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Physiological Differences Between Transcendental Meditation and Rest

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The continuing discussion of the effect of meditation on reduction of somatic arousal, including the recent comments of Morrell (July 1986), has suggested modifications in the conclusions of Holmes (1984). Because of our own long-standing interest in the topic, this discussion has stimulated us to look more deeply and with a quantitative approach at the questions addressed by Holmes. The results of our investigation differ from those of Holmes and relate to points raised by Morrell.

The first question in this discussion is whether meditation produces a state of relaxation that is physiologically different from eyes-closed rest. We decided as a first step to compare, using meta-analysis procedures (Glass, McGaw, & Smith, 1981), only the effects of Transcendental Meditation (TM) technique with rest, because the large majority of studies on this topic use this technique and because discrimination can only be obscured by mixing studies of meditation techniques that may be quite different in methods and effects. Through a series of computer searches, we found all articles through 1985 in Psychological Abstracts, Science Citation Index, Social Science Citation Index, Index Medicus, and Sociological Abstracts that used the key words *meditation* or *relaxation response.* The reference section of each article was also reviewed for additional articles. Studies that assessed physiological effects of the TM technique were included.

In his previous review, Holmes (1984) included only studies in which meditation was compared with a resting control condition. However, because meta-analytic procedures allow estimation of effect sizes across studies, it is possible to use this approach to include all studies of the TM technique and to separately compute an effect size of meditation and rest based on all the available data. Because all studies monitored changes from a baseline period to a period of practice of the TM technique or eyes-closed rest, a within-subjects effect size can be calculated as the change in mean value from before the practice to during the practice, divided by the standard deviation of the prepractice period (Andrews, Guitar, & Howie, 1980; G. V. Glass, personal communication, April 30, 1987). This approach assumes a baseline period of comparable length among the studies that did and did not include resting controls. In the present sample this was in fact the case, $t(27) = 0.41$ (not significant), with one study from each group not specifying length; the overall mean baseline length was 14.6 minutes.

Effect sizes were computed for the following variables: basal skin resistance, spontaneous skin resistance responses, respiration rate, heart rate, and plasma lactate levels. These were variables for which there were at least five studies measuring the effects of the TM technique and three measuring the effects of eyes-closed rest, and for which the earliest studies of the technique showed significant effects (Orme-Johnson, 1973; Wallace, Benson, & Wilson, 1971). An additional set of between-subject effect sizes was calculated on those studies that included both meditation and rest control groups, comparing the difference in means during the premeditation or prerest control period. This approach was used to see whether TM meditators showed less physiological arousal than controls did prior to meditation or rest, possibly because of cumulative effects of the practice, and also to determine whether initial levels of the variables interact with the state changes reported during the practice.

There were 31 studies that included one of the five variables and for which effect sizes could be computed. A number of major studies were found that included both TM and rest control groups. These studies were published after the Holmes (1984) article was submitted (Bagga & Gandhi, 1983; Delmonte, 1984; Gallois, 1984; Jevning, Wilson, O'Halloran, & Walsh, 1983; Wolkove, Kreisman, Darragh, Cohen, & Frank, 1984). A complete list of studies included and the variables for which effect sizes were computed for each study can be obtained from the authors.[1]

As indicated in Table 1, the TM technique was associated with a significantly larger effect size than eyes-closed rest was for the variables of basal skin resistance, respiration rate, and plasma lactate. Heart rate and spontaneous skin resistance responses did not show significance, the latter because of large variability between studies and the former because it does not discriminate between practices even though it was the single most widely used variable. The results also indicate that those practicing the TM technique for some time show significantly lower baseline levels of spontaneous skin resistance responses, respiration rate, heart rate, and plasma lactate prior to meditation than comparison subjects do prior to rest.

