

**Table 1**  
*Meta-Analysis of Experimental Studies of Delay of Gratification*

Study	df	Effect size (r)
Mischel & Baker (1975) Average of nonindependent contrasts for consume relevant ideation versus control and transform relevant ideation versus control.	22	.268
Mischel & Ebbesen (1970) Main study: Reward present conditions (combined) versus reward not present.	28	.504
Replication: Reward present versus reward not present	22	.326
Mischel, Ebbesen, & Zeiss (1972) Experiment 1 Play with toy and think fun (combined) versus no distraction	45	.701
Experiment 2 Think fun versus think sad and think rewards (combined)	23	.566
Experiment 3 Think fun and no ideation (combined) versus think rewards	14	.954
Mischel & Moore (1973) Average of nonindependent contrasts between "relevant image" and "no slide" within the working and waiting conditions	107	.247
Mischel & Moore (1980) Average of five nonindependent contrasts used to "test for differences between groups of particular interest" (p. 218)	81	.305
Moore, Mischel, & Zeiss (1976) Average of the three nonindependent contrasts "of particular interest" (p. 422)	42	.459

observation that requires neither precision nor representativeness: At the very least, the two sets of effect sizes lie on overlapping distributions. To the extent that personality's influence on delay can be disparaged on the grounds of small effect size and "variance explained," at least some experimental effects must come in for equivalent disparagement.

It might be wiser to disparage neither these person *nor* situation effects on the grounds of their effect sizes. A growing body of evidence both empirical (Funder & Ozer, 1983) and statistical (Ozer, 1985; Rosenthal & Rubin, 1979) suggests that effect sizes within the broad range considered here (about .20 to .50) are larger and more important than psychologists have traditionally tended to think. Therefore, although we have tried to provide some important information not included

within the Mischel (1984) article, we offer it in the same overall spirit of showing how both personality factors and experimental manipulations of cognition can be important influences on delay behavior.

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#### More on Determinants of Delay of Gratification

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Funder and Harris (this issue, pp. 475-476) have raised anew the question of the comparative strength of dispositional and situational determinants of behavior. They show, through a reanalysis of data cited by Mischel (1984), that neither "situational" nor dispositional variables account

for the majority of variance in delay of gratification. This point, of course, is not new: Bowers (1973) showed that dispositions and situations, as measured by responses on "S-R" inventories of various domains, accounted for only about 10% of behavioral variance each. Although it is true that they have analyzed the differential contributions of dispositional and "situational" variables to variance in actual, as opposed to self-reported behavior, even this is not new (Funder & Ozer, 1983).

Based on data summarized by Mischel, they calculated that "situational" variables account for approximately 20% of the variance (average effect size = .45), leaving the majority of variance unexplained. They also calculated that the dispositional variables listed by Mischel (1984, Table 1, p. 355) account for approximately 8% of the variance (median correlation = .29). However, Funder and Harris correctly noted (p. 476) that the dispositional variables in question were measured 12 years after the behavioral test, presumably restricting the correlations in question. Their analysis carries two implications: Situational effects are not as powerful as Mischel supposed them to be, and dispositional effects might have proved more powerful than they did, had they been measured closer to the time of the behavioral test.

There is no gainsaying the conclusion that the "situational" effects studied by Mischel leave the majority of behavioral variance unexplained. However, Funder and Harris may have underestimated the strength of the "situational" effects and overestimated the strength of the dispositional effects obtained in Mischel's experiments. For example, in calculating effect sizes for the study by Mischel and Baker (1975), they averaged the contrasts for relevant-consummatory ideation versus control and relevant-nonconsummatory ideation versus control, yielding an  $r$  of 0.27. However, Funder and Harris ignored the four contrasts produced by the two factors in the experiment (relevant-consummatory versus relevant-nonconsummatory, relevant-consummatory versus irrelevant-nonconsummatory, etc.), which appear just as germane theoretically (if not more so) and each of which produces effects larger than those cited in their Table 1. Moreover, they failed to note that the correlations listed in Mischel's (1984) Table 1 include *all* those that attained statistical significance, regardless of their theoretical status. As is well known, such empirically derived correlations capitalize on chance and are rather unstable (Bem & Funder, 1978; Mischel & Peake, 1982).

