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# Functional Relations Are Not Models: A Note on Covariation Detection

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Surber's (1986) arguments about testing covariation detection models and about assessment of variable importance are essentially correct but inappropriately applied to analyses of functional relations. Our main points were as follows: (a) Past theorizing in several domains assumed that all *r*s that are equivalent in Pearson's terms are perceived as equivalent; (b) our data invalidate this assumption; (c) functionally, error variance may be the most important determinant of covariation judgment; (d) several models may account for these functional relations, but we have not tested these models; (e) researchers comparing human judgments with normative models such as Pearson's *r* need to be aware of the obtained functional relations to avoid misinterpretation of deviations from normativeness.

In a recent article (Lane, Anderson, & Kellam, 1985), we investigated the effects of the variance of *X*, the regression slope, and the error variance on subjects' judgments of the relations between variables. The major conclusion of our research was that different ways of producing relations with the same value of Pearson's correlation lead to different judgments of relatedness. In her commentary on our article, Surber (1986) argued that we had not succeeded in testing Pearson's correlation as a psychological model of covariation judgment although she acknowledged that we conceptualized our experiment as examining the functional relation between the three cues and judgment of covariation rather than as a test of Pearson's equation as a psychological model. This latter acknowledgment is at the heart of our disagreements. Specifically, we neither attempted to test nor claimed to have tested Pearson's correlation as a psychological model of judgmental processes underlying covariation detection. We simply examined the functional relation between the three components of Pearson's correlation and subjects' judgments.

Our research was stimulated, in part, by the assumptions and methods that appear in several different theoretical contexts concerning covariation detection. For example, in domains as diverse as attribution theory and behavioral learning models it is assumed that organisms can and do detect environmental covariations. Within these and various other domains, examinations of covariation detection capacities have compared subjects' judgments to some normative model (such as Pearson's *r*) and have drawn general conclusions from systematic departures from the model. The findings of Jennings, Amabile, and Ross (1982), for example, have been interpreted as demonstrating that people use a generally conservative strategy in theory-free data evaluation. But past research has failed to consider that different choices of covariations in preparation of stimulus materials (i.e., materials that produce the same *r*s using different slope, error, and variance of *X* components) may lead to different functional relations between *r* and subjects' judgments. That is, past research has assumed that all *r*s that are equivalent in Pearson's terms also are equivalent in the eyes of subjects. Our main point, based on our data, was that this assumption is invalid.

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Obviously, there are a number of ways in which the obtained differences in the judged relatedness of identical *r*s could arise. One possibility is that subjects weight the three components of *r* differently than in the Pearson formula. Another possibility is that people make holistic judgments, not componential ones. A third possibility is that subjects misperceive the components themselves, but then combine the (incorrectly perceived) values as in the Pearson formula.

Surber apparently believes that we intended to test the Pearson model and that we claimed evidence to reject the model. The third possibility above is explicated by Surber as an example of how our (supposed) rejection of the model could be premature. Had we claimed to be testing that model and then rejected it, Surber would be correct. But the careful reader will note that we *did not* claim to be model testing nor did we claim to have disproved any particular model. Indeed, we even explicitly mentioned this third alternative as a possibility in the discussion as follows: "It could be argued that subjects' judgments of relatedness differ for scatterplots with the same value of Pearson's correlation because subjects perceive the components inaccurately rather than because they combine these components in a different manner . . ." (p. 648).

Our position is that we have uncovered an empirical phenomenon that is interesting in its own right and deserving of further study. We also feel that researchers who use normative models of covariation detection as a basis of comparison for human judgments need to be aware of our findings so that they do not misinterpret their own. Finally, one of the goals of further research should be to test various explanations of the discovered empirical phenomenon. Surber obviously agrees with this latter goal, just as we agree with her that testing such explicit models is not easy and that the problems she mentions in model testing are important. But we never claimed to be testing a model and never claimed that one or the other of the possibilities mentioned above was proved (or disproved) by our data. Thus, in her efforts to make her model-testing points, Surber has built a straw man and knocked him down.

In the last section of her commentary, Surber took issue with our claim that error variance has a stronger effect on subjects' judgments than does either slope or variance of *X*. She concluded that "Lane et al. have not attempted to use any of the extant approaches and have not articulated their own approach to the

measurement of the psychological importance of variables in judgment" (p. 109). This is simply not true. We were well aware of the problems in measuring the importance of variables in judgment research and took great care to say exactly what we meant when we said that error variance is the most important component. Specifically, we said:

> It appears that the lower the error variance, the higher the judgment of the relation even if the slope and/or the variance of $X$ are reduced so as to maintain the value of Pearson's correlation at a constant level. *In this sense, people are influenced more by error variance than by either slope or variance of X relative to how these factors influence Pearson's correlation.* . . . (pp. 644, emphasis added)

We agree that our data do not demonstrate that error variance is more important than the other two variables in any *absolute* sense, but we did not make such a claim. Surber has built and knocked down another straw man. For the very specific sense in which we used the term, the data provided overwhelming support.

In summary, Surber's commentary contains one possible explanation of the empirical phenomenon we observed. It is essentially the same as one of the three we offered in our article, but it is a good elaboration that may further research on the judgment processes underlying our phenomenon. Although we do not favor this explanation, the question of its veracity (or that of the other possibilities mentioned) is not germane to the points we made in our article.

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