Reaction to Cummings on Addiction

John D. Lovern
*Veterans Administration Hospital
Loma Linda, California*

Cummings (December 1979) is to be vigorously applauded for bringing to light the seriousness of the drug and alcohol addiction problem and the inability of traditional psychotherapy to deal with this problem effectively. Those of us who work daily with addicted people hold most of Cummings's statements to be self-evident, but we suffer frequent frustration at the apparent unwillingness of other psychologists to listen to and consider these ideas. That Cummings has bravely stated his views in our official journal and lent his prominent name to them does much to reduce my sense of separation from my profession and my chagrin at not being taken seriously in a matter that I know to be profoundly serious.

Unfortunately, I see in Cummings's article an assumption (or set of assumptions) that I regard as unfortunate, both for psychologists who work in addiction and for our clients/patients. He betrays his attitude by devoting only a short paragraph to Alcoholics Anonymous (AA) and speaking in a way that dismisses AA as not worthy of serious consideration in the treatment of addictions (i.e., "I respect AA, *but* . . ."). Though Cummings confesses that he, like many of us, used to treat addiction "in the traditional, ineffective fashion," until he was apparently enlightened, I suggest that he needs further enlightenment, in this case about AA and the need to recognize its value.

Cummings seems to believe that AA's sole value is that it "turns a destructive rescue game . . . into a positive rescue game" (p. 1126). I submit that AA has far greater value than this and is, in fact, the source of most of the ideas that he presents under the label "psychological model of addiction." In addition, AA can be a great resource in helping us to make our treatment interventions more effective and durable, as well as in contributing significantly to their cost-effectiveness.

The crucial element in Cummings's model of addiction is the idea that "addiction is not merely popping something into one's mouth but a constellation of behaviors that constitute a way of life" (pp. 1121–1122). Addicts, then, are people who must give up their destructive, addiction-supporting games (or traits) and develop new, constructive ways of living. Compare these ideas with some quotations of AA's cofounder, Bill W. (1967):

It has often been said of AA that we are interested only in alcoholism. That is not true. We have to get over drinking in order to stay alive. But anyone who knows the alcoholic personality by firsthand contact knows that no true alky ever stops drinking permanently without undergoing a profound personality change. (p. 1)

No, sobriety is only a bare beginning; it is only the first gift of the first awakening. If more gifts are to be received, our awakening has to go on. As it does go on, we find that bit by bit we can discard the old way of life—the one that did not work—for a new life that can and does work under any conditions whatever. (p. 8)

For a therapeutic intervention to successfully effect a life-style change, it must make its biggest impact on life *outside* the therapy situation, as Rotter (1972) reminds us. If we gear our therapy to preparing clients/patients for, and sending them to, AA, we can take advantage of a resource for reinforcing the therapy by making it an important part of their lives. Not only can AA help enhance generalization and transfer of therapeutic effects, but it is also available at practically any time or any place, and it can be expected to continue to be available for the forseeable future. It can therefore reinforce life-style change now, do so conveniently, and continue to do so for a long time to come.

Many of us are interested in finding ways to develop cost-effective service delivery programs, especially in light of the possibility that a national health insurance system will be put into effect (Cummings, 1977). How can we overlook the possibilities inherent in the use of AA as a source of follow-up treatment? When our clients/patients make the transition from professional programs to AA, they can receive treatment as frequently or as long as they want or need it, at absolutely no cost to the public or third-party payers. To fail to take advantage of this fact seems wasteful.

Inasmuch as AA's model of addiction is very similar to Cummings's psychological model (in fact, AA's model *is* a psychological model), and in consideration of the therapeutic and economic advantages offered by AA, I feel strongly that Cummings and other psychologists should take another look at AA, swallow their pride, stop trying to "reinvent the wheel," and wisely make use of the gifts that AA holds out to us.

