Social Communication Intervention via Telehealth Following Traumatic Brain Injury in Adults: A Systematic Review

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COMPULSORY DECLARATION

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

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Abstract

Introduction: Traumatic brain injury (TBI) is a leading cause of death and disability, affecting millions of individuals annually worldwide. Neuropsychological sequalae following TBI are multiple and heterogenous, with the *combination* of such deficits having a marked impact on individuals' ability to effectively communicate. Functional implications of impaired communicative competence are profound and long-lasting, therefore warranting intervention. Neurorehabilitation targeting social communication post-TBI has to date been dominated by social skills and communication partner training, with telehealth (TH) delivery thereof recently emerging as a promising avenue of intervention. *Method:* This systematic review of social communication intervention via TH following TBI, was structured according to PRISMA guidelines, involved searching six scientific databases and included four studies which were evaluated using Cicerone et al.'s (2009) and Tate et al.'s (2008) checklists for methodological quality. *Results:* In both a single-case experimental design and clinical trial of a TH intervention program, TBIconneCT, improvements in conversational skills and quality were recorded on independent observer and self-report measures. Results of participants receiving TH intervention appear comparable to that of in-person (IP) counterparts. Across studies the logistics of TH delivery did not seem to detract from the subjective experience of intervention with participants reporting high levels of acceptability, and feasibility. *Conclusion:* The current evidence base for TH options for social communication intervention post-TBI is scarce, yet findings of initial studies are promising. Given the potential of TH to expand the variety and reach of neurorehabilitation services, and the current global shift towards virtual functioning, further such research appears warranted.

Keywords: traumatic brain injury; cognitive communication; social communication; telehealth; neuropsychological rehabilitation.

List of Abbreviations

ABI	Acquired Brain Injury
ADL	Activity of Daily Living
ARM	Agnew Relationship Measure
CCRSA	Communication Confidence Rating Scale for Aphasia
CINAHL	Cumulative Index to Nursing and Allied Health Literature
CP	Communication Partner
CPT	Communication Partner Training
HIC	High-Income Country
ICC	Intraclass Correlation Coefficient
ICT	Information and Communication Technologies
IP	In-Person
ITT	Intention-to-Treat
LCQ	La Trobe Communication Questionnaire
LMIC	Low-to-Middle-Income Country
MPC	Measure of Participation in Communication
MSC	Measure of Support in Communication
PART-O	Participation Assessment with Recombined Tools (Objective)
PRISMA	Preferred Reporting Items for Systematic Reviews and Meta-Analysis
PROSPERO	Prospective Register of Systematic Reviews
QOLIBRI	Quality of Life After Brain Injury
RCT	Randomised Controlled Trial
SCED	Single Case Experimental Design
SST	Social Skills Training
TBI	Traumatic Brain Injury
TH	Telehealth

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Introduction

Traumatic brain injury (TBI) is a major cause of global mortality and morbidity, with close to 69 million people sustaining TBIs each year worldwide (Dewan et al., 2018). The neuropsychological sequalae following TBI typically impact multiple domains of cognition such as attention, memory, and executive functioning – and the impact of each can no doubt be profound. However, very often the *combination* of these deficits has a particularly detrimental effect on individuals' ability to effectively communicate. The functional consequences of such impaired communicative competence are far-reaching, and persist well into the chronic phase of recovery, thus strongly warranting rehabilitation. Avenues of intervention for social communication impairment following TBI have, to date, been dominated by social skills training (SST) and communication partner training (CPT). Notably, both formats lend themselves well to online delivery. This holds great potential for increasing the variety and reach of neurorehabilitation services, especially against the backdrop of the growing field of telehealth (TH) and global shift towards virtual functioning driven by the COVID-19 pandemic. Further exploration of the feasibility and efficacy of TH options for rehabilitation targeting social communication post-TBI thus appears both relevant and timely indeed.

Review of Literature

Definition, Classification, and Common Consequences of TBI

TBI is a type of acquired brain injury (ABI) in which the brain is damaged via external force, namely the acceleration, deceleration, and rotational forces that typically occur during such impact (Frost et al., 2013). TBIs vary in severity – being either mild, moderate, or severe. Most often, severity is measured according to degree and duration of loss of consciousness and post-traumatic amnesia (Hawryluk & Manley, 2015). Furthermore, a dose response relationship exists between TBI and prognosis – with greater severity being predictive of poorer outcome. The effects of TBI are far-reaching in that they often lead to impairments in several domains – including physical, psychological, cognitive, social, and emotional (Mar et al., 2011; Rabinowitz & Levin, 2014). While outcomes following severe TBI are characteristically heterogenous, impairments in communication, specifically, are notably common – with prevalence rates of over 75% (MacDonald, 2017). Further, communication deficits post-TBI are significant given that they typically translate into problems with *social* communication.

Social Communication Impairment Post-TBI

There is a large body of research confirming that social communication impairments are common outcomes following TBI (Finch et al., 2016; MacDonald, 2017; Wiltshire & Ehrlich, 2014). Such impairments are not only common, but chronic – tending to persist beyond the acute phase of TBI (Dahlberg et al., 2006; Rietdijk, Power, Attard, Heard, & Togher, 2020a). The severity and persistence of such sequelae provide strong rationale for developing effective rehabilitation intervention to target social communication deficits post-TBI.

Definition and Impact. Social communication is generally understood as the ability to facilitate productive, meaningful interactions with others (Steel & Togher, 2019). This type of conversing is distinct through its emphasis on the functional, pragmatic aspect of communication. Successful social communication depends both on linguistic competence as well as a complex range of cognitive abilities such as self-monitoring, knowledge of social conventions and boundaries, impulse control, and emotion regulation (Dahlberg et al., 2007).

As such, the social communication abilities of individuals with TBI are frequently disrupted due to the diverse cognitive and behavioural outcomes following injury (Struchen, 2014). For example, cognitive changes such as concrete thinking, perseveration, decreased generativity and planning, impulsivity, and poor self-regulation may translate into breakdowns in social communication characterised by abrupt subject changes, tangential speech, literal interpretation of statements, repetition, diminished initiation of social interaction, and inappropriate comments (Sim et al., 2013; Struchen, 2014).

Indeed, such sequelae are denoted ‘cognitive communication’ impairment. This term was developed to explicitly distinguish between communication impairment resulting from motor speech deficits and primary language disorders (such as aphasia) from the kind of impaired communicative competence that arises *secondarily* to underlying neurocognitive deficits frequently sustained following brain injury (Togher et al., 2014). This distinction is important since it recognises the unique mechanism of injury and subsequent impact on effective communicative and social competence that typifies TBI – and accordingly, has resulted in the increasing adoption of this term, ‘cognitive communication’, in the rehabilitation literature (MacDonald & Wiseman-Hakes, 2010).

Effective communication is central to social competence. As such, deficits in conversational skills often impact functioning across a number of social settings (Braden et al., 2010). Notably, impairments in social communication lead to decreased quality of conversation. Indeed, interactions with individuals with TBI have been rated as being less

interesting, frequently inappropriate, and more effortful (Coelho et al., 2002). The impact of such deficits can be profound. Such diminished social competence leads to unsuccessful social encounters, discouraging others from initiating and continuing social contact with individuals with TBI – and fostering further avoidance of social situations on the part of the injured individual (McDonald et al., 2008; Struchen, 2014). It is common for individuals, post-TBI, to report a diminished social network – owing to the loss of previous friendships and intimate relationships (Douglas, 2019; Struchen et al., 2011; Temkin et al., 2009). Such poor social re-integration can further contribute to feelings of loneliness and isolation and decreased satisfaction and quality of life (Gerber & Gargaro, 2015).

Furthermore, deficits in social communication also pose a major obstacle to returning to work (Rietdijk et al., 2013). Indeed, impaired interpersonal communication is cited as one of the primary reasons for workplace separation (Meulenbroek et al., 2016). Research points to conversational skills being predictive of vocational status post-injury, with higher (and therefore more positive) outcomes on communication measures being positively associated with stable employment post-TBI (Douglas et al., 2016; Meulenbroek & Turkstra, 2016).

In addition to the work setting, communication deficits also impact more proximal social settings, such as the home environment. Impoverished conversational skills demand greater effort from family members to initiate, guide and maintain conversations (Godfrey & Shum, 2000; Struchen, 2014). Effective communication is thus made effortful, straining interpersonal relationships, and adding to the perceived burden of care for family members and partners of individuals with TBI (Wedcliffe & Ross, 2001).

The centrality of communication to effective everyday functioning, means that communication impairment post-TBI often has profound, observable ramifications – and has thus garnered a growing research interest in the potential for rehabilitation following TBI. Remediation of such deficits, post-TBI, is made possible, to varying degrees, through neuropsychological rehabilitation intervention (Zimmermann et al., 2017).

Neuropsychological Rehabilitation

Impairments following TBI can markedly alter individuals' capacity for activities of daily living (ADLs) as well as quality of life – thereby highlighting the importance of intervention aimed at improving functional capacity.

Definition and Approaches

Neuropsychological rehabilitation involves collaboration between patients and health-care professionals to assist the patient in returning, as far as possible, to their premorbid levels of functioning (Wilson, 2008). Either a restorative or compensatory approach towards

rehabilitation is typically adopted. Restorative strategies aim to restore premorbid functioning by reconnecting neural pathways through cognitive retraining. In contrast, rather than remediate impairments, a compensatory approach attempts to facilitate functioning by means of alternative strategies that bypass an individual's deficits (Koehler et al., 2012). Given that severe TBI often results in irreversible damage, compensatory methods – rather than restorative – are preferential and more commonly applied in rehabilitation with such injuries (Barman et al., 2016). Additionally, there is strong support for the efficacy and generalisability of compensatory methods to ADLs in the rehabilitation literature (Cicerone et al., 2019; Nadar & McDowd, 2010).

Modes of Intervention for Social Communication Deficits

Given the importance of social competence in facilitating successful everyday functioning, rehabilitation interventions targeting social communication certainly appear justified. Despite the considerable amount of literature describing the negative impact of social communication disability following TBI, the evidence base for effective interventions, targeting such deficits in TBI populations, remains limited (Douglas, 2017; Harrison-Felix et al., 2018).

To date, two avenues of intervention are typically taken when addressing deficits in social communication – SST or CPT (Hansen et al., 2019; Togher et al., 2014). This trend is indeed reflected in my summary table of intervention studies targeting social communication deficits in individuals with TBI (see Appendix A). Traditional SST involves training the individual with TBI directly, whereas CPT aims to improve the conversational interaction by shifting the focus of intervention to their communication partners (CP) – typically a family member, friend, or spouse (Togher et al., 2004). Notably, all the studies included in my summary table (see Appendix A) utilised either SST or CPT, with the more recent studies adopting a combined approach and exploring new delivery formats for intervention, namely TH.

SST. Historically, social communication interventions post-TBI have predominantly been based on SST for the individual with TBI (MacDonald & Wiseman-Hakes, 2010; Togher et al., 2016). Such interventions seek to alter the communication style of individuals by promoting the adoption of prosocial behaviours (e.g., turn-taking) and extinguishing ineffective or deleterious communication habits (e.g., interrupting; Togher et al., 2013). Key features of SST include the identification and demonstration of goal behaviours – followed by repeated rehearsal of communication skills through role-play, observing others, and feedback from group discussion, video, or audio-recordings (in essence, a restorative

approach). Skills transference is targeted through homework assignments designed to promote the practice of trained skills in real life situations (Dahlberg et al., 2007).

CPT. Compared to SST, CPT offers a more *compensatory* approach to intervention. It has been noted that CPs – family, friends, and spouses – of individuals with TBI often alter their style of communicating in ways that prevent equal participation and exacerbate communication impairments (Rietdijk, Power, Attard, Heard, & Togher, 2020b). CPT, in recognising the contribution of CPs to conversational interactions, thus seeks to teach partners elaborative and supportive conversational strategies that minimize the impact of social communication deficits and improve both the quality and degree of their partner's participation in conversation. This approach thus offers a way of bypassing individuals' deficits, post-TBI, that limit their capacity to apply the social skills learned in training.

TH. Both SST and CPT while typically delivered in-person (IP) can also be delivered via TH – and options for doing so, while nascent, appear to hold promise for expanding rehabilitation opportunities for social communication intervention. TH is a recent development in neuropsychological rehabilitation and describes the delivery of rehabilitation to users in a remote location, via information and communication technologies (ICT; Theodoros & Russell, 2008). Because the kinds of activities typically comprising social communication rehabilitation are predominantly verbal and visual, such treatment lends itself well to delivery via TH (Brennan et al., 2009). Currently, there is a modest but growing research base documenting the efficacy of TH for individuals with TBI and their family members (Brunner et al., 2017; Ownsworth et al., 2018; Rietdijk et al., 2012). The majority of studies conducted thus far report positive outcomes (Coxe et al., 2020; Damianakis et al., 2016; Powell et al., 2016).

A notable benefit of TH includes its ability to extend access to rehabilitation services to individuals who are geographically remote, such as in rural areas (Brennan et al., 2009; Rietdijk et al., 2019). Expanding the reach of such services is particularly valuable in low- to middle-income country (LMIC) contexts, such as South Africa, in which little to no formal neuropsychological rehabilitation infrastructure currently exists (Schrieff-Elson & Thomas, 2017) and in which the opportunity to offer such services via TH could thus be opportune.

While the past decade has seen a rapid increase in internet access worldwide, there remains a significant proportion of people who are not using the internet – with recent statistics demonstrating that the majority of this population is from the developing world. For example, at the end of 2019, 87% of the total population in developed countries were using the internet, compared to 66% in developing countries and a mere 38% in least developed

countries (International Telecommunication Union, 2020). That said, within such LMICs, affordable technologies, internet, and mobile cellular subscriptions, are undoubtedly on the rise (International Telecommunication Union, 2020; Ngwa et al., 2020; Shuvo et al., 2015). Indeed, a previous survey found that within 15 of the 24 LMICs surveyed, 20% of people reported using the internet daily (Pew Research Center, 2014). Nonetheless, TH interventions are dependent on various technological factors that remain a challenge in many LMICs – for example, the cost and speed of data and network coverage, affordability of devices like computers, lack of technical literacy, and unreliable (or non-existent) electricity supply (Shuvo et al., 2015; Tiene, 2004; Wallis et al., 2017). As such, directly transferring TH interventions to lower-resourced contexts may prove problematic, and likely require further research into local contexts, as well as extensive planning and adaptations if it is to be successfully implemented in resource-constrained settings.

Currently in LMICs, the use of TH within healthcare is still in the early stages of development. Recent reviews indicate that the use of TH in LMICS is predominantly focused on distance education and aiding local clinicians in medical decision making through provision of specialist expertise, supervision, and training programs (Acharibasam & Wynn, 2018; Shuvo et al., 2015). Studies implementing actual intervention via TH are less prevalent and are largely pilot studies. Several examples of such rehabilitation initiatives are available in the field of stroke (Bettger et al., 2019) with programmes targeting injury adjustment and physical therapy having been successfully conducted via mobile phones in LMICs such as Ghana (Sarfo et al., 2018), Uganda (Kamwesiga et al., 2018) and India (Sureshkumar et al., 2016). The field is in its infancy yet conclusions regarding the efficacy and feasibility of TH for the provision of healthcare in LMICs while tentative at present, are promising.

With ongoing advancements in technology, great opportunity exists for further research to build the evidence base for the efficacy of TH for social communication intervention in TBI. Indeed, the past decade has already seen considerable growth in the options for rehabilitation via remote delivery and this is anticipated to continue increasing (Bier et al., 2018; Brennan et al., 2009).

Neuropsychological Rehabilitation in High-Income Countries Versus LMICs.

The disparity between high-income countries (HICs) and LMICs is evident not only in TH rehabilitation specifically, but also more broadly in the field of neuropsychological rehabilitation as a whole. Of note, out of the 16 studies reviewed in my summary table (see Appendix A), 15 arose from HICs. Undoubtedly, there exists a distinct gap in the area of research into neurorehabilitation in LMIC settings, like South Africa (Schrieff-Elson &

Thomas, 2017). Furthermore, this gap strongly warrants addressing given that whilst a prominent, global, health concern, the burden of TBI is disproportionally distributed between HICs and LMICs with incidence being prominently higher in the latter. The increased burden of TBI in some LMICs can be attributed, in part, to context-specific factors. For example, in South Africa, two major causes of TBI include elevated rates of both motor vehicle accidents and interpersonal violence – common mechanisms of TBI (Naidoo, 2013). That said, it is within such countries, that provision and access to neurorehabilitation remains most limited. This paradox highlights the need to develop neuropsychological rehabilitation methods that are both feasible, and effective, within such resource constrained settings. A constructive step towards such a goal, involves the careful evaluation of existing rehabilitation methods and guidelines before the design and implementation of intervention. Accordingly, clinicians can benefit from evidence-based practice and systematic reviews of the current literature.

Evidence-based Practice and Systematic Reviews in Neuropsychological Rehabilitation

Evidence-based practice has undoubtedly become a central feature of the healthcare environment. Essentially, this movement urges clinicians to critically assess the available treatment options and implement only those practices that have been proven efficacious according to objective outcome measures and rigorous research – thereby maximising both the cost-effectiveness and quality of patient care (Chelune, 2010; Rosswurm & Larrabee, 1999). To this end, systematic reviews play an instrumental role in the journey from theoretical research to applied practice (Lavis et al., 2005; Loring & Bowden, 2014). Indeed, in providing synthesis of the key findings of current research and intervention efforts, systematic reviews offer clinicians valuable insight into the amount and quality of supporting evidence for rehabilitation techniques. It is for this reason, that this study undertook a systematic review of the current evidence for TH intervention targeting social communication impairment following TBI. Furthermore, given the current COVID-19 pandemic and the associated need for and trend towards online intervention, comprehensive evaluation of the evidence to date is timely and relevant indeed.

Systematic Reviews on Communication Intervention via TH

To the best of my knowledge, there is one previous systematic review that has looked at the use of ICT in rehabilitation for people with cognitive communication impairment following TBI (Brunner et al., 2017). Notably, this earlier review adopted a broader scope than that of the current review, examining the use of technology in general (e.g., including assistive technology and social media). Within this broader aim, the authors investigated the use of ICT specifically and noted that of the studies included, none had offered cognitive

communication intervention for individuals with TBI but were either aimed at providing support to family members of individuals with TBI or at training cognitive skills in general. The authors did note however, that at the time, Rietdijk et al. (2015) were in the process of piloting a social communication skills training intervention delivered via TH to a TBI population and their CPs – which has since been published (Rietdijk et al., 2019). Since this previous review only included studies up until 2015, the current study adds value therefore, by providing a more up-to-date systematic review of such studies.

Additionally, the current systematic review is specific to adult samples of individuals with TBI while Brunner et al. (2017) included samples of both adolescents *and* adults. Similarly, I adopted a more specific focus on intervention delivered via ICT, in comparison to the previous review's broad definition of technology and varied types of interventions. This distinction is worthwhile to make since the use of ICT involves unique barriers and challenges such as technical audio-visual issues and their impact on the quality of the client-therapist relationship (Henry et al., 2017; Lin et al., 2018).

Lastly, Coleman et al. (2015) conducted a systematic review on the use of TH for the assessment and treatment of cognition and communication skills in people with ABI. Notably, this review did not delineate between cognitive communication impairment, and motor speech or primary language disorders – and included samples of mixed impairment. In contrast, the current review adds value to the current literature by considering cognitive communication impairment in isolation since this specific kind of impairment is not only unique to TBI, but distinct in its mechanism of injury and associated outcomes from motor speech and primary language deficits (Togher et al., 2014).

Rationale

The marked prevalence and profound impact of social and cognitive communication deficits on everyday functioning, following TBI, has spurred on the development of rehabilitation interventions targeting such impairment. In particular, TH stands out as an interesting and emerging field. Given its potential to expand the scope of neuropsychological rehabilitation and the ever-increasing use of technology, this field is predicted to grow. It is important that the growth of TH is informed by clinicians and not merely the result of increasing digitisation (Brennan et al., 2009). Furthermore, rehabilitation efforts are strengthened when founded on evidence-based research that has proven effective in practice. This systematic review of the research to date in this field, offers a thorough investigation of the methods and results of these pioneering studies, evaluating the efficacy of TH intervention for social communication post-TBI according to the strength of the current

available evidence. The findings of current research can help guide further research and TH intervention efforts targeting social communication post-TBI.

Aim and Objective

I conducted a systematic review of studies that have implemented social communication intervention via TH with adults with TBI. The objective was to summarise and assess the outcomes and any barriers encountered in such studies, in order to establish their efficacy as well as draw up recommendations to inform future rehabilitation efforts of this nature.

