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EFFECTS OF SELF-MONITORING ON NORMAL SMOKING BEHAVIOR [1]

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The *Ss* in behavior modification research are sometimes required to monitor and record their own behavior. Such self-monitoring may be reactive, producing its own behavior changes, and confounding treatment effects. In this study, observers unobtrusively monitored *Ss'* smoking frequency and duration throughout base-rate, experimental, and return-to-base periods. In the experimental period, *Ss* self-monitored either their frequency of smoking or not smoking. Smoking frequency and duration were significantly affected by such self-monitoring, indicating that self-monitoring is a reactive data-gathering procedure. The implications of this finding are discussed.

Webb, Campbell, Schwartz, and Sechrest (1966) have drawn attention to the experimental problem of reactivity, which is the tendency for certain experimental measurement operations to function as an unintended, independent source of influence on the behaviors being measured. Research has shown, for example, that the *E* (Rosenthal, 1966), the psychological tester (Marwit & Marcia, 1967), the stimulus properties of the measurement situation (Murstein, 1959), and the measurement audience (Sarbin & Allen, 1964) can all function as reactive elements in research with human *Ss*.

The effect of reactivity is to reduce the representativeness of the behaviors sampled and to limit the generality of the experimental conclusions. Thus, it represents an especially significant problem for investigators attempting to obtain true base rates, or to measure naturally occurring behaviors. Elsewhere, Sidman (1960) has discussed the methodological problems involved in obtaining stable and representative base rates. To overcome some of these problems, behavior modification investigators have begun using in vivo assessment procedures, and have asked *Ss* to monitor and report on their own behavior. For example, in therapy research on sexual inadequacy, weight control, or smoking reduction, *Ss* are often required to keep records on their patterns of sexual activity, eating, or smoking (e.g., Azrin & Powell, 1968; Wolpe & Lazarus, 1966).

It is reasoned that by having *Ss* perform the assessment function, the investigator gains efficient access to otherwise unavailable, private behaviors, while also minimizing the reactive effects that might result from an *E's* obtrusive presence. It is possible, however, that having *Ss* monitor their own behavior is also a reactive procedure, producing behavior changes above and beyond those produced by specific treatments.

This possibility is suggested by data from a recent stop-smoking study conducted at the University of Wisconsin (McFall & Hammen, in press). All *Ss* were required to carry a record booklet and keep an hour-by-hour, base-rate record of their smoking behavior. Base rates were recorded over a period of four days, or 96 consecutive hours. Several *Ss* remarked that as a result of such self-monitoring, their normal smoking behavior was significantly changed. This occurred, apparently, despite specific instructions that they were not to alter their smoking behavior in any way during this period.

These subjective reports of change were corroborated, in part, by the fact that there was only a modest correlation ($r = .55$) between *Ss'* original estimates of their daily smoking rate and the actual smoking rate observed during the base-rate period. The *Ss* smoked significantly fewer cigarettes per day

[1] A brief summary of this research was presented at the meeting of the Midwestern Psychological Association, Chicago, May 1969. The author wishes to thank E. Mavis Hetherington for her helpful comments on this paper.

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than they had reported smoking under normal circumstances ($p < .01$). Unfortunately, it was not possible to determine from these data whether the *Ss* were inaccurate in estimating their normal cigarette consumption or whether the base-rate behavior was atypical due to the reactivity of the self-monitoring procedure.

If, as suspected, having *Ss* keep records of their own smoking behavior does alter that smoking behavior, this fact raises further questions. First, what is the nature of such effects? Second, might different self-monitoring instructions have different effects on the monitored behavior? For example, in the specific case of smoking research, what might be the differential effects of having some *Ss* record only those instances in which they actually light a cigarette, while having other *Ss* record those instances in which they consider smoking, but do not actually light a cigarette? For *Ss* who are trying to stop smoking, to record each smoking act might be the equivalent of recording negative events, or failure experiences, and thus could be aversive. On the other hand, to record each instance of resistance to temptation might be the equivalent of recording success experiences, and thus be positively reinforcing. This difference in focus—tracking positive as opposed to negative behaviors—could differentially affect the behavior being studied.

Finally, regardless of the kinds of effects potentially resulting from self-monitoring, any research requiring *Ss* to record their own behavior must confront the problem of the validity of the data so collected. That is, how accurately do *Ss* monitor and report their own behaviors?

