

BOOK REVIEWS

The Psychology of Left and Right, by M. C. Corballis and I. L. Beale. New York: Lawrence Erlbaum Associates, 1976. pp. x + 227.

This book is concerned with examining the fundamental principles of left-rightedness, symmetry and asymmetry, in the living world. On the face of it, it might seem a little odd that an entire book should be devoted to the *psychology* of left and right (the authors' terms of reference necessarily limit the discussion of symmetry and laws of conservation). Is the problem worthy of such exhaustive treatment? Does this presage a treatise on the psychology of up and down? Yes and no. Reflect, for a moment, on two interesting conundrums: (a) Why do mirrors seem to reverse left and right but not up and down or back and front? (b) Would we be able to tell left from right if we were perfectly bilaterally symmetrical? Corballis and Beale set the scene of their book by posing a number of such intriguing questions and hypotheses. They lose no time demonstrating that the implications of such questions go well beyond the point of anecdote and belly-scratching *schreibtisch-experiment*. Before commenting on the substance of their book, however, I have an agreeable duty to say a few words about the authors.

As most readers of this journal will be aware, Mike Corballis and Ivan Beale are New Zealand graduates. (I was going to say "New Zealanders" but I understand M.C. has taken out Canadian citizenship. Before, during, or after the last Olympics, I wonder?) Both are acknowledged first-class researchers and have had published a substantial amount of material on the left-right question. This material has encompassed animal and human research and trenchant overviews such as that which appeared in a recent issue of *Scientific American*. Knowing the authors and being moderately familiar with their work, I guess I wasn't too surprised at the quality of this book—at the unlaboured erudition (each chapter is prefaced by an appropriate little quote, from Saint-Exupéry, Lewis Carroll, and others) and at their generally clear communicative style. I'm not being overly patronising. It's unfortunately all too rare to find a scientist (two in this case!) who combines a capacity for decent research, an ability to analyze and integrate the ideas of others, and a flair for writing.

The book is organized into three non-discrete sections. Chapters 1-7 deal with the psychological implications of symmetry and asymmetry and dwell at some length on the experimental evidence for, and theoretical notions underlying, left-right confusion (or discrimination, if you prefer). Chapters 8 and 9 carefully probe the quite complicated issues of the evolution and genetic determination of symmetry and asymmetry. These two chapters make rather heavy going; the research

discussed is admittedly complex "the anterioposterior axis . . . added to the dorsoventral one to define a midsagittal plane . . . the environmental influences then [being] indifferent only with respect to left and right" (p. 91). I tried to put this in simpler language but have to admit to a singular lack of success! Chapters 10-12 deal systematically but selectively with a number of more general problems; the relationship between cerebral dominance, handedness, and reading disabilities, mirror writing, and the development of the left-right sense.

The book is not a theoretical polemic and the authors have no particular axe to grind. A number of hypotheses are advanced which permeate the entire exercise. The most important of these may be briefly noted. First, the question of distinguishing left from right—the problems of distinguishing left from right as opposed to labelling "left" and "right." Corballis and Beale note that

Just as the ability to copy script does not prove that one can read, so the ability to give mirror-image responses to mirror-image stimuli does not prove that one can tell left from right [p. 14].

And again:

. . . confusion about "left" and "right" need not imply confusion about left and right! One should exercise care in drawing conclusions from experiments or tests in which the subject is required to use the labels "left" and "right", particularly with reference to other objects or bodies. This is most strikingly illustrated [in] Gerstmann's and Turner's syndromes; in both cases there is evidence that an apparent left-right confusion can be attributed in fact to a deficit to tell left from right *per se*. In general, it is probably safest to avoid the labels "left" and "right" in testing the ability of human subjects to tell left from right [p. 20].

On this point, I was reminded of a recent television programme in which a young lady met an untimely death skiing over a precipice. It seems that she had been told to stick to the right side of poles marking-out a particularly treacherous ski slope. Unfortunately she interpreted "the right side" to mean the right side; it turned out the right side actually led over a precipice.

