of patients with medial temporal lobe. *Neuropsychology, 18(3),* 418-425. Retrieved from http://psycnet.apa.org/journals/neu/18/3/418/

Bouwmeester, L., Van de Wege, A., Haaxma, R., & Snoek, J. W. (2015). Rehabilitation in a complex case of topographical disorientation. *Neuropsychological Rehabilitation, 25(1),* 1–14. doi:10.1080/09602011.2014.923318

Brooks, B. M., McNeil, J. E., Rose, F. D., Greenwood, R. J., Attree, E. A., & Leadbetter, A. G. (1999). Route learning in a case of amnesia: a preliminary investigation into the

efficacy of training in a virtual environment. *Neuropsychological Rehabilitation, 9(1),* 63–76. doi: 10. 1080/713755589

Brunsdon, R., Nickels, L., & Coltheart, M. (2007). Topographical disorientation: Towards an integrated framework for assessment. *Neuropsychological Rehabilitation, 17(1),* 34–52. doi:10.1080/09602010500505021

Cho, Y. S., & Sohlberg, M. M. (2015). Training adults with brain injury how to help-seek when lost: A pilot study. *Brain Impairment, 16(2),* 90–103.

Cho, S. Y., Sohlberg, M. M., Albin, R., Diller, L., Horner, R., Rath, J., & Bullis, M. (2017). Training adults with acquired brain injury how to help seek when wayfinding: an understudied critical life skill. *Neuropsychological Rehabilitation, 6,* 928-945. doi: 10.1080/09602011.2017.1344131

Claessen, M. H. G., van der Ham, A. J. M., Jagersma, E., & Visser-Miely, J. M. A. (2015). Navigation strategy training using virtual reality in six chronic stroke patients: a novel and explorative approach to the rehabilitation of navigation impairment. *Neuropsychological Rehabilitation, 26(5-6),* 822-46. doi: 10.1080/09602011.2015.1045910

Clare, L., & Jones, R. S. P. (2008). Errorless learning in the rehabilitation of memory impairment: a critical review. *Neuropsychology Review, 18(1),* 1-23. doi: 10.1007/s11065-008-9051-4

Deipolyi, A. R., Rankin, K. P., Mucke, L., Miller, B. L., & Gorno-Tempini, M. L. (2007). Spatial cognition and the human navigation network in AD and MCI. *Neurology, 69(4),* 986–997.

Easterbrook, P. J., Gopalan, R., Berlin, J. A., & Matthews, D. R. (1991). Originally published as Volume 1, Issue 8746, Publication bias in clinical research. *The Lancet, 337*(8746), 867–872. doi: 10.1016/0140-6736(91)90201-Y

Ekstrom, A., Copara, M. S., Isham, E. A., Wang, W., & Yonelinas, A. P. (2011). Dissociable networks involved in spatial and temporal order source retrieval. *Neuro Image, 56,* 1803– 1813. doi:10.1016/j.neuroimage.2011.02.033

Evans, J. J., Wilson, B. A., Schuri, U., Andrade, J., Baddeley, A., Bruna, O., …. Taussik, I. (2000). A comparison of "errorless" and "trial-and-error" learning methods for teaching individuals with acquired memory deficits. *Neuropsychological Rehabilitation, 10(1), 67-101,* doi: 10.1080/096020100389309

Graf, P., & Schacter, D. L. (1985). Implicit and explicit memory for new associations in normal and amnesic subjects. *Journal of Experimental Psychology, Learning, Memory and Cognition, 11(3),* 501-518.

Hegarty, M., Montello, D. R., Richardson, A. E., Ishikawa, T., & Lovelace, K. (2006). Spatial abilities at different scales: Individual differences in aptitude-test performance and spatial layout learning. *Intelligence, 34,* 151–176.

Incoccia, C., Magnotti, L., Iaria, G., Piccardi, L., & Guariglia, C. (2009). Topographical disorientation in a patient who never developed navigational skills: the (re)habilitation treatment. *Neuropsychological Rehabilitation, 19(2),* 291–314. doi: 10.1080/0960201080 2188344

Juengst, S. B., Arenth, P. M., Raina, K. D., McCue, M., & Skidmore, E. R. (2014). Affective state and community integration after traumatic brain injury. *American Journal of Physical Medicine and Rehabilitation, 93(12),* 1086-1094. doi: 10.1097/PHM.0000000000000163

Kelly, D. M., & Gibson, B. M. (2007). Spatial navigation: spatial learning in real and virtual environments. *Spatial Learning, 2,* 111-124.

Kessels, R. P. C., van Loon, E., & Wester, A. J. (2007). Route learning in amnesia: a comparison of errorless and trial and error learning in patients with the Korsakoff syndrome. *Clinical Rehabilitation, 21,* 905-911. doi: 10.1177/0269215507077309

Kirsch, N., Shenon, M., Spirl, E., Rowan, J., Simpson, R., Schreckenghost, D., & LoPresti, E. F. (2004). Web-based assistive technology interventions for cognitive impairments after traumatic brain injury: a selective review and two case studies. *Rehabilitation Psychology, 49,* 200–212.

Kratochwill, T. R., Hitchcock, J., Horner, R. H., Levin, J. R., Odom, S. L., Rindskopf, D. M., & Shadish, W. R. (2010). Single-case designs technical documentation. Retrieved from http:// ies.ed.gov/ncee/wwc/pdf/wwc_scd.pdf

Kratochwill, T. R., Hitchcock, J., Horner, R. H., Levin, J. R., Odom, S. L., Rindskopf, D. M., & Shadish, W. R. (2013). Single-case intervention research design standards. *Remedial and Special Education, 34(1),* 26–38.

Labate, E., Pazzaglia, F., & Hegarty, M. (2014). What working memory subcomponents are needed in the acquisition of survey knowledge? Evidence from direction estimation and shortcut tasks. *Journal of Environmental Psychology, 37,* 73–79. DOI:10.1016/j.jenvp.2013.11.007.

Lam, R. W., Kennedy, S. H., McIntyre, S. H., & Khullar, A. (2014). Cognitive dysfunction in major depressive disorder: effects on psychosocial functioning and implications for treatment. The Canadian Journal of Psychiatry, 59(12), 649-654. doi: 10.1177/070674371405901206

Latini-Corazzini, L., Pascale Nesa, M., Ceccaldi, M., Guedj, E., Thinus-Blanc, C., Cauda, F., ... Pe´ruch, P. (2010). Route and survey processing of topographical memory during navigation. *Psychological Research, 74(6),* 545–559. doi:10.1007/s00426-010-0276-5

Lemoncello, R., Sohlberg, M. M., & Fickas, S. (2010a). How best to orient travellers with acquired brain injury: a comparison of three directional prompts. *Brain Injury, 24(3),* 541-549. doi: 10.3109/02699051003610425

Lemoncello, R., Sohlberg, M. M., & Fickas, S. (2010b). When directions fail: Investigation of getting lost behaviour in adults with acquired brain injury. *Brain Injury, 24(3),* 550-559, doi: 10.3109/02699050903446807

Livingstone, S. A., & Skelton, R. W. (2007). Virtual environment navigation tasks and the assessment of cognitive deficits in individuals with brain injury. *Behavioural Brain Research, 185,* 21-31.

Lloyd, J. Riley, G. A., & Powell, T. E. (2009). Errorless learning of novel routes through a virtual town in people with acquired brain injury. *Neuropsychological Rehabilitation, 19(1),* 98-109. doi: 10.1080/09602010802117392.

Lynch, K. (1960). The Image of the City. Cambridge, Massachusets: MIT Press

Maxwell, J. P., Masters, R. S. W., Kerr, E., & Weedon, E. (2001). The implicit benefit of learning without errors. *Quarterly Journal of Experimental Psychology, 54A,* 1049–1068.

Mazaux, J. M., & Richer, E. (1998). Rehabilitation after traumatic brain injury in adults. *Disability and Rehabilitation, 20(12),* 435-447. doi: 10.3109/09638289809166108

McLeod, S. (2013). What is validity? Retrieved from https://www.simplypsychology.org/validity.html

Nadar, M. S., & McDowd, J. (2010). Comparison of remedial and compensatory approaches in memory dysfunction: a comprehensive literature review. *Occupational Therapy in Health Care, 24(3),* 274-289. doi:10.3109/07380577.2010.483269

Newbigging, E. D., & Laskey, J. W. (1996). Riding the bus: teaching an adult with a brain injury to use a transit system to travel independently to and from work. *Brain Injury, 10(7),* 543-550. doi: 10.1080/026990596124250

National Institute for Health and Care Excellence. (2012). Appendix F quality appraisal checklist for quantitative intervention studies. Retrieved from https://www.nice.org.uk/process/pmg4/chapter/appendix-f-quality-appraisal-checklist-quantitative-intervention-studies.

OCEBM Levels of Evidence Working Group. (2011). "The Oxford 2011 Levels of Evidence". Oxford Centre for Evidence-Based Medicine. http://www.cebm.net/index.aspx?o=5653

Piccuci, L, Caffo, A. O., & Bosco, A. (2011). Besides navigation accuracy: gender differences in strategy selection and level of spatial confidence. *Journal of Environmental Psychology, 31(4),* 430-438.

Rivest, J., Svoboda, E., McCarthy, J., & Moscovitch, M. (2016). A case study of topographical disorientation: behavioural intervention for achieving independent navigation. *Neuropsychological Rehabilitation, 28(5),* 797-817. doi: 10.1080/09602011.2016.1160833

Rose, F. D., Brooks, B. M., & Rizzo, A. A. (2005). Virtual reality in brain damage rehabilitation: review. *Cyber Psychology & Behavior, 8(3),* 241–262. doi:10.1089/cpb.2005.8.241

Ruggiero, G., Frassinetti, F., Iavarone, A., & Iachini, T. (2014). The lost ability to find the way: topographical disorientation after a left brain lesion. *Neuropsychology, 28(1),* 147–160. doi:10.1037/neu0000009

Schepers, V. P., Visser-Meily, A. M., Ketelaar, M., & Lindeman, E. (2006). Post stroke fatigue: course and its relation to personal and stroke-related factors. *Archives of Physical Medicine and Rehabilitation, 87,* 184–188. doi:10.1016/j.apmr.2005.10.005

Sohlberg, M. M., Fickas, S., Hung, P-F., & Fortier, A. (2007). A comparison of four prompt modes for route finding for community travellers with severe cognitive impairments. *Brain Injury, 21(5),* 531-538. doi: 10.1080/02699050701311000

Solé, B., Jimenez, E., Torrent, C., Reinares, M., del Mar Bonnin, C., Torres, I., ….. Vieta, E. Cognitive impairment in bipolar disorder: treatment and prevention strategies. *International Journal of Neuropharmacology, 20(8),* 670-680. doi: 10.1093/ijnp/pyx032.

Tate, R. L., Perdices, M., McDonald, S., Togher, L., & Rosenkoetter, U. (2014). The design, conduct and report of single-case research: Resources to improve the quality of the neurorehabilitation literature. *Neuropsychological Rehabilitation, 24(3-4),* 315-331. doi: 10.1080/09602011.2013.875043

Tate, R. L., Rosenkoetter, U., Wakim, D., Sigmundsdottir, L., Doubleday, J., Togher, L., …. Percides, M. (2015). *The Risk of Bias in N-of-1 Trials (RoBiNT) Scale: an expanded manual for the critical appraisal of single-case reports.* Syndney, Australia: The PsycBITE Group.

Teasell, R., Bayona, N., Marshall, S., Cullen, N., Bayley, M., Chundamala, J., …. Tu, L. (2007). A systematic review of the rehabilitation of moderate to severe acquired brain injuries. *Brain Injury, 21*(2), 107–112.

Titov, N., & Knight, R. G. (2000). A procedure for testing prospective remembering in persons with neurological impairments. *Brain Injury, 14,* 877–886.

Tripathi, A., Kumar Kar, S., & Shukla, R. (2018). Cognitive deficits in schizophrenia: understanding the biological correlates and remediation strategies. *Clinical Psychopharmacology and Neuroscience, 16(1),* 7-17. doi: 10.9758/cpn.2018.16.1.7

Turkstra, L. S. (2008) Conversation-based assessment of social cognition in adults with traumatic brain injury. *Brain Injury, 22(5),* 397-409. doi: 10.1080/02699050802027059

UKABIF, Government debate on brain injury, 7pm -10pm (approx) Monday 18th June 2018, House of Commons chamber, June 2018.

Van der Ham, I. J. M., Van Zandvoort, M. J. E., Meilinger, T., Bosch, S. E., Kant, N., & Postma, A. (2010). Spatial and temporal aspects of navigation in two neurological patients. *Neuro Report, 21,* 685–689. doi:10.1097/WNR.0b013e32

Whiteneck, G. G., Gerhart, K. A., & Cusick, C. P. (2004). Identifying environmental factors that influence the outcomes of people with traumatic brain injury. *Journal of Head Trauma Rehabilitation, 19,* 191–204.

Wolbers, T., & Hegarty, M. (2010). What determines our navigational abilities? *Trends in Cognitive Sciences, 14(3),* 138-146. doi: 10.1016/j.tics.2010.01.001

Yip, B. C. B., & Man, D. W. K. (2013). Virtual reality-based perspective memory training program for people with acquired brain injury. *Neuro Rehabilitation, 32,* 103–115. doi: 10. 3233/NRE-130827

ii) Empirical Paper

Virtual Reality Route Learning in Adults with Traumatic Brain Injury

1. Abstract

Background

Research in wayfinding tasks has demonstrated that people with traumatic brain injury (TBI) have difficulties using distal landmarks to build a mental map of space associated with hippocampal damage. However, people with TBI appear to be relatively spared when proximal landmarks are available due to an ability to associate these landmarks with directions using their intact caudate nuclei. The purpose of the present study was to investigate whether this distal landmark navigation impairment is also found in route learning in people with TBI.

Method

A sample of 17 participants with TBI and 17 matched neurologically-healthy controls completed two conditions of a Virtual Reality (VR) route learning task, one route condition with proximal landmarks and the other with distal landmarks. Participants' completed three learning laps of each route and a test lap which measured their memory of the routes. Both groups completed a navigation strategy questionnaire after completing each route.

Results

Both groups of participants performed significantly better on the proximal than the distal landmark route. Participants with TBI were significantly worse than control participants at performing the distal but not the proximal landmark route. Participants' reported using an egocentric associative strategy to learn both landmark routes.

Discussion

The results indicate that route learning is a navigation task that supports egocentric associative learning independent of landmark type. Nevertheless, as participants with TBI were significantly worse than control participants at route learning using distal landmarks this

suggests that there is a relationship between distal landmarks, route learning and the cognitive effects of TBI specifically, which is possibly due to impairments in building a mental map of space using the hippocampus.

2. Introduction

The ability to learn a route is vitally important to lead an independent life as an adult and there is extensive research to suggest that people with traumatic brain injury (TBI) find it difficult to learn new routes after their injury (Barrash, Damasio, Adolphs, & Tranel, 2000; Skelton, Ross, Nerad & Livingstone, 2006; Sorita et al., 2012). A TBI is defined as damage to the brain that occurred after birth resulting from an external mechanical force for example from a car accident, fall or an assault (Lezak, Howieson, Bigler, & Tranel, 2012).

Navigation is a particularly complex cognitive skill involving the interaction of memory and executive functioning abilities and navigation impairment has been shown to be a common cognitive effect resulting from TBI (Ruggiero, Frassinetti, Iavarone, & Iachini, 2014; van der Ham et al., 2010). One of the brain structures that has been shown to be crucial to successful spatial navigation and is particularly vulnerable from damage due to TBI is the hippocampus (Tate & Bigler, 2000; Livingstone & Skelton, 2007; Atkins, 2011; Green et al., 2014; Woolley et al., 2015). For example, Green et al. (2014) found a reduction in hippocampal volume in at least 70% of people with mild-severe TBI at five months post-injury. However, there is research evidence to suggest that the importance of the hippocampus to successful navigation may vary depending on the navigation task, the type of landmarks used as spatial cues and the navigation strategy adopted for learning.

To date most of the research evidence investigating the association between landmark-type and navigation learning strategy has been conducted in wayfinding navigation tasks (Burgess, Maguire & O'Keefe, 2002; King, Burgess, Hartley, Vargha-Khadem & O'Keefe,

2002; Goodrich-Hunsaker, Livingstone, Skelton & Hopkins, 2010). Wayfinding can be defined as knowing ones position in physical space and using this information to navigate in familiar and unfamiliar locations (Aguirre & D'Esposito, 1999). Furthermore, landmarks can be divided into categories according to how far away they are from the navigator. Proximal landmarks are those relatively close to the navigator and distal landmarks are further away in the distance. It is thought that wayfinding using distal landmarks requires an individual to build up a mental map of how those landmarks are related to each other in space, enabling the individual to visualise the environment from a bird's-eye perspective, independent of their own viewpoint (Eichenbaum, Stewart & Morris, 1990; Bohbot & Corkin, 2007; Livingstone & Skelton, 2007). This is called allocentric navigation and has been associated with the hippocampus (O'Keefe & Dostrovsky, 1971; Muller & Bostock, 1994; Possin et al., 2017).

However, wayfinding using proximal landmarks is thought to involve a different navigation strategy where an individual learns to associate landmarks with a directional response dependent on their viewpoint from a ground-level perspective (Bohbot, Iaria & Petrides, 2004; Nadel & Hardt, 2004; Woolley et al. 2015). This is called egocentric navigation and is thought to be related to a brain structure called the caudate nucleus which seems to be less vulnerable to damage from TBI (Iaria, Petrides, Dagher, Pike & Bohbot, 2003; Nadel & Hardt, 2004; Serra-Grabulosa, 2005; Possin et al., 2017). In a functional MRI study, Iaria et al. (2003) found that participants who used spatial landmark strategies to solve a wayfinding task in a computer-generated environment had significantly increased activation in their right hippocampus. However, participants who used non-spatial strategies to solve the task had significantly increased activation within the caudate nucleus. Therefore, the learning strategy and brain structure utilised for successful wayfinding may be different depending on whether proximal or distal landmarks are available as spatial cues.

Nice (2015) conducted a pilot study aiming to investigate whether these findings in wayfinding could be replicated in route learning. Route learning is defined as learning a specific path that joins two locations (Lloyd, Riley & Powell, 2009b). Specifically, it was hypothesised that 16 participants with TBI would show a relatively greater disadvantage than 16 neurologically-healthy control participants on route learning using distal landmarks compared to proximal landmarks. The results of the pilot study supported this prediction, providing further support for a link between hippocampal damage in people with TBI and difficulties navigating using distal landmarks due to the role of the hippocampus in building a mental map of space. However, there was some concern about the findings of this pilot study as the neurologically-healthy control participants' scores on the routes may have been unreliable due to the task not being of sufficient difficultly to detect whether control participants were also worse at route learning using distal compared to proximal landmarks.

Furthermore, some researchers have suggested that one cannot make the same links between allocentric navigation strategies, distal landmarks and the hippocampus in route learning that you can with wayfinding (Trullier, Wiener, Berthoz, & Meyer, 1997; Hartley, Maguire, Spiers, & Burgess, 2003; Waller & Lippa, 2007). By definition, the act of learning a route requires associations between landmarks and decision points along the route, independent of landmark type. When neurologically-healthy participants are wayfinding, there is evidence to suggest that over 50% will choose to adopt an egocentric strategy even though both types of navigation strategy are available and many studies do not take the individual variations in preferences for navigation strategies into account (Nadel & Hardt, 2004; Iaria et al. 2003; Bohbot et al. 2004). In Nice (2015), control participants mean ratings of using bird's-eye view (allocentric) strategies to route learn in the distal condition was 1.5, where 1 is 'I did not use this strategy at all' and 5 is 'I almost totally used this strategy' and for people with TBI it was 1. Therefore, from this pilot study it is not clear whether participants with TBI are more

impaired at route learning using distal landmarks than proximal landmarks due to an impairment in their ability to build a mental map of space considering that even control participants did not report using this strategy to learn the distal landmark route.

One possible explanation is that the neurologically-healthy participants may have been able to build up a mental map of the spatial relationships between the landmarks using their intact hippocampi even if not actively choosing an allocentric strategy. There have been research studies to support the notion that neurologically-healthy adults are able to flexibly switch between egocentric and allocentric strategies depending on the demands of the navigation task and that egocentric encoding precedes allocentric encoding (Harris, Wiener & Wolbers, 2012; DeCondappa, 2016; Colombo et al., 2017). Furthermore, these studies found that this flexibility in being able to switch between navigation strategies and the use of allocentric strategies in particular declines with age linked to reduced hippocampal function. Nevertheless, these studies were not specific to route learning.

Overall there is a body of research evidence to suggest that adults with TBI are impaired at wayfinding using distal landmarks and allocentric strategies. However, there is a paucity of research evidence investigating how these factors present in route learning specifically. If adults with TBI are found to be relatively more impaired at route learning using distal landmarks than proximal landmarks, then it may be possible to develop rehabilitation strategies to help improve route learning and independence in adults with TBI based on these findings. Therefore, further research is needed to build on the preliminary findings from the pilot study conducted by Nice (2015) to investigate whether there is a difference in route learning performance between adults with TBI and neurologically-healthy controls and whether this is related to landmark and navigation strategy type. This is the purpose of the present study.

