

**PR7101 Assignment by Kate Kemp (for a Masters Degree programme at Bath Spa University.**

A written critique of:

Huppert, F. and Johnson, D. (2010) A controlled trial of mindfulness training in schools: the importance of practice for an impact on well-being. Journal of Positive Psychology, Volume 5, Issue 4, 264-274

**Introduction**

Mindfulness is a meditation-based practice which has been used by the medical profession for the last 20 years or so to help people suffering from stress, anxiety, depression and chronic pain. Initially used with adults, there have been recent developments in using mindfulness training with adolescents in clinical settings. Mindfulness training teaches the ability to recognise, and eventually, feel in control of habitual, negative ways of thinking.

I am the Special Needs Coordinator (SENCO) at a special school for students aged 4-16 with social, emotional and behaviour difficulties (the Link). In the summer of 2011 a group of staff from the school, including myself, undertook an 8 week mindfulness training course. This was prerequisite training before we could then teach mindfulness techniques to our students.

Whilst undertaking the mindfulness course myself and reading what I could find about the subject I came across this article and was interested in what the authors had to say about the use of mindfulness training to improve the well-being of adolescents in a school rather than clinical setting.

My interest in mindfulness is part of my wider interest in the development of emotional self-regulation (Blakemore and Frith, 2005) and I intend the focus of my dissertation to be an exploration of the most effective ways of improving the emotional self-regulation of the students with whom I work. By emotional self-regulation I mean the ability to recognise and manage ones feelings and emotions.

The research question the writers of this article posed was whether training in mindfulness can enhance the well-being of school children. They define well-being as ‘feeling good and functioning well’ and discuss ‘the growing acceptance that schools should not only provide children with formal education but should also consider the well-being of the child as a whole’ (p.265). They contrast programmes whose outcomes are intended to reduce negative symptoms –eg bullying- with programmes whose purpose is to enhance positive symptoms-eg feeling good about oneself in order to explain why they have chosen to examine the effects of mindfulness training.

This article was published in the Journal of Positive Psychology and the authors place their research in the context of the theory and practice of positive psychology. They use Seligman’s (2002) definition of positive psychology as being the ‘scientific understanding and promotion of what makes life go well’ (p.265).

This paper is referenced on the Mindfulness in Schools Project website (2011). Comments and evaluation from the pupils involved in the study are given in considerable detail on the website but are not included in the paper itself. I agree with Lewis and Porter (2007) when they say that there are many reasons for involving children and young people in the process of research, most significantly ‘a belief that research will be better or more meaningful and have greater validity in revealing children’s views and experiences’ and also a belief in ‘the importance of democratic participation’. I will return to the omission of the pupils’ voice shortly.

In this report the authors comment on what they describe as ‘anecdotal or qualitative information’ from previous studies into the effects of mindfulness training with children and make it very clear that in this research they particularly wanted to use quantitative evaluation measures as well as qualitative measures in order to provide a ‘convincing demonstration of the benefits of the mindfulness training’. Their research can therefore be evaluated in the light of whether they have provided this ‘convincing’ demonstration or not.

### **Assumptions**

In using quantitative evaluation measures the authors are making the ontological assumption that mindfulness is the kind of phenomenon that can be measured and the epistemological assumption that it is possible to measure how mindful an individual might have become and compare that person with another person.

Whilst I accept that it is perfectly possible to say that one child is twice as tall as another child (by measuring them), I do not think it is possible to say that one child is twice as mindful as another. This is what Ryle (1949) calls a ‘category mistake’ whereby a property is ascribed to something which could not possibly have that property. The idea that mindfulness is the kind of quality that can be measured on an interval or ratio scale-and hence be studied using parametric statistics- is a ‘category mistake’.

Whilst the authors have made every effort in their statistical analysis to account for variables there was no consideration given to the relationship between the teachers who delivered the training and the students nor indeed of that between the teachers of the ‘control groups’ and their students.

The stance of the researchers would seem to be that mindfulness is a technique that can be lifted off the shelf, as it were, and applied to students regardless of the context in which it is being delivered and more importantly the personality, integrity and authenticity of the deliverer.

In using pre and post intervention questionnaires concerning such factors as ‘well-being’, ‘resilience’ and ‘personality’ the researchers seem to hold the ontological assumption that people, and in this case, adolescents, will answer questions about themselves in a reliable, honest and consistent way.

A further ontological assumption they appear to hold is that the respondents to the questionnaires will have a common understanding of what the questions/statements

mean. The statement in the well-being scale (WEMWBS; Tennant et al., 2007)) ‘I’ve been feeling close to other people’ might, for example, be particularly confusing for a student on the ASD spectrum without further clarification.

