

ESPS PEER-REVIEW REPORT

Name of journal: World Journal of Psychiatry

ESPS manuscript NO: 23186

Title: Review of key telepsychiatry outcomes

Reviewer's code: 02445191

Reviewer's country: Canada

Science editor: Shui Qiu

Date sent for review: 2015-11-03 15:31

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CLASSIFICATION	LANGUAGE EVALUATION	SCIENTIFIC MISCONDUCT	CONCLUSION
<input type="checkbox"/> Grade A: Excellent	<input type="checkbox"/> Grade A: Priority publishing	Google Search:	<input type="checkbox"/> Accept
<input type="checkbox"/> Grade B: Very good	<input type="checkbox"/> Grade B: Minor language polishing	<input type="checkbox"/> The same title	<input type="checkbox"/> High priority for publication
<input type="checkbox"/> Grade C: Good	<input type="checkbox"/> Grade C: A great deal of language polishing	<input type="checkbox"/> Duplicate publication	<input type="checkbox"/> Rejection
<input type="checkbox"/> Grade D: Fair	<input type="checkbox"/> Grade D: Rejected	<input type="checkbox"/> Plagiarism	<input type="checkbox"/> Minor revision
<input type="checkbox"/> Grade E: Poor		<input type="checkbox"/> No	<input type="checkbox"/> Major revision
		BPG Search:	
		<input type="checkbox"/> The same title	
		<input type="checkbox"/> Duplicate publication	
		<input type="checkbox"/> Plagiarism	
		<input type="checkbox"/> No	

COMMENTS TO AUTHORS

Systematic Review of Key Telepsychiatry Outcomes Sam Hurley et al Given that most people with mental health problems do not receive any help, there is tremendous need for innovations in health care delivery. Telepsychiatry clearly has marked potential to address the existing mental health disparities in both developed and developing countries. It can access effectively many patients in rural areas as well as those who otherwise have limited access to care. The authors conducted a systematic review of the telepsychiatry literature, nearly 2000 studies. They identified patterns of findings related to satisfaction, reliability of clinical assessment, treatment and implementation outcomes, cost-effectiveness and legal issues. They found that, overall, patients and providers are generally satisfied with telepsychiatry services and telepsychiatry is a cost-effective approach that is comparable to face-to-face services in terms of reliability of clinical assessments and treatment outcomes. The patient response to self-report questionnaires, qualitative interviews, and mixed-method assessment indicated that they are comfortable using this technology, appreciate the practical benefit of avoiding travel, and are less concerned than providers about potentially adverse impacts of telepsychiatry on the therapeutic alliance. In terms of care quality, the evidence reviewed



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suggested that telepsychiatry is comparable to face-to-face in the reliability of assessment and effective treatment of a range of behavioral and mental health disorders. Treatment outcomes appeared statistically equivalent. Furthermore the authors reviewed literature focusing on practical factors related to the implementation of telepsychiatry such as adaptability of telepsychiatry to routine care settings, cost-effectiveness, and legal issues. Telepsychiatry also appeared comparable to FTF in terms of service utilization patterns and cost-effectiveness. I think this an important review article on one of the critical issues about the delivery of psychiatric care. Provides a practically important message about the value of telepsychiatry. Very well, clearly written. Careful, critical analysis of the literature in its full extent. Although some condensation may be possible, I would publish the manuscript in its present form, without any changes. Particularly valuable are the authors' recommendations for future studies. They recommenda to shift focus from establishing equivalent reliability between telepsychiatry and FTF assessments to identifying which types of assessments are most amenable to the telepsychiatry, which types of assessments are most difficult to administer via telepsychiatry, and which types of adaptations help improve telepsychiatry assessments.

