Peter Harzem and Michael D. Zeiler (Eds.)
Advances in Analysis of Behaviour, Volume 2. Predictability, Correlation and Contiguity.
Chichester, England: Wiley, 1982.
Pp. 430.

Michael Zeiler and Peter Harzem (Eds.)
Advances in Analysis of Behaviour, Volume 3. Biological Factors in Learning.
Chichester, England: Wiley, 1983.
Pp. 410.

Reviewed by K. Geoffrey White.

Fashions in experimental psychology tend to prevail whenever research funds are in short supply, as at present. Yet the enduring problems of psychology persist as constant reminders that basic research on fundamental questions about behaviour is essential to sustaining the entire discipline. Volumes such as the *Advances in Analysis of Behaviour* series edited by Professors Harzem and Zeiler are thus important contributions to the enterprise.

The few chapters in Volume 2 of the series are loosely organized around the contingency-versus-contiguity problem. Most contributors address the problem fairly indirectly. Catania and Keller (Chapter 4) make perhaps the most serious attempt to deal with it, with a clear and incisive analysis of what is important in a contingency. Their main idea is that a contingency reflects a causal relation between response and reinforcer and that the sensitivity of behaviour to contingencies can be based on any of a number of "contingency features" such as the distribution of interreinforcer intervals or interreinforcer intervals without responses. In some neat experiments, Catania and Keller examine the effects of a transition from VI schedules, where a contingency exists, to VT where it is removed, or to VI with variable delay of reinforcement. The latter procedure is interesting because it has similar properties to VT but retains the contingency. The data show that responding is maintained under VI but drops out with an equivalent VT. Clearly the contingency is essential, although as Catania and Keller point out, the contingency features that are effective at any given time may vary. The logical and empirical arguments offered by Catania and Keller justify pigeon's pecks as worthy causes.

Although other chapters in Volume 2 deal with the organizing theme less directly, they provide very useful summaries of research in particular areas of current interest. Good examples are Hineline's chapter on avoidance, Lea's on foraging, and Fantino's on his delay-reduction hypothesis. One fine contribution in Volume 2 is Harzem and Harzem's scholarly treatment of inhibitory properties of reinforcers (Chapter 3). Their argument that reinforcers are unconditioned inhibitory stimuli is supported by the occurrence of post-reinforcement pauses (PRP) on schedules with consant reinforcement probability. They also describe some new data showing that PRP increases with an increase in the magnitude of continuous reinforcement (where reinforcement probability is always 1.0). in that the PRP contributes to response rate, this result is counterintuitive — it suggests that increases in reinforcement magnitude generate reductions in response rate. But it helps to make sense of the chaotic literature on reinforcement magnitude that otherwise didn't take PRP into account. Harzem and Harzem point to the biological utility of the inhibitory effect of each reinforcement — an animal can switch to other stimuli while feeding, for example. They also mention its consistency with the logic of Bishop Butler who observed in 1726 that if man was continuously to seek pleasure, the unchecked pleasures would emaciate him and lead to his destruction. "God, thought Bishop Butler, would never have created such a man". And thanks to the unconditioned inhibitory properties of reinforcers as proposed by the Harzems, God didn't have to.

One theme that did keep cropping up throughout Volume 2 was that for behaviour to be sensitive to contingencies, animals must be able to integrate events over time and to respond on the basis of correlations among reinforcement rates and response rates (Catania and Keller, Hineline) Foraging, for instance, requires animals to be sensitive to reinforcement rates over extended periods of time (Lea). Perhaps it was inevitable that the problem of causes for behaviour under reinforcement contingencies should boil down to the question of temporal integration, a process that has seen very little empirical

investigation in either human or animal research areas. Unfortunately, as far as I know, there are no extant treatments that suggest mechanisms for temporal integration.

The chapters in Volume 3 take a generally biological approach to the analysis of behaviour. Many provide very useful summaries of recent experimentation in a given area such as Terman's chaper on circadian rhythms, Logan's attractive description of the role of gene-environment interactions in the development of bird song, Timberlake's ecological analysis of learning using social stimuli (other animals) to predict food or water, and the summary by Maier and others of the impressive programme of research they have been conducting on the role of pain inhibition in learned helplessness. These contain some new and exciting contributions. In general, the chapers in Volume 3 are representative of the current trend towards enhancing the ecological validity of the analysis of behaviour by examining multi-response environments and ecologically-relevant behaviours. This trend is the outcome of the trenchant and often misplaced criticism of earlier treatments of learning on the grounds that learning was subject to biological constraints and that learning principles could not be generalized across species. The chapters in Volume 3 elegantly demonstrate that it is possible to integrate principles governing the development of behaviour within an individual's own lifetime with principles describing evolutionary bases for behaviour. In this regard, Shettleworth's (Chapter 1) distinction between mechanism and function is useful in that it distinguishes the biologically adaptive functions of specific forms of learning from mechanisms for learning which may be unrelated to specific adaptive behavioural consequences.

