Contents Issue 60

Editorial

Welcome to the 60th (yay!) issue of the PsyPAG Quarterly. Thank you to all our contributors, who have ensured that this issue is packed with really interesting articles.

In this issue, we have David Moore's first column as chair, in which he reviews the recently held PsyPAG Annual Conference, and gives us an insight into his vision for the future of PsyPAG. On the PsyPAG theme, this issue also features some personal reflections by Prof. Mark Griffiths on PsyPAG in the 1980s. In this motivational article, Prof. Griffiths reflects on the way his active involvement as a PsyPAG committee member has helped him succeed in his academic career from a very early stage, and highlights the ways in which postgraduate students can make use of the many opportunities offered by PsyPAG.

On a more evangelical note: remember that if you are a postgraduate psychology student in the U.K., you are automatically a member of PsyPAG – so why not make use of it? And if reading the article by Prof. Griffiths inspires you to become involved, the next opportunity is just around the corner – the PsyPAG committee is calling for proposals from anyone wishing to host the next PsyPAG conference. You don't have to be a committee member – all you need is willingness and a good proposal. In this issue, Rachel Pye reflects on her own experience of hosting this year's PsyPAG conference, and offers some excellent advice and motivation for anyone interested in taking on this responsibility for 2007. If you're considering submitting a proposal, or even if you just want to read a good story, check out Rachel's piece in the current issue. For anyone who would like more details, feel free to contact Rachel at vicechair@psypag.co.uk

In addition to the PsyPAG running theme, a large section in this issue is dedicated to methodology. First, we have Dr. Andy Field's 'Bluffer's guide to meta-analysis', a highly useful (and entertaining!) article for anyone needing a helping hand through the meta-analytic jungle, as well as an invaluable resource to those seeking to refine their knowledge on the matter and who might one day embark on the meta-analytic journey themselves. Next, we

have a thought-provoking piece by Dr. Paul Morris, in which he argues that we need to interpret the significance of our results by considering their real-world implications, rather than simply comparing them against some arbitrary effect size taken out of our statistics textbooks. Concluding the methodology section, John McAlaney reviews 'Mixing Methodologies in Psychology', an edited book exploring the often artificial and largely detrimental division psychologists generally draw between qualitative and quantitative methods, and the ways in which these approaches can be usefully combined in research practice.

Also in this edition, Dimitrios Xenias introduces us to the puzzling aspects of facial behaviour and presents the pros and cons of different theoretical models underpinning research in this area by reviewing his doctoral work on social context influences; and Vicky Lovett tells us of her fantastic experience attending the International Association of Behavior Analysis Conference in Beijing. On the conference note, please have a look at our website (www.psypag.co.uk) for details of application procedures and deadlines for the PsyPAG conference bursaries.

The submission deadline for the next Quarterly is 7th October. There are guidelines for contributors on the inside front cover, and Prof. Mark Griffiths gives some highly useful tips of getting your written message across in this issue. If you are interested in writing an article, contact us by emailing quarterly@psypag.co.uk

Best wishes,
Alexa Ispas.

On behalf of the Quarterly Editorial Team, 2005-2006
Cornelia Ho, Alexa Ispas, James Jackson, Faith Martin,
David Moore and Glenda Pennington.

The PsyPAG Quarterly team would like to express their gratitude to our sponsors, The British Psychological Society.

Chair's Column

David Moore, Sheffield Hallam University

This is the first time I am writing my column as PsyPAG chair and in the wake of our annual conference there is much to say. Taking over as chair comes at a time of great change with many of our members leaving to finish writing up their theses or dissertations, and still more who are no longer postgraduates as well as a new group joining us. This time of transition is met with mixed emotion; we are excited to be joined by new friends with new and stimulating ideas, but sad to be left by those who have contributed a great deal to the group and whose opinions we have come to greatly respect and rely on. In the same vein this transitional period within the group provides the perfect opportunity to enact genuine change and I hope as you read on you will agree that there are a number of interesting plans in the pipeline.

It would be irresponsible of me to write this article without first acknowledging all of the out going committee, the people who have worked so hard to make PsyPAG what it is today. I would like to offer special thanks to Rachel Pye, the contributions she has made while occupying the post of vice chair by helping to organise so many of our activities and meetings have been second to none and we are very happy that she will staying on as cognitive representative and for her continuing advice. I also acknowledge my predecessor Cedric Ginestet, in his time as chair he has helped PsyPAG grow from a small collection of individuals to a large organisation with the capacity for real influence, he too will be missed. Although we are losing a number of representatives all who have contributed a exceptional amount on time and energy one in particular deserves a special mention, Althea Valentine has been a devoted PsyPAGGer for over 4 years now and in that time has helped to organise countless events and has served in almost every role she feasibly could. In addition to this not only was she heavily involved in the writing of our constitution but also seems to have memorised this giving us a continual reference source whenever we're not sure what course to take. Thea, you will be missed and on behalf of the committee we wish you all the best for the future.

PsyPAG has recently held its 21st annual conference at the University of Reading. There were two parallel sessions which showed the exceptional range of topics being examined by the next generation of psychological researchers. With the focus on early career development PsyPAG invited three keynote speakers who although young and fresh faced out of their PhDs are already well respected and influential not only within their own fields but more broadly within the discipline. On the fist day we were treated to a review of research looking at local and global processing in individuals with Williams syndrome by Emily Farran from the University of Reading; Andy Field then ended the second day by presenting us with a comprehensive and systematic series of studies providing evidence for his model on how fear and phobias develop in children; on the final morning, Michelle Ryan's presented us with the idea that as women are starting to break through the 'glass ceiling' in industry, they are faced with a 'glass cliff' which causes them to get more high risk senior posts with failure almost guaranteed. This final talk was enough to get almost every remaining delegate into the lecture theatre, no matter how serious their hangover (you know who you are!).

In addition we were once again treated to presentations reflecting the best of what the postgraduate community has to offer, which provoked many discussions, growing friendships and possible future collaborations. Three presentations deserve a special mention: Katherine Liversidge of the University of Edinburgh and Meghna Singhal of the University of Reading shared the runners up prizes for their talks on how music can alleviate pain during movement and the nature of mother-child relationships in children with anxiety disorders respectively. The committee had little difficulty in deciding to award the first prize to Tannaze Tinati of Southampton University for her presentation on the use of emotional face targets in the attentional blink task. All of the prize winners have been asked to write feature articles for the Quarterly.

In an attempt to use this event to the greatest benefit of all those presenting, the PsyPAG committee had two of it members who were at each talk fill out 'feedback' forms about each presentation. Delegates were then able to speak to the committee members about their presentations. This was

a first for PsyPAG, and possibly any conference, and proved to be very popular with delegates.

I feel my first column should look forwards more than backwards and so I would like to welcome all those individuals who have joined the committee at this year's AGM and to welcome back so many of the people who I have got to know in the past 12 months. I look forward to working with you all. Although many of the posts were filled at the AGM, if you check the back page of the Quarterly you will see that we still have positions available. If you think you might be interested in any these please contact me on chair@psypag.co.uk or Julie Freeborn our information officer on info@psypag.co.uk.

As I have said I wish to look towards the future of the group and this forum gives me the opportunity to tell all the postgraduates who read our publication what I see as the direction of the organisation. As a group we now have more representatives than ever before, ranging across all the sub-disciplines of psychology, as well as general national postgraduate committees and international psychological organisations. As such we are in a perfect position to inform postgraduates of relevant opportunities this is something we need to do much more successfully. To achieve this, from the next issue of the Quarterly onwards there will be a short section of representatives reports where the people within each area will be asked to write a few sentences about what is happening in their sub-discipline, so watch this space for more developments. We are also trying to make better contact with university departments, so that more postgraduates will know about our events and bursaries. This will hopefully encourage even more postgraduates to attend our events. We also hope that more postgraduates will be available on the e-mail system to offer assistance with your questions as well as hopefully providing links to other people conducting similar research

It is also very important that with our committee the size that it is we restructure to ensure that we are as efficient as possible in all of our activities. It is because of this that we are currently undergoing a review of how best to access the knowledge of all those on the committee. We are also looking to connect with postgraduates more successfully than in the past and offer more events which are dealing with the issues that you would like to know about. If

you have something you would like to see PsyPAG offer please send me your suggestion and we will do everything we can to organise things which will help you in your postgraduate study.

I would like to again thank the PsyPAG committee past and present for electing me as their, and your, chair. I hope that over the next two years I will prove to be successful in this role and an asset to the psychological postgraduate community.

Social context influences on facial behaviour
Dimitrios Xenias, Cardiff University

Facial behaviour has fascinated people since antiquity. The study of facial behaviour – which is considered to be the richest channel of non verbal communication (Tubbs & Moss, 2003) - can be seen as part of the study of nonverbal communication; believed to be the cradle of all forms of social cognition (Rochat & Striano, 1999) – which in turn can be seen as part of social and cognitive psychology (see Fig. 1).
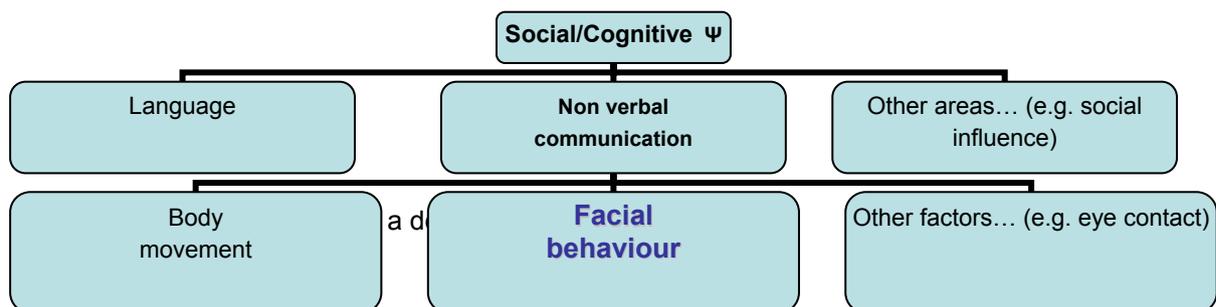

Figure 1. Facial behaviour as a domain in Psychological "space".

The power and omnipresence of an interaction between emotion and social context was evident to civilisations at least as far back as ancient Greece. For example, Aristotle believed that certain emotions could be

9

evoked in situations in which they were not only elicited by a stimulus, but also *required* (Aristotle, book VII in Peters, 1886). Moreover, a link between emotions and bodily symptoms was also established (Aristotle, trans. 1907).

Even by the 19th century, the systematic study of facial expression faced several challenges and important setbacks; e.g. *physiognomy* (the belief that specific personal traits are reflected in facial features) and *theocratic beliefs* (the view that expressive mechanisms were given to humans in order to manifest God-given passions of the soul). More recent accounts on the matter were conceived around the second half of the 19th century, when the French neurologist Duchenne de Boulogne published a study of facial musculature and of several facial expressions (Duchenne, 1862). Later, Darwin (1872) published his influential account on emotional expressions of humans and animals and at least partly used Duchenne's work. According to Fridlund,(1994), he even borrowed some of Duchenne's original photos, and never returned them(!). Darwin's view continues to influence two major views on facial behaviour, the Neurocultural model (Ekman, 1989) and Behavioural Ecology (Fridlund, 1994).

Nowadays, it is believed that facial muscle movements transmit more information than any other single body system, and that expressions are so powerful that they can influence others as well as our relationships to them, without even touching or talking to them (Smith & Scott, 1997). There also seems to be agreement on the notion that facial expressions preceded speech during the course of evolution, and that they communicate crucial information about danger, food, dominance and so on (see Fridlund, 1994).

The latter forms the basis for the Behavioural Ecology View on facial expression (Fridlund, 1994, 1997). This view claims that the face expresses our motives and intentions, but also that the face does not show anything relevant to emotions because emotions, in the ordinary sense of the word, do not exist; alleged links to "emotions" only reflect labels signifying what to do and "how to feel" in a specific situation (Fridlund, 1994). Instead, the face signifies what the person intends to do, and for the same reason, all expressions are considered genuine. For instance, a "felt" smile usually signifies "willingness to interact", while a "fake" smile would show readiness to appease, none of which is more genuine than the other (Fridlund, 2002).

Fridlund (1994) named this model a "one-factor" model, contrasting the "two-factor" neurocultural model, where two factors are responsible for facial displays (underlying emotion and social context rules – see below).

Facial display

Social Motivation/Context ⟶ ☺ ☹

Figure 2: Scheme representing the behavioural ecology model.

