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Gentlemen:

Enclosed are two copies of a manuscript, proposed for publication
in the Real Estate Journal.

Thank you.

Yours truly,

M.E. Spautz
Senior Lecturer in
Management Studies

Encl. 1

c.c. A.J. Williams

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FACTORS WHICH MAKE FOR FAILURE - IN BUSINESS AND ACADEMIA

by M.E. Spautz, Ph.D.
Senior Lecturer in Management Studies
University of Newcastle

In the April-May, 1975 issues of the Real Estate Journal, A.J. Williams published a 2-part research report. In it numerous statements were made, the correctness of which is open to question. Here is a point-by-point criticism, working from front to back.

1. On page 18 of the first part, he quotes his doctoral thesis as follows:-
"Ill-health and a distinct pattern of emotional stress were evident in a large number of the affected families. Marital tension was also apparent in 9 percent of the cases studied." I have studied the cited thesis and can find no evidence in support of this quotation.
2. In the very next sentence he describes this "emotional stress" etc, in terms of the "impact of 863 failures", which clearly establishes the emotional stress as a dependent variable (or effect), not as an independent variable (or cause). Granted, emotional stress is usually both, but here he is discussing the last two links in a causal chain. To continue: On page 22, in describing failed businessmen, he wrote: "For so many of them, the whole experience will prove a traumatic and debilitating lesson - one which may take years to fully recover from." In the present research study, this sort of failure-induced stress response is called "Affective Reaction", although it was measured in strictly verbal terms, as we shall see in the next point. However, despite the obvious dependent status of Affective Reaction, the author's flow-chart on page 14 of the second part depicts it as occurring before small business performance (success vs. failure)!
3. On page 14 of the second part of the article, he describes another study from that same thesis, in which he tested and interviewed 250 small businessmen, including 75 failures (bankrupted). In the interviews which he personally conducted to get the data, he obviously knew which ones were failures, thereby opening the door to biases (probably mostly unconscious) in favour of his hypotheses. A more rigorous approach would have involved using trained interviewers and a "blind" research procedure. On page 15, he states: "Data were

collected on all possible factors". Since numerous potentially important factors, such as intelligence, creativity, ethical attitudes and practices, ethnic and racial heritage, and political persuasion were not measured but could have been, surely this must be a gross overstatement!

4. On page 15 he provides a tabular summary showing a list of alleged predictor variables in relation to a criterion. The nature of the criterion is not indicated in or near the table itself, but is rather buried in small print on another page. In view of the title of the second part, "Factors Which Make for Success", the casual reader would naturally but incorrectly tend to infer that the criterion was success, and not Affective Reaction.
5. According to information in the referenced doctoral thesis, some of the page 15 tabulated variables are bimodally distributed - and so is the criterion. Since the tabulated rankings are "based on Pearson product-moment correlations", which cannot be legitimately calculated from such data, the entire table must be rejected. (Incidentally, the term "ratings" would be more appropriate than the term "rankings" in this context.)
6. Also on page 15, he writes: "Most successful entrepreneurs tend to be aggressive/detached, rather than compliant in their interpersonal responses". I can find no evidence, either in the articles being criticized, or in the referenced thesis, to support this statement. Again, since these alleged predictor variables were measured after the failure of 30% of the subjects, the author seems to have got the alleged cause-effect relationship backwards. Interestingly, in his thesis he wrote: "To establish that A could be a cause of B, then A (the independent variable) must occur or change before B" (p. 6). Clearly, the author has not taken his own advice on this critical matter!
7. On the same page he makes similar claims about other alleged predictors, in some cases without adequate evidence. A recurring error is in transmogrifying high correlation coefficients and large chi-square values, whether spurious or not, into the claim that "most successful entrepreneurs ..." Such a claim would be

inappropriate, in some of these cases at least, even with valid statistics!

8. In that same table he identifies "marital and family responsibilities" as very important (4 on a 5-point scale). In the unpublished thesis (p. 390), however, the relevant chi-square value is non-significant, and the correlation coefficient is only 0.25 (rounded). To be consistent with other such values in the table, rank 2, not 4, should have been used. Here he has apparently reported his own results incorrectly, although on a relatively minor factor.
9. On page 16 he writes: "Scores on Affective Reaction are significantly correlated with Small Business Performance scores (correlation coefficient is .8992) ..." Several comments:
 - (a) Nowhere in the article is "Small Business Performance" defined, which makes it impossible for a critical reader to properly evaluate his conclusions.
 - (b) In the unpublished thesis, it is well defined, but is distinctly bimodal in distribution.
 - (c) The same for Affective Reaction, as well as Achievement Motivation and other important alleged predictors.
 - (d) In the unpublished thesis, the bivariate distribution (scatter diagram) of Affective Reaction and Small Business Performance is shaped like a dumbbell, owing to the marked bimodality of both variables. Because computation of Pearson r with such distributions is illegitimate, the reported value of .8992 is spuriously high - not to mention also backwards in terms of the alleged causal direction as depicted on page 14 of the second part. Judging from the shapes of the two ends of the dumbbell, a reasonable estimate of the correct correlation, with pooled data, would be about .40.
 - (e) Note the unusual reporting of the correlation coefficient to four decimal places!
 - (f) In the unpublished thesis the reliability of the Affective Reaction variable is reported as "0.8341 (significant at

$p < .001$ in a two-tailed test...)" (p. 318). Surely in this case a one tailed test would suffice to guard against error! Also, that value is difficult to reconcile with the validity coefficient of .8992, since reliability supposedly sets an upper limit on validity.

- (g) Even if the value of .8992 were correct, the statement that "individuals who can adapt to and cope with the stress of managing a small firm are the best performers, as measured in this study" would not be warranted by the evidence presented. (This is not to say that the statement is false, or that stress resistance is not important for success.)
 - (h) The conclusion as to the importance of Affective Reaction as a determinant of success in Australia is inconsistent with the results of the Dun and Bradstreet study (cited on page 17 of the first part), which attributed 92% of business failures in America to incompetence and inexperience. By his own admission, "the above pattern is true in this country also". How then could he later invoke post-failure Affective Reaction as the most important variable, "The Key Factor", in small business performance?
10. On page 16 he draws the following conclusion, which is not supported in the article: "Protestant owners perform better than those from other religious groups". I have studied the information in his unpublished thesis, and make the following observations:
- (a) Amongst the relatively successful subjects only (i.e. those with performance scores above 18, which is roughly the "success" group, with $N = 174$), Protestants and Jews are equally (50%) proportionally represented at the highest levels of success (i.e. at scores above 48).

(b) Amongst the relatively unsuccessful (failed?) subjects only (i.e. those with scores below 19), about 18% of Protestants and 0% of Jews had scores below 7. This set of observation runs counter to his conclusion!

Admittedly, these are small samples, especially of Jews (n = 13). However, his conclusions should have taken account of that fact. A reasonable prediction would be that in a larger scale study Jews would prove to be at least as successful as Protestants, owing to their oft-proven superior intelligence as well as other advantages associated with success. (For example, a 1959 study by B.C. Rosen, cited in Williams' thesis, found Jews to be higher than Protestants on need for achievement, although the difference was not statistically significant.)

In summing, it seems that the main conclusions of Mr. Williams' report, and therefore the doctoral thesis which it encapsulates, is of questionable validity, owing to serious elementary methodological errors and misinterpretations. A thoroughgoing reanalysis would require access to the original raw data base, which he is understandably reluctant to allow. Instead, he has promised to rework his statistics and publish a correction. In Vita Veritas.