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## **Tm At Folsom Prison: A Critique of Abrams and Siegel**

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The article by Abrams and Siegel, "The Transcendental Meditation Program at Folsom State Prison: A Cross Validation Study" is examined and found wanting in several respects. Abrams and Siegel are criticized for inadequate controls which seriously limit the conclusions to be drawn from their study. In addition, the authors have taken greater than normal liberties in interpreting their statistical results. It is suggested that if there are any rehabilitative effects from Transcendental Meditation, they are not documented by Abrams and Siegel's paper.

## TM AT FOLSOM PRISON

### A Critique of Abrams and Siegel

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**I**n a recent article, Abrams and Siegel (1978) claim to have examined the effects of a Transcendental Meditation program at Folsom Prison and to have found "significant reductions in anxiety, neuroticism, hostility, insomnia, and behavioral infractions" (p. 15). However, a careful look at the procedure used and the data obtained may lead many to question the strength of the authors' arguments.

The first point of contention results from the authors' failure to use any form of placebo control or to shield the Ss from the Es main expectations. Basically, what Abrams and Siegel have done is to take two groups of people, tell one they will be given training which will radically alter its members lives, show them

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the way in which this alteration will be assessed (by the use of the same instruments in the pre- and posttests),<sup>1</sup> and then ask them if they *feel* their lives have been altered. This group was then compared with a second group whose members were told they would not receive treatment until a later date and therefore to expect no change in their behavior.

To find significant changes in self-report data under these conditions is hardly surprising, but to imply that this is in any way connected to rehabilitation (as Abrams & Siegel do in their title) is grossly misleading.

A more parsimonious explanation of the data would be to suggest that the Ss were simply altering their responses in an effort to accommodate the Es. One method of attempting to control for a Social Desirability set in Ss is to use an instrument with an SD or "lie" scale. Abrams and Siegel report the use of just such an instrument on page 8 (the EPI Lie Scale). However, when the data from the EPI are presented on page 10, we only find listings for "extroversion and neuroticism." The lie scale scores are therefore made conspicuous by their absence.

If this were the extent to which the authors erred, it would hardly be remarkable, but Abrams and Siegel compound the original failure to adequately control by distorting the results which were obtained. One example of this is found on page 11 of their report, where the authors state:

The multivariate F for the circulatory measures was not significant ( $F_{3,33} = 1.00$ ). Although the difference between the groups was not statistically significant, the postmeasure pulse rate of TM I was 6 1/2 beats less per minute than Control I. Were it not for attrition, this difference would have been sufficient for statistical significance.

Abrams and Siegel should be made aware that results are either statistically significant or not statistically significant, with no grey area in between. The above statement assumes that the lost data from the individuals who dropped out of the experiment would be supportive of their hypothesis. If one is willing to make assumptions of this nature, one need not bother to engage in hypothesis testing at all.

A more flagrant example of an attempt to distort the data appears on page 14 where the authors state:

The prison records of behavioral infractions occurring during the 14-week test interval were consulted to gauge the program's effect on the more manifest aspects of personality. During the aforementioned interval, 403 incidents were recorded for the entire prison population of nearly 1,900 inmates. Whereas between five and six incidents would have been the expected number from the TM I group, none occurred. *It may also be noted there were no incidents involving the Control I group.* The control group's behavior was probably influenced by their knowing that they were eligible to receive the TM Program following the post-measurement [italics added].

This is the first published instance, of which I am aware, in which the total absence of behaviors in both the experimental *and* control groups is attributed to the effects of the independent variable! A similar nonevent occurred in phase two of the experiment, but on page 15 it is treated in a more respectable fashion; thus:

Consulting the prison records of behavioral infractions for the 14-week test interval for the second validation, 350 recorded incidents were found for the total prison population, which had declined to about 1,700 men. None of these altercations involved a TM II or a Control II subject.

While the authors use these data to support the efficacy of TM as a treatment program, a more probable explanation for the data is that both TM I and II and Control I and II are significantly different from (and therefore not representative of) the prison population as a whole. Unfortunately, Abrams and Siegel do not interpret the data in this manner, but instead refer to "significant reductions in . . . behavioral infractions" (p. 15) occurring as a result of the TM program. Having thus reified this "reduction in behavioral infractions" the authors go on to state that "Reductions in negative personality traits and behavioral infractions should be understood to be indications of readiness to peacefully abide by the laws of society." Even with adequate supporting

data, it would be difficult to justify such a leap of faith. Without such data, however, the statement becomes mere cant.

It may be that there are positive effects accruing from the use of a TM program in a prison setting. Abrams and Siegel refer to the fact that "450 additional inmates have requested to participate in future courses" (p. 18). This would seem to indicate a general degree of acceptance of the program by the inmates. However, at a cost of \$135,000 (\$300 per inmate), and with the lack of demonstrated benefit resulting from such a program, this author must wonder if the monies could not be better spent.

### NOTE

1. This point is well covered in Robert Lana's chapter in *Artifact in Behavioral Research* (Rosenthal & Rosnow, 1969).

### REFERENCES

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