Clinical Use of Meditation as a Self-Regulation Strategy: Comments on Holmes's Conclusions and Implications

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In a review of the experimental evidence on meditation and somatic arousal reduction, Holmes (January 1984) drew two primary conclusions and implications: (a) The personal and professional use of meditation as an antidote for high somatic arousal is not justified by the existing research data (p. 8), and (b) there is no evidence that meditation is more effective for reducing somatic arousal than simple resting (p. 9).

This review has a vital and important bearing on the clinical use of meditation, the role of meditation as a treatment of choice compared to other self-control strategies, and general clinical implications for stress management. Based on Holmes's review, one would be reluctant to use meditation for treating hyperarousal and quite wary of whether it had any clinical utility at all as a relaxation or stress management self-regulation strategy.

Because I previously reviewed the same literature and arrived at somewhat different conclusions and very different implications, it seemed important to carefully compare Holmes's conclusions and implications to mine to see where there are similarities and where and why there are divergences.

In a previous work (Shapiro, 1980, Ch. 5), the clinical and physiological experimental evidence on meditation as a self-regulation strategy was reviewed. This review was extended, refined, and published as a cover article in the *American Journal of Psychiatry* (Shapiro, 1982). In that review, meditation was compared with other clinical self-control strategies including biofeedback, hypnosis, and progressive relaxation, among others.

The following conclusions were reached regarding the physiological experimental evidence: (a) It seems clear that meditation can bring about a generalized reduction in many physiological systems, thereby creating a state of relaxation; (b) the original belief that we would be able to discriminate meditation as a unique physiological state has not been confirmed—on either an autonomic or metabolic level, or in terms of EEG patterns; (c) it is not clear from the available data that this state is differentiated from the effects of other relaxation techniques, whether they be hypnosis or deep muscle relaxation; and (d) most studies have found that the constellation of changes is significantly different between meditation groups and placebo control groups, but not between meditation and other self-regulation treatments.

In terms of clinical comparisons between meditation and other self-regulation strategies, the following was concluded: Compared to other self-regulation strategies (including progressive relaxation, Benson's relaxation response, a pseudomeditation treatment, anti-meditation treatments, self-administered systematic desensitization, and cardiovascular and neuromuscular biofeedback), meditation appears to be equally but no more effective. This is true for clinical problems such as anxiety, anxiety in alcoholics, alcohol consumption, insomnia, and borderline hypertension.

What Are the Similarities and Differences in These Two Reviews, and What Accounts for the Latter?

There are similarities in the two reviews in that Holmes and I both acknowledged (a) the arousal reduction aspect of meditation and (b) that meditation does not appear to be unique as a self-regulation strategy (my term) or compared to simple resting (Holmes's term). What, then, are my concerns about Holmes's article? Why are there specific differences even though similar material was reviewed?

I see three problems with Holmes's article. First, he appeared to be as biased negatively in his interpretation of meditation as a self-regulation strategy as previous claims have been biased in favor of its usage. As is noted below, several times Holmes made "subjective" interpretations in trying to explain away data that did not fit his thesis. Second, the clinical interpretations of his review are overgeneralized and misleading, based, I believe, on an apparent insensitivity to clinical issues and applied clinical research in self-control strategies. Third, there is a seeming insensitivity to meditation as an independent variable (i.e., the different types of meditation) as evidenced by his own 1983 study (Holmes, Solomon, Cappo, & Greenberg, 1983), and there also seem to be difficulties in terms of what is legitimately defined as a proper control group when meditation is being researched as a self-regulation strategy.

To list some of my specific concerns, in a footnote on the first page of his article, Holmes dismissed previous reviews of the literature by saying that

> unfortunately, the conclusions drawn in previous reviews generally cannot be accepted because the authors were selective in the investigations they cited; disregarded methodological problems in drawing conclusions from investigations; and/or indiscriminately mixed results of case studies, uncontrolled investigations, and appropriately controlled experiments in drawing their conclusions. (p. 1)

Because one of the reviews that he cited was a book I authored (Shapiro, 1980)[1], I have some difficulty believing that Holmes, in fact, read this book, because his concerns are exactly the points that we made in Chapter 5 dealing with meditation as a self-regulation strategy. In fact, my colleagues, David Giber and Roger Walsh, and I presented tables detailing methodological problems of every study published about meditation and addressing the very points that Holmes made. Further, Shapiro (1982) is not cited, and the work of Woolfolk (1975), which is a very careful physiological and methodological review of the literature, and the work of Smith (1975), which is a careful review of meditation and psychotherapy, are also likewise dismissed. This global dismissal of all prior reviews does not appear well founded.