2.1 Aims

The main study aim was to explore the impact of landmark-type (proximal or distal) on Virtual Reality (VR) route learning in people with TBI compared to a neurologically-healthy control group. Route learning was taught and tested in VR as this technology has been shown to have good ecological validity and equivalence to real-world route learning (Darken & Banker, 1998; Stanton, Wilson, Foreman & Duffy, 2000; Lloyd, 2007). A further primary aim was to look at the impact of landmark-type (proximal or distal) on subjective reporting of navigation strategies including allocentric and egocentric strategies in participants with TBI compared to neurologically-healthy controls. Specifically, the study aimed to test the following research questions and hypotheses:

1. **Is there a difference between landmark conditions or groups in performance on a VR route learning task?**

- Hypothesis 1: Participants with TBI will perform worse than control participants on a VR route learning task
- Hypothesis 2: Both groups of participants will perform better when route learning using proximal compared to distal landmarks
- Hypothesis 3: Participants with TBI will show relatively greater impairment than control participants when route learning using distal compared to proximal landmarks

2. **Is there a difference between landmark conditions or groups in ratings of navigation strategies used on a VR route learning task?**

- Hypothesis 4: Both groups of participants will give significantly higher ratings for using an egocentric strategy to learn the proximal route than an allocentric strategy
- Hypothesis 6: Control participants will give significantly higher ratings than participants with TBI for using an allocentric strategy to learn the distal route

3. Method

3.1 Design

The study used a mixed factorial design with group (TBI or controls) as the between-subjects factor and landmark-type (proximal or distal) as the within-subjects factor. Participants completed both landmark route conditions and the route order was counterbalanced to control for practice and fatigue effects. Specifically, eight participants with TBI and eight controls completed the proximal route first and nine participants with TBI and nine controls completed the distal route first.

G*power (Faul, Erdfelder, Lang & Buchner, 2007) was used to conduct an a priori power analysis to determine the sample size needed to identify a moderate effect size (eta=0.25) in a mixed ANOVA with a between and within-subjects interaction and with alpha set at 0.05. The analysis determined that a total sample size of 34 would be required to achieve power of 0.8 (assuming a correlation of 0.5 between the repeated measures).

3.2 Participants

Participants were 17 adults with closed TBI recruited from outpatient rehabilitation services and day centres in the West Midlands. Inclusion criteria were a closed TBI of at least moderate severity having occurred at least six months prior to the study commencing, older than 18 at the time of the injury and self-reported difficulties with memory. Only participants with a closed TBI were included which is where the brain has been damaged from the movement of the brain inside the skull and the skull has not been fractured (Lezak et al., 2012). This type of TBI has been associated with diffuse injury to the axons that communicate within and between brain structures and to global cognitive effects such as memory and executive functioning which are important cognitive abilities for navigation (Kinnunen et al., 2011; Wolf & Koch, 2016). The severity of TBI was determined by the participant's self-report of length of post-traumatic amnesia (PTA) of being at least 24 hours, meeting the classification criteria

for TBI of at least moderate severity (Nakase-Richardson et al., 2011). Furthermore, the keyworkers who were working with the participants verified the severity of the participants' TBI and memory difficulties from the medical notes. Participants with closed TBIs with complications such as surgery to relieve intracranial pressure were not excluded from the study. Exclusion criteria were marked difficulties with comprehension or physical difficulties that would make it difficult to operate the joystick and understand the task instructions.

17 neurologically-healthy control participants individually matched to the participants with TBI for gender and an approximation of age and education-level were recruited through convenience sampling. The researchers aimed to match control participants to participants with TBI for age within a 2 year difference and education-level as those who completed education before degree-level and at degree-level. Exclusion criteria were being under 18 years of age, physical difficulties which would make it difficult to operate the joystick, comprehension difficulties and a history of head injury that required hospital treatment.

3.3 Apparatus and Materials

Participants with TBI completed a demographics questionnaire, a famous landmark recognition screening test and three standardised neuropsychological memory assessments prior to the VR route learning task. The demographics questionnaire collected data on age, gender and education-level as well as details specific to the TBI such as length of time in PTA. Control participants also provided information on their age, gender and education-level. The memory assessments were administered to participants with TBI only to provide objective detail about the severity of cognitive problems in this group. Participants with TBI and neurologically-healthy control participants completed the two landmark routes (proximal and distal) and a navigation strategy questionnaire after completion of each route.

Famous Landmark Recognition Test

The famous landmark recognition test was designed to screen participants with TBI for difficulties recognising familiar landmarks, as an inability to recognise landmarks would compromise the results of the study (Aguirre & D'Esposito, 1999). Participants with TBI were shown photographs of five famous landmarks and asked to verbally recall the name of each. One point was allocated for a correctly recalled landmark. The landmarks used were Big Ben, the Eiffel Tower, the Leaning Tower of Pisa, the Statue of Liberty and Stonehenge.

Adult Memory and Information Processing Battery List Learning subtest (Coughlan, Hollows & Coughlin, 1985)

The Adult Memory and Information Processing Battery (AMPIB) list learning task assesses verbal learning and recall. There are five trials where a researcher reads aloud a list of 15 words and the participant recalls as many words from the list as possible after each trial. A distractor list of 15 new words is then presented for verbal recall on the sixth trial. This is followed by a final trial where the participant is asked to recall as many words from the original list as possible without the original list being repeated. Reliability for the whole scale has been reported as 0.77 and reliability for the distractor trial as 0.73 (Coughlan et al., 1985). Difficulties with verbal memory as measured on this task have been shown to correlate positively with poor route learning performance in participants with TBI (Lloyd, 2007).

Wechsler Memory Scale IV Spatial Addition subtest and Symbol Span subtest (Wechsler, 2009)

The Spatial Addition and Symbol Span subtests of the Wechsler Memory Scale-IV are designed to measure visual-spatial working memory. The spatial component of the visuo-spatial sketchpad (VSSP) of working memory has been shown to be important for route learning as it is thought that successful route learning requires the storage of a series of visual images along the route (Mallot & Gillner, 2000; Meilinger et al., 2008). For the Spatial

Addition subtest participants were shown a stimulus booklet containing a pattern of red and blue circles on a grid. The stimulus booklet was then removed from view and the participant replicated the pattern of circles on their own copy of the grid from memory. For the Symbol Span subtest participants were shown a stimulus booklet containing a series of abstract symbols. The stimulus booklet was removed from view and the participant was required to point to the correct symbols in the order they were originally presented. Reliability of both subtests was reported as >0.8 and test re-test as >0.7 (Wechsler, 2009).

VR Route Learning Task

Two virtual environments, one with proximal and one with distal landmarks were built by a small to medium enterprise company called Daden who were commissioned to produce the software specifically for the task (see Figures 1 and 2; Appendices C and D). Each route consisted of 18 possible turns which is similar to the pilot route in Nice (2015) but extended by three turns to increase difficulty with the aim of reducing ceiling effects in control participants. The base routes contained an equal number of junctions and landmarks and so were equivalent for difficulty before the landmarks were applied. In the proximal condition each turn was associated with a landmark that was randomly allocated to one of four street corners and in the distal condition to one of four distant viewpoints around a 90 degree arc in the participant's forward field of view. The routes were presented to the participants on a laptop and a joystick was used to move within the routes. The measure of route performance in each landmark condition was how many correct turns (out of 18) participants completed.

Figure 1. Example of a junction on the proximal route (learning lap) with bus-stop as the proximal landmark

Figure 2. Example of a junction on the distal route (learning lap) with an archway as a distal landmark

Navigation Strategy Questionnaire

A navigation strategy questionnaire was used to explore the strategies that participants reported using to learn the routes (see Appendix B). The navigation strategy questionnaire was developed by Lloyd (2007), however, some of the items were altered for this study to improve the face validity of the questionnaire in capturing whether participants used allocentric or egocentric strategies specifically. Participants rated their use of ten common wayfinding strategies on a 5-point Likert scale where 0=not at all, 1=a little, 2=a moderate amount, 3=a lot and 4=almost completely. The original questionnaire developed by Lloyd (2007) showed adequate internal reliability in neurologically-healthy participants (α=0.60, N=70).

3.4 Procedure

The researcher first met with keyworkers and other clinicians at the outpatient rehabilitation services and day centres to explain the study, the inclusion/exclusion criteria and to provide copies of the information sheet for participants with TBI (see Appendix J). Staff were asked to identify potential participants who met the inclusion criteria and to provide them with the information sheet. Participants who were interested in taking part made direct contact with the researcher via the contact details provided on the information sheet or indicated to a staff member that they would like to speak to the researcher about the study. All participants were given a minimum of 24 hours to consider whether to participate before arranging an appointment at their rehabilitation centre to complete the tasks. At the start of the first research session participants were given the opportunity to ask questions and those wishing to proceed, read and signed the consent form (see Appendix K) and then completed the demographic questionnaire, the famous landmark recognition test and the memory assessments.

Participants with TBI were then offered a short break. Following this they completed a practice task in a neutral VR room to get used to using the joystick to navigate before

completing the main VR route learning task. The participants then completed the first route (proximal or distal depending on the counterbalance) which consisted of three learning laps and a test lap. Participants were informed that their task was to try to remember the route and that the yellow arrows would show them what direction they needed to turn. They were also told that the arrows would disappear in the test lap and so the landmarks would help them remember the route. On the first learning lap, participants were asked to call out the names of the landmarks along the route to ensure that they had noticed them. Participants then completed a further two learning laps with the arrows visible. For the test lap, the instructions that they would need to complete the route by memory was repeated. If participants made a wrong turn at any of the junctions along the route in the test lap the researcher took the participant back to the junction and pointed out the correct turning. One point was given for each correct turning and all 18 turnings were recorded by the researcher. Participants completed the navigation strategy questionnaire directly after completing the route. Participants with TBI completed the second landmark route and navigation strategy questionnaire approximately one week later to control for the impact of fatigue on their performance of the second route.

Neurologically-healthy control participants were recruited through convenience sampling by distributing the information sheet for control participants (see Appendix J) to contacts of the researchers. Potential control participants who met the criteria to be matched to a participant with TBI on gender, age and education-level contacted the researcher via the contact details provided on the information sheet and were given a minimum of 24 hours to consider their participation. A research session was then arranged either to be held at the University of Birmingham or at the participant's home during which the consent form (see Appendix K), demographic questions, VR route learning task and navigation strategy questionnaires were all completed. The procedure for completing the VR route learning task

and navigation strategy questionnaires was the same as for the participants with TBI, however, control participants completed both routes in the same session.

The author collected 13 datasets for the participants with TBI and 10 datasets for the neurologically-healthy control participants. The remaining participants' data were collected by four other researchers. All researchers followed the above procedure.

3.5 Ethical Approval

The study described in this thesis forms part of a larger study with multiple researchers. Original ethical approval for the study was granted by the Birmingham, East, North and Solihull NHS Research Ethics Committee in 2010 (see Appendix A). An amendment to increase the number of turnings in each route from 15 to 18 and to add researchers to the study was granted by the ethics committee in 2017.

4. Results

4.1 Demographics

The demographic information for age, gender and education-level for both groups is presented in Table 1. An independent t-test suggested that there was no significant difference in the mean age of the two groups, $t(32)=0.36$, $p=0.972$. A chi-squared test was performed on the education-level data and showed that there was not a significant association between group (TBI or controls) and education-level (pre-degree or degree), $\chi^2(1)=0.654$, $p=0.419$.

Table 1. Participants' age, gender and education-level demographics

		Gender		Education-Level	
Group	Mean Age (years) (SD)	Males	Females	Pre-Degree	Degree
TBI (n=17)	42.94 (14.50)	14	3	14	3
Controls (n=17)	42.76 (14.36)	14	3	12	5

The mean time post injury for participants with TBI was 34.8 months (SD=43.22) which equals to 2.9 years. The mean length of PTA was 5.9 weeks (SD=5.31) with all participants with TBI experiencing PTA for at least 24 hours meeting the criteria for TBI of at least a moderate severity. The cause of TBI for eight participants was from a vehicle accident, four from a fall and one from an assault. The cause of TBI for four participants was not disclosed. None of the neurologically-healthy control participants reported having experienced a head injury that required hospital treatment.

4.2 Neuropsychological Tests

The results of the neuropsychological tests completed by participants with TBI are presented in Table 2. Lower percentile cut-offs were set as 75 for high average, 25 for average, 9 for low average, 2 for well below average and below 2 as impaired.

Table 2. Neuropsychological test age Scaled Scores and Classifications for the TBI group (n=17)

Neuropsychological Test	Classifications (lower percentile cut-offs)	Classification Frequencies (n=17)	Mean age normed Scaled Scores/Z-Scores (SD)
AMIPB verbal recall (lists A1-5)	High Average (75)	1	1.14
	Average (25)	3	-0.21 (0.14)
	Low Average (9)	4	-0.85 (0.07)
	Well Below Average (2)	2	-1.45 (0.06)
	Impaired (>2)	7	-3.14 (0.98)
AMIPB delayed recall (list A6)	High Average (75)	0	-
	Average (25)	2	-0.35 (0)
	Low Average (9)	6	-0.87 (0.24)
	Well Below Average (2)	0	-
	Impaired (>2)	9	-3.26 (0.78)
WMS-IV Spatial Addition	High Average (75)	5	12.8 (0.84)
	Average (25)	6	9 (1.10)
	Low Average (9)	2	8 (1.41)
	Well Below Average (2)	4	5.5 (0.58)

	Impaired (>2)	0	-
WMS-IV Symbol Span	High Average (75)	2	13 (0)
	Average (25)	7	10 (1.15)
	Low Average (9)	4	6.5 (0.58)
	Well Below Average (2)	4	4.75 (0.5)
	Impaired (>2)	0	-

4.3 Route Learning Performance

All of the participants with TBI were able to name at least 4 of the 5 famous landmarks indicating that participants were able to recognise and recall landmarks in the route learning task. All participants in the study were able to operate the joystick and successfully completed the three practice routes by following the yellow arrows before completing the test route in both the proximal and distal landmark conditions.

Research Question 1: Is there a difference between landmark conditions or groups in performance on a VR route learning task?

The descriptive statistics for the route performance data by route (proximal and distal) and group (TBI and controls) are displayed in Table 3 and Figure 3.

Table 3. Descriptive statistics for number of correct turnings (out of 18) for both groups in each route condition

	TBI			Controls		
Landmark Condition	Mean correct turnings/18 (SD)	Median correct score	Range of scores	Mean correct turnings/18 (SD)	Median correct score	Range of scores
Proximal	15.88 (3.00)	17.50	9-18	17.18 (1.24)	18.00	14-18
Distal	12.24 (2.97)	13.00	6-17	15.88 (1.36)	16.00	13-18

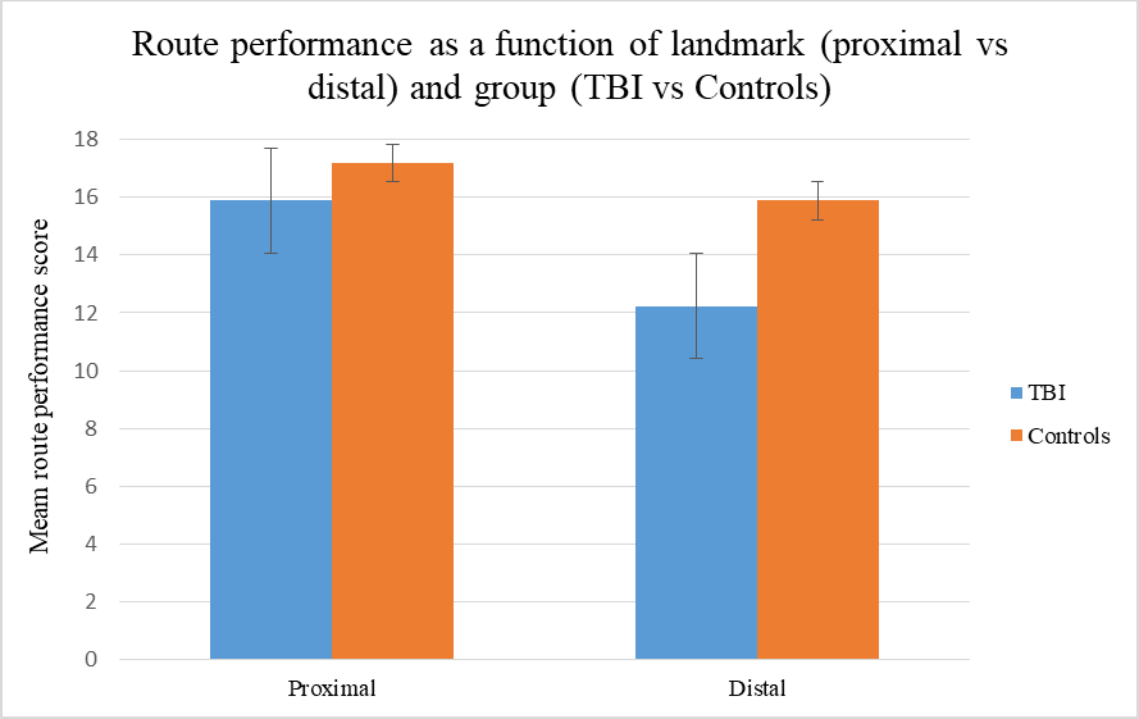


Figure 3. Bar graph showing the mean correct turnings (out of 18) as a function of group and route

The data were checked for suitability for parametric analysis to determine whether it would be appropriate to carry out a mixed 2x2 ANOVA on the data with landmark-type (proximal vs distal) as the within-subjects factor and group (TBI vs control) as the between-subjects factor. As the route data were found to violate the assumptions of parametric analysis it was decided that a transformation of the data would need to be applied to control for these violations (see Appendix G). However, the transformation of the data did not improve the non-normality and heteroscedasticity of the proximal data.

The F-test is very robust against non-normal distribution, especially in a fixed-effects model as in this study which means that the analysis can be conducted even if the data violates one of the assumptions that underlie the use of the test, (Field, 2009, p. 155). Therefore, it was decided to carry out a mixed 2x2 ANOVA and if the ANOVA returned statistically significant main effects and interaction based on the transformed data, this should be verified using

appropriate non-parametric tests. The non-parametric tests would estimate the potential impact of the non-normality and heteroscedasticity of the proximal data on the ANOVA model.

Based on the transformed data, results of the ANOVA showed that both of the main effects were statistically significant (landmark-type $F(1,32)=59.64$, $p<0.001$; group $F(1,32)=10.97$, $p=0.002$) and a significant interaction was observed between landmark-type and group ($F(1,32)=7.388$, $p=0.011$, $\eta 2=0.19$). This interaction accounted for approximately 19% of the variation in the data. Two Mann-Whitney tests were then conducted to analyse between group differences within each route condition and two Wilcoxon Signed-Rank tests were conducted to analyse the within route differences for each group. Bonferroni correction was applied to these post-hoc tests to adjust the significance alpha to $p=0.0125$ to control for the inflated type I error rate. The Mann-Whitney tests found a significant difference between the TBI group and control group on distal route performance ($U=31.50$, $z=-3.934$, $p<0.001$, $r=-0.675$) but not on proximal route performance ($U=120.00$, $z=-0.930$, $p=0.352$). The Wilcoxon Signed-Rank tests found a significant difference between proximal and distal route performance for the TBI group ($T=0$, $z=3.625$, $p<0.001$, $r=0.622$) and the control group ($T=4.50$, $z=2.717$, $p=0.007$, $r=0.466$).

Research Question 2: Is there a difference between landmark conditions or groups on self-reported navigation strategies used on a VR route learning task?

The second analysis explored whether landmark-type (proximal or distal) affected subjective reporting of navigation strategies in participants with TBI compared with neurologically-healthy control participants. The questionnaire items that measured use of egocentric and allocentric strategies specifically was the focus of this analysis. Thus, participants' ratings for item 5 'I used landmarks on the corners of the street to build a bird's-eye map' (allocentric strategy) and item 6 'I associated landmarks on the corners of the street

with which way to turn at each junction' (egocentric strategy) for the proximal condition was compared. Participants' ratings for item 7 'I used landmarks in the distance to build a bird's-eye map' (allocentric strategy) and item 8 'I associated landmarks in the distance with which way to turn at each junction' for the distal condition was compared. The descriptive statistics for the strategy ratings data are displayed in Table 4.

Table 4. Descriptive statistics for strategy ratings (out of 4) for both groups in each route condition

	TBI			Controls		
Strategy Questionnaire Item (Route)	Mean strategy rating/4 (SD)	Median strategy rating	Range of scores	Mean strategy rating/4 (SD)	Median strategy rating	Range of scores
Item 5 (Proximal)	1.529 (0.355)	1	0-4	0.933 (0.358)	0	0-4
Item 6 (Proximal)	2.706 (0.340)	3	0-4	3.400 (0.163)	3	2-4
Item 7 (Distal)	1.706 (0.371)	2	0-4	1.267 (0.408)	0	0-4
Item 8 (Distal)	2.235 (0.315)	2	0-4	2.467 (0.307)	3	0-4

The data were found to violate the assumptions of parametric analysis and transformation did not improve the normality of the data (see appendix G). However, as the F-test is very robust against non-normal distribution it was decided to conduct a 2x2 mixed ANOVA on the non-transformed data with strategy (allocentric vs egocentric) as the within-subjects factor and group (TBI vs controls) as the between-subjects factor for the proximal and distal conditions separately (comparing participants' ratings on items 5 and 6 for the proximal condition and items 7 and 8 for the distal condition). The results of the ANOVAs found a significant main effect of strategy for both the proximal and distal routes, ($F(1,30)=47.927$, $p<0.001$ and $F(1,30)=8.867$, $p=0.006$ respectively). Furthermore, there was a non-significant main effect of group for the proximal condition ($F(1,30)=0.018$, $p=0.850$) and the distal

condition (F(1,30)=0.065, p=0.800). There was also a non-significant interaction between strategy and group in the distal condition (F(1,30)=1.333, p=0.257, η2=0.043). However, a significant interaction was found between strategy type and group in the proximal condition (F(1,30)=6.011, p=0.020, η2=0.167). This interaction accounted for approximately 17% of the variation in the data.