Behavioural Ecology attacked the Neurocultural model for facial expressions – dominant in the 1970's and 1980's (Ekman & Friesen, 1971; Ekman, 1989). The latter proposes a one-to-one correspondence of emotions to particular displays (e.g. Ekman & Friesen, 1971) springing from neurological mechanisms, or *Facial Affect Programmes*; in addition, other mechanisms, imposed by society, known as *display rules* (Ekman, 1989), regulate these expressions and dictate who can show what to whom and when. For example, a "genuine" smile supposedly shows happiness, but if it happens to occur during a funeral, then it is inhibited by a display rule that dictates that we are not supposed to laugh during funerals.

Social Context filter
(Display Rules)   **Facial display**

Emotion ⟶ 🚫 ⟶ ☺ ☹
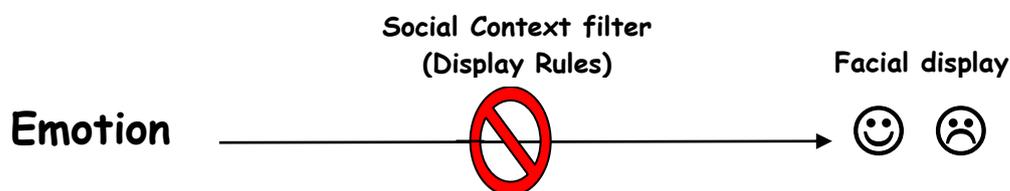
Figure 3: Scheme representing the Neurocultural model.

Several studies (Jakobs, Manstead & Fischer, 2001; Kappas, Bherer, & Thériault, 2000; Manstead, Fischer & Jakobs, 1999; Hess, Banse & Kappas, 1995) failed to support Behavioural Ecology since there still was minimal facial activity when told to inhibit it, and the activity related to the nature of stimuli. Therefore, not only was there activity despite social context demands, but also the activity was relevant to the emotional reaction expected relevant to the stimuli. Behavioural Ecology would explain this as "over-learned

reaction" – yet it is not clear how could we "over-learn" facial muscle activity as a response to specific stimuli, to the extent of automaticity.

On the other hand, many studies have also found evidence against the Neurocultural model (e.g. reviews by Russell, 1994, 1997; Fernandez-Dols & Ruiz-Belda, 1997), on methodological as well as conceptual issues. For instance, concerning the former, Russell (1994) criticised the use of forced-choice methodology, and the fact that Ekman may have induced cross-cultural contamination in some of his studies. Fridlund (1994) has repeatedly criticised the one-to-one correspondence between facial displays and any underlying "emotions"; Fernandez-Dols & Ruiz-Belda (1997) consider the social context better related to facial expression than "emotions". To complicate matters, studies that attempt to elicit spontaneous facial displays by mood induction may be flawed, as according to Smith (1993) no emotion arises in a situation where nothing is truly at stake for the participant. Similarly, Parkinson (2005) has criticised the inefficiency of laboratory manipulations to induce emotions.

In any case, there is no evidence to suggest that any facial actions which can be linked to an affective status cannot also be produced voluntarily. Thus, the focus should not be on what the "emotional contribution" to facial activity is, but to what extent social motivation alone can manipulate facial displays. In other words, the right question is: "What are the limits of the behavioural ecology view of facial displays?".

My own research findings do not support either model. In particular, in one experiment participants were asked to pose six facial expressions for a fellow participant (friend or stranger) who had to guess what these displays were. The muscular activity of the sending participant was recorded by facial electromyography (EMG) on two facial muscles (Corrugator Supercilii, involved in frowning, and Zygomaticus Major, involved in smiling) and according to Fridlund and Cacioppo's (1986) guidelines for facial EMG recording. No significant differences were found for pairs comprised of friends, compared to the pairs of strangers, for either muscle site.
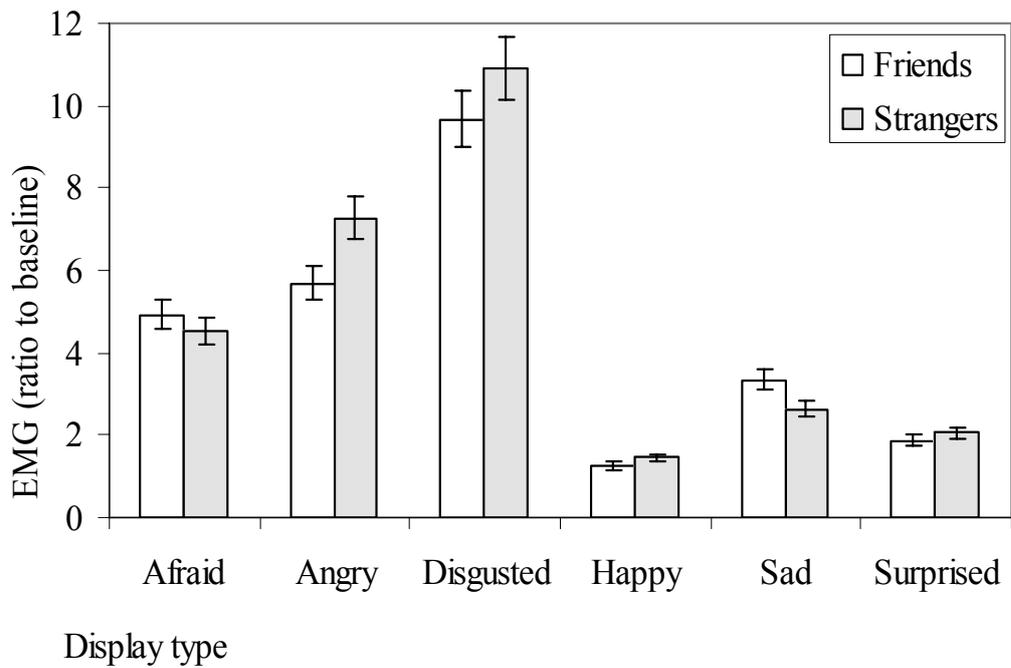
Figure 4. EMG activity on the Corrugator Supercilii site for six display types in two social context types. Error bars represent the standard error of the mean.

However, friends and strangers differed significantly at an earlier moment in the experiment, specifically at the moment when they first saw their interaction partner. At that moment, friends' EMG activity at both sites was significantly higher than strangers' (see fig 5). What exactly was responsible for this difference is not clear, and further studies would need to clarify this.
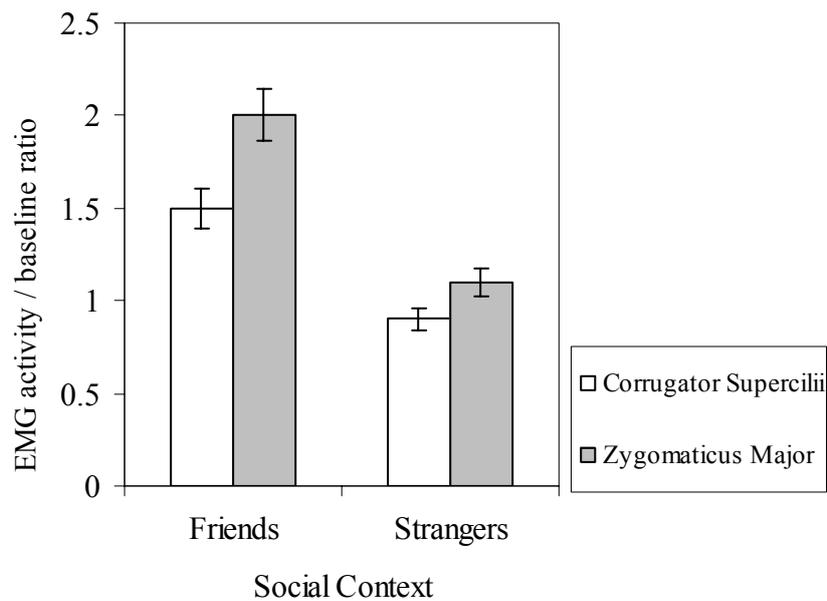
Figure 5. EMG activity on the Corrugator Supercilii and Zygomaticus Major sites at spontaneous moment. Error bars represent the standard error of the mean.

However, at minimum, this result boosted confidence in the null result for posed activity, by showing that during the spontaneous moment there was "something" going on that was absent during the posed expression moments. That "something" could be either emotion or social motivation (i.e. either being happy to see the friend, or showing they want to interact with them). However, in the latter case it seems strange that the motivation to interact with the same person would disappear during the experiment, while it could make sense to hypothesize that an emotion of "happiness" for seeing a friend was present at first, but then subdued and was not replaced by other emotions given the artificiality of the posed expression task. Such explanation would favour a mixed contribution or "two-factor" approach to facial activity, rather than a "one-factor" model such as Behavioural Ecology.

Moreover, in later studies, videos of the above interactions were shown to judges naïve to the original experiment. Friends' and strangers' videos were judged differently by these independent viewers, for several parameters such as clarity, genuineness, intensity of display, and response latency. Almost all of the above measures differed for social context *and* display type. It is not the purpose of this paper to present these findings in detail; however it is important to note that there was no consistent pattern of judgement for any particular display or context type. On the contrary, although significant, each interaction between context and display type was unique. This suggests the facial signal may be more complex than previously thought - as well as that it only bears a small correlation with the actual muscular intensity.

In addition, the neurocultural model has been criticised for circularity of arguments (see Kappas, 1996; Parkinson, 2005) while behavioural ecology has been criticised for over-inclusiveness of facial expressions (Parkinson, 2005). In accordance with the proposals of Jakobs et al., (2001); Kappas et al., (2000); Manstead, et al., (1999) and Hess et al., (1995), it is suggested that "one-" or "two- factor" views may not adequately capture the parameters involved in facial expression. Rather, the notion that a combination of several

factors, subject to contextual influences, result in facial behaviour may be a more plausible explanation of what really takes place.

A recent model attempting to overcome the above problems and incorporate more than one or two "factors" involved in facial expression is the "Superlens" model of social context and cultural conventions on facial activity (Kappas & Descôteaux, 2003). In this model (see fig. 6), redundant sending channels represent communicative aspects that are sent but not received (e.g. when the encoder sends a message which is not decoded); while redundant decoding channels represent information perceived but never actually sent (e.g. when the receiver reads an intention "into" the sender, when in fact it was irrelevant to the sender).
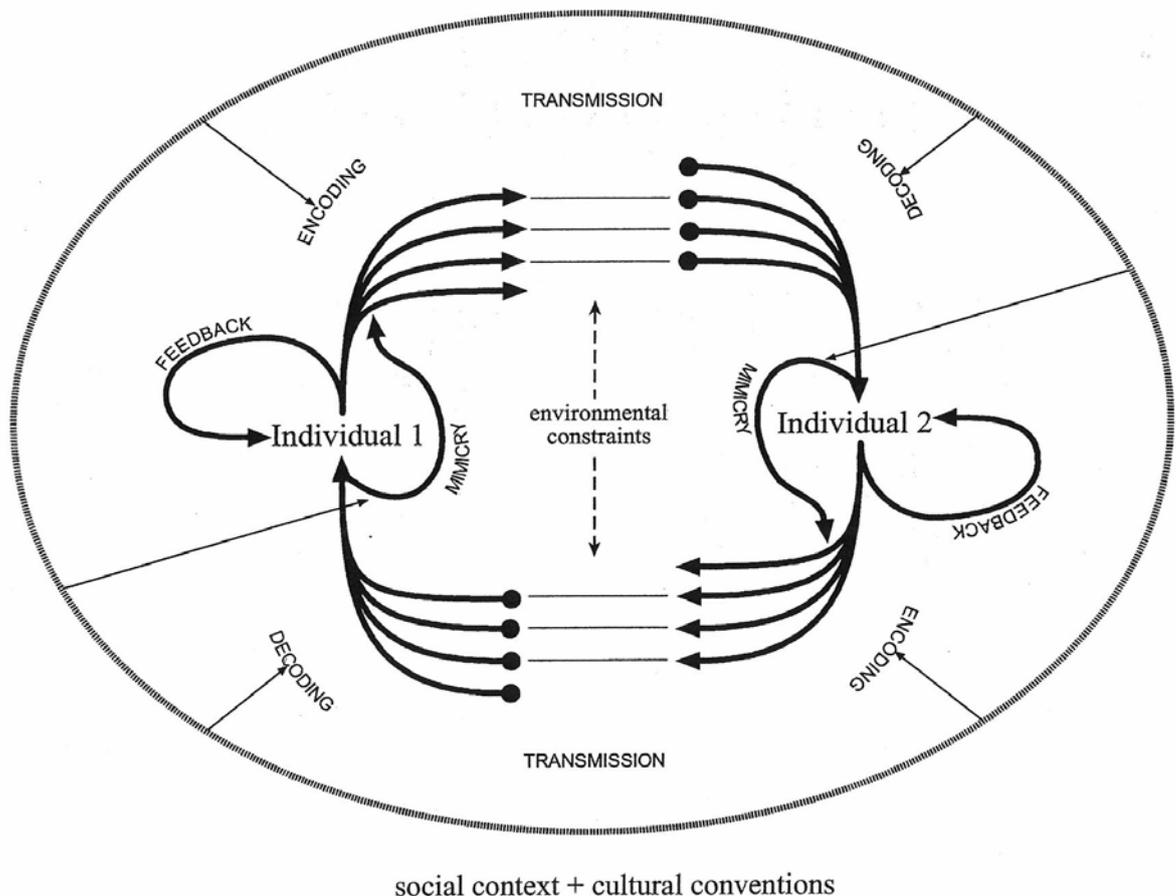

Figure 6. The Superlens model of facial expression and cultural conventions (Kappas & Descôteaux,
2003).