The biologically-oriented research represented by Volume 3 does indeed represent an advance on the state of affairs of 10 years ago. A major drawback to the biological or ecological approach, however, is that in the absence of the quantification that characterizes current laboratory-based experimental analyses of behaviour, the approach relies heavily on metaphors derived from naturalistic observation. Foraging is one such metaphor, replete with terminological embellishment (search, procurement, handling, identification) and reified purpose (optimization). Collier's (Chapter 7) distinction btween closed and open economies, for example, largely relies on the strength of the metaphor. In turn, the metaphor relies on the empirical validity of the distinction, thus encountering tautology. Although the translation of familiar and novel laboratory procedures or behavioural processes into naturalistic terms is an attractive proposition, it doesn't generate explanatory power. Future advances may need to pay heed to the old principle of parismony as well as to warnings about descriptions disguised as explanations.

Max Abbott (Ed.)
Community Mental Health Services in New Zealand: Report and Reactions.
Auckland: Mental Health Foundation of New Zealand, 1983.
Pp. 85, $2.00.

Reviewed by John F. Smith.

This slim, soft-covered booklet contains an overview paper on Community Health Services in New Zealand, and, a collection of reactions to the paper from a wide range of persons involved in, or interested in, such human services.

The paper, first presented by Max Abbott in Christchurch in March 1982, commences with a re-airing of some of the basic questions/tensions which always arise when considering the philosophy of the Community Mental Health Movement, that is the clinical medicine model versus the public health medicine model, the individual oriented purative perspective versus a system oriented preventative mode. The fact that such issues occur within larger political/economic contexts is made clear as are the political/power issues which arise within and among professional groups working in this area as they redefine, or are forced to redefine, their roles. The development, implementation and eventual outcome of the United States Community Mental Health Act of 1963 is clearly and critically summarised and leads to coverage of a range of community mental health centres in New Zealand. The community mental health centres out of Carrington Hospital in Auckland, and other centres in

Nelson, Christchurch and the Hutt Valley are described and discussed within their professional and political contexts. The paper concludes with a consideration of and questions for, the future of community mental health in New Zealand.

The reaction statements to the above paper come from an extremely wide range of persons including social workers, psychiatrists, nurses, politicians, epidemiologists and psychologists. Their comments range across political support through constructive and/or parochially defensive comments directed specifically at the paper, to personal statements and thoughts on future developments in the community mental health scene. One suspects that not all writers were aware their comments were to be published given the candour of some and the cryptic nature of others!

The strength of this publication is in its focus on raising pertinent questions about the scope and direction of community mental health services in New Zealand. Its particular strength is that it attempts this within the context of experiences gleaned from overseas, particularly the United States, and the more recent experiences of our own endeavours in New Zealand. This volume should be valuable reading for all psychologists in practice or in training, who are concerned about the provision of efficient and rational mental health services in New Zealand.

John Nicholson & Halla Beloff (Eds.)
Psychology Survey 5.
British Psychological Society, 1984.
Pp. 425.

Reviewed by Ken Strongman.

The fifth survey sponsored by the British Psychological Society covers 15 specialised chapters. According to the editors' introduction, the aims were to produce an eclectic survey from authors who have made significant contributions to their subjects, writing in a style which would appeal to the mid-course psychology student. They describe the result as a mixture of core subjects and peripheral subjects (as defined by their understanding of British University psychology departments) with a strong cognitive thread holding them together.

The book is certainly eclectic. Its chapters range from Computational Vision and Visual Word Identification to Counselling and Delinquency, from Women and Mental Illness to Drugs and Human Information Processing. There is some coverage of Cognition, Perception, Social and Developmental Psychology, Personality, and Clinical and Organisational Psychology. However, the eclecticism is not successfully structured; cognition is hardly the linking concept between, say, trait psychology and social stress.

As all psychologists know, the word "significant" is open to a wide variety of interpretations. Whether or not all of the 17 contributors to *Psychology Survey 5* have made significant contributions to their fields of interest is debateable. It is indisputable that some have; for example, the study of absentmindedness would have been almost entirely forgotten without the work of James Reason, and organisational psychology clearly owes something to Peter Herriott. However, some of the other contributors barely reach significance in their chapters, indeed in some cases before this publication they have barely made it onto the printed page.