Method

Procedure

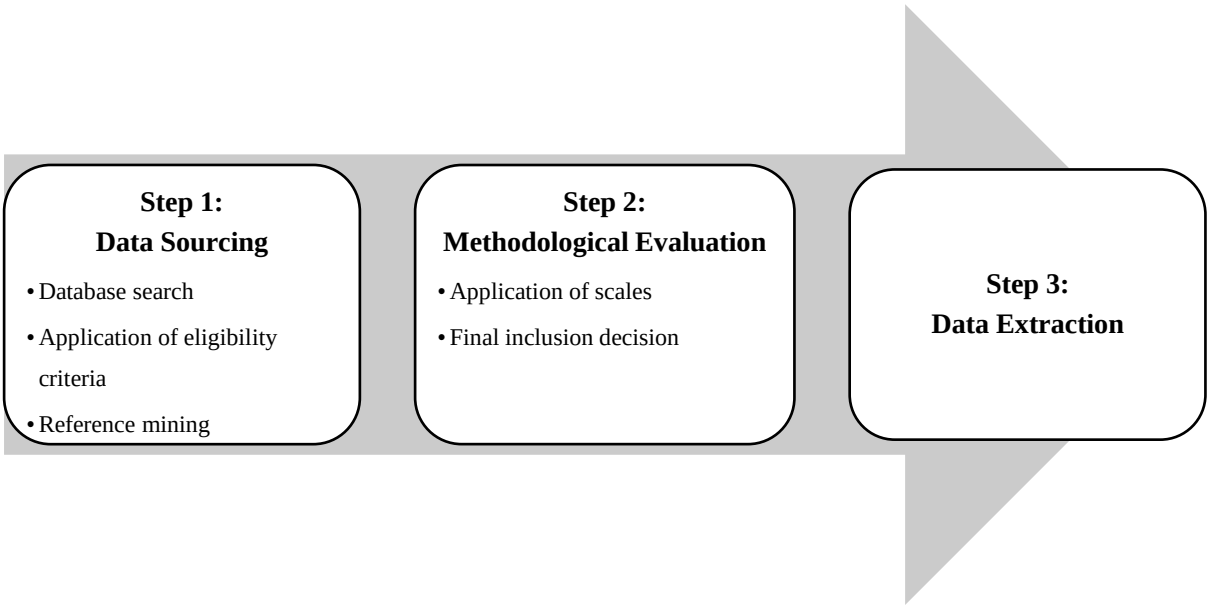
The protocol for this study drew on the recommended guidelines of the Preferred Reporting Items for Systematic Reviews and Meta-Analysis (PRISMA) statement and was structured in accordance with the PRISMA 2020 checklist (Page et al., 2021; see Appendix B). I registered this protocol with the International Prospective Register of Systematic Reviews (PROSPERO) on 14 January 2021 – registration ID: CRD42021223667 (available from: https://www.crd.york.ac.uk/prospero/display_record.php?ID=CRD42021223667). It must be noted that this protocol was automatically published without the PROPSERO team reviewing it for eligibility. This procedure was adopted to prevent backlog of submissions awaiting approval, due to the focus on approving studies related to COVID-19. This study was approved by an ethical approval committee in the Department of Psychology at the University of Cape Town – reference number: PSY2020-021 (see Appendix C).

Search Strategy

Figure 1 outlines the steps taken in this systematic review. I then elaborate on each of the steps listed in that figure, below.

Figure 1

Steps Taken in the Systematic Review Process



Step 1: Data Sourcing

Database Search. I (TCT), and a second reviewer (AS[1]), conducted an initial literature search by accessing the following online databases: PubMed, PsycINFO, Cumulative Index to Nursing and Allied Health Literature (CINAHL), Scopus, Web of Science, and Cochrane Library. For each key term comprising the research question, a free language search set was constructed by two reviewers (AS and TCT) based on this study's objective of assessing the efficacy of social communication intervention via TH with people with TBI. These search sets are detailed in Table 1. Boolean logic was then used to combine these search sets in order to construct the final search in each of the databases.

Table 1

Free Language Search Sets for Key Terms

Key Term	Phrase
Communication	"cognitive communication" OR "cognitive-communication" OR "social communication" OR communicat* OR "communication skills" OR conversat* OR discourse
Intervention	intervention OR interven* OR rehabilitation OR rehabilitate OR rehab OR rehab* OR program OR programme OR training OR train* OR remediation OR remediat* OR treatment OR treat* OR therapy
Telehealth	"information and communications tech*" OR "information and communication tech*" OR ICT OR "video conferenc*" OR "video-conferenc*" OR video OR online OR internet OR telehealth OR telemedicine OR telerehab* OR telecare OR telecommunicat* OR "tele-health" OR "tele-medicine" OR "tele-rehab*" OR "tele-care" OR "tele-communicat*"
TBI	"traumatic brain injury" OR "traumatic brain injuries" OR TBI

Note. TBI = traumatic brain injury.

* denotes any ending in word including, but not limited to, "y", "s", "ion", "ive", "al", "ing"

Additionally, several of the selected databases make use of a controlled vocabulary of terms that are assigned to their records as part of their indexing system, designed to make articles more discoverable. As such, where applicable, relevant index terms were also

[1] To reduce potential bias inherent in a single reviewer's perspective, Alexa Soule (AS), a MA student in Neuropsychology, also independently ran database searches, screened and selected articles for inclusion, and rated the included studies for methodological quality.

selected and included within the Boolean phrases. These terms are detailed in Table 2. For the final search strategies conducted in each of the databases see Appendix D.

When devising the key terms, I opted not to use the Population, Intervention, Comparison, Outcomes (PICO) or PICO Study Design (PICOS) tools as these organising frameworks are designed to optimise searches specifically for quantitative and qualitative studies respectively. I intentionally chose not to filter my search according to a particular kind of study design given that TH intervention for social communication following TBI is an emerging, and thus relatively modest, field of research. As such, I chose key terms that captured the essential elements of the research question without placing too narrow a focus on study design to avoid running the risk of missing relevant studies.

Table 2

Index Terms for Key Terms According to Database

Database	Index Terms			
	Communication	Intervention	Telehealth	TBI
PubMed	Social communication disorder OR Interpersonal relations	Rehabilitation OR Rehabilitation Research OR Treatment outcome	Telemedicine OR Telecommunication	Brain Injuries, traumatic
PsycINFO	Communication OR Communication Skills OR Reciprocity OR Social Communication OR Communication Skills Training	Rehabilitation OR Neuropsychological Rehabilitation OR Neurorehabilitation OR Occupational Therapy OR Psychosocial Rehabilitation	Telemedicine OR Online Therapy OR Teleconferencing OR Teleconsultation OR Telepsychiatry OR Telepsychology OR Telerehabilitation	Traumatic Brain Injury OR Brain Injuries OR Brain Concussion OR Head Injuries
CINAHL	Communication Skills OR Communication Skills Training OR Social Skills Training	Rehabilitation	Telerehabilitation OR Remote Consultation OR Videoconferencing OR Telehealth OR Telemedicine OR Teleconferencing	Brain Injuries
Cochrane Library	Social communication disorder OR Interpersonal relations	Rehabilitation OR Rehabilitation Research OR Treatment outcome	Telemedicine OR Videoconferencing	Brain Injuries

Note. Scopus and Web of Science do not make use of controlled vocabulary for their indexing systems and therefore are not included in this table.

CINAHL = Cumulative Index to Nursing and Allied Health Literature

AS and I conducted independent searches of each database on January 27, 2021. For each database, we retrieved the same number of records. All citations retrieved in this initial search of the outlined databases were exported to EndNote (Version 20; The EndNote Team, 2013) – a reference management computer software. Using the application's inbuilt tools, duplicate citations were detected and removed. Following this, the resulting list of citations

were uploaded to Rayyan (Ouzzani et al., 2016) – a web application designed for documenting references in systematic reviews. Once more, I checked for and removed duplicate citations.

Application of Eligibility Criteria. Studies were accepted for inclusion based on their fulfilment of the following criteria: Eligible articles needed to include the categories of the selected Boolean phrases (see Table 1) in their title and/or abstracts. There was no limit on the type of study considered, although all articles needed to be peer-reviewed as this ensured only methodologically and ethically sound studies were reviewed. Furthermore, only articles written or translated into English were considered since I am only fluent in English and cannot accurately interpret data presented in any other language.

Notably, only studies implementing intervention for cognitive-communication impairment – not motor speech or primary language disorders (such as aphasia) – were eligible, since the former is a distinct type of impairment, and as such, necessitates a different type of intervention to the latter (Togher et al., 2014). Additionally, studies had to implement rehabilitation via TH. This review, in accordance with current conceptualisations, specified TH as the delivery of rehabilitation through the use of ICT to clients in a remote location (Theodoros & Russell, 2008). As such, studies involving assistive technology were not considered since such intervention lies outside the particular focus and scope of this review. Interventions combining both IP and TH rehabilitation were considered if at least 50% of the total intervention sessions were conducted remotely.

Furthermore, eligibility also depended on characteristics of the samples utilised within studies. This review is specifically focused on individuals with TBI - injury of any severity was considered since there is literature supporting the presence of cognitive communication impairment in samples with mild (Blyth et al., 2012; LeBlanc et al., 2020) as well as moderate to severe TBI (Rietdijk et al., 2019). Notably, in studies utilising ABI samples, data relating to the TBI participants was extracted if reported in isolation.

Additionally, only adult samples were considered in this review, i.e., participants 18 years and older. There are marked differences in the anatomy and physiology of the developing versus mature brain which translate into significantly different outcomes and recovery following paediatric and adult TBI (Figaji, 2017). As such, only adult samples were included and in the cases of mixed samples, the adult data was extracted if reported separately. Finally, only research conducted on human participants was considered.

Article Selection According to Inclusion Criteria. The reference list uploaded to Rayyan was then evaluated by both reviewers (TCT and AS) according to the inclusion

criteria outlined above. Rayyan allows for the grouping of citations into those that are acceptable, and those that are unfit for inclusion, with each reviewer having the opportunity to record their reasoning for their decision. Initially each reviewer was blind to the other's decisions until both had completed reviewing all articles and sorted them into the following categories: 'include', 'exclude' and 'maybe'.

After consulting with one another about the articles upon which the reviewers disagreed, consensus was reached, and the resulting articles were selected for inclusion pending review of their methodological quality.

Reference Mining. To maximise the specificity of the literature search, the reference lists of the included articles were examined for any other possible relevant citations that met the outlined eligibility criteria. Similarly, the same process was applied to studies reported within other systematic reviews found in the literature search.

Step 2: Methodological Evaluation

The final step in selecting articles for inclusion involved evaluating their risk for potential bias. To this end, randomised controlled trials (RCTs) were reviewed using Cicerone et al.'s (2009) methodological quality checklist (see Appendix E), and single-subject designs were assessed against Tate et al.'s (2008) checklist for single case experimental designs (SCED; see Appendix F). Each of these scales is numeric, assigning a value of either zero or one for each criterion comprising the checklist. As such, higher total scores are reflective of stronger methodological quality. To allow for meaningful comparison between the scores of the different methodologic quality scales, a percentage rating was calculated for each article by dividing the article's credit score against the total score of the scale.

Each article was independently evaluated by the two reviewers using the checklist suited to its study design. Thereafter, to achieve 100% agreement, the reviewers discussed any discrepancies for every article until consensus was reached for each of the quality criteria.

RCTs. The checklist for RCTs (Cicerone et al., 2009) comprises eight criteria of internal validity, five descriptive criteria (relating to outcome measurement and sample size) and three statistical criteria, resulting in a total possible maximum score of 16. Depending on their total quality score, studies are rated as having high (≥ 12), moderate (6–11), or low (≤ 5) methodological quality (Cicerone et al., 2009).

Single-Subject Designs. The SCED checklist (Tate et al., 2008) was developed to address key problem areas of single subject designs; namely, operationalisation and sufficient

sampling of target behaviours, determining treatment efficacy, observer bias and generalisation. The scale comprises 11 criteria relating to these issues, with items 2–11 contributing to a total methodological quality score – the maximum possible score being 10 (Tate et al., 2008). Notably, item one (description of the clinical history) does not contribute to the quality score but was included for the sake of comparability with item one (description of eligibility criteria) of the PEDro scale (Maher et al., 2003) – a widely used, standardised rating scale for RCTs against which the SCED scale was modelled.

Step 3: Data Extraction

Relevant data was extracted from each article included in the finalised list of studies for review. This was achieved by means of an Excel spread sheet with specified headings, namely author/s, year of publication, journal of publication, study title, study design, sample size and characteristics, intervention implemented, outcome measures utilised, results reported, conclusions, limitations, and recommendations.

Results

The results for each of the steps taken in this review, namely data sourcing, methodological evaluation and data extraction are presented below. The outcome of the database search is reported first, followed by a detailed methodological evaluation of the included studies and lastly, I present a description of each of the studies' characteristics, outcome measures and associated findings.

Step 1: Data Sourcing

Database Search

Table 3 outlines the number of citations found in each database. For both reviewers, the initial search yielded a total of 834 articles. After removing duplicates (n = 331), a final total of 503 citations was obtained. The number of citations per database, detailed in Table 3, was identical for both reviewers.

Table 3

Number of Citations Found in Each Database

Database	Citations
CINAHL	97
Cochrane Library	100
PsycINFO	141
PubMed	190
Scopus	121
Web of Science	185
Total	503

Note. CINAHL = Cumulative Index to Nursing and Allied Health Literature

Application of Eligibility Criteria

The 503 citations were then screened according to the eligibility criteria outlined in the Method section. All the articles were first independently reviewed by myself and AS. Cohen's κ was run to determine if there was agreement between both raters on the articles to be included (N = 503). There was moderate agreement between the two raters, κ = .495 (95% CI, .147 to .842), p < .001. This is the proportion of agreement over and above chance agreement. Cohen's kappa (κ) can range from negative one to positive one. Based on the guidelines from Altman (1999), and adapted from Landis and Koch (1977), a kappa (κ) of .495 represents a moderate strength of agreement.

After completing independent evaluation, exclusion decisions were unblinded and the reviewers discussed all conflicting decisions such that 100% consensus was reached. A total of 481 articles were excluded on the basis of their title and/or abstract, according to the eligibility criteria outlined above. This left 22 articles that needed full-text review to decide on their inclusion, 18 of which were then excluded (reasons listed in figure 2 below). This

resulted in a total of four articles selected for inclusion in the review[2,3,4,5]. Notably, this is a marked drop-off from the original pool of 503 articles. The largest proportion of articles were excluded because, while their titles and/or abstracts included the relevant key search terms, the studies themselves did not conduct actual intervention rehabilitation. Another common reason for exclusion was studies utilising mixed samples of participants with TBI *and* ABI had insufficient data to extract results relating to solely TBI participants. Additionally, multiple studies were systematic reviews, focused on assessment – rather than treatment – of social communication deficits, or included technology (such as devices), but did not employ TH in the sense adopted for this review.

Reference Mining

The reference lists of the final four selected articles as well as systematic reviews obtained in the literature search were explored for any further potential citations. Notably, no other eligible studies were identified for inclusion. While four studies are markedly few, academic consensus stipulates that there is no minimum number of studies for conducting systematic reviews, as long as the search process is thorough, and well documented (Shokraneh, 2015). As such, the full text of the four selected articles were then subjected to methodological quality review. A summary of the steps taken to reach the final number of studies is provided below in Figure 2 – a PRISMA 2020 flow diagram – in accordance with the guidelines stipulated by the PRISMA 2020 Checklist and statement (Page et al., 2021).

[2] Rietdijk, R., Power, E., Brunner, M., & Togher, L. (2019). A single case experimental design study on improving social communication skills after traumatic brain injury using communication partner telehealth training. Brain Injury, 33(1), 94–104.

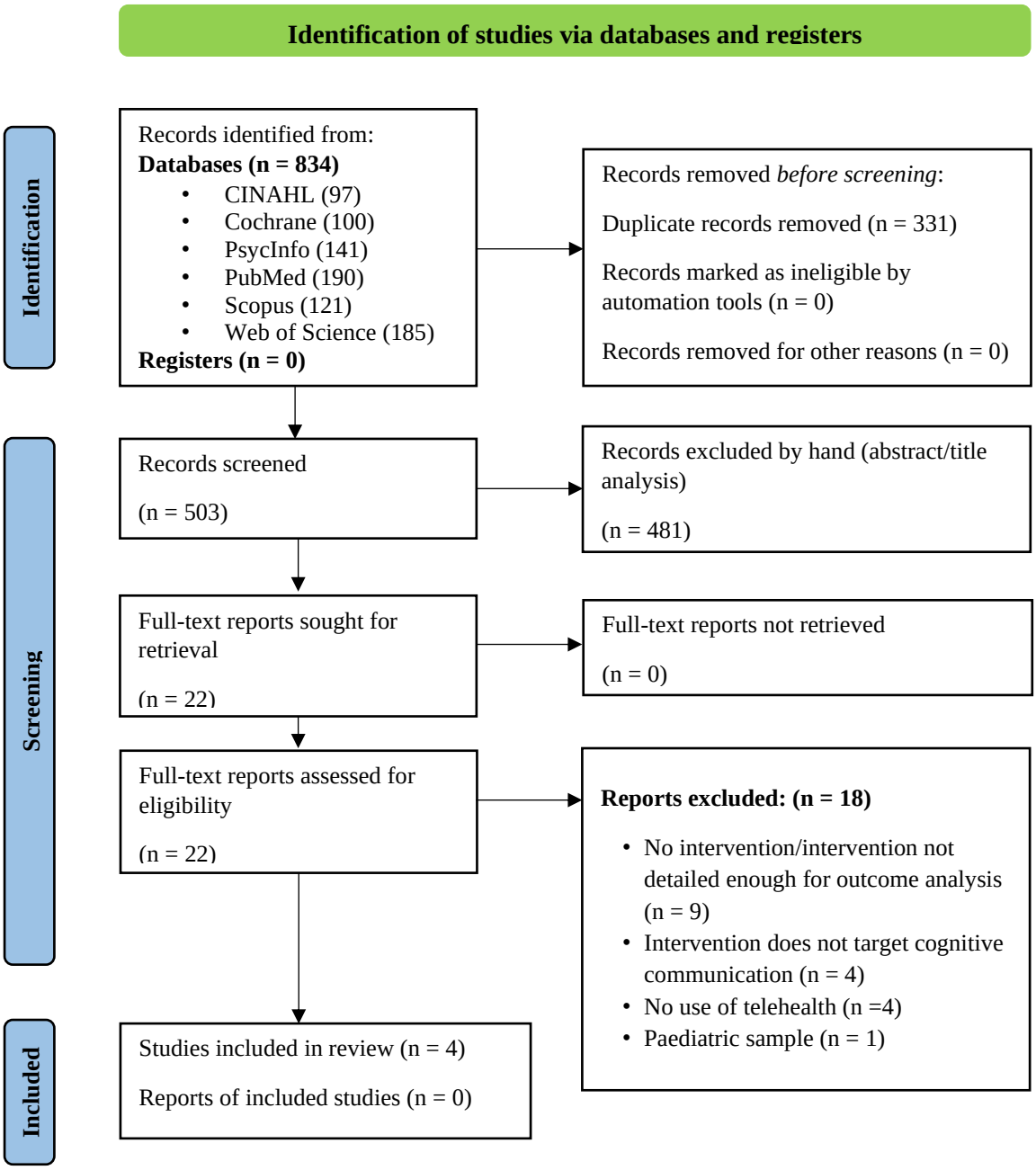
[3] Rietdijk, R., Power, E., Attard, M., Heard, R., & Togher, L. (2020a). Improved conversation outcomes after social communication skills training for people with traumatic brain injury and their communication partners: A clinical trial investigating in-person and telehealth delivery. Journal of Speech, Language, and Hearing Research, 63(2), 615–632.

[4] Rietdijk, R., Power, E., Attard, M., Heard, R., & Togher, L. (2020b). A clinical trial investigating telehealth and in-person social communication skills training for people with traumatic brain injury: Participant-reported communication outcomes. Journal of Head Trauma Rehabilitation, 35(4), 241-253.

[5] Rietdijk, R., Power, E., Brunner, M., & Togher, L. (2020). Protocol for a clinical trial of telehealth-based social communication skills training for people with traumatic brain injury and their communication partners. Brain Impairment, 21(1), 110–123.