All these factors should be taken into account in order to capture the complexity of the ongoing processes of encoding and decoding facial expressions. Thus, facial activity is seen as the output of parallel streams of determinants, - including non-emotional factors - that are coupled and decoupled as a function of context.

Although currently under development, this model seems more promising for the task of describing the interaction of facial activity and social context. Moreover, given initial conditions, this model predicts specific outcomes. The inherent ability to predict outcomes renders this model less vulnerable to circular arguments or post-hoc explanations. Further clarifications and refinements will allow this model to transcend to a more applied status, providing a viable alternative to hitherto dominant but perhaps less adequate models.

Endnote

The work presented in this paper is based on the author's doctoral research at the Psychology Department of the University of Hull. The author wishes to thank PsyPAG for supporting his participation to the 14th General Meeting of the European Association of Experimental Social Psychology, in Würzburg, Germany, where parts of the above findings were presented.

References

Aristotle (1886). *Nicomachean ethics* (F. H. Peters, Trans.). London: Kegan Paul, Trench & co.

Aristotle (trans. 1907). *De Anima* (R.D. Hicks, Trans.). Cambridge: University Press.

Duchenne de Boulogne, C.B. (1862/1990). *The Mechanism of Human Facial Expression.* (edited and translated by R. Andrew Cuthbertson) Cambridge: Cambridge University Press.

Darwin, C.R. (1872 / 1921). *The expression of the emotions in man and animals.* London: Murray.

Ekman, P. (1989). The argument and evidence about universals in facial expressions of emotion. In H.Wagner & A.Manstead (Eds.), *Handbook of Social Psychophysiology.* (pp.143-164). London: John Wiley.

Ekman, P. & Friesen, W.V. (1971). Constants across cultures in the face and

emotion. *Journal of Personality and Social Psychology*, *17*, 124-129.

Fernández-Dols, J.M. & Ruiz-Belda, M.A. (1997). Spontaneous facial behaviour during intense emotional episodes: Artistic truth and optical truth. In J.A. Russell & J.M. Fernández-Dols, (Eds.), *The psychology of facial expression* (pp. 255-274)*.* Cambridge: Cambridge University Press.

Fridlund, A.J. (1994). *Human facial expression: An evolutionary view*. San Diego, CA: Academic Press.

Fridlund, A.J. (1997). The new ethology of human facial expressions. In J.A. Russell, & J.M. Fernández-Dols (Eds.), *The psychology of facial expression* (pp. 103-129). New York: Cambridge University Press.

Fridlund, A. J. (2002). The behavioural ecology view of smiling and other facial expressions. In M. Abel (Ed.), *An empirical reflection on the smile* (pp. 45 - 82). New York: Edwin Mellen Press.

Fridlund, A.J., & Cacioppo, J.T. (1986). Guidelines for Human Electromyographic Research. *Psychophysiology*, *23*, 567-589.

Hess, U., Banse, R., & Kappas, A. (1995). The intensity of facial expression is determined by underlying affective state and social interaction. *Journal of Personality and Social Psychology*, *69*, 280-288.

Jakobs, E., Manstead, A.S.R., & Fischer, A. (2001). Social context effects on facial activity in a negative emotional setting. *Emotion*, *1*, 51-69.

Kappas, A., & Descôteaux, J. (2003). Of butterflies and roaring thunder: Nonverbal communication in interaction and regulation of emotion. In P. Philippot, R.S. Feldman, & E.J. Coats, (Eds.) *Nonverbal behavior in clinical settings*. (pp.45-74). London: Oxford University Press.

Kappas, A., Bherer, F., & Thériault, M. (2000). Inhibiting facial expressions: Limitations to the voluntary control of facial expressions of emotion. *Motivation and Emotion*, *24*, 259-270.

Manstead, A.S.R., Fischer, A. & Jakobs, E. (1999). The social and emotional functions of facial displays. In P. Philippot, R.S. Feldman, & E.J. Coats (Eds.), *The social context of nonverbal behavior* (pp.287-313). Cambridge: Cambridge University Press.

Parkinson, B. (2005). Do facial movements express emotions or communicate motives? *Personality and Social Psychology Review, 9*, 278- 311.

Russell, J.A. (1994). Is there universal recognition of emotion from facial expression? A review of the cross-cultural studies. In J.A. Fridlund, *Human facial expression: An evolutionary view* (pp. 191-268)*. .* San Diego: Academic Press.

Russell, J.A. (1997). Reading emotions form and into faces: Resurrecting a dimensional-contextual perspective. In J.A. Russell & J.M Fernández-Dols, (Eds.), *The psychology of facial expression* (pp. 295-320)*.* Cambridge: Cambridge University Press.

Smith, C.A., & Scott, H.S. (1997). A componential approach to the meaning of facial expressions. In J.A. Russel, & J.M. Fernandes-Dols (Eds.), *The Psychology of Facial Expression* (pp.229-254)*.* Cambridge: Cambridge University Press.

Tubbs, S.L., & Moss, S. (2003). *Human communication: principles and contexts* (9th ed.). New York: McGraw-Hill.
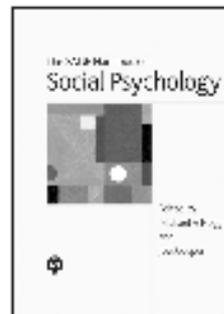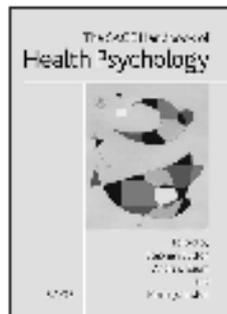
APS 6100 advert psych.indd   1
11/07/2008   11:33:32

A Bluffer's Guide to Meta-Analysis[1]

Dr. Andy Field, University of Sussex

What Is The Point of a Meta-Analysis?

Psychologists are typically interested in finding general answers to questions. For example, Lotze et al (2001) did a study to see which areas of the brain were activated during anal stimulation: they inserted balloons (not party ones) into people's rectums and inflated them while the person was in an fMRI scanner. Then they sang happy birthday and … OK, they didn't, but they really did do the balloon thing. One of the areas of the brain in which they were interested was the secondary somatosensory cortex (S2). Lotze et al. were probably interested in what brain regions were activated in their sample as a means of extrapolating to a wider population. However, what typically happens in science, is some other people then come along, they think 'hmm, shoving balloons up people's arses looks like a fun way to spend some research money' and off they go with their fMRI scanner and balloons to traumatise the local college populous. Of course, sooner or later, many more researchers will realise that this whole bum balloon thing is much more fun than whatever it is they're supposed to be doing, and before you know it, the literature is riddled with research papers (and the world is riddled with people who have conditioned surprised expressions on their face whenever they see an fMRI scanner). Can we assimilate all of these studies to improve the accuracy of our conclusions about which brain areas are activated by having crazy psychologists inflate balloons up our back passages?

Until about 30 years ago, the answer was simply to do a subjective evaluation of the literature. A typical review would entail the author collating articles on the given topic, summarising them and placing some kind of subjective weight on their findings. They might then - if you're lucky - conclude something about the topic of interest: perhaps that a certain area of the brain reliably lights up when your bottom is accosted by a balloon. These reviews have the obvious flaw that even the most discerning of researchers could give

---

particular importance to studies that others might believe to be relatively less important. This can sometimes lead to quite long and heated debates in which different researchers reach different conclusions from the same literature. Meta-analysis rose out of a desire to objectify literature reviews using statistics. In short it is used to discover how big an effect actually is and what factors moderate that effect.

What Steps Do I have to take?

When doing a meta-analysis you basically follow these steps:

Step 1: Do a Literature Search

The first step in meta-analysis is to search the literature for studies that have addressed the same research question (e.g. the ISI Web of Knowledge, PubMed, PsycInfo). We might also search relevant conference proceedings, hand-search relevant journals (in case the searches missed anything), search the reference sections of the articles that we have found, and consult people we consider to be experts in the field – all of this is an attempt to avoid the file drawer problem (which we will discuss later on).

Step 2: Decide on some 'Objective' Criteria for Including Studies

OK, so we've got lots of studies, but obviously some of them might be useless. Badly conducted research can only serve to add bias into our meta-analysis, therefore, it's common to come up with some kind of inclusion criteria for studies. For example, in fMRI there are a variety of ways to process the enormous amounts of data that spew out, and you might reasonably decide that you'll include studies that follow a particular analysis protocol. Likewise, in a meta-analysis of a therapeutic intervention like cognitive behavioural therapy (CBT), you might decide on a working definition of what constitutes CBT, and maybe exclude studies that don't have proper control groups and so on. Your criteria will depend on what you're studying and any specific methodological issues in the field. You cannot exclude studies because you don't like the author. It is important that you formulate a

precise set of criteria that is applied throughout; otherwise you may well be introducing subjective bias into the analysis.

It is also possible to classify studies into groups, for example methodologically strong or weak, and then see if this variable moderates the effect size (see Field, 2003a); by doing so you can see whether methodologically strong studies (by your criteria) differ in effect size to the weaker studies.

Step 3: Calculate the Effect Sizes

Once you have collected your articles, you need to find the effect sizes within them, or calculate them for yourself. I covered effect sizes (what they are, calculating them etc.) a few issues ago (see Field & Wright, 2006), so I won't re-explain them here. Articles may not report effect sizes, or may report them in different metrics; your first job is to get effect sizes for each paper that represent the same effect and are expressed in the same way. If you were using r (my preferred effect size, and yes, you know you have officially become a dork when you have a favoured effect size measure), this would mean obtaining a value for r for each paper you want to include in the meta-analysis. A given paper may contain several r's depending on the sorts of questions you are trying to address with your meta-analysis. For example, I was recently involved in a meta-analysis of cognitive impairment in PTSD and 'cognitive impairment' was measured in a variety of ways in individual studies which meant I was often dealing with several effect sizes within a given article.

Step 4: Do the Meta-Analysis

This is the hard bit, which, if you've got to this stage, will seem ironic it'll probably have taken you most of your life to do steps 1 to 3. The main function of meta-analysis is to estimate the effect size in the population (the 'true' effect) by combining the effect sizes from a variety of articles. Specifically, the estimate is a weighted mean of the effect sizes. The 'weight' that is used is usually a value reflecting the sampling accuracy of the effect size. This makes statistical sense, because if an effect size has good sampling accuracy (i.e. it's likely to be an accurate reflection of reality) then it is weighted highly, whereas effect sizes that are a bit dodgy (are imprecise

estimates) are given less weight in the calculations. Typically, as with any statistic, effect sizes based on large samples are more accurate reflections of the population than those based on small samples, the weight used is the sample size (or some function of it).

What can we get out of the meta-analysis?

✓ The 'true' effect size. That is the actual size of the effect in the population. For example, the true effect in the population of doing CBT on anxious children compared to waiting list controls. You can also compute confidence intervals for this true effect (whoopee!).

✓ The significance of the 'true' effect size. Actually, this isn't very interesting because significance is a function of sample size and so this really tells us nothing very useful (see Field & Wright, 2006). Nevertheless, you can do it if you like (see Field, 2001, because I'm not going to explain it in this article).

✓ Meta-analysis can also be used to estimate the variability between effect sizes across studies (the homogeneity of effect sizes), but again, this in itself, isn't that interesting. There is accumulating evidence that effect sizes should be heterogeneous across studies in the vast majority of cases (see, for example, the NRC paper, 1992). So, you can check if you like, but these tests of homogeneity typically have low power, and I'm of the view that unless there is evidence to the contrary, heterogeneous effect sizes should be assumed.

✓ More interesting (no, really), is that given there is variability in effect sizes in most cases, this variability can be explored in terms of moderator variables (see Field, 2003a). For example, we might find that CBT including group therapy produces a larger effect size for improvement in eating disorders than CBT without a group component.

That's about it really.