In addition, when actually look-

[1] Although perhaps only a trivial error, in the interest of accuracy, it should be noted that Holmes mistakenly cited the reference to this volume as Chicago: Aldine, whereas the correct citation is New York: Aldine.

ing at studies that supported the efficacy of meditation as compared to resting, in every case Holmes dismissed the positive data with rhetoric rather than concrete evidence. All he provided in refutation were post hoc speculations.

Several examples (and there are many more) follow (emphasis mine):

1. In discussing the study by Elson, Hauri, and Cunis (1977), Holmes wrote that "the decline in arousal evidenced by meditating subjects *can probably* be attributed to regression to the mean" (p. 4).

2. In reviewing the work of Orme-Johnson (1973), Holmes said "It is *most likely that* the effects were due to subject selection factors" (p. 4).

3. When discussing the work of Parker, Gilbert, and Thoreson (1978), Holmes stated, "*One must question* the replicability and generalizability of the one set of positive findings" (p. 5).

4. Holmes stated, "EMG activity was assessed in four experiments, but *only two* of those provided evidence that meditating subjects experienced less muscle tension than did resting subjects" (p. 5). (Why say *only two*, rather than one-half, or 2 of 4?)

5. Regarding Fenwick et al.'s (1977) study on oxygen consumption, Holmes wrote, "but in that case, the control subjects listened to classical music rather than simply resting and *it is likely* that the music influenced respiration" (p. 5).

I am concerned that every time there was a difference that favored meditation, Holmes found an alternative negative explanation. His views were merely subjective interpretations, in a sense no different from those efforts by researchers with a different cognitive set who previously tried to show that meditation was a totally unique strategy.

In addition to a negative series of interpretations, I believe he also misstated information. For example, when he looked at the biochemical factors, Holmes said, "Overall then, these findings do not provide evidence that meditation reduces arousal as measured by various biochemical factors" (p. 5). This, in fact, is not true. What Holmes was trying to say is that meditation *does in fact* lower arousal, but from the experimental evidence does not lower it any more than other self-control strategies as measured by physiological measures. This is quite a different finding, and my belief is that it causes us to ask a different set of questions.

Another problem I had with Holmes's article is that he was often very selective in terms of the data to which he attended. For example, when he discussed Goleman and Schwartz's (1976) article, he did not include one of their major findings; that is, the quickness in reaction time with which meditators returned to baseline conditions compared to controls. When Holmes discussed Kirsch and Henry's (1979) work, he looked only at the variable of heart rate and concluded that "these findings did not provide any evidence for the utility of meditation for controlling arousal in threatening situations" (p. 7). In fact, the main thrust of Kirsch and Henry's study was quite the contrary. Their study was one of many that showed that improvement occurred in all three treatment groups (desensitization/relaxation, desensitization/meditation, and meditation only) according to self-report and behavioral measures, and although there were no significant differences in improvement among the three treatment groups, this improvement was significantly greater than improvement in the no-treatment control group. This interpretation of the study is significantly different from Holmes's interpretation.

Part of the problem may be that Holmes seemed to be using those "simply resting" as a control group, whereas previous authors have used resting as one of the interventions to which meditation is compared. This is admittedly a difficult issue in terms of what one defines as a placebo control group. Was "just resting," as in the Holmes, Solomon, Cappo, and Greenberg (1983) study, a treatment rather than a true control group?[2] I believe it can be argued, as I have done elsewhere, that resting may in fact access a relaxation response similar to that which occurs during meditation. When one is instructed to "simply rest," one may choose to focus on one's breathing in a slow relaxed way or on a specific object of attention; or one may choose to use cognition, using phrases such as "Let yourself relax now" or "This is a time just to be quiet." Thus, I would argue that it becomes very difficult to call "simply resting" a placebo control group.

Summary, Implications, and Future Directions

I believe that Holmes's article raises some important points that need to be seriously addressed. It is a useful article in that it counters the hosannas with which meditation was initially greeted and does, in fact, make an effort to carefully evaluate the efficacy of meditation. I believe it errs in its negative bias in interpretation of data and in overstating the negative case for meditation's clinical use. For example, I do not believe that at this point any one self-control strategy, whether hypnosis, a cognitive behavioral strategy, meditation, or progressive relaxation, should be considered a unimodal treatment of choice. Holmes's citation of the study by Puente and Beiman (1980) speaks well to this point. It is a point that my colleague, Steven Zifferblatt, and I made over eight years ago (Shapiro, 1978; Shapiro & Zifferblatt, 1976), suggesting that meditation could be made more effective as a clinical strategy by combining it with some of the cognitive stress management, coping, and inoculation strategies (Meichenbaum, 1977; Suinn & Richardson,