REFERENCES

Cummings, N. A. The anatomy of psychotherapy under national health insurance. *American Psychologist*, 1977, *32*, 711–718.
Cummings, N. A. Turning bread into stones: Our modern antimiracle. *American Psychologist*, 1979, *34*, 1119–1129.
Rotter, J. B. Some implications of a social learning theory for the practice of psychotherapy. In J. B. Rotter, J. E. Chance, & E. J. Phares (Eds.), *Applications of a social learning theory of personality*, New York: Holt, Rinehart & Winston, 1972.
W., Bill. *The A.A. way of life*. New York: Alcoholics Anonymous World Services, 1967.

Thinking and Feeling: A Skeptical Review

Michael H. Birnbaum
University of Illinois at Urbana-Champaign

Zajonc (February 1980) claims that poetry, intuition, and experimental evidence support the claim that preferences need no inferences. His review is one-sided, however, and fails to mention that a simpler theory can account for the evidence he cites (Birnbaum & Mellers, 1979a, 1979b).

Zajonc cites research by Matlin (1971), Moreland and Zajonc (1977, 1979), Wilson (1979), and Kunst-Wilson and Zajonc (1980) as evidence that
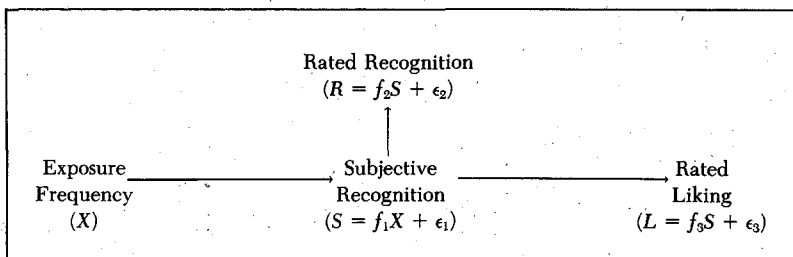
Figure 1. The null hypothesis—A one-mediator model of exposure effects.

recognition does not mediate the exposure effect. In these studies, stimuli that were presented with greater frequency were evaluated more favorably. Furthermore, the exposure effect (positive correlation between stimulus exposure and liking) persisted even when rated familiarity or other measures of recognition were partialed out. Birnbaum and Mellers (1979b) showed that this partial correlation (cited as evidence by Zajonc, 1980, pp. 161–163) is not a diagnostic test of the null hypothesis that stimulus recognition mediates the exposure effect. This partial correlation is predicted to be positive by the theory that Zajonc hopes to refute.

Consider the experiment of Matlin (1971), as reviewed by Zajonc. Stimuli ("Turkish-like" words) were presented with different exposure frequencies and were then rated on liking (affect) and recognition ("old" or "new"). The null hypothesis (Birnbaum & Mellers, 1979b) is depicted in Figure 1, where X is exposure frequency, the manipulated independent variable; R and L are respectively rated recognition and rated liking, the dependent variables; S is the subjective recognition mediator; and ϵ_1, ϵ_2, and ϵ_3 are mutually uncorrelated residuals. Unless the dependent variables are perfectly valid (an unreasonable assumption), the partial correlation arguments of Zajonc are not relevant to the evaluation of the above null hypothesis (Birnbaum & Mellers, 1979b).

To illustrate how this null hypothesis can account for the data of Matlin (1971; see Table 1 of Zajonc, 1980), the hypothetical data in Table 1 were generated from the hypothesis that stimulus recognition mediates the exposure effect. Each row of Table 1 represents a different hypothetical stimulus, which either was shown to the subjects during the initial phase of the experiment ($X = 2$, old) or was not presented until the rating phase of the experiment ($X = 0$, new). Subjective recognition (S) is assumed to depend in part on objective exposure:

$$S = X + \epsilon_1, \qquad (1)$$

where ϵ_1 is uncorrelated with X. The two dependent variables, rated familiarity (R) and rated liking (L), are assumed to be linearly related to the mediator (S) as follows:

$$R = S + \epsilon_2, \qquad (2)$$

$$L = S + \epsilon_3, \qquad (3)$$

where ϵ_2 and ϵ_3 are uncorrelated with each other and with S. Note that ϵ_1, ϵ_2, and ϵ_3 were constructed to be factorially combined from exactly two levels (0, 2), which guarantees that they will be mutually uncorrelated for the example.

