Has mental health harnessed the digital revolution?

In their editorial, Hollis et al. focus on the third of three digital revolutions – access to real-time patient data (‘connected health’) – but also highlight the benefits of the first two revolutions – unlocking value in electronic medical records and new forms of access that allow patients direct control.

How far have the first two digital revolutions embedded benefits for patient care in mental health? The first revolution (word-processing, from the 1960s) allowed people with little training to prepare, edit and duplicate high-quality documents. The second comes with the internet: ability to access, transmit, share and edit these documents. From these two revolutions, what can patients (and carers and professionals) expect as the outputs of the mental health IT system?

A basic expectation is that, before every face-to-face clinical encounter, the IT system can easily deliver an accurate background history, account of recent treatment, and up-to-date care plan. (This list could be extended to include a summary history, complete history, and non-stigmatising vulnerability and risk history.) The safety of these documents is assured by accessibility (so that they are used and reviewed often) and accuracy (confirmed by patients and carers). They must be easily readable, to be safely understood, and actually used (as unread documents do not convey information).

The quality of these ‘output’ documents must convey respect for patients, carers and professionals and the interactions between them. For patients and carers, documents summarising core parts of their present or past lives must carry real-world acceptability in appearance and structure. Clinical staff able to take pride in their documentation (being clear, respectful, accurate and useful) will welcome sharing them with patients, carers and other professionals. Finally (as in the ordinary world) the IT system should save time for professionals (and patients and carers), freeing up treatment time.

Simple IT technology can deliver this for professionals, patients and carers. Hollis et al. note that mental health patients’ use of technology is similar to the general UK population, with three-quarters of adults accessing the internet daily, half via a smartphone. I have only anecdotal knowledge of how far mental health IT is delivering the benefits of the first two revolutions.

Hollis et al. summarise key challenges for the third revolution in connected health:

1. Ensuring that patients and their needs remain at the centre of technology development and implementation.
2. Rapidly increasing the evidence base for the clinical effectiveness of digital technology.
3. Ensuring that the opportunity provided by data sharing between patients, carers and clinicians does not threaten privacy and undermine public trust. Finally, patients, clinicians and NHS commissioners require an agreed framework to evaluate the core features of new technologies including usability, content, safety, clinical- and cost-effectiveness.

These still apply with equal force to the first two digital revolutions.

Selective reporting of results in guidelines

Taylor and Perera argue persuasively that the 2014 National Institute for Health and Care Excellence (NICE) schizophrenia guideline promotes cognitive-behavioural therapy (CBT) and other psychosocial interventions beyond the evidence. Its conclusions with respect to CBT also seem open to another charge, that of selective reporting: the highlighting of favourable results while unfavourable ones are suppressed.

In its evidence summary (p. 232), NICE states that ‘when compared with standard care, CBT was effective in reducing hospitalisation rates up to 18 months following the end of treatment’. NICE actually examined rehospitalisation rates in three of the large series (more than 100) of meta-analyses they carried out (data available at www.nccmh.org.uk). One of these compared CBT with standard care at up to 18 months and found a significant effect (5 trials, 910 patients, relative risk (RR) 0.76, 95% CI 0.61–0.94). Another compared CBT with standard care at 2–4 years and failed to find a significant advantage (2 trials, 513 patients, RR 0.82, 95% CI 0.64–1.05). The third meta-analysis compared CBT with ‘other active treatments’ (which consisted in all but one case of putatively inactive control interventions such as befriending and supportive counselling) at up to 2 years; this was again non-significant (5 trials, 506 patients, RR 1.07, 95% CI 0.86–1.33). The findings of the two negative meta-analyses are not mentioned in the NICE guideline. Neither does NICE mention that CBT was not found to be effective against relapse when compared with either standard care (3 trials, 460 patients, RR 0.85, 95% CI 0.50–1.41) or other active treatments (4 trials, 416 patients, RR 1.05, 95% CI 0.85–1.30). This omission is difficult to understand given the obvious relationship between relapse and rehospitalisation.

NICE goes on to state that ‘CBT was shown to be effective in reducing symptom severity as measured by total scores on items, such as the PANSS and BPRS, at both end of treatment and at up to 12 months’ follow-up’. This was the case in the comparison between CBT and standard care, where there was a significant effect for CBT at the end of treatment (13 trials, 1356 patients, standardised mean difference (SMD) 0.27, 95% CI 0.45 to 0.10), as well as in meta-analyses of 6- and 12-month follow-up data. However, the findings were non-significant in the comparisons between CBT and ‘other active treatments’ both at end of treatment (6 trials, 396 patients, SMD 0.13, 95% CI 0.32 to 0.07) and at all follow-up points. Once again, NICE conveys an impression of uniform evidence of effectiveness against symptoms, whereas the reality is that an entire subset of pre-planned meta-analyses gave negative results.