Step 5: Write it up, lie back and Wait to see your first Psychological Bulletin Paper

Psychological Bulletin is one of the top ranking psychology journals in the universe. It is filled with meta-analyses. Meta-Analysis is the route to academic fame, fortune, the love of your department and the respect of your peers (or is that the other way around?)[2]. How do you write one up? Just follow Rosenthal's (1995) excellent guidelines; apart from being (as ever with Rosenthal) very sensible and very clearly-written, they were also published in Psychological Bulletin so they can hardly complain can they☺

How Do You Do A Meta-Analysis?

Ah, the tricky Step 4 eh? Well, obviously, there's just one way to do it, right? WRONG! This being statistics and everything there are numerous ways to do a meta-analysis, all of them are sort of different in different ways, involve making decisions about your data and have led some people (that'll be me then) to make small careers out of trying to establish which method is 'best'.

A Few of the More Important Issues to Bear in Mind

There are lots of issues to bear in mind and I've written about some of them (Field, 2001, 2003a, b; 2005a, b); to be fair, Schulze has written about them in more detail and rather more convincingly as have many others (Hunter & Schmidt, 2004; Rosenthal & DiMatteo, 2001). In terms of doing a meta-analysis, the main issues (as I see them) are:

1. Which Method Should I use?

2. Which conceptualisation of my data should I assume?

Actually, these two issues are linked. There are two ways to conceptualise meta-analysis: fixed effects and random effects models[3]. The fixed-effect model assumes that studies in the meta-analysis are sampled from a population in which the average effect size is fixed. Put another way, sample effect sizes should be homogenous because they come from the same population with a fixed average effect.

---

[2] At this point I should add that despite knowing this and despite having done lots of things involving meta-analysis, I've never actually done one and submitted it to Psychological Bulletin. Which just proves what an idiot I am.
[3] There are mixed models too, but I'm going to ignore them: see Overton, 1998.

The alternative assumption is that the average effect size in the population varies randomly from study to study: studies in a meta-analysis come from populations that have different average effect sizes, so, population effect sizes can be thought of as being sampled from a 'superpopulation'. See just about anything by me in the reading list for some further explanation. Put another way, the effect sizes should be heterogeneous because they come from populations with varying average effect sizes.

How is this tied up with the method we use? Well, statistically speaking, the main difference between fixed- and random-effects models is in the amount of error. In fixed-effects models there is error introduced because of sampling studies from a population of studies. This error exists in random-effects models but there is *additional* error created by sampling the populations from a superpopulation (see Field, 2005b for some diagrams). So, calculating the error of the mean effect size in random-effects models involves estimating two error terms, whereas in fixed-effects models there is only one error term. This has some implications for computing the mean effect size.

The two most widely-used methods of meta-analysis are those by Hunter & Schmidt (2004) which is a random effects method, and the method by Hedges and Colleagues who provide both fixed- and random-effects methods. I mentioned earlier on that there were rarely grounds to assume the fixed-effects case, that is, effect sizes are homogenous. You can trust me on this, or you can read the NRC (1992) report, or Hunter and Schmidt (2000) or Field (2005a) who argue or present data supporting this position. Despite overwhelming evidence that variable effect sizes are the norm in psychological data, this hasn't stopped lots of people from using fixed-effects methods. In fact, fixed effects methods are routinely applied to data even when effect sizes are variable (see Hunter & Schmidt, 2000) and this can have some fairly entertaining results such as a massive bias in resulting statistics (see Field, 2003b). To add to the confusion, the methods differ according to the effect size measure you use. I'm going to assume we're using *r*, but if you're using *d* you have to use slightly different equations (see Hedges & Vevea, 1999; Hunter & Schmidt, 2004).

Hedges and Colleagues' Method (Hedges & Olkin, 1985; Hedges & Vevea, 1998)

In this method, effect sizes are first converted into a standard normal metric (using Fisher's r-to-Z transformation) before calculating a weighted average of these transformed scores (in which *r* is the effect size from study *i*:

$$z_{r_i} = \tfrac{1}{2} Log_e \left(\tfrac{1+r_i}{1-r_i} \right)$$

(1)

The transformation back to *r* is simply:

$$r_i = \frac{e^{(2z_i)} - 1}{e^{(2z_i)} + 1}$$

(2)

In the fixed-effect model, the transformed effect sizes are used to calculate an average in which each effect size is weighted by the inverse within-study variance of the study from which it came (for correlation coefficients the sample size, *n*, minus three):

$$\bar{z}_r = \frac{\sum_{i=1}^{k} w_i z_{r_i}}{\sum_{i=1}^{k} w_i} = \frac{\sum_{i=1}^{k} (n_i - 3) z_{r_i}}{\sum_{i=1}^{k} (n_i - 3)} \quad ,$$

(3)

in which *k* is the number of studies in the meta-analysis.

This average is used to calculate the homogeneity of effect sizes. The resulting statistic Q has a chi-square distribution with k − 1 degrees of freedom:

$$Q = \sum_{i=1}^{k} \left(w_i \right) \left(z_{r_i} - \bar{z}_r \right)^2$$

(4)

If you wanted to apply a fixed effects model you could stop here. However, as I've tried to convince you, this would be a bad thing, so read on. To calculate the random-effects average effect size, the weights use a variance component that incorporates both between-study variance and within-study

variance. The between-study variance is denoted by τ^2 and is simply added to the within-study variance. The weighted average in the z metric is, therefore:

$$\bar{z}_r^* = \frac{\sum_{i=1}^{k} w_i^* z_{r_i}}{\sum_{i=1}^{k} w_i^*}$$

(5)

in which the weights $\left(w_i^*\right)$ are defined as:

$$w_i^* = \left(\tfrac{1}{w_i} + \tau^2\right)^{-1}$$

(6)

The between-study variance can be estimated in several ways (Hedges & Vevea, 1998; Overton, 1998), however, Hedges and Vevea use Q (which we came across earlier), k, and a constant, c:

$$\tau^2 = \frac{Q-(k-1)}{c}$$

(7)

where the constant, c, is defined (for correlation coefficients) as:

$$c = \sum_{i=1}^{k}\left(w_i\right) - \frac{\sum_{i=1}^{k}\left(w_i\right)^2}{\sum_{i=1}^{k}\left(w_i\right)} \quad .$$

If τ^2 is negative then it is set to zero (because the variance between-studies cannot be negative). Having calculated τ^2, it is used to calculate the weights $\left(w_i^*\right)$, which in turn are used to calculate the mean effect size using equation 5. This average effect size must be converted back to the r metric (equation 2) before being reported.

Finally, it is useful to construct confidence intervals for the mean effect size (see Field, 2005c for a detailed explanation of confidence intervals and what they mean). To calculate these confidence intervals we need to know the standard error of the mean effect size is:

$$SE\left(\bar{z}_r^*\right) = \sqrt{\frac{1}{\sum_{i=1}^{k} w_i^*}}$$

(8)

which uses the weights we've already calculated.

The confidence interval around the average effect size is easily calculated using the standard error and the two-tailed critical value of the normal

distribution (which is 1.96 for the most commonly used 95% confidence interval). The upper and lower bounds are calculated by taking the average effect size and adding or subtracting its standard error multiplied by 1.96:

$$CI_{Upper} = \bar{z}_r^* + 1.96 SE\left(\bar{Z}_r^*\right)$$

(9)

$$CI_{Lower} = \bar{z}_r^* - 1.96 SE\left(\bar{Z}_r^*\right)$$

(10)

These values are again transformed back to the *r* metric before being reported.

Hunter and Schmidt Method (Hunter & Schmidt, 2004)

Although this method's greatest virtue is its emphasis on isolating and correcting for sources of error such as sampling error and reliability of measurement variables, it is dealt with here in only its simplest form. Unlike Hedges' method the untransformed effect-size estimates, *r*, are used to calculate the weighted mean effect size, and the weight used is simply the sample size, *n*:

$$\bar{r} = \frac{\sum_{i=1}^{k} n_i r_i}{\sum_{i=1}^{k} n_i}$$

(11)

Hunter and Schmidt (2004) argue that the variance across sample effect sizes consists of the variance of effect sizes in the population and the sampling error and so the variance in population effect sizes is estimated by correcting the variance in sample effect sizes by the sampling error. The variance of sample effect sizes is the frequency weighted average squared error:

$$\sigma_r^2 = \frac{\sum_{i=1}^{k} n_i (r_i - \bar{r})^2}{\sum_{i=1}^{k} n_i}.$$

(12)

The sampling error variance is calculated as:

$$\sigma_e^2 = \frac{\left(1 - \bar{r}^2\right)^2}{\bar{N} - 1}$$

(13)

The variance in population effect sizes is estimated by subtracting the sampling error variance from the variance in sample effect sizes:

$$\hat{\sigma}_\rho^2 = \sigma_r^2 - \sigma_e^2$$

(14)

Hunter and Schmidt recommend correcting this estimate for artefacts (see Hunter & Schmidt, 2004) and then constructing credibility intervals. These intervals are based on taking the average effect size and adding or subtracting from it the square root of the estimated population variance multiplied 1.96 (for a 95% interval):

$$\text{Credibility Interval}_{\text{Upper}} = \bar{r} + 1.96\sqrt{\hat{\sigma}_\rho^2}$$

(16)

$$\text{Credibility Interval}_{\text{Lower}} = \bar{r} - 1.96\sqrt{\hat{\sigma}_\rho^2}$$

(17)

An Example

In my last Bluffer's guide on effect sizes, I used an example of whether listening to Cradle of Filth (CoF) turns people into Granny-murdering devil-worshippers. In that example, we exposed unborn children to Cradle of Filth (or not) and observed how they turn out years later. Now clearly, this is a topic that would interest lots of researchers so let's imagine lots of researchers had addressed a similar question (perhaps using different methodologies, and different outcome measures). We can follow the steps outlined above:

Step 1: Do a Literature Search

Ok, we searched the ISI Web of Knowledge, PubMed, PsycInfo etc. and found the studies listed in Table 1.

Table 1: Summary of articles found on CoF and satanic activity.

Study	Journal	Measures	Rating/Comment

Incon & Tennent(2002)	Knitting Pattern Review	Grannies Murdered	***** (Nice Stats)
Little, Bo & Peep (2002)	Journal of Sacrificial Goats	Goats Sacrificed	***** (Nice Graphs)
Beelzibub (2003)	Norwegian Journal of Satanism	Daemons summoned	***** (Good use of GLMs)
Osbourne (2004)	Bird Eater Monthly	Doves eaten	***** (Helpful effect-sizes)
Fisher[4] (2004)	Goat Lover monthly	Goats abducted	****
Field & Hedgerow (2003)	Latvian Journal of Shite Research	Pentagrams drawn	* A bit iffy really …

Step 2: Decide on some 'Objective' Criteria for Including Studies

You might add some ratings based on your systematic criteria (see Table 1) to help you identify features of the study, or generally assess the quality of the research). Exclusions might also have to be made on the grounds that the authors do not report enough information for effect sizes to be computed. In this case we'll exclude the Field & Hedgerow article on the basis that everything I do is rubbish.

Step 3: Calculate the Effect Sizes

Next, we calculate the effect sizes for each study, and maybe tabulate them with other helpful information (such as the sample size on which the effect size is based, *N*) – Table 3.

---

[4] Apparently, Fisher (yes, the one who invented lots of things like ANOVA) had a pet Goat that he took to work with him.

Table 3: Effect sizes and sample sizes for remaining studies.

Study	Journal	Measures	N	Effect (r)
Incon & Tennent(2002)	Knitting Pattern Review	Grannies Murdered	135	-.68
Little, Bo & Peep (2002)	Journal of Sacrificial Goats	Goats Sacrificed	1235	-.79
Beelzibub (2003)	Norwegian Journal of Satanism	Daemons summoned	570	-.74
Osbourne (2004)	Bird Eater Monthly	Doves eaten	190	.12
Fisher (2004)	Goat Lover Monthly	Goats abducted	52	.24
Field & Hedgerow (2003) Excluded for Being Shite				

Step 4: Do the Meta-Analysis

Let's first, do the Hunter-Schmidt method because this, frankly, is easier.

We can first extend our table to compute the weighted effect sizes (Table 4):

Table 4: Weighted effect sizes for the studies in our meta-analysis.

Study	N	r	Nr
Incon & Tennent(2002)	135	-.68	-91.80
Little, Bo & Peep (2002)	1235	-.79	-975.65
Beelzibub (2003)	570	-.74	-421.80
Osbourne (2004)	190	.12	22.80
Fisher (2004)	52	.24	12.48
Total	**2182**		**-1452.97**

Using equation 11, we get a weighted mean of $-.666$:

$$\bar{r} = \frac{-1452.97}{2182}$$
$$= -.666$$

This means that the true effect in the population is a strong negative effect of listening to CoF on satanic behaviour (i.e. the more you listen to CoF, the less goat abducting, granny murdering etc. you engage in). To calculate the credibility interval, we can use the true effect we've just calculated to create a new table (Table 5).

