The point of view represented in this 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.

REFERENCE

A more detailed response to Kihlstrom is available from David C. Funder, Department of Psychology, Harvard University, 33 Kirkland St., Cambridge, MA 02138. After July 1, 1986, correspondence should be addressed to David C. Funder, Department of Psychology, University of Illinois, 603 East Daniel St., Champaign, IL 61820.

**Reply to Kihlstrom**

David C. Funder
Harvard University

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 others. 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**

John Wallace
Edgehill Newport

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 (Pendergast, 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.

REFERENCES