Turning to the style of the book, some of the authors have managed to write at the level asked of them. For example, the chapters by Reason, by Wilding and by Warburton & Wesnes do a fine job of communication to the informed but non-specialist psychologist. Others have failed in this. Some (although not many) have produced excellent contributions but at too high a level, Michael Morgan on Computational Vision for example. Whereas some are clearly at too low a level. Examples for this are Norman Tult on Delinquency and Jennifer Williams on Women and Mental Health, although it may simply be impossible to deal with this topic at a more elevated level. In the midst of this eclecticism of topic and style, Michael Billig's article on Political Ideology stands out. It is well written, easily understood, erudite and informative.

For the sake of completeness, *Psychology Survey 5* should be bought for, or by, University libraries. But it should be omitted from individual libraries, unless it appears in a cheaper paperback version. Some of the

articles are worthy and would make good starting points for students interested in particular topics. Overall, though, it smacks of the rather old-fashioned idiosyncracies that for many years characterised British academic psychology (and perhaps still do) and also contains more than a hint of selection of material being based on a jobs-for-the-boys approach. The final impression is of a slightly arrogant production.

A. Ralph Hakstian and Raymond B. Cattell (Eds.)
Comprehensive Ability Battery (CAB).
Institute for Personality and Ability Testing, Inc. 1975, 1976, 1982.

Reviewed by John Adcock.

The publication of this test battery, with the addition of the now available U.S. and Canadian norms, will doubtless give a new boost to more comprehensive measurement in the intellectual area. It is the most ambitious effort yet made on a practical basis. Guilford's monumental work has been more theoretical in its impact and the ETS Kit is still at an experimental stage.

The multidimensional approach to intelligence raises a number of interesting problems. The Spearman/Thurstone controversy has been appropriately solved by the acceptance of a second-order factor, but the relationship of the first-order factors may be rather more complex than is sometimes assumed (Adcock, 1964). Verbal and spatial tests may both involve a similar cognitive ability, but the testees may differ in the degree to which they can manipulate words or spatial images quite apart from the intelligence continuum. Again, there may be an underlying speed of cerebral processing which underlies all three primary factors as has been suggested by Eysenck and co-workers (1982). But when we are concerned with abilities in the plural we assume that for various tasks, typically vocational, the individual abilities may be differentially involved. In evaluating a test battery such as the CAB these points need to be kept in mind, but first let us consider the range of sub-tests involved. There are twenty in all. How well do they span the intellectual spectrum? They can be considered as falling into three groups. There is firstly a group which includes most of the well-attested abilities inspired by Thurstone's research: verbal (V), numerical (N), spatial (S), perceptual speed (P), span, (Ms) and rote or associative memory (Ma), word fluency (W), speed of figural closure (Cs) and inductive reasoning (I). These represent well-recognised concepts.

Another group of tests relate to specific vocational aptitudes: mechanical ability (Mk), spelling skill (Sp), auditory ability (pitch, etc) (AA), aesthetic appreciation (E), representative drawing (RD), and precision of hand-eye co-ordination (A for 'aiming').

In a third group we could place factors which relate to flexibility and originality of meaningful closure, factors which can be regarded as relating to creativity in various forms: ideational fluency (Fi), flexibility of figural closure (Cf), spontaneous ideational flexibility (Fs), originality (O).

With such a comprehensive battery it becomes important to know to what degree the various measures are related. Despite the existence of extensive data to provide norms no tables of intercorrelations are supplied and we are left in the dark as to the factorial structure of the battery. One becomes particularly curious about similar functions across figural and semantic modalities. The high correlation between verbal and non-verbal intelligence tests would suggest that speed of figural closure (Cs) could be related to a corresponding semantic factor, but New Zealand studies (Adcock and Webberly 1971, Adcock & Martin 1971) have indicated that Cs is specifically a figural factor and that the corresponding semantic factor is, in fact, something in the nature of what we often refer to as 'insight', recognising the event as related to our total cognitive reference frame. A comprehensive factor analysis of the battery should greatly facilitate interpretation of the scores obtained.

The layout of the test is admirable. A mass of test data can be administered in minimal time (theoretically about two hours for the battery) and machine scoring is provided for where this is feasible (i.e. the first fourteen tests). Assessment of the results is facilitated in the 1982 edition by provision of norms based on 5841 U.S. and Canadian high school students. These normative data relate to male and female subjects separately. Some of the

tests are omitted for some samples but complete data are given for 2351 subjects. This provides a reasonable basis for the use of the battery at least in North America, and could encourage some N.Z. use.