Figure 2

PRISMA 2020 Flow Diagram



Step 2: Methodological Evaluation

Given the marked paucity of studies in this field, and consequently, the few studies included following the data sourcing step, rather than applying a strict cut-off score for methodological quality and excluding more articles on this basis, it was decided that all eligible articles would be reviewed for methodological quality – and regardless of their respective level of quality, would be included and reported on. Notably, in keeping with the aim of this review, it was determined to be more beneficial to examine the *available* evidence and to evaluate weaknesses in the design and methodological quality of studies in the field to date (Cherney et al., 2013). As such, all eligible studies were included for review and subjected to methodological review.

Of the four reviewed studies, one was a SCED, and three were partially randomised controlled trials. It is worth clarifying that 'partially' randomised refers to the fact that the procedure to allocate participants to the trial arms was not completely random. In the case of the three such studies reviewed (Rietdijk, Power, Attard, Heard, & Togher, 2020a, 2020b; Rietdijk, Power, Attard, & Togher, 2020), a certain number of participants were specifically allocated to TH training given their remote geographic location, while the remaining participants were allocated to TH or IP training using blocked randomisation, hence their classification as partially randomised controlled trials.

The methodological quality of each article was first independently evaluated by the two reviewers using the quality checklist by Cicerone et al. (2009) or Tate et al. (2008), depending on its study design. Regarding inter-rater agreement, intraclass correlation coefficient (ICC) for total score correlations could not be run as there were too few studies to return a valid ICC, as per the guidelines by Bujang and Baharum (2017). As such, Cohen's κ was run to determine the strength of agreement between both raters for both the RCTs and the SCED. These results are depicted in Table 4.

Table 4

Summary of Point-to-Point Inter-Rater Reliability

Rating Scale	Percentage Agreement Range	Average Unweighted Kappa Coefficient
RCT Checklist (Cicerone et al., 2009)	75–87.5%	.561 (*n = 2)*
SCED Checklist (Tate et al., 2008)	72.73%	Non-significant

Note. RCT = Randomised controlled trial. SCED = Single case experimental design. Only studies with significant kappa correlation coefficients were included in average kappa calculation.

For the RCTs, there was moderate agreement between the two raters, $\kappa = .561$ (95% CI, .147 to .842), $p < .001$. Cohen's κ was non-significant for the SCED. Additionally, a percentage of agreement between reviewers was calculated for each article, based on the consensus across the checklist criteria. The percentage agreement for all four studies ranged from 72.73%–87.5%, indicating moderate to high strength of agreement.

Following independent evaluation, the reviewers then discussed all discrepancies such that total consensus across the rating criteria for each study was achieved. The final quality ratings of the RCTs and the SCED are illustrated below in Table 5 and Table 6, respectively. All four studies were of sound methodological quality having fulfilled at least 66 % of their respective rating checklist criteria.

Notably, both quality rating scales use a dichotomous response format, such that all criteria are scored either zero or one, and thus weighted equally in terms of their contribution to the overall quality score. As such, it is important to give due consideration to exactly which criteria are failed and the relative risk these specific flaws in design pose to the validity of the study's findings.

RCTs

Of the three RCTs, two studies (Rietdijk, Power, Attard, Heard, & Togher, 2020a, 2020b) were of a high methodological quality (≥ 12 positive ratings), and one (Rietdijk, Power, Attard, & Togher, 2020) was moderate (6–11 positive ratings), with the studies' total scores ranging between 10–13 out of a possible total of 16. The ratings of each criterion across the individual studies are reported in Table 5 below.

It must be noted that all three of these articles are based on a single partially randomised controlled trial (Rietdijk, Power, Brunner, & Togher, 2020) with each study reporting a different outcome of this original trial. Rietdijk, Power, Attard, Heard, and Togher (2020a) and Rietdijk, Power, Attard, Heard, and Togher (2020b) respectively analyse the primary and secondary outcome measures of the intervention program (described below) while Rietdijk, Power, Attard, and Togher (2020) explore both quantitative and qualitative measures of acceptability of the TH and IP delivery formats. Because the rating checklist (Cicerone et al., 2009) awards credit based on whether there is sufficient detail reported for each criterion, the individual studies were evaluated for methodological quality separately.

Notably, the methodological quality criteria for which *all three* studies did *not* receive credit are due to the design of the original trial (Rietdijk, Power, Brunner, & Togher, 2020), while the discrepancies in quality ratings across the three studies are a result of differences in their respective outcome measures and the reporting thereof.

Table 5

Methodological Quality Criteria of RCTs

Criteria	Rietdijk, Power, Attard, Heard, & Togher (2020a)	Rietdijk, Power, Attard, Heard, & Togher (2020b)	Rietdijk, Power, Attard, & Togher (2020)
Internal Validity	-	-	-
Eligibility criteria specified	1	1	1
Randomisation	0	0	0
Treatment allocation concealed	1	1	1
Baseline characteristics	1	1	1
Interventions described	1	1	1
Cointerventions	1	1	0
Outcome measures blinded	1	0	0
Outcome measures relevant	1	1	1
Descriptive	-	-	-
Withdrawal & dropout rates	1	1	1
Short-term outcomes measured	1	1	1
Long-term outcomes measured	1	1	0
Timing of outcome measures equivalent	0	0	0
Sample size described	1	1	1
Statistical	-	-	-
ITT analysis	0	0	N/A
Point estimates and variability	1	1	1
Statistical comparison of treatment effects	1	1	1
Total (out of 16)	13	12	10
Percentage (%)	81.25	75.00	66.67[a]

Note. RCT = Randomised controlled trial. ITT = Intention-to-treat.

[a] Percentage of criteria met was calculated using a total maximum score of 15 since the ITT analysis criterion was not applicable.

Quality Criteria Unfulfilled. All three studies did not meet the following three criteria: 1) randomisation, 2) timing of outcome measures equivalent, and 3) Intention-to-Treat (ITT) analysis.

Randomisation. According to the quality rating checklist (Cicerone et al., 2009), credit is only awarded when allocation of intervention and control participants is described in sufficient detail and is fully randomised. While Rietdijk, Power, Attard, Heard, and Togher (2020a, 2020b) and Rietdijk, Power, Attard, and Togher (2020) adequately specified their method of randomisation, the studies failed this criterion as participant allocation was only *partially* randomised. That said, partial randomisation was an intentional decision taken by the authors who provide strong motivation for doing so (Rietdijk, Power, Brunner, & Togher, 2020).

While full randomisation of study participants is a key way of increasing an intervention's quality and rigour, the practical challenges of recruitment mean this is not always feasible. Furthermore, participants were recruited from both metropolitan and rural regions. Participants from rural areas who resided further from the IP training site were thus allocated to the TH training group while those located closer, were randomised to TH or IP training using blocked randomisation with the clinicians enrolling participants being blind to allocation sequence (Rietdijk, Power, Attard, Heard, & Togher, 2020a, 2020b; Rietdijk, Power, Attard, & Togher, 2020). Similarly, rather than allocating the available participants to a third control group, the researchers opted to use an historical control group of a previous study with identical eligibility criteria (Togher et al., 2016) in order to increase the study's power. Such a design thus maximised the study's rigour within real practical constraints and simultaneously enabled a more representative sample of both metropolitan and rural participants across both delivery formats (Rietdijk, Power, Attard, Heard, & Togher, 2020a).

Timing of Outcome Measures Equivalent. Credit is awarded for this item if the timing of the outcome assessment is identical for all intervention groups and for all important outcome assessments (Cicerone et al., 2009). Both Rietdijk, Power, Attard, Heard, and Togher (2020a) and Rietdijk, Power, Attard, Heard, and Togher (2020b) failed this criterion due to their use of historical control group data, which administered the follow-up assessment after a longer interval than the treatment groups. For the TH and IP training groups across both studies (Rietdijk, Power, Attard, Heard, & Togher, 2020a, 2020b), the follow up assessment was administered 3 months after the post-assessment, while the historical control group completed follow-up 6 months post-assessment (Togher et al., 2016).

The researchers again reported their reasoning for doing so, stating that a follow-up period of 3, compared to 6, months would maximise participant retention and enable the completion of data collection within the study's allocated timeline. Furthermore, given that all participants (intervention and control) were in the chronic stage of recovery, the possibility of spontaneous recovery occurring and impacting results would be minimal. Thus, the unequal timing of outcome assessment was considered justifiable.

Rietdijk, Power, Attard, and Togher (2020) did not make use of the historical control group data yet still failed this criterion, as the timing of outcome assessment was not identical for *all* outcomes across training arms. One of the measures of programme acceptability analysed in this study included a self-report measure, which participants and their CPs filled out in a post-intervention session following completion of the intervention. The time-interval between the final intervention session and the post-intervention assessment, across training dyads, ranged from 2–7 days. That said, any possible effects this variability had on the outcome measured would be present across dyads in both the TH *and* IP training arms, thus minimizing its potential for bias.

ITT Analysis. The statistical concept of ITT is intended to preserve the prognostic balance produced by the original random treatment allocation, by including every participant involved in randomisation regardless of subsequent withdrawal or non-compliance (Gupta, 2011). In accordance with this concept, the quality checklist awards credit, based on whether participants who withdraw prior to baseline observations are reported, and if all participants who received baseline evaluation are included in the pre-post intervention analysis as per ITT principles.

Both Rietdijk, Power, Attard, Heard, and Togher (2020a) and Rietdijk, Power, Attard, Heard, and Togher (2020b) identify the number of participants in each group that completed baseline evaluations but did not complete the intervention program, however they do not include these individuals in the pre-post intervention analysis and as such, do not meet the criterion.

Rietdijk, Power, Attard, and Togher (2020) could not be awarded credit for this item, although notably this is owing to the kind of data collected, rather than a pitfall in study design. Rietdijk, Power, Attard, and Togher (2020) collected qualitative data in the form of an interview and a self-report questionnaire (Agnew Relationship Measure [ARM]). The interview was aimed at collecting participants' subjective experiences of the intervention, and the questionnaire provided a rating of the participants' therapeutic alliance with their treating clinician. As such, both these measures were administered *once*, at the *end* of the

intervention. Since no pre-post comparison is made, ITT analysis does not logically apply to such data.

Similarly, the quantitative data collected included process measures of the intervention itself including treatment fidelity, number of sessions attended, and degree of homework completion. The purpose of such data was to gauge program acceptability and, in this way, provide an evaluation of the feasibility and efficacy of such an intervention, as opposed to quantifiable treatment effects calculated based on change from baseline to post-intervention. As such, ITT analysis does not apply and therefore credit could not be awarded.

Differences in Quality Criteria Across RCT Studies. The quality criteria ratings for which the three studies differed included 1) co-interventions, 2) outcome assessor blinded, and 3) long-term outcome assessment.

Co-interventions Avoided or Equivalent. The criteria checklist awards credit according to whether sufficient information is provided regarding participants' possible exposure to alternative treatment outside that of the study. If co-intervention is present, credit can still be awarded if adequate detail regarding the equivalence of cointervention between groups is reported and controlled for. Such is the case for Rietdijk, Power, Attard, Heard, and Togher (2020a), and Rietdijk, Power, Attard, Heard, and Togher (2020b), which note that participants in both the treatment and control groups were allowed to continue with any ongoing multidisciplinary rehabilitation provided that any speech therapy did not involve social communication skills or CP training – which notably, Rietdijk, Power, Attard, Heard, and Togher (2020b) report was confirmed via liaison with the treating speech pathologist. Even with this proviso in place, none of the control participants participated in concurrent speech pathology intervention during the time of the study (Rietdijk, Power, Attard, Heard, & Togher, 2020a). Given the sample is the same in all three studies, the same is true of Rietdijk, Power, Attard, and Togher (2020), however, the article makes no mention of co-interventions and therefore fails this criterion. This highlights the importance of detailed reporting in rehabilitation intervention research.

Outcome Measurement Blinded. Ideally, the individual conducting the outcome assessment should be blind to the participants' treatment condition in order to remove any potential bias. The primary outcome measure reported in Rietdijk, Power, Attard, Heard, and Togher (2020a) comprised recordings of conversations between participants with TBI and their CPs. These were then rated using scale measures by an independent, blinded assessor and the study therefore received credit for this criterion.

Rietdijk, Power, Attard, Heard, and Togher (2020b), on the other hand, analysed the secondary, participant-reported outcomes of the original trial which comprised a self-report questionnaire (La Trobe Communication Questionnaire [LCQ]) that participants and their CPs filled out both before and after the intervention, with the assistance of an independent clinician in an interview type format. Thus, seeing as the data is subjective self-report and participants are aware of their treatment condition, credit for this criterion could not be awarded. Additionally, while the study design enabled blinding of the clinician assisting with data collection, unblinding did occur for some assessments (16%) due to clinicians inadvertently being made aware of participants' location and training mode, and thus the study also did not meet this criterion due to failings in its design.

Similarly, Rietdijk, Power, Attard, and Togher (2020) had clinicians conduct a post-intervention assessment during which participants and their CPs completed a self-report measure to report on the therapeutic alliance with their treating clinician. Barring the subjective nature of the data itself, the researchers had nonetheless planned for blinded independent clinicians to collect this data and thereby limit any further bias. That said, in 23% of the interviews, clinicians were inadvertently unblinded by participants who shared information that revealed their training mode.

Long Term Outcomes Measured. The quality checklist awards credit for this criterion if long-term outcome assessment is conducted more than 3 months after the completion of the intervention and the results thereof are reported in detail within the article. Rietdijk, Power, Attard, and Togher (2020) could not receive credit for this criterion since the post-intervention interviews were conducted within 2-57 days of the final intervention session. The intention of these interviews was to explore the participants' perspectives of the different intervention modes (TH or IP). Typically, long-term outcome assessment is useful for measuring the sustained effects of an intervention and, as such, represents a valuable measure of study quality and rigour. However, given the context of this study, conducting the follow-up assessment immediately after the intervention when participants' recall of their experience would be clearest is arguably more beneficial and informative than if it were to be conducted after a longer time interval. As such, failure to meet this criterion for this particular study may not in fact be a reflection of poor methodological quality.

Quality Criteria Fulfilled. All three RCTS (Rietdijk, Power, Attard, Heard, & Togher, 2020a, 2020b; Rietdijk, Power, Attard, & Togher, 2020) met the remaining methodological quality criteria: 1) eligibility criteria specified, 2) treatment allocation concealed, 3) baseline characteristics similar, 4) interventions described, 5) outcome

measures relevant, 6) withdrawal and dropout rates described and acceptable, 7) short-term outcomes measured, 8) sample size described, 9) point estimates and variability provided, and 10) statistical comparison of treatment effects.

Internal Validity Criteria. Notably, all three studies were awarded credit for the following five (out of eight) checklist criteria pertaining specifically to internal validity. Namely, 1) inclusion and exclusion criteria were explicitly stated, 2) treatment allocation was concealed from the investigators, 3) groups were comparable on important baseline characteristics, 4) descriptions of experimental and control interventions were detailed, and 5) outcome measures were congruent and relevant to the intended effects of intervention. The fulfilment of these criteria lends support to the rigour of these studies.

In particular, it is noteworthy that treatment allocation was adequately concealed, and interventions were described in sufficient detail since these two areas are noted as being particularly problematic in the literature concerning RCTs for cognitive rehabilitation (Cicerone et al., 2009). The concealment of treatment allocation strengthens the validity of a study by minimising the possible bias inherent in its design. Indeed, there is research to suggest that inadequate concealment can result in exaggerated estimates of treatment effect that are significant enough to detract from the inherent value of employing randomized versus observational study designs (Kunz & Oxman, 1998; Schulz et al., 1995).

Regarding the descriptions of interventions, this is essential for allowing both the replication of intervention studies, and for comprehensive evaluation of their quality (Ludemann et al., 2017). Furthermore, detailed description of the various components comprising interventions enables greater insight into their relative contributions to treatment effects and can clarify which precise elements are most beneficial in terms of treatment efficacy – benefitting the design of future interventions (Meulenbroek et al., 2019).

Lastly, it is worth mentioning an important caveat regarding the equivalence of baseline characteristics. In order to receive credit for this criterion, the checklist stipulates that participants in the different treatment conditions should be comparable at the outset on all important characteristics including demographic variables and injury status – such as time since injury (Cicerone et al., 2009). Notably, in both Rietdijk, Power, Attard, Heard, and Togher (2020a) and Rietdijk, Power, Attard, Heard, and Togher (2020b), eligibility criteria for both treatment and control participants were identical with the exception of months since injury – this being 6 and 9 months for treatment and control groups, respectively. Indeed, statistical analysis demonstrated a significant difference in time post-injury between the IP and control groups (p = .029). That said, the quality checklist recognises the relative

importance of various baseline characteristics and states that reviewers should use their discretion when deciding to award credit for this particular criterion. As such, the awarding of full credit is permitted even if some characteristics between groups are not equivalent at baseline.

This qualification applies to both Rietdijk, Power, Attard, Heard, and Togher (2020a) and Rietdijk, Power, Attard, Heard, and Togher (2020b) – the reason being that while control participants may have experienced slightly longer time since injury, both control and IP participants were in the chronic stage of recovery (Rietdijk, Power, Attard, Heard, & Togher, 2020a) thus making it unlikely that a difference of a few months would have a marked impact on treatment outcomes. Furthermore, the authors (Rietdijk, Power, Attard, Heard, & Togher, 2020a, 2020b) also ran multiple regression models to investigate the potential influence of months post-injury on their respective outcome measures and in both cases, found that months post-injury was not a significant predictor of change in conversation ratings (Rietdijk, Power, Attard, Heard, & Togher, 2020a) or LCQ variables (Rietdijk, Power, Attard, Heard, & Togher, 2020b). In light of this, full credit was awarded for this criterion.

Descriptive Criteria. Adequate description of the basic elements of study design frequently forms part of frameworks for assessing methodological quality of rehabilitation interventions (Cicerone et al., 2009). Notably, all three RCTs met the following three out of the five descriptive criteria including: 1) clear description and reporting of withdrawal and dropout rates, 2) short-term outcome assessment (conducted within 3 months of intervention completion) is reported and analysed and, 3) sample size is described. Furthermore, the descriptive criterion which all three studies failed (timing of outcome measures equivalent) was given due consideration and deemed justifiable by the authors. Only one study (Rietdijk, Power, Attard, & Togher, 2020) was not awarded credit for one further item (long-term outcome measurement) and this criterion can be argued as being irrelevant within the particular context of the study's intended aim and outcome measure. The studies' fulfilment of these five fundamental design principles is thus comprehensive and indicative of sound methodology.

Statistical Criteria. Lastly, all three studies fulfilled two out of the three statistical criteria providing an important objective measure of intervention efficacy – these being 1) reporting of point estimates and measures of variability for outcome measures and 2) employing statistical analyses that include direct comparisons between treatment conditions (i.e., between-group effects).

SCED Study

The SCED study (Rietdijk et al., 2019) was of high methodological quality, scoring eight out of a possible maximum of 10 on the quality checklist (Tate et al., 2008) used for its evaluation. The scores for each of the quality criteria used to assess this SCED study are presented in Table 6.

Table 6

Methodological Quality Criteria of SCED Study

	Methodological Quality Criteria										Total [a]	%
Study	Target behaviours	Design	Baseline	Sampling behaviour during treatment	Raw data record	Inter-rater reliability	Independence of assessors	Statistical analysis	Replication	Generalisation		
Rietdijk et al., 2019	1	1	0	1	1	0	1	1	1	1	8	80

Note. [a] This score can range from 0–10.

Quality Criteria Unfulfilled. Rietdijk et al. (2019) failed to meet only two criteria. The first unmet criterion relates to establishing a sufficient baseline for the intervention's target behaviour. Along with two other items ('sampling behaviour during treatment' and 'raw data record'), this criterion was included in the checklist in order to address one of the major issues common to SCEDs – intrasubject variability in behaviour across assessment points (Tate et al., 2008). To best control for this, studies should take sufficient samples of the behaviour targeted by the intervention before, (i.e., establish baseline), during and after the intervention.

This checklist regards a baseline to be adequately stable if sample behaviour is measured on at least three occasions prior to commencing intervention (Tate et al., 2008). Notably, Rietdijk et al. (2019) specifically state that the target behaviour was sampled on three occasions pre-intervention in order to establish a baseline. However, the authors note later on that this proved inadequate when guidelines for visual inspection (Kratochwill et al.,

2013) of their collected data demonstrated that the three pre-intervention assessments did not provide a stable enough baseline pattern.