A second critical premise is that a perfectly bilaterally symmetrical organism would be unable to tell left from right. The fact the brain appears to have a fair degree of structural symmetry has, of course, been used to *explain* left-right confusion. In the '30s Orton argued that a stimulus correctly recorded in the dominant hemisphere was recorded in mirror-image form in the non-dominant hemisphere, and

that if the activity of the non-dominant hemisphere was not suppressed, left-right confusion would result and be manifested in a variety of reading disorders. Corballis and Beale effectively dispose of this view (often cited today with a measure of uncritical relish) on logical and neurological grounds, but nevertheless maintain that memory engrams, rather than stimulus traces, *might* be mirror-imaged in the two hemispheres. The problem of interhemispheric mirror-image reversal remains unresolved. The authors do, however, argue that left-right confusion is related to hemispheric symmetry:

It is possible that the left-right sense depends on the development of cerebral lateralization. The most powerful advocate of this view was undoubtedly Samuel T. Orton . . . Although we were critical of Orton's account of the mechanisms producing mirror-image reversal, it is nonetheless a plausible notion that cerebral lateralization could underlie the ability to tell left from right, if only because it provides the necessary structural asymmetry [p. 156].

A third major hypothesis concerns the development of the left-right sense itself. Much of the authors' thinking on the psychology of left and right is encapsulated in the following paragrah:

Although left-right equivalence may be reinforced by experience, we suspect that it is more fundamentally a product of evolution in a world where parity is largely conserved. Our built-in disposition to treat mirror-image patterns as equivalent helps us understand that the same faces may appear in opposite profiles or that a two-dimensional pattern appears reversed if viewed from the other side. However, for humans in particular, the problem is to learn that in certain specific situations, largely created by man, left and right are not equivalent. We have argued that children learn this in relation to their own developing laterality, including handedness and cerebral lateralization. Functional asymmetries appear first in response systems and provide strategies or mnemonics enabling the child to discriminate mirror-image stimuli. Subsequently, asymmetries become internalized and invade perceptual as well as response systems so that we may speak of a "left-right sense." [p. 159].

The last chapter (13) has been added as a sort of buttress for the argument that asymmetry is part and parcel of the nature of things. Conservation of parity implies, of course, that for fundamental physical interactions, it is impossible to distinguish left from right. Parity is, however, not always conserved. Corballis and Beale ask in conclusion:

If the evolution of asymmetry can be understood in terms of the conservation of parity in the strong interactions and observable laws of nature, is there a common thread underlying the emergence of consistent asymmetry of nuclear decay, the asymmetry of living molecules, and the left-right gradient which seems to underlie the morphological asymmetries of animals and men, including cerebral asymmetries in the human brain? We have no idea [p. 197].

Well, I suspect that by the time the reader gets to this question he will be thinking there is.

Cosmetically, the book is very nice. A substantial contents index is included along with an author index (the omission of this latter index has virtually ruined many otherwise excellent texts). Chapters are well-laid out, each with a brief preview of the material covered and a concluding overview. A number of droll anecdotes are sprinkled throughout the pages. Were you aware that 15.8% of Rembrandt's self-portraits show the left profile, while 78.8% of female nonkin portraits by the same artist show the left profile? (You'll have to read the book to clarify this riddle.) Or again, that "left-right confusion may be symptomatic of a more profound confusion between anal and genital impulses"? [p. 178]. Or should it be, as the authors favour, a matter of back-front confusion? The book will serve as an excellent supplementary text for courses in developmental psychology, cognitive psychology, animal behaviour, and neuropsychology. There is little competition. Dimond's *The Double Brain* (1972) has a much more restricted coverage and has neither the lucidity nor the cohesiveness of the present book. Gazzaniga's *The Bisected Brain* (1970) is likewise restricted to a much narrower domain of behavioural phenomena.

