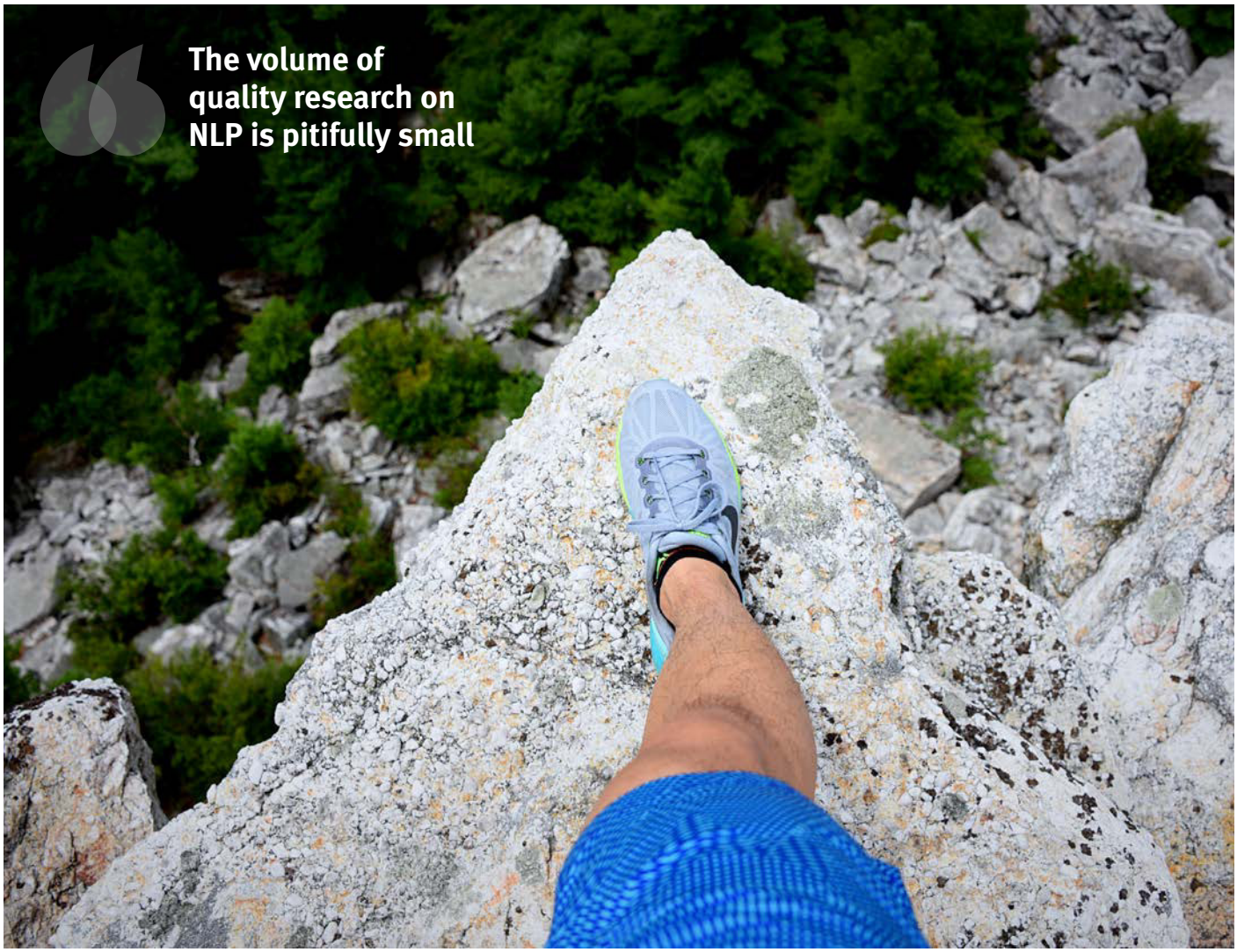
NLP research, equipoise and reviewer prejudice


By Professor Bruce Arroll and Associate Professor Suzanne Henwood



This article is being offered in the 'Provoking Debate' section of *Rapport* to share the experience of two senior academics attempting to publish NLP research in the medical field.