The purpose of the present study was to investigate the three problems discussed above. First, it sought to determine whether self-monitoring, in the form of keeping records of one's own smoking behavior, significantly alters that behavior. Second, and more specifically, it looked for differential effects resulting from two kinds of self-monitoring instructions—that is, the differential effects of asking *Ss* to record occasions on which they do smoke, as opposed to asking them to record occasions on which they decide against smoking. Third, the study sought to determine how accurately *Ss* tend to keep records on their own smoking behavior.

METHOD

Subjects

Sixteen college students who were regular cigarette smokers participated as *Ss* in this study. In addition, 16 nonsmokers participated as experimental assistants, or as observers (*Os*). Four additional nonsmokers served as alternate, or "standby" *Os*. Each of the regular *Os* was paired with an *S* and recorded this *S*'s smoking behavior over three experimental treatment periods.

The experiment was conducted in the context of an eight-week summer session course in abnormal psychology at the University of Wisconsin. The class met five days a week, Monday through Friday, 11:30 A.M. to 12:20 P.M. Forty-five students were enrolled in the course—20 men and 25 women. Out of these students, 16 regularly smoked cigarettes and had done so for at least one year; 6 were men and 10 were women. The class also included at least 16 students who were not currently, and who had never been, cigarette smokers; 6 of these nonsmokers were men and 10 were women.

Procedure

First class day. On the first day of class, all students were asked to fill out a "Personal Information Form." Some items on this form asked for ordinary anamnestic data, such as name, age, sex, marital status, grade point average, year in school, major, psychology courses taken, and work experience. Other items were more personal and behavioral in nature, asking about anxiety level, tendency to behave assertively, amount of "will power," tendency toward self-awareness, expected grade in the course, and estimated intelligence relative to classmates. Embedded among these behavioral questions was an item asking if they smoked cigarettes. Smokers were asked to indicate how long they had been smoking and how many cigarettes they smoked per day on the average. Nonsmokers were asked if they had once been smokers, and if so, to explain.

During this first class period, students were told that they would be required to participate as *Es* in various experiments during the summer semester. Although they were given the option of writing a paper in lieu of being *Es*, no students chose this alternative.

Finally, the class was told that they were to be given daily quizzes at the beginning of each class period, rather than the ordinary midterm and final exams. To facilitate the quiz administration, students would be assigned permanent seats. The seats were to be arranged in paired columns. Each student pair would exchange answer sheets for purposes of grading each day's quiz. Tests and answer forms were to be handed out and collected each day by means of the column structures.

In the interim between the first and second class meetings, a seating chart was constructed by the investigator on the basis of students' responses to the smoking question on the Personal Information Form. Students of like sex were assigned paired seats in such a manner that for each student pair in the first four rows, the student to the teacher's left was always a nonsmoker, and the student on the right was a regular smoker. Four additional nonsmokers were assigned seats as "alternates" in the fifth row of the nonsmoker columns. Students were defined as regular smokers only if they had been smoking cigarettes daily for at least one year. Students were labelled as nonsmokers only if they were not currently smoking and had never smoked. Students who were either recent or only occasional smokers were excluded from the study and assigned the remaining seats at the rear of the seating columns, that is, in rows 5 and 6.

Second class day. At this meeting, the students were not told of their seating assignments, but were again permitted freedom of seating. No quiz was given, and the class period was devoted to a normal lecture and discussion of introductory course content. However, with five minutes remaining in the period, the names of 20 nonsmokers (16 plus 4 alternates) were called—with no mention of the basis for their selection—and they were asked to stay after class to discuss participating in an experiment. The remaining students were dismissed early, and all of them left the room without having to be urged.

The nonsmokers, *O*s, were informed that they were to record the smoking behavior of their seating partners each day and for the duration of the course. They were to construct a daily record sheet in their notebooks and record the exact times at which their *S*s lit and extinguished their cigarettes. Records were to be made as unobtrusively as possible, so that *S*s would not become aware that their behavior was being observed. Times were to be recorded as closely as possible to the moment when the behavior occurred. The *O*s agreed not to discuss their role in the experiment with anyone.

Although absences were expected to be infrequent due to the planned daily quiz, in the event that an *O* did miss class, the *O* seated immediately behind the absent *O* was to "cover" for him, making certain to indicate the source of the data on the record sheet. The *O*s were encouraged to see the professor privately if problems arose in the data collection procedure. Finally, *O*s were given a chance to ask questions about their task. In response to one such question, the professor assured them that they would not themselves become the objects of secret observations.