The average effect size calculated by Funder and Harris might well have been different if Mischel's table had included only those variables deemed theoretically relevant on an a priori basis.

In this context, it is curious that Funder and Harris failed to cite available evidence concerning the effects of dispositional variables measured closer in time to the behavioral observation of interest. In a study by one of the authors, but presumably not available to Mischel when he wrote his article, children's delay behavior was observed in an experimental test administered at age 4 (Funder, Block, & Block, 1983). Q-sort measures of various dispositional variables were also taken at ages 3, 4, 7, and 11. Therefore, the study provides an estimate of the effects of dispositional variables specified in advance as theoretically relevant and assessed very close in time to the experimental test. Table 1 shows the correlation coefficients between relevant dispositional variables and delay behavior, aggregated across two experimental tasks. Of special interest are the variables assessed at age 4—very close to the time at which the behavioral observations were made. The original report provides separate analyses of the critical item for boys (Funder et al., 1983, Table 3) and girls (Funder et al., 1983, Table 4). The present table shows weighted averages (Funder and Harris did not analyze for gender differences). Two predictors are particularly relevant: "undercontrol" (a dispositional construct theoretically related to delay behavior), and the critical item "Is unable to delay gratification." According to Funder et al. (1983), resiliency is not relevant except as a sort of moderator variable. As can be seen, the average correlations, obtained so close in time to the behavior in question, are almost identical to the average correlations reported by Mischel, taken 12 years after the fact (undercontrol,  $r = -.25$ ; critical item,  $r = -.27$ ).

Thus, relevant dispositional variables, assessed close in time to the experimental tasks, accounted for approximately 7% of the variance in actual behavior. One does not quite know what to make of this outcome, or of the fact that aggregation (the ego-control scale consists of 100 Q-Sort items) produced no better prediction than the lone critical item. But Mischel's essential point remains intact. If one accepts the terms of Funder and Harris's argument, dispositional variables of the sort measured by the Q-sort are not very powerful determinants of behavior, and they are less powerful than the sorts of "situational" variables commonly examined in experimental studies.

**Table 1**  
*Dispositional Correlates of Delay of Gratification*

Predictor	Age			
	3	4	7	11
Undercontrol	-.28	-.25	-.46	-.32
Resiliency	-.11	.23	.16	-.18
Critical item <sup>a</sup>	-.15	-.27	-.26	-.21

Note. Data from "Delay of Gratification: Some Longitudinal Personality Correlates" by D. C. Funder, J. H. Block, & J. Block, 1983, *Journal of Personality and Social Psychology*, 44, pp. 1198-1213.

<sup>a</sup> "Is unable to delay gratification."

Of course, one need not accept the terms—or, at least, their application in the present instance. Many of the "situational" variables studied by Mischel are nothing of the sort, which is why the adjective has been enclosed in quotation marks. Many are experimental operationalizations that refer to mental states: distracting oneself, thinking about the reward, and the like. Conceptually, these are just as "internal" as the traits measured by techniques such as the Q-sort. The experimental manipulations stand in for naturally occurring "cognitive social-learning *person* variables" of the sort discussed by Mischel (1973, p. 264, emphasis added). And they are quite different from the sorts of environmental variables considered by classic experimental social psychology (see Funder & Ozer, 1983). The person has not been abandoned by cognitive social learning theory, as some critics would have it; rather, the personality variables relevant to the explanation of individual behavior have been reconstrued in terms of flexible mental structures and processes, rather than the stable and consistent behavioral dispositions envisaged by conventional psychometric approaches.