The article explores some of the apparent prejudices which appear to be present, restricting the dissemination of NLP results, raising serious questions for the NLP community which is struggling to establish and share its research activity.

We know that the volume of quality research on NLP is pitifully small. While there are many studies (*1) there appear to be only 5 randomised controlled trials (*2) in a 2012 review of studies and only one of these reported a benefit. Interestingly, the authors made the comment that the use of NLP should only be done in the context of a research study. That, of course, will not meet the approval of NLP practitioners.

An additional search of psych information from 2013 to 2017 found no references to NLP and random (to elicit the clinical trials). Wikipedia, while not quality assured, is very visible and states: 'The balance of scientific evidence reveals NLP to be a largely discredited pseudoscience. Scientific reviews show it contains numerous factual errors, and fails to produce the results asserted by Bandler and Grinder.' The strongest case for it being not effective comes from the Sturt review which examined the 5 randomised controlled studies and a number of before and after studies (where a group is measured at baseline, have an intervention and a further measure at the end of the study – there can also be a control group covering

the same period of time). However the rigour of that systematic review has also been questioned: … not only has limited research been done but little rebuttal has been offered regarding the negative findings in the literature which were often the result of seriously flawed research designs based on inadequate training in and understanding of the NLP material under study … The unpacking of 35 years of research, publications, interpretations, conclusions and meta-analyses of an ill-defined 'NLP field' is presented in Gray et al, chapter 8. (*1)

To the NLP practitioner the techniques can seem very powerful and effective. The trap with that is that there may be other explanations for the effectiveness.

Outlined here are our experiences with the rapid phobia cure. It could be argued that the fact that the client has turned up for therapy (and potential exposure) is enough to explain the benefit that some people clearly get. It could also be that the empathy of the therapist could induce confidence in the client thereby bringing about a reduction in the fear. Therapist empathy and merely turning up for treatment is a powerful intervention and this is known as confounding. There may also be unknown confounders that are happening in the client's life that make the difference especially with 'soft outcomes' which is what subjective symptoms are. It is clear then that to deal with the issue of confounders a randomised controlled trial is needed as this ensures such confounders are equally distributed between the intervention and the control groups. Thus, if there is a difference at the end of the study (that is statistically significant) it is possible to conclude that the difference (benefit) was due to the intervention.

In an attempt to remedy this situation we planned and conducted a randomised controlled trial of the rapid phobia cure for people with a fear of heights. (*3) We attempted to design the perfect trial. It was registered with the clinical trials registry, randomisation was concealed and done through a remote computer, participants were not told what they were not getting, and the final questionnaire was emailed back to the study to ensure that the information was elicited blind to the study personnel. The control group was a 15-minute meditation and hence much more active than a waiting list control. Participants were also asked to sign what we called

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a certificate of validation stating that the interviewers had not indicated that the treatment that participant got was any better than the one they did not get. We did not do a behavioural activation test which is considered the gold standard for phobia studies as we were using four recruitment sites and it was difficult to find an outside set of stairs to test the phobic state. To compensate for this we used a new questionnaire called the Heights Interpretation Questionnaire (HIQ) as it had been validated against actual height situations. (*4, 5) It has a range from 16 to 80. The results have been presented at the Society of Academic Primary Care conference Dublin 2016 as a poster. The findings were 98 (92%) returned their questionnaire and were included in the intention to treat analysis (this is, as far as we can ascertain, the largest study ever undertaken on fear of heights). The proportion of participants with an HIQ score of less than 26 (low fear of heights) at the end of the study was 34.6% (18/52) in the intervention group and 15.2% (7/46) in the control group and this was statistically significant ($p=0.028$), showing the phobia cure is an effective option for treating fear of heights.

What was interesting was the response we got when we sent the paper in for potential publication in a number of credible, high quality journals.

The following are comments from the first review by the first medical journal: 'It seems that the authors themselves did the intervention and also the control treatment. Thus it was not blinded and performed by doctors with a possible special interest and preference. This is a major problem and could indeed disqualify the study. It is not solved by the "certificate" of validation as it concerns the actual delivery of the intervention.'