Third class day. At the beginning of this class period, students were handed a dittoed seating chart and asked to take their assigned seats. Then, for the first time since the course began, the professor lit a cigarette and casually observed that the "No Smoking" sign posted at the front of the class pertained only to afternoon classes![8]

Base observation period. Beginning with the fourth class meeting and coinciding with the administration of the first quiz, *O*s began keeping records on their *S*s' smoking behavior. This base period lasted for nine consecutive class days. During this time, no *O* reported difficulty in obtaining or recording the smoking data.

Self-monitoring period. The professor introduced the self-monitoring treatment, on the tenth class day, by announcing that he was interested in cigarette smoking patterns among college students, and wanted to enlist the class's cooperation in collecting some naturalistic data on this topic. He instructed all students sitting in the first and third pair of columns to keep a daily tally of the number of cigarettes smoked during class. He then instructed students sitting in the second and fourth pair of columns to keep a similar tally of the number of occasions during each class day when they considered smoking a cigarette but, for whatever the reason, did not do so. These instructions were addressed to the entire class, but, of course, only the previously identified smokers were actually affected by them. As a rationale for this data collection, students were told that both kinds of data, when combined, could shed light on the response characteristics of the smoking habit. Finally, the professor emphatically urged all students not to make any changes whatsoever in their normal "preexperimental" smoking behavior. The whole idea, he emphasized, was to discover how smokers actually behave under normal circumstances. This could only be accomplished if everyone continued to smoke "naturally."

Smokers followed these instructions for a period of 13 consecutive class days. Throughout this period, *O*s continued to record *S*s' smoking without *S*s' knowledge. At the end of the thirteenth day of self-monitoring, the professor collected the data from all of the *S*s, told smokers to stop recording the data, and thanked them for their cooperation.

Return-to-base period. For the next nine consecutive class days, and until the end of the summer session, *O*s continued recording their *S*s' smoking behavior. On the final class day, the professor collected these data from *O*s and explained the experiment to the class, encouraging students to express any reactions or questions regarding the experiment. At this time, smokers were asked whether they had been aware that they were being observed; only one *S* admitted to being aware.

[8] The professor had noted that on the first two days of class, the smoking rate among smokers had been rather low. Consequently, he decided to attempt to facilitate smoking by discounting the significance of the prohibitive sign, and by systematically modeling smoking behavior. For the remainder of the course, he always lit a cigarette during the first minute of each class hour.

RESULTS

Although the smoke and no-smoke groups initially contained eight *S*s each, the following analyses include data for only six *S*s per group. Two *S*s from the smoke group were eliminated because they never smoked at all throughout the entire experiment. In one case, the *S* subsequently explained that he never smoked except at home; in the second case, the *S* had quit smoking after the preexperimental questionnaire was given, and before the start of the experiment. Two *S*s were also excluded from the no-smoke group. One *S* revealed that she never smoked except at home. The other *S* was eliminated because she had been aware of being observed; her *O* had secretly disclosed the nature of the experiment to her, and then failed to collect the necessary data.

Before proceeding to the main experimental analyses, data from the preexperimental questionnaire were examined to make certain that no initial differences existed between the two experimental groups, between the two sets of *O*s, or between smokers and *O*s. In all these comparisons, no significant differences were found.

Smoking frequency. This first analysis dealt with the mean number of cigarettes smoked in class per day, based on the *O*s' records. Considering each experimental period separately, a mean smoking rate was computed for each *S* (total number of cigarettes/days observed). Individual *S*'s means were then combined to arrive at the means for the two experimental groups. The resulting smoking rates are shown in Figure 1. For the nine-day base period, the groups were practically identical (smoke = .847, no-smoke = .849). In the self-monitoring period, however, the means for the two groups differed significantly (Mann-Whitney $U = 5$, $p < .05$).[4] The smoke group increased its smoking rate to 1.396, while the no-smoke group's rate decreased to .588. Again, in the return-to-base period, there was a significant difference between the means of the two groups (Mann-Whitney $U = 5$, $p < .05$), with both groups increasing over their rates in the preceding period (smoke = 1.583, no-smoke = .683).

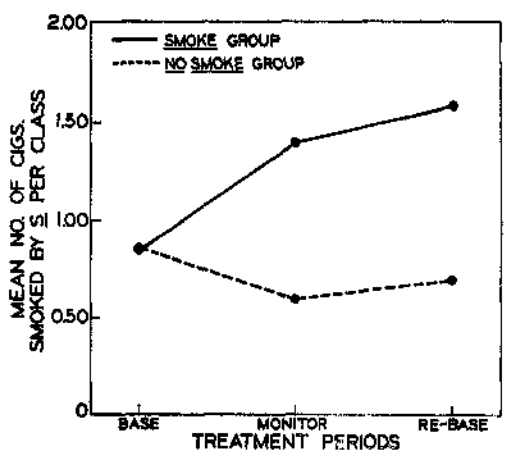

FIG. 1. Smoking frequency within treatment periods for two conditions of self-monitoring.