Selective reporting arises when authors fail to publish data altogether, or when they arbitrarily decide which analyses and results to report in a publication. The NICE 2014 recommendations...
for CBT seem to be an example of the latter practice being applied to the results of multiple meta-analyses.


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Authors’ reply: We thank Dr McKenna (and colleagues) for his interest in our editorial, and respect his long record of research into schizophrenia. His point about the authors of influential national clinical guidelines such as NICE, the British Association for Psychopharmacology (BAP) and the Scottish Intercollegiate Guidelines Network (SIGN) needing to take negative evidence into account is well made, and analogous to the ALLtrials movement in pharmacoepidemiology. Schizophrenia is such a common and potentially devastating illness that it is incumbent on mental health professionals such as psychologists and psychiatrists to work together to deliver best-evidenced treatments.

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Does previous experience of antidepressants form the expectations necessary for a placebo response?

Leuchter et al’s findings extend the current understanding of the placebo response and raise important questions regarding the design of antidepressant trials. An important finding was that expectation of medication effectiveness predicted treatment response in the placebo group only, which suggests that expectations of treatment benefit are required for a placebo response.

It is thought that the placebo response results from an interaction between expectations and learning. In studies of placebo analgesia, experimental paradigms often involve a conditioning procedure to induce an expectation of benefit from treatment. One widely used paradigm involves thermal pain stimulation and application of an inert cream. Following application of the cream, the thermal energy is reduced to non-painful levels to condition the participant to believe the cream has analgesic properties. Subsequently, laser stimulation continues at painful levels, and participants report the stimulation as less painful. The implication is that an expectation of analgesia, induced by exposure to the cream’s ‘analgesic’ properties, results in a placebo response. Learning to expect an effect has also been shown to influence emotional processing. Petrovic et al measured responses to aversive pictures in healthy volunteers following administration of placebo ‘anxiolytic’ medication and its reversal, and found that participants reported aversive pictures as less distressing when they believed they had received anxiolytic medication, and more distressing when they believed this had been reversed. This result shows that a learned expectation, induced through exposure to a medication, can cause changes in emotional processing.

In the study reported by Leuchter et al, there was a relationship between expectation of benefit and treatment response in the placebo group. However, these patients did not undergo a conditioning procedure to induce an expectation of benefit. What caused these patients to expect a benefit? Could the therapeutic environment and consent process for starting an antidepressant engender a powerful expectation of benefit on its own? Or does this expectation come from previous experience of benefit from antidepressant treatment? The data from this study suggest the latter, as the expectations seemed to be formed at the time of enrolment. We could perhaps answer this question more fully through assessment of the relationship between previous response to antidepressant treatment and placebo response in this trial. It is possible that more patients in the placebo group had previously benefitted from treatment than in the medication group, and if this were so, it would lend support to the idea that previous experience of benefit from antidepressant treatment could cause a placebo antidepressant response. This could be an important consideration in future antidepressant drug trials.

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Authors’ reply: Huneke & Baldwin raise important points regarding the interpretation of our study results and the relationship of our findings to the broader placebo literature. It is challenging to compare the results from our study with the literature cited by them. As they note, studies of placebo analgesia generally are performed in healthy volunteers not being treated for a chronic illness. Such studies examine the placebo effect, namely the response of naturally occurring symptoms of a chronic illness (in this case major depressive disorder, or MDD) within the context of a clinical trial. Because patients with MDD have long courses of illness and treatment, they commonly enter treatment studies with pre-existing expectations and beliefs, and our participants had indeed formed expectations about medications at the time of study enrolment. We concluded that these expectations were probably formed by factors external to the study, and speculated on the role that
external social factors (such as direct-to-consumer advertising) might have had in forming positive medication expectations.

Huneke & Baldwin raise the point that our participants’ medication expectations, which predicted placebo response, may have been formed, at least in part, by the consent process and initial exposure to the study environment. Participants consented and had their introduction to study personnel prior to rating their expectations of improvement. Although we do not know to what extent medication expectations might have been influenced by this initial exposure, a significant effect is unlikely. Participants rated the degree to which they expected that treatment in general, and medication in particular, would be helpful in relieving their depression. If participants’ initial exposure to the study milieu shaped expectations, it would be expected to influence both medication and general treatment expectations. Yet, only participants’ ratings of medication expectations predicted response to placebo. The selective relationship between medication, but not general, treatment expectations and placebo outcome suggests the influence of a process outside of the study milieu.