Thus, while Rietdijk et al. (2019) planned their study design, such that a stable baseline of target behaviour could be established, the authors report this nonetheless proved to be lacking – and as such, the study failed on this criterion. That said, it should be considered that what qualifies as a stable baseline is, in and of itself, contested in the literature (Manolov et al., 2016; Smith, 2012). While there is consensus that a greater number of data points ensures greater accuracy, the ruling on the minimum number of observations required and how to interpret these, is disputed. For example, in contrast to visual analysis, Beeson and Robey (2006) argue that analysis of effect sizes is a more valid way of evaluating intervention efficacy in SCEDs and propose that a minimum of three baseline probes is indeed sufficient for calculating such effect sizes. In light of this, the failure of Rietdijk et al. (2019) to meet this particular criterion should be interpreted with caution.

Additionally, Rietdijk et al. (2019) were not awarded credit for the criterion of inter-rater reliability. This criterion was included in the quality checklist for the purpose of evaluating steps taken to minimise observer bias. While Rietdijk et al. (2019) report the inter-rater reliability coefficients for the scales used, these data are the coefficients calculated from the original trial testing the measure's psychometric properties (Togher et al., 2010). In the study conducted by Rietdijk et al. (2019), two independent assessors used the rating scales to score conversation samples of the participants at different time points and the average of the two assessors' scores was used for analysis. The article does not report the inter-rater reliability of these two assessors, and therefore fails to meet this checklist criterion. That said, it is notable that the inter-rater reliability of the rating scales used has been found to be high (ICC = .84–.89 and .85–.97) and the fact that the authors (Rietdijk et al., 2019) report these psychometric properties is indicative of their choice to use valid and reliable outcome measures.

Quality Criteria Fulfilled. The fact that Rietdijk et al. (2019) meet all the remaining checklist criteria speaks to the study's strong methodological quality. The criteria for this quality checklist were designed to target key problem areas within single-subject designs proposed by authorities in the field of SCED research (Tate et al., 2008), namely, operationalisation and sufficient sampling of target behaviours, determining treatment efficacy, observer bias and generalisation. Thus, the study's almost perfect fulfilment of these criteria provides strong support for the validity and rigour of this SCED.

Step 3: Data Extraction

The results of the data extraction are depicted in Tables 7–11. Table 7 lists the citation information of the studies such as the journal and year of publication, and title of the article. Table 8 notes the design characteristics of the studies and the samples that they utilised. Finally, Tables 9–11 present the communication, feasibility, and functional outcome measures. I present in more detail the results of each of the relevant subsections below: design and sample characteristics, interventions utilised, outcome measures and associated results.

Table 7

Citation Information Extracted from Articles Selected for Review

Authors	Year	Journal	Title
Rietdijk et al.	2019	Brain Injury	A single case experimental design study on improving social communication skills after traumatic brain injury using communication partner telehealth training
Rietdijk, Power, Attard, Heard, & Togher	2020a	Journal of Speech, Language, and Hearing Research	Improved conversation outcomes after social communication skills training for people with traumatic brain injury and their communication partners: A clinical trial investigating in-person and telehealth delivery
Rietdijk, Power, Attard, Heard, & Togher	2020b	Journal of Head Trauma Rehabilitation	A clinical trial investigating telehealth and in-person social communication skills training for people with traumatic brain injury: Participant-reported communication outcomes
Rietdijk, Power, Attard, & Togher	2020	Journal of Telemedicine and Telecare	Acceptability of telehealth-delivered rehabilitation: Experiences and perspectives of people with traumatic brain injury and their carers

Design and Sample Characteristics

Table 8 outlines the design aspects of the four studies. In terms of design, one study (Rietdijk et al., 2019) was a SCED, and the three remaining (Rietdijk, Power, Attard, Heard, & Togher, 2020a, 2020b; Rietdijk, Power, Attard, & Togher, 2020) were all based on a single partially randomised controlled trial (Rietdijk, Power, Brunner, & Togher, 2020), with each study reporting on the different outcome measures utilised. That said, the participants and sampling procedure, including participant recruitment, randomisation, and allocation, was identical for all three of these latter studies since they are all a product of the same RCT.

Both the SCED (Rietdijk et al., 2019) and RCT-based studies (Rietdijk, Power, Attard, Heard, & Togher, 2020a, 2020b; Rietdijk, Power, Attard, & Togher, 2020) made use of an adult sample, with participants being aged 18 years or older. Across studies, all participants had sustained moderate-to-severe TBI defined as a score on the Glasgow Coma Scale of 9–12 (moderate) or 8 and less (severe) and/or a period of post-traumatic amnesia (PTA) of 1–24 h (moderate) or more than 24 h (severe). Likewise, in both the SCED and RCT, time since injury was a minimum of 6 months for participants with TBI receiving intervention. Additionally, in the RCT (Rietdijk, Power, Brunner, & Togher, 2020), control participants had sustained TBI at least 9 months previously. Regarding sample size, the SCED was conducted with two individuals with TBI, and their respective CPs resulting in a sample size of four participants. The RCT (Rietdijk, Power, Brunner, & Togher, 2020) compared a treatment group divided into TH (n = 19) and IP training arms (n = 17) against an historical control group (n = 15). Rietdijk, Power, Attard, Heard, and Togher (2020a, 2020b) analysed data of all three groups thus resulting in a total sample size of 51 participants, while Rietdijk, Power, Attard, and Togher (2020) made comparisons between the two training arms (TH and IP) and did not use the control data, thus obtaining a sample size of 36 participants. Both the SCED and RCT were conducted in Australia, with participants being recruited from brain injury rehabilitation units located in Australia.

Notably, the same intervention program – TBIconneCT – was used in both the SCED (Rietdijk et al., 2019) and RCT (Rietdijk, Power, Brunner, & Togher, 2020). TBIconneCT is a modified version of TBI Express, which recently has been rated as the TBI CPT program with the highest level of evidence according to a systematic review of this field (Wiltshire & Ehrlich, 2014). The key differences between TBI Express and the adapted TH version – TBIconneCT – include the latter program's online delivery format compared to the former's IP format, individual compared to group training sessions and decreased number of training hours.

TBIconneCT is a neuropsychological rehabilitation program, designed to improve social communication between individuals with TBI and their CPs (Rietdijk et al., 2019). The program comprises 10 training sessions, approximately 1.5 hours long, with additional homework tasks to be completed between sessions. Both the individual with TBI as well as a CP are required to complete training together. The program is designed such that it can be conducted IP or via TH – regarding the latter, training takes place via videoconferencing platforms with participants and the clinician participating from remote locations. For example, in all the reviewed studies participants used Skype (Rietdijk et al., 2019, Rietdijk, Power, Attard, Heard, & Togher, 2020a, 2020b; Rietdijk, Power, Attard, & Togher, 2020). TBIconneCT was conducted as outlined above in the RCTs as well as the SCED. Additionally, all participants received a hard copy of the TBIconneCT manual and were sent email summaries of session content.

The topics comprising the program content include: (1) Introducing the program (2), TBI and its impact on communication (3), Use of effective communication strategies (4), Collaboration (5), Elaboration and (6), Questioning style. Final modules are dedicated to revision and additional practice of learned strategies as well as developing a plan for maintaining progress (Rietdijk et al., 2019). The program content is taught through didactic instruction, video-modelling, rehearsing conversations and reviewing recordings of conversations in order to identify communication challenges and strategies to target the dyad's individualized communication goals (Rietdijk et al., 2019). Furthermore, to enhance delivery via videoconferencing, the program content is made visible to participants through screen sharing. Additionally, presentation slides can be emailed to participants enabling them to follow along at home.

Table 8

Design Aspects of Studies Reviewed

Authors	Country of Origin	Study type	Sample	Sample size	Intervention
Rietdijk et al., 2019	Australia	SCED	Two adults (ages 24 and 33 yrs) having sustained severe TBI at least 6 months previously, resulting in clinically significant social communication deficits, and their CPs. CPs were nominated by the participants with TBI, the only exclusion criteria being a history of severe brain injury.	*n* = 4	TBIconneCT conducted via Skype
Rietdijk, Power, Attard, Heard, & Togher, 2020a	Australia	RCT	Adults (age 18–70 yrs) who had sustained moderate-to-severe TBI at least 6 months previously, resulting in clinically significant social communication deficits and CPs nominated by the participants with TBI, the only exclusion criteria being a history of severe brain injury. Historical control participants were recruited using identical eligibility criteria excepting the time post-injury was slightly longer (9 months).	IP intervention (*n* = 17) TH intervention (*n* = 19) Historical controls (*n* = 15)	TBIconneCT. The IP training group conducted sessions with a clinician in their homes and the TH group conducted sessions over Skype.
Rietdijk, Power, Attard, Heard, & Togher, 2020b	Australia	RCT	Same as above.	Same as above.	Same as above.
Rietdijk, Power, Attard, & Togher, 2020	Australia	RCT	Adults (age 18–70 yrs) who had sustained moderate-to-severe TBI at least 6 months previously, resulting in clinically significant social communication deficits and CPs nominated by the participants with TBI, the only exclusion criteria being a history of severe brain injury.	IP intervention (*n* = 17) TH intervention (*n* = 19)	Same as above.

Note. SCED = Single Case Experimental Design; RCT = Randomised Controlled Trial; TBI = Traumatic Brain Injury; CP = Communication Partner; IP = In-person; TH = Telehealth.

Communication Outcome Measures and Results

Both the principal RCT (Rietdijk, Power, Brunner, & Togher, 2020) and SCED (Rietdijk et al., 2019) measured communication skills pre- and post-intervention. Table 9 lists the communication outcome measures utilised across studies and their associated results. All studies made use of quantitative outcome measures – including independent, as well as self- and other-report rating tools. I elaborate on the results for each of the outcome measures below.

Table 9

Communication Outcome Measures and Associated Main Findings

Authors	Outcome Measure	Main Findings
Rietdijk et al., 2019	Exchange structure analysis Adapted Kagan Scales Bond & Godfrey Scales LCQ (total score) CCRSA	Both participants and their CPs experienced positive changes on some of the self and other report measures as well as independent blinded clinician ratings of communication competence.
Rietdijk, Power, Attard, Heard, & Togher, 2020a	Adapted Kagan Scales	Trained participants (TH and IP) had significant improvements, relative to control participants, in participation in casual conversation. Trained CPs (TH and IP) had significant improvements, relative to controls, in support in casual conversation. For six of the eight rating variables, effects sizes between the IP and TH groups ranged from negligible to small, suggesting the outcomes between IP and TH training were relatively comparable.
Rietdijk, Power, Attard, Heard, & Togher, 2020b	LCQ (Total score and items with perceived positive change)	Trained participants (TH and IP) perceived greater improvements in communication over the course of intervention and following months, but this was not consistent across all measures. Both TH and IP participants reported improved communication, with the outcomes on some variables favouring the TH group.

Note. LCQ = La Trobe Communication Questionnaire; CCRSA = Communication Confidence Rating Scale; MSC = Measure of Support in Conversation; MPC = Measure of Participation in Conversation; TH = Telehealth; IP = In-person; CP = Communication Partner. Only the included studies with communication outcome measures are contained in this table.

Exchange Structure Analysis. Exchange structure analysis was the primary outcome measure utilised in Rietdijk et al. (2019).

***Measure Description*.** Exchange structure analysis is a linguistic form of assessment that breaks down transcripts of conversation into fragments of information known as "moves". These moves are then coded to form part of an exchange. Transcripts may be coded for things like information giving moves, responsive (clarifying, confirming, or challenging) moves, or testing questions. In this study (Rietdijk et al., 2019), the specific type and frequency of moves chosen for analysis was selected in relation to the two dyads' conversation goals, for example, to reduce testing questions. The coding process was conducted by two speech pathologists who independently reviewed the verbatim transcripts of conversation samples, with any discrepancies being resolved by consensus.

***Results Across Studies*.** In Rietdijk et al. (2019) exchange structure analysis was applied to a task of casual conversation for Participant 1 during which he and his CP chatted about any topic for eight minutes. For Participant 2 and his CP, analysis was applied to a news-related conversation during which they discussed headlines and photos from a news website for eight minutes. The different conversation genres were chosen for evaluation in accordance with each dyads' specific communication goals.

For Participant 1, the frequency of responsive listening moves was greater in each of the post-intervention samples compared to those of pre-intervention. Furthermore, the number of moves during intervention sessions were equal to or greater than the lowest number of pre-intervention moves. Participant 2 and his CP aimed to reduce the frequency of the CP's testing questions. The greatest number of moves recorded in a pre-intervention conversation was nine, compared to three and two in intervention and post-intervention sessions respectively. Furthermore, testing questions were present in all the pre-intervention samples, but in only four out of 10 intervention samples, and one out of three post-intervention samples.

Due to the instability of the baseline pattern of moves in pre-intervention conversation samples, a treatment effect could not be demonstrated according to guidelines for visual inspection (Kratochwill et al., 2013). That said, it is encouraging that these patterns are in line with the anticipated trajectory of conversation moves across both participants. It appears that intervention was helpful in increasing the frequency of Participant 1's requests for clarification and confirmation in keeping with his goal of becoming a more responsive listener. Similarly, the results of Participant 2 suggest that intervention was effective in building the ability of his CP's supportive conversation behaviours.

Adapted Kagan Scales. Both the RCT (Rietdijk, Power, Attard, Heard, & Togher, 2020a) and SCED (Rietdijk et al., 2019) made use of the Adapted Kagan Scales (Kagan et al., 2004).

***Measure Description*.** These scales represent two global ratings of conversation quality, used to assess the conversational skills of both the individual with TBI and the CP. The Measure of Support in Conversation (MSC) and the Measure of Participation in Conversation (MPC) were originally designed for assessing the conversations of people with aphasia (Kagan et al., 2004), but have since been adapted for use with TBI populations (Togher et al., 2010). Both the adapted MSC and MPC are 9-point Likert scales ranging from zero to four, with higher scores indicating higher levels of functioning in conversation. Further, both have proven reliable when administered via videoconferencing (Rietdijk et al., 2018).

The MSC is subdivided into Acknowledging and Revealing Competence subscales. The MSC measures the CP's ability to use appropriate, non-patronising language and to confirm the individual with TBI understands and has opportunity to respond in conversation. ICCs for inter-rater (0.85–0.97) and intra-rater (0.80–0.90) reliability are high for this scale (Togher et al., 2010). Similarly, the MPC comprises two subscales – Interaction and Transaction. The latter refers to the degree to which the individual with TBI attempts to share the conversational interaction while the former represents their ability to convey content and demonstrate understanding. ICCs for both inter-rater (ICC = .84–.89) and intra-rater reliability (ICC = .81–.92) for these scales are good (Togher et al., 2010).

***Results Across Studies*.** Both the MSC and MPC were used to evaluate participants in Rietdijk et al. (2019) and Rietdijk, Power, Attard, Heard, and Togher (2020a) on casual as well as more structured conversation tasks. In both the SCED and RCT, findings as a whole lend support to the efficacy of TH intervention in improving social communication as measured by independent observers.

In the SCED, a difference of at least 0.5 on the rating scale was regarded as clinically meaningful change and indicative of a treatment effect. Each individual's post-intervention data was compared to their respective pre-intervention performance. On ratings of casual conversation, there was meaningful positive change on both subscales of the MPC (Interaction and Transaction) for Participant 1, but not for Participant 2. No change was detected on the MSC subscales for either Participant 1 or 2. On news-related conversation, both participants demonstrated meaningful positive change on MPC Interaction but not on MPC Transaction and only Participant 1 showed such change on MSC Acknowledge

Competence. The remaining subscales demonstrated no meaningful change. While participants differed on the particular scales showing improvement, these results nonetheless demonstrate that both participants experienced positive changes in social communication skills following intervention.

Rietdijk, Power, Attard, Heard, and Togher (2020a) addressed the efficacy of their intervention in 2 parts. Firstly, trained participants (TH and IP) were compared to controls using t-tests associated with planned contrast ANOVAs on the difference between ratings from pre- to post-intervention as well as post-intervention to follow-up. Secondly, IP participants were compared to TH participants, with analysis focusing more on the magnitude of effect sizes, over significance given the small size of the sample.

Regarding the first research question, at post-intervention, on ratings of casual conversation, there were significant differences between trained and control groups, on all variables of the MSC and MPC, with trained participants showing improvements while controls' ratings declined. Effect sizes ranged from medium to large (Cohen's d = 0.71–0.88). On tasks of purposeful conversation, there was a significant difference between groups on MPC Transaction (d = 0.80, p = .01), in favour of the trained participants. There were no significant differences on the remaining MPC and MSC variables, with effect sizes ranging from negligible to small (d = 0.11–0.27).

In contrast, at follow-up, the reverse of this pattern could be observed. On casual conversation ratings, trained participants' ratings declined while controls improved. There was no significant difference on MSC Reveal Competence (the primary outcome measure) although the effect size came close to large (d = 0.77, p =.08). There were significant differences between the two groups on MSC Acknowledge Competence (d = 0.89, p =.02,) and MPC Transaction (d = 1.05, p = .01), and a difference trending significance on MPC Interaction (d = 0.82, p = .05) with effect sizes in the medium to large range. On purposeful conversation tasks, trained participants' ratings improved, while controls declined. That said, there were no significant differences, and effect sizes were in the medium range (d = 0.42–0.71). Given the above, results of the first research question provide support in favour of the efficacy of TBIconneCT over no-intervention, although the maintenance of treatment gains in the long term appears inconclusive.

Regarding the second research question, comparing TH to IP, there were no significant differences on MPC or MSC variables for casual conversation at post-intervention, and effect sizes were negligible to small (d = 0.03–0.26) suggesting minimal difference between the two formats. For purposeful conversation, findings across all MSC

and MPC variables were in favour of IP participants. Effect sizes ranged from small for MSC Acknowledge Competence (d = 0.24) and MSC Reveal Competence (d = 0.21), and medium for MPC Interaction (d = 0.64), to large for MPC Transaction (d = 0.83). Differences were not statistically significant, except for that of MPC Transaction (p = .03).

At follow-up, on casual conversation, there were no significant differences between the delivery formats, and effect sizes were negligible to small (d = 0.07–0.41). Both TH and IP participants declined in their ratings on these variables, although this was greater for those in TH. Similarly, there were no significant differences between groups on ratings for purposeful conversation. There was a medium effect size, in favour of TH, for MPC Interaction (d = 0.56) with TH participants improving and IP declining. Effect sizes were small for the remaining variables (d = 0.20–0.43), with improvements being greater for TH relative to IP on MPC Transaction, and greater for IP compared to TH on MSC Acknowledge and Reveal Competence. Taken as a whole, the results comparing the two delivery formats suggest no drastic differences between TH and IP intervention and provide some measure of support for their comparable efficacy.

Bond and Godfrey Scales. Only Rietdijk et al. (2019) made use of the Bond and Godfrey scales (Bond & Godfrey, 1997).

***Measure Description*.** These ratings scales use the same format as the Adapted Kagan scales (nine-point Likert scale ranging from zero to four), to evaluate the quality of conversations of people with TBI in terms of their appropriateness, effortfulness, as well as how interesting, and rewarding they are. Appropriateness and interestingness focus on the contribution of the individual with TBI, evaluating the suitability of topic choice and logical flow of conversation, as well as the degree to which they can engage and maintain the attention of their CP. Effortfulness ratings reflect the amount of work required from the CP in maintaining flow of conversation, and how rewarding a conversation is reflects the overall level of enjoyment derived from the interaction (Bond & Godfrey, 1997).

Results from the SCED. Only the SCED (Rietdijk et al., 2019) made use of these independent rating scales, in addition to the Adapted Kagan Scales, as part of their assessment of global conversation quality. A difference of at least 0.50 on the rating scale was regarded as clinically meaningful change and indicative of a treatment effect. Each individual's post-intervention data was compared to their respective pre-intervention performance, on the discourse genre (casual versus news-related) suited to their respective communication goals. Participant 1's post-intervention conversation (news-related) was rated as less rewarding, and Participant 2's post-intervention conversation (casual) was rated as

less interesting, as indicated by a clinically meaningful change of 0.50 on the rating scale. Post-intervention conversations of both participants were rated the same scores as their pre-intervention conversations on all other variables (appropriate, interesting, rewarding and effort), except for a very slight increase (0.25) in effortfulness at post-intervention for Participant 1 – although this was not large enough to be considered clinically meaningful. On the whole, these results suggest that the overall quality of conversations post-intervention remained largely similar to that of pre-intervention.

LCQ. The LCQ was used as a self- and other-report measure in both the SCED (Rietdijk et al., 2019) and the RCT (Rietdijk, Power, Attard, Heard, & Togher, 2020b).