ESPS PEER-REVIEW REPORT

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CLASSIFICATION	LANGUAGE EVALUATION	SCIENTIFIC MISCONDUCT	CONCLUSION
<input type="checkbox"/> Grade A: Excellent	<input type="checkbox"/> Grade A: Priority publishing	Google Search:	<input type="checkbox"/> Accept
<input type="checkbox"/> Grade B: Very good	<input checked="" type="checkbox"/> Grade B: Minor language polishing	<input type="checkbox"/> The same title	<input type="checkbox"/> High priority for publication
<input type="checkbox"/> Grade C: Good		<input type="checkbox"/> Duplicate publication	
<input type="checkbox"/> Grade D: Fair	<input type="checkbox"/> Grade C: A great deal of language polishing	<input type="checkbox"/> Plagiarism	<input checked="" type="checkbox"/> Rejection
<input checked="" type="checkbox"/> Grade E: Poor	<input type="checkbox"/> Grade D: Rejected	<input checked="" type="checkbox"/> No	<input type="checkbox"/> Minor revision
		BPG Search:	<input type="checkbox"/> Major revision
		<input type="checkbox"/> The same title	
		<input type="checkbox"/> Duplicate publication	
		<input type="checkbox"/> Plagiarism	
		<input checked="" type="checkbox"/> No	

COMMENTS TO AUTHORS

The topic of this article is relevant to contemporary mental health care, and written clearly. However it has multiple aims and in trying to answer them all it unfortunately does none of them well. This is not a systematic review conducted to rigorous standards - there is no quality assessment of the evidence and therefore bias in findings and synthesis cannot be ruled out. I list some key points that would need addressing below: 1. The article is focused solely on video-conferencing, not telepsychiatry as a whole - this should be made clear in the title and in the abstract early on. 2. The abstract is not informative from the point of view of a systematic review. at a minimum it should include the number of databases searched. the number of included articles (733) is high and not in agreement with the number reported in the main article. 3. Given that there is no transparency in data quality, the authors cannot conclude there is a robust evidence base. At most they demonstrate the size of the relevant literature but not much more. Such conclusions and core tips need to be tempered. 4. Inclusion criteria are not well justified - why was a date limit of 2000 set and what impact might this have on the findings of the review. 5. Evidence of treatment effect was limited to adequately powered RCTs. Power is not the only marker of quality in RCT design. what about the



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other objectives/ It seems no restrictions were placed on study design for these which gravely undermines the synthesis and conclusions drawn. Greater definition of the categories of interest is need i.e. what constitutes an implementation outcome? how does a legal issue differ from a n ethical one, and which are eligible for this review? 6. the number of included studies is not clear and appears to be presented by different criteria-topic categories are intermingled with study design. A PRISMA flow chart would be a welcome addition to help readers navigate through the text. 7. Why are review papers and non-data based outputs e.g. commentaries and program descriptions included? 8. Findings are synthesised in the absence of rigorous quality appraisal. There is no reference to sample size in relation to the satisfaction studies, or study/population content or the validity of the tools used. Similar limitations occur in the section relating to implementation and legal issues. No distinction is drawn between qualitative and quantitative research or the relative positioning of these in their ability to answer the question(s) being addressed. Risk of bias in the studies reporting treatment effects is not considered. This limits the utility of this article to no more than that of a narrative, selective review. 9. The method of synthesising the literature and its presentation does not warrant the strength of the conclusions drawn in the first paragraph of the discussion. These need tempering to avoid misleading readers. 10. The authors distinguish between quantitative and experimental designs mid way through their discussion - the two are not mutually exclusive and so this statement needs revision.



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CLASSIFICATION	LANGUAGE EVALUATION	SCIENTIFIC MISCONDUCT	CONCLUSION
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		<input type="checkbox"/> Plagiarism	
		<input checked="" type="checkbox"/> No	

COMMENTS TO AUTHORS

An excellent job in covering the literature. Provides an excellent resource. Perhaps I missed it but it would be useful to provide a geographic picture of where the telepsych literature originates: USA? Canada? Europe? Australia/NZ? Africa/Asia? It may be beyond the scope of the article but it might be interesting if there was some discussion about telepsych/telemedicine as a function of the health care delivery system of different countries, eg, USA vs Canada vs UK vs France, Germany, etc. Also for the discussion might be some speculation about the how the new telepsychiatry companies that are popping up all over the internet might change the practice of telepsych for practitioners and patients over the next few years.