## **Methodology**

The methodological decision which the researchers made was to use a controlled, experimental design with statistical analysis of data in order to examine the relationship between mindfulness and well-being. Another way to describe this would be to say that they have decided to use quantitative methods within the positivist experimental paradigm to collect and present information about qualitative subjects.

The design used is that of pre-test-post-test control and experimental group design as described in Cohen, Manion and Morrison (2007).

The researchers started out with the clear hypothesis that engagement in a mindfulness programme will improve students’ well-being and this, together with the methods used, places the research clearly within the positivist paradigm. In calling the paper ‘a controlled trial’ it is clear that they wish it to be seen as having scientific credentials. In addition whilst they view results from previous studies where ‘anecdotal or qualitative information is a valuable starting point’ they clearly believe that measuring the effects of the training in a quantifiable manner is not only possible but will be more ‘convincing’.

However I would agree with Eisner (1988) when he says:

‘What I have even more quarrel with is the view that a scientifically acceptable research method is ‘objective’ or ‘value-free’, that it harbours no particular point of view. All methods and all forms of representation are partial, they limit as well as illuminate what through them we are able to experience’ (p.19)

As mentioned previously it is evident that a large amount of qualitative data was gained during the course of the research but not analysed or included in this study. It seems curious that the researchers used a ‘multi-method approach’ to collect their data (as advocated by Cohen et al, 2000) but had not then chosen to triangulate their findings (Hitchcock and Hughes, 1995). This, I believe, would have added to the validity of their results.

Much of the data which the researchers analysed was collected from on-line questionnaires thus suggesting that they were keen to avoid interviewer-bias. Participants in the mindfulness groups were also asked a series of questions about the course itself for example how helpful had they found the course and would they continue to practice. It is not clear who asked these questions and how open-ended the questions were as they are not included in the paper. However the answers to one of these questions - ‘how often did you practice outside of the course?’ - have been used to support their principal finding from the research which was that participant improvements in mindfulness and psychological well-being were related to the amount of individual practice undertaken outside the course.

In their attempt to provide ‘a convincing demonstration of the benefits’ the researchers have tried to pin down all variables between the control and intervention groups pre and post questionnaires. As Hammersley and Atkinson (1995) say:

‘the most important feature of scientific theories is that they are open to and are subjected to test: they can be confirmed, or at least falsified, with certainty. This requires the exercise of control over variables.....Without any control over variables, it is argued, one can do no more than speculate about causal relationships, since no basis for testing hypothesis is available’ (p5).

In attempting to exercise control over variables the researchers have taken the decision not to include any variables regarding the relationship between teachers and students and the delivery of the mindfulness intervention itself. In my view, therefore, it is only possible to ‘speculate’ about the relationship between mindfulness and well-being in this context.

### **Data Collection**

The research was carried out with fourteen and fifteen year old boys from two independent boys’ schools. A total of n=173 students took part in the study from eleven religious education classes. Six of these classes were taught the mindfulness programme by one or other of their usual religious education teachers. These two teachers were experienced mindfulness practitioners. At the same time the five other classes (the control groups) were being taught their usual religious studies lessons by other teachers.

The participants in both the control and intervention groups completed on-line questionnaires relating to mindfulness awareness, resilience, well-being and personality before and after the delivery of the mindfulness programme to the intervention group.

Short questionnaires were chosen in order to make sure the students completed them. The questionnaires all used scaling systems and the authors describe in depth the reliability and validity of all of them from previous studies.

In addition the pupils who had undertaken the mindfulness course were asked, on completion, a number of other questions relating to their experience of the course. As stated earlier it is not clear who asked these questions or how they were asked but the answers from them are reported in tabular form at the end of the paper. Other comments of a qualitative nature appear, as mentioned earlier, on the Mindfulness in Schools Project website. However these are not referred to in this paper.

The data collected is entirely self-reported i.e. the only data used is that acquired from the students themselves by questionnaire. There was no attempt made for example to compare any self-perceived changes with changes reported by teacher or parents. This sort of comparison (or triangulation) is commonly used in assessing behaviour issues and diagnoses (for ASD and ADHD for example) and could have provided more objective measurements of the effects of the mindfulness programme if that was what was required.

## **Data analysis**

Baseline data from all students was analysed using analysis of variance (ANOVA; Tabachnick, 1996) in order to determine if there were any significant differences between the control and experimental groups and, according to the researchers, none were found.