The differential effects of smoke versus no-smoke monitoring instructions are most clearly revealed in a between-groups comparison of the individual *S*'s change in rate from the base to monitor periods. Change scores for the two treatment groups showed a highly significant difference (Mann-Whitney $U = 0$, $p < .002$). A similar comparison of change scores from the monitor to the return-to-base periods, as might be expected, was not significant.

To test for the specific effects of each of the two monitoring instructions, comparisons were made between treatment-period means for each group separately. The smoke group's rate increased significantly from base to monitor period (Wilcoxon matched-pairs signed-ranks test, $p < .05$); the difference between their base and return-to-base rates was also significant ($p < .05$), but the observed increase between monitor and return-to-base periods failed to achieve significance. For the no-smoke group, similar comparisons failed to yield any statistically significant differences.

Conceivably, the observed treatment differences in smoking rates during the 13-day monitor period could be due to differences obtained primarily during the first few days, with such differences diminishing over time. In other words, one might question the stability of the self-monitoring effects. Are *S*s temporarily influenced by the demand char-

[4] All statistical comparisons reported in this paper were two-tailed tests.

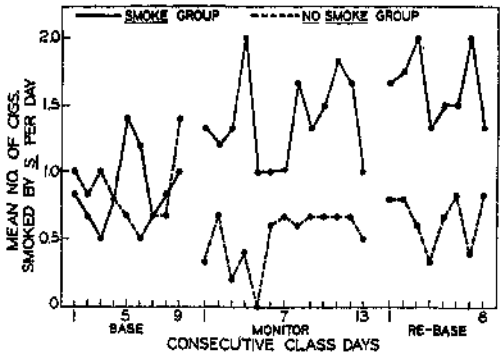

FIG. 2. Smoking frequency across consecutive class days for two conditions of self-monitoring.

acteristics (Orne, 1962) for producing the requested data, only to lapse once again into their normal smoking pattern? To test this possibility, day-to-day smoking rates were computed for each of the two experimental groups. The results are shown in Figure 2.

Although there is an insignificant tendency for *Ss* to show the strongest treatment effects during the first 3–5 days of the monitor period, such effects seem quite stable and persist throughout the entire period. Moreover, prominent residual effects are retained throughout the return-to-base period. The monitoring effects clearly are not transitory.

Smoking duration. The second aspect of *Ss'* smoking behavior to be examined was that of duration, that is, the number of minutes required to consume each cigarette. From *Os'* records, the mean duration per cigarette was computed for each *S* in each of the experimental periods.[5]

Means for the two experimental groups are shown in Figure 3. Since there were no significant differences between groups in the base, monitor, or return-to-base periods, the two groups were combined for subsequent analyses.

The decreased time per cigarette between the base and monitor periods was significant at the .05 level for the combined groups (Wilcoxon matched-pairs signed-ranks test). Although the difference between means in the monitor and return-to-base periods was significant for smoke *Ss* alone (Wilcoxon, $p < .05$), it was not significant for the combined groups. It appears that there was a significant main effect of self-monitoring on the time the *Ss* devoted to each cigarette, with both smoke and no-smoke *Ss* significantly reducing their smoking duration in the monitoring period. These self-monitoring effects tended to disappear when *Ss* stopped tracking their smoking behavior. In contrast to the findings in the smoking-frequency measure, smoking duration showed no differential effects associated with the two monitoring treatments.

A final observation regarding the independence of the frequency and duration measures: One might argue that within a fixed time period, duration should decrease as frequency increases, and vice versa. But an examination of Figures 1 and 3 indicates that these measures were, in fact, relatively independent. For example, no-smoke *Ss* showed a decrease in both frequency and duration during the monitor period, while smoke *Ss*, in the return-to-base period, showed no reduction in duration despite a frequency rate nearly twice their base rate. A correlational analysis of the frequency and duration measures during the base period revealed that they were actually positively, but not significantly, related ($rs = .44$); that is, those *Ss* who tend to