[2] On the one hand, Holmes, Solomon, Cappo, and Greenberg's effort (1983) in many ways dealt very carefully with certain methodological problems of prior studies. However, there are certain areas that are not addressed in the study, including (a) subject selection, that is, how were the recruits selected, what was their motivation, and what were they hoping to learn; this has been shown to be a confounding variable in our studies; (b) whether there was a true control group; and (c) whether he mixed intervention strategies, that is, used not only transcendental meditation (TM) meditators (as an effortless meditation) but also Siddhi meditators (an advanced form of TM, which often involves effortful as opposed to effortless concentration). Further, Holmes et al.'s study supports that meditation does in fact decrease arousal (and one wonders whether it would have decreased more if the Siddhi meditators had not been a confounding variable in the intervention group). Finally, Holmes seemed to be generalizing from several nonclinical studies such as his own to psychotherapy and stress management. Generalizing from a nonclinical population must be done with extreme caution.

1971). Clearly, this is an important area for further controlled investigation.

A second important area for further investigation that follows from the above would be a component analysis of meditation to separate the active from the inert aspects (Shapiro, 1980, Ch. 8; Walsh, 1984). In other words, how much of meditation's effect is due to antecedent variables of preparation, environmental planning, and components of the behavior itself—physical posture, attentional focus and style, and breathing? Furthermore, by breaking meditation into its various components, it might be possible to determine which aspects of meditation might be profitably combined with other self-regulation strategies (cf. Woolfolk, 1984).

A third area for future research would involve attention to a more careful detailing of the issues involved in the clinical use of self-control strategies. As one example of this, a systems and cybernetic model has been detailed elsewhere (Shapiro, 1983a), illustrating how the following dimensions are relevant to applied clinical research on self-control strategies: motivation; individual responsibility; belief systems; adherance/compliance; matching of dependent variable (clinical problem) with independent variable (selection of intervention technique), including a very specific rationale between the two; relationship variables between therapist and patient; and belief systems, of both the therapist and the patient.

A final area for investigation—cognitive and subjective aspects of meditation—is one that Holmes acknowledged he did not look at, but instead referred readers to Shapiro, (1980) and Smith, (1975).

One of the original purposes of meditation, if we look at the philosophical and cultural context of the technique, was to create a deeper sensitivity to perceptual and cognitive stimuli (Kasamatsu & Hirai, 1966) and a profound change in a person's awareness and reaction to himself or herself, others, and the world. Research on meditation in order to determine cognitive, subjective, and attentional changes (i.e., meditation as an altered state of consciousness)—the phenomenology of meditation—is, I believe, quite an important area fcr further investigation (Shapiro & Walsh, 1984).

Despite methodological and conceptual problems (Walsh, 1980), this approach, valued by the Eastern traditions for centuries, is just beginning to gain some favor within the Western scientific community. For example, although Morse, Martin, Furst, and Dubin (1977) found that there were no significant differences in physiological responses to three relaxation states, they pointed out that there were significant differences in the subjects' evaluations of these states, as did Gilbert, Parker, and Claiborn (1978). Therefore, Morse et al. concurred with Charles Tart's remark that "in [the] subject's own estimate of his behavior, an internal state is a rich and promising source of data which some experimenters tend to ignore in their passionate search for objectivity" (cited in Morse et al., 1977, p. 323). Similarly, Curtis and Wessberg (1975/1976) noted that more positive subjective changes were reported by the meditation group than by the control relaxation group even though there was no difference on physiological measures. They suggested that if meditation has a unique effect, it seems to be different from a visceral or neuromuscular effect. Thus, the phenomenological or subjective experiences of meditation—meditation as an altered state of consciousness—may be an important and critical area for future scientific examination, and readers are referred to the following for more detailed reviews: Davidson (1976); Shapiro and Giber (1978); Shapiro (1980, Ch. 7); and Shapiro (1983b).

In summary, I believe that there is an important benefit that can be gained from the proper use of meditation as a clinical self-regulation strategy for arousal reduction, as well as an altered state of consciousness for "psychotherapy and personal growth." Setting aside the question of meditation's uniqueness, we are now confronted by the issue of developing more precision as to when to use meditation rather than other self-regulation strategies. Although meditation seems to be no more effective as a clinical intervention for arousal reduction than other self-regulation strategies, this is not a reason either to use or refrain from using meditation. It appears that we now have several self-regulation strategies that are more effective than controls in the alleviation of certain clinical problems. Our task is to be sensitive to possible adverse effects and contraindications (Otis, 1984) and to design more sophisticated and precise research strategies in order to help clarify which self-regulation strategy is the treatment of choice for which patient with what clinical problem. My hope is that a more unbiased review of the literature, neither nay-saying nor filled with hosannas, will help researchers evolve that necessary next step of methodological sophistication.

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