***Measure Description*.** The LCQ measures perceived conversational abilities of the individual with TBI (Douglas et al., 2007) and is a widely used measure of communication. The self-report form comprises 30 items representing social communication problems. Respondents are required to rank the frequency of each item on a four-point scale. The range of possible total scores is 30 to 120, with higher scores suggesting greater perceived frequency of communication difficulty. Evaluations of the LCQ total score indicate the instrument has high internal consistency (Cronbach's α: self-report = .91; close other = .92) and acceptable test-retest reliability (ICC: self-report = .81, close other = .87; Douglas et al., 2007). Furthermore, the LCQ is reliable and suitable for administration via videoconferencing (Hoepner & Turkstra, 2013; Rietdijk et al., 2017).

A second response format is also possible to elicit judgements of change over time, such as pre- and post-intervention (Douglas et al., 2007). For each item, participants are also asked to rate the amount of change observed since a previous time point – on a scale with ratings "happens more"; "no change", and "happens less". In Rietdijk, Power, Attard, Heard, and Togher (2020b) the number of items with perceived positive change (for both self- and other-report) was also evaluated, with scores ranging from zero to 30 (higher scores suggesting greater perceived progress). Notably, the number of items with perceived positive change has not been widely used and further investigations of this outcome measure's psychometric properties are needed.

Results Across Studies. Rietdijk et al. (2019) included the LCQ as a secondary measure, focusing on the total LCQ score, and gave a brief description regarding the number of items with perceived positive change noted at post-intervention and follow up. Similarly, the LCQ was a secondary measure of the principal RCT (Rietdijk, Power, Brunner, & Togher, 2020) and reported in full by Rietdijk, Power, Attard, Heard, and Togher (2020b).

Both the total LCQ score as well as number of items with positive change (self- and other-report) was collected and analysed.

In the SCED, Participant 1 and his CP both reported statistically reliable positive change on the LCQ total score from pre- to post-intervention, with these perceived changes being maintained at follow-up. Participant 2 and his CP also recorded improved total LCQ scores from pre- to post-intervention, but this was not sufficient to be statistically reliable. Both dyads reported positive change on several items at post-assessment (range 15–24 items), as well as follow-up (range 10–22 items) suggesting maintenance of these treatment gains. Notably, Participant 1's CP noted a greater number of items with positive change (19) at post-intervention compared to Participant 1 (16) and again at follow-up (13 versus 10). In contrast, Participant 2 noted a greater number of items with positive change (24) at post-intervention than their CP (15) as well as follow-up (22 versus 16). On the whole, these findings suggest that intervention was effective in producing positive change in social communication skills observable to both the individuals with TBI as well as their CPs.

Regarding the RCT, Rietdijk, Power, Attard, Heard, and Togher (2020b) adopted a similar approach to analysis of the self- and other-report data as that of Rietdijk, Power, Attard, Heard, and Togher (2020a) with the independent rating data. Firstly, trained participants (TH and IP) were compared to controls using planned orthogonal contrasts on the difference between total LCQ scores from pre- to post-intervention as well as post-intervention to follow-up. Secondly, IP participants were compared to TH participants, with analyses focusing more on the magnitude of effect sizes, over significance given the small size of the sample. This same procedure was repeated for the number of LCQ items with perceived positive change at post-intervention and follow-up.

Notably, there were no significant differences on the total LCQ scores between trained and control groups, on both self- and other-report, at post-assessment or follow-up. The effect sizes for self-report were negligible to small, although the effect size for other-report (CPs) was medium (d = 0.60, p = .10) and close to medium at follow-up (d = 0.49, p = .17), with trained participants noting greater improvement than controls at both timepoints.

Similarly, when comparing TH to IP participants on the total LCQ score, there were no statistically significant differences between groups on self- and other-report, at post-assessment or follow-up. At post-assessment, the effect size was small (d = 0.11, p = .79) for self-report, but medium (d = 0.70, p = .07) for other-report with TH participants reporting larger reduction in the frequency of communication difficulties. At follow-up, the effect size for self-report scores was medium (d = 0.58, p = .19) with TH participants reporting greater

improvement. On other-report scores however, the effect size was negligible (d = 0.03, p = .95). While such results suggest that the two delivery formats are comparable, with neither being markedly inferior to the other, this finding would need to be repeated, and with larger samples, before being considered convincing evidence. Nonetheless, when considering the potential of TH, it is promising that differences between TH and IP were by no means drastic, and in some cases in favour of TH.

Rietdijk, Power, Attard, Heard, and Togher (2020b) also analysed the number of LCQ items with positive change, at post-intervention and follow-up, to provide an indication of participants' perceived progress. In contrast to the results of the total LCQ scores, when comparing trained (TH and IP) participants to controls, significant differences with large effect sizes (d = 1.94–2.37) were found for both self- and other-report, at post-assessment and follow-up. Trained participants reported greater number of items with positive change, reflecting greater perceived progress, compared to controls.

When comparing TH to IP, the pattern of results on number of items with positive change, is congruent with that of the total LCQ scores for both self- and other-report. No significant differences were found between groups, on self- or other-report, both at post-assessment and follow-up. Effect sizes were small for self- and other-report, at post-assessment (d = 0.17–0.23). At follow-up, there was a large effect size (d = 0.90, p = .10), favouring TH, on self-report but a negligible effect size (d = 0.07, p = .89) on other-report. Taken as a whole, the findings of the LCQ data suggest that TBIconneCT is effective in producing positive gains in social communication skills observable to both the individual with TBI and independent observers. Furthermore, when contrasting TH to IP intervention, treatment gains were comparable across groups.

Communication Confidence Rating Scale for Aphasia. In addition to the LCQ, the Communication Confidence Rating Scale for Aphasia (CCRSA) was the only other self-report rating scale utilised and was employed in the SCED (Rietdijk et al., 2019), but not the RCT.

***Measure Description*.** The CCRSA measures individuals' confidence in their communication abilities across different settings (Cherney et al., 2011). Respondents score items using a scale ranging from zero to 100, with higher total scores indicating greater confidence. Psychometric evaluation of the CCRSA with people with aphasia, confirm the usefulness of this measure, reporting an item reliability of .96 and person reliability of .81 (Babbitt et al., 2011). Further, Rietdijk, Power, Brunner, and Togher (2020) report that, in their unpublished research, the CCRSA was found to have acceptable test-rest reliability (r =

.86) with individuals with TBI, across two administrations (one in person, one via videoconferencing) spaced 2 weeks apart. While the CCRSA was not originally designed for administration via TH, this finding suggests that delivery via videoconferencing could be carried out without compromising participants' responses in any notable way.

Results from the SCED. Both participants reported positive change from pre- to post-intervention, although this change was only statistically reliable for Participant 1. Follow-up assessment was conducted 3 months post-intervention for Participant 1 and demonstrated no further statistically reliable change indicating treatment gains post-intervention were maintained. Participant 2 conducted follow-up 9 months after completing the intervention and demonstrated a very slight decline in scores, but this was not statistically reliable.

Feasibility and Telehealth Acceptability Outcome Measures and Results

Rietdijk et al. (2019) and Rietdijk, Power, Attard, and Togher (2020) were the only studies to include measures of feasibility and acceptability of the TH interventions conducted. These measures, detailed in Table 10, were almost identical across the included studies and collected both quantitative and qualitative data.

Process Measures. Both Rietdijk et al. (2019) and Rietdijk, Power, Attard, and Togher (2020) included process measures as a means of evaluating the practical implementation of TH intervention. Process measures across both studies were very similar, and included number of sessions completed, and degree of homework completion. In addition to this, Rietdijk et al. (2019) also recorded instances of videoconferencing connection problems as well as audio and video issues during sessions.

Attendance of dyads completing TH intervention in both the SCED and RCT was good. The two participants and their CPs in Rietdijk et al. (2019) completed all 10 training sessions via Skype. In the RCT (Rietdijk, Power, Attard, & Togher, 2020) there were no significant differences in the proportion of IP versus TH dyads who completed training, nor in the median number of weeks taken to complete training. While three dyads in the TH group (compared to two in the IP group) withdrew from the programme prematurely, the reasons for doing so were in no way related to difficulties with the TH delivery format, but were due to a) family crisis, b) the programme schedule being too great a time commitment, and c) a carer being unable to attend sessions on a regular basis.

Regarding homework completion, TH delivery does not appear to impact on the extent or frequency of participants' homework completion. In the SCED (Rietdijk et al., 2019), both participants completed homework either fully or partially in all (Participant 1) or almost all (Participant 2) sessions. Similarly, 15 out of the 16 TH dyads in the RCT (Rietdijk,

Power, Attard, & Togher, 2020) had homework rated as mostly or moderately complete across the program (compared to 12/15 IP dyads), with only one TH dyad completing a minimal amount of homework across sessions (compared to three IP dyads). Notably, differences in homework completion between TH and IP groups were not statistically significant.

Additionally, as part of their process measures, Rietdijk et al. (2019) recorded technical problems during sessions which varied across the participants. Participant 1 experienced connection issues in only two out of 10 sessions, minor audio and video problems in one and four sessions, respectively, but no major audio or visual problems across the intervention. Participant 2 had slightly greater difficulty, experiencing connection issues in four sessions, and audio (minor in eight and major in one) and video problems (minor in 10) in almost all sessions.

Treatment Fidelity. In both the SCED (Rietdijk et al., 2019) and RCT (Rietdijk, Power, Attard, & Togher, 2020), an independent clinician reviewed a sample of session recordings (10% and 20% respectively) using a checklist to record whether core components of the intervention were present or not. These components included aspects such as, a) discussing completion of homework, b) reviewing home practice conversation, c) discussing dedicated manualised content, d) recording a practice conversation, e) reviewing previous practice conversation, and f) providing a written summary of key strategies and homework tasks. 100% of core intervention components were present in every one of the sessions reviewed for fidelity in the SCED (Rietdijk et al., 2019) and similarly, all core process and content items were present in 97% of sessions reviewed in the RCT (Rietdijk, Power, Attard, & Togher, 2020). Notably, the instances in which items were absent occurred in TH sessions although reasons for their unfulfillment were not related to the delivery format. Indeed, it was noted that two process items may have been omitted owing to tension between the person with TBI and their CP, and one content item was absent because the person with TBI became too fatigued to continue with the session. These findings indicate that high levels of treatment fidelity were obtained in TH sessions in both the SCED and RCT.

Therapeutic Alliance. In both studies, (Rietdijk et al., 2019; Rietdijk, Power, Attard, & Togher, 2020) short forms of the ARM were administered post-intervention to provide information on participants' perceptions of their therapeutic alliance with the researcher responsible for administering the intervention. Rietdijk, Power, Attard, and Togher (2020) made use of the 12-item form (ARM-12) while Rietdijk et al. (2019) utilised the five-item form (ARM-5). The ARM is a widely used measure of alliance (Cahill et al., 2012) – that

assesses bond, partnership, confidence, openness and initiative within the client-therapist relationship. For both short-forms, items are rated according to a seven-point Likert scale, with higher total scores reflecting stronger therapeutic alliance. Evaluations of the ARM-5 and ARM-12 have reported acceptable levels of internal consistency and alternative forms reliability (Cahill et al., 2012).

Therapeutic alliance ratings were positive for TH participants. In the SCED (Rietdijk et al., 2019), both people with TBI and their CPs had ratings greater than five (out of a possible total of seven) for each of the four subscales comprising the ARM-5, these being bond, partnership, confidence, and openness (range: 5.33–7.00). Median ratings for the two subscales (core alliance and openness) comprising the ARM-12, were also high for both people with TBI and CPs (range: 5.7–7.00) in the TH group. Furthermore, the ratings of TH participants were comparable to that of the IP group with no significant differences detected between the two training arms (Rietdijk, Power, Attard, & Togher, 2020). Taken as a whole, these findings suggest that a positive therapeutic alliance can be formed when conducting intervention via TH.

Qualitative Interview. Both studies (Rietdijk et al., 2019; Rietdijk, Power, Attard, & Togher, 2020) conducted qualitative interviews following the completion of the intervention in order to gain further insight into participants' subjective experiences of the intervention program. Rietdijk et al. (2019) focused on positive and difficult aspects of the program itself, of using videoconferencing to complete it and recommendations for future implementation. Similarly, Rietdijk, Power, Attard, and Togher (2020) concentrated on understanding participants' perspectives on TH compared to IP intervention formats.

Feedback from participants receiving TH intervention in both the SCED and RCT was positive and endorsed the use of such a delivery format. Rietdijk et al. (2019) described how their participants reported the use of videoconferencing to be "easy" and that it did not detract from their experience of the intervention. Indeed, in contrast to their previous experiences of IP therapies, both Participant 2 and their CP described using Skype as "approachable", finding the online format less intimidating than the more direct face-to-face engagement of typical IP formats.

Similar themes emerged from the data collected by Rietdijk, Power, Attard, and Togher (2020). Participants described the experience of TH as, by and large, equivalent to IP intervention. Furthermore, a major advantage of TH delivery noted by participants included its ability to increase access to rehabilitation services. The use of technology removed the barrier of distance, and similarly, the reduced travel time meant rehabilitation sessions could

be incorporated into daily routines with greater flexibility and convenience. Additionally, participants also noted the potential for TH to lead to increased opportunities for interaction. The technical skills gained in intervention, could be used to seek out social connection in other areas of their life – for example, one dyad reported beginning to use Skype to connect with family members. Lastly, TH was favoured for its ability to preserve personal boundaries, with some participants describing the TH delivery format as being less intrusive relative to IP therapy.

The downside of TH delivery was largely related to technical issues. Firstly, problems with internet connection, audio and visual disturbances during sessions were described as disruptive – taking up session time and interfering with the flow of conversation. Additionally, in conjunction with the limited window view, participants felt that therapists sometimes lost out on information namely, non-verbal body language and cues (e.g., nervous fidgeting), that would have perhaps been easily picked up in IP sessions. That said, participants also described being able to successfully navigate these challenges.

Participants tended to describe the communication processes of IP therapy as more personal and natural. The use of non-verbal communication, body language and being able to observe how the clinician behaved and interacted in conversation were noted as particularly helpful aspects. Additionally, some participants thought that the face-to-face interaction facilitated a more genuine connection with the therapist, and lead to a greater sense of accountability to participate fully and complete homework tasks. While some participants enjoyed the opportunity to interact in a physical social setting, others described the logistics and effort of IP interaction as burdensome.

When asked to compare TH directly against IP delivery, a major determining factor for participants' preference for either delivery mode was personal characteristics. Specifically, older age and limited technical savviness were related to a preference for IP delivery while privacy concerns were associated with a preference for TH. Notably, increased experience of TH, appeared to increase participants' acceptance of the delivery mode. This was evidenced by the fact that, when given a choice between the two formats, all the participants who reported a preference for TH compared to IP, had received TH intervention. Indeed, of the 17 participants who reported a preference for IP intervention, only three had been part of the TH group. The possibility that acceptance of TH can be increased with experience is especially noteworthy, and encouraging, in light of the current move towards conducting rehabilitation services online due to the global COVID-19 pandemic. Given the urgent need to limit face-to-face contact, access to conventional IP therapies has been

drastically reduced, thereby making openness to TH intervention even more desirable and necessary.

Table 10

Feasibility and Telehealth Acceptability Outcome Measures and Associated Main Findings

Authors	Outcome Measure	Main Findings
Rietdijk et al., 2019	Process measures: • Session attendance • Homework completion • Videoconferencing connection problems • Audio-visual disruptions Treatment fidelity was confirmed using checklist of core components Therapeutic alliance – ARM Qualitative interview	Participants completed at least some homework in all (P1) or almost all (P2) sessions, both had connection issues in some sessions, minor audio and video technicalities in almost all sessions and major audio-visual problems in one session. Each of the core intervention components was present in every session. On the ARM, participants' ratings across categories were 5 or greater, suggesting a positive therapeutic alliance with the clinicians. Both training dyads described using Skype as a positive experience.
Rietdijk, Power, Attard, & Togher, 2020	Process measures: • Session attendance • Homework completion Treatment fidelity was confirmed using checklist of core components Therapeutic alliance – ARM Qualitative interview	No significant difference in the proportion of dyads completing training in TH (84%) and IP (88%) groups and in the median number of weeks to complete training ($U = 125.5$, $z = 0.22$, $p = 0.83$). No statistically significant difference found between TH and IP groups' homework completion ($p = 0.41$). All core components were rated as present in 97% sessions. ARM ratings for both groups were high and did not significantly differ statistically. Qualitative data centered around 3 major themes regarding TH increasing access to rehabilitation, IP delivery offering rehabilitation based on natural human interaction and weighing up the delivery formats against each other.

Note. ARM = Agnew Relationship Measure; P1 = Participant 1; P2 = Participant 2; CP = Communication Partner; TH = Telehealth; IP = In-person. Only the included studies with feasibility and telehealth acceptability outcome measures are contained in this table.

Functional Outcome Measures and Results

Rietdijk et al. (2019) was the only study to include functional outcome measures. These measures and their associated results, listed in Table 11, are described below.

Quality of Life After Brain Injury. The Quality of Life After Brain Injury (QOLIBRI) is a cross-culturally developed measure that assesses health-related quality of life following TBI, across six dimensions (cognition, self, daily life and autonomy, social relationships, emotions, and physical functioning). Each of these scales demonstrates good test-retest reliability (ICC = .75–.85) and is internally consistent (Cronbach's α = .75–.89; von Steinbuechel et al., 2010).

Rietdijk et al. (2019) made use of the Reliable Change Index to analyse change on the QOLIBRI from pre- to post-intervention, as well as post-intervention to follow-up for both participants. Statistically reliable change was demonstrated by a change index of 1.96 or greater. For participant 1, on the Cognition and Self subscales, there was statically reliable positive change from pre- to post-intervention. Thereafter there was no further statistically reliable change at follow-up, suggesting that the changes observed in these domains were maintained. There were no other statistically reliable changes reported on the remaining subscales, apart from a negative change on the Emotions subscale, from post-intervention to follow-up. Participant 2 demonstrated statistically reliable positive change on one subscale – Daily Life and Autonomy – from pre- to post-intervention, with this change being maintained at follow-up. These results suggest that overall, participants' health-related quality of life remained stable following intervention.

Participation Assessment with Recombined Tools – Objective. This is an objective measure of level of participation in society following TBI, designed for either IP or telephonic administration (Whiteneck et al., 2011). Respondents rank the frequency of several social activities, with higher scores indicating greater participation. The Participation Assessment with Recombined Tools – Objective (PART-O) has been found to meet conventional benchmarks of substantial reliability with a test-retest reliability of .79 to .87, item reliability of .99, and person reliability of .86 (Bogner et al., 2017; Whiteneck et al., 2011).

As with the QOLIBRI, the reliable change index was used to analyse changes in data from pre- to post-intervention, and post-intervention to follow-up. Both participants indicated no statistically reliable change on any of the three subscales (Productivity, Social Relations and Out and About) comprising this measure. That said, while not statistically reliable, their

scores across subscales remained the same or increased from pre- to post-intervention. Likewise, at follow-up, both participants' scores remained the same or increased, except for negligible declines on the Out and About subscale. These results suggest that participants' participation in society was not obviously influenced by the social communication skills intervention they received.

Table 11

Functional Outcome Measures and Associated Main Findings for the SCED

Authors	Outcome Measure	Main Findings
Rietdijk et al., 2019	QOLIBRI PART-O	On the QOLIBRI, P1 reported statistically reliable positive change on 2/6 subscales from pre- to post-intervention which was maintained at follow-up. He reported statistically reliable negative change on 1/6 subscales from post- to follow-up assessment. P2 reported statistically reliable positive change on 1 subscale of the QOLIBRI from pre- to post-intervention. Neither participant reported statistically reliable change on the PART-O.

Note. QOLIBRI = Quality of Life After Brain Injury; PART-O = Participation Assessment with Recombined Tools (Objective); P1 = Participant 1; P2 = Participant 2. Only included studies with functional outcome measures are contained in this Table.

Discussion

The aim of this systematic review was to investigate the current evidence base for social communication interventions implemented via TH for adults with TBI. The online format of TH holds great potential for increasing the variety and reach of neurorehabilitation services, especially against the backdrop of technological advances and global shift towards virtual functioning driven by the COVID-19 pandemic. This is particularly true of rehabilitation targeting social communication impairment since the activities typically comprising such treatment are predominantly verbal and visual in nature, thus lending itself well to delivery via TH. The review of such emerging research is thus timely and relevant. What follows is a discussion of the research to date in this field, the outcomes and barriers

encountered in implementing social communication interventions via TH, as well as their limitations, and recommendations for future rehabilitation efforts of this nature.