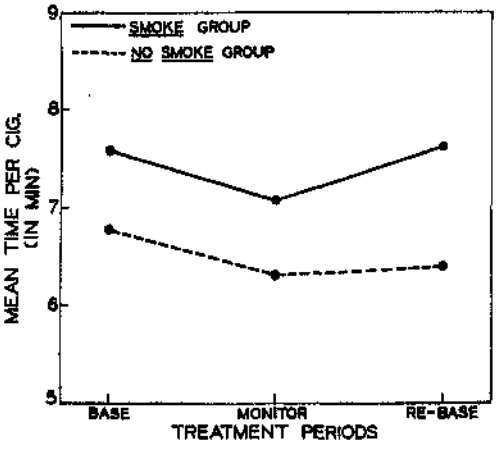

FIG. 3. Smoking duration within treatment periods for two conditions of self-monitoring.

[5] Those cigarettes begun or finished outside of the class period were excluded from these analyses. Furthermore, two *Ss* in the no-smoke group were excluded from some comparisons since they failed to smoke any cigarettes during certain experimental periods.

smoke more frequently also tend to smoke each cigarette for a longer duration. A similar analysis of *S*s' change scores from base to monitor periods indicates that if anything, there is a slight tendency for an increase in frequency to be associated with longer durations ($rs = .25$, $p > .10$). Thus, in general, frequency and duration appear to be relatively separate aspects of the smoking response, each showing a different pattern of effects due to self-monitoring.

Reliability of S*s' and* O*s' data.* The final experimental question pertains to the reliability or accuracy of the data collected by a self-monitoring procedure. It might be argued that the reliability of data collected by the *O*s was not inherently any better than that of the *S*s, and thus cannot be used as a validating standard. However, it could be assumed that since *O*s were less personally involved in the observed behavior, their reports would be more objective.

The daily smoking frequency records kept by smoke-group *S*s during the monitoring period were correlated with the *O*s' frequency records over the same period. An overall correlation of .61 was obtained over all six smoker-*O* pairs, or over a total of 70 days of observation.[6] For each of the six *S*-*O* pairs considered separately, the correlations ranged from −.05 to 1.00, with three out of six pairs above .75. Finally, a correlation between the mean frequencies reported by each *S* and his *O*—that is, based on six pairs of observations —yielded highly significant results, with correlations of .96 ($p < .01$). An analysis of the *S*-*O* discrepancies, where they existed, revealed that nearly always it was the *O* who counted fewer cigarettes than counted by the smoker. Across the six *O*s, there were 22 instances where the *O* failed to indicate a cigarette when the smoker did, and only six instances where the reverse occurred.

Although the most obvious interpretation would be that *O*s were underrepresenting their *S*s' smoking rate, two unplanned sources of data tend to suggest otherwise. First, it happened that a regularly scheduled, midweek class meeting was cancelled during the monitoring phase of the experiment; no data should have been reported for that day. However, four out of the six smoke *S*s turned in a record for that day, whereas only one of the *O*s made this error. For two of the *S*s and for the one *O*, their error was clearly a clerical one, since they did not report data for the following day when, in fact, class had been held. Nevertheless, that means that the other two *S*s had probably compiled their records, in part, on the basis of retrospective accounting. There is no evidence that *O*s did this.

The second set of fortuitous data comes from the fact that some *O*s misunderstood their instructions regarding what to do in the event that an *O* in front of them was absent. Their misunderstanding led to 10 instances in which an *S*'s behavior for a particular day was reported by two *O*s.[7] When this occurred, the two *O*s showed 76% agreement regarding the *S*'s smoking frequency, with the *O* sitting nearest the *S* recording more cigarettes than the other, more distant *O*. Moreover, for those cigarettes scored by both *O*s, there was surprising agreement regarding the precise minutes during which the *S*s smoked.

DISCUSSION

This study has demonstrated experimentally something that has long been suspected by psychologists and laymen alike: When an individual begins paying unusually close attention to one aspect of his behavior, that behavior is likely to change, even though no change may be intended or desired.

Nearly everyone has had the experience of becoming self-conscious about his behavior and, as a result, experiencing a change in that behavior. For example, if a person's attention is drawn to the way he walks, holds his hands, or swings a golf club, it often makes it difficult for him to perform these activities naturally; he is likely to walk stiffly, not know what to do with his hands, or swing his golf

[6] An analogous comparison was also made between no-smoke *S*s' daily records of their decisions *not* to smoke and their *O*s' daily records of smoking. There was a low, nonsignificant, positive relationship between these measures ($r = .25$).

