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Questionable Chronometry: Does Antarctic Isolation Produce Cognitive Slowing?

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An earlier study (White, Taylor & McCormick, 1983) investigated cognitive processing in police recruits and personnel exposed to Antarctic isolation. It was concluded that: (1) "An increase in the rate of change in processing time under Antarctic conditions was found, and (2) wintering over increased overall item completion time." Contrary to these conclusions, the present paper questions the procedure and appropriateness of the data analysis. Cognitive slowing has not as yet been substantiated as an Antarctic phenomenon. Recommendations for improvements in research methodology are also noted.

Subjective data obtained from interviews in Antarctica have provided support for the notion of cognitive slowing following Antarctic isolation (Mullin, 1960; Mullin & Connery, 1959; Natani & Shurley, 1974: Rivolier, 1974; Taylor, 1980). Contrary to these intriguing anecdotal findings, controlled investigations involving direct objective measures of cognitive performance have shown slight improvements or no significant change in cognitive function (Barabasz & Gregson, 1978, 1979; Gregson, 1978a, 1978b; Ventsenostev, 1973). These later results are consistent with restricted enviromental stimulation laboratory findings reviewed by Suedfeld (1980, p. 37-40). The recent study by White, Taylor and McCormick, (1983), published in the *New Zealand Journal of Psychology,* concluded that: (1) "An increase in the rate of change in processing time under Antarctic conditions wås found, and (2) wintering over increased overall item completion time." The study has attracted some international attention because it is the first "objective measures" investigation to support the early anecdotal reports. Unique Antarctic findings can become rather prominent in media intended for the general public so scrutiny within professional disciplines becomes especially important.

To evaluate the new results of White, Taylor and McCormick (1983), it is important to know precisely when and where the initial and final testing of their sample of Scott Base personnel was completed. How the mid-winter dates of August, 1979 and August, 1980 shown in their caption to Fig. 4, page 39, have any comparability with the earlier Barabasz and Gregson studies cited above is not clear. Scott Base data from the earlier studies were collected in December, after Ss completed an initial enviromental adjustment period, and in October immediately following Antarctic winter isolation. The August to August mid-winter data collection points are particularly puzzling as pre-post measures since the entire Scott Base winter over staff is replaced every October. If the data collection times are in fact August, 1979 and 1980 "pre" — "post" winter data would have been obtained on different groups of Antarctic Subjects.

Serious problems also appear in the data analysis section. White, Taylor and McCormick (1983) confuse times to task completion and rates of completion. Evidence of Antarctic cognitive "slowing down" is based on an interaction between groups (Scott Base personnel versus police recruits) and test items, which relies on item processing times in seconds and not on rates, in terms of items completed per unit time (White, Taylor & McCormick, 1983, p. 39, col. 1). The distinction is, unfortunately, a crucial one as soon as an argument rests upon the statistical significance of interactions, let alone their post hoc interpretability. The procedure employed has to be robust against errors of estimation which can

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arise from at least three different sources: (a) the raw data are discontinous scores or are heteroscedastic or both, it being commonly known that reaction or task completion times can have very skew distributions and the skewness varies across subjects; (b) the statistical model used is not valid, if there are F ratios less than unity the scources implicated can be uninterpretable and we have not been given the full design; and (c) the model and its effects have to be robust under all reasonable transformations of the dependent variable. An example of this problem and its treatment, appears elsewhere (Gregson, 1984, p. 39, table 7).

Source (c) is the most suspect aspect, as it is known (Box and Cox, 1964, Box and Tiao, 1973) that interactions in ANOVA are or can be very sensitive to variable transformations. Taking an inverse can create or destroy an interaction unless it is robust over the range of transformations employed. An observed alpha level of .05 is, in a small empirical study, itself an error-prone decision rule, not a sharp reliable point. Particularly when prior results suggest that the effect studies can be suspect on psychological grounds, very cautious and not permissive significance levels are mandatory. Box (1980, p. 396) noted "efficient model building requires both *diagnostic checking* and *model robustification* (his italics), where by robustification I mean judicious and grudging elaboration of the model to ensure against particular hazards. Robustification becomes necessary when it is known that likely, but not easily detectable model discrepancies can yield badly misleading analysis". If we know something a priori, it may be disastrous to omit it.

The earlier work (Barabasz 1979, 1980; M. Barabasz, A. Barabasz and Mullin, 1983; Barabasz & Gregson, 1978, 1979; Gregson, 1978a, 1978b; Ventsenostev, 1973) revealed several significant findings about wintering over, implicating sensory adaption and desensitisation in olfaction, no shifts in associated reaction times to odours, shifts in suggestibility and hypnotizability in an increasing direction, associated reduction of EEG amplitudes for real stimuli and enhancement of EEGs to suggested stimuli, slight cognitive performance improvement in recall on a symbol string identification task, stability in elapsed time estimations with very big individual differences in accuracy, and stability in gustatory qualitative perception.

It is worth noting that Rivolier (1975) used a battery of tests including D48, measuring "rapidity of Comprehension" on French personnel in Antarctica but does not report any specific slowing-down in this context. He did however report a great diversity of maladjustments from obsessional psychasthenia to hypermania. Given Rivolier's much larger samples it seems wise to sit with the open view that anything can happen, depending upon the levels of stress induced in the working enviroment; mean results are almost meaningless.

This total pattern seriously underscores the need for tests which are opaque in their purpose to the subjects, given the prevalent and widely known Antarctic folklore about "slowing down", and tests should be double blind with regard to the beliefs of the subjects and the theoretical commitments of the investigators. This issue is especially important in Antarctic research because cues in the design or procedure might communicate E's hypothesis and lend the subject to provide data confirming E's predictions. Orne (1959) referred to these subtle cues as "experimental demand characteristics". Orne (1959) advised that demand characteristics be assessed by post experimental inquiry using an independent E with no prior involvement in the experiment. Such a person, using clinical interview techniques is more inclined and able to elicit information from subjects that they might hesitate to give to E who has been involved in data collection. The procedure has been employed successfully in a laboratory study (Barabasz, 1981) and certainly should be considered for use in Antarctic research on cognitive function.

Unless it can be shown with reanalysis that the new results of White, Taylor and McCormick are tenable, and are specifically related to a period of wintering-over independent of data collection demand characteristics, the conclusion of Barabasz and Gregson (1978) stands, namely "reports of decrements in Antarctica have . . . to be well substantiated and based on more than self-reports in interviews or paper and pencil tests".

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