Notably, the current evidence for social communication provides no direct comparison between TH intervention and a no-intervention control group. In the SCED (Rietdijk et al., 2019) both participants received intervention via TH, and their post-intervention data is compared against their respective pre-intervention data. In the RCTs (Rietdijk, Power, Attard, Heard, & Togher, 2020a, 2020b), TH and IP participants are combined and compared as one treatment group to controls, thus any comparison of TH verse no-intervention is obscured by the inclusion of IP participants. It is important to note, however, that included in their analyses is a direct comparison of TH to IP participants (Rietdijk, Power, Attard, Heard, & Togher, 2020a, 2020b). Furthermore, these findings suggest that TH and IP are by and large, across communication measures, comparable. Thus, it is reasonable to consider that the findings of comparisons drawn between TH and IP (combined) and no intervention, would still apply when comparing TH alone to controls. Of course, this cannot replace studies specifically testing TH against no intervention, and the results from the current evidence must be interpreted conservatively.

Evidence for Improvement in Social Communication

Taken as a whole, the current evidence provides support for the efficacy of TH intervention for social communication deficits following TBI. Improvements in communication skills and quality were recorded by both independent observers as well as participants themselves, in both the SCED (Rietdijk et al., 2019) and the RCT-based studies (Rietdijk, Power, Attard, Heard, & Togher, 2020a, 2020b; Rietdijk, Power, Attard, & Togher, 2020).

In the SCED, both dyads' conversations at post-intervention and follow-up reflected improvements in their respective communication goals as evidenced by in-depth analysis of the content and structure of their conversations. Similarly, improvements in *global* conversation quality were also recorded on the Adapted Kagan Scales. This suggests that TBIconneCT, the intervention tool used in the studies reviewed, can help target specific areas of communication identified as problematic, and simultaneously improve the overall quality of conversations between people with TBI and their CPs. Additionally, improvement following intervention was corroborated by the participants' own subjective ratings. On both the LCQ and CCRSA self-report rating scales, both participants reported positive change post-intervention, with these changes being largely maintained at follow-up assessment. Furthermore, the other-report scores from CPs on the LCQ also demonstrated improvement

following intervention, and maintenance at follow-up indicating that CPs perceived notable differences in their partners.

It is notable that a similar pattern of findings was also repeated in the larger group study (Rietdijk, Power, Brunner, & Togher, 2020), increasing the confidence with which we can draw such conclusions. Trained participants demonstrated significant positive change on five of the eight MPC (participation) and MSC (support) ratings at post-intervention, while controls declined in their ratings. At follow-up, however, differences between groups were somewhat diminished, with no significant differences noted on the majority of MSC and MPC ratings (Rietdijk, Power, Attard, Heard, & Togher, 2020a). The improvement in trained participants noted by independent observers was corroborated by the findings of an additional measure - the LCQ self- and other-report. Participants with TBI *and* CPs who had received training recorded greater improvement at post-intervention and follow-up, on both the total LCQ scores and number of LCQ items with positive change (Rietdijk, Power, Attard, Heard, & Togher, 2020b).

Longevity of Treatment Gains

It remains uncertain whether the gains made following intervention can be sustained in the long-term due to the difference in time points for follow-up assessment between trained and control participants – this being 3-months and 6-months post-intervention, respectively (Rietdijk, Power, Attard, Heard, & Togher, 2020a, 2020b; Togher et al., 2016). From post-assessment to follow-up, on the Adapted Kagan scales, trained participants declined on some of their ratings on which controls had improved (Rietdijk, Power, Attard, Heard, & Togher, 2020a). These differences were significant on MPC Transaction and MSC Acknowledge Competence in casual conversation specifically. Notably, this decline was substantial (d = 1.05 and 0.89) and occurred over a considerably shorter time period than the improvement noted for the controls, casting doubt on the longevity of treatment gains observed at post-intervention.

In contrast, trained participants increased in their mean ratings for MSC and MPC variables in purposeful conversation at follow-up while that of controls declined. In particular, after significant improvement at post-intervention on MPC Transaction, trained participants showed further improvement with a medium effect size (d = 0.71) relative to controls. The unequal timing of follow-up assessments between the groups, however, makes it unclear whether these gains would continue their upward trajectory as skills are consolidated, or decline with time. Similarly, the same ambiguity applies to the results of Rietdijk, Power, Attard, Heard, and Togher (2020b). While trained participants noted a

significantly greater number of items with perceived positive change compared to controls at follow-up, it remains questionable whether such improvement would have been reported had the trained group been assessed 6-months from completing intervention. In light of the above and given the promising results observed at post-intervention, further research into the long-term maintenance of treatment gains seems warranted indeed.

Casual Versus Purposeful Conversation

Notably, the different conversation genres (casual versus purposeful), did seem to differ slightly in their responsiveness to the effects of the intervention. This was most clearly demonstrated in the RCT with instances of significant change occurring disproportionately more on casual conversation tasks compared to purposeful, both at post-intervention and follow-up (Rietdijk, Power, Attard, Heard, & Togher, 2020a). For example, when comparing trained versus control participants, at post-intervention significant differences were noted on all of the MSC and MPC variables for casual conversation, but only one of these four subscales for purposeful conversation. Similarly, at follow-up, significant change was detected on two of these four subscales for casual conversation but none of the subscales in purposeful conversation.

Such a pattern of results could be reflective of the different conversation genres' sensitivity to training. It has been said that casual conversation, is especially challenging for people with TBI due to its interactive nature and demanding social requirements (Bogart et al., 2012) and that these aspects make this style of conversation particularly sensitive to cognitive communication impairment (Coelho et al., 2005). Social communication difficulties may occur more frequently in casual conversations, compared to purposeful ones, since they provide less structure and thus are more likely to result in interruptions, tangential speech, or diminished topic initiation on the part of the person with TBI. This may mean that the skills learnt in training make a more obvious difference in casual conversation, as there is greater opportunity to apply them compared to purposeful conversations in which the recruitment of such skills is perhaps slightly less necessary.

That said, such a striking difference between casual and purposeful conversation was not observed in the SCED (Rietdijk et al., 2019). Instances of significant change on the MSC and MPC were roughly equal - two and three in casual and purposeful conversation respectively. While it would be imprudent to count this finding from two participants as evidence against the trend identified in the group study, it does draw attention to the need for replication thereof and further investigation into the effects of intervention across different styles of conversation.

Measuring Communication via TH

As evidenced by the number of outcome measures discussed, there are various ways of operationalising and measuring communication behaviours – reflective of the complexity of communication itself. That said, current ways of measuring communication appear to nonetheless translate easily into TH contexts and formats. For example, conversations between dyads, whether part of the IP or TH training group, were video-recorded and these recordings were then reviewed and rated by independent clinicians (Rietdijk, Power, Attard, Heard, & Togher, 2020a, 2020b). Similarly, the conversations of participants in the SCED were recorded and transcribed before being subject to exchange structure analysis. Self- and other-report questionnaires as well as interviews were successfully conducted via Skype, with the questionnaires provided to participants via email. As such, it appears the accommodations necessary for measuring communication via TH are indeed minimal and uncomplicated making their administration comparable to that of IP formats.

The current global COVID-19 pandemic has spurred a sudden interest and need for tele-assessment methods. In the interest of maintaining patients' ongoing care, the *adaption* of existing measures used in clinical practice has been prioritised compared to the development of entirely new instruments designed for online administration since the latter process is more time-consuming (Hewitt et al., 2020; Krach et al., 2020). Adapting tests for tele-assessment requires deviating from the instruments' standardized face-to-face procedures, testing environment and norms and thereby runs the risk of introducing error and impacting psychometric accuracy (Brock et al., 2012; Kline, 2015). Furthermore, the current literature on psychometric equivalency of psychological tests adapted for tele-assessment is notably scarce and very few measures have been evaluated for this purpose (Krach et al., 2020). Similarly, within the realm of social communication skills post-TBI specifically, there is a distinct lack of research demonstrating the equivalence of tele-assessment (Rietdijk et al., 2018). While preliminary investigation into the reliability of virtual administration of the MSC and MPC Adapted Kagan Scales has been conducted (Rietdijk et al., 2018), further study into the psychometric validity and accuracy of these instruments and other communication measures like them, would be prudent and should be further explored.

Independent Versus Subjective Ratings

There is value in including both independent and subjective communication outcome measures when evaluating neuropsychological rehabilitation intervention. Across the included studies, independent rating scales corroborated what participants with TBI, and their CPs reported for themselves regarding improved communication. In this way, support for the

intervention's efficacy is strengthened, having been measured, and confirmed from multiple viewpoints. Furthermore, self-report scales serve as a key way of including the input of those participating in the intervention themselves. As main stakeholders, and potential beneficiaries of the intervention, their perspective of its ability to produce meaningful real-life change is undoubtedly valuable.

That said, there are notable challenges to using and interpreting subjective ratings (self- and other-report). One such concern includes the potential for social desirability bias to influence participants' ratings in a way that promotes the intervention they received (van de Mortel, 2008). This potentially holds true for the findings of the LCQ items with perceived positive change. Rietdijk, Power, Attard, Heard, and Togher (2020b) found no significant differences between trained and control groups at either post-intervention or follow-up for the total LCQ scores, and the associated effect sizes ranged from negligible to medium. In contrast, for number of items with perceived positive change, significant differences with large effect sizes (d = 1.93–2.43) in favour of trained participants were found at post-intervention and follow-up, on both self- and other-report. This may be due to the latter tool's greater susceptibility to social desirability given that participants are asked explicitly to rate whether there has been change following intervention and whether this was negative or positive, compared to the total LCQ score based on ratings of the frequency of communication behaviours. Given this, the finding that objective independent ratings *also* indicate positive change post-intervention becomes even more meaningful.

Modifying Existing Intervention Programs for TH Delivery

It is worth noting that the TH intervention used in the reviewed studies, TBIconneCT, is an adaptation of an existing intervention program designed for IP delivery – TBI Express (Togher et al., 2013). While no formal direct comparisons were made, the authors (Rietdijk, Power, Attard, Heard, & Togher, 2020a) do note the similar outcomes on independent ratings of conversation quality at post-intervention assessment between the two programmes. Both the TBIconneCT and TBI Express participants improved significantly relative to controls, on the MPC and MSC scales in casual conversation and the MPC Transaction scale in purposeful conversation. Additionally, those trained with TBI Express also had significant improvement on MPC Interaction scale in purposeful conversation, unlike the TBIconneCT participants. On the whole, these findings suggest that training with TBIconneCT produced treatment gains in similar areas to those observed with TBI Express.

That said, comparisons of follow-up data paint a less promising picture. While participants who completed TBI Express maintained treatment gains at a 6-month follow-up

(Togher et al., 2013), a significant decline was noted for TBIconneCT participants on two scales in casual conversation – MSC Acknowledge Competence and MPC Transaction. Given that these participants were followed-up at 3-months post-intervention, this decline is even more noteworthy. The authors (Rietdijk, Power, Attard, Heard, & Togher, 2020a) acknowledge the diminished durability of treatment effects and suggest that possible explanations for this could be the reduced number of training hours or lack of the group component in TBIconneCT. Thus, while the TH adaptation of TBI Express produced similar outcomes to that of the original IP intervention programme, further consideration needs to be given to how to better maintain these treatment effects in the long term. Nonetheless, overall, these comparisons suggest that modifying IP interventions for TH delivery format could be a feasible and effective clinical possibility. This is encouraging, as it points to the potential for increasing access to rehabilitation intervention programmes by making them available via TH to those who live far from such services.

Evidence for Feasibility and Acceptability of TH Intervention

Both the SCED (Rietdijk et al., 2019) and RCT (Rietdijk, Power, Attard, & Togher, 2020) included measures of feasibility in order to evaluate the practicality of implementing intervention in TH format as well as the participants' acceptability and subjective experience of such delivery. The findings of the reviewed studies suggest that from a logistical point of view, a TH delivery format is indeed feasible. This is particularly noteworthy given that the potential for technical disruptions is one of the major perceived barriers for both clinicians and clients with regards to using TH (Ownsworth et al., 2020). While technical issues like audio-visual and connection problems were noted, these did not appear to impact significantly on the implementation of the intervention, or on participants' participation. Indeed, attendance, and degree of homework completion was comparable between TH and IP participants – both being relatively high. Furthermore, treatment fidelity was close to perfect – with the essential components of the program process and content present in almost every TH session. Notably, in the few instances in which core items were lacking, their absence was not due to technical issues relating to TH delivery but rather to personal factors related to the participants (e.g., conflict with their CP, fatigue). While such findings are certainly encouraging, it must be recognised that they stem from a limited evidence base of two studies – only one of which was completed at a large scale (Rietdijk, Power, Brunner, & Togher, 2020). Additionally, given the differences in access and quality of technology in different contexts, further replication of intervention administered via TH is needed before firm conclusions can be drawn regarding its feasibility.

Importantly, the practicalities of TH delivery did not appear to detract from participants' subjective experiences of the intervention. Across the included studies, both quantitative and qualitative measures of programme acceptability indicated high levels of satisfaction with TH intervention. While participants acknowledged differences in interacting virtually (e.g., limited viewing window, less non-verbal cues) compared to IP, ratings of client-therapist relationships remained positive. This is particularly notable, since there is concern present in the literature regarding the impact of TH intervention on the development of a strong therapeutic alliance (Henry et al., 2017; Lopez et al., 2019).

Furthermore, participants' subjective experiences corroborated these quantitative findings and gave further insight into the advantages and challenges of TH intervention. Themes emerging from qualitative data, centred on the ability of TH to increase access to rehabilitation services, as well as its increased convenience and minimal logistics. While technical issues were an acknowledged issue with TH delivery, this was not presented as an overwhelming challenge for participants. The nature of interacting via videoconferencing was described as being different to IP conversing, with some participants finding TH interactions less personal and natural compared to IP, and others preferring the virtual connection due to it feeling less intrusive than face-to-face communicating. Ultimately, participants' opinions on TH acceptability and their preference for either delivery format was largely determined by personal characteristics. Thus, while from a practical perspective, TH intervention appears largely feasible, its efficacy may also, in part, depend on the particular individuals making use of it. Indeed, this very principle has been reported elsewhere in the TH literature (Adams et al., 2021; Lawson et al., 2020)

Evidence for Functional Impact of TH Intervention

The value of including functional outcome measures in intervention studies lies in their ability to reflect the impact that intervention has on the participant's broader context – i.e., whether the gains observed in treatment translate into or extend to real life everyday contexts (Hall et al., 2001). It is thus encouraging that for the participants in the SCED (Rietdijk et al., 2019) change on the QOLIBRI (measuring quality of life) was largely positive, with three instances of such change being statistically reliable. Similarly, although no statistically reliable change was detected on the PART-O (measuring participation in society), scores generally remained the same or improved post-intervention and at follow-up. That said, such findings should be interpreted cautiously for the following reasons.

Participant 1 reported statistically reliable change for two subscales (Cognition and Self) which include questions probing thinking abilities (e.g., concentration, memory,

expression, and comprehension) and self-esteem, respectively. Changes in these domains may have improved as a function of perceived improvement in communication abilities, although a direct association cannot be assumed. Similarly, the statically reliable positive change noted for Participant 2 on the Daily Life and Autonomy subscale may also have been influenced by treatment gains in the area of communication. Notably, several questions comprising this section of the questionnaire relate to participation in social settings, work and education and leisure activities. It is plausible that Participant 2, as a result of improved ability and confidence in conversing with others, may have consequently felt more capable of engaging in such activities as these listed on the questionnaire response items.

That said, apart from these three subscales on the QOLIBRI, no other statistically reliable change was detected on any of the remaining subscales of both the QOLIBRI and PART-O. Once again, the content of the questionnaires must be considered when interpreting these results (see Appendices G and H). Both the QOLIBRI and PART-O are broad measures, in the sense that they span several domains of everyday functioning – including several in which one may not readily expect improvements in communication to necessarily impact an individual's functioning therein. For example, the QOLIBRI requires participants to rate how bothered they are by their physical sequelae and emotions, and the PART-O requires respondents to rate how frequently they engage in sports, go shopping or eat in a restaurant. Participation in these kinds of activities could be influenced by improved communication, although, equally as much could be a function of reasons unrelated to communication ability (e.g., premorbid personal preference). As such, these broad functional outcome measures may not be the most apt or accurate indicator of the intervention's ecological validity due to their wide scope and somewhat ambiguous questions.

The results on the functional outcome measures discussed are of course limited having been drawn from a sample of two participants and thus conclusions regarding the broader impact of social communication intervention cannot be inferred. On the other hand, one should keep in mind the changes observed in the communication rating scales that evaluate participants' confidence and ability in everyday *conversational* contexts *outside of intervention*. Indeed, as discussed above, the results of the LCQ and CCRSA included several indications of statistically reliable positive change across studies (Rietdijk et al., 2019; Rietdijk, Power, Attard, Heard, & Togher, 2020b). Such findings indicate that TH intervention for social communication does produce change at a broader, everyday level to some degree, indicative of the intervention's ecological validity.

Limitations and Considerations

The number of papers in this review is an acknowledged limitation. Additional papers may have been identified through the use of alternative search strategies (e.g., hand searching journals) or through using additional search terms. Given that only studies written or translated into English were eligible for inclusion, additional studies in other languages may have existed. Since the research base comprises so few studies, the conclusions that can be drawn from it are limited in terms of their reliability and generalisability.

Furthermore, the current studies in this field are all conducted by one research group. The same authors are responsible for publishing the SCED (Rietdijk et al., 2019) as well as the trial protocol for the principal RCT (Rietdijk, Power, Brunner, & Togher, 2020), raising concern around the potential bias in the interpretation of findings. Additionally, all the studies to date have been conducted in the same geographical context – Australia. Thus, this introduces further reservations concerning the generalisability of the current research findings regarding the efficacy and feasibility of TH. Lastly, apart from potential cultural bias, this is also worth noting given that access to and quality of technology differs markedly between HICs (such as Australia) and LMICs (such as South Africa), and that internet access is an essential prerequisite for TH interventions.

While the evidence base for TBIconneCT is, at present, restricted to a HIC context, the increasing reach and use of internet and technology in LMICs suggests that the program, and other TH interventions like it, could be viable options in the future, in these contexts too. Certainly, this stands as a potential future area of research well worth investigating given the potential for TH to increase access to rehabilitation services in LMIC contexts where such services are exceedingly needed, yet distinctly lacking.

An additional consideration is the fact that TH intervention for social communication has at present only been trialled with individuals with moderate-to-severe TBI. While one would expect that interventions producing treatment gains in moderate-to-severe TBI populations would likely do the same in samples with milder TBI, such conclusions cannot be made with certainty given that no actual studies to date have been conducted with populations of individuals with mild TBI. Given that *mild* TBI accounts for 80-90% of TBI cases, and that this population also commonly experiences impairments in cognitive communication (Blyth et al., 2012; LeBlanc et al., 2020), it appears worthwhile indeed to establish an evidence base for interventions targeting such deficits with this particular population too.

Lastly, the sample size of the principal RCT (Rietdijk, Power, Brunner, & Togher, 2020) was not large enough to conduct a non-inferiority trial for comparison between the TH and IP intervention groups. As such, both Rietdijk, Power, Attard, Heard, and Togher (2020a) and Rietdijk, Power, Attard, Heard, and Togher (2020b) opted to examine the magnitude of any effects observed between TH and IP treatment groups using effect sizes, and *p*-values to assess whether any of these differences were significant based on the sample size available. The appeal of a non-inferiority design is that it serves as a more robust way of evaluating the relative efficacy of two kinds of intervention – or, more precisely, whether one kind of treatment is not 'unacceptably worse' than the current treatment of choice (Head et al., 2012) – in this case TH versus IP rehabilitation.

In the context of TH, such analysis would be particularly helpful since it answers more reliably if, even if there are some costs to doing intervention online, it is not exceedingly less effective than its IP equivalent (i.e., still useful). The answer to this question is especially pertinent, in the context where adopting TH intervention could mean increasing people's access and ability to participate in rehabilitation. However, the sample size of the current studies (Rietdijk, Power, Attard, Heard, & Togher, 2020a, 2020b) were not sufficiently large enough to meet the requirements for conducting such statistical analysis. The authors acknowledge this and for this reason, declare that this particular research question (TH compared to IP) was exploratory in nature having no specific a priori hypothesis formulated. As such, while we cannot draw firm conclusions regarding the non-inferiority of TH to IP intervention from the current evidence base, the magnitude of effect sizes found does suggest that conducting larger non-inferiority trials is worthwhile.