[7] Ten such days of "double coverage" occurred out of a total of 372 possible days of observation. This represents only about 2% error, which is surprisingly low.

club awkwardly. These effects of self-consciousness, or self-monitoring, are also seen in the manipulative strategies of the Zen master, for example, "Be spontaneous" (Watts, 1961), the psychotherapeutic approach advanced by Haley (1963), the "one-upmanship" *primary hamper* gambit of Potter (1950), and especially the neurotic patterns of behavior found in hysterics, depressives, obsessives, etc., where excessive self-monitoring is one of the central features.

Specifically, the present study has shown that self-monitoring of smoking behavior alters that behavior; that focusing on positive instances of smoking increases the frequency of smoking, while decreasing the time spent per cigarette; and that focusing on negative instances of smoking decreases smoking frequency, while also decreasing time per cigarette.

There are at least two major implications to be drawn from these findings. First, it appears unavoidable that data collected by means of a self-monitoring procedure will differ significantly from data collected without the *S*'s knowledge. If an *E* seeks to discover how his *S* behaves normally, then an unobtrusive measurement operation is imperative (see Webb et al., 1966).

In some instances, it is not vital that undistorted base-rate data be obtained; rather, it is only necessary that the data be stable and that such base rates be sensitive to subsequent experimental interventions. In this case, it would be reasonable to enlist the *S*'s cooperation in collecting base-rate records. However, the specific instructions given to *S* are likely to affect the kind of data obtained, and these instructional effects must be taken into account. The representativeness of self-monitored data, and the generalizations drawn from them will necessarily be limited to the specific conditions under which data collection occurred. Moreover, since some of the self-monitoring effects in the present study appeared quite persistent, it suggests that treatment effects may be masked, or might take the form of an interaction with the data collection procedure. These possibilities should be acknowledged.

The second major implication of these findings is that when more than one treatment group is involved in a study, with *S*s in the different groups carrying out somewhat different self-monitoring instructions, it is necessary that a pretreatment control period be conducted to assess the differential effects of such self-monitoring instructions, prior to the implementation of the actual treatments. For example, in smoking research, it would be advisable to have *S*s collect data on their "normal" smoking behavior (a base-rate period); then introduce the instructions for the differential smoking treatments, but continue with a control period of additional base-rate observations; and then finally begin the actual treatments only after the rates in the control period have stabilized. Only in this way can the investigator distinguish between the self-monitoring effects associated with the instructions and the specific effects of the treatments per se.

The final problem raised by the present study—the accuracy of self-monitoring records—is a difficult one with no simple solution. Ideally, one should arrange for an independent check on the validity of the data reported by *S*s (e.g., Powell & Azrin, 1968). At the very least, one should minimize those conditions that promote inaccuracy. In the past, some researchers have increased the likelihood of data falsification by confounding the data-collection procedure with their *S*s' task motivation. That is, *S*s were differentially reinforced, or positively evaluated, only for turning in certain kinds of data. It would be much better if *S*s were reinforced primarily for providing complete and accurate data regardless of whether these data happened to confirm the experimental hypothesis. In the smoking clinics conducted by this author and his colleagues, for example, considerable emphasis was placed on the value of accurate "negative" data; *S*s were told at great length of the scientific worth of data showing the failures, as well as the successes. Of course, if at all possible, it is best to keep the *S*s "blind" regarding the experimental hypotheses.

The *S*s in this study differed from *S*s found in most smoking research in that they were not necessarily motivated to stop smoking, nor committed to a behavior change program. In fact, every effort was made to minimize

Ss' motivation to stop smoking as a factor in this study—for example, the professor modeled smoking and stressed the importance of continuing to smoke normally. Nevertheless, this study may have implications for stop-smoking or other behavior therapy research programs. It is possible that the reactive effects of self-monitoring, when intentionally incorporated into a treatment program, may be used constructively to facilitate desired behavior changes.[8] Of course, this possibility requires further research.

In conclusion, this study has drawn attention to the methodological problems involved in asking *Ss* to monitor and record their own behavior. Data collected in this manner are quite likely to show reactivity, or self-monitoring effects; thus, self-monitoring as a data-collection procedure should be employed only with a full awareness of its shortcomings.

[8] For example, McFall and Hammen (in press) are currently investigating the therapeutic value of self-monitoring for smoking reduction. In a therapy study on obesity, Stollak (1967) found that a self-monitoring treatment group (which he had considered a control group) showed the greatest improvement.

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EDITOR'S NOTE

The following two discussions by Martin T. Orne and by Frederick H. Kanfer were prepared at the request of the Editor.