Mann-Whitney and Wilcoxon Signed-Rank tests were then conducted as post-hoc tests to compare the between and within group differences in strategy ratings for each route separately with the significance alpha adjusted to p=0.0125. The Mann-Whitney tests did not find a significant difference between the TBI group and control group on egocentric ratings in the proximal condition (U=96.50, z=-1.258, p=0.208) or the distal condition (U=115.00, z=-0.486, p=0.627). There was also not a significant difference between the groups on allocentric strategy ratings in the proximal condition (U=95.50, z=1.283, p=0.200) or the distal condition (U=106.00, z=0.855, p=0.393). However, the Wilcoxon Signed-Rank tests did find a significant within-subjects difference for ratings of using an allocentric and egocentric strategy to learn the proximal route (T=3.00, z=-2.537, p=0.011 for participants with TBI and T=0, z=-3.222, p=0.001 for control participants). A significant difference was also found between control participants' allocentric and egocentric strategy ratings in the distal route but a significant difference was not found for TBI participants (T=3.00, z=2.431, p=0.015 and T=3.50, z=1.044, p=0.296 respectively). Figures 10 and 11 show the mean ratings for each of these questions broken down both by group for ease of comparison.

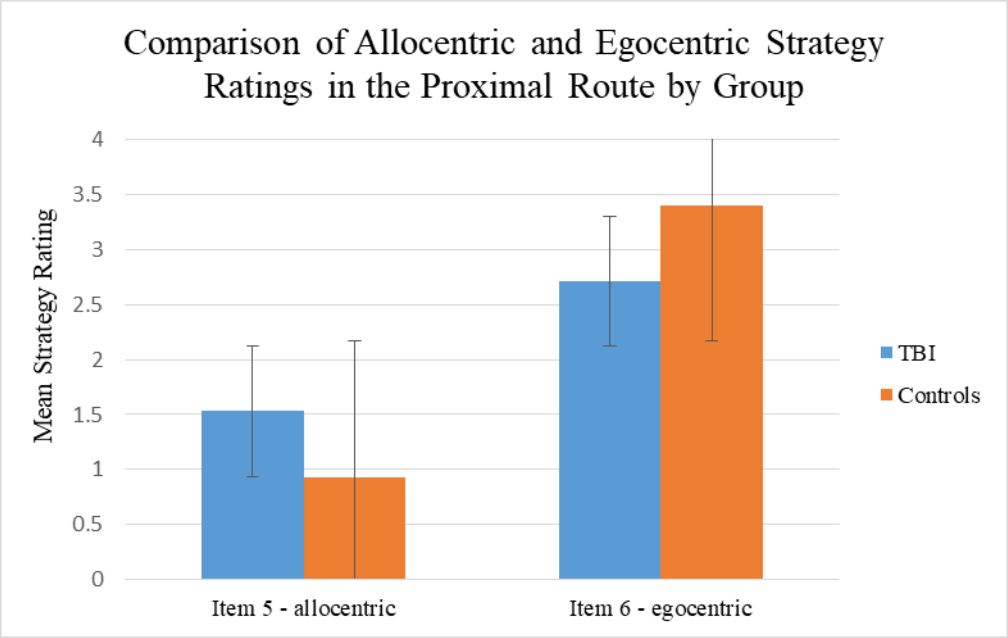


Figure 4. Bar graph showing the mean allocentric and egocentric strategy ratings by group in the proximal landmark route

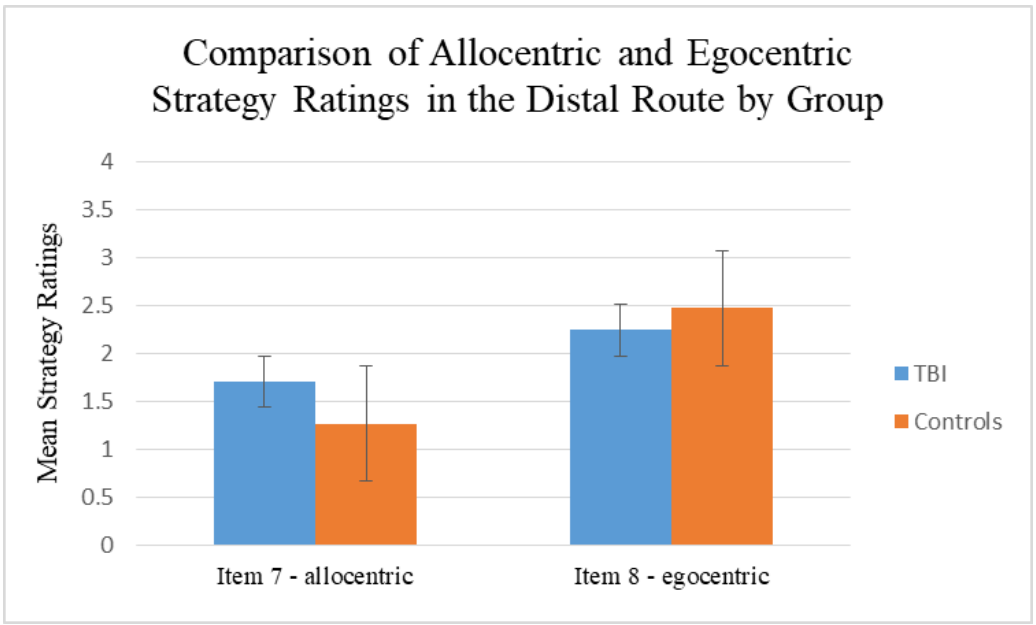


Figure 5. Bar graph showing the mean allocentric and egocentric strategy ratings by group in the distal landmark route

5. Discussion

The first aim of this study was to explore whether there are landmark (proximal or distal) and group (TBI or neurologically-healthy controls) differences in performance on a VR route learning task. Based on previous research demonstrating that people with TBI are impaired at navigation tasks (Barrash et al., 2000; van der Ham et al., 2010), that the hippocampus is important for spatial navigation and is particularly vulnerable to damage from TBI (Tate & Bigler, 2000; Green et al., 2014), it was hypothesised that the participants with TBI in this study would perform significantly worse than neurologically-healthy controls on the VR route learning task. This hypothesis was supported by the findings of a significant main effect of group on route learning performance with the TBI group making significantly fewer correct turnings overall than the control group.

Furthermore, due to the previous research suggesting that route learning as a navigation task differs from wayfinding in that the nature of learning a specific path joining two locations requires an egocentric strategy, it was hypothesised that both groups would perform better in the proximal than the distal condition (Trullier et al., 1997; Waller & Lippa, 2007). The significant main effect of route and the significant repeated measures post-hoc findings provide evidence to support this hypothesis. Specifically, both groups made significantly more correct turnings in the proximal than the distal route. However, this finding contradicts Nice (2015) who found that control participants' proximal and distal route performance did not significantly differ. Nevertheless, control participants' performance in the VR route learning task in the study by Nice (2015) may have been affected by the routes not being of sufficient difficulty to detect a within-subjects effect in the control group. Increasing the number of turnings in each route by three in the current study may have allowed this difference to be detected.

However, from these results alone, it is not possible to rule out that the distal route may have been greater in difficulty than the proximal route, accounting for the significantly worse performance of both groups on the distal route. Nevertheless, a significant interaction was found between landmark-type and group on route performance. Between-subjects post-hoc analysis showed that participants with TBI were significantly worse than control participants at learning the distal route but not the proximal route. Therefore, although both groups experienced a significant decrease in their route learning performance on the distal route, this decrease was significantly greater for participants with TBI. This finding supports an association between route learning using distal landmarks and the effects of TBI specifically and not just an increased difficulty of the distal route. These findings are therefore consistent with previous research evidence that participants with TBI are impaired at navigating using distal landmarks possibly linked to reduced hippocampal functioning (e.g. Burgess et al., 2002; Goodrich-Hunsaker et al., 2010), demonstrating this in a route learning task specifically.

The second aim of this study was to explore whether there are landmark or group differences in self-reported ratings of navigation strategies. The first hypothesis proposed that both groups would give significantly higher ratings for using an egocentric strategy than an allocentric strategy to learn the proximal route given previous research findings of an association between proximal landmarks, egocentric learning and the caudate nucleus (Iaria et al., 2003; Nadel & Hardt, 2004; Woolley et al., 2015). Supporting this hypothesis, within-subjects post-hoc analysis of the strategy data indicated that both groups reported using an egocentric strategy, 'I associated landmarks on the corners of the street with which way to turn at each junction', significantly more than an allocentric strategy, 'I used landmarks on the corners of the street to build a bird's-eye map', to learn the proximal route.

However, the hypothesis that control participants would give significantly higher ratings of using an allocentric strategy to learn the distal route than participants with TBI was

not supported by these results. Between-subjects post-hoc analysis found that there was no significant difference between the groups in their ratings for item 7 ‘I used landmarks in the distance to build a bird’s-eye map’ in the distal condition. Furthermore, both groups’ mean ratings for use of an allocentic strategy in the distal route was <2 (less than a moderate amount), suggesting that neither group used an allocentric strategy to learn the distal route. This is consistent with the findings in Nice (2015) in which both participants with TBI and control participants gave low ratings for using allocentric strategies to learn the distal route.

The findings from the navigation strategy data in this study therefore can be argued to provide further support to the research by Trullier et al. (1997) and Waller and Lippa (2007) that the association between landmark-type and learning strategy depends on the navigation task. Both groups of participants in this study rated using an egocentric strategy significantly higher than an allocentric strategy to learn both the proximal and distal routes which differs from the research evidence in wayfinding that egocentric strategies are associated with proximal landmarks and allocentric strategies are associated with distal landmarks specifically (Nadel & Hardt, 2004; Livingstone & Skelton, 2007). Thus, route learning may be a navigation task that utilises egocentric learning independent of landmark-type.

Nevertheless, this does not adequately explain why participants with TBI show a disadvantage at route learning using distal landmarks if route learning is a task that uses egocentric strategies to learn routes independent of landmark-type. In reference to previous research, it is possible that control participants had encoded the distal route both egocentrically and allocentrically but that because egocentric encoding preceded allocentric encoding, this egocentric frame of reference was more in participants’ awareness (Harris et al., 2012; DeCondappa, 2016). It has been suggested that initial spatial learning is processed egocentrically and that this egocentric information then feeds into an allocentric spatial map forming a stable representation of the environment which can be accessed from spatial memory

at a later date if needed (Harris et al., 2012). Thus, when only distal landmarks were available in the distal route, control participants may have been able to supplement their egocentric route knowledge with allocentric spatial knowledge from their intact hippocampi even if this was outside of their awareness.

5.1 Limitations and Future Research

Reliance on self-report ratings of navigation strategy use in this study can be considered a limitation of the study design in that there was not a condition which would objectively test whether participants with TBI and control participants' used egocentric or allocentric strategies. Furthermore, use of self-report measures by people with TBI have been called into question due to participants' low self-awareness (Toglia & Kirk, 2000). Future research could therefore explore ways to objectively measure navigation strategy use to more robustly test whether route learning impairment using distal landmarks in participants with TBI is related to difficulties using allocentric strategies to build a mental map of space using the hippocampus. One way to find evidence of a link between route learning, landmark-type, strategy and brain function may be to replicate the route learning task in a functional Magnetic Resonance Imaging (fMRI) study. An fMRI study could test whether there are differences in activation in the hippocampi and caudate nuceli of participants with TBI and neurologically-healthy control participants when recalling a distal and proximal landmark route.

Furthermore, there are inherent limitations in drawing conclusions from samples of less than forty participants (Field, 2009, p156). In small sample sizes, normal distribution and significant findings at $p<0.05$ can be hard to identify due to low statistical power. Moreover, the proximal route data were significantly negatively skewed in this study suggesting that increasing the difficulty of the routes from those used in Nice (2015) did not overcome the proximal ceiling effect. Additionally, the mean navigation strategy ratings for control

participants may have been impacted by the two missing sets of questionnaires. Due to these limitations it was necessary to verify the parametric results with non-parametric tests which do not assume normality of distribution or homogeneity of variance. The conclusions of this study may therefore be strengthened if the study was replicated in a larger sample with greater statistical power to more reliably detect a normal distribution and significant effect.

The virtual environment used in the current study was designed to represent an ecologically valid environment in which to train and test route learning. A number of studies have demonstrated the equivalence of wayfinding behaviour in virtual and real environments, as well as the transfer of navigation training from VR to the real-world (e.g. Darken & Banker, 1998; Stanton et al., 2000). Thus, VR environments may offer great potential in the assessment and rehabilitation of navigation difficulties in people with TBI. However, there are still fundamental differences between route learning in VR and route learning in real life that may reduce the ecological validity of the VR route learning task. For example, real-world environments are unlikely to contain only proximal or distal landmarks. This leads to the question of why people with TBI are impaired at route learning in real life if as shown in this current study, they are not impaired at learning a route with proximal landmarks. Is it that the control in the virtual environment of factors that compete for cognitive resources, such as, movement and noise, allow participants with TBI to attend to the proximal landmarks in VR and use them to route learn when this would be more difficult in the real-world? If the aim is to be able to develop a standardised measure of route learning and training tool using VR for clinical practice then further research into whether improvements in route learning using this technology transfers to the real-world is needed.

5.2 Conclusion

In conclusion, this study found that participants with TBI were impaired on a VR route learning task when only distal landmarks were available to learn the route. Without an objective measure of navigation strategy use it is difficult to be certain of the reason for this distal landmark impairment. Nevertheless, as the participants with TBI were not impaired at route learning using proximal landmarks it seems reasonable to conclude that there is an association between distal landmarks, route learning and the cognitive effects of TBI specifically. Based on previous research in wayfinding, this association may be a result of an impairment in the ability of people with TBI to use distal landmarks to build a mental map of space due to hippocampal damage. However, further research is needed to explore this, for example, using fMRI technology. The finding that route learning using proximal landmarks seems to be a relatively spared ability in people with TBI could have important clinical implications for rehabilitation. With more research evidence, it may be possible in the future to support people with TBI with navigation difficulties to use an egocentric proximal landmark strategy to learn routes in their communities, increasing their independence.

6. References

Aguirre, G. K. & D'Esposito, M. (1999). Topographical disorientation: a synthesis and taxonomy. *Brain : A Journal of Neurology, 122(9),* 1613–1628. Retrieved from http://www.ncbi.nlm.nih.gov/pubmed/10468502

Atkins, C. M. (2011). Decoding hippocampal signaling deficits after traumatic brain injury. *Translational Stroke Research, 2(4),* 546–555. doi:10.1007/s12975-011-0123-z

Barrash, J., Damasio, H., Adolphs, R. & Tranel, D. (2000). The neuroanatomical correlates of route learning impairment. *Neuropsychologia, 38(6),* 820–36. Retrieved from http://www.ncbi.nlm.nih.gov/pubmed/10689057

Bohbot, V. & Corkin, S. (2007). Posterior parahippocampal place learning in HM. *Hippocampus, 17(9),* 863-872. Retrieved from http://onlinelibrary.wiley.com/doi/10.1002/hipo.20313/full

Bohbot, V., Iaria, G. & Petrides, M. (2004). Hippocampal function and spatial memory: evidence from functional neuroimaging in healthy participants and performance of patients with medial temporal lobe. *Neuropsychology, 18(3),* 418-425. Retrieved from http://psycnet.apa.org/journals/neu/18/3/418/

Burgess, N., Maguire, E. A. & O'Keefe, J. O. (2002). The human hippocampus and spatial and episodic memory. *Neuron, 35(4),* 625–641. doi:10.1016/S0896-6273(02)00830-9

Colombo, D., Serino, S., Tuena, C., Pedroli, E., Dakanalis, A., Cipresso, P., & Riva, G. (2017). Egocentric and allocentric spatial reference frames in aging: A systematic review. *Neuroscience and Biobehavioral Reviews, 80,* 605-621. http://dx.doi.org/10.1016/j.neubiorev.2017.07.012

Coughlan, A. K., Hollows, S. E., & Coughlin, A. K. (1985). *The Adult Memory and Information Processing Battery (AMIPB): Test Manual.* Psychology Department, St James' Hospital.

Darken, R. P. & Banker, W. P. (1998). Navigating in natural environments: A virtual environment training transfer study. In *Virtual Reality Annual International Symposium, 1998. Proceedings, IEEE,* 12-19.

DeCondappa, O. (2016). The use of landmark-based wayfinding strategies across the adult lifespan. (PhD thesis), University of Bournemouth, UK. Retrieved from http://eprints.bournemouth.ac.uk/24751/1/DE%20CONDAPPA%2C%20OlivierPh.D.2016.pdf

Eichenbaum, H., Stewart, C. & Morris, R. G. M. (1990). Hippocampal representation in place learning. *The Journal of Neuroscience 10(11),* 3531-3542. Retrieved from http://www.jneurosci.org/content/10/11/3531.short

Faul, F., Erdfelder, E., Lang, A-G. & Buchner, A. (2007). G*Power 3: a flexible statistical power analysis program for the social, behavioral, and biomedical sciences. *Behavior Research Methods, 39(2),* 175-91.

Field, A. (2009). *Discovering statistics using SPSS. (3rd ed.).* Los Angeles, London, New Delhi, Singapore and Washington DC: Sage

Goodrich-Hunsaker, N. J., Livingstone, S. A., Skelton, R. W. & Hopkins, R. O. (2010). Spatial deficits in a virtual water maze in amnesic participants with hippocampal damage. *Hippocampus, 20(4),* 481-91. doi:10.1002/hipo.20651

Green, R. E. A., Colella, B., Maller, J. J., Bayley, M., Glazer, J., & Mikulis, D. J. (2014). Scale and pattern of atrophy in the chronic stages of moderate-severe TBI. *Frontiers in Human Neuroscience, 8,* 67. doi:10.3389/fnhum.2014.00067

Harris, M. A., Wiener, J. M. & Wolbers, T. (2012). Aging specifically impairs switching to an allocentric navigational strategy. *Frontiers in Aging Neuroscience, 4(29).* http//doi:10.3389/fnagi.2012.00029

Hartley, T., Maguire, E., Spiers, H. & Burgess, N. (2003). The well-worn route and the path less traveled: distinct neural bases of route following and wayfinding in humans. *Neuron, 37(5),* 877-888.

Iaria, G., Petrides, M., Dagher, A., Pike, B. & Bohbot, V. D. (2003). Cognitive strategies dependent on the hippocampus and caudate nucleus in human navigation: variability and change with practice. The Journal of Neuroscience : *The Official Journal of the Society for Neuroscience, 23(13),* 5945-5952. Retrieved from http://www.ncbi.nlm.nih.gov/pubmed/12843299

King, J. A, Burgess, N., Hartley, T., Vargha-Khadem, F. & O'Keefe, J. (2002). Human hippocampus and viewpoint dependence in spatial memory. *Hippocampus, 12(6),* 811–820. doi:10.1002/hipo.10070

Kinnunen, K. M., Greenwood, R., Powell, J. H., Leech, R., Hawkins, P. C., Bonnelle, V., (2011). White matter damage and cognitive impairment after traumatic brain injury. *Brain 134,* 449–463. doi: 10.1093/brain/awq347

Lezak, M. D., Howieson, D. B., Bigler, E. D., & Tranel, D. (2012). *Neuropsychological assessment (5th Edition).* Oxford University Press.

Livingstone, S.A. & Skelton, R.W. (2007). Virtual environment navigation tasks and the assessment of cognitive deficits in individuals with brain injury. *Behavioural Brain Research, 185(1),* 21-31. doi:10.1016/j.bbr.2007.07.015

Lloyd, J. (2007). An investigation into route learning strategies for people with acquired brain injury (Doctoral thesis), University of Birmingham, UK.

Lloyd, J., Riley, G. A. & Powell, T. E. (2009b). Errorless learning of novel routes through a virtual town in people with acquired brain injury. *Neuropsychological Rehabilitation: An International Journal, 19(1),* 98-109.

Mallot, H. A. & Gillner, S. (2000). Route navigating without place recognition: What is recognised in recognition-triggered responses? *Perception, 29(1),* 43–55. doi:10.1068/p2865

Meilinger, T., Knauff, M. & Bulthoff, H. (2008). Working Memory in Wayfinding—A Dual Task Experiment in a Virtual City. *Cognitive Science: A Multidisciplinary Journal, 32(4),* 755-770. doi:10.1080/03640210802067004

Muller, R. & Bostock, E. (1994). On the directional firing properties of hippocampal place cells. *Journal of Neuroscience, 14(12),* 7235-7251. Retrieved from http://www.researchgate.net/profile/Robert_Muller5/publication/15203414_On_the_directional_firing_properties_of_hippocampal_place_cells/links/0912f50c0b952ba47e000000 .pdf

Nadel, L., & Hardt, O. (2004). The Spatial Brain. *Neuropsychology, 18(3),* 473-476. doi:10.1037/0894-4105.18.3.473

Nakase-Richardson, R., Sherer, M., Seel, R. T., Hart, T., Hanks, R., Arango-Lasprilla, J. C, ….. Hammond, F. (2011). Utility of post-traumatic amnesia in predicting 1-year

productivity following traumatic brain injury: comparison of the Russell and Mississippi PTA classification intervals. *Journal of Neurology Neurosurgery Psychiatry, 82*, 494-499.

Nice, L. (2015). An investigation into community travel patterns, navigational strategies and virtual reality route learning after an acquired brain injury (PhD thesis). University of Birmingham, UK.