Now consider what happens when the hypothetical liking data are analyzed as in Zajonc's Table 1. Table 2 shows the results for the hypothetical data, sorted according to objective frequency (X = old or new) and rated familiarity ($R < 3$, "new"; or $R > 3$, "old"). There are main effects of both objective frequency and rated familiarity on liking, holding the other variable constant at either level, as in the data of Matlin (1971) and Moreland and Zajonc (1977). Zajonc (1980) erroneously interprets these partial correlations as evidence for the conclusion that a second mediator, unconscious affect, is required in addition to the stimulus recognition mediator. The result that leads to this unnecessary conclusion is implied by the null hypothesis that Zajonc rejects. The null hypothesis that stimulus recognition mediates the exposure effect (Equations 1, 2, and 3) implies that even when the subject says he or she has never seen the stimulus before ("new"), there will be an effect of exposure frequency on liking (Tables 1 and 2). Zajonc's conclusions are therefore unwarranted by the data.

TABLE 1
Hypothetical Example

Exposure frequency (X)	ϵ_1	Subjective recognition (S)	ϵ_2	Rated recognition (R)	ϵ_3	Rated liking (L)
0 (new)	0	0	0	0 ("new")	0	0
0 (new)	0	0	0	0 ("new")	2	2
0 (new)	0	0	2	2 ("new")	0	0
0 (new)	0	0	2	2 ("new")	2	2
0 (new)	2	2	0	2 ("new")	0	2
0 (new)	2	2	0	2 ("new")	2	4
0 (new)	2	2	2	4 ("old")	0	2
0 (new)	2	2	2	4 ("old")	2	4
2 (old)	0	2	0	2 ("new")	0	2
2 (old)	0	2	0	2 ("new")	2	4
2 (old)	0	2	2	4 ("old")	0	2
2 (old)	0	2	2	4 ("old")	2	4
2 (old)	2	4	0	4 ("old")	0	4
2 (old)	2	4	0	4 ("old")	2	6
2 (old)	2	4	2	6 ("old")	0	4
2 (old)	2	4	2	6 ("old")	2	6

Note. The data in this example were generated from the one-mediator model; that is, X is the manipulated variable objective frequency; S is subjective recognition, $S = X + \epsilon_1$; R is rated familiarity, $R = S + \epsilon_2$; and L is rated affect, $L = S + \epsilon_3$.

TABLE 2

Average Affect Ratings as a Function of Objective Familiarity (Old–New) and Rated Familiarity ("Old"–"New")

Objective familiarity	Rated familiarity		
	"Old"	"New"	Mean
Old	4.33	3.00	3.67
New	3.00	1.67	2.33
Mean	3.67	2.33	

Note. Hypothetical data based on Table 1. Rated familiarity is "new" or "old" when $R < 3$ or $R > 3$, respectively. These hypothetical data resemble Table 1 of Zajonc (1980), yet they were generated from the hypothesis Zajonc (1980) rejects.

The experiments of Moreland and Zajonc (1977) have been criticized by Birnbaum and Mellers (1979a, 1979b) and thus are not discussed in detail here, except to note the following: Birnbaum and Mellers's (1979a) one-mediator model provides a better fit to Moreland and Zajonc's (1977) data than does Moreland and Zajonc's (1979) two-mediator model, shown in Figure 2 of Zajonc (1980). In fact, the linear one-mediator model provides a reasonably good fit to the data, even though Moreland and Zajonc's (1977) variables show nonlinear trends (see Birnbaum & Mellers, 1979a, Figure 2). The residual correlations between liking and exposure frequency computed for Experiments 1 and 2 of Moreland and Zajonc, with the single mediator partialed out, were only .03 and .06, hardly convincing in magnitude for data based on 40 subjects. Birnbaum and Mellers (1979a) concluded that the data of Moreland and Zajonc (1977), even as reanalyzed by Moreland and Zajonc (1979), do not

rule out the one-mediator model of exposure effects.