It appears that the reviewer is implying at best advocacy at worst fraud. It seems to us that all researchers have a special interest and preference (and are advocates at some level) and the task is to design a study to minimise those criticisms. We appealed the review and were turned down on the review.

We submitted it to a second medical journal and this is one of the comments: 'The authors make reference to the "controversial" nature of NLP in the background and objectives and uncertainty about the evidence base. Since NLP has been used by doctors since the 1970s, the paucity of credible evidence should act as the loudest of alarm bells. Has NLP been systematically ignored by researchers or (and perhaps far more likely), are file drawers stuffed full of studies that have not been published due to statistically non-significant findings?

'The attempt in this manuscript to apply a randomised control trial design is to be applauded. However, the case for why "NLP" should warrant our attention after 40 years of failing to produce any evidence is not established. As such, I would strip away any reference to "NLP" and focus purely on calling the intervention what it actually is – a visualisation technique.'

We think most of this comment is simple prejudice and speculation and ignores our attempts to do good science. There appears to be no comment offered on the rigour and quality of research design or its subsequent application, which is surprising.

Another reviewer wrote: 'NLP is unfortunate and unnecessary baggage for this research and for its potential application by clinicians in primary care. The fact that some of us old GPs might have learnt some NLP 45 years ago and persist in using selected bits of it is not a sufficient argument for basing a clinical intervention on this theoretical model. Furthermore, the NLP model has been so broadly criticised that clinicians are either not aware of it or are likely biased against it. The report does not describe the training of the intervention clinicians enough to know if the NLP model is necessary or sufficient to provide this treatment.

'What seems clear is that the intervention described in the study is a therapy that has been in the mainstream of psychology and primary care interventions since before NLP came on the scene. It looks like pretty classic systematic desensitisation therapy. The use of colour images is a bit of a twist and the running-the-film-backward feature may be novel. There is nothing described here to ▶

suggest that the intervention requires the theoretical underpinning of NLP. Furthermore, the study design does not allow us to learn if any particular element of the intervention-related NLP contributes to its success. Therefore, the NLP infrastructure of the intervention – although it may be intriguing to the authors – creates unfortunate, unnecessary and unscientific baggage for the design of the intervention or interpretation of the results of this study.'

We do not think that it is necessary in the first randomised controlled trial to try and ascertain which element is the effective part of the intervention. It is first of all necessary to show the full intervention works and after that tease out the components.

And finally: 'It feels like the theoretical basis of the intervention does not hang together with the practical implications for practice. Would the authors be able to scrap the entire NLP discussion and simply give an operational description of the intervention they offered? Would such a move be acceptable to the authors and intellectually honest for their research?'

This last point we have decided to follow, which is sad for NLP. We have referenced the method to a classic description of the rapid phobia cure described by Lewis Walker but have removed references to NLP. (*6) The reviewer does ask us about being intellectually honest about the research. For us it is a balance of either overcoming the prejudice and not getting published or getting the study published in a high quality journal. The rapid phobia cure is a form of exposure therapy (or desensitisation therapy) to a feared situation that is relevant to the individual client and done from a very safe place – in the movie theatre and up in the projection box.

Our feeling about all this is one of frustration.

We do not know if there are 'many randomised trials' with negative results sitting in researchers' drawers. The most likely situation is that there is an absence of evidence rather than evidence of absence. In our view there is equipoise about the effectiveness of the rapid phobia cure. We feel we have designed a very tight study which is the result of 30 years of doing research into unblinded therapies. We think we have got that right. What we were not expecting is the antagonism to NLP and the speculation as to what or what has not been researched previously.

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Our view is that NLP developed as a therapeutic intervention without much research as it seemed so effective that doing research would not help. Also practitioners are not usually trained in research and it requires people living in the research world to conduct high quality research. Our view now is that therapists / practitioners should not do research unless in tandem with experienced researchers or ensure they have extensive research training prior to doing research alone, to ensure the best possible chance of publication of results.

Our experience makes us wonder whether this 'prejudice' against NLP is an issue solely within the medical community (the two journals were medical journals). We do not think so and we may have had a worse reception with the psychological community where we encounter verbal comments such as 'that was disproved 30 years ago'.