Table 5: Calculating Credibility Intervals

Study	N	r	$(r-(-.666))^2$	$n(r-(-.666))^2$	Nr
I & T (2002)	135	-.68	.000196	0.26	-91.80
L, B & P (2002)	1235	-.79	.015376	18.99	-975.65
B (2003)	570	-.74	.005476	3.12	-421.80
O (2004)	190	.12	.617796	117.38	22.80
F (2004)	52	.24	.820836	42.68	12.48
Total	2182			182.43	-1452.97

Column 4 (and its sum) represent the entire top half of equation 12 (column 3 is a stepping stone to get there), the sum of column 2 is the bottom half of this equation. This gives us (for equation 12):

$$\hat{\sigma}_r^2 = \frac{182.43}{2182} = 0.0836$$

The sampling error variance (equation 13) can be obtained more directly. The only value we don't have is the average sample size, but we have the total from Table 5 so we can just divide this by the number of studies (in this case 5). We get for equation 13:

$$\hat{\sigma}_e^2 = \frac{\left(1-\bar{r}^2\right)^2}{\frac{\bar{N}}{N}-1}$$

$$= \frac{\left(1-\left(-.666\right)^2\right)^2}{\frac{2182}{5}-1}$$

$$= 0.000711$$

The two values we've just calculated are then used to estimate the variance of population effect sizes (equation 14):

$$\hat{\sigma}_\rho^2 = \hat{\sigma}_r^2 - \hat{\sigma}_e^2$$

$$= 0.0836 - 0.000711$$

$$= 0.0829$$

This value is then used to create 95% credibility intervals (equations 16 and 17):

$$\text{Credibility Interval}_{\text{Upper}} = \bar{r} + 1.96\sqrt{\hat{\sigma}_\rho^2} = -.10$$

$$\text{Credibility Interval}_{\text{Lower}} = \bar{r} - 1.96\sqrt{\hat{\sigma}_\rho^2} = -1.23$$

Ok, that's the easy one out of the way, now Hedges' method. I've rounded off the decimal places, but obviously accuracy is important so when doing the actual calculations I have kept all decimal places in, so you might get slightly different values if you just use my rounded off values from the tables. However, you can download an Excel spreadsheet with formulae for this method. It uses these data, but you can easily extend it to your own meta-analysis by adding rows to the tables: you might have to tweak some of the formula so that they refer to the correct cells though: http://www.sussex.ac.uk/Users/andyf/pgstat.html)

Table 6 shows the main steps in the fixed-effects method. Again, we have our studies, our sample sizes (n) and our effect sizes (r). The first thing is to calculate the weights in equation 3. This is simple, just subtract 3 from the sample size (see column 3). Next, we have to convert r into the z metric using equation 1 (this is shown in column 6). Finally, we times these z values by the weights (i.e. we multiply column 3 with column 6) and this gives us column 7. The sum of column 7 is the top half of equation 3, and the sum of

column 3 is the bottom half[5]. Therefore, to get the true effect size, we use these two sums:

$$\bar{z}_r = \frac{-1933.83}{2167} = -0.8924$$

Remember that this is in the z-metric, so we must convert back to *r* using equation 2, which gives us a true effect of –0.7126.

Table 6: Hedges' Fixed-effects meta-analysis

Study	*n*	*w*	w²	*r*	z_r	*W* × z_r
I & T (2002)	135	132	17424	-0.68	-0.83	-109.44
L, B & P (2002)	1235	1232	1517824	-0.79	-1.07	-1320.00
B (2003)	570	567	321489	-0.74	-0.95	-538.92
O (2004)	190	187	34969	0.12	0.12	22.55
F (2004)	52	49	2401	0.24	0.24	11.99
		2167	1894107			-1933.83

We need this value to calculate Q, the homogeneity of effect sizes in equation 4. Table 7 shows the steps here. We use the weights from each study again (*N* – 3), which is the left half of equation 4. The right hand side consists of the effect size (in z units) minus the mean effect size that we just calculated (–0.8924)—see column 4 of table 7. We then square these values (Column 5) and then multiply by the weights (Column 6). We sum the resulting values to give us the result of equation 4, *Q* = 297.18.

---

[5] You might be wondering what column 4 is all about (the weights squared), these values come in useful later on.

Table 7: Calculating the Homogeneity of Effect Sizes

Study	w	z_r	$(z_r)-(-0.892)$	(Column 4)^2	$W \times$ Column 5
I & T (2002)	132	-0.83	0.06	0.004	0.53
L, B & P (2002)	1232	-1.07	-0.18	0.032	39.49
B (2003)	567	-0.95	-0.06	0.003	1.91
O (2004)	187	0.12	1.01	1.026	191.89
F (2004)	49	0.24	1.14	1.293	63.36
	2167				**297.18**

We now have to use this value of Q to calculate tau^2 (equation 7). However, first we have to calculate c, and we already have all of the values we need in Table 6; that is, the sum of weights (bottom of column 3) and the sum of weights squared (bottom of column 4). This gives us:

$$c = \sum_{i=1}^{k} (w_i) - \frac{\sum_{i=1}^{k} (w_i)^2}{\sum_{i=1}^{k} (w_i)}$$
$$= 2167 - \frac{1894107}{2167}$$
$$= 1292.93$$

Using Equation 7, we can get tau^2 as (k is the number of studies in the meta-analysis, and Q we have just computed):

$$\tau^2 = \frac{Q-(k-1)}{c} = \frac{297.18 - (5-1)}{1292.93} = 0.227$$

We can now use this value to calculate the weights for the random-effects model (equation 6). Again, you can break this equation down into a table (Table 8).

Table 8: Calculating the random-effects mean effect size.

Study	N	w	1/w	$1/w + tau^2$	1/Column 5	z_r	(W*) × z_r
I & T (2002)	135	132	0.008	0.23	4.30	-0.83	-3.562
L, B & P (2002)	1235	1232	0.001	0.23	4.42	-1.07	-4.740
B (2003)	570	567	0.002	0.23	4.41	-0.95	-4.188
O (2004)	190	187	0.005	0.23	4.34	0.12	0.523
F (2004)	52	49	0.020	0.25	4.07	0.24	0.997
		2167			21.53		-10.97

Table 8 begins by breaking down equation 6. First we calculate the original weights ($n - 3$), we then divide 1 by these values ($1/(n–3)$), next we add our previously calculated value of tau^2 (0.227) to give us the inside of the brackets in equation 6. Finally, we divide 1 by these values and end up with the weights for the random-effects method (column 6). We already know the z-score values of the effect sizes, so we need to simply multiply these new values to give us the new weighted effect sizes (column 8). The mean is derived from equation 5, the top half of which is the sum of column 8 in Table 8 and the bottom half of which is the sum of column 6 in Table 8:

$$\bar{z}_r^* = \frac{\sum_{i=1}^{k} w_i^* z_{r_i}}{\sum_{i=1}^{k} w_i^*} = \frac{-10.97}{21.53} = -0.509$$

Remember that this is in the z-metric, so we must convert back to r using equation 2, which gives us a true effect of –0.469. Notice, this result is a fair bit smaller than the fixed-effects model!

To get the confidence intervals we use equation 8 to find the standard error of our mean effect size; the sum of weights is 21.53 (see Table 8), which gives us:

$$SE\left(\bar{z}_r^*\right) = \sqrt{\frac{1}{\sum_{i=1}^{k} w_i^*}} = \sqrt{\frac{1}{21.53}} = 0.215$$

To get the 95% confidence intervals we times this by 1.96 and then either add or subtract it from the mean effect size:

$$CI_{Upper} = \bar{z}_r^* + 1.96SE\left(\bar{Z}_r^*\right) = -0.509 + 0.422 = -0.087$$

$$CI_{Lower} = \bar{z}_r^* - 1.96SE\left(\bar{Z}_r^*\right) = \text{-0.509-0.422 = -0.931}$$

Again, these values are in z units so we have to convert back to *r* using equation 2, which gives us an upper CI of -0.087 (it hasn't changed to 3 decimal places because it is close to zero) and a lower value of –0.731. As with the Hunter Schmidt method, the results show that listening to CoF has a negative effect on satanic behaviours, but notice that the estimate of the true effect is lower than when the Hunter-Schmidt method was used.

Step 5: Write it up, lie back and Wait to see your first Psychological Bulletin Paper

Or spend the rest of your life in an asylum after all of that maths☺

That's All Bloody Hard Work, Can't I just get SPSS to Do It?

No actually (well, not unless you write your own syntax)! Hunter and Schmidt do provide software on the CD ROM in their book for doing their method. There are also (I believe) some commercially available packages that do it, but frankly they're a bit of a rip-off: it's not *that* hard.

Which Method is Best?

Our two analyses yielded different results which beg the question of which method is most accurate. Several people have compared methods: Schulze (2004) did a whole book of simulations that compared these and other methods and it's well worth a look. I have also published a couple of papers doing similar things (but less extensively than Schulze): Field, 2001; 2005a. We are not the only people to have studied these issues but given I've already written a very long Bluffer's guide, I'll just say, if you're interested, look at these sources for reviews of the evidence.

Other Problems with Meta-Analysis

Publication Bias and The 'File Drawer' Problem: Publication bias refers to the fact that significant findings are more likely to be published than non-significant findings. It is sometimes known as the 'file drawer' problem because non-significant research is more likely to end up in the researchers file drawer than in a journal. This bias can be substantial, estimates suggest that significant findings are eight times more likely to be submitted than non-significant ones and 97% of articles in psychology journals reported significant results. The effect of this bias is that meta-analytic reviews are likely to over-estimate mean effect sizes.

Artefacts: Effect sizes are influenced by the quality of the research and quality precision of measurement of variables. The error in the measurement of variables will vary across studies and correlational research is prone to variance in the range of scores elicited from participants (range variation). Hunter and Schmidt (1990) have suggested statistical techniques for correcting for measurement error and range variation and you are well-advised to consult their book.

Further Reading

The following are good introductions to the issues in meta-analysis (in alphabetic order): Field (2005b, 2003a), Hunter & Schmidt (2004); Rosenthal (1990), Rosenthal & DiMatteo (2001).

References

Field, A. P. (2005a). Is the meta-analysis of correlation coefficients accurate when population effect sizes vary? Psychological Methods, 10 (4), 444-467.

Field, A. P. (2005b). Meta-analysis. In J. Miles & P. Gilbert (eds.) A handbook of research methods in clinical and health psychology (pp. 295-308). Oxford: Oxford University Press.

Field, A. P. (2003a). Can meta-analysis be trusted? The Psychologist, 16, 642-645.

Field, A. P. (2003b). The problems in using Fixed-effects models of meta-analysis on real-world data. Understanding Statistics, 2, 77–96.

Field, A. P. (2001). Meta-analysis of correlation coefficients: a Monte Carlo comparison of fixed- and random-effects methods. Psychological Methods, 6 (2), 161–180.

Field, A. P., & Wright, D. B. (2006). A bluffer's guide to effect sizes. Psy-Pag Quarterly, 58, 9-23.

Hedges, L. V., & Olkin, I. (1985). *Statistical methods for meta-analysis*. Orlando, FL: Academic Press.

Hedges, L. V. & Vevea, J. L. (1998). Fixed- and random-effects models in meta-analysis. *Psychological Methods*, *3*, 486-504.

Hunter, J. E., & Schmidt, F. L. (2000). Fixed effects vs. random effects meta-analysis models: implications for cumulative knowledge in Psychology. *International Journal of Selection and Assessment*, *8*, 275–292.

Hunter, J. E., & Schmidt, F. L. (2004). Methods of meta-analysis: Correcting error and bias in research findings (Second edition). Newbury Park, CA: Sage.

Lotze, M., Wietek, B., Birbaumer, Ehrhardt, J., Grodd, W., & Enck., P. (2001). Cerebral activation during anal and rectal stimulation. NeuroImage, 14, 1027-1034.

National Research Council (1992). Combining information: Statistical issues and opportunities for research. Washington, D.C.: National Academy Press.

Overton, R. C. (1998). A comparison of fixed-effects and mixed (random-effects) models for meta-analysis tests of moderator variable effects. Psychological Methods, 3, 354-379.

Rosenthal, R. (1995). Writing meta-analytic reviews. Psychological Bulletin, 118, 183-192.

Rosenthal, R. (1991). Meta-analytic procedures for social research (revised). Newbury Park, CA: Sage.

Rosenthal, R., & DiMatteo, M. R. (2001). Meta-analysis: Recent developments in quantitative methods and literature reviews. Annual Review of Psychology, 52, 59-82.

Schulze, R. (2004). Meta-analysis: a comparison of approaches. Cambridge, MA: Hogrefe & Huber.