(At this point I feel my negative id crying out for succour. Is there nothing in the book you can find to bitch about? Yes, dear submerged one there is. On page 109, ten lines from the bottom, "p. 77" should read "p. 277.")

One final observation. There has been a hell of a lot of rubbish about laterality and cerebral dominance published during the past ten years—and it's still coming out. Researching left-right differences has become an end in itself (the same thing happened with P-A learning) and it seems that if you own a tachistoscope there is just no excuse for perishing. Too often, there has been too little theoretical justification for studies of left-right differences. For me, the biggest virtue of Corballis and Beale's book is that it sets this research squarely in the context of more fundamental and general psychological questions.

Clearly, a book of outstanding merit and one which will do much for the international reputation of its authors. Dare I hope that a few kudos will rub-off onto New Zealand psychology?

M. J. White.

In the assessment of an Handbook, for what should we look? To find the answer to this question, I consulted my COD (Concise Oxford Dictionary) and found that it is a short treatise (a literary composition dealing more or less systematically with a definite subject) or a manual (a small book for handy use, or a book of the forms to be used by priests in the administration of the Sacraments), or possibly a guide book or a book of rudiments. All of which aided my task no end, for what was there to do but to count the systematizations (more or less), measure the size and weight of the work, determine whether all the Sacraments could be properly administered and, finally, to count the rudiments. However, notwithstanding the precision of this approach, the total number of Sacraments, rudiments, *etcetera* will tell the reader little when he knows not how many of these essentials may be obtained elsewhere. Thus, it is more proper to compare the frequencies of these occurrences in a relativistic manner with an economic substitute, let us say 6 annual subscriptions to the *Journal of the Experimental Analysis of Behavior*.

But, enough of this William Cobbett style. The new Handbook is a vast piece of work, nearly 700 large pages, with all the traditional, and perhaps all the new, areas of operant covered. Compared with the old Honig, there is more material of a physiological nature (5 or 6 chapters), a little more stimulus control and conditioned reinforcement, and some interesting work on language. The business of schedule-induced behaviour and of behaviour maintained by punishment features; Premack is given his due and some of the more recent work by Timberlake and Allison is mentioned; autoshaping and elicitation of contrast crop up all over the place; and there is a useful chapter on the by-products of aversive control. In fact, the 22 chapters do indeed contain all the Sacraments, both ancient and modern. The amount of Psychology now covered by operant must surely impress (or possibly, peeve) the reader from another area, if he should read it. Actually, I doubt me that he will, for this is far from an introductory text. It is for this reason, apart from its amazing cost, that the Handbook will be used only by the upper echelons of studentdom—say Stage 3 and beyond. Perhaps I have changed, or the students have changed, or the book really is pitched at a different level from the old Honig, which I used to use at Stage 2 before it got tediously out-of-date.

There are other reasons too why I feel diffident in using it as a text. I don't know what the authors' instructions were, but the chapters are often idiosyncratic to the author and, moreover, instead of reflecting the spirit of the times reflect more the (fleeting) spirit of the moment. Let me illustrate the idiosyncraticity. de Villiers' chapter on concurrent schedules consists very largely of a last-ditch defence of Herrnstein's

theory, and in places it reads very like parts of the Hull-Tolman controversy. de Villiers states, with no empirical evidence that I can ascertain, that those who fail to get strict matching in concurrent schedules have had some bug in their experiments—like odd previous training, too short a COD (changeover delay) or an unfortunate sequence of experimental conditions. It is sad for de Villiers' arguments that the same bone may be pointed at just about all the experiments and that these 'methodological problems' do not, by their presence or absence, allow us to differentiate the experiments which get matching from those which do not. But it is a fine armoury for eliminating those experiments which do not fit in. In this chapter, most of the facts are there but I would have looked for: rather more up-to-date material on the generalized matching law (some authors have 1976 references); less material largely duplicated from the de Villiers and Herrnstein Psychological Bulletin article; more consideration of alternative approaches; and above all, a correct spelling of Trevett. Fantino's chapter on conditioned reinforcement is pretty idiosyncratic too, but he is often defending his own model of concurrent chain choice. I thought I had shown that to be wrong. Oh well, try again.