Perhaps the time has come to end the battle of the correlation coefficients between Cronbach's (1957) two disciplines; perhaps this is a vain hope. In any event, nothing seems to be gained by constructing a false dichotomy between "personality factors and experimental manipulations of cognition" (p. 4): Cognitive structures and processes are part and parcel of personality.

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## Reply to Kihlstrom

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The circumstances under which experimental and personality effect sizes are calculated are so different that it is probably best not to take too seriously their absolute magnitudes. All one can conclude safely from comparing them is that they lie on overlapping distributions. One kind of variable cannot be chosen as more important than the other on the basis of effect size. That was the central message of our comment, and it still stands, regardless of what one makes of Kihlstrom's recalculations in his comment (this issue, pp. 477-479).

Personality variables as assessed by an instrument such as the Q-sort and experimental manipulations of cognition such as are studied by Mischel and his followers are overlapping constructs that are similar in some ways and different in oth-

ers. Both involve psychological processes that must ultimately lie inside the head; but personality variables also refer explicitly to observed patterns of overt social behavior, whereas cognitive variables also refer to explicitly hypothesized mediating processes. One can claim "both . . . can be important" (our phrase) or that they are "part and parcel" of the same thing (Kihlstrom's phrase). The two conclusions are not very different, and are both correct.

The purpose of our comment was *not* to establish the superiority, nor even precise equivalence, of personality variables relative to cognitive ones. As mentioned already, that may not be possible, in principle. Rather we wished to help "end the battle of the correlation coefficients" by countering the frequent implication (found again in Kihlstrom's comment) that cognitive factors can be preferred on the grounds of effect size alone.

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## The Alcoholism Controversy Revisited

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In his response (Marlatt, 1985) to my criticisms (Wallace, 1985) of his earlier article (Marlatt, 1983), Marlatt suggested that he was accused of ignoring biological factors in alcoholism. In fact, I did not criticize Marlatt for neglecting psychobiology but did criticize Nathan and Wiens, the action editors of the special volume, for failing to provide readers with rapidly developing information in behavioral genetics, psychopharmacology, and behavioral neurochemistry.

More important, however, was Marlatt's apparent distress with my criticisms of the Sobell and Sobell (1973) research on controlled drinking for hospitalized *gamma* alcoholics. Marlatt apparently believed that the Sobell research was methodologically adequate, particularly with regard to assignment of subjects to

conditions. I do not believe that the Sobell research was methodologically adequate and, among other things, do not accept their procedures for random assignment of subjects. The Sobells reported using a procedure of coin flipping by the experimenter as the means for achieving random assignment. Because coin flipping can be influenced both intentionally and unintentionally by experimenters, use of a table of random numbers is clearly the acceptable procedure.

That a less than adequate procedure for random assignment in this instance resulted in a statistically significant order effect in the Sobell research (Pendery, Maltzman, & West, 1982) was not commented on by Marlatt or Doob (1984). It may be, however, that arguments over random assignment in the Sobell research are simply wasted energy because, in the final analysis, the treatment procedures employed did not result in either *sustained controlled drinking* in the controlled drinking projects or *stable abstinence* in the abstinence-trained subjects. In effect, from a more general perspective, the Sobell experiment was simply another "no effect" and, hence, "no difference" study. Comparisons between two treatment conditions, neither of which had an important, reliable effect, is not fruitful regardless of how subjects may have been assigned initially.

In the final analysis, the intriguing question raised by the Sobell research and by Marlatt is not, "Can hospitalized *gamma* alcoholics be taught to drink in a controlled manner and can this teaching result in sustained controlled drinking over time?" For the vast majority of hospitalized *gamma* alcoholics, the answer to this question is clearly, "No, they cannot be taught to do this." The Sobell research did not reverse this commonsense observation and traditionally held view about alcoholics.

The intriguing question is, "Why have we continued to argue about a no-difference study for over a decade?" The answer to this question would surely constitute an interesting study in the sociology of knowledge.

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