Recommendations

While the current evidence base for TH intervention targeting social communication impairment post-TBI is modest, the insights offered by these pioneering studies are nonetheless valuable in guiding future research and intervention efforts in this field. Some of the questions raised by this review warrant further investigation in order to determine guidelines for best clinical practice. What follows are some recommendations for future research efforts based on the findings of the current study.

Expanding the Evidence Base: Greater Replication of SCEDs

While RCTs are largely regarded as the golden standard in evidence-based treatment, it has been argued that single subject designs, especially in young fields in which RCTs are not widely available, can contribute meaningful data (MacDonald & Wiseman-Hakes, 2010). Given the heterogeneity of TBI and likewise its impact on social communication and insight,

as well as individuals' acceptability of TH, SCEDs are an effective way of answering this call for individualised, tailored rehabilitation. It would be prudent to consider further replication of SCEDs investigating the efficacy of TH intervention for social communication post-TBI.

Expanding the Evidence Base: Group studies

In order to draw firmer conclusions regarding treatment efficacy and feasibility, larger scale studies are necessary. Given the infancy of this field, smaller pilot studies like that conducted in SCEDs should serve as the basis for the design and implementation of larger group studies with bigger samples.

Long Term Follow-Up

The data from the available studies regarding the maintenance of treatment effects at follow-up remains unclear. Future studies, comparing control and treatment groups measured at the *same* distance and *longer* distance from post-intervention, could offer valuable insight into the longevity of treatment gains, and whether continued intervention into the long-term is necessary.

Best Communication Outcome Measurement

Both subjective report and independent observation measures for communication exist and a combination of both appears useful. Further research into standardised assessment tools, in particular the number of LCQ items with perceived positive change, is needed. The latter was developed as a supplementary clinical tool yet has not been used widely nor investigated psychometrically like that of the Total LCQ forms. Given the instrument's potential clinical usefulness, yet simultaneous susceptibility to social desirability bias, further research into its reliability and validity is merited.

Conversation Genres

Findings from the current studies suggest that different conversation genres (casual versus purposeful) may differ in their responsiveness to intervention. Further studies, using larger samples, and clearer operationalisation of casual versus purposeful conversation tasks, would be useful before drawing firm conclusions.

Best Functional Outcome Measurement

Analysis of the functional outcome measures included in this review highlight that the impact of training on everyday life can be nuanced and subtle and thus, may not best be captured by surveys probing overly broad domains. Instead, the ecological validity of intervention may be more clearly and accurately explored through qualitative interviews which allow respondents to describe more fully how, and to what extent, they feel training has impacted their daily lives.

Representative Samples

The available evidence considers only moderate to severe TBI. Given that social communication deficits present in people with mild TBI too, it could be useful to investigate the usefulness of intervention for this latter population as well. Similarly, current research is restricted to a HIC context – studies in LMICs are necessary in order to establish the efficacy and feasibility of TH interventions in more resource-constrained settings.

Blended IP and TH Intervention

The studies to date offered intervention exclusively IP or via TH. In the current context of the COVID-19 pandemic in which a blended approach of both IP and TH functioning is being adopted, it may be useful to explore such a delivery format in rehabilitation programmes too, and how this may influence intervention efficacy and feasibility.

Summary and Conclusion

In conclusion, the current evidence base for TH intervention targeting social communication deficits post-TBI is markedly limited. Given the rapid increase in digitisation and tele-services, and the supposed ease with which social communication intervention methods translate to TH formats, this finding is indeed surprising. Furthermore, given the current worldwide move towards virtual operations, further research into this field appears warranted indeed. While the evidence base at present is scarce, the studies that have been conducted do nonetheless highlight the potential for TH intervention in targeting social communication deficits post-TBI.

In both a SCED and RCT, improvements in social communication skills and conversation quality were observed by independent observers as well as participants with TBI and their CPs, following TH delivery of social communication intervention – TBIconneCT. Furthermore, in comparison to IP intervention, results of participants in the TH condition appear comparable, suggesting that modifying intervention for TH delivery did not detract from its efficacy. Similarly, across studies the logistics of TH delivery did not appear to detract from the subjective experience of intervention with participants reporting high levels of satisfaction, acceptability, and feasibility.

TBIconneCT stands as an example of effective TH intervention for social communication deficits post-TBI. While further research is required to cement such conclusions, the findings from these emerging studies are promising indeed. If we are to maximise on the potential of such interventions, future studies in this field are both necessary

and justified in order to expand on clinical possibilities for rehabilitation and strengthen the evidence base for TH intervention for social communication deficits following TBI.

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Appendix A

Summary of Intervention Studies Targeting Social Communication Deficits Post-TBI

Authors	Study design	Participants	Intervention	Outcomes
			Social skills training	
Braden et al., 2010	Cohort study with pre-post intervention and follow-up assessments	Individuals with TBI and concomitant neurological conditions or current psychiatric disorders	GIST [a]	Improved outcomes on subjective social communication skills measures, post-intervention and at 6 months follow-up. Participants with TBI and additional diagnoses of psychiatric/psychological disorders or other neurological complications showed similar patterns of improvement relative to TBI-only participants
Dahlberg et al., 2007	Randomized treatment and deferred treatment, controlled trial	Individuals with moderate to severe TBI and impairment in social communication skills as indicated by score on the adapted Social Communication Skills Questionnaire	GIST	Most significant improvement noted in general participation in conversation, external and internal relation, social style, clarity of expression and speech characteristics. Partial support for improved social integration and quality of life.
Harrison-Felix et al., 2018	Multicentre randomized controlled trial	179 civilian, military, and veteran adults with TBI and social competence difficulties, at least 6 months post-injury	Intervention group: GIST Control group: Traditional classroom sessions using the same curriculum with brief supplemental individual sessions but without structured group interaction	Social competence skills improved for individuals with TBI in both treatment conditions.
McDonald et al., 2008	Randomized controlled trial comparing a social skills program with social activity alone or with waitlisted control	Individuals with severe, chronic acquired brain injury referred for social skills deficits	Twelve-week social skills treatment program encompassing weekly 3 hour-group sessions focused on remediating social behaviour and perception and 1-hour individual sessions addressing psychological issues such as mood, and self-esteem.	Modest treatment effects which were limited to direct measures of behaviour. Improvement circumscribed to partner-directed behaviours. No change detected on measures of social functioning and participation

O'Reilly et al., 2000	Multiple baseline design	Two individuals with severe TBI, employed as shop assistants, working 20 hours a week	Problem-solving model taught to apply appropriate social skills. Sessions included modelling, and role play of social situations with a trained therapist as well as a feedback component on performance.	Self-report and observations of others confirmed significant increases in targeted social skills both in training and employment settings, post-intervention and at 6 months follow-up.
			Communication partner training	
Behn et al., 2012	Single blinded randomized controlled study.	Ten paid carers randomly selected from a post-acute residential rehabilitation programme	Adaptation of TBI Express [b]	Trained carers were more able to acknowledge and reveal the competence of people with TBI. Conversations were perceived as more appropriate, interesting and rewarding compared to the control group. Improvements were confined to structured conversations and were maintained at 6-month follow-up
Behn et al., 2015	Qualitative design	Paid caregivers of individuals with TBI	Adaptation of TBI Express.	Paid carers described improved knowledge and use of strategies, improved communication and positive emotional experiences
Goldblum & Alant, 2009	Randomised controlled trial	Customer service managers, customer care assistants and frontline customer service workers from a large national retail supermarket chain	Once-off, 4-hour-long training session, developed based on previously established principles of diversity awareness training. Training used video recordings and feedback, within small group discussion format.	Results indicated increased confidence and knowledge in identifying barriers to, and facilitators of, interaction with customers with a cognitive communication disorder following a TBI
Togher et al., 2004	Randomised controlled trial	20 police officers	6-week programme, comprising six 2-hour sessions of communication strategies training based on the theoretical model of Systemic Functional Linguistics. Sessions included case studies, role-play and small group activities.	Training led to the provision of appropriate feedback, support and structure of everyday interactions on the part of police officers during telephonic conversation with individuals with TBI.

Combined social skills training and communication partner training				
Hoepner & Olson, 2018	Mixed methods pre-post observational design	Single individual with moderate to severe TBI and his communication partner	16-week video self-modelling intervention completed by individual with TBI in conjunction with his communication partner	Individual with TBI demonstrated increased awareness of conversational competence and a reduction in negative communication behaviours. The communication partner increased positive conversational supports and reduced antagonistic statements
Mann et al., 2015	Analysis of portion of data collected from larger study (Togher et al., 2013)	Individuals with severe chronic traumatic brain injuries and non-injured communication partners	TBI Express	"Kagan plus" dyads had obvious changes in their questioning practices that facilitated selection of topics and the development of related talk, Conversely, the "Kagan neutral" dyads exhibited less obvious differences in their questioning practices after training, which meant that improvement in communication postintervention was less apparent.
Rietdijk et al., 2019	Feasibility study involving single case ABA experimental design	Two participants with severe TBI and significant social communication deficit - and their non-injured communication partners	TBIconneCT[c]	Positive changes on self-report of confidence and competence in conversation and improved quality of life. Communication partners reported positive change on measures of participant's conversational competence and qualitatively described improved communication. Independent raters indicated meaningful positive changes in communication abilities of participants and their communication partners.
Rietdijk, Power, Attard, Heard, & Togher, 2020a	Partially randomised controlled trial, including an in-person intervention group, a telehealth intervention group, and a historical control group	Individuals with moderate-to-severe TBI and social communication skills deficits - and their non-injured communication partners	TBIconneCT (either in person or via telehealth)	Trained participants with TBI had significant improvements in participation in casual conversation compared to controls. Trained communication partners also had significant improvements compared to controls on ratings of support in casual conversations
Sim et al., 2013	Multi-centre non-randomized controlled trial comparing an intervention and control group.	Individuals with severe chronic traumatic brain injuries and non-injured communication partners	TBI Express	Joint training resulted in participants with TBI, increasing contribution to conversation, improved turn taking and social appropriateness. Communication partners improved their use of positive collaborative and elaborative strategies and decreased their use of negative questioning.

Togher et al., 2013	Three arm non-randomized controlled trial comparing communication partner training (JOINT) with individual treatment (TBI SOLO) and a waitlist control group	Individuals with severe chronic traumatic brain injuries and non-injured communication partners	TBI Express	Communication partner training improved conversational performance compared to training individuals with TBI alone and a waitlist control group on the primary outcome measures. Results were maintained at six months follow-up.
Togher et al., 2016	Three arm non-randomized controlled trial comparing communication partner training (JOINT) with individual treatment (TBI SOLO) and a waitlist control group	Individuals with severe chronic traumatic brain injuries and non-injured communication partners	TBI Express	Communication partner training (JOINT) improved conversational performance compared to training the individual with TBI alone and a waitlist control group on a measure of perceived communication ability (LCQ). The TBI SOLO group improved on the LCQ compared to the CONTROL group. Results were maintained at six months post-training.

Note. GIST = Group Interactive Structured Treatment. TBI = Traumatic Brain Injury

[a] GIST is a manualized intervention for social competence comprising weekly group sessions. [b] TBI Express is a ten-week conversational skills treatment program encompassing weekly group and individual sessions. [c] TBIconneCT is a modified version of TBI Express adapted for use with individuals via videoconferencing.

Appendix B

PRISMA 2020 Checklist

PRISMA 2020 Systematic Review Checklist

Section and topic	Item #	Checklist item	Location where item is reported
Title	1	Identify the report as a systematic review.	1
Abstract	2	See the PRISMA 2020 for Abstracts checklist.	3
		Introduction	
Rationale	3	Describe the rationale for the review in the context of existing knowledge.	18-19
Objectives	4	Provide an explicit statement of the objective(s) or question(s) the review addresses.	19
		Methods	
Eligibility criteria	5	Specify the inclusion and exclusion criteria for the review and how studies were grouped for the syntheses.	23
Information sources	6	Specify all databases, registers, websites, organisations, reference lists and other sources searched or consulted to identify studies. Specify the date when each source was last searched or consulted.	20,22, 26
Search strategy	7	Present the full search strategies for all databases, registers and websites, including any filters and limits used.	95-101
Selection process	8	Specify the methods used to decide whether a study met the inclusion criteria of the review, including how many reviewers screened each record and each report retrieved, whether they worked independently, and if applicable, details of automation tools used in the process.	22-24
Data collection process	9	Specify the methods used to collect data from reports, including how many reviewers collected data from each report, whether they worked independently, any processes for obtaining or confirming data from study investigators, and if applicable, details of automation tools used in the process.	26-27
Data items	10a	List and define all outcomes for which data were sought. Specify whether all results that were compatible with each outcome domain in each study were sought (e.g., for all measures, time points, analyses), and if not, the methods used to decide which results to collect.	25
	10b	List and define all other variables for which data were sought (e.g., participant and intervention characteristics, funding sources). Describe any assumptions made about any missing or unclear information.	41
Study risk of bias assessment	11	Specify the methods used to assess risk of bias in the included studies, including details of the tool(s) used, how many reviewers assessed each study and whether they worked independently, and if applicable, details of automation tools used in the process.	24,29

PRISMA 2020 Systematic Review Checklist

Section and topic	Item #	Checklist item	Location where item is reported
Effect measures	12	Specify for each outcome the effect measure(s) (e.g., risk ratio, mean difference) used in the synthesis or presentation of results.	N/A
Synthesis methods	13a	Describe the processes used to decide which studies were eligible for each synthesis (e.g., tabulating the study intervention characteristics and comparing against the planned groups for each synthesis (item #5)).	N/A
	13b	Describe any methods required to prepare the data for presentation or synthesis, such as handling of missing summary statistics, or data conversions.	N/A
	13c	Describe any methods used to tabulate or visually display results of individual studies and syntheses.	N/A
	13d	Describe any methods used to synthesize results and provide a rationale for the choice(s). If meta-analysis was performed, describe the model(s), method(s) to identify the presence and extent of statistical heterogeneity, and software package(s) used.	N/A
	13e	Describe any methods used to explore possible causes of heterogeneity among study results (e.g., subgroup analysis, meta-regression).	N/A
	13f	Describe any sensitivity analyses conducted to assess robustness of the synthesized results.	N/A
Reporting bias assessment	14	Describe any methods used to assess risk of bias due to missing results in a synthesis (arising from reporting biases).	N/A
Certainty assessment	15	Describe any methods used to assess certainty (or confidence) in the body of evidence for an outcome.	N/A
		Results	
Study selection	16a	Describe the results of the search and selection process, from the number of records identified in the search to the number of studies included in the review, ideally using a flow diagram.	28
	16b	Cite studies that might appear to meet the inclusion criteria, but which were excluded, and explain why they were excluded.	27
Study characteristics	17	Cite each included study and present its characteristics.	41, 44
Risk of bias in studies	18	Present assessments of risk of bias for each included study.	32, 38

PRISMA 2020 Systematic Review Checklist

Section and topic	Item #	Checklist item	Location where item is reported
Results of individual studies	19	For all outcomes, present, for each study: (a) summary statistics for each group (where appropriate) and (b) an effect estimates and its precision (e.g., confidence/credible interval), ideally using structured tables or plots.	N/A
Results of syntheses	20a	For each synthesis, briefly summarise the characteristics and risk of bias among contributing studies.	N/A
	20b	Present results of all statistical syntheses conducted. If meta-analysis was done, present for each the summary estimate and its precision (e.g., confidence/credible interval) and measures of statistical heterogeneity. If comparing groups, describe the direction of the effect.	N/A
	20c	Present results of all investigations of possible causes of heterogeneity among study results.	N/A
	20d	Present results of all sensitivity analyses conducted to assess the robustness of the synthesized results.	N/A
Reporting biases	21	Present assessments of risk of bias due to missing results (arising from reporting biases) for each synthesis assessed.	N/A
Certainty of evidence	22	Present assessments of certainty (or confidence) in the body of evidence for each outcome assessed.	N/A
		Discussion	
Discussion	23a	Provide a general interpretation of the results in the context of other evidence.	63-64, 68
	23b	Discuss any limitations of the evidence included in the review.	67
	23c	Discuss any limitations of the review processes used.	67
	23d	Discuss implications of the results for practice, policy, and future research.	69
		Other information	
Registration and protocol	24a	Provide registration information for the review, including register name and registration number, or state that the review was not registered.	20
	24b	Indicate where the review protocol can be accessed, or state that a protocol was not prepared.	20
	24c	Describe and explain any amendments to information provided at registration or in the protocol.	N/A
Support	25	Describe sources of financial or non-financial support for the review, and the role of the funders or sponsors in the review.	N/A
Competing interests	26	Declare any competing interests of review authors.	1

PRISMA 2020 Systematic Review Checklist

Section and topic	Item #	Checklist item	Location where item is reported
Availability of data, code & other materials	27	Report which of the following are publicly available and where they can be found: template data collection forms; data extracted from included studies; data used for all analyses; analytic code; any other materials used in the review.	N/A

Note. Taken from*:* Page, M. J., McKenzie, J. E., Bossuyt, P. M., Boutron, I., Hoffmann, T. C., Mulrow, C. D., Shamseer, L., Tetzlaff, J. M., Akl, E. A., Brennan, S. E., Chou, R., Glanville, J., Grimshaw, J. M., Hróbjartsson, A., Lalu, M. M., Li, R., Loder, E. W., Mayo-Wilson, E., McDonald, S., … & Moher, D. (2021). The PRISMA 2020 statement: An updated guideline for reporting systematic reviews. *BMJ, 372*(71), Article 105906. https://doi.org/10.1136/bmj.n71

Appendix C

UCT Department of Psychology Ethical Approval Letter

UNIVERSITY OF CAPE TOWN

Department of Psychology

University of Cape Town Rondebosch 7701 South Africa
Telephone (021) 650 3417
Fax No. (021) 650 4104

02 December 2020

Taryn Christie-Taylor
Department of Psychology
University of Cape Town
Rondebosch 7701

Dear Taryn

I am pleased to inform you that ethical clearance has been given by an Ethics Review Committee of the Faculty of Humanities for your study, *Social Communication Intervention via Telehealth Following Traumatic Brain Injury in Adults: A Systematic Review.* The reference number is PSY2020-021.

I wish you all the best for your study.