As to the spirit of the moment, the (then) new and novel idea that contrast was to be explained by elicited responses comes in for a heavy explanatory burden. By the way, the idea is hardly novel. Remember Grant's work on eyeblink conditioning around 1947? I am badly biased against this (and similar) explanations, and the Handbook was obviously written in the first flush of enthusiasm for this explanation. Many of the chapters will stand as monuments to the idea. Yes, a work like this just has to be out-of-date—the labour of getting it together must have been immense—but I would have preferred that it was out-of-date in that it dealt comprehensively with the then-known data, with a cafeteria of theories thrown in as condiments. Theories, which constitute a large part of this book, age badly. Data don't. On these grounds, JEAB scores, and so does the original Honig.

But what about the relation of data to facts? I specified data above because I am getting to be very worried about facts. Let me discriminate these for you. Facts are what people *say* about their, and others', data. Facts often, but not always, originate from a journal article, are sanctified in other journal articles, and finally deified in Handbooks. Which of this list is a fact?—the conditioned emotional response, conditioned reinforcement, elicited responses in contrast (with altered topography), autoshaping, contrast itself, response matching and matching to immediacy of reinforcement. I'll tell you the answer: probably none of them, at least in their usual meanings. A good argument could be made that none of these can consistently be shown to occur. Three out of four animals show some, four out of five show others. By increasing the N we could probably show that we were unlikely to be wrong, but this is not blooming good enough. I will go one step further

and say that using these 'facts' as explanations of other data is downright immoral. I thought the Sacraments warned us against this. I reviewed a paper recently which compared some data with a statement made about some data obtained in the 1960s. They (present data, past statement) were different, and the suggested reconciliation was accomplished with difficulty. Had the present researchers looked closely at the past *data* they would have found them identical. So are straw men woven. The past statement, by the way, was a 'fact' deified in the Handbook—animals match their responses to relative immediacies of reinforcement. They don't. Similar things happened with the last Honig; for example, one needs a COD (changeover delay, again) to get matching. Not true. So you have the choice: rely on data, rely on facts or rely on Handbooks. I leave it to you.

Perhaps I have been over critical. Despite getting very annoyed in places with this book, I have enjoyed reading it. It will indeed be useful to the teacher in the area, who will be able to use each chapter as a base on which to build his own interpretations. The publishers will be sad that few of us will actually buy the book (which I believe is \$40 in the U.S., a much more possible price), but I feel that our libraries need multiple copies, and I think you will agree with me when you see it.

As a final note, I see that two companion volumes are to be published. They are the Handbook of Applied Operant Behavior, edited by Leitenberg (Prentice-Hall) and Social and Instrumental Processes, edited by Brigham and Catania (Irvington).

M. C. Davison.

Helping Troubled Children, by Michael Rutter, New York: Plenum Press, 1976, 376 pp.

This book provides an overview of the most commonly occurring psychiatric disorders of middle childhood. The author's stated aim is to "provide an account of the thinking and principles underlying the understanding and treatment of these common problems." This aim is largely achieved.

Rutter discusses, in turn, diagnostic principles, developmental considerations, environmental correlates, commonly occurring types of disorders, and treatment methods. Theories and procedures are not only outlined but also evaluated. Frequently the exposition is built around one or more illustrative case histories.

Diagnosis is approached largely from a functional point of view, childhood disorders being grouped under such headings as phobias, depressive states, obsessive behaviours, conduct disorders, underachievement (learning deficits), enuresis and encopresis. The chapters on child development cover patterns of development from birth to

adolescence, the effects of chronic illness, and sex, temperament, and ability differences. The chapters on environmental correlates review studies of the relationships between child psychiatric disorders and such variables as child rearing and disciplinary practices, number of parents, separation from parents, parental disorder, social class, school, and peer group factors.