In addition the baseline scores from the personality questionnaire were compared with those from the mindfulness, resilience and well-being questionnaires using multiple regression analysis in order to determine pre-existing associations between personality and the other outcome measures.

Following the teaching of the mindfulness course both sets of students' answers from the pre-intervention questionnaires of both groups were compared with the post-intervention questionnaires. This would have been a large amount of data to manipulate as they were looking at the relationship between the answers from four questionnaires measuring differing aspects of well-being and personality from both the control and intervention group and then pre and post intervention. A significant number of variables come into play and the authors go into great depth to explain how they have analysed the data which is presented in both written and tabular form.

In addition the researchers appear to have decided that a certain level of practice outside the actual teaching of the course may have been a further variable and so the results from the students who had participated in the course were further analysed using this additional data. As mentioned earlier it is not clear where the information about the amount of practice outside of the course came from.

The researchers report that their main finding from the data analysis was that increase in mindfulness and well-being measures correlated with the amount of practice undertaken outside of the classroom i.e. those students who had practiced mindfulness techniques outside of the course were more likely to show improved psychological well-being. This, they stated, correlated with findings from previous adult studies where the greater the amount of home practice the greater the evident benefits.

A further finding which they highlight is that the students who had rated themselves as more anxious and less emotionally stable showed greater benefit from the mindfulness training.

They did not find a significant overall group difference between the intervention group and control group and account for this by the shortness of this intervention in comparison with more usual mindfulness courses.

## **Other issues**

The researchers report that the parents of the students in both the mindfulness and control groups were informed about the study and, I assume, were asked for their

consent as this was refused by one parent and another expressed reservations. These two students were not included in the study.

No other reference is made to ethical considerations. For example the assumption is made that teaching mindfulness to the students will be of benefit to them, or at least will do them no harm although this is implicit by their citation of the many previous studies cited where the benefits of learning mindfulness techniques had been observed.

In the discussion toward the end of the article the authors outline some of the limitations they recognise including the fact that the control classes had different teachers to those in the mindfulness classes. They say that 'observed benefits may have been in part related to differences between teachers'. It would seem to me that there are always going to be differences between teachers and wonder how it would be possible to quantify the effect of the relationship between a teacher and his or her students in relation to the teaching of any particular subject.

Researcher effects were however minimised by the use of on-line questionnaires rather than being delivered in person. As pointed out earlier there were some follow up questions asked and it is not clear in what context this took place. Indeed it is not clear whether the researchers had any direct contact with either the students or the teachers.

Other limitations they recognise are the lack of random allocation to intervention and control groups, the fact that all the participants were boys and the fact that the control group were not engaged in any home study-the factor which they identify as significant in the effectiveness of the mindfulness programme.

The possibility that any improvements in the self-reported well-being of the mindfulness group could be as a result of them having the special attention of being involved in the programme, the so-called Hawthorne effect (as described by Cook (1962) for example), is not discussed. In using a positivist paradigm the researchers may have been trying to account for the Hawthorne effect by remaining as distant and uninvolved as possible although the Hawthorne effect can conversely help to validate research conducted in an interpretivist or participatory paradigm (Coombs and Smith, 2003)

The researchers do not specifically address the fact that the research was carried out in two independent schools and the possible social class bias that this might have produced (Hitchcock and Hughes, 1995).

## **Conclusions**

Hitchcock and Hughes (1995) define social research as the 'collection and analysis of information on the social world, in order to understand and explain that world better'. (p5). It is clear that this article is not aiming to explain to the lay person or help anyone not conversant with statistical analysis of data understand the effects of mindfulness training.

The researchers stated that their aim was to provide a ‘convincing demonstration of the benefits of the mindfulness training’. They do not make clear who it is they are wishing to convince.

Whilst I appreciate the efforts of the researchers to try and ‘prove’ the effects of mindfulness training with adolescents I think that the methodology they have employed is limited. Having collected both qualitative and quantitative data they have chosen to focus on the latter at the expense of showing ‘the richness and complexity of human behaviour’ and consequently ‘demonstrating concurrent validity’ (Cohen et al, 2000).

My view would be that the inclusion of the qualitative information gained from the participants and their teachers would have added considerably to this paper and, for me, made it much more ‘convincing’.

My dissertation will focus on improving my own practice and will therefore lie clearly within the interpretivist paradigm. As a result of carrying out this critique I will now be particularly aware of issues of validity and the benefits of a mixed or multi-method approach to collecting data. For me what is paramount is that, as Harré (1993) says, researchers (including myself) should ‘develop methodologies commensurate with the nature of the phenomenon they are studying’ (*cited in* Coombs and Smith, 2003).

**Word count: 2944**

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