Spearman Correlations of .60 Are Not Poor

For many years, therapists have assumed a relationship between impairment and disability. More recently, researchers have begun to examine this assumption. "Motor Function and Activities of Daily Living Assessments: A Study of Three Tests for Persons With Hemiplegia" by Filiatrault, ArsenaULT, Dutil, and Bourbonnais (AJOT, September 1991, pp. 806-810) is a laudable example of such an examination. I have, therefore, no desire to malign their important work. What I want to address is the authors' interpretation of their Spearman coefficients. Although the matter may appear trite at first glance, it arises often in reports of correlational research and may influence the use of research by clinicians.

Filiatrault et al. described as "poor" Spearman correlations of .60 and .61. Granting that it is important to judge correlations on more than mere significance, I believe that the authors' description of the correlations as poor may devalue them excessively. Published interpretations of correlation magnitudes tend to categorize correlations as follows: < .20 = very weak; .20-.40 = low, small, weak; .40-.70 = moderate; .70-.90 = high, strong, marked; > .90 = very strong, very high (Huck, Cormier, & Bounds, 1974; Okolo, 1990; Rowntree, 1981; Williams, 1986). Thus, the correlations reported by Filiatrault et al. might be considered moderate. Although correlations of moderate magnitude leave the majority of variance between two variables unexplained, they should not be discarded as unimportant when obtained in studies of human beings. Feinstein (1985) wrote that "considering the variability of medical data, r values of .5 or higher are regarded as quite good." Similarly, Cohen and Cohen (1983) indicated that in the nonphysical sciences "where relationships are not strong, correlations (of whatever kind) only infrequently are as large as .50" (p. 144). In a later publication, Cohen (1988) wrote that workers in pure and applied psychology (i.e., clinical, educational, personal) "normally encounter correlation coefficients above the .50-.60 range only when the correlations are measurement reliability coefficients" (p. 78).

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References

Issue on Clinical Reasoning Lacks Papers on Mental Health

I found the Special Issue on Clinical Reasoning (November 1991) most interesting and informative. I particularly liked the interdisciplinary nature of the articles, noting that many of the original sources were from philosophy, sociolinguistics, anthropology, and medical sociology. As an educator with 15 years of recent clinical experience, I understand the importance of clinical reasoning and of simply helping people to think. Other fields enhance and enrich our own ability to provide "good enough" treatment.

I was disappointed, however, that the field of psychology was not represented; nor was there an article that studied the clinical reasoning process of an occupational therapist who worked in the area of psychosocial dysfunction. I hope that this does not represent the total Clinical Reasoning Study funded by AOTA, that other fields of knowledge will be used, and that occupational therapists who work in psychological and social rehabilitation will be studied. This is important for two reasons. First, the thinking processes that were described and explicated are usually second nature to occupational therapists who work in psychosocial functioning. This is the way that they routinely and consciously carry out treatment. Second, there is much talk, at least in California, of the shrinking practice base in psychosocial dysfunction. Lack of focus on psychosocial occupational therapists and lack of interest directed toward their knowledge base minimize their contributions.

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Correction

For "Efficacy and Efficiency: Self-Designed Versus Instructor-Designed Study Tools" by Erica B. Stern and Ruth S. Hassanain (March 1992, Vol. 46, No. 3, pp. 253-258):

The final sentence of the abstract should read, "The differences in efficacy and efficiency were similar for students of different learning styles (as classified by Witkin's field-dependence/field-independence continuum)." In addition, the final source in Table 1 should read, "Test × Card × Field." The AJOT editorial staff regrets this error and hopes readers were not inconvenienced.

Correction


In the first sentence of the last paragraph on p. 225, the citation given for the Movement Assessment of Infants was incorrect. The sentence should read, "The Movement Assessment of Infants (Chandler, Andrews, & Swanson, 1980) was developed by physical therapists in response to a need for a neonatal intensive care unit follow-up evaluation tool." The AJOT editorial staff regrets this error and hopes readers were not inconvenienced. ▲

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