Reference has already been made to the lack of data with regard to intercorrelation of the tests, but there are some correlation data which indirectly throw some light on this matter. There are a number of tables of correlations between individual sub-tests and success in various school subjects. For 70 eleventh grade males the N score correlates .78 with Mathematics, .67 with Chemistry and .66 with Physics, but only .11 with Biology, while V correlates .47 with English, .36 with French and only .08 with Physics. For the most part S shows very little correlation with any of these subjects. For one group (n=147) the highest correlation is .26 with Social Studeis and the lowest –.04 with Physics. Some of the correlations are almost freakish. It is difficult to accept a correlation of .91 between Spelling and Physics but the fact that the three other groups for which data are available give rs of .39, .53 and .34 does seem to confirm that for physicists spelling and numerical ability are the two prime requisites, in that order. A table giving the means for the combined groups would make these data easier to evaluate.

Multiple correlations between CAB and WAIS scores indicate the degree to which the battery captures the general factor. Correlation with the WAIS Full Scale is .80 and with Performance and Verbal .70 and .69 respectively. At this point it should be noted that a composite score derived from five of the CAB tests (V+2N+I+Ma+2Sp) has reliabilities of .89 for males and .92 for females. This is only slightly below the comparable DAT figures at a cost of only about half the time.

Some comment on the individual tests may perhaps be appropriate here. *CAB 1* follows traditional lines for *verbal ability* (word meanings and proverbs) and *numerical sophistication* (basic arithmetic material), and uses rotated figures for *spatial ability*. Mutilated words are employed for *speed of closure* but here there is the complication that the answer has to be matched against a checklist of jumbled letter groups, one of which, when rearranged, corresponds to the relevant word. It may be queried whether this does not involve too much deductive reasoning. This needs some research, but the fact that the test has a correlation of .63 with Chemistry and only .28 with English reinforces this suspicion.

CAB 2

The first of this group of tests is a variant of the traditional *perceptual speed* tests, the second is an *inductive reasoning* test along established lines, the third is a *flexibility of closure* test in a well-established form, while the fourth is an *associative memory* test (pairing designs with numbers). The fifth test is of *mechanical knowledge*, a rather specialised vocational test. It is worthy of comment that reasoning appears only in inductive form. One could ask whether *deductive reasoning* is assumed to be covered by this or whether it is a separate factor too unimportant to be included in the battery. The precise significance of a memory test which depends upon association of a design with a number could also be questioned. There is in the design a spatial element which could well intrude to an unknown degree and numbers are so general in their significance that exchange may be difficult. This is one of the tests not included in the 1975 revised E.T.S. kit.

CAB 3–4

Meaningful memory depends upon recalling noun-adjective pairs. The *spelling* test simply requires the detection of errors. *Auditory ability* requires the recognition of tonal differences in pairs of tape-presented notes and pairs of musical phrases. The *aesthetic* test is again of conventional type and necessarily depends upon current standards of taste. Moreover it gives the impression of being rather restricted in range.

CAB–5

This booklet includes two further vocational tests, *Aiming* (A) and *Representational Drawing* (RD), whose rationale is quite clear but for which no empirical data on usefulness are available. In addition to these there are four of the closure factors referred to earlier as related to creativity and originality. They are *Closure flexibility* (Cf) ("hidden figures), *Spontaneous flexibility* (Fs) calling for regrouping of objects into new classifications,

Ideational flexibility (Fi) where the maximum number of suitable adjectives is required regardless of their quality, and *Originality* (O) where the "examinee" must find a way to combine two commonplace objects-not generally considered together to form a new and functional object. One would dearly like to know whether fecundity in quantity in this area is positively or negatively related to quality. It is tantalising to know that data are available to supply valuable information here.

It will be noted that all the tests in this last booklet are such that they cannot be machine scored, a restriction which will perhaps mitigate against their use. However, there are many workers interested in this area now and we may hope that more information will be forthcoming.

The battery is a praiseworthy attempt to meet the need for a practical multifactorial abilities measure. It is to be hoped that it will make some contribution to filling a very obvious gap in our testing repertoire and that it will encourage further research to give us a better understanding of this area. In the meantime perhaps the authors will provide us with more statistical information from the data at their disposal. Evidence of the predictive usefulness of these two tests would be welcome.

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Eysenck (Ed.) 1982. *A Model for Intelligence*. Berlin, West Germany.

Rudolf H. Moos and Bernice S. Moos (Eds.) *Family Environment Scale.*

Palo Alto, California: Consulting Psychologists Press, 1981.

Reviewed by John Gamby.