O'Keefe, J. & Dostrovsky, J. (1971). The hippocampus as a spatial map. Preliminary evidence from unit activity in the freely-moving rat. *Brain Research, 34(1),* 171-175. Retrieved from http://www.sciencedirect.com/science/article/pii/0006899371903581

Possin, K. L., Kim, H., Geschwind, M. D., Moskowitz, T., Johnson, E. T., Sha, S. J, …… Kramer, J. H. (2017). Egocentric and allocentric visuospatial working memory in premotor Huntington's Disease: a double dissociation with caudate and hippocampal volume. *Neuropsychologia, 101*, 57-64. doi: 10.1016/j.neuropsychologia.2017.04.022

Ruggiero, G., Frassinetti, F., Iavarone, A. & Iachini, T. (2014). The lost ability to find the way: topographical disorientation after a left brain lesion. *Neuropsychology, 28(1),* 147-60. doi:10.1037/neu0000009

Serra-Grabulosa, J. M. (2005). Cerebral correlates of declarative memory dysfunctions in early traumatic brain injury. *Journal of Neurology, Neurosurgery & Psychiatry*, *76(1),* 129–131. doi:10.1136/jnnp.2004.027631

Skelton, R. W., Ross, S. P., Nerad, L., & Livingstone, S. A. (2006). Human spatial navigation deficits after traumatic brain injury in the arena maze, a virtual Morris water maze. *Brain Injury, 20(2),* 189-203. doi:10.1080/02699050500456410

Sorita, E., N'kaoua, B., Larrue, F., Criquillon, J., Simion, A., Sauzéon, H., & Mazaux, J-M. (2012). Do patients with traumatic brain injury learn a route in the same way in real and virtual environments? *Disability and Rehabilitation, 35(16),* 1371-1379. doi:10.3109/09638288.2012.738761

Stanton, D., Wilson, P., Foreman, N., & Duffy, H. (2000). Virtual environments as spatial training aids for children and adults with physical disabilities. In 3rd International Conference on Disability, Virtual Reality and Associated Technologies.

Stevens, J. (2002). *Applied multivariate statistics for the social sciences (4th ed.).* Mahwah, NJ: Lawrence Erlbaum Associates Publishers

Tate, D. F., & Bigler, E. D. (2000). Fornix and hippocampal atrophy in traumatic brain injury. *Learning & memory, 7(6),* 442-446. Retrieved from http://learnmem.cshlp.org/content/7/6/442.short

Toglia, J. & Kirk, U. (2000). Understanding awareness deficits following brain injury. *NeuroRehabilitation, 15(1),* 57-70. Retrieved from http://www.ncbi.nlm.nih.gov/pubmed/11455082

Trullier, O., Wiener, S. I., Berthoz, A. & Meyer, J. A. (1997). Biologically based artificial navigation systems: review and prospects. *Progress in Neurobiology, 51(5),* 483-544. Retrieved from http://www.ncbi.nlm.nih.gov/pubmed/9153072

Van der Ham, I. J. M., Van Zandvoort, M. J. E., Meilinger, T., Bosch, S. E., Kant, N., & Postma, A. (2010). Spatial and temporal aspects of navigation in two neurological patients. *Neuro Report, 21,* 685–689, doi:10.1097/WNR.0b013e32

Waller, D. & Lippa, Y. (2007). Landmarks as beacons and associative cues: their role in route learning. *Memory & Cognition, 35(5),* 910-24. Retrieved from http://www.ncbi.nlm.nih.gov/pubmed/17910176

Wechsler, D. (2009). Wechsler Memory Scale-(WMS-IV). The Psychological Corporation: New York.

Wolf, J. A., & Koch, P. A. (2016). Disruption of network synchrony and cognitive dysfunction after traumatic brain injury. *Frontiers in Systems Neuroscience, 10(43).* doi: 10.3389/fnsys.2016.00043

Woolley, D. G., Mantini, D., Coxon, J. P., D'Hogge, R., Swinnen, S. P., & Wenderoth, N. (2015). Virtual water maze learning in humans increases functional connectivity between posterior hippocampus and dorsal caudate. *Human Brain Mapping, 36(4),* 1265-1277.

iii) Public Dissemination Document

PUBLIC DISSEMINATION DOCUMENT: VIRTUAL REALITY ROUTE LEARNING IN PEOPLE WITH TRAUMATIC BRAIN INJURY

---

1. BACKGROUND AND PURPOSE

People with brain injuries often experience difficulties navigating from one location to another which has been found to negatively impact on independence, community integration and mental wellbeing (Barrash, Damasio, Adolphs, & Tranel, 2000; Juengst, Arenth, Raina, McCue & Skidmore, 2014; Rivest, Svoboda, McCarthy & Moscovitch 2016). However, neuro-rehabilitation services do not currently have a clear set of evidence-based guidelines for how to effectively rehabilitate navigation. If research evidence for an effective navigation rehabilitation strategy can be found and implemented in services then this could be of great benefit to people with brain injuries.

The aim of this document is to highlight the key findings from a systematic review and an experimental research study that have been conducted into navigation difficulties in people with brain injuries. It was hoped that the findings from the experimental research study would add to the literature base about how route learning difficulties in people with traumatic brain injury (TBI) relates to difficulties using particular types of navigation strategies and types of landmarks in the environment to learn routes. It was hoped that the findings from a systematic review on the effectiveness of rehabilitation strategies for improving navigation difficulties in people with brain injuries would suggest an evidence-base for a strategy that can be used in navigation rehabilitation in clinical practice.

---

2. SYSTEMATIC REVIEW

2.1 METHOD

Research databases were searched for studies which had investigated the effectiveness of a rehabilitation strategy for improving navigation in people with brain injuries. The findings of the studies were compared and evaluated according to whether there were any weaknesses in the study design which could have led to invalid and unreliable conclusions.

2.2 RESULTS

Fifteen studies were found which met the inclusion criteria for the review. The studies were categorised according to the type of rehabilitation strategy used. This included strategies generated by the person such as learning routes through a guided procedure without making errors (errorless learning) which can then be implicitly recalled by the person (e.g. Brooks et al., 1999; Lloyd, Riley & Powell, 2009b). Another category of rehabilitation strategy included external strategies which focussed on making the environment easier to navigate such as having a written list of directions with landmarks as external cues (e.g. Sohlberg, Fickas, Hung, & Fortier, 2007). Some studies also used a hybrid approach combining a person and an environmentally-oriented strategy (e.g. Rivest et al., 2016).

Overall, the review found that there is currently not enough good quality evidence for the effectiveness of any one particular strategy on which to base guidelines for the rehabilitation of navigation impairment. Nevertheless, the quality of three of the studies was reasonably good. One of these studies found effectiveness for teaching people with brain injury routes using an errorless learning technique through a Virtual Reality simulation of a real town (Lloyd et al., 2009b). Another study found that women with brain injury may benefit from a group therapy teaching help-seeking that can be used if they become lost whilst navigating (Cho et al., 2017). The third study found effectiveness for auditory direction prompts delivered through an electronic device (Sohlberg et al., 2007).

2.3 NEXT STEPS

More research needs to be conducted investigating the effectiveness of different types of rehabilitation strategies for improving navigation and the research needs to be of good quality so that it is possible to make valid and reliable conclusions to inform clinical practice. Nevertheless, due to the variation in types of brain injuries and physical and cognitive effects it may not be possible to develop a strategy that will be effective for this whole population (Incoccia, Magnotti, Iaria, Piccardia & Guariglia, 2009). Instead, future research may find particular strategies to be effective depending on the severity of navigation impairment and the setting of rehabilitation, for example, a hospital compared to a community setting. Services may then be able to develop a toolbox of clinically effective strategies which can be used depending on the specific needs of each service-user.

---

3. EXPERIMENTAL RESEARCH STUDY

3.1 METHOD

Participants were 17 people with TBI recruited from rehabilitation services in the West Midlands. A control group of 17 participants who had never had a brain injury were matched to the participants with TBI for age, gender and education-level. Both groups of participants completed a Virtual Reality route learning task in two conditions, one where only landmarks at street-level were available and one where only landmarks in the distance were available. Both routes were made up of 18 turnings. Participants were given three laps to learn each route where yellow arrows were present on the computer screen to guide the way. Participants completed a test lap for each route where the arrows disappeared and participants had to complete the routes from memory. This resulted in a total correct turning score for each route for each participant out of a maximum of 18. Both groups also completed a questionnaire after

each route which aimed to assess whether participants used an association strategy to learn the routes e.g. 'I turn left when I see the bus stop' or a bird's-eye view map strategy e.g. mentally visualising their position in the environment from an overhead perspective to work out the correct direction.

3.2 RESULTS

Participants with TBI were found to be impaired at route learning with landmarks in the distance as they made significantly fewer correct turnings in the route with landmarks in the distance than control participants (see Figure 1). However, participants with TBI were not significantly worse than control participants at learning the route with landmarks at street-level. Furthermore, both groups of participants reported using an association strategy to learn both routes and not a bird's-eye view map strategy.

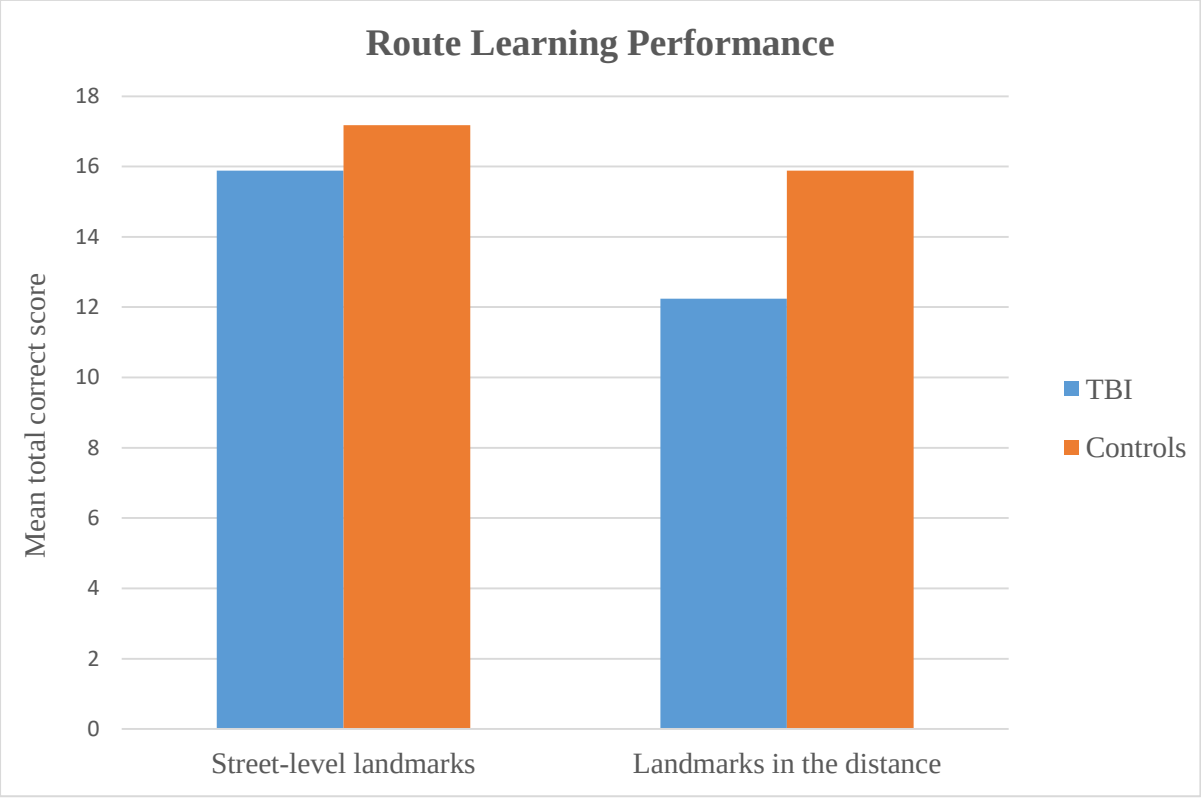


Figure 1. Bar graph showing the mean correct turnings (out of 18) as a function of group and route

3.3 NEXT STEPS

As participants with TBI were found to be impaired at route learning using landmarks in the distance specifically, further research should be conducted to investigate whether this is linked to damage in a particular brain structure and research using Magnetic Resonance Imaging (MRI) technology could explore this. There is previous research evidence to suggest that people with brain injuries are impaired at navigating in familiar and unfamiliar locations due to damage to the part of the brain called the hippocampus which is important in building a mental map of the spatial relationships between landmarks to support successful navigation (Bohbot & Corkin, 2007; Livingstone & Skelton, 2007; Possin et al., 2017). However, the finding in the current study that people with TBI are not impaired in their ability to route learn using landmarks at street-level suggests that it may be possible to teach people with TBI to associate landmarks at street-level with directions to effectively rehabilitate route learning impairment. Future research should further explore this.

4. References

Barrash, J., Damasio, H., Adolphs, R. & Tranel, D. (2000). The neuroanatomical correlates of route learning impairment. *Neuropsychologia, 38(6),* 820–36. Retrieved from http://www.ncbi.nlm.nih.gov/pubmed/10689057

Bohbot, V. & Corkin, S. (2007). Posterior parahippocampal place learning in HM. *Hippocampus, 17(9),* 863-872. Retrieved from http://onlinelibrary.wiley.com/doi/10.1002/hipo.20313/full

Brooks, B. M., McNeil, J. E., Rose, F. D., Greenwood, R. J., Attree, E. A., & Leadbetter, A. G. (1999). Route learning in a case of amnesia: a preliminary investigation into the efficacy of training in a virtual environment. *Neuropsychological Rehabilitation, 9(1),* 63–76. doi: 10. 1080/713755589

Cho, S. Y., Sohlberg, M. M., Albin, R., Diller, L., Horner, R., Rath, J., & Bullis, M. (2017). Training adults with acquired brain injury how to help seek when wayfinding: an understudied critical life skill. *Neuropsychological Rehabilitation, 6,* 928-945. doi: 10.1080/09602011.2017.1344131

Livingstone, S.A. & Skelton, R.W. (2007). Virtual environment navigation tasks and the assessment of cognitive deficits in individuals with brain injury. *Behavioural Brain Research, 185(1),* 21-31. doi:10.1016/j.bbr.2007.07.015

Lloyd, J., Riley, G. A. & Powell, T. E. (2009b). Errorless learning of novel routes through a virtual town in people with acquired brain injury. *Neuropsychological Rehabilitation: An International Journal, 19(1),* 98-109.

Incoccia, C., Magnotti, L., Iaria, G., Piccardi, L., & Guariglia, C. (2009). Topographical disorientation in a patient who never developed navigational skills: the (re)habilitation

treatment. *Neuropsychological Rehabilitation, 19(2),* 291–314. doi: 10.1080/0960201080 2188344

Juengst, S. B., Arenth, P. M., Raina, K. D., McCue, M., & Skidmore, E. R. (2014). Affective state and community integration after traumatic brain injury. *American Journal of Physical Medicine and Rehabilitation, 93(12),* 1086-1094. doi: 10.1097/PHM.0000000000000163

Possin, K. L., Kim, H., Geschwind, M. D., Moskowitz, T., Johnson, E. T., Sha, S. J, …… Kramer, J. H. (2017). Egocentric and allocentric visuospatial working memory in premotor Huntington's Disease: a double dissociation with caudate and hippocampal volume. *Neuropsychologia, 101,* 57-64. doi: 10.1016/j.neuropsychologia.2017.04.022

Rivest, J., Svoboda, E., McCarthy, J., & Moscovitch, M. (2016). A case study of topographical disorientation: behavioural intervention for achieving independent navigation. *Neuropsychological Rehabilitation, 28(5),* 797-817. doi: 10.1080/09602011.2016.1160833

Sohlberg, M. M., Fickas, S., Hung, P-F., & Fortier, A. (2007). A comparison of four prompt modes for route finding for community travellers with severe cognitive impairments. *Brain Injury, 21(5),* 531-538. doi: 10.1080/02699050701311000

Appendix B: Navigation Strategy Questionnaire

Route-learning questionnaire
Please tick the most appropriate box that applies to you

	Not at all	A Little	A moderate amount	A lot	Almost completely
1. I tried to remember the sequence of left and right turns I took					
2. I had no idea of the way so I guessed					
3. I tried to think in what direction I was going, in terms of North- South, East-West					
4. I tried to keep track of the general direction I came from and which way I was going					
5. I used landmarks on the corners of the street to help me build up a 'birds-eye' map					
6. I associated a landmark on the corner of the street with which way to turn at each junction					
7. I used landmarks in the distance to help me build up a 'birds-eye' map					
8. I associated landmarks in the distance with which way to turn at each junction					
9. I followed my instinct without knowing how I did it					
10. I used a verbal description of the route as I went along and remembered that					
11. Please describe any other strategies you used to remember the route:					

Appendix C: Landmark Stimuli

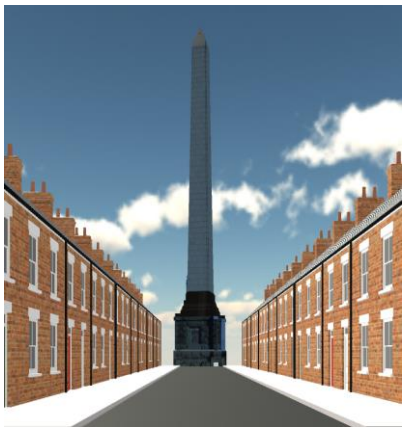
Distal Landmarks

1) Car park

2) Grey building

3) Blue building

4) Waterworks



5) Monument

6) Church

7) Ferris wheel

8) Mosque

9) Yellow clock tower

		
10) Crane	11) Cooling tower	12) Control tower
		
13) War memorial	14) Town hall	15) Clock tower
		
16) Tower block	17) Archway	18) Pylon

Proximal Landmarks

1) Traffic cone

2) Bicycle rack

3) Litter bin

4) Speed camera

5) Skip

6) Tree

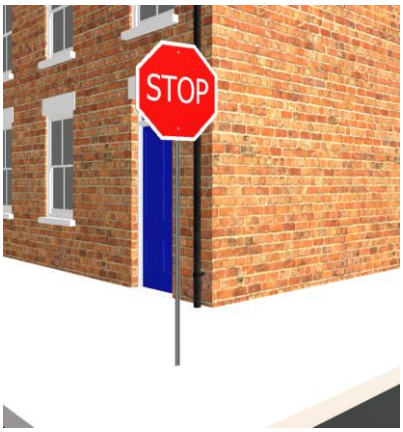
7) Lamp post

8) Traffic lights

9) Advertising board

10) Bus stop

11) Telephone box



12) Stop sign

13) Post box

14) Bench

15) Waste bin

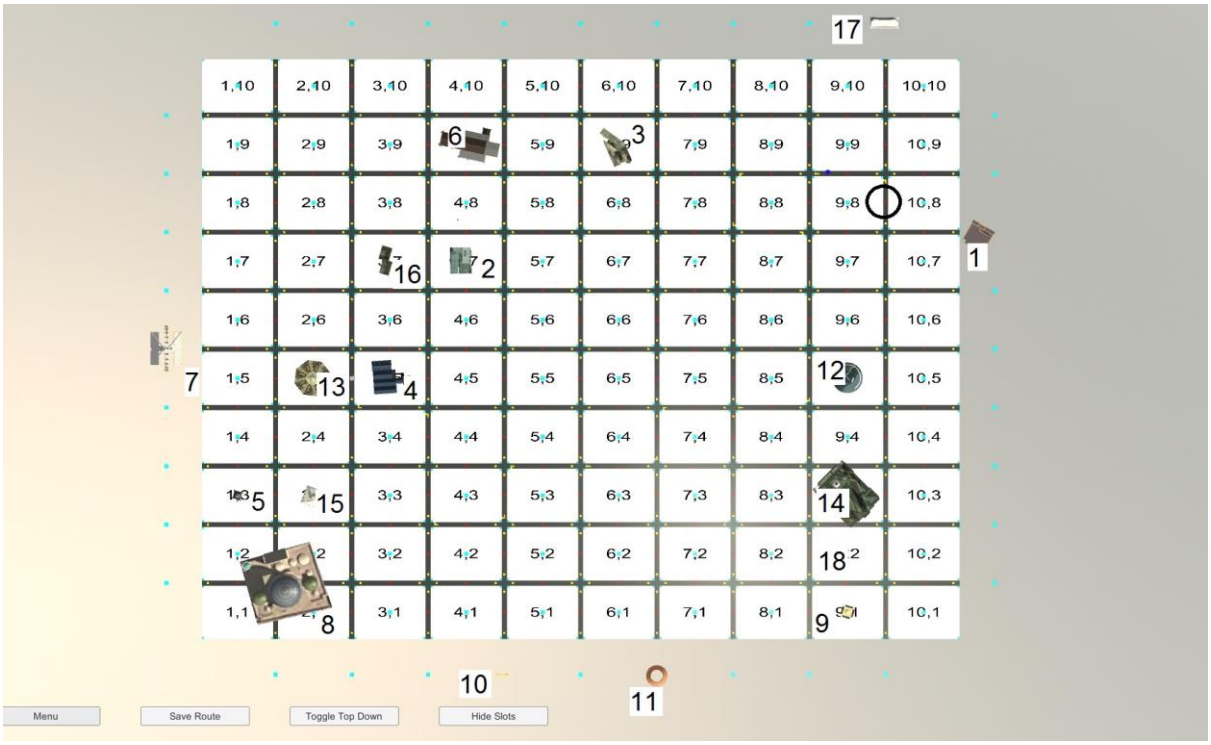
16) Flower box

17) Station sign

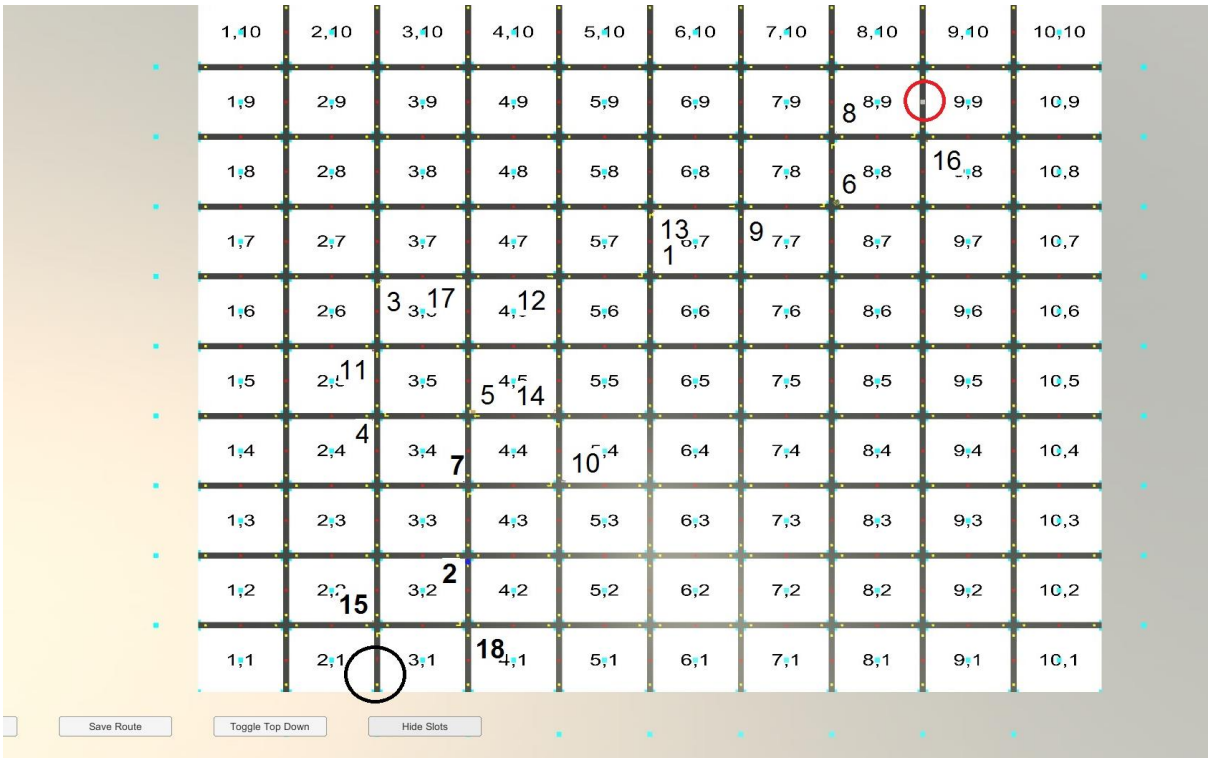
18) Street light

Appendix D: Landmark placements in the virtual environment

Distal Landmarks



Proximal Landmarks



Appendix F: NICE quality checklist for quantitative studies assessment ratings for the experimental group studies included in the review

