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COMPLEMENTARITY IN MARITAL ADJUSTMENT:
COMMENT

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Regarding the study by Levinger and Sonnheim (1) in which they found no differences in complementarity between normal and disturbed couples, I suspect that the samples were not truly different. If marital counseling was a low-cost community service, and if both

groups were not psychologically naive as seems likely, some of the couples with light conflicts may have felt freer to use such a service than some of the couples with more severe conflicts. I wonder whether the authors' statement about some marriage counselees (among some 20 in all) having higher marital satisfaction scores than the normals does not suggest this.

Furthermore, our experience with verbal and paper-and-pencil tests of marital satisfaction and various other psychological variables has been less than satisfactory. In measuring dominance, aggressiveness, etc. among high school students we found good correspondence with family constellation hypotheses in the case of ratings by classmates and of counts of relevant actual events in the subjects' past and present, but little relationship in the case of self-ratings. The reason may be that a person rates himself high on dominance, e.g., because he has little success in reality with his attempts to dominate others, or because he has a fair amount of success but wants still more, or—sometimes—because he *is* dominant.

I regret that the authors have omitted cases of only children, step-siblings, broken homes and missing information. Losses of family members—which we found correlated also with the amount of information missing on family data—have shown up among “disturbed” and divorced couples, among parents of “disturbed” children, among delinquents and juvenile prisoners as a clear trend. I also regret that objective data (e.g. ages of spouses at wedding, time distance of first child from year of marriage, number of children, sexes of children and their sequences, sibling positions of parents) have not been made use of to a greater extent, if for no other reason than to check whether the two samples can be considered matched.

It should also be remembered that a study of “happily married” and divorced couples, i.e. of groups distinguished by stronger criteria than the authors', yielded data in significant agreement with the duplication theorem (3), and that two other studies, one on monosexual sibling positions among husbands(5), the other on large age differences among spouses (4), traced some factors capable of confounding the duplication theorem.

I agree with Levinger's and Sonnheim's arguments on similarity and dissimilarity as bases for interpersonal relationships. I have differentiated interpersonal relationships of identification and those of direct intercourse, i.e. parallel and complementary relationships (2, p. 127). Those types or aspects of interpersonal relationships,

however, can be more clearly disentangled than the authors seem to believe. I do not agree with the authors' somewhat pessimistic outlook on this complex field of research. I would say, though, that it calls for larger arrays of data and more persistent theorizing than various clinical and social psychologists seem to have patience for.

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COMPLEMENTARITY IN MARITAL ADJUSTMENT:
REJOINDER

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In his comment on our paper (1), Toman (6) makes several points which we should like to discuss. His first point, that our samples of normal and disturbed couples differed less in marital conflict than did his own groups, is a matter for speculation. One would wonder why parents of children in psychotherapy (7) would necessarily have any more severe marital conflicts than agency couples. In our two groups there was slight overlap in a composite factor index of marital satisfaction, but the difference between the groups was very large ($t = 4.78$, $p < .0001$).

His second point, concerning the validity of paper-and-pencil self-ratings of dominance, is a valid criticism. Nevertheless, Table 5 and further data presented in conjunction with it give no reason to believe that actual back-home relative influence was associated with birth order. Furthermore, Jones (3) and Murphy, Murphy, and Newcomb (4) have reported contradictory and inconclusive evidence on this topic, and Schachter (5) has maintained that first-borns are more dependent and influencible than later-borns.