The final chapter outlines treatment procedures. Equal emphasis is given to psychotherapeutic approaches, behavioural approaches, medication, and social work. This is not, however, a 'how to do it' book. In both the final chapter and the illustrative case histories, treatment procedures are sketched in only broad outline, not in prescriptive detail. The author has attempted to write, in ordinary non-technical language, a book for the 'general reader and the non-psychiatric professional'. This makes for a book which reads easily but which often glosses over rather important distinctions. For example, reinforcing and aversive consequences become, in the translation to ordinary language, 'encouragements and discouragements.'

For the paraprofessional, the student, and the educated parent who is seeking a reasonably comprehensive and non-technical survey of child psychiatric theory and practice, this book can be recommended. For those directly involved in day-to-day work with disturbed children the book will offer some insights but is rather too theoretical and lacking in prescriptive direction to be of great interest.

John Church.

Inner Speech and Thought, by A. N. Sokolov, Plenum Publishing Corporation, 1975, 283 pp. \$8.34.

This text narrows the gap between early philosophical speculations on the relationship between language and thought that reached their peak in the writings of Ogden and Richards (1923) and Vygotskii (1934), and more recent experimental efforts that have been made in the attempt to throw light on this age-old problem. The break in the continuity of research in this intriguing area of investigation was of course due to the behaviourist revolution which denigrated thinking to no more than a motor-speech habit. Some recent empirical attempts have been made in the West to revive the problems within the framework that is loosely termed cognitive psychology, however this revival may have been premature—the relationship between language and thought still represents an almost inaccessible region for investigation by objective methods.

The stated aim of this book is to investigate the role played by motor speech stimuli in cerebral activity. It provides abundant empirical support for the existence of the "inner speech" mechanism postulated by Vygotskii and it shows that inner speech can be manipulated independently of ongoing overt vocal activity. In an ingenious

series of experiments, Sokolov examines the effects of articulatory and auditory interference upon linguistic tasks by such methods as having the Subjects initiate a stanza from Pushkin's "The snowstorm covers the sky with darkness . . ." while simultaneously translating abstract and gramatically complex sentences from Hardy's "Tess of D'Urber-villes". Other experiments examine the relationship between inner speech and concrete thinking and the role of verbalisation in problem solving. Wide use is made of speech myograms to demonstrate the existence of covert vocal activity during problem solving tasks.

Earlier sections of the book provide an excellent account of the background to this area of research by drawing upon material from such diverse theorists as Plato and Einstein. Sokolov then builds empirically upon the writings of Vygaskii and Blonksii and their colleagues to provide an impressive body of data to support the notion of an inner speech process in thought. The author freely admits that the problem of the precise relationship between language and thought remains as obscure as ever and one is left with the impression that while the inner speech process has been clearly identified in his studies, its actual role in the thought process remains undetermined.

In general, Sokolov's approach is stimulating and novel, the results of his research are carefully integrated with contemporary theory and new directions for future research in this important area are opened up by the reported studies.

M. B. Simmonds.

Exploring Sex Differences, Edited by Barbara Lloyd and John Archer, Academic Press, London, 1976.

Given the current upsurge of research interest in gender differences, an up-to-date scholarly review of the literature on sex differences would be valuable. This book attempts to up-date the information available in Maccoby and Jacklin's book, to provide detailed reviews of a wide number of areas, and assess the current status of the biological/social determinants controversy. However, it is less than adequate if providing a comprehensive and sound scholarly assessment of the work on sex roles and gender differences.

The first chapter by Barbara Lloyd provides a reasonable and well balanced introduction which reviews some of the important studies in the field. The next four chapters all discuss in some manner the question of biological versus cultural determinism of sex differences. Ullian (Chapter 2) reports a study of the development of concepts of gender appropriate behaviour in children, for which she makes some rather astonishing claims. Strathern, on the other hand, suggests that gender concepts lead to social classification and differentiation which tends to support the original constructs. Rosenblatt and Cunningham argue

the case for a decision on the basis of cross cultural evidence, ignoring that while a lack of consistency across cultures can disprove a universal theory, consistency across cultures is not an adequate proof. Kypnis discusses the problem of why women achieve less when there is no evidence of intellectual inferiority.