Beyond effect sizes: Or why statisticians are not qualified to interpret your results

Dr. Paul Morris, University of Portsmouth

In a previous article, (Quarterly Issue No 58), Field and Wright provided a concise and cogent account of the limitations of significance levels in the context of NHST (Null Hypothesis Significance Testing), as well as the benefits of reporting effect sizes. The take home message of their article was that statistical significance can always be achieved through the use of a large enough number of participants, and that effect sizes are preferable as they are relatively insensitive to increases in participant numbers. In the current article I demonstrate that effect sizes - on their own or in combination with significance levels - are important, but potentially just as misleading as interpreting significance levels alone. My starting point is that whether a research finding is important is entirely a human judgement. Statistical significance and effect size provide different and complementary information about samples and their likely relation to populations, but what they do not (and cannot) tell you is whether the results you have found are meaningful or important. I will demonstrate that a big effect size is no more or less a guide to whether an effect is meaningful or important, than a result with an extreme significance level. To illustrate this point I will give some hypothetical examples.

Example 1: Choosing a windsurfing holiday location.

I am a keen windsurfer. When I go on holiday I want somewhere that is windy. Thus I gather mean wind speeds in knots for the month I wish to go on holiday, from twenty locations on each of two islands that are my potential holiday destinations. I am going to use the data to help me decide which island I am going to visit. I carry out an independent groups t test on the data.

Group Statistics

	island	N	Mean	Std. Deviation	Std. Error Mean
windspeed	island A	20	3.7000	1.86660	.41739
	island B	20	7.7000	1.78001	.39802

Independent Samples Test

		Levene's Test for Equality of Variances		t-test for Equality of Means						
									95% Confidence Interval of the Difference	
		F	Sig.	t	df	Sig. (2-tailed)	Mean Difference	Std. Error Difference	Lower	Upper
windspeed	Equal variances assumed	.258	.615	-6.936	38	.000	-4.00000	.57674	-5.16755	-2.83245
	Equal variances not assumed			-6.936	37.915	.000	-4.00000	.57674	-5.16764	-2.83236

It is clear from the SPSS output that there is a large statistically significant difference between the wind measurements taken on the two islands. The effect size is also large. The Cohen's *d* value is 2.2 (Cohen's *d* is the number of standard deviations difference between the means, it is intrinsically meaningful in that it directly relates to the overlap of standard deviation error bars that can be seen on error bar graphs of means; a small effect size is about .2, a medium effect size is .5 and a large effect size is over .8).
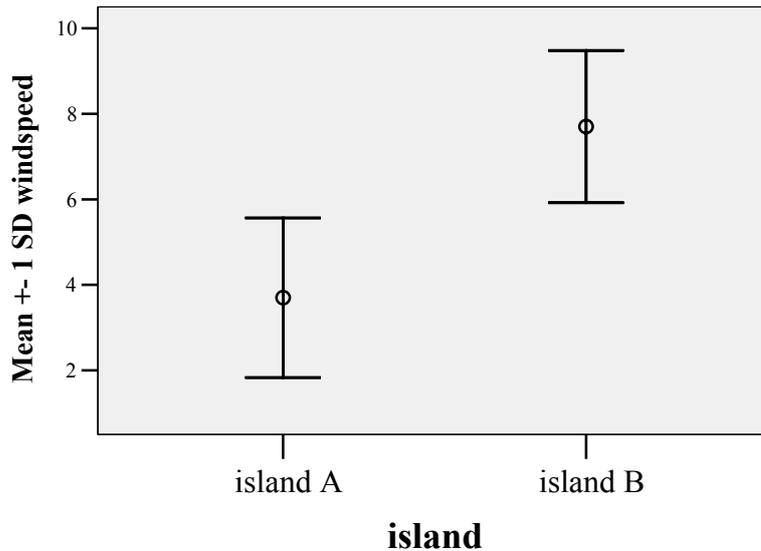
Figure 1. Wind speed as a function of island (note that it is easy to see that the there are about 2 SDs difference between the means which directly relates to the Cohen's *d* value)

It is very clear that there is statistically significant difference between the wind speed on these two islands is associated with a large effect size. From your knowledge of effect sizes it should now be very clear that I should go to island B for the holiday. However, you could not be more wrong. To understand why, I have to explain a little bit about windsurfing. For anybody but a novice, windsurfing is only fun when boards are planing (i.e. going fast and skimming the water like a ski boat at speed, rather than displacing the water like rowing boats or ships). Boards only plane in about +12 knots of wind. Therefore to a windsurfer, the fact that island B has better wind than island A is irrelevant as in neither case is the wind speed above 12 knots. They would both be pants for windsurfing! But note the ability to interpret the statistical outcomes is dependent on an intimate knowledge of the variables used. A non-windsurfing statistician would be of no use!

Example 2. Sex offenders treatment programme.

A new intervention is being evaluated for the treatment of recidivist sex offenders. 200 repeat sex offenders were randomly allocated to a standard treatment regimen or to a new improved treatment regimen.

Group Statistics

	type of treatment	N	Mean	Std. Deviation	Std. Error Mean
motivation to reoffend	standard treatment	100	60.8200	8.47859	.84786
	new treatment	100	58.4400	8.52841	.85284

Independent Samples Test

		Levene's Test for Equality of Variances		t-test for Equality of Means						
									95% Confidence Interval of the Difference	
		F	Sig.	t	df	Sig. (2-tailed)	Mean Difference	Std. Error Difference	Lower	Upper
motivation to reoffend	Equal variances assumed	.995	.320	1.979	198	.049	2.38000	1.20258	.00849	4.75151
	Equal variances not assumed			1.979	197.99	.049	2.38000	1.20258	.00849	4.75151

From the SPSS output it is clear that the effect is only just significant and the effect size calculation reveals that the effect size is small (Cohen's *d* = .2). From figure 2 it is clear that the samples are hugely overlapping. The sample size is also quite big. So with your new found knowledge about sample sizes and significance levels, and effect sizes it might lead you to think that this new treatment is not very much to write home about.
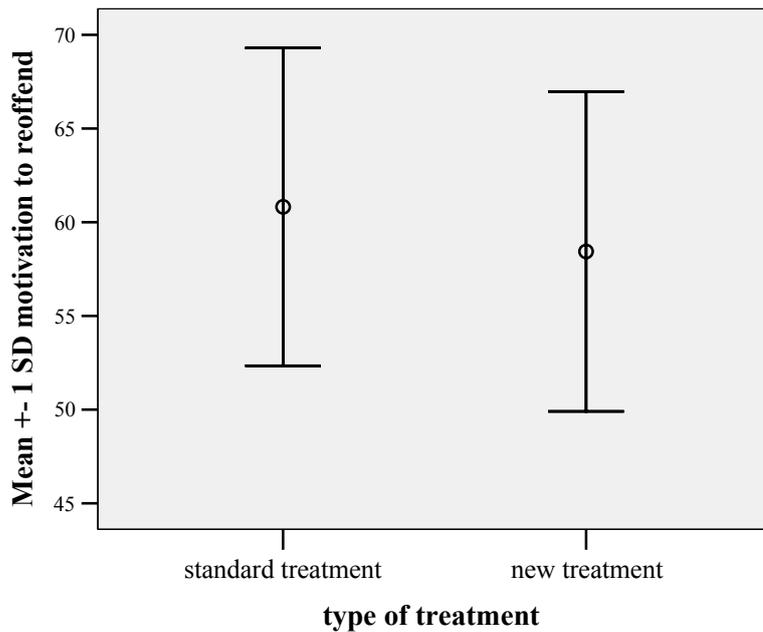
Figure 2. Motivation to re-offend as a function of treatment type

However, if I gave you the information that previous research had shown that for every one point reduction in the motivation to re-offend score, the number of offences committed in a year by an individual went down by 10%, and this could represent tens of victims per offender. Also the new treatment duration was a tenth of that of the previous treatment, and was much less stressful for the psychologist. In this case a result with a small effect size can still be very important.

A further example relates to new therapeutic drugs. For some cancers there are no effective treatments at present. If a drug could be produced that has a tiny positive effect in terms of increased survival rates, then this drug would have a good chance of development simply because it is better than nothing. However, in reality a decision about whether the increase in survival rate was important would depend on a whole range of other variables such as the stability of the drug, cost of manufacture, side effects, the potential market size and even delivery method.

The take home message from these examples is that whether an effect is important or meaningful is a judgement based on a huge number of factors, not just the statistical outcomes. The really pernicious aspect of NHST, is the use of .05 as a decision criterion for whether a result is important or not. The

replacement of significance levels with effect sizes as a <u>decision criterion</u>, is equally pernicious. Statisticians cannot decide whether an effect is important unless they have domain specific expertise about the research area. The decision about whether an effect is important can only be made by the experts in the area of research. A small effect size is not necessarily unimportant, and a big effect size is not necessarily important. The outcomes of statistical tests are only another form of description of the data. They were never intended to be anything else. A decision criterion taken from a textbook on statistics can never tell you whether an effect is important; that decision is down to you as a researcher and your knowledge of the research area. In clinical drug research, a requirement for ethical approval is that you specify the magnitude of the effect size that is thought to be important; you must justify the effect size chosen in terms of the wider potential costs and benefits of the particular drug and the relevant disorder.

I want to make it clear that I do not reject the use of effect sizes or statistical significance for that matter; I am fully signed up to the quantitative approach to research. All I am saying is that the statistical results can help you make an informed decision, but you cannot rely on a priori general decision criteria derived from a textbook to make a decision about the results for you. The distinction between test results and descriptive statistics is spurious. The result of a statistical test is just another form of description of the data in terms of the probable nature of the relationship between the samples used and the parent population.

The irony of the current situation is that the founders of modern NHST never intended it to be used in the current fashion. NHST was developed by Ronald Fisher, Jerzy Neyman and Egon Pearson. Fisher was in vehement dispute with Neyman and Pearson about its correction interpretation. Another grand irony is that the current format of NHST is a bastard amalgam of both interpretations that would have been rejected by the three great statisticians. See Cowles (1989) for a full history of the Fisher vs. Pearson disputes and a paper by Lenhard (2006) for the arguments surrounding NHST in particular. R.A. Fisher, who comes in for a fair amount of implied criticism for his recommendation of the .05 significance level, certainly never intended that it be used as a simple decision criterion. He saw it as a useful guide within a

series of experimental studies. In his classic text "The Design of Experiments" he states "In order to assert that a natural phenomenon is experimentally demonstrable we need, not an isolated record but a reliable method of procedure" (Fisher 1949, p. 14). Fisher would need no lesson on the importance of the magnitude of the differences between groups; his work was extremely applied, much of it on the impact of different treatments to crops (see his early classic paper on the manurial response of the potato!). One major criticism of the traditional use of NHST is that the choice of .05 is arbitrary. However, an equally valid criticism can be made with reference to effect sizes. There is no mathematical or scientific justification for the allocation of the numerical values labelled small, medium and large.

Psychology has a particular problem with the interpretation of statistical results that is not shared with other disciplines such as medicine. The problem in Psychology is that the actual values of the numbers we record associated with our dependent variables often have no intrinsic meaning for us. What does an extraversion score of ten actually mean? What does an alpha abundance score of six mean for an individual? The dependent variables we measure do not have a clear relationship to the real world. We often know that one group scores higher than another group, but have no idea of the potential impact of the difference in the world. This is in stark contrast to medicine where almost all of the dependent variables measured, such as blood pressure or white blood cell count, have interpretable implications for the health or illness of patients. The numbers mean something to the physician in a way that many of the measures taken by psychologists do not. As a researcher, the actual values of what you are measuring should mean something to you; it is your research and your area of expertise; the numbers in your data should be a huge amount more informative to you than to any other statistician! Psychologists are not wilfully stupid in their selection of dependent variables. The difficulty for psychologists is that what we want to measure (e.g. intelligence, sociability, extraversion, suggestibility) are difficult to translate from the level of conceptual dependent variable to operational dependent variable. However, we should make every effort having developed a dependent variable, to demonstrate how our measures relate to the world outside the lab.

The increasing use of really sophisticated statistical tests is exacerbating the difficulty of relating findings to the real world in any meaningful way. Sophisticated tests in essence just partition variance in ever more complex ways. The outcomes of structural equation models, path analysis or confirmatory factor analysis are extremely difficult to relate in any meaningful way to conceptual variable under study. The problem with any kind of mathematical transformation is that the data is distorted by the process. For example, even a simple statistic such as the standard deviation is always a misrepresentation of the raw data, as it implies a symmetrical distribution (real world distributions are never perfectly symmetrical). Such distortions are multiplied in even comparatively simple tests such as ANOVA, and multiplied exponentially in really complex analyses. However, there are serious attempts in many hard science disciplines such as engineering and medicine to present the data as much as possible in the raw (Tufte, 2001). An example is the back to back stem and leaf dot plot. In such a plot the viewer can see every data point; can see the shape of the distributions; the degree of overlap of the distributions and even the outliers. The decision about whether there is an important difference between the two groups is then made on basis of the information contained in the graph.