The differences in results from those reported by Holmes (1984) warrant comment. One possible factor is the addition of more studies, some of which do report differences between subjects practicing the TM technique and control subjects on physiological variables. Another factor is the inherent weakness of a vote-counting approach to synthesizing research results. Hedges and Olkin (1980) noted the remarkable fact that if studies are divided into three groups (nonsignificant result or significant change in either direction) based on a two-tailed t test between experimental and control groups at the .05 level in each study, the probability of not finding a significant result using a vote-counting procedure, requiring more than one third of the studies to show a significant positive result given 10 studies each

[1] We were able to calculate effect sizes only for studies in which sufficient data were provided (means and standard deviations were either listed or graphed, or statistical tests were provided from which the desired effect sizes could be estimated (Glass et al., 1981). Letters requesting additional information were sent to authors of articles for which reported data were insufficient to calculate effect sizes, and follow-up letters were sent in many cases when there was no response. Data received by the time this comment was written, over 10 months after letters were sent, were included in the analysis. Also, studies were only included if they assessed effects of the normal practice of the TM technique, unconfounded by experimental intervention, if they allowed at least a 10-minute period of meditation, and if they studied normal adult populations rather than residential treatment or rehabilitation populations. All effect sizes were adjusted according to the table of Hedges (Glass et al., 1981, p. 113) so that they were unbiased estimates of population effect sizes.

Table 1
Mean Effect Size for Physiological Variables During Transcendental Meditation (TM) or Rest

Variable	TM		Rest		Difference		Pretest		Difference	
	M	*n*	*M*	*n*	*t*	*df*	*M*	*n*	*t*	*df*
Basal GSR	.826	12	.182	8	1.94*	18	.480	8	1.32	7
Spontaneous GSR responses	−.781	5	−.396	5	−1.03	8	−.529	7	−2.78*	6
Respiration rate	−.461	15	−.112	7	−2.06*	20	−.345	8	−1.99*	7
Heart rate	−.210	19	−.142	12	−0.61	29	−.534	12	−3.40**	11
Plasma lactate	−.617	5	−.228	4	−3.84**	7	−.354	4	−5.95**	3

Note. GSR = Galvanic skin response.
* $p < .05$. ** $p < .01$.

with a sample size of 20, is .966 for a population effect size of .40.

One issue raised by Morrell (1986) as well as Holmes (1984) is whether the ability to detect physiological changes may be hampered by a "floor effect" or the "law of initial values" by which the ability to decrease levels of physiological variables is mitigated by low initial values. We did find that TM meditators showed significantly lower initial physiological arousal during the baseline period on four of six variables. However, two of these variables (respiration and plasma lactate) showed significant changes during TM practice in comparison with rest, suggesting that a floor effect may not be a limiting factor for all variables. Also consistent with this suggestion is the fact that recent studies have found more extreme reductions in somatic arousal during periods reported by subjects as states of "pure consciousness"[2] during the TM technique (Badawi, Wallace, Orme-Johnson, & Rouzere, 1984; Farrow & Hebert, 1982). In light of this, the most useful research strategy may not be to average over an entire period what appears to be a dynamic process.

Although Holmes (1984) clearly limited his review to reductions in "somatic arousal" during meditation and rest, it is also important to note that the concept of reduced somatic arousal does not capture the full range of physiological effects reported in well-controlled studies of the TM technique, such as electroencephalogram (EEG) coherence (Dillbeck & Bronson, 1981), hemispheric EEG lateralization (Bennett & Trinder, 1976), increased plasma arginine vasopressin (O'Halloran et al., 1985), and improved reflex responses (Wallace, Mills, Orme-Johnson, & Dillbeck, 1983; Warshal, 1980). A number of these findings suggest greater alertness during or after the practice, consistent with the original suggestion that the practice of the technique results in a state of "restful alertness" rather than merely arousal reduction (Maharishi Mahesh Yogi, 1966, p. 196).

The second fundamental question addressed by Holmes (1984) is whether meditation is useful in the control of arousal in threatening (stressful) situations. Morrell (1986) noted evidence that meditators respond differently to stress and pointed out that this response should not be assessed in too simplistic a fashion, that is, without assessing responses in the context of the interaction of physiological systems. In addition, we argue that "control of arousal in threatening situations" (Holmes, 1984, p. 8) may not be physiologically adaptive, and it is clearly not the goal of the TM technique, which from a physiological perspective is described as increased integration and adaptive efficiency (Wallace, 1986, pp. 99–108). In fact, the most adaptive response to stress would probably not be absence of physiological response to a threatening situation, but rather rapid recovery after stress, possibly even preceded by increased physiological mobilization if the stress was severe and could be anticipated; such a pattern is consistent with the research evidence (Goleman & Schwartz, 1976; Orme-Johnson, 1973; Puente & Beiman, 1980).

