



## Editor's Note: What the Journal Publishes: The Journal's Mission

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It is with enthusiasm and great humility that I take on the editorship of *Applied Psychophysiology and Biofeedback*. I have been doing research in the area of applied psychophysiology for over 50 years, and have published more of my work in this journal than anywhere else. Some of my papers undoubtedly could have been published in journals with overall higher impact factors, as evidenced by the fact that these are among the widely cited papers of our journal. I published here because this journal best represents my professional and scholarly interests, and I encourage other readers similarly to think of publishing their best work here.

In addition to compatibility with our interests, there are other reasons for wanting to support our journal in this way. As a field, despite its long tradition of respect for scientific validation, applied psychophysiology and biofeedback are still often thought of as “alternative” approaches to treatment, sometimes as fringy. Only a continuing track record of high-quality research will change this perception. For members of the Association for Applied Psychophysiology and Biofeedback, our sponsoring organization, an important goal is to gain acceptance by organizations in various areas of application, including medical societies that publish treatment guidelines for various diseases and disorders and various psychological, educational, and athletic organizations. Where the data warrant it, applied psychophysiology should become recommended and sometimes first-line interventions. The job of our journal is to provide a place from where those data can be disseminated.

At the outset, I should say that I am only interested in publishing papers that involve applied psychophysiology. However, “applied psychophysiology” requires a definition. Biofeedback is certainly “psychophysiological,” but so are muscle relaxation and breathing exercises. What about meditation or mindfulness exercises that may target some of the

processes involved in neurofeedback? What about studies showing psychophysiological effects of cognitive behavior therapy? What about studies of psychophysiological processes that have only a tenuous relationship to anything applied? Here a judgment call is required. I will seriously consider all such studies, and be open to opinions from the readership about them.

However, despite professional as well as scholarly dedication to our field, we must guard against being regarded as a promoter of applied psychophysiology and biofeedback, rather than as a venue for dispassionate presentation of scientific data. With that in mind, I welcome publication of well designed studies showing *failure* to achieve significant effects as well as those showing successes, where studies are sufficiently powered to show this. If applied psychophysiology is ineffective in some applications, knowledge of this is just as important as knowledge of effectiveness in others. Studies finding successful applications are important for validating our field. Publication of studies finding lack of success will caution others not to spend their valuable time venturing into blind alleys, and not to offer ineffective interventions to their patients or clients.

Since lack of significance sometimes reflects more on the power of a study than on the phenomenon studied, I urge all contributors to report effect sizes as well as significance levels. Although highly powered studies are obviously preferable, smaller studies also can make meaningful contributions to the literature. Indeed, it is almost impossible for researchers in our field to get sufficient financial backing to conduct the type of Phase III trials that routinely are carried out for evaluating pharmaceutical agents, usually involving tens of thousands of study participants. Results of many small studies can be amalgamated in systematic reviews and, preferably, meta-analyses, combining the effects of many smaller trials, some of which are carried out as doctoral dissertations or student research projects. Also successful small trials will encourage scientists to repeat them to cross validate the findings.

The gold standard for outcome studies is the randomly controlled trial. Studies using convenience samples of

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people treated differently in general practice are much less valuable. Design of proper controls for biofeedback studies is tricky, often with no perfect standard. Some control conditions, such as paced breathing at faster rates or thinking of body sensations without instructions to control them, can produce a meditative or relaxation effect. Others, such as false biofeedback, can be frustrating and may lead to arousal. The choice of control condition depends on what one is controlling *for*. It may be reasonable to control for passage of time and for the effect of going through evaluation procedures multiple times. Here a simple “no treatment” control is sufficient. Control for expectancy or for the fact that one is engaging in an experimental study (the “Hawthorne” effect) is more complicated. Some real or ersatz treatment is called for. Comparison with a treatment of known effect is a reasonable procedure, but requires a test of equivalence for the effects of the two methods. Superiority of biofeedback over effects of another treatment of proven effectiveness would be a particularly powerful findings. A still more complicated control is needed where it is important to detect a specific biofeedback mechanism for the outcome effects. In HRV biofeedback research, we sometimes may be interested in determining whether the effects are due to specific stimulation of the baroreflex or the vagus nerve system by biofeedback. Here a comparative biofeedback-like procedure may be necessary, such as paced breathing at higher rates, in order to rule out the possibility that effects are due to other mechanisms that might be inherent in the biofeedback procedure, e.g., meditative concentration on body processes or specific effects of other biofeedback procedures.

Sometimes single or multiple case studies may be reportable. In my judgment, only studies demonstrating a completely novel biofeedback method or application to a disorder that never has previously been studied would warrant publication in an archival journal such as *Applied Psychophysiology and Biofeedback*. Replications of previously reported research also can be useful in order to show the reliability of effects.

I highly encourage submission of systematic literature reviews with meta-analyses of randomly controlled trials. The limitations of this methodology are well-known, for example difficulty interpreting combinations of “apples and oranges” in methodologies and outcome measures. A well-designed and highly powered controlled outcome study can sometimes tell us more than an amalgam of many poorly done studies. However, meta-analysis methodology has sufficiently advanced so that limitations of the method are minimized. Meta analyses should include an unbiased estimate of effect size (usually Hedges  $g$ ), measures of dispersion (the  $Q$  statistic), calculation of random (vs. fixed) effects, both forest and funnel plots, interpretation of outlying studies, a statistic showing whether dispersion is interpretable (the  $I$  square statistic), and the 95% prediction interval within which the effect sizes for a future study would be predicted to fall (calculated from tau or

tau squared). Also, estimation of the clinical significance of outcome effects is important. A meta-analysis could include results from thousands of study participants, such that very small and clinically inconsequential changes could render a statistically significant finding.

In my first few months of editorship I also have been faced with an additional editorial decision, concerning studies that do not report treatment outcomes, but show psychophysiological correlates of important disease, emotional, or functional conditions. This, I believe, is a judgment call. I would not be interested in publishing studies on relationships that are already well established, e.g., the use of EEG recordings to diagnose epilepsy. On the other hand, the use of EEG, HRV, or other psychophysiological methods to diagnose emotional problems may be of interest, particularly if a study provides guidance to clinicians for determining the usefulness for modifying these psychophysiological parameters.

As a final note, I strongly recommend that supervisors and mentors instill in their students a sense of duty to publish their work, even after their course and degree requirements have been fulfilled and even when they do not intend to pursue research careers. I think of research as an almost sacred activity for those who engage in it, not to be taken lightly as a mere way station to an academic degree or clinical career. The end product for such work should never be just to fulfill a course or degree requirement. It should be a publication that lets the entire scientific community know what has been found. Students should know this prior to starting their projects.

With these considerations in mind, I particularly welcome randomly controlled studies and meta-analyses of all applications of applied psychophysiology and biofeedback. For completely novel applications, I will seriously consider studies with convenience samples as controls or multiple case studies. I also will seriously consider replication studies and studies of psychophysiological correlates of disease, emotion, or functioning. Only under very unusual circumstances will I consider individual case studies or studies without psychophysiology as part of an intervention or as an outcome measure. The bottom line for accepting all studies is the quality of the science: study design, appropriateness of outcome measures and use of statistics, accuracy of interpretation, honesty and integrity of the research team, appropriate protection of human beings (along with certification by an appropriate certifying board), use of APA style, and presentation of material in clear, concise, and grammatically perfect scientific English.

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