We agree with Huneke & Baldwin that it would be instructive to learn more about participants’ previous experiences with antidepressant treatment and how this might affect current medication expectations, as well as the likelihood of placebo response. In this regard, we recently examined the potential role of prior antidepressant treatment and placebo treatment response in these same participants. Self-report data collected from a subset of participants from the parent study revealed that previous experience with antidepressant medication was significantly associated with poorer response to placebo. Interestingly, among those who had received prior antidepressant treatment, their self-report of response to prior treatment was not significantly related to expectations in the current trial or to placebo outcome. This finding suggests that antidepressant-experienced participants may show classic conditioning effects, consistent with our previously reported findings. The finding that prior antidepressant exposure, regardless of response, predicts placebo outcome is worthy of future study.


Methodological considerations in determining the effects of films with suicidal content

We read carefully the article by Till et al., which focuses on a laboratory experiment to determine the effect of films with suicidal content. This important issue has been largely unexplored in terms of research bringing to bear on practice. The study is well conceptualised and the scales and questionnaires used are highly suitable, especially in terms of internal consistency and targeting study population. However, we would still like to highlight a few limitations of the study.

1 The unpredictable nature of suicide in participants with no or low suicidality is of major concern, especially for ethical reasons.
2 Either obtaining a detailed clinical history (medical and psychiatric) and mental state examination by mental health professionals or using a screening instrument like the Composite International Diagnostic Interview” prior to the laboratory experiment would have helped in ruling out other psychiatric disorders as a part of exclusion criteria and would have served the purpose adequately.
3 The possibility of unreliable responses among participants with ongoing psychotic illnesses like schizophrenia in all the scales cannot be ruled out completely.
4 The Erlanger Depression Scale3 consists of 9 statements on a printed form with 5 possible answers ranging from ‘accurate’ to ‘not true’, and has been wrongly described as having 8 items rated on a scale from 0 (completely wrong) to 4 (exactly right).
5 The Reasons for Living Scale,4 which has 72 items, has been wrongly described as having 48 items. It is only in the revised scale that 24 out of 72 items were dropped because of ambiguous factor loading.
7 Other factors like camera positioning,6 audio quality, lighting, and special effects studied for stimulating cue-induced craving in substance use disorders, have a qualitative role in predicting outcome and not only how the film ends.

Authors’ reply: Regarding the questionnaires discussed by Drs Jha & Kumar, we want to clarify some statements regarding some of the measures of our study. As they correctly pointed out, the Erlanger Depression Scale1 consists of 9 items, but only 8 of these items are used to calculate the score for depression, and the first item of the scale is a ‘warm-up’ item used for introduction to the scale. Further, the 48-item scale by Linehan and colleagues2 is commonly referred to as the Reasons for Living Inventory, even though earlier versions of this scale may exist.

We agree that factors other than the outcome of the suicidal crisis portrayed in the films (e.g. camera positioning, audio quality, lighting, special effects) might have determined the impact.
of the movies on the audiences, and this was discussed in our paper. Further studies are necessary to determine the effect of movies that do not differ with regard to characteristics other than the crisis outcome.

Regarding the screening process, ethical considerations and the safety of participants are of course a main priority. Therefore, we excluded individuals with high depression or suicidality scores from participation in the study and offered psychological counselling to them and to all participants after the screening, to help them cope with any distress they may have experienced due to exposure to the films or answering questions on suicidality. The screening process was approved by the Ethics Committee of the Medical University of Vienna and the Vienna General Hospital (study protocol 942/2011, date 24/11/2011). Of note, there is no evidence of general harmful effects of answering questions on suicidality among depressed patients or the general population. Obtaining a detailed clinical history or examining the mental state with screening instruments such as the Composite International Diagnostic Interview, as suggested by Jah & Kumar, would have further increased the participants’ time spent on completing questionnaires, which may have resulted in negative consequences on participation. It is also important to note that suicidal ideation scores among study participants with baseline suicidality above the median who watched the suicide film were still considerably lower after the film screening than suicidal ideation scores of individuals with a history of suicidal ideation or parasuicide in previous studies (e.g. Linehan et al). We also checked for incoherent responses during the screening process in order to identify potentially unreliable responses. There were no contradictory or inconsistent responses in the questionnaires, and there were no indicators of psychotic illnesses among the participants during briefing and debriefing of the study, which were both conducted by a psychologist (B.T.).

1 Lehrl S, Gallwitz A. Erlanger Depression Scale (EDS) [in German]. Vless, 1983.

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