In summary all kinds of statistical results, from basic summary statistics, to the results of complex MANCOVAs, are actually descriptive. Effect sizes and significance levels are just another level of description. The mistake with NHST is to use decision criterion - either significance levels or effects sizes - that are specified a priori that have nothing specific to do with the research question under scrutiny. It is an uncomfortable truth that however rigorous the scientific method used to produce results, the importance of research results is in the end a matter of opinion. The task of the researcher is to carry out the empiricism in a rigorous and replicable manner and then justify their interpretation of the importance of the results.

References

Cowles, M. (1989) *Statistics in Psychology An Historical Perspective*. Hillsdale NJ: LEA.

Fisher, R.A. (1949) *The Design of Experiments, 5th edition*. Edinburgh: Oliver and

Boyd.

Lenhard, J. (2006) Models and statisical inference: The controversy between Fisher and Neyman-Pearson. *British Journal of the Philosophy of Science. 57*, 69-91.

Tufte, E.R. (2001) *The Visual Display of Quantitative Information*. New York: Graphics Press.

Book review: 'Mixing Methodologies in Psychology: The integration of qualitative and quantitative methods in theory and practice'/ edited by Todd et al

John McAlaney, University of Paisley

The main premise of *Mixing Methodologies* is to explore the division that exists in psychology between quantitative and qualitative research methods. The central argument of the text is that this divide is largely artificial and often detrimental to research progress. The book is split into four sections, with the first beginning with an obligatory although succinctly written overview of the way in which research is classed as qualitative or quantitative. However this is then taken a step further than in a typical methodology book by giving a background as to why these different methods exist and both the overt and covert factors which may steer a researcher towards a particular choice. In doing so it challenges the reader's preconceptions of quantitative/ qualitative methodological approaches and prompts the reader to question why they use the methodology that they themselves do.

This is followed in the second section by three chapters detailing different approaches to combining qualitative and quantitative methods. The first of these, by Harre and Crystal, discusses the use of quantitative approaches with discursive analysis data. This is followed by Clarke's chapter on the respective restrictions faced by purely qualitative or purely quantitative studies. Although these are basic points which most readers will be familiar with it is nevertheless well written and reinforces the underlying message of the limitations of relying solely on one approach. The final chapter of the second section is slightly more specialised and discusses the potential role of qualitative methods in confirmatory factor analysis, in addition to coining the phrase qualiquantology.

The third section of the book follows up on the approaches discussed in section two with some practical examples of studies which combine methodologies. These are used to evidence the comments on combining methodologies made in the previous sections and do provide persuasive examples. One particularly notable and potentially very useful part of this section, Todd and Lobeck's chapter on survey and focus group research, addresses the problems a researcher may face when combined methodological approaches produce conflicting results.

The fourth and final section of the book discusses using different methodological approaches on a larger scale by comparing qualitative and quantitative studies into the same phenomena. As this section stresses, a combination of methodologies need not necessarily occur in the same study for the benefits of a mixed approach to be gained. Instead it is possible to look at the mixture of quantitative and qualitative studies which will almost invariably exist (to varying degrees) in the literature of a topic and draw together the findings from both approaches. This is particularly well exemplified in Nicolson's chapter on postnatal depression which uses a more personal tone and quotes from interviews to illustrate how qualitative studies can complement and challenge a large pre-existing body of literature derived from quantitative sources. The book finishes with suggestions for future research which combines methodologies, with the well made point that questioning our methodological approaches can often act as a catalyst for improvement in the field.

Mixing Methods achieves a rare feat for a book on methodology – it is genuinely engaging and enjoyable to read. Like any edited book with multiple authors it inevitably has some variation in the quality of writing between chapters and is slightly geared towards some psychologists – such as clinical – more than others. For tutors or lecturers of a psychology research methodology course it certainly provides a wealth of information to help start discussion on the often asked but deceptively difficult question as to what the difference is between quantitative and qualitative approaches. It is however perhaps not a book necessarily suited for those new to research methodology. The authors do introduce and explain the more basic concepts of research well but a reader with practical research design experience will arguably

benefit far more from this book than one without such experience. Nevertheless it is an interesting and thought provoking text which even the most experienced researcher will find useful.

Tips on…getting your written message across
Professor Mark Griffiths, Psychology Division, Nottingham Trent University

In academic life there are many different types of things that we have to write. This can range from a simple memo to a full blown academic journal article or book. Here are a few very general tips in the writing process.

Know your reader – It does not matter what type of communication it is, whether it is an academic paper, a memo, a technical document, an e-mail etc., it is important to know who the reader is. This will dictate style, tone, formality etc.

Have a rough plan in your head before writing – If before writing you have an idea of the main points you wish to address, you will find it easier to get writing. It may be helpful to group similar ideas together to produce a more cohesive bit of writing.

Break up ideas into chunks – To avoid "information overload", it is often said there should be only one subject or issue per paragraph (unless you are writing out a list). Complicated ideas or concepts can still be communicated in short sentences to help the reader assimilate the content.

Use jargon only where necessary – Avoid the use of technical jargon unless the writing specifically demands it.

Proof read documents backwards at least once – This makes it easier to spot glaring spelling mistakes. Reading text in a normal way often makes words

which are spelled incorrectly appear correct as the reader knows what to expect.

Beware the spell checker – Spell checkers, while useful, can still fail to spot incorrect words if they are words spelt correctly in other circumstances (affect/effect, there/their, practice/practise etc.). Use a dictionary if you have to!

3rd International Association of Behavior Analysis Conference: Beijing, China, 25 -27th November 2005

Vicky Lovett, University of Wales, Bangor

Going to Beijing, I wasn't sure what to expect, having neither attended a conference before, nor visited China. However, arriving on the first day of the conference I found it all to be a welcome surprise. The evening reception started the conference with a bang. The Chinese organisers welcomed us warmly with entertainment that included Chinese dancers, acrobats and musicians. The conference was based in the Kerry Hotel, whose wonderful staff ensured all the events were organised beautifully with great decorations and traditional Chinese food.

Holding the 3rd international ABA conference in China was an interesting concept. Behaviour Analysis in China could be considered to still be in its infancy and I think Dr Yanging Guo (Institute of Mental Health, Peking University: People's Republic of China) said it best, "Behaviour Analysis in China is like a toddler learning to find its feet".

The conference was broken down into invited talks, symposia and workshops occurring in parallel sessions. I was impressed by the variety of talks, ranging from an abundance of applied topics, e.g. presentations on autism, early intervention, and developmental disabilities, to more experimental topics such as verbal acquisition and computerised reinforcement schedules. It was interesting to see that an effort had been

made to make the conference more widely available for Chinese delegates with many of the talks translated into Mandarin.

One invited symposium I thoroughly enjoyed was 'Behavior Analysis around the World'. Contributions to this symposium were made by representatives from Australia, Israel, Japan, Mexico, People's Republic of China, Republic of Korea, United Kingdom and ABA International. This was a wonderful talk showing that Behaviorism is thriving around the world and is not a discipline found solely in America. The symposium really made clear the wide variety of research being conducted across the world. I was particularly impressed by Dr. Pauline Horne (University of Wales, Bangor) who discussed the large amount of Behaviour Analytic research being conducted here in the UK.

The poster sessions were heaving, with presentations in both Mandarin and English. Many volunteers were found working as interpreters, which was particularly useful when talking with some of the Chinese delegates. I did not have much opportunity to look at the posters as I was helping present posters for the Wales Centre for Behaviour Analysis. I did manage to meet a lot of new people at the poster session, but I feel this part of the conference program could have been organized better. Presenting delegates had no time, either pre- or post-session to look at other posters. It would have been useful if there had been an arrangement to view the posters before or after the main session.

My own talk, 'Can 6 month olds imitate?', was on the last day as part of a symposium on imitation. As this was the first time I had attended a conference, I was naturally concerned about what my peers might think of my work. Luckily, I received a very positive response. I found the discussions after the talk to be wholly rewarding and now feel much more confident answering questions relating to my research which, prior to this event, was something I had been apprehensive about. The whole experience proved to be rather thought provoking, as some of the questions made me consider some of the wider implications of my research. For this reason, if for no other, I believe presenting at conferences should be an essential part of a postgraduate student's program, something that PsyPAG helps to promote.

Aside from the academic part of the conference, it was great to meet new people but also good to catch up with old friends. I was lucky enough to again meet Professor Ono from Komazawa University, who had been at my university for a year. At the conference he introduced me to some of his students and colleagues. It was wonderful to hear of the work they are doing in Japan. One of his students, Michiko, helped me greatly with table etiquette in China, and we have remained in contact since the conference.

On leaving the conference I felt sad and relieved. It had gone well, and it would have been nice to stay longer, but the flight back to the UK was a long one. However, I did not have the awful task faced by some of the American delegates, who would actually land in America earlier than they had left China. Amazing, the wonders of international travel.

Postgraduate Occupational Psychology Conference

9 & 10 January 2007, preceding the Annual DOP Conference
Marriott City Centre Hotel, Bristol

Are You:

• An MSc or PhD student in the area of Occupational Psychology?
• Interested in a career in Occupational Psychology?
• Wanting to meet like-minded colleagues and potential employers?
• Hoping to present your research at a prestigious conference?

**IF YOU ANSWERED YES TO ANY OF THE ABOVE,
THE POP CONFERENCE IS NOT TO BE MISSED!**

Interactive skills **WORKSHOPS**:
- Q & A Expert Panel "Everything you wanted to know about being an Occupational Psychologist"
- Selling Skills Workshop
- Stress Risk Assessment - What is it and how do you do it?
- An Introduction to Coaching and its Principles
- How to Get Published
- Facilitation Skills Workshop
- Chartership Workshop

There will also be a **CAREER** networking event, and postgraduate **RESEARCH** presentations and posters.

KEYNOTE SPEAKERS:
- Professor Catherine Cassell, Manchester Business School, University of Manchester
- Dr Phil Leather, Institute of Work, Health and Organisations, University of Nottingham

A **DINNER on board the Glassboat** and **QUIZ** will be held on the evening of the 9th January

REGISTRATION FEES from £85. Opportunity to pay in two instalments.

Join us for what promises to be a fascinating and informative event!

For further information, submission details and a registration form visit our
website www.bps.org.uk/pop2007

PsyPAG in the 1980s : A personal reflection

Professor Mark Griffiths, Psychology Division, Nottingham Trent University

Back in 1989, a year and a half into my PhD at the University of Exeter, I was elected as the Postgraduate Representative for the BPS Social Psychology Section, taking over from Jonathan Smith (now – as you are probably aware - the country's leading expert on Interpretative Phenomenological Analysis!). What I didn't know following my election was that I would have to attend meetings of the recently formed Psychology Postgraduate Affairs Group (PsyPAG) which had been in operation for two or three years.

I was vaguely aware of PsyPAG before attending my first meeting at University of East London. Before the *PsyPAG Quarterly,* there used to be something called the *Psychology Postgraduate Newsletter*. I managed to get an article published in Issue 9 of the *Psychology Postgraduate Newsletter* in 1988 (something on the BPS PhD guidelines) which turned out to be the very last issue before evolving into the *PsyPAG Quarterly.*

At that first meeting I met a lot of individuals – many of whom I have now completely lost touch with. At that point, there wasn't a Chair. All meetings were chaired by the Secretary/Treasurer, which at that point was a woman called Donna Coleston (University of Leeds). I have no idea whatever happened to Donna. I heard she had dropped out of Psychology completely. She was one of the friendliest people I ever met and made me feel very welcome on the committee. I inherited her approach and would always try to be as friendly as possible to all new incoming members on the committee.

During 1989 and 1990 I became very active on behalf of PsyPAG (some might say evangelical) and by Issue 5 of the *Psy-PAG Quarterly* I took over as the editor from Guy Sutton (Manchester Metropolitan University). Even back then I was constantly publishing things and I certainly (ab)used my position as editor to disseminate as much as possible. I also persuaded my fellow Exeter postgraduates to come onto the committee and by the beginning of 1990. My friends Gary Calderwood (Occupational Section Rep) and James Lang

(Developmental Section Rep) were elected. By this time, Liz Hellier (University of Plymouth) had become the Secretary/Treasurer. We were a very close knit team and were heavy socialisers. I made some great friends at the time including Renee Bleau (University of Strathclyde) and Caroline Bell (University of East London). I spent many a drunken night with fellow PsyPAGers putting the world of psychology to rights. Great days (and even better nights).