Yours sincerely

Catherine Ward
Professor
Chair: Ethics Review Committee

Appendix D

Database Search Strategies

CINAHL

Search ID#	Search Terms	Search Options
S13	S3 AND S6 AND S9 AND S12	**Expanders** - Apply equivalent subjects **Search modes** - Boolean/Phrase
S12	S10 OR S11	**Expanders** - Apply equivalent subjects **Search modes** - Boolean/Phrase
S11	(MH "Telerehabilitation") OR (MH "Remote Consultation") OR (MH "Videoconferencing") OR (MH "Telehealth+") OR (MH "Telemedicine") OR (MH "Teleconferencing")	**Expanders** - Apply equivalent subjects **Search modes** - Boolean/Phrase
S10	"information and communications tech*" OR "information and communication tech*" OR ICT OR "video conferenc*" OR "video-conferenc*" OR video OR online OR internet OR telehealth OR telemedicine OR telerehab* OR telecare OR telecommunicat* OR "tele-health" OR "tele-medicine" OR "tele-rehab*" OR "tele-care" OR "tele-communicat*"	**Expanders** - Apply equivalent subjects **Search modes** - Boolean/Phrase
S9	S7 OR S8	**Expanders** - Apply equivalent subjects **Search modes** - Boolean/Phrase
S7	Intervention OR interven* OR rehabilitation OR rehab OR rehab* OR rehabilitate OR program OR programme OR training OR train* OR remediation OR remediat* OR treatment OR treat* OR therapy	**Expanders** - Apply equivalent subjects **Search modes** - Boolean/Phrase
S6	S4 OR S5	**Expanders** - Apply equivalent subjects **Search modes** - Boolean/Phrase
S5	(MH "Communication Skills") OR (MH "Communication Skills Training") OR (MH "Social Skills Training")	**Expanders** - Apply equivalent subjects **Search modes** - Boolean/Phrase
S4	"cognitive communication" OR "cognitive-communication" or "social communication" OR communicat* OR "communication skills" OR conversat* OR discourse	**Expanders** - Apply equivalent subjects **Search modes** - Boolean/Phrase
S3	S1 OR S2	**Expanders** - Apply equivalent subjects **Search modes** - Boolean/Phrase
S2	(MH "Brain Injuries+")	**Expanders** - Apply equivalent subjects **Search modes** - Boolean/Phrase
S1	"Traumatic brain injury" OR "traumatic brain injuries" or TBI	**Expanders** - Apply equivalent subjects

Cochrane

#1	("Traumatic brain injury" OR "traumatic brain injuries" or TBI) (Word variations have been searched)	S	Limits 4531
#2	MeSH descriptor: [Brain Injuries] explode all trees	MeSH	2370
#3	#1 OR #2	Limits	5445
#4	("cognitive communication" OR "cognitive-communication" or "social communication" OR communicat* OR "communication skills" OR conversat* OR discourse) (Word variations have been searched)	S	Limits 29072
#5	MeSH descriptor: [Interpersonal Relations] explode all trees	MeSH	6230
#6	MeSH descriptor: [Social Communication Disorder] explode all trees	MeSH	11
#7	#4 OR #5 OR #6	Limits	33390
#8	(Intervention OR interven* OR rehabilitation OR rehab OR rehab* OR rehabilitate OR program OR programme OR training OR train* OR remediation OR remediat* OR treatment OR treat* OR therapy) (Word variations have been searched)	S	Limits 1276831
#9	MeSH descriptor: [Rehabilitation] explode all trees	MeSH	35803
#10	MeSH descriptor: [Rehabilitation Research] explode all trees	MeSH	3
#11	MeSH descriptor: [Treatment Outcome] explode all trees	MeSH	139925
#12	#8 OR #9 OR #10 OR #11	Limits	1273321
#13	("information and communications tech*" OR "information and communication tech*" OR ICT OR "video conferenc*" OR "video-conferenc*" OR video OR online OR internet OR telehealth OR telemedicine OR telerehab* OR telecare OR telecommunicat* OR "tele-health" OR "tele-medicine" OR "tele-rehab*" OR "tele-care" OR "tele-communicat*") (Word variations have been searched)	S	Limits 49900
#14	MeSH descriptor: [Telemedicine] explode all trees	MeSH	2618
#15	MeSH descriptor: [Videoconferencing] explode all trees	MeSH	207
#16	#13 OR #14 OR #15	Limits	48803
#17	#3 AND #7 AND #12 AND #16	Limits	100
#18	Type a search term or use the S or MeSH buttons to	S MeSH	Limits N/A

✖ Clear all

☐ Highlight orphan lines

PsycINFO

Search ID#	Search Terms	Search Options
S13	S3 AND S6 AND S9 AND S12	**Expanders** - Apply equivalent subjects **Search modes** - Boolean/Phrase
S12	S10 OR S11	**Expanders** - Apply equivalent subjects **Search modes** - Boolean/Phrase
S11	(MH "Telerehabilitation") OR (MH "Remote Consultation") OR (MH "Videoconferencing") OR (MH "Telehealth+") OR (MH "Telemedicine") OR (MH "Teleconferencing")	**Expanders** - Apply equivalent subjects **Search modes** - Boolean/Phrase
S10	"information and communications tech*" OR "information and communication tech*" OR ICT OR "video conferenc*" OR "video-conferenc*" OR video OR online OR internet OR telehealth OR telemedicine OR telerehab* OR telecare OR telecommunicat* OR "tele-health" OR "tele-medicine" OR "tele-rehab*" OR "tele-care" OR "tele-communicat*"	**Expanders** - Apply equivalent subjects **Search modes** - Boolean/Phrase
S9	S7 OR S8	**Expanders** - Apply equivalent subjects **Search modes** - Boolean/Phrase
S8	(MH "Rehabilitation+")	**Expanders** - Apply equivalent subjects
S7	Intervention OR interven* OR rehabilitation OR rehab OR rehab* OR rehabilitate OR program OR programme OR training OR train* OR remediation OR remediat* OR treatment OR treat* OR therapy	**Expanders** - Apply equivalent subjects **Search modes** - Boolean/Phrase
S6	S4 OR S5	**Expanders** - Apply equivalent subjects **Search modes** - Boolean/Phrase
S5	(MH "Communication Skills") OR (MH "Communication Skills Training") OR (MH "Social Skills Training")	**Expanders** - Apply equivalent subjects **Search modes** - Boolean/Phrase
S4	"cognitive communication" OR "cognitive-communication" or "social communication" OR communicat* OR "communication skills" OR conversat* OR discourse	**Expanders** - Apply equivalent subjects **Search modes** - Boolean/Phrase
S3	S1 OR S2	**Expanders** - Apply equivalent subjects **Search modes** - Boolean/Phrase
S2	(MH "Brain Injuries+")	**Expanders** - Apply equivalent subjects **Search modes** - Boolean/Phrase
S1	"Traumatic brain injury" OR "traumatic brain injuries" or TBI	**Expanders** - Apply equivalent subjects

PubMed

Term 1: TBI

Search #1: Free language keyword search:

“Traumatic brain injury” OR “traumatic brain injuries” OR TBI

Search #2: MeSH terms:

"Brain Injuries, Traumatic"[MeSH]

Search #3: Combination Free and MeSH

#1 or #2

Term 2: Communication

Search #4: Free language keyword search:

“cognitive communication” OR “cognitive-communication” OR “social communication” OR communication OR communicating OR communicate OR “communication skills” OR conversation OR conversing OR discourse

Search #5: MeSH terms:

("Social Communication Disorder"[MeSH]) OR "Interpersonal Relations"[MeSH]

Search #6: Combination Free and MeSH
#4 OR #5

Term 3: Intervention

Search #7: Free language keyword search:

intervention OR intervene OR rehabilitation OR rehabilitate OR rehab OR program OR programme OR training OR train OR remediation OR remediate OR treatment OR treat OR treating OR therapy

Search #8: MeSH terms:

(("Rehabilitation"[MeSH]) OR "Rehabilitation Research"[MeSH]) OR "Treatment Outcome"[MeSH]

Search #9: Combination Free and MeSH
#7 OR #8

Term 4: Telehealth

Search #11: Free language keyword search:

“information and communications technology” OR “information and communication technology” OR “information and communications technologies” OR “information and communication technologies” OR ICT OR “video conference” OR “video-conference” OR “video conferencing” OR “video-conferencing” OR video OR online OR internet OR telehealth OR telemedicine OR telerehabilitation OR telerehab OR telecare OR telecommunication OR telecommunications OR tele-health OR tele-medicine OR tele-rehabilitation OR tele-rehab OR tele-care OR tele-communication OR tele-communications

Search #12: MeSH terms:

("Telecommunications"[MeSH]) OR "Telemedicine"[MeSH]

Search #13: Combination Free and MeSH
#11 OR #12

Search #14: Final combination of all terms

#3 AND #6 AND #9 AND #13

Syntax for final search (Search #14)

(("Traumatic brain injury" OR "traumatic brain injuries" OR TBI) OR ("Brain Injuries, Traumatic"[MeSH])) AND (("cognitive communication" OR "cognitive-communication" OR "social communication" OR communication OR communicating OR communicate OR "communication skills" OR conversation OR conversing OR discourse) OR (("Social Communication Disorder"[MeSH]) OR "Interpersonal Relations"[MeSH])) AND ((intervention OR intervene OR rehabilitation OR rehabilitate OR rehab OR program OR programme OR training OR train OR remediation OR remediate OR treatment OR treat OR treating OR therapy) OR ((("Rehabilitation"[MeSH]) OR "Rehabilitation Research"[MeSH]) OR "Treatment Outcome"[MeSH])) AND (("information and communications technology" OR "information and communication technology" OR "information and communications technologies" OR "information and communication technologies" OR ICT OR "video conference" OR "video-conference" OR "video conferencing" OR "video-conferencing" OR video OR online OR internet OR telehealth OR telemedicine OR telerehabilitation OR telerehab OR telecare OR telecommunication OR telecommunications OR tele-health OR tele-medicine OR tele-rehabilitation OR tele-rehab OR tele-care OR tele-communication OR tele-communications) OR (("Telemedicine"[MeSH]) OR "Telecommunications"[MeSH]))

Scopus

***** Searched within article title, abstract and keywords

Search #1

(TITLE-ABS-KEY("Traumatic brain injury" OR "traumatic brain injuries" or TBI))

Search #2
AND (TITLE-ABS-KEY("cognitive communication" OR "cognitive-communication" or "social communication" OR communicat* OR "communication skills" OR conversat* OR discourse))

Search #3
AND (TITLE-ABS-KEY(Intervention OR interven* OR rehabilitation OR rehab OR rehab* OR rehabilitate OR program OR programme OR training OR train* OR remediation OR remediat* OR treatment OR treat* OR therapy))

Search #4
AND ((TITLE-ABS-KEY("information and communications tech*" OR "information and communication tech*" OR ICT OR "video conferenc*" OR "video-conferenc*" OR video OR online OR internet)) OR (TITLE-ABS-KEY(telehealth OR telemedicine OR telerehab* OR telecare OR telecommunicat* OR "tele-health" OR "tele-medicine" OR "tele-rehab*" OR "tele-care" OR "tele-communicat*")))

Final Search Syntax
(TITLE-ABS-KEY("Traumatic brain injury" OR "traumatic brain injuries" OR tbi)) AND (TITLE-ABS-KEY ("cognitive communication" OR "cognitive-communication" OR "social communication" OR communicat* OR "communication skills" OR conversat* OR discourse)) AND (TITLE-ABS-KEY(intervention OR interven* OR rehabilitation OR rehab OR rehab* OR rehabilitate OR program OR programme OR training OR train* OR remediation OR remediat* OR treatment OR treat* OR therapy)) AND ((TITLE-ABS-KEY ("information and communications tech*" OR "information and communication tech*" OR ict OR "video conferenc*" OR "video-conferenc*" OR video OR online OR internet)) OR (TITLE-ABS-KEY (telehealth OR telemedicine OR telerehab* OR telecare OR telecommunicat* OR "tele-health" OR "tele-medicine" OR "tele-rehab*" OR "tele-care" OR "tele-communicat*")))

Web of Science

* Searched within Topic fields (TS)

Term 1: TBI

- **#1**: TS= ("Traumatic brain injury" OR "traumatic brain injuries" or TBI)

Term 2: Communication

- **#2**: TS= ("cognitive communication" OR "cognitive-communication" or "social communication" OR communicat* OR "communication skills" OR conversat* OR discourse)

Term 3: Intervention

- **#3**: TS= (Intervention OR interven* OR rehabilitation OR rehab OR rehab* OR rehabilitate OR program OR programme OR training OR train* OR remediation OR remediat* OR treatment OR treat* OR therapy)

Term 4: Telehealth

- **#4**: TS= ("information and communications tech*" OR "information and communication tech*" OR ICT OR "video conferenc*" OR "video-conferenc*" OR video OR online OR internet OR telehealth OR telemedicine OR telerehab* OR telecare OR telecommunicat* OR "tele-health" OR "tele-medicine" OR "tele-rehab*" OR "tele-care" OR "tele-communicat*")

Combination →185 results

- **#5**: Combine: #1 AND #2 AND #3 AND #4

Appendix E

Methodological Quality Rating Scale for RCTs

Internal Validity

A. Inclusion and exclusion criteria were explicitly stated.

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

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

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

C. **Baseline characteristics:**
The participants in different treatment conditions should be comparable at start of treatment on important characteristics, such as demographic variables (age, sex, education), injury severity, time since injury, severity of impairment, and value of the primary outcome measure. Characteristics of both the experimental and control groups must be described to receive credit.

The reviewer may need to determine the relative importance of various baseline characteristics and may elect to give full credit on this item even if some characteristics are not equivalent at baseline.

In cases in which the outcomes between groups differ on a variable, and this variable was not equivalent at baseline, a negative rating should typically be given even if other baseline characteristics are equivalent.

If statistical comparisons are not conducted on baseline variables, the reviewer determines whether the information provided is adequate to consider the groups similar on important baseline characteristics.

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

Experimental condition:

1. The nature of the intervention is described in sufficient detail to understand how the interventions were provided (e.g., individual or group) and the methods used to promote change (eg, repetitive practice of exercises, development of compensatory strategies).
2. The total duration of treatment is provided, either in terms of length of treatment or termination criteria.
3. The intensity of treatment is provided, in terms of hours, number of sessions, frequency of sessions, and so forth.

Control Intervention

1. For no-treatment conditions (including wait-list controls), duration of the nontreatment or wait-list condition should be equivalent to duration of the experimental treatment.
2. For alternative treatment conditions, the nature of the control intervention is described as outlined for "Experimental intervention."

E. Cointerventions:

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

F. Outcome assessor blinded:

In order to receive credit, both
(1) the person conducting the outcome assessment should be unaware of the participant's treatment condition, and
(2) objective outcome measures are used, including objective neuropsychologic measures, standardized structured
interviews, or standardized clinical rating.

If only self-report by the participant is used, and the participant is aware of his/her assignment to treatment condition, no credit is given.

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

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

Descriptive Criteria

H. Withdrawal and dropout rates:

Participants included in the study but who did not complete the observation period or were not included in the analyses must be described, and reasons for withdrawal should be provided.

If the percentage of withdrawals and dropouts does not exceed 20% for short-term outcome and 30% for long-term outcome and does not lead to substantial bias, a "yes" is scored (these percentages are arbitrary and not supported by the literature).

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

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

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

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

*Adapted from Cicerone, K. D., Azulay, J., & Trott, C. (2009). Methodological quality of research on cognitive rehabilitation after traumatic brain injury. *Archives of Physical Medicine and Rehabilitation*, *90*(11), 52–59. https://doi.org/10.1016/j.apmr.2009.05.019

Appendix F

Methodological Quality Rating Scale for SCEDs

Descriptions of Items in the Single Case Experimental Design (SCED) Scale

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

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

Note. Adapted from: Tate, R. L., Mcdonald, S., Perdices, M., Togher, L., Schultz, R., & Savage, S. (2008). Rating the methodological quality of single-subject designs and *n*-of-1 trials: Introducing the Single-Case Experimental Design (SCED) Scale. *Neuropsychological Rehabilitation, 18*(4), 385–401. https://doi.org/10.1080/09602010802009201

Appendix G

QOLIBRI Questionnaire

In the first part of this questionnaire we would like to know **how satisfied** you are with different aspects of your life since your brain injury. For each question please choose the answer which is closest to how you feel now (including the past week) and mark the box with an "X". If you have problems filling out the questionnaire, please ask for help.

PART 1

A. These questions are about your thinking abilities now (including the past week).

	Not at all	Slightly	Moderately	Quite	Very
1. How satisfied are you with your ability to concentrate, for example when reading or keeping track of a conversation?					
2. How satisfied are you with your ability to express yourself and understand others in a conversation?					
3. How satisfied are you with your ability to remember everyday things, for example where you have put things?					
4. How satisfied are you with your ability to plan and work out solutions to everyday practical problems, for example what to do when you lose your keys?					
5. How satisfied are you with your ability to make decisions?					
6. How satisfied are you with your ability to find your way around?					
7. How satisfied are you with your speed of thinking?					

B. These questions are about your emotions and view of yourself now (including the past week).

	Not at all	Slightly	Moderate..	Quite	Very
1. How satisfied are you with your level of energy?					
2. How satisfied are you with your level of motivation to do things?					
3. How satisfied are you with your self-esteem, how valuable you feel?					
4. How satisfied are you with the way you look?					
5. How satisfied are you with what you have achieved since your brain injury?					
6. How satisfied are you with the way you perceive yourself?					
7. How satisfied are you with the way you see your future?					

C. These questions are about your independence and how you function in daily life now (including the past week).

	Not at all	Slightly	Moderately	Quite	Very
1. How satisfied are you with the extent of your independence from others?					
2. How satisfied are you with your ability to get out and about?					
3. How satisfied are you with your ability to carry out domestic activities, for example cooking or repairing things?					
4. How satisfied are you with your ability to run your personal finances?					
5. How satisfied are you with your participation in work or education?					
6. How satisfied are you with your participation in social and leisure activities, for example sports, hobbies, parties?					
7. How satisfied are you with the extent to which you are in charge of your own life?					

D. These questions are about your social relationships now (including the past week)

	Not at all	Slightly	Moderately	Quite	Very
1. How satisfied are you with your ability to feel affection towards others, for example your partner, family, friends?					
2. How satisfied are you with your relationships with members of your family?					
3. How satisfied are you with your relationships with your friends?					
4. How satisfied are you with your relationship with a partner or with not having a partner?					
5. How satisfied are you with your sex life?					
6. How satisfied are you with the attitudes of other people towards you?					

PART 2

In the second part we would like to know **how bothered** you feel by different problems. For each question please choose the answer which is closest to how you feel now (including the past week) and mark the box with an "X". If you have problems filling out the questionnaire, please ask for help.

E. These questions are about how bothered you are by your feelings now (including the past week).

	Not at all	Slightly	Moderately	Quite	Very
1. How bothered are you by feeling lonely, even when you are with other people?					
2. How bothered are you by feeling bored?					
3. How bothered are you by feeling anxious?					
4. How bothered are you by feeling sad or depressed?					
5. How bothered are you by feeling angry or aggressive?					

F. These questions are about how bothered you are by physical problems now (including the past week).

	Not at all	Slightly	Moderately	Quite	Very
1. How bothered are you by slowness and/or clumsiness of movement?					
2. How bothered are you by effects of any other injuries you sustained at the same time as your brain injury?					
3. How bothered are you by pain, including headaches?					
4. How bothered are you by problems with seeing or hearing?					
5. Overall, how bothered are you by the effects of your brain injury?					

Appendix H

PART-O Questionnaire

First, I am going to ask you questions about your activities in a typical week.

(P)1. In a typical week, how many hours do you spend working for money, whether in a job or self-employed?

☐

Category	Score
None	0
1-4 hours	1
5-9 hours	2
10-19 hours	3
20-34 hours	4
35 or more hours	5
Don't know/not sure/refused	9

<Same categories used for next questions>

(P)2. In a typical week, how many hours do you spend in school working toward a degree or in an accredited technical training program, including hours in class and studying?

☐

(P)3. In a typical week, how many hours do you spend in active homemaking, including cleaning, cooking and raising children?

☐

Now, I will ask you about how often you do several other activities.

(S)4. In a typical week, how many times do you socialize with friends, in person or by phone? Please do not include socializing with family members.

☐

Category	Score
None	0
1-4 times	1
5-9 times	2
10-19 times	3
20-34 times	4
35 or more times	5

<Same categories used for next questions>

(S)5. In a typical week, how many times do you socialize with family and relatives, in person or by phone?

☐

(S)6. In a typical week, how many times do you give emotional support to other people, that is, listen to their problems or help them with their troubles?

☐

(S)7. In a typical week, how many times do you use the Internet for communication, such as for e-mail, visiting chat rooms or instant messaging?

☐

(O)8. In a typical week, <u>how many days</u> do you get out of your house and go somewhere? It could be anywhere – it doesn't have to be anyplace "special".

☐

Category	Score
None	0
1-2 days	1.25
3-4 days	2.50
5-6 days	3.75
7 days	5
Don't know/not sure/refused	9

<u>Now I have questions on how often you do various activities in a typical month...</u>

(O)9. In a typical month, how many times do you eat in a restaurant?

☐

Category	Score
None	0
1-4 times	1
5-9 times	2
10-19 times	3
20-34 times	4
35 or more times	5

<Same categories used for next questions>

(O)10. In a typical month, how many times do you go shopping? Include grocery shopping, as well as shopping for household necessities, or just for fun.

☐

(O)11. In a typical month, how many times do you engage in sports or exercise outside your home? Include activities like running, bowling, going to the gym, swimming, walking for exercise and the like.

☐

The next questions also ask about activities in a typical month, but the answer categories are different.

(O)12. In a typical month, how many times do you go to the movies?

☐

Category	Score
None	0
1 time	1
2 times	2
3 times	3
4 times	4
5 or more times	5
Don't know/not sure/refused	9

<Same categories used for next questions>

(O)13. In a typical month, how many times do you attend sports events in person, as a spectator?

☐

(O)14. In a typical month, how many times do you attend religious or spiritual services? Include places like churches, temples and mosques.

☐

(S)15. Switching, now, to a somewhat different kind of question... Do you live with your spouse or significant other? (If YES, skip to PART Question 17;, if NO, ask Question 16 ; When Item 15 is Yes, assign score of 5 to both Q15 and Q16, for a total of 10 points. This results in assignment of 10 points for living with spouse, 5 points for being in an intimate, non-marital relationship, and 0 points if neither is applicable.)

☐

Category	Score
No	0
Yes	5
Don't know/not sure/refused	9

<Same categories used for next questions>

(S)16. Are you currently involved in an ongoing intimate, that is, romantic or sexual, relationship?

☐

(S)17. [Not including your spouse or significant other], do you have a close friend in whom you confide?

☐