that? The population under study and the naturalistic limitations of the study design direct researchers not to stray too far from the specific results. But, importantly, the uniqueness of the study—its large sample size and independent sponsorship—may, in our view, help to expand the scope of subsequent research and push the envelope in creative design. Indeed, as we close this chapter of our own scientific inquiry on the subject for the present, we are hopeful that our next research initiative in a few years or so will rest on significant interim advances. Until then, we are appreciative of ideas and suggestions for improvement.

In this issue of the *American Psychologist*, we do not want to participate in a debate that values either experimental or naturalistic design more than the other. Both have value depending on the question to be addressed. We chose the survey method to answer the general question of therapy's effectiveness in a real-world setting. Right alongside the survey results in the November 1995 *CR* article, we published an entire page about the specific types of therapy that are best suited for specific problems and acknowledged that these recommendations were based on controlled studies.

We want to acknowledge Martin Seligman's role as a consultant in our current research; his insight and expertise were invaluable in helping us design our questionnaire and conduct the analysis. At the same time, we reaffirm our independent position on the interpretation of these data. The *CR* (1995) article and this comment (and nothing else) outline our position on the results of our study and its ramifications. In summary, we think prospective consumers of mental health care services have benefited from this research. We hope the scientific method in this field has advanced in a meaningful way as well.

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Errors in Seligman's "The Effectiveness of Psychotherapy: The *Consumer Reports* Study"

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Seligman's (December 1995) article presents an interesting view of how a consumer survey can be used to evaluate psychotherapy as a healing technique. The article undoubtedly will and should be influential. Unfortunately, though, the article is marred by three methodological errors and a puzzling inconsistency.

The first error—the typographical slip—is the bane of all of us. Figures 2, 3, and 4 (Seligman, 1995), which present some of the major data, incorrectly report either the total size of the sample or the size of the various subgroups receiving therapy from different sources. This error is easy to see: The total *N* is given as 2,738, but the subgroup *ns* add up to 2,938. If the error is in the total *N*, no material conclusions are affected. This is probably the case, as mistyping a single digit could have produced the error. However, if the error is in one of the subgroup *ns*, the seriousness depends on which subgroup is affected.

Figures 2–4 (Seligman, 1995) purport to show three things: (a) that therapy extending for more than six months produces a higher rate of symptom improvement than therapy extending for less than six months; (b) that the improvement rates are about equal for psychiatrists, psychologists, and social workers; and (c) that the rates for these three types of therapists exceed the rates obtained by physicians and marriage counselors. Visually, the graphs dramatically make these points. However, neither the figures nor the text presents the numbers or percentages of clients who received brief (less than six months) or extended (more than six months) therapy, separately for type of therapist. For instance, in the figures, psychiatrists have a slight advantage over psychologists both for clients who received brief therapy and for clients who received extended therapy (in Figure 1, 32% and 52% major improvement for psychiatrists vs. 31% and 46% major improvement for psychologists). But suppose that psychologists see a higher percentage of clients in extended therapy than psychiatrists do—a fact not addressed in Seligman's article. Then the overall rates of improvement could be markedly higher for psychologists than psychiatrists. Or the reverse could be true. What these figures do show is that, conditional on length of therapy, the statements about the equivalencies of therapists with different training are true. This is not a trivial alteration of the original statement.

Seligman (1995) stated, "The dropout rates due to the resolution of the problem were uniform across duration of treatment (less than one month = 60%; 1–2 months = 66%; 3–6 months = 67%; 7–11 months = 67%; 1–2 years= 67%; over two years = 68%)" (p. 971). The dropout rates were anything but uniform. Each interval reported covers a different, progressively increasing number of calendar months. Of the people who were still in therapy at the start of Month 1, 67% resolved their problem in the following month. Of the people who were still in therapy after 1 year, 67% resolved their problem in the next 12 months. The dropout rate was 67% per month in the first case and an average of 6% per month in the second case.

As a further puzzle, if the "successful resolution" figures just cited are correct, all but 5% of the sample should have left therapy with successful resolution after six months (60% would leave in less than one month; of the remaining 40%, 67% would leave in the next month, etc.). This is difficult to reconcile with Figures 2–4 (Seligman, 1995), which uniformly report major improvement rates of less than 33% for clients with six months or less of therapy. Either successful resolution of a problem by psychotherapy does not entail major improvement or, as is more likely, survey respondents are very sensitive to exactly the way in which a question is worded.

These errors are not at all innocuous. They are directly relevant to the cost-effectiveness of psychotherapy, a topic dear to the managers of HMOs. If Figures 2–4 (Seligman, 1995) are to be believed literally, general psychotherapy clients should be channeled to social workers unless some specific requirement (e.g., need for drugs) indicates that medical intervention is required. This leaves little place for the clinical psychologist. When dropout rates are correctly interpreted, the HMO managers' dilemma becomes apparent. The managers' costs for psychotherapy will generally increase linearly with the length of treatment. The dropout rates indicate that the longer a person has been in psychotherapy, the lower is the probability of successful termination in the next month. Weighed against this, of course, is the very real possibility that people in long-term therapy may have more serious problems than clients who can rapidly complete treatment. This will have to be quantified so that cost–benefit calculations can be made. Seligman made an impressive argument for using surveys to assist in such calculations. What researchers now need to do is to refine the ways in which they treat the resulting data.

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