I certainly played up my PsyPAG duties on my CV as I started to apply for psychology lectureships in May 1990 and was pleased that I got interviews for all five jobs that I applied for. Even at that stage, I had a lengthy CV of publications padded out with many of my *Psy-PAG Quarterly* writings. I'm sure this helped swing things in my favour and by June 1990 I had been offered a full-time temporary psychology lectureship by the University of Plymouth at 23 years of age.

Although I ceased to be a postgraduate in 1990, I still actively write for the postgraduate community and constantly publish advice type articles in a whole host of outlets including the THES, BMJ, BMJ Careers and (of course) the PsyPAG Quarterly. I had a truly wonderful time as a PsyPAG committee member and I honestly believe it provided me with a springboard for future success. I have always been told that you get back out of life what you put in to it. Well, I was highly pro-active with PsyPAG and I will always be grateful for the chance that I had to contribute.

Organising a PsyPAG conference

Rachel Pye, **University of Reading**

I slept for thirteen hours straight last Thursday night. The previous three days had been hectic, as co-organiser of this year's PsyPAG conference. But it really wasn't as stressful as you would think. There had been a couple of busy periods earlier – around the abstract submission and registration deadlines, but by the time the conference came around, everything that could be anticipated was arranged, and the unexpected – well we'd just have to cross those bridges when we reached them.

Natalie had suggested we organise the 2006 conference at last year's conference dinner. It seemed a really good idea at the time, but then we had just imbibed a fair amount of wine. Come the next day, the idea was still there, and we started to seriously think about whether it was possible. We spoke to the head of the psychology department, Judi Ellis, who was very supportive, as were other PGs in the department. So we decided to propose Reading as the venue for the committee meeting in October. Luckily, the committee were also very supportive, and it was official. The 2006 annual conference would be held at Reading.

Of course, there was some bureaucracy, mainly with the finance department; we needed an internal account in order to receive the university discounts, but as PGs our status was a bit woolly. But once the initial organisation had been done, there wasn't much to do for a while. We posted the forms and deadlines on the conference mini-site, and waited for the submissions to arrive.

We received a fair number of submissions, though fewer than we'd hoped for. In total, there were around 65 delegates, many of these presenters. The timing may have been a problem – we had chosen the 25th–27th July as it was the first week of the school holidays, and therefore wouldn't coincide with my PhD testing (most of which is school-based), but of course anyone with children will have found these dates difficult. The Exeter conference had

taken place toward the end of August, which the PsyPAG committee had been concerned about. However, taking into account the difference in access for Exeter and Reading, I think Exeter had a higher attendance, and the timing may have been a factor (it's also much, much prettier!).

The conference ran quite smoothly – despite Thames Water cutting off the water supply to the campus on one of the hottest days of the year! The heat was something we hadn't anticipated, and though we moved the parallel session from the hottest seminar room into the air-conditioned lecture theatre, there was nowhere to put the other session. We haven't analysed the feedback yet, and of course my view is rather biased, but it seemed that people enjoyed the conference, and more importantly found it useful. We introduced a feedback system this year, whereby people giving oral presentations were watched by two raters, looking at movement, clarity, structure of the talk etc. This is definitely something that could be improved upon – maybe by videoing sessions so presenters can see for themselves the points raised. After all, the point of the PsyPAG conference is to allow PGs a chance to practise presenting to an audience, as well as networking with others in a similar situation. The social side of the conference has always been consistently high, but we need to make the experience as useful as possible.

Workshops this year were held by the Postgraduates Who Teach (PGwT) network, talking about how to teach and mark work effectively; and Doug Brown of the BPS who spoke to us about why we should work with rather than against the media. Workshops are an important part of the conference, as they add another practical learning aspect for delegates. Our keynote speakers were also well received. All were young researchers, excelling in their field, and I hope people were inspired by them. A big thank you goes to Emily Farran (Reading), Andy Field (Sussex) and Michelle Ryan (Exeter), not only for their fantastic talks, but also for their willingness to speak at the conference, to join in with the social events, and for Andy and Michelle, to stay in the university accommodation when they didn't have to!

If we're thanking people, then I have to thank the three MSc students Samantha Johnson, Naomi Andrew and Nafsika Limniaou, who manned the reception desk, and were just generally brilliant throughout the conference.

Without their proactiveness, and willingness to muck in, Natalie and I would have found the experience a lot more difficult. We have to thank Glen Pennington, PsyPAG Communication Officer, for all her help, and Dave Moore, PsyPAG Chair, who worked really hard behind the scenes securing alternative funding, which enabled us to keep the cost for delegates as low as possible. Natalie deserves a massive applause – her chilledness overcame my natural tendency to stress, and she really made it an enjoyable experience.

All in all, I'm really glad Natalie had that extra glass last year. There have been busy periods, but the satisfaction of knowing that I can organise a three-day conference is worth it. And I'm sure this will give my CV a boost!

Think you can do it?

The PsyPAG committee vote on conference proposals at the October meeting – do you think you could organise the 2007 conference?

You need the support of your department – Reading let us use seminar rooms for the parallel sessions free of charge, use the staff room for the wine reception, and also contributed financially. All requests were considered.

A strong organising team is essential. Natalie and I worked well together: I'm the stressy one and she's laid back. We're both relatively well organised, and allocated the different responsibilities according to our strengths. You need a good group to cover the reception desk too, as during the conference there are lots of little things that need doing.

You need to keep the cost to the delegate as low as possible. So you need to keep room hire as low as possible, and plan cheap but acceptable accommodation. You also need to think about travel costs – is your university relatively easy to get to?

Who would you invite to be a keynote speaker, and do they fit into a general theme? What workshops would you organise?

If you're interested, you need to put a proposal together by the end of September, with full costing. Please contact me (vicechair@psypag.co.uk) for more details. Good luck!

Dates for the Diary

From	To	Title	Booking information
28th Sept '06	28th Sept '06	Creating Inclusive Organisations	DOP Learning a Living website http://www.bps.org.uk/dopws06/bookings/bookings_home.cfm
29th Sept '06	29th Sept '06	Mindfulness-based Cognitive Therapy	Margarita Radon 07860 229815
30th Sept '06	2nd Oct '06	Food for the Brain Conference	http://www.foodforthebrain.org/content.asp?id_Content=1648
3rd Oct 06	3rd Oct '06	Promoting Mental Health and Well-being in Communities: Psychological Perspectives	http://www.bps.org.uk/document-download-area/document-download$.cfm?file_uuid=AF299E58-1143-DFD0-7ED4-6AFC15052CB5&ext=pdf
4th Oct '06	4th Oct '06	Emotional Intelligence in the Workplace: Using the EBW	DOP Learning a Living website http://www.bps.org.uk/dopws06/bookings/bookings_home.cfm
24th Oct '06	24th Oct '06	Establishing a CMHT Network	Sarah Dilks, on sarah.dilks@slam.nhs.uk; Telephone: 0207 525 2751.
6th Nov '06	8th Nov '06	GTiCP Annual Conference	http://www.bps.org.uk/document-download-area/document-download$.cfm?file_uuid=B2F211FC-1143-DFD0-7EEE-F467969C4B8D&ext=doc
9th Nov '06	9th Nov '06	The British Psychological Society Edinburgh Lectures	http://www.bps.org.uk/document-download-area/document-download$.cfm?file_uuid=AECBD4D2-1143-DFD0-7EF4-84B6BDDC255B&ext=pdf
10th Nov '06	12 Nov '06	Education Section Annual Conference	www.bps.org.uk/education
17th Nov '06	17th Nov '06	DCP SIGOPAC Screening for distress	Dr. Shirley Anderson shirley.anderson@fvpc.scot.nhs.uk
4th Dec '06	4th Dec '06	The British Psychological Society London Lectures	http://www.bps.org.uk/document-download-area/document-download$.cfm?file_uuid=AECC76FD-1143-DFD0-7E2C-AA3E5DC8A4E9&ext=pdf

Organise the 2007 Annual PsyPAG Conference!

It's not as hard as you might think!

The PsyPAG committee are calling for proposals to host the 2007 PsyPAG Annual Conference.   You don't need to be a committee member; all you need is willingness and a good plan.

PsyPAG Committee 2006-2007

POSITION	POSITION CURRENTLY HELD BY …	DATE ELECTED	DATE POSITION IS DUE FOR ELECTION
Core Committee Members			
Chair	David Moore	April 2006	April 2008
Vice-Chair			
Communications Officer	Glenda Pennington	AGM 2006	AGM 2008
Information Officer	Julie Freeborn	AGM 2006	AGM 2008
Treasurer	Anthony Moss	AGM 2006	AGM 2008
Quarterly Editors (up to 6 positions)	Alexa Ispas	AGM 2005	AGM 2007
	Paul Hutchings	AGM 2006	AGM 2008
	Damien Williams	C/O 10/06	AGM 2007
	Ross Lorimer	C/O 10/06	AGM 2007
	Daniel Rhind	C/O 10/06	AGM 2007
Division Representatives			
Division of Clinical Psychology	Kiran Hans	C/O 12/05	N/A
Division of Counselling Psychology (DCoP)	Silvia Pimentel	AGM 2006	AGM 2008
Division of Educational and Child Psychology	Irina Roncaglia	AGM 2005	AGM 2007
Scottish Division of Educational Psychology	POSITION VACANT		
Division for Teachers and Researchers in Psychology	Dimitrios Tsivrikos		
Division of Forensic Psychology	POSITION VACANT		
Division of Health Psychology	Angel Chater	AGM 2006	AGM 2008
Division of Neuropsychology	Jo Fludder	AGM 2005	AGM 2007
Division of Occupational Psychology	Catherine Steele	AGM 2005	AGM 2007

POSITION	POSITION CURRENTLY HELD BY …	DATE ELECTED	DATE POSITION IS DUE FOR ELECTION
Division of Sport and Exercise Psychology	Andrew Manley	AGM 2006	AGM 2008
Section Representatives			
Cognitive Psychology Section	Rachel Pye	AGM 2005	AGM 2007
Consciousness & Experiential Psychology Section	POSITION VACANT		
Developmental Psychology Section	George Koulieris	C/O 10/2005	AGM 2006
History and Philosophy of Psychology Section	POSITION VACANT		
Lesbian & Gay Psychology Section	POSITION VACANT		
Mathematical, Statistical and Computing Section	Gillian Smith	AGM 2006	AGM 2008
Psychobiology Section	Julia Santomauro	AGM 2005	AGM 2007
Psychology of Education Section	Edward Sosu	AGM 2006	AGM 2008
Psychology of Women Section	POSITION VACANT		
Psychotherapy Section	Angel Chater	AGM 2006	AGM 2008
Qualitative Methods Section	Sue Alexander	AGM 2005	AGM 2007
Social Psychology Section	Gamze Baray	AGM 2005	AGM 2007
Transpersonal Psychology Section	Alasdair Gordon-Finlayson	AGM 2005	AGM 2007
Board Representatives			
Membership and Professional Training Board	POSITION VACANT		
Publications and Communications Board	Melissa Wallace	C/O 10/06	AGM 2007
Research Board (Chair + 1 other position)	Dave Moore Julie Freeborn	AGM 2006 AGM 2006	AGM 2008 AGM 2008
Other Committees			

POSITION	POSITION CURRENTLY HELD BY …	DATE ELECTED	DATE POSITION IS DUE FOR ELECTION
Conference Standing Committee	Rosie Meek	AGM 2005	AGM 2007
International Committee	Alexa Ispas	AGM 2005	AGM 2007
National Postgraduate Committee	Natalie Lynch	AGM 2006	AGM 2008
PPC Rep	Alexa Ispas		
The Standing Committee for the Promotion of Equal Opportunities	POSITION VACANT		
Special Interest Group of Coaching Psychologists	Sandra Haase	AGM 2005	AGM 2007
Undergraduate Liaison Officer & Student Members Group (SMG)	POSITION VACANT		
Branch Representatives			
North East of England Branch	POSITION VACANT		
North West of England Branch	Heather Wood	AGM 2005	AGM 2007
Northern Ireland Branch	Gillian Smith	AGM 2005	AGM 2007
Scottish Branch	Jo Edson	C/O 10/06	AGM 2007
South West of England Branch	POSITION VACANT		
Welsh Branch	Emma McDonald	AGM 2006	AGM 2008
Wessex and Wight Branch	Dally Virk	AGM 2006	AGM 2008
West Midlands Branch	Catherine Steele	AGM 2005	AGM 2007
Home Counties Development Officer	Angel Chater	AGM 2005	AGM 2007
London Branch Development Officer	Jason Codner	AGM 2005	AGM 2007
Coaching Psychology	Sandra Haase	AGM 2005	AGM 2007