In summary, our quantitative analysis indicates that the research evidence is generally consistent with the hypothesis that there is reduced somatic arousal during the TM technique in comparison with rest, yet at the same time other physiological changes indicative of increased alertness are also present. In addition, although there is evidence that TM meditators respond differently to stress than controls do, this difference may be assessed more successfully by looking at adaptive efficiency of physiological processes rather than reduction of somatic arousal during stress.

[2] Pure consciousness is described by subjects as an experience of consciousness without the activity of thought, in which consciousness is awake to itself alone (Farrow & Hebert, 1982). According to the Vedic tradition of India from which the technique comes, it is the experience of a unified field of consciousness at the basis of all subjective and objective existence (Maharishi Mahesh Yogi, 1966, pp. 25–28).

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An Unconnected Special Issue

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The recent special issue of the *American Psychologist* on psychological science and education (October 1986) is illustrative of the current state of psychological theory in an important way: The articles seem largely unconnected.

Evidence of this disconnectedness can be found in the scholarly and well-written article by Dweck (1986) on motivational processes affecting learning. Reflecting on a series of studies concerning the relation between school girls' attainments and their expectancies for future performance, Dweck (1986) wrote that "knowledge of past successes does not appear to arm them for confrontations with future challenges" (p. 1043). This statement directly contradicts a basic premise of Bandura's self-efficacy theory, that is, that expectations of personal efficacy are based upon four sources, the primary source being performance accomplishments (Bandura, 1977). Despite the potential benefits of an integration of Dweck's and Bandura's research (and despite Bandura's presentation of these views in an article published in the February 1982 issue of the *American Psychologist* as part of a Distinguished Scientific Contribution Award from APA), Dweck ignored Bandura's work.

An empirical look at the entire issue provides further evidence of disconnectedness. Of the 1,036 references listed (including the 17 articles in the special issue), only 79 (8%) can be found in more than one reference list. Of those 79 references, 20 (2%) refer to articles in the special issue itself. Thus, only 6% of the material represents a cross-fertilization of psychology and education.

This lack of integration of psychological theory is the rule, not the exception. The diversity of theoretical approaches apparent in psychology journals and among psychological practitioners leads people within (Staats, 1983) and outside (Waldholz, 1986) the field to view psychology as chaotic and as an art rather than a science. This is particularly true in the area of psychotherapy, in which more than 400 approaches are said to be operating (Karasu, 1986).

Diversity has its costs. The abundance of the profession's theories and methods is matched only by its lack of public influence. As Glaser and Takanishi (1986) noted, psychological research that could offer a knowledge base for effective educational policy is being largely ignored. Psychologists' disunity makes it difficult to explain their work and its importance. Equally difficult is the task of obtaining public support for their activities, whether it be third-party reimbursement or federal grants.

One answer to this dilemma is to increase efforts to unify psychological theories (Staats, 1983). Forsyth and Strong (1986) recently called for "fuller development of the theoretical side of psychological science" (p. 113) to aid in a unification of research in counseling and clinical psychology. What might such a unified theory look like? First, a unified theory should be interconnected: At its foundation should be an integration of diverse approaches. Second, such a theory should be surprising: It should make strong, unexpected predictions that would intrigue the investigative interests of researchers. Third, a unified theory should be applicable: The theory should be specific enough to permit technological derivations. Finally, a unified theory should be aesthetically pleasing: It should be packaged so as to appeal to current artisans and practitioners in psychology.

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Comment on Szasz's View of Suicide Prevention

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As a college professor, student of suicide, and especially as a practicing mental health professional, I cannot in good conscience allow Szasz's article (1986) to go unchallenged. Szasz has again presented his unique view of matters relating to "control" of patients, but he has failed to consider the whole picture.

His essential point is that psychologists do not have the right (nor for that matter, does anyone) to prevent a suicide if the suicidal person chooses not to seek professional help. According to Szasz, sui-