Study: Evans et al. (2000) A comparison of errorless and trial and error learning methods for teaching individuals with acquired memory deficits.	
Study Design: Group study approach – within-subjects	
Section 1: Population:	
1.1. Is the source population or source area well described? Was the country (e.g. developed or non-developed, type of healthcare system), setting (primary schools, community centres etc.), location (urban, rural), population demographics etc. adequately described?	Comments: Nine health centres across Europe and 1 in Argentina. Exact location and population demographics not clearly described. Not clear what kind of health centre. Rating: -
1.2. Is the eligible population or area representative of the source population or area? Was the recruitment of individuals, clusters or areas well defined (e.g. advertisement, birth register)? Was the eligible population representative of the source? Were important groups under-represented?	Comments: Not clear how participants were recruited from the health centres, other than that they met the inclusion criteria. Not clear why/how the exact number of participants was reached for each experiment in each phase (as this varied but the same inclusion criteria applied). Sample demographics were provided but no gender information. TBI may be under-represented. Rating: -
1.3 Do the selected participants or areas represent the eligible population or area? Was the method of selection of participants from the eligible population well described? What % of selected individuals or clusters agreed to participate? Were there any sources of bias? Were the inclusion or exclusion criteria explicit and appropriate?	Comments: Method of selecting participants was not well described, just that they met the inclusion criteria and were attending one of the health centres. No percentages reported. Inclusion criteria were stated but how the sample sizes were reached for each experiment was not clear. Rating: -
Section 2: Method of allocation to intervention (or comparison)	
2.1 Allocation to intervention (or comparison). How was selection bias minimised? Was allocation to exposure and comparison randomised? Was it truly random ++ or pseudo-randomised + (e.g. consecutive admissions)? If not randomised, was significant confounding likely (−) or not (+)? If a cross-over, was order of intervention randomised?	Comments: Within-subjects design – all p's did the trial and error, errorless and backwards chaining conditions for experiment 2, trial and error and forward chaining exp 5, trial and error and errorless exp 6, exp 9. Order of conditions was counterbalanced but not randomised. Rating: -

2.2 Were interventions (and comparisons) well described and appropriate? Were interventions and comparisons described in sufficient detail (i.e. enough for study to be replicated)? Was comparisons appropriate (e.g. usual practice rather than no intervention)?	Comments: The experimental conditions were described in sufficient detail for replication. Comparisons between conditions was appropriate. Rating: ++
2.3 Was the allocation concealed? Could the person(s) determining allocation of participants or clusters to intervention or comparison groups have influenced the allocation? Adequate allocation concealment (++) would include centralised allocation or computerised allocation systems.	Comments: N/A
2.4 Were participants or investigators blind to exposure and comparison? Were participants and investigators – those delivering or assessing the intervention kept blind to intervention allocation? (Triple or double blinding score ++) If lack of blinding is likely to cause important bias, score −.	Comments: Blinding was not possible. Therapists needed to know condition in order to deliver the experiment. Similarly, participant needed to know the instructions for the condition. Rating: N/A
2.5 Was the exposure to the intervention and comparison adequate? Is reduced exposure to intervention or control related to the intervention (e.g. adverse effects leading to reduced compliance) or fidelity of implementation (e.g. reduced adherence to protocol)? Was lack of exposure sufficient to cause important bias?	Comments: Yes Rating: +
2.6 Was contamination acceptably low? Did any in the comparison group receive the intervention or vice versa? If so, was it sufficient to cause important bias? If a cross-over trial, was there a sufficient wash-out period between interventions?	Comments: Rating: N/A – within-subjects design
2.7 Were other interventions similar in both groups? Did either group receive additional interventions or have services provided in a different manner?	Comments: NR

Were the groups treated equally by researchers or other professionals? Was this sufficient to cause important bias?	
2.8 Were all participants accounted for at study conclusion? Were those lost-to-follow-up (i.e. dropped or lost pre-, during or post-intervention) acceptably low (i.e. typically <20%)? Did the proportion dropped differ by group? For example, were drop-outs related to the adverse effects of the intervention?	Comments: NR
2.9 Did the setting reflect usual UK practice? Did the setting in which the intervention or comparison was delivered differ significantly from usual practice in the UK? For example, did participants receive intervention (or comparison) condition in a hospital rather than a community-based setting?	Comments: NR – the setting was not sufficiently described to determine whether it reflected normal UK practice.
2.10 Did the intervention or control comparison reflect usual UK practice? Did the intervention or comparison differ significantly from usual practice in the UK? For example, did participants receive intervention (or comparison) delivered by specialists rather than GPs? Were participants monitored more closely?	Comments: NR
Section 3 Outcomes	
3.1 Were outcome measures reliable? Were outcome measures subjective or objective (e.g. biochemically validated nicotine levels ++ vs self-reported smoking −)? How reliable were outcome measures (e.g. inter- or intra-rater reliability scores)? Was there any indication that measures had been validated (e.g. validated against a gold standard measure or assessed for content validity)?	Comments: Objective measure of test accuracy – exp 2 – 3 blocks of 3 test trials of a 10-step route – score/90, also a long delay test score of a single trial after an hour score/10 – 1 point for each correct step taken. Similar scoring procedure in exp 5, 6, 9. One researcher doing the scoring? No inter-rater reliability reported. No validity/reliability data for the route/stepping stone maze tasks reported. Rating: +
3.2 Were all outcome measurements complete? Were all or most study participants who met the defined study outcome definitions likely to have been identified?	Comments: Outcome measures for all p's who took part. But don't know drop out. Rating: +

3.3 Were all important outcomes assessed? Were all important benefits and harms assessed? Was it possible to determine the overall balance of benefits and harms of the intervention versus comparison?	Comments: Important outcomes to the study aims were assessed. Rating: +
3.4 Were outcomes relevant? Where surrogate outcome measures were used, did they measure what they set out to measure? (e.g. a study to assess impact on physical activity assesses gym membership – a potentially objective outcome measure – but is it a reliable predictor of physical activity?)	Comments: Outcomes relevant to the hypotheses Rating: +
3.5 Were there similar follow-up times in exposure and comparison groups? If groups are followed for different lengths of time, then more events are likely to occur in the group followed-up for longer distorting the comparison. Analyses can be adjusted to allow for differences in length of follow-up (e.g. using person-years).	Comments: N/A – no follow up
3.6 Was follow-up time meaningful? Was follow-up long enough to assess long-term benefits or harms? Was it too long, e.g. participants lost to follow-up?	Comments: N/A
Section 4: Analyses	
4.1 Were exposure and comparison groups similar at baseline? If not, were these adjusted? Were there any differences between groups in important confounders at baseline? If so, were these adjusted for in the analyses (e.g. multivariate analyses or stratification). Were there likely to be any residual differences of relevance?	Comments: N/A – within-subjects design
4.2 Was intention to treat (ITT) analysis conducted?	Comments: NR

Were all participants (including those that dropped out or did not fully complete the intervention course) analysed in the groups (i.e. intervention or comparison) to which they were originally allocated?	
4.3 Was the study sufficiently powered to detect an intervention effect (if one exists)? A power of 0.8 (that is, it is likely to see an effect of a given size if one exists, 80% of the time) is the conventionally accepted standard. Is a power calculation presented? If not, what is the expected effect size? Is the sample size adequate?	Comments: NR A power calculation is not presented.
4.4 Were the estimates of effect size given or calculable? Were effect estimates (e.g. relative risks, absolute risks) given or possible to calculate?	Comments: NR
4.5 Were the analytical methods appropriate? Were important differences in follow-up time and likely confounders adjusted for? If a cluster design, were analyses of sample size (and power), and effect size performed on clusters (and not individuals)? Were subgroup analyses pre-specified?	Comments: ANOVA to measure difference in means between conditions – appropriate. Not clear whether confounders adjusted for. Rating: +
4.6 Was the precision of intervention effects given or calculable? Were they meaningful? Were confidence intervals or p values for effect estimates given or possible to calculate? Were CI's wide or were they sufficiently precise to aid decision-making? If precision is lacking, is this because the study is under-powered?	Comments: p-values for effect sizes were given and Bonferroni correction to the alpha for significance was applied. CIs were not reported. Rating: -
Section 5: is there a clear rationale for rehabilitation approach used?	Aims and hypothesis stated that errorless learning would be more beneficial for learning practical skills than other types of learning – not a hypothesis specific to route learning rehabilitation though Rating: +
Section 6: Summary	
6.1 Are the study results internally valid (i.e. unbiased)?	Comments:

How well did the study minimise sources of bias (i.e. adjusting for potential confounders)? Were there significant flaws in the study design? **6.2 Are the findings generalisable to the source population (i.e. externally valid)?** Are there sufficient details given about the study to determine if the findings are generalisable to the source population? Consider: participants, interventions and comparisons, outcomes, resource and policy implications.	Internal validity: Lack of description about recruitment process and selection of p's is a potential source of bias. Objective scoring procedure for route/stepping stone maze score. But validity and reliability of tests used not reported. Rating: - External validity: Not clear about the source population, p details such as gender, education level, tasks were paper and pencil tests, no outcome measure of real-world route learning/wayfinding. Rating: -

Study: Errorless learning of novel routes through a virtual town in people with acquired brain injury. Lloyd, Riley & Powell. (2009).	
Study Design: Repeated measures, within-subjects design	
Section 1: Population:	
1.3. Is the source population or source area well described? Was the country (e.g. developed or non-developed, type of healthcare system), setting (primary schools, community centres etc.), location (urban, rural), population demographics etc. adequately described?	Comments: The population was described as patients with ABI attending outpatient rehab centres and day centres. Not clear if this was a rural or city community. Developed country – UK. Rating: +
1.4. Is the eligible population or area representative of the source population or area? Was the recruitment of individuals, clusters or areas well defined (e.g. advertisement, birth register)? Was the eligible population representative of the source? Were important groups under-represented?	Comments: Not clear how the researchers got to 20 patients from the outpatient day centres other than they met the inclusion/exclusion criteria. Not clear if important groups were underrepresented – does not report numbers of males/females Rating: -
1.3 Do the selected participants or areas represent the eligible population or area? Was the method of selection of participants from the eligible population well described? What % of selected individuals or clusters agreed to participate? Were there any sources of bias? Were the inclusion or exclusion criteria explicit and appropriate?	Comments: Participants were selected based on meeting inclusion/exclusion criteria, but not clear how 20 were selected specifically. Not reported the % of people asked who agreed to participate. The inclusion and exclusion criteria were explicit and appropriate. Rating: +
Section 2: Method of allocation to intervention (or comparison)	

2.1 Allocation to intervention (or comparison). How was selection bias minimised? Was allocation to exposure and comparison randomised? Was it truly random ++ or pseudo-randomised + (e.g. consecutive admissions)? If not randomised, was significant confounding likely (−) or not (+)? If a cross-over, was order of intervention randomised?	Comments: N/A - Within-subjects design so no allocation of p's to groups. Order of routes and allocation of routes to errorless or errorful conditions was counterbalanced.
2.2 Were interventions (and comparisons) well described and appropriate? Were interventions and comparisons described in sufficient detail (i.e. enough for study to be replicated)? Was comparisons appropriate (e.g. usual practice rather than no intervention)?	Comments: The procedure for p's completing the errorful and errorless routes was well described and could be replicated. The procedure for the 2 conditions was comparable. Rating: ++
2.3 Was the allocation concealed? Could the person(s) determining allocation of participants or clusters to intervention or comparison groups have influenced the allocation? Adequate allocation concealment (++) would include centralised allocation or computerised allocation systems.	Comments: N/A
2.4 Were participants or investigators blind to exposure and comparison? Were participants and investigators – those delivering or assessing the intervention kept blind to intervention allocation? (Triple or double blinding score ++) If lack of blinding is likely to cause important bias, score −.	Comments: Researcher needed to know the condition. Rating: N/A
2.5 Was the exposure to the intervention and comparison adequate? Is reduced exposure to intervention or control related to the intervention (e.g. adverse effects leading to reduced compliance) or fidelity of implementation (e.g. reduced adherence to protocol)? Was lack of exposure sufficient to cause important bias?	Comments: All 20 p's completed both conditions. Rating: ++
2.6 Was contamination acceptably low? Did any in the comparison group receive the intervention or vice versa?	Comments: N/A – within-subjects

If so, was it sufficient to cause important bias? If a cross-over trial, was there a sufficient wash-out period between interventions?	
2.7 Were other interventions similar in both groups? Did either group receive additional interventions or have services provided in a different manner? Were the groups treated equally by researchers or other professionals? Was this sufficient to cause important bias?	Comments: N/A
2.8 Were all participants accounted for at study conclusion? Were those lost-to-follow-up (i.e. dropped or lost pre-, during or post-intervention) acceptably low (i.e. typically <20%)? Did the proportion dropped differ by group? For example, were drop-outs related to the adverse effects of the intervention?	Comments: No participants dropped out Rating: ++
2.9 Did the setting reflect usual UK practice? Did the setting in which the intervention or comparison was delivered differ significantly from usual practice in the UK? For example, did participants receive intervention (or comparison) condition in a hospital rather than a community-based setting?	Comments: Outpatient rehab/day centres reflects services in the UK. Rating: ++
2.10 Did the intervention or control comparison reflect usual UK practice? Did the intervention or comparison differ significantly from usual practice in the UK? For example, did participants receive intervention (or comparison) delivered by specialists rather than GPs? Were participants monitored more closely?	Comments: Researchers delivered the intervention but this would not be sig. diff from support workers doing the same in usual UK practice at day centres. Unclear whether using a VR programme would be usual UK practice as currently there is no rehab guidance for route learning in brain injury services Rating: +
Section 3 Outcomes	
3.1 Were outcome measures reliable? Were outcome measures subjective or objective (e.g. biochemically validated nicotine levels ++ vs self-reported smoking −)? How reliable were outcome measures (e.g. inter- or intra-rater reliability scores)?	Comments: Objective outcome measures – number of errors made on test trial of routes. Used neuro tests to confirm memory difficulties that have good psychometric properties.

Was there any indication that measures had been validated (e.g. validated against a gold standard measure or assessed for content validity)?	Not reported whether there was inter-rater reliability in error scores or whether the VR route test had been validated. Rating: +
3.2 Were all outcome measurements complete? Were all or most study participants who met the defined study outcome definitions likely to have been identified?	Comments: All p's completed the test trial of both routes. Rating: ++
3.3 Were all important outcomes assessed? Were all important benefits and harms assessed? Was it possible to determine the overall balance of benefits and harms of the intervention versus comparison?	Comments: The important outcome of whether p's made fewer errors remembering the routes after errorless vs errorful learning methods was assessed. No delayed recall measure. Rating: +
3.4 Were outcomes relevant? Where surrogate outcome measures were used, did they measure what they set out to measure? (e.g. a study to assess impact on physical activity assesses gym membership – a potentially objective outcome measure – but is it a reliable predictor of physical activity?)	Comments: Outcome measures were relevant. Rating: +
3.5 Were there similar follow-up times in exposure and comparison groups? If groups are followed for different lengths of time, then more events are likely to occur in the group followed-up for longer distorting the comparison. Analyses can be adjusted to allow for differences in length of follow-up (e.g. using person-years).	Comments: N/A
3.6 Was follow-up time meaningful? Was follow-up long enough to assess long-term benefits or harms? Was it too long, e.g. participants lost to follow-up?	Comments: N/A
Section 4: Analyses	
4.1 Were exposure and comparison groups similar at baseline? If not, were these adjusted? Were there any differences between groups in important confounders at baseline?	Comments: N/A – within-subjects design

If so, were these adjusted for in the analyses (e.g. multivariate analyses or stratification). Were there likely to be any residual differences of relevance?	
4.2 Was intention to treat (ITT) analysis conducted? Were all participants (including those that dropped out or did not fully complete the intervention course) analysed in the groups (i.e. intervention or comparison) to which they were originally allocated?	Comments: NR
4.3 Was the study sufficiently powered to detect an intervention effect (if one exists)? A power of 0.8 (that is, it is likely to see an effect of a given size if one exists, 80% of the time) is the conventionally accepted standard. Is a power calculation presented? If not, what is the expected effect size? Is the sample size adequate?	Comments: Power NR 20 is likely to be a sufficient sample size to detect an effect. NR
4.4 Were the estimates of effect size given or calculable? Were effect estimates (e.g. relative risks, absolute risks) given or possible to calculate?	Comments: Effect sizes were given. Data analysed using paired samples t-test. ANOVA used to analyse diff between number of errors across the 3 trials in errorful condition. Rating: +
4.5 Were the analytical methods appropriate? Were important differences in follow-up time and likely confounders adjusted for? If a cluster design, were analyses of sample size (and power), and effect size performed on clusters (and not individuals)? Were subgroup analyses pre-specified?	Comments: Analyses were appropriate. Rating: +
4.6 Was the precision of intervention effects given or calculable? Were they meaningful? Were confidence intervals or p values for effect estimates given or possible to calculate? Were CI's wide or were they sufficiently precise to aid decision-making? If precision is lacking, is this because the study is under-powered?	Comments: P-values were given. CIs not given. Rating: -

Section 5: is there a clear rationale for approach used?	Clear aims and rationale stated based on previous research. Hypothesis – study aimed to assess the benefits of errorless over errorful learning in route learning task Rating: ++
Section 5: Summary	
5.1 Are the study results internally valid (i.e. unbiased)? How well did the study minimise sources of bias (i.e. adjusting for potential confounders)? Were there significant flaws in the study design? **5.2 Are the findings generalisable to the source population (i.e. externally valid)?** Are there sufficient details given about the study to determine if the findings are generalisable to the source population? Consider: participants, interventions and comparisons, outcomes, resource and policy implications.	Comments: Internal validity: no follow-up or delayed recall route to see if the effect was maintained. Not clear how participants were recruited. Good rationale. Good description of procedures for replication. Appropriate analyses. Rating overall: + External validity: No test of real-world route learning – whether benefits generalised, however VR simulates the real-world. Rating overall: +

Study: Route learning in amnesia: a comparison of trial and error and errorless learning in patients with Korsakoff syndrome. Kessels, van Loon, Wester. (2007).	
Study Design: repeated measures, within-subjects design	
Section 1: Population:	
1.5. Is the source population or source area well described? Was the country (e.g. developed or non-developed, type of healthcare system), setting (primary schools, community centres etc.), location (urban, rural), population demographics etc. adequately described?	Comments: The country, setting and location were well described. Korsakoff clinic of the Psychiatric Hospital Vincent Van Gogh in the Netherlands. Population demographics not well described – patients with memory deficits? Rating: +
1.6. Is the eligible population or area representative of the source population or area? Was the recruitment of individuals, clusters or areas well defined (e.g. advertisement, birth register)? Was the eligible population representative of the source? Were important groups under-represented?	Comments: Convenience sample recruitment method. Patients with korsakoffs may not be representative of all patients with memory deficits. Rating: -
1.3 Do the selected participants or areas represent the eligible population or area?	Comments:

Was the method of selection of participants from the eligible population well described? What % of selected individuals or clusters agreed to participate? Were there any sources of bias? Were the inclusion or exclusion criteria explicit and appropriate?	How convenience sample was selected was not well described e.g. was it the first 10 patients asked or the first 10 patients on a clinic list? Not reported how many patients were asked in total. The inclusion and exclusion criteria were explicit but not clear why limited to korsakoffs patients only. Rating: -
Section 2: Method of allocation to intervention (or comparison)	
2.1 Allocation to intervention (or comparison). How was selection bias minimised? Was allocation to exposure and comparison randomised? Was it truly random ++ or pseudo-randomised + (e.g. consecutive admissions)? If not randomised, was significant confounding likely (−) or not (+)? If a cross-over, was order of intervention randomised?	Comments: N/A – within-subjects
2.2 Were interventions (and comparisons) well described and appropriate? Were interventions and comparisons described in sufficient detail (i.e. enough for study to be replicated)? Was comparisons appropriate (e.g. usual practice rather than no intervention)?	Comments: The 2 route conditions were well described in enough detail to replicate. Trial and error comparison appropriate. Test of trial and error not actually trial and error? (were corrected on the test if they made a mistake). Rating: +
2.3 Was the allocation concealed? Could the person(s) determining allocation of participants or clusters to intervention or comparison groups have influenced the allocation? Adequate allocation concealment (++) would include centralised allocation or computerised allocation systems.	Comments: N/A
2.4 Were participants or investigators blind to exposure and comparison? Were participants and investigators – those delivering or assessing the intervention kept blind to intervention allocation? (Triple or double blinding score ++) If lack of blinding is likely to cause important bias, score −.	Comments: experimenter needed to know the condition in order to present the right instructions Rating: N/A
2.5 Was the exposure to the intervention and comparison adequate?	Comments:

Is reduced exposure to intervention or control related to the intervention (e.g. adverse effects leading to reduced compliance) or fidelity of implementation (e.g. reduced adherence to protocol)? Was lack of exposure sufficient to cause important bias?	All p's completed both routes. Rating: ++
2.6 Was contamination acceptably low? Did any in the comparison group receive the intervention or vice versa? If so, was it sufficient to cause important bias? If a cross-over trial, was there a sufficient wash-out period between interventions?	Comments: N/A
2.7 Were other interventions similar in both groups? Did either group receive additional interventions or have services provided in a different manner? Were the groups treated equally by researchers or other professionals? Was this sufficient to cause important bias?	Comments: N/A
2.8 Were all participants accounted for at study conclusion? Were those lost-to-follow-up (i.e. dropped or lost pre-, during or post-intervention) acceptably low (i.e. typically <20%)? Did the proportion dropped differ by group? For example, were drop-outs related to the adverse effects of the intervention?	Comments: All p's completed the study. Rating: ++
2.9 Did the setting reflect usual UK practice? Did the setting in which the intervention or comparison was delivered differ significantly from usual practice in the UK? For example, did participants receive intervention (or comparison) condition in a hospital rather than a community-based setting?	Comments: The Dutch hospital setting seems to reflect usual UK practice of an inpatient facility for people with severe memory impairments. However the hospital setting is not well described. Rating: +
2.10 Did the intervention or control comparison reflect usual UK practice? Did the intervention or comparison differ significantly from usual practice in the UK? For example, did participants receive intervention (or comparison) delivered by specialists rather than GPs? Were participants monitored more closely?	Comments: Researchers who delivered the intervention would be similar to a support worker role in the hospital similar to the UK. Unclear whether the errorless learning intervention and trial and error comparison is similar to UK as there is no current guidance on rehab for route learning in UK brain injury services.