Wilson (1979) and Kunst-Wilson and Zajonc (1980) used procedures in which actual exposure appeared to have a low correlation (.10) with rated recognition (or oldness) and a higher correlation (.18) with affect (see Table 3). These studies also misinterpret implications of the one-mediator theory. The pattern of correlations below the diagonal in Table 3 can be well-approximated by the one-mediator null hypothesis, as shown by the predictions of this theory (above the diagonal). Note that the obtained correlations between rated recognition and both exposure frequency and liking are small (.10 and −.05). Such a pattern is not predicted by the theory of Zajonc (1980), which would predict that only the correlation between stimulus exposure and recognition would be reduced by the experimental manipulations thought to reduce stimulus recognition (f_1). Instead, both correlations involving stimulus rec-

ognition appear small and nonsignificant, suggesting that the recognition measure used was of low validity.

Some psychologists have argued that reinforcement (affect?) is necessary for learning rather than vice versa. Zajonc seems to have missed this literature, which seems relevant to his contention that his own position contradicts contemporary views in psychology. As Zajonc notes, his approach recalls arguments found in Freudian theory. Like Freudian theory, many of the statements in his article contain emotive or poetic meaning, but are devoid of empirical or scientific implication. Zajonc may indeed be correct that scientists evaluate theories without thinking or understanding. His article seems to be evidence relevant to that point. The evidence cited by Zajonc, however, does not support the claim that the "cold" theory he opposes can be rejected on scientific grounds. The key findings can easily be explained by the theory that recognition mediates the exposure effect.

TABLE 3

Intercorrelations Among Major Variables of Wilson (1979, Experiment 2) and Predictions of the One-Mediator Model

Variable	X	L	R
Exposure frequency (X)		.18	.09
Rated affect (L)	.18		.02
Rated oldness or recognition (R)	.10	−.05	

Note. Based on data from 24 subjects. The argument for two mediators apparently rests on the claim that the correlation between exposure frequency and affect is statistically significant, while the other two correlations (below the diagonal) are not. However, nonsignificance does not prove that no relationship exists, and a one-mediator model gives a reasonable fit to the correlations (above the diagonal).

REFERENCES

Birnbaum, M. H., & Mellers, B. A. One-mediator model of exposure effects is still viable. *Journal of Personality and Social Psychology*, 1979, 37, 1090–1096. (a)

Birnbaum, M. H., & Mellers, B. A. Stimulus recognition may mediate exposure effects. *Journal of Personality and Social Psychology*, 1979, 37, 391–394. (b)

Kunst-Wilson, W. R., & Zajonc, R. B. Affective discrimination of stimuli that cannot be recognized. *Science*, 1980, 207, 557–558.

Matlin, M. W. Response competition, recognition, and affect. *Journal of Personality and Social Psychology*, 1971, 19, 295–300.

Moreland, R. L., & Zajonc, R. B. Is stimulus recognition a necessary condition for the occurrence of exposure effects? *Journal of Personality and Social Psychology*, 1977, 35, 191–199.

Moreland, R. L., & Zajonc, R. B. Exposure effects may not depend on stimulus recognition. *Journal of Personality and Social Psychology*, 1979, 37, 1085–1089.

Wilson, W. R. Feeling more than we can know: Exposure effects without learning. *Journal of Personality and Social Psychology*, 1979, 37, 811–821.

Zajonc, R. B. Feeling and thinking: Preferences need no inferences. *American Psychologist*, 1980, 35, 151–175.