This is in contrast to the development of Acceptance and Commitment Therapy which was started by research psychologists in the 1980s and only used as therapy in the 1990s. (*7)

The key thing with research is that as you would not do NLP untrained so research is a special discipline that also needs training. What NLP practitioners can do if they wish to get into research is talk with local academics / researchers, e.g. in psychology, psychiatry or family practice departments, and then start collecting audit / pilot data. If they can show some good outcomes this could be the basis of a research project and the ultimate study is a randomised controlled trial. More low quality NLP research is not going to help the cause.

As a footnote: since writing this article, our paper has been accepted and can be reported as 'in press 2017'; B Arroll, et al. 'A brief treatment for fear of heights: a randomised controlled trial of a novel imaginal intervention'. ●

References

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(*6) L Walker. *Changing with NLP – a casebook of neurolinguistic programming.*
(*7) SC Hayes. 'Acceptance and commitment therapy, relational frame theory, and the third wave of behavioural and cognitive therapies'.

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Information on a small randomised medical trial of the NLP Movie rewind Phobia Process, done in New Zealand.

The intervention was done as a single 15 minute session (designed for use by doctors). Ninety-eight participants (92%) returned their questionnaire and were included in the intention to treat analysis. The HIQ (Heights Interpretation Questionnaire) score <26 was 34.6% (18/52) in the intervention group and 15.2% (7/46) in the control group RR = 2.26, 95% CI (1.05, 4.95) and p = 0.028. The numbers needed to treat is six 95% CI (3 to 36). Participants with scores <26 report their fear of heights is very much improved. There was a 4.5-point difference in the HIQ score at eight weeks (p = 0.055) on the multiple regression analysis.

http://www.conference.co.nz/files/docs/gp17/sample%20research%20presentation.pdf
http://journals.sagepub.com/doi/full/10.1177/0091217417703285

The two doctors running the study will do as much as they can to get the message out there. The process, by the way, was done as a single 15 minute session able to be delivered by doctors (rather than a full NLP session), and randomly compared to a single 15 minute session of a guided meditation, with the clients not knowing whether their process was the "real" one or not. This is an attempt to distinguish the NLP process from simple placebo response. Half of clients (52) got the NLP process, and half (46) got the placebo. On the Heights Interpretation Questionnaire the percentage of clients reporting 8 weeks later that their fear of heights is "very much improved" was 34.6% in the NLP group and 15.2% in the control group.

Presentation Paper to Conference

IS THE NEUROLINGUISTIC PROGRAMMING INTERVENTION RAPID PHOBIA CURE EFFECTIVE FOR FEAR OF HEIGHTS? A RANDOMIZED CONTROLLED TRIAL (RCT)

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Aim: The evidence base for Neurolinguistics Programming (NLP) therapies is limited and the therapies are controversial. We wished to evaluate the effectiveness of the rapid phobia cure (RPC), a popular NLP technique, in an RCT. The RPC has had no clinical trial evaluation.

Conference theme addressed: Clinical management. It deals with equity by offering a rapid treatment for phobias.

Methods: Participants needed a score of >29 on the Heights Interpretation Questionnaire (HIQ). The design was an RCT with concealed randomisation and blinded to other participants' intervention. The intervention was a single "rapid phobia cure" session or a control group with a 15 minute meditation. The outcome was the proportion of participants with a score <26 on the HIQ at 8 weeks on an emailed questionnaire to maintain blinding. The intervention requires the participants, in their imagination, to run a movie forward, in black and white, of a fearful experience and then backward in colour. They do this three times or more until the fear of heights is diminished.

Results: 98 (92%) returned their questionnaire and were included in the intention to treat analysis. The proportion of participants with an HIQ score < 26 was 34.6% (18/52) in the intervention group and 15.2% (7/46) in the control group RR=2.26, 95% CI (1.05, 4.95) and (p=0.028). The number needed to treat was six 95% CI (3 to 36). The intervention takes less than 15 minutes in actual practice.

Conclusions: This is the first RCT of the rapid phobia treatment. It is brief, easily learnt, safe, low cost and probably effective.