	Rating: +
Section 3 Outcomes	
3.1 Were outcome measures reliable? Were outcome measures subjective or objective (e.g. biochemically validated nicotine levels ++ vs self-reported smoking −)? How reliable were outcome measures (e.g. inter- or intra-rater reliability scores)? Was there any indication that measures had been validated (e.g. validated against a gold standard measure or assessed for content validity)?	Comments: Outcome measure was the number of errors made when tested on each route. Objective. Max number of errors = 8. Routes were around the hospital grounds – routes were chosen by the researchers. Inter-rater reliability not reported. Rating: +
3.2 Were all outcome measurements complete? Were all or most study participants who met the defined study outcome definitions likely to have been identified?	Comments: Rating: ++
3.3 Were all important outcomes assessed? Were all important benefits and harms assessed? Was it possible to determine the overall balance of benefits and harms of the intervention versus comparison?	Comments: No test of delayed recall/maintenance of learning. Rating: +
3.4 Were outcomes relevant? Where surrogate outcome measures were used, did they measure what they set out to measure? (e.g. a study to assess impact on physical activity assesses gym membership – a potentially objective outcome measure – but is it a reliable predictor of physical activity?)	Comments: Outcomes seemed relevant to the study, although no aims/hypotheses were stated to know the relevance. Rating: -
3.5 Were there similar follow-up times in exposure and comparison groups? If groups are followed for different lengths of time, then more events are likely to occur in the group followed-up for longer distorting the comparison. Analyses can be adjusted to allow for differences in length of follow-up (e.g. using person-years).	Comments: N/A – no follow up
3.6 Was follow-up time meaningful? Was follow-up long enough to assess long-term benefits or harms?	Comments: N/A – no follow up

Was it too long, e.g. participants lost to follow-up?	
Section 4: Analyses	
4.1 Were exposure and comparison groups similar at baseline? If not, were these adjusted? Were there any differences between groups in important confounders at baseline? If so, were these adjusted for in the analyses (e.g. multivariate analyses or stratification). Were there likely to be any residual differences of relevance?	Comments: N/A – within subjects
4.2 Was intention to treat (ITT) analysis conducted? Were all participants (including those that dropped out or did not fully complete the intervention course) analysed in the groups (i.e. intervention or comparison) to which they were originally allocated?	Comments: NR
4.3 Was the study sufficiently powered to detect an intervention effect (if one exists)? A power of 0.8 (that is, it is likely to see an effect of a given size if one exists, 80% of the time) is the conventionally accepted standard. Is a power calculation presented? If not, what is the expected effect size? Is the sample size adequate?	Comments: 10 p's is a small sample. Not likely to be sufficiently powered. Power calculation not reported. Rating: NR
4.4 Were the estimates of effect size given or calculable? Were effect estimates (e.g. relative risks, absolute risks) given or possible to calculate?	Comments: NR
4.5 Were the analytical methods appropriate? Were important differences in follow-up time and likely confounders adjusted for? If a cluster design, were analyses of sample size (and power), and effect size performed on clusters (and not individuals)? Were subgroup analyses pre-specified?	Comments: Reasons were not given for using non-parametric tests rather than paired samples t-test, assume this is because the results violated the assumptions of parametric tests. Rating: -
4.6 Was the precision of intervention effects given or calculable? Were they meaningful?	Comments: P-values were given. CIs not given.

Were confidence intervals or p values for effect estimates given or possible to calculate? Were CI's wide or were they sufficiently precise to aid decision-making? If precision is lacking, is this because the study is under-powered?	Rating: -
Section 5: is there a clear rationale for approach used?	No study aims or hypotheses stated, not clear of exact purpose of study Rating: -
Section 6: Summary	
6.1 Are the study results internally valid (i.e. unbiased)? How well did the study minimise sources of bias (i.e. adjusting for potential confounders)? Were there significant flaws in the study design? **6.2 Are the findings generalisable to the source population (i.e. externally valid)?** Are there sufficient details given about the study to determine if the findings are generalisable to the source population? Consider: participants, interventions and comparisons, outcomes, resource and policy implications.	Comments: Internal validity: main threats to internal validity – the routes may have been too easy (only 8 turns), routes around a familiar hospital setting. Small sample. No follow up. Rating: - External validity: convenience sample all from the same clinic. Korsakoffs may not generalise to other ABIs. But was a real-world route learning intervention although the real-world was a hospital setting so not community-based. Rating: -

Study: How best to orient travellers with acquired brain injury: A comparison of three directional prompts. Lemoncello, Sohlberg & Frickas. (2010a).	
Study Design: Mixed between and within subjects design	
Section 1: Population:	
1.7. Is the source population or source area well described? Was the country (e.g. developed or non-developed, type of healthcare system), setting (primary schools, community centres etc.), location (urban, rural), population demographics etc. adequately described?	Comments: Described the country, city, and setting. The population - people with ABI. Rating: +
1.8. Is the eligible population or area representative of the source population or area? Was the recruitment of individuals, clusters or areas well defined (e.g. advertisement, birth register)?	Comments: Recruitment through advertising. P's recruited from local supported living and support groups. Sample may not be representative of population with ABI – had psychiatric diagnoses including depression and schizophrenia.

Was the eligible population representative of the source? Were important groups under-represented?	Rating: -
1.3 Do the selected participants or areas represent the eligible population or area? Was the method of selection of participants from the eligible population well described? What % of selected individuals or clusters agreed to participate? Were there any sources of bias? Were the inclusion or exclusion criteria explicit and appropriate?	Comments: P's selected by responses to advertisement to local support groups and may not be representative (see above). 20 p's initially but excluded 2 due to physical mobility issues so sample was 18. Not clear whether more than 20 people were asked. Inclusion criteria including people with psychiatric diagnoses may not be appropriate – may confound their cog. Impairments. Rating: -
Section 2: Method of allocation to intervention (or comparison)	
2.1 Allocation to intervention (or comparison). How was selection bias minimised? Was allocation to exposure and comparison randomised? Was it truly random ++ or pseudo-randomised + (e.g. consecutive admissions)? If not randomised, was significant confounding likely (−) or not (+)? If a cross-over, was order of intervention randomised?	Comments: Rating: N/A Order of presentation of the 4 routes was counterbalanced (Latin Square). No randomisation of groups as the groups were ABI and controls.
2.2 Were interventions (and comparisons) well described and appropriate? Were interventions and comparisons described in sufficient detail (i.e. enough for study to be replicated)? Was comparisons appropriate (e.g. usual practice rather than no intervention)?	Comments: The procedure was well described to allow replication. Comparisons of the 4 routes (different direction types) was appropriate. Rating: ++
2.3 Was the allocation concealed? Could the person(s) determining allocation of participants or clusters to intervention or comparison groups have influenced the allocation? Adequate allocation concealment (++) would include centralised allocation or computerised allocation systems.	Comments: N/A
2.4 Were participants or investigators blind to exposure and comparison?	Comments: Rating: N/A

Were participants and investigators – those delivering or assessing the intervention kept blind to intervention allocation? (Triple or double blinding score ++) If lack of blinding is likely to cause important bias, score −.	
2.5 Was the exposure to the intervention and comparison adequate? Is reduced exposure to intervention or control related to the intervention (e.g. adverse effects leading to reduced compliance) or fidelity of implementation (e.g. reduced adherence to protocol)? Was lack of exposure sufficient to cause important bias?	Comments: Yes all p's completed all 4 routes. Rating: ++
2.6 Was contamination acceptably low? Did any in the comparison group receive the intervention or vice versa? If so, was it sufficient to cause important bias? If a cross-over trial, was there a sufficient wash-out period between interventions?	Comments: N/A
2.7 Were other interventions similar in both groups? Did either group receive additional interventions or have services provided in a different manner? Were the groups treated equally by researchers or other professionals? Was this sufficient to cause important bias?	Comments: Rating: N/A
2.8 Were all participants accounted for at study conclusion? Were those lost-to-follow-up (i.e. dropped or lost pre-, during or post-intervention) acceptably low (i.e. typically <20%)? Did the proportion dropped differ by group? For example, were drop-outs related to the adverse effects of the intervention?	Comments: All p's completed the study. Rating: ++
2.9 Did the setting reflect usual UK practice? Did the setting in which the intervention or comparison was delivered differ significantly from usual practice in the UK? For example, did participants receive intervention (or comparison) condition in a hospital rather than a community-based setting?	Comments: The community city setting with supported living and support groups reflects UK settings. Real street based route learning. Rating: +

2.10 Did the intervention or control comparison reflect usual UK practice? Did the intervention or comparison differ significantly from usual practice in the UK? For example, did participants receive intervention (or comparison) delivered by specialists rather than GPs? Were participants monitored more closely?	Comments: Patients given memory aids/retrieval strategies does not differ from usual UK practice. Researchers fulfilled similar roles to support workers. Rating: +
Section 3 Outcomes	
3.1 Were outcome measures reliable? Were outcome measures subjective or objective (e.g. biochemically validated nicotine levels ++ vs self-reported smoking −)? How reliable were outcome measures (e.g. inter- or intra-rater reliability scores)? Was there any indication that measures had been validated (e.g. validated against a gold standard measure or assessed for content validity)?	Comments: Outcome measures were mixture of objective and subjective ratings by 2 researchers scoring accuracy (objective), directness (subjective – hesitation vs direct). Second rater scored videos for 50% of p's – agreement of 90.6% Rating: +
3.2 Were all outcome measurements complete? Were all or most study participants who met the defined study outcome definitions likely to have been identified?	Comments: All outcome measures were complete. Rating: ++
3.3 Were all important outcomes assessed? Were all important benefits and harms assessed? Was it possible to determine the overall balance of benefits and harms of the intervention versus comparison?	Comments: Also measured confidence using the directions and preference. Did not measure delayed recall or if strategies were used in daily life. Rating: +
3.4 Were outcomes relevant? Where surrogate outcome measures were used, did they measure what they set out to measure? (e.g. a study to assess impact on physical activity assesses gym membership – a potentially objective outcome measure – but is it a reliable predictor of physical activity?)	Comments: Hesitancy a necessary measure? If hesitant but still get to the right end-goal, is it a problem? Rating: +
3.5 Were there similar follow-up times in exposure and comparison groups?	Comments: N/A

If groups are followed for different lengths of time, then more events are likely to occur in the group followed-up for longer distorting the comparison. Analyses can be adjusted to allow for differences in length of follow-up (e.g. using person-years).	
3.6 Was follow-up time meaningful? Was follow-up long enough to assess long-term benefits or harms? Was it too long, e.g. participants lost to follow-up?	Comments: No follow up Rating: N/A
Section 4: Analyses	
4.1 Were exposure and comparison groups similar at baseline? If not, were these adjusted? Were there any differences between groups in important confounders at baseline? If so, were these adjusted for in the analyses (e.g. multivariate analyses or stratification). Were there likely to be any residual differences of relevance?	Comments: Groups were different but this was one of the independent measures i.e. brain injury vs control group Rating: N/A
4.2 Was intention to treat (ITT) analysis conducted? Were all participants (including those that dropped out or did not fully complete the intervention course) analysed in the groups (i.e. intervention or comparison) to which they were originally allocated?	Comments: NR
4.3 Was the study sufficiently powered to detect an intervention effect (if one exists)? A power of 0.8 (that is, it is likely to see an effect of a given size if one exists, 80% of the time) is the conventionally accepted standard. Is a power calculation presented? If not, what is the expected effect size? Is the sample size adequate?	Comments: No power analysis reported. Sample size is adequate but could be stronger (18 in each group). Rating: NR
4.4 Were the estimates of effect size given or calculable? Were effect estimates (e.g. relative risks, absolute risks) given or possible to calculate?	Comments: NR
4.5 Were the analytical methods appropriate?	Comments:

Were important differences in follow-up time and likely confounders adjusted for? If a cluster design, were analyses of sample size (and power), and effect size performed on clusters (and not individuals)? Were subgroup analyses pre-specified?	Mixed models analysis appropriate for the design. Rating: +
4.6 Was the precision of intervention effects given or calculable? Were they meaningful? Were confidence intervals or p values for effect estimates given or possible to calculate? Were CI's wide or were they sufficiently precise to aid decision-making? If precision is lacking, is this because the study is under-powered?	Comments: P-values were given. CIs were not given. Rating: -
Section 5: is there a clear rationale for approach used?	Clear purpose, rationale, research questions and hypothesis stated Rating: ++
Section 5: Summary	
5.1 Are the study results internally valid (i.e. unbiased)? How well did the study minimise sources of bias (i.e. adjusting for potential confounders)? Were there significant flaws in the study design? **5.2 Are the findings generalisable to the source population (i.e. externally valid)?** Are there sufficient details given about the study to determine if the findings are generalisable to the source population? Consider: participants, interventions and comparisons, outcomes, resource and policy implications.	Comments: Internal validity: main limitation is that each route consisted of only 3 turns – most routes in real life are longer than this, does not place that great a demand on memory. Rating: - External validity: The ABI group included psychiatric diagnoses. Only 3 turns not generalizable to normal route finding? Rating: -

Study: A comparison of four prompt modes for route finding for community travellers with severe cognitive impairments. Sohlberg, Fickas, Hung & Fortier. (2007).	
Study Design: Repeated measures, within-subjects design	
Section 1: Population:	
1.9. Is the source population or source area well described?	Comments:

Was the country (e.g. developed or non-developed, type of healthcare system), setting (primary schools, community centres etc.), location (urban, rural), population demographics etc. adequately described?	The country, setting and location was well described. The population was people living in small towns with ABI and cognitive impairments. Rating: +
1.10. Is the eligible population or area representative of the source population or area? Was the recruitment of individuals, clusters or areas well defined (e.g. advertisement, birth register)? Was the eligible population representative of the source? Were important groups under-represented?	Comments: Recruitment was well defined – convenience sample local assistive living facilities asking service providers to identify and refer interested residents with severe cog. Impairments. Rating: +
1.3 Do the selected participants or areas represent the eligible population or area? Was the method of selection of participants from the eligible population well described? What % of selected individuals or clusters agreed to participate? Were there any sources of bias? Were the inclusion or exclusion criteria explicit and appropriate?	Comments: Method of selection described. 22 in total referred, 20 met criteria. Inclusion and exclusion criteria seem appropriate and explicit. Rating: ++
Section 2: Method of allocation to intervention (or comparison)	
2.1 Allocation to intervention (or comparison). How was selection bias minimised? Was allocation to exposure and comparison randomised? Was it truly random ++ or pseudo-randomised + (e.g. consecutive admissions)? If not randomised, was significant confounding likely (−) or not (+)? If a cross-over, was order of intervention randomised?	Comments: N/A.
2.2 Were interventions (and comparisons) well described and appropriate? Were interventions and comparisons described in sufficient detail (i.e. enough for study to be replicated)? Was comparisons appropriate (e.g. usual practice rather than no intervention)?	Comments: Procedure was described in sufficient detail to be replicated. Comparison of the routes and 4 prompt modes appropriate. Rating: ++
2.3 Was the allocation concealed? Could the person(s) determining allocation of participants or clusters to intervention or comparison groups have influenced the allocation?	Comments: N/A

Adequate allocation concealment (++) would include centralised allocation or computerised allocation systems.	
2.4 Were participants or investigators blind to exposure and comparison? Were participants and investigators – those delivering or assessing the intervention kept blind to intervention allocation? (Triple or double blinding score ++) If lack of blinding is likely to cause important bias, score −.	Comments: Rating: N/A
2.5 Was the exposure to the intervention and comparison adequate? Is reduced exposure to intervention or control related to the intervention (e.g. adverse effects leading to reduced compliance) or fidelity of implementation (e.g. reduced adherence to protocol)? Was lack of exposure sufficient to cause important bias?	Comments: All p's completed the routes. Rating: ++
2.6 Was contamination acceptably low? Did any in the comparison group receive the intervention or vice versa? If so, was it sufficient to cause important bias? If a cross-over trial, was there a sufficient wash-out period between interventions?	Comments: N/A
2.7 Were other interventions similar in both groups? Did either group receive additional interventions or have services provided in a different manner? Were the groups treated equally by researchers or other professionals? Was this sufficient to cause important bias?	Comments: N/A – no other interventions
2.8 Were all participants accounted for at study conclusion? Were those lost-to-follow-up (i.e. dropped or lost pre-, during or post-intervention) acceptably low (i.e. typically <20%)? Did the proportion dropped differ by group? For example, were drop-outs related to the adverse effects of the intervention?	Comments: All p's completed study. Rating: ++
2.9 Did the setting reflect usual UK practice?	Comments:

Did the setting in which the intervention or comparison was delivered differ significantly from usual practice in the UK? For example, did participants receive intervention (or comparison) condition in a hospital rather than a community-based setting?	The community assisted living setting reflects settings in the UK where people with ABI may be living. Rating: +
2.10 Did the intervention or control comparison reflect usual UK practice? Did the intervention or comparison differ significantly from usual practice in the UK? For example, did participants receive intervention (or comparison) delivered by specialists rather than GPs? Were participants monitored more closely?	Comments: Intervention of providing memory devices/aids is usual UK practice to help people with ABI. Real street-based route learning. Researchers providing a similar role to support workers. Rating: ++
Section 3 Outcomes	
3.1 Were outcome measures reliable? Were outcome measures subjective or objective (e.g. biochemically validated nicotine levels ++ vs self-reported smoking −)? How reliable were outcome measures (e.g. inter- or intra-rater reliability scores)? Was there any indication that measures had been validated (e.g. validated against a gold standard measure or assessed for content validity)?	Comments: Accuracy score out of 5 for each turn, 5 for perfect turning made, points deducted for missing turning, asking a question and hesitancy. Independent rater looked at 45% of videos and agreement was 89.9% Rating: +
3.2 Were all outcome measurements complete? Were all or most study participants who met the defined study outcome definitions likely to have been identified?	Comments: Rating: ++
3.3 Were all important outcomes assessed? Were all important benefits and harms assessed? Was it possible to determine the overall balance of benefits and harms of the intervention versus comparison?	Comments: Confidence and preference also assessed. No delayed recall outcome. Rating: +
3.4 Were outcomes relevant? Where surrogate outcome measures were used, did they measure what they set out to measure? (e.g. a study to assess impact on physical activity assesses gym membership – a potentially objective outcome measure – but is it a reliable predictor of physical activity?)	Comments: Is hesitancy a useful outcome? Rating: +

3.5 Were there similar follow-up times in exposure and comparison groups? If groups are followed for different lengths of time, then more events are likely to occur in the group followed-up for longer distorting the comparison. Analyses can be adjusted to allow for differences in length of follow-up (e.g. using person-years).	Comments: No follow up N/A
3.6 Was follow-up time meaningful? Was follow-up long enough to assess long-term benefits or harms? Was it too long, e.g. participants lost to follow-up?	Comments: N/A
Section 4: Analyses	
4.1 Were exposure and comparison groups similar at baseline? If not, were these adjusted? Were there any differences between groups in important confounders at baseline? If so, were these adjusted for in the analyses (e.g. multivariate analyses or stratification). Were there likely to be any residual differences of relevance?	Comments: Within-subjects design N/A
4.2 Was intention to treat (ITT) analysis conducted? Were all participants (including those that dropped out or did not fully complete the intervention course) analysed in the groups (i.e. intervention or comparison) to which they were originally allocated?	Comments: NR
4.3 Was the study sufficiently powered to detect an intervention effect (if one exists)? A power of 0.8 (that is, it is likely to see an effect of a given size if one exists, 80% of the time) is the conventionally accepted standard. Is a power calculation presented? If not, what is the expected effect size? Is the sample size adequate?	Comments: 20 p's. No power calculation. Rating: NR
4.4 Were the estimates of effect size given or calculable?	Comments:

Were effect estimates (e.g. relative risks, absolute risks) given or possible to calculate?	NR
4.5 Were the analytical methods appropriate? Were important differences in follow-up time and likely confounders adjusted for? If a cluster design, were analyses of sample size (and power), and effect size performed on clusters (and not individuals)? Were subgroup analyses pre-specified?	Comments: Repeated-measures ANOVA Rating: +
4.6 Was the precision of intervention effects given or calculable? Were they meaningful? Were confidence intervals or p values for effect estimates given or possible to calculate? Were CI's wide or were they sufficiently precise to aid decision-making? If precision is lacking, is this because the study is under-powered?	Comments: P-values given but not CIs. Rating: -
Section 5: Is there a clear rationale for approach used?	Clear rationale, research questions and hypotheses stated. Rating: ++
Section 6: Summary	
6.1 Are the study results internally valid (i.e. unbiased)? How well did the study minimise sources of bias (i.e. adjusting for potential confounders)? Were there significant flaws in the study design? **6.2 Are the findings generalisable to the source population (i.e. externally valid)?** Are there sufficient details given about the study to determine if the findings are generalisable to the source population? Consider: participants, interventions and comparisons, outcomes, resource and policy implications.	Comments: Internal validity: Initial orientation (which way to face) was given to p's, wouldn't happen normally in navigation. Small sample. Rating: + External validity: Only p's with severe cog. Impairments were included. Real- world navigation but only 7 turnings. Rating: +

Study: Lemoncello, Sohlberg & Frickas. (2010b). When directions fail: investigation of getting lost behaviour in adults with acquired brain injury.
Study Design: Matched control group comparison design
Section 1: Population:

1.11. Is the source population or source area well described? Was the country (e.g. developed or non-developed, type of healthcare system), setting (primary schools, community centres etc.), location (urban, rural), population demographics etc. adequately described?	Comments: Described the country, city, and setting. The population - people with ABI. Rating: +
1.12. Is the eligible population or area representative of the source population or area? Was the recruitment of individuals, clusters or areas well defined (e.g. advertisement, birth register)? Was the eligible population representative of the source? Were important groups under-represented?	Comments: Recruitment through advertising. P's recruited from local supported living and support groups. Sample may not be representative of population with ABI – had psychiatric diagnoses including depression and schizophrenia. Rating: -
1.3 Do the selected participants or areas represent the eligible population or area? Was the method of selection of participants from the eligible population well described? What % of selected individuals or clusters agreed to participate? Were there any sources of bias? Were the inclusion or exclusion criteria explicit and appropriate?	Comments: P's selected by responses to advertisement to local support groups and may not be representative (see above). 20 p's initially but excluded 2 due to physical mobility issues so sample was 18. Not clear whether more than 20 people were asked. Inclusion criteria including people with psychiatric diagnoses may not be appropriate – may confound their cog. Impairments. Rating: -
Section 2: Method of allocation to intervention (or comparison)	
2.1 Allocation to intervention (or comparison). How was selection bias minimised? Was allocation to exposure and comparison randomised? Was it truly random ++ or pseudo-randomised + (e.g. consecutive admissions)? If not randomised, was significant confounding likely (−) or not (+)? If a cross-over, was order of intervention randomised?	Comments: Rating: N/A No randomisation of groups as the groups were ABI and controls.
2.2 Were interventions (and comparisons) well described and appropriate? Were interventions and comparisons described in sufficient detail (i.e. enough for study to be replicated)? Was comparisons appropriate (e.g. usual practice rather than no intervention)?	Comments: The procedure was well described to allow replication. Rating: ++
2.3 Was the allocation concealed?	Comments: N/A

Could the person(s) determining allocation of participants or clusters to intervention or comparison groups have influenced the allocation? Adequate allocation concealment (++) would include centralised allocation or computerised allocation systems.	
2.4 Were participants or investigators blind to exposure and comparison? Were participants and investigators – those delivering or assessing the intervention kept blind to intervention allocation? (Triple or double blinding score ++) If lack of blinding is likely to cause important bias, score −.	Comments: Rating: N/A
2.5 Was the exposure to the intervention and comparison adequate? Is reduced exposure to intervention or control related to the intervention (e.g. adverse effects leading to reduced compliance) or fidelity of implementation (e.g. reduced adherence to protocol)? Was lack of exposure sufficient to cause important bias?	Comments: N/A
2.6 Was contamination acceptably low? Did any in the comparison group receive the intervention or vice versa? If so, was it sufficient to cause important bias? If a cross-over trial, was there a sufficient wash-out period between interventions?	Comments: N/A
2.7 Were other interventions similar in both groups? Did either group receive additional interventions or have services provided in a different manner? Were the groups treated equally by researchers or other professionals? Was this sufficient to cause important bias?	Comments: Rating: N/A
2.8 Were all participants accounted for at study conclusion? Were those lost-to-follow-up (i.e. dropped or lost pre-, during or post-intervention) acceptably low (i.e. typically <20%)? Did the proportion dropped differ by group? For example, were drop-outs related to the adverse effects of the intervention?	Comments: All p's completed the study. Rating: ++

2.9 Did the setting reflect usual UK practice? Did the setting in which the intervention or comparison was delivered differ significantly from usual practice in the UK? For example, did participants receive intervention (or comparison) condition in a hospital rather than a community-based setting?	Comments: The community city setting with supported living and support groups reflects UK settings. Real street based route learning. Rating: +
2.10 Did the intervention or control comparison reflect usual UK practice? Did the intervention or comparison differ significantly from usual practice in the UK? For example, did participants receive intervention (or comparison) delivered by specialists rather than GPs? Were participants monitored more closely?	Comments: Patients given memory aids/retrieval strategies does not differ from usual UK practice. Researchers fulfilled similar roles to support workers. Rating: +
Section 3 Outcomes	
3.1 Were outcome measures reliable? Were outcome measures subjective or objective (e.g. biochemically validated nicotine levels ++ vs self-reported smoking −)? How reliable were outcome measures (e.g. inter- or intra-rater reliability scores)? Was there any indication that measures had been validated (e.g. validated against a gold standard measure or assessed for content validity)?	Comments: Outcome measures were mixture of objective and subjective ratings by two researchers scoring accuracy, wayfinding strategy, directness. Second rater scored videos for 50% of p's – agreement of 88% Rating: +
3.2 Were all outcome measurements complete? Were all or most study participants who met the defined study outcome definitions likely to have been identified?	Comments: All outcome measures were complete. Rating: ++
3.3 Were all important outcomes assessed? Were all important benefits and harms assessed? Was it possible to determine the overall balance of benefits and harms of the intervention versus comparison?	Comments: Did not measure delayed recall or if strategies were used in daily life but otherwise all important outcomes were assessed. Rating: +
3.4 Were outcomes relevant? Where surrogate outcome measures were used, did they measure what they set out to measure? (e.g. a study to assess impact on physical activity	Comments: Hesitancy a necessary measure? If hesitant but still get to the right end-goal, is it a problem?

assesses gym membership – a potentially objective outcome measure – but is it a reliable predictor of physical activity?)	Rating: +
3.5 Were there similar follow-up times in exposure and comparison groups? If groups are followed for different lengths of time, then more events are likely to occur in the group followed-up for longer distorting the comparison. Analyses can be adjusted to allow for differences in length of follow-up (e.g. using person-years).	Comments: N/A
3.6 Was follow-up time meaningful? Was follow-up long enough to assess long-term benefits or harms? Was it too long, e.g. participants lost to follow-up?	Comments: No follow up Rating: N/A
Section 4: Analyses	
4.1 Were exposure and comparison groups similar at baseline? If not, were these adjusted? Were there any differences between groups in important confounders at baseline? If so, were these adjusted for in the analyses (e.g. multivariate analyses or stratification). Were there likely to be any residual differences of relevance?	Comments: Groups were different but this was one of the independent measures i.e. brain injury vs control group Rating: N/A
4.2 Was intention to treat (ITT) analysis conducted? Were all participants (including those that dropped out or did not fully complete the intervention course) analysed in the groups (i.e. intervention or comparison) to which they were originally allocated?	Comments: NR
4.3 Was the study sufficiently powered to detect an intervention effect (if one exists)? A power of 0.8 (that is, it is likely to see an effect of a given size if one exists, 80% of the time) is the conventionally accepted standard. Is a power calculation presented? If not, what is the expected effect size? Is the sample size adequate?	Comments: No power analysis reported. Sample size is adequate but could be stronger (18 in each group). Rating: NR

4.4 Were the estimates of effect size given or calculable? Were effect estimates (e.g. relative risks, absolute risks) given or possible to calculate?	Comments: NR
4.5 Were the analytical methods appropriate? Were important differences in follow-up time and likely confounders adjusted for? If a cluster design, were analyses of sample size (and power), and effect size performed on clusters (and not individuals)? Were subgroup analyses pre-specified?	Comments: Mixed models analysis appropriate for the design. Rating: +
4.6 Was the precision of intervention effects given or calculable? Were they meaningful? Were confidence intervals or p values for effect estimates given or possible to calculate? Were CI's wide or were they sufficiently precise to aid decision-making? If precision is lacking, is this because the study is under-powered?	Comments: P-values were given. CIs were not given. Rating: -
Section 5: is there a clear rationale for approach used?	Clear purpose, rationale, research questions and hypothesis stated Rating: ++
Section 5: Summary	
5.1 Are the study results internally valid (i.e. unbiased)? How well did the study minimise sources of bias (i.e. adjusting for potential confounders)? Were there significant flaws in the study design? **5.2 Are the findings generalisable to the source population (i.e. externally valid)?** Are there sufficient details given about the study to determine if the findings are generalisable to the source population? Consider: participants, interventions and comparisons, outcomes, resource and policy implications.	Comments: Internal validity: ABI group included psychiatric diagnoses. Phones were already connected simplifying the task. Rating: - External validity: The ABI group included psychiatric diagnoses. Relies on an external person to provide assistance. Rating: -

Appendix G: Analysis to check the assumptions of ANOVA

The proximal and distal route performance data were first checked for outliers and missing data. There were no missing data, however, graphical analysis of the data indicated that there were outliers in the data for both groups on the proximal route and for the control group on the distal route (see Figures 6 and 7).

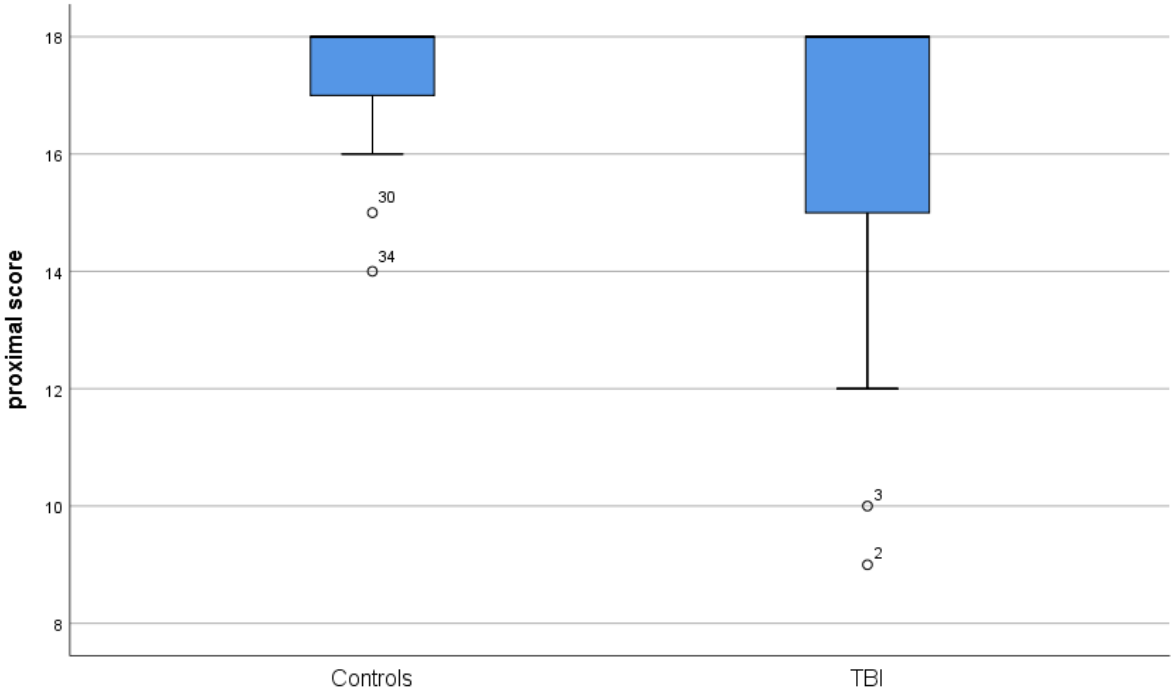


Figure 6. Boxplot displaying the range of proximal correct turning scores out of 18 for the TBI and Control groups

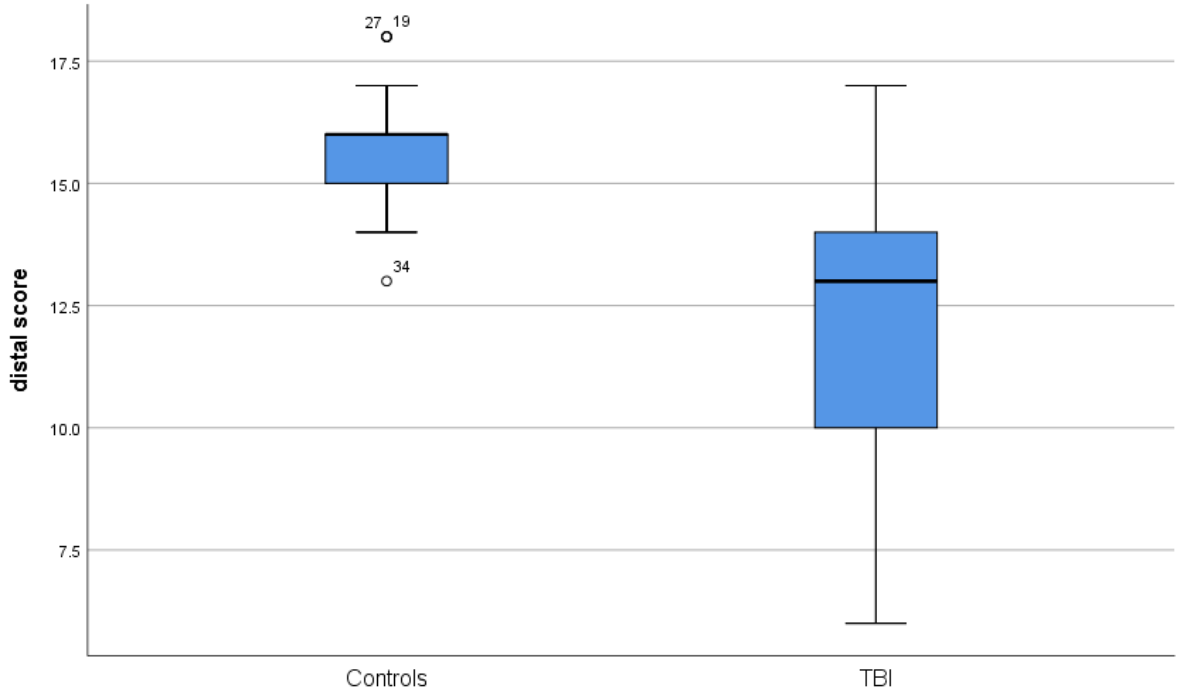


Figure 7. Boxplot displaying the range of distal correct turning scores out of 18 for the TBI and Control groups

Secondly, the assumption for parametric analysis that the data are normally distributed was tested using the Shapiro-Wilk test. The Shapiro-Wilk test is the recommended test for detecting deviations from normality in sample sizes of 10-50 (Stevens, 2002). The Shapiro-Wilk test showed significant deviations from normality for both groups in the proximal data ($W(17)=0.749$, $p<0.01$ for the TBI group and $W(17)=0.724$, $p<0.01$ for the control group). Specifically, the proximal data were significantly negatively skewed for both groups as can be seen from Figures 4 and 5 which show that the median total correct proximal score for the TBI group was 17.5 and 18 for the control group, where 18 is the maximum total correct score that can be achieved. However, data for the distal condition for both groups fell within the parameters of normal distribution, ($W(17)=0.947$, $p=0.414$ for the TBI group and $W(17)=0.914$, $p=0.117$ for the control group).

The second assumption of parametric analysis to be tested was equality of variance between the groups in performance on each route and this was tested through Levene's test. Levene's test showed significant heteroscedasticity (unequal variance) in both the proximal (Levene's $F(1,32)=9.887$, $p=0.004$) and the distal (Levene's $F(1,32)=10.574$, $p=0.003$) conditions. As the route performance data violated both the assumption of normal distribution and homogeneity of variance and there also appears to be outliers, it was decided that a transformation of the data would need to be applied to control for these violations.

The Box Cox test was used to establish whether a power transformation could control for the heteroscedasticity in the data in both route conditions and non-normality in the proximal data. This indicated that a power transformation of 3.29 would provide the best control of these issues. The Shapiro-Wilk and Levene's test were conducted on the transformed data and indicated that the distal transformed data were normally distributed ($W(17)=0.948$, $p=0.424$ for the TBI group and $W(17)=0.896$, $p=0.058$ for the control group) and homogenous (Levene's $F(1, 32)=0.817$, $p=0.373$). However, the proximal transformed data violated the assumptions of normality ($W(17)=0.776$, $p=0.001$ for the TBI group and $W(17)=0.739$, $p<0.001$ for the control group) and homogeneity of variances (Levene's $F(1, 32)=9.861$, $p=0.004$).

Research Question 2: Is there a difference between landmark conditions or groups on self-reported navigation strategies used on a VR route learning task?

The data were checked for missing data and outliers and the questionnaire data for two control participants were found to be missing. Furthermore, outliers were found in the datasets for strategy items 5 and 8 (see Figures 8-11).

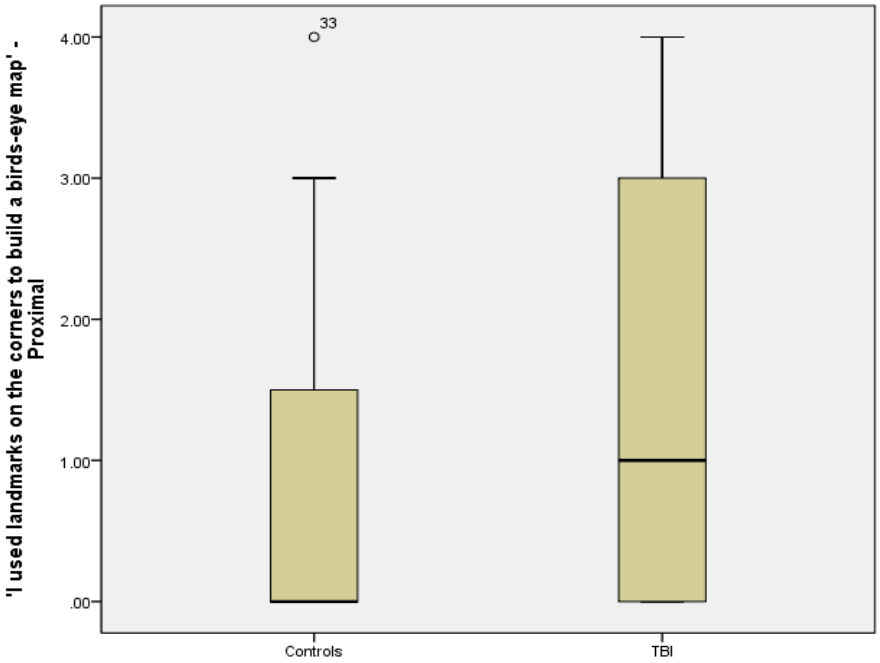


Figure 8. Boxplot displaying the range of participants' ratings for strategy item 5 'I used landmarks on the corners of the street to help me build up a 'birds-eye' map'

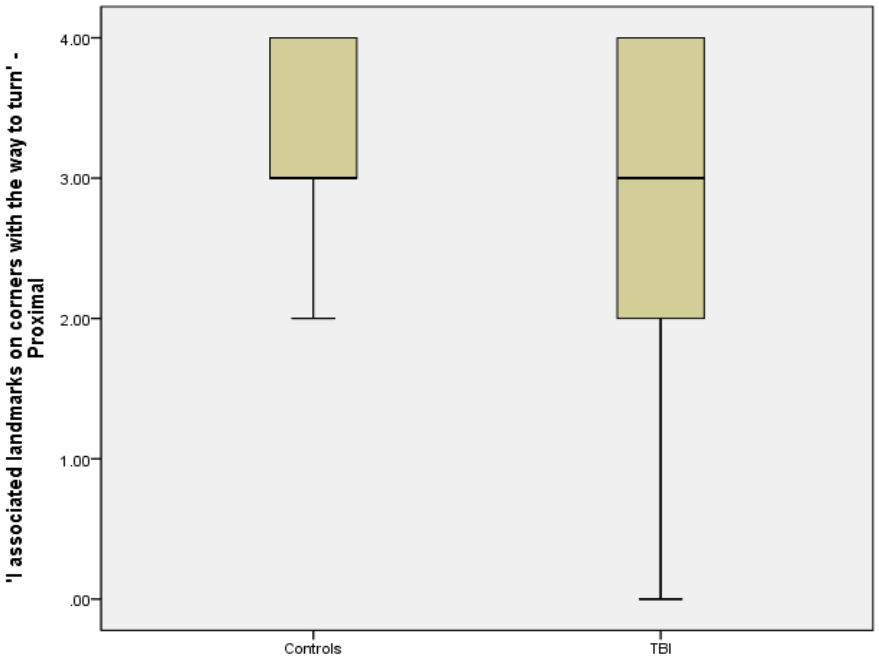


Figure 9. Boxplot displaying the range of participants' ratings for strategy item 6 'I associated a landmark on the corner of the street with which way to turn at each junction'