Description

90 items to be marked True or False. Purports to measure three dimensions of Environmental Press. The dimensions are *Relationship* (3 nine-item subscales titled Cohesion, Expressiveness, Conflict) *Personal Growth* (5 nine-item subscales titled Independence, Achievement Orientation, Intellectual Cultural Orientation, Active-Recreational Orientation, Moral-Religious Emphasis) and *System Maintenance* (2 nine-item subscales, Organisation, and Control).

Form R (the only Form seen) elicits members' perceptions of present maritial or nuclear family environments. Form I rewords Form R items to get percepts of ideal family environments. Form E elicits peoples' expectations of family settings not yet experienced, e.g., what the family may be like when the baby comes. There are short forms (S) of all these, in each case the first 40 items.

Data on all but Form R are rudimentary.

Manual (38 pp), $16.75; Test Booklets, pkg of 25, $14.20; Answer Sheets, pkg of 50, $11.10; Profile Sheets, pkg of 50, $11.10; Key, $4.10. Supplied through NZCER.

FES in Context

FES is one of the *Social Climate Scales* (SCS) devised by Moos and his co-workers. The scales are constructed on the premise that "environments have unique personalities" and that the climate of an environment can be measured as accurately as any individual's personality. You couldn't give fairer warning than that.

The SCS address environments as varied as families, classrooms, prisons or university hostels. Their conceptual framework originates in H. A. Murray's concept of Environmental Press. Respondents say how they perceive the setting under study. Subscale totals are averaged across persons to get a consensual score which gives information about what its like to be in that setting.

All the SCS are closely alike in structure and item content. Users may wonder about Moos' confidence that scales so similar will usefully sample responses to such a variety of behaviour settings. Family therapists may, for example, prefer that FES give more representation to system maintenance and system change. Yet rationale and structure are neat and persuasive.

Reviews of all the SCS may be found in a purgatorial labyrinth of Volume 1 of Buros' (1978) 8MMYB. Because they share rationale,

structure and content each review of each Scale sheds some light on any of the other Scales. There are about 12 reviews — from Richards' well-argued case against all of the SCS, through Dreyer (FES), Sines (FES), Lanyon (Community Oriented Programmes Environment Scale, or COPES) Eash (Classroom Environment Scale, or CES) and Pace (CES).

There is considerable goodwill toward the entire project. Reviewers all lament the paucity of validation data, though there is often confusion whether they mean *clinical* or *environmental* data. Getting either will be difficult. Richards has it right when he identifies the failure to deal with logical types which pervades Moos et al's scale construction.

In fact data can be treated (a) as an array of individual subjective reports or (b) a consensual response which itself exerts a directional influence on behaviour, including all examples of (a). We are dealing not with personality test data, but structured reports of personal views on a milieu. What validation might mean under these conditions needs to be debated by and with Moos.

Comment on the FES

Administration of the Scale is simple. The user reads all items to all family members present, and each fills in an answer form. This saves time and controls within reason for literacy problems. Items can also be clarified, but the language level would make use inadvisable for children under 12. Allow 10–15 minutes for administration, 5–10 minutes per protocol and 10 minute for averaging of scores. This is economical for a scale of its kind.

The averaging of family members' scores on subscales buries data e.g. a high individual score on a subscale combined with a low individual score may give the same average as 2 medial individual scores. Moos should warn users to keep all individual profiles and reconstruct his Manual accordingly.

In particular, beware the Scoring instructions on page 12 of the Manual. Averaged family scores may be converted using Appendix A as directed. *Individual scores cannot.* Enter this Appendix with individual scores and you will produce undue numbers of 'deviant' Standard Scores. This mess may have resulted from the scale-makers' confusion about logical types. Users wishing to convert individual scores should enquire of Moos what to do.

Nine-item subscales may seem tiny; on the other hand their internal consistency is satisfactory to good. At this point it can be assumed that using any Short Form would be a wast of time.

It seems likely that the FES will be used by some workers here. It is disarmingly face-valid, the construction is disciplined and professional as to content, and the rationale is persuasive.

There is need for a co-ordinated series of studies on how the Scale behaves in use. The FES could be given to clearly defined nuclear family samples, e.g. to couples in the various decades of married life, and to couples with one, two and three children; to single-parent families over time' to Polynesian samples and so on.

I would accept Moos and Moos' (1976) thesis that typologies of family environments can be identified within the framework of the FES, and that these can be related to certain types of social or clinical problem. An experienced therapist/user could also use individual and family profiles to structure family discussions.

This Scale is simple to administer and score. It provides useful leads for discussion and suggestive lines for investigation. When there is more information about how clearly specified groups respond to it, it could be useful to family practitioners.

As an assessment instrument it is very complex indeed.

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