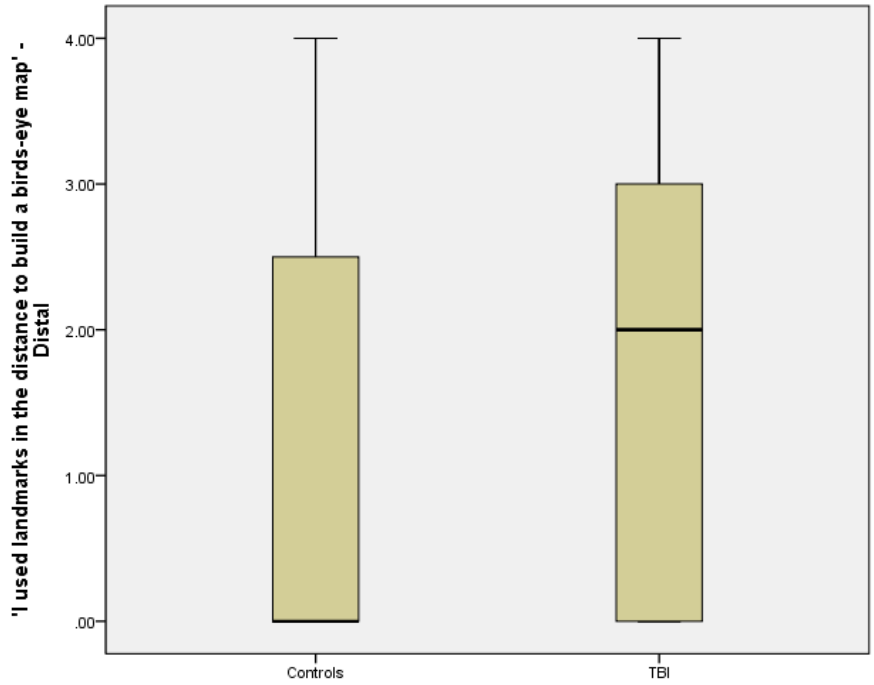


Figure 10. Boxplot displaying the range of participants' ratings for strategy item 7 'I used landmarks in the distance to help me build up a 'birds-eye' map'

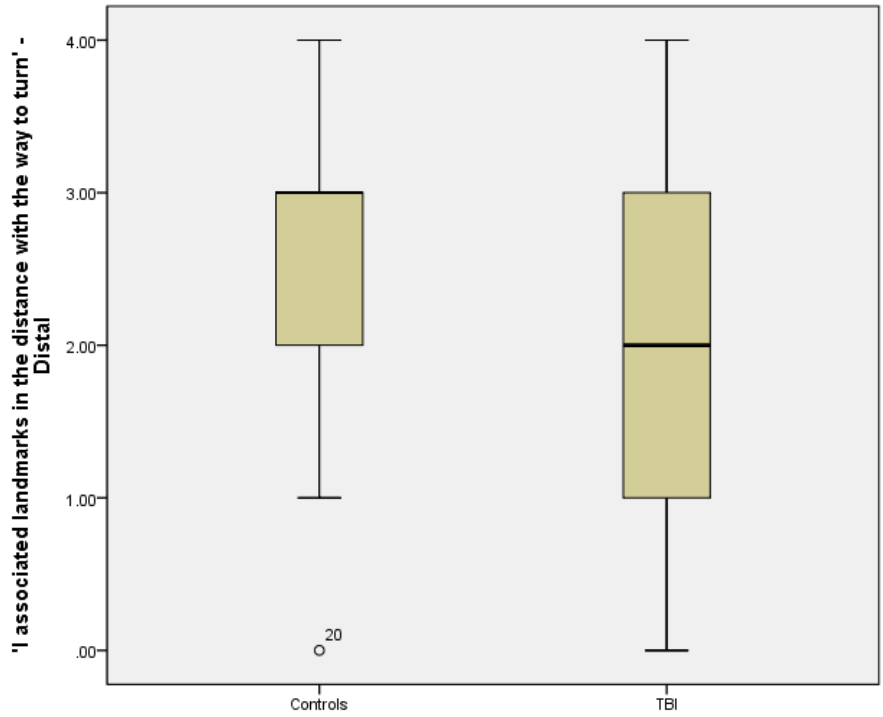


Figure 11. Boxplot displaying the range of participants' ratings for strategy item 8, 'I associated landmarks in the distance with which way to turn at each junction'

Furthermore, the Shapiro-Wilk test showed violations of normality for the egocentric and allocentric strategy items except for item 8 (see Table 5). However, the items were found to have equal variance as shown by Levene's test with one exception of item 6 (see Table 6). Due to the outliers in the data and the violations of the assumptions of parametric tests, the strategy data were transformed using a power transformation of 1.46 as indicated by the results of a Box Cox test. However, the transformation did not improve the normality of the data.

Table 5. Shapiro-Wilk test results for the egocentric and allocentric navigation strategy items

Navigation Strategy Item	Route*Group	Shapiro-Wilk's W	P-Value *significant <0.05
5 - I used landmarks on the corners of the street to help me build up a 'birds-eye' map	Proximal TBI	0.865	0.018*
	Proximal Controls	0.718	<0.001*
6 - I associated a landmark on the corner of the street with which way to turn at each junction	Proximal TBI	0.815	0.003*
	Proximal Controls	0.761	0.001*
7 - I used landmarks in the distance to help me build up a 'birds-eye' map	Distal TBI	0.843	0.009*
	Distal Controls	0.770	0.002*
8 - I associated landmarks in the distance with which way to turn at each junction	Distal TBI	0.917	0.129
	Distal Controls	0.917	0.175

Table 6. Levene's test of unequal variance results for each of the strategy items

Navigation Strategy Item	Route	Levene's F	P-Value *significant <0.05
5 - I used landmarks on the corners of the street to help me build up a 'birds-eye' map	Proximal	0.232	0.634
6 - I associated a landmark on the corner of the street with which way to turn at each junction	Proximal	5.482	0.027*
7 - I used landmarks in the distance to help me build up a 'birds-eye' map	Distal	0.029	0.866

8 - I associated landmarks in the distance with which way to turn at each junction	Distal	0.900	0.766

Appendix H: OCEBM Levels of Evidence Working Group. "The Oxford 2011 Levels of Evidence". Oxford Centre for Evidence-Based Medicine.

Question	Step 1 (Level 1*)	Step 2 (Level 2*)	Step 3 (Level 3*)	Step 4 (Level 4*)	Step 5 (Level 5)
How common is the problem?	Local and current random sample surveys (or censuses)	Systematic review of surveys that allow matching to local circumstances**	Local non-random sample**	Case-series**	n/a
Is this diagnostic or monitoring test accurate? (Diagnosis)	Systematic review of cross sectional studies with consistently applied reference standard and blinding	Individual cross sectional studies with consistently applied reference standard and blinding	Non-consecutive studies, or studies without consistently applied reference standards**	Case-control studies, or "poor or non-independent reference standard**	Mechanism-based reasoning
What will happen if we do not add a therapy? (Prognosis)	Systematic review of inception cohort studies	Inception cohort studies	Cohort study or control arm of randomized trial*	Case-series or casecontrol studies, or poor quality prognostic cohort study**	n/a
Does this intervention help? (Treatment Benefits)	Systematic review of randomized trials or *n*-of-1 trials	Randomized trial or observational study with dramatic effect	Non-randomized controlled cohort/follow-up study**	Case-series, case-control studies, or historically controlled studies**	Mechanism-based reasoning
What are the COMMON harms? (Treatment Harms)	Systematic review of randomized trials, systematic review of nested case-control studies, *n*of-1 trial with the patient you are raising the question about, or observational study with dramatic effect	Individual randomized trial or (exceptionally) observational study with dramatic effect	Non-randomized controlled cohort/follow-up study (post-marketing surveillance) provided there are sufficient numbers to rule out a common harm. (For long-term harms the duration of follow-up must be sufficient.)**	Case-series, case-control, or historically controlled studies**	Mechanism-based reasoning
What are the RARE harms? (Treatment Harms)	Systematic review of randomized trials or *n*-of-1 trial	Randomized trial or (exceptionally) observational study with dramatic effect			
Is this (early detection) test worthwhile? (Screening)	Systematic review of randomized trials	Randomized trial	Non -randomized controlled cohort/follow-up study**	Case-series, case-control, or historically controlled studies**	Mechanism-based reasoning

* Level may be graded down on the basis of study quality, imprecision, indirectness (study PICO does not match questions PICO), because of inconsistency between studies, or because the absolute effect size is very small; Level may be graded up if there is a large or very large effect size. ** As always, a systematic review is generally better than an individual study.

There are two different ways to interpret Level 1 evidence for treatment benefits as it is currently stated. The intended interpretation is: “either N-of-1 randomized trials or systematic reviews of randomized trials”. The wrong interpretation is: “either systematic reviews of randomized trials or systematic reviews of n-of-1 trials”.

Appendix I: Glossary of Terms

Term	Definition
Acquired Brain Injury	Damage to the brain that occurred after birth, including traumatic brain injury such as through a fall or road traffic accident as well as acquired damage resulting from a brain tumour, stroke, substance misuse, brain infection or deprivation of oxygen (Teasell et al., 2007)
Topographical Disorientation	A severe impairment in navigating in an environment due to the effects of acquired brain injury (Aguirre & D'Esposito, 1999)
Navigation	The ability to find your way from one spatial location to another (Wolbers & Hegarty, 2010)
Wayfinding	Knowing ones position in space and using this information to navigate in familiar and unfamiliar locations independent of any specific route (Livingstone & Skelton, 2007)
Route Learning	Learning a specific path that joins two separate spatial locations (Lloyd et al. 2009b)
Virtual Reality	An advanced form of computer software that allows the user to interact with a computer-generated version of a real environment (Brooks et al. 1999)
Compensatory Rehabilitation	Strategies used to rehabilitate an area of physical or cognitive impairment by modifying behaviour or the environment in order to compensate for the impairment (Kirsch et al. 2004)
Person-Oriented Strategies	Compensatory rehabilitation strategies which are initiated by the person without any modifications of the environment such as learning a mnemonic recall strategy or errorless learning a task sequence (Kirsch et al. 2004)
Environmentally-Oriented Strategies	Compensatory rehabilitation strategies where the environment is modified to reduce the cognitive demands of a task such as using an external aid as a navigation prompt (Kirsch et al. 2004)
Hybrid Strategies	Compensatory rehabilitation strategies which use a combined person-oriented and environmentally-oriented approach (Kirsch et al. 2004)
N-of-1 Research Studies	Any research study of a single participant (Tate et al. 2015)
Single-Case Experimental Designs	The experimental study of a single participant who serves as his or her own control (Tate et al. 2015)

Internal Validity	Whether the effects observed in a study are due to the independent variable and not another factor (McLeod, 2013)
External Validity	Whether the effects observed in a study can be generalised across settings, people and time (McLeod, 2013)
Errorless Learning	Acquiring a skill through repeated exposure to the correct information without making errors (Baddeley & Wilson, 1994)
Backwards Chaining	An errorless learning approach where an individual is taught a task sequence in steps beginning with the last step. The trainer demonstrates all steps and when the individual has mastered the last step of the sequence without prompting, the trainer prompts all steps minus the last two steps of the sequence and this is gradually increased until the individual has mastered the sequence (Evans et al. 2000)
Forwards Chaining	An errorless learning approach where an individual is taught a task sequence in steps beginning with the first step. The trainer demonstrates/prompts the first step and gets the individual to copy, moving onto the second step and so on until they have mastered the sequence (Evans et al. 2000)
Fading-of-cues	An errorless learning approach where an individual is taught a task sequence in steps where cues such as verbal prompts and physical pointing are gradually faded as steps are learned (Rivest et al. 2016)
Implicit Memory	A memory for an event, procedural sequence or experience that is produced indirectly, without an explicit recall request and without awareness that memory is involved (Graf & Schacter, 1985)
Explicit Memory	Conscious memory for general knowledge or information about personal experiences that an individual retrieves in response to a specific need or request to do so (Graf & Schacter, 1985)
Survey Knowledge	Knowledge of a spatial environment processed from a survey/aerial (bird's-eye view) perspective (Claessen et al. 2015)
Route Knowledge	Knowledge of a spatial environment processed from a route/street-level (point-of-view) perspective (Claessen et al. 2015)
Landmarks	Objects in a spatial environment with distinctive features such as shape or colour that have the potential to help people to orientate or find their way in the environment (Lynch, 1960)

Appendix J: Participation Information Leaflets

PARTICIPANT INFORMATION LEAFLET (for participants with TBI)

An investigation into route learning in a virtual environment

Introduction
This study is being carried out by Dr Theresa Powell (Programme Director, Doctorate in Clinical Psychology) with the help of a doctoral student and masters students from the School of Psychology, University of Birmingham. It has been funded by Birmingham Community Healthcare NHS Foundation Trust. We would like to invite you to take part in our study.

What is the study about?
The study is about how people learn a route after they have had a brain injury. It is common after a brain injury for people to have physical or cognitive difficulties (e.g. learning or memory problems). There are a number of different ways to help people learn a route during rehabilitation. We feel that the way in which you learn a route may make a difference and so we want to compare two different approaches.

What will I have to do?
You will be invited to attend two sessions lasting around one to one and a half hours each, at your rehabilitation centre/hospital or at the University of Birmingham.

First we will obtain basic details of your injury from your medical notes and ask you some basic general questions e.g. your education and employment

We will then carry out some tests relating to your memory and spatial learning.

Then you will be asked to carry out a route learning task. You will use a joystick to move through some streets that you will see displayed on a computer screen. You will have instructions to tell you which way to go. You will then be asked to move around the route without the instructions to see what you have learned.

The researcher will then ask you what strategies you used when you were trying to learn the route.

What are the risks?
Every effort has been made to minimise the risks involved in this study. There are no bright or flashing images in the study but if you have suffered any adverse effects when viewing a television screen, you may not wish to volunteer.

What are the benefits?
There may not be any benefits to you personally but there is a possibility that it *may* help decide the best way to help you learn as part of your rehabilitation. In the future we hope this research will help us decide the best way to help other people learn and remember routes themselves.

What happens to the information?
The University of Birmingham is the sponsor for this study based in the United Kingdom. We will be using information from you and your medical records if you have provided this consent in order to undertake this study and will act as the data controller for this study. This means that we are responsible for looking after your information and using it properly. The University of Birmingham will keep identifiable information about you for 10 years after the study has finished.

Your rights to access, change or move your information are limited, as we need to manage your information in specific ways in order for the research to be reliable and accurate. If you withdraw from the study, we will keep the information about you that we have already obtained. To safeguard your rights, we will use the minimum personally-identifiable information possible.

You can find out more about how we use your information at:
https://www.birmingham.ac.uk/schools/psychology/centres/cap/index.aspx.

Who else is taking part?
We hope to find 25 people who have had a traumatic brain injury, who will take part over a three year period.

What if I change my mind during the study?
You are under no obligation to take part. If you decide not to take part at any point during the study, this will not affect any aspect of your current treatment. You are free to withdraw from the study at any point until the study is published and your data will be destroyed.

What if something goes wrong?
It is very unlikely that something could go wrong. However, the researchers are indemnified by the University of Birmingham.

If you wish to speak to someone who is NOT involved in the research about any issues raised, you can contact your key worker or the Patient Advice and Liaison service,

What happens at the end of the study?
The data will be analysed and published as part of a thesis and also in an academic journal. If you would like, you will be given a copy of the results of your tests and you will be sent a summary of research findings.

What if I have more questions or do not understand something?
Please contact myself Dr Theresa Powell or Amy Garrood using the contact details below.

What happens next if I decide to take part?
Please contact us directly using the details below. We will then ask you to read and sign a consent form and we will answer any other questions you may have.

Contact details (removed from this appendix)

PARTICIPANT INFORMATION LEAFLET (for control participants)

An investigation into route learning in a virtual environment

Introduction
This study is being carried out by Dr Theresa Powell (Programme Director, Doctorate in Clinical Psychology) with the help of a doctoral student and masters students from the School of Psychology, University of Birmingham. It has been funded by Birmingham Community Healthcare NHS Foundation Trust. We would like to invite you to take part in our study.

What is the study about?
The study is about how people learn a route after they have had a brain injury. It is common after a brain injury for people to have physical or cognitive difficulties (e.g. learning or memory problems). There are a number of different ways to help people learn a route during rehabilitation. We feel that the way in which people learn a route after a brain injury may make a difference and so we want to compare two different approaches. We need to compare how people with a brain injury learn a route to how people with no history of brain injury learn a route, and so we are also asking for people who have not had a brain injury to take part in this study.

What will I have to do?
You will be invited to attend a session lasting around one and a half hours, at the University of Birmingham.

First we will ask you some basic general questions e.g. your education and employment.

Then you will be asked to carry out a route learning task. You will use a joystick to move through some streets that you will see displayed on a computer screen. You will have instructions to tell you which way to go. You will then be asked to move around the route without the instructions to see what you have learned. The researcher will then ask you what strategies you used when you were trying to learn the route.

What are the risks?
Every effort has been made to minimise the risks involved in this study. There are no bright or flashing images in the study but if you have suffered any adverse effects when viewing a television screen, you may not wish to volunteer.

What are the benefits?
There may not be any benefits to you personally. However, in the future we hope this research will help us decide the best way to help people with a brain injury learn and remember routes.

What happens to the information?
The University of Birmingham is the sponsor for this study based in the United Kingdom. We will be using information from you in order to undertake this study and will act as the data controller for this study. This means that we are

responsible for looking after your information and using it properly. The University of Birmingham will keep identifiable information about you for 10 years after the study has finished.

Your rights to access, change or move your information are limited, as we need to manage your information in specific ways in order for the research to be reliable and accurate. If you withdraw from the study, we will keep the information about you that we have already obtained. To safeguard your rights, we will use the minimum personally-identifiable information possible.

You can find out more about how we use your information at:
https://www.birmingham.ac.uk/schools/psychology/centres/cap/index.aspx.

Who else is taking part?
We hope to find 25 people who have not had a brain injury to take part as a control group as well as 25 people who have had an acquired brain injury.

What if I change my mind during the study?
You are under no obligation to take part. You are free to withdraw from the study at any point until the study is published.

What if something goes wrong?
It is very unlikely that something could go wrong. However, the researchers are indemnified by the University of Birmingham.

If you wish to speak to someone who is NOT involved in the research about any issues raised, you can contact

What happens at the end of the study?
The data will be analysed and published as part of a thesis and also in an academic journal. If you would like, you will be given a copy of the results of your tests and you will be sent a summary of research findings.

What if I have more questions or do not understand something?
Please contact myself Dr Theresa Powell or Amy Garrood using the contact details below.

What happens next if I decide to take part?
Please contact us directly using the details below if you would like to take part. We will then ask you to read and sign a consent form and we will answer any other questions you may have.

Contact details (removed from this appendix)

Appendix K: Consent Forms for Empirical Study

Consent Form for participants with TBI

THE UNIVERSITY
OF BIRMINGHAM
School of Psychology
Edgbaston
Birmingham
B15 2TT

CONSENT FORM

Title of Project: Virtual Reality Route Learning after Traumatic Brain Injury

	initial box
1. I confirm that I have read and understand the participant information sheet dated 08.7.18 for the above study. I have had the opportunity to consider the information, ask questions and have had these answered satisfactorily.	
2. I understand that my participation is voluntary and that I am free to withdraw at any time, without giving any reason, without my medical care or legal rights being affected.	
3. I understand that relevant sections of any of my medical notes and data collected during the study, may be looked at by individuals from the research team at the University of Birmingham, by regulatory authorities or from the NHS Trust, where it is relevant to my taking part in this research. I give permission for these individuals to have access to my records.	
4. I am over 16 years of age	
5. I agree to take part in the above study.	

Would you like to be invited to take part in future studies carried out in the School of Psychology, University of Birmingham and for your contact details to be kept for this purpose?

Yes / No (please delete)

Name of Participant (Print)	Date	Signature
_____	_____	_____

Name of Person taking consent	Date	Signature
_____	_____	_____

Consent Form for Control Participants

THE UNIVERSITY
OF BIRMINGHAM
School of Psychology
Edgbaston
Birmingham
B15 2TT

CONSENT FORM

Title of Project: Virtual Reality Route Learning after Traumatic Brain Injury

	e initial box
4. I confirm that I have read and understand the participant information sheet dated 08.7.18 for the above study. I have had the opportunity to consider the information, ask questions and have had these answered satisfactorily.	☐
5. I understand that my participation is voluntary and that I am free to withdraw at any time, without giving any reason, without my medical care or legal rights being affected.	☐
6. I understand that data collected from my participation during the study, may be looked at by individuals from the research team at the University of Birmingham, or by regulatory authorities. I give permission for these individuals to have access to my data.	☐
4. I am over 16 years of age	☐
6. I agree to take part in the above study.	☐

Would you like to be invited to take part in future studies carried out in the School of Psychology, University of Birmingham and for your contact details to be kept for this purpose?

Yes / No (please delete) ☐

Name of Participant (Print)	Date	Signature
_____	_____	_____
Name of Person taking consent	Date	Signature
_____	_____	_____