In chapter six McGuiness argues that sex differences in adults stem from early perceptual and motor differences in children, although the environment may tend to equalize the differences. Such a thesis is novel and challenging, yet the evidence seems remarkably unconvincing.

Chapters seven and eight deal with the effects of the male and female hormones on behaviour. These reviews are valuable in collecting and sifting the recent evidence. Both authors conclude that no firm conclusions can be drawn as the interaction between learning and hormones is inadequately understood. Messent points out that the effects of female hormones on human behaviour is small and seems even slighter than that of male hormones.

In Chapter nine, May summarizes the extensive differences in psychopathology fairly adequately, but concludes that at present we do not understand them.

The final article by Archer is an admirably cautious review suggesting that it is premature to assume that biological explanation can account for gender differences in human behaviour and that the magnitude of gender differences has often been overestimated. He suggests that in most areas of human behaviour gender differences are of much less importance than other sources of variability.

The book is on the whole disappointing in both the originality of the thinking and the power of the review articles. I found the most valuable reviews were those on the effects of hormones on behaviour by Rogers and Messent. The opening and concluding chapters by Lloyd and Archer were by far the best balanced and integrated. The Archer article in particular is a well presented and balanced summary of the current status of biological explanations which may be very useful as a general reference.

Gabrielle Maxwell.

Hypnosis: Trance as a Coping Machine, by Fred H. Frankel. Plenum Medical Book Company, New York, 1976, 172 pp.

While up-to-date publications on hypnosis based on sound research seem to abound overseas few are found on the New Zealand market. The Psychological Hypnosis Division of the American Psychological Association apparently boasts a larger membership than the entire N.Z.P.S. and hypnosis was formally accepted as a treatment modality by the British and American Medical Associations over 25 years ago.

Despite such a level of acceptance overseas many prominent New Zealand psychologists and psychiatrists are unfamiliar with hypnosis. When the subject is discussed mutterings of its supposed ineffectiveness may be heard while in the same breath fears of the consequences of its use are expressed. To conceptualize a procedure that is both ineffective yet has dangerous consequences tends to stretch the imagination. The first chapters of Dr Frankel's book are particularly helpful in this respect. Landmarks in the history of hypnosis are critically reviewed including past and current applications as well as limitations and potentialities. These sections are written in clear and concise language. Review remarks and conclusions are thoroughly supported by references to past and recent research investigations. Well controlled experimental studies, supporting hypnosis, by Ernest and Josephine Hilgard are emphasised.

Studies demonstrating the superiority of hypnosis to elaborate placebo procedures are reviewed. Characteristics of hypnotic responsivity are also discussed in terms of stability of hypnotizability, distribution of hypnotizability in the population, correlates of age and sex, hypnotizability in relation to cognitive and personality traits, and correlation with hypnosis-like experiences outside of hypnosis.

The balance of the volume, chapters 5-14, is primarily concerned with clinical behaviour and hypnotizability and the treatment of phobic behaviour. Much of this latter material is presented in case study form. The case of Martha and Harriet is especially interesting. Martha was referred for psychiatric assistance in evaluating her physical symptoms and help in understanding an angry voice, (Harriet), that spoke out of her mouth periodically offending those around her. Despite the long prior history marked improvement in symptoms and reconciliation of the differences between Martha and Harriet were achieved in seven hypnosis sessions. It was refreshing to note the egalitarian reference to clinical psychologists and psychiatrists in the case history review. In this respect it was clearly a U.S. publication. Such recognitions of the diagnostic expertise of the clinical psychologist, in New Zealand medical publications and courtrooms, are conspicuous only by their absence.

The majority of the case study material, although interesting, is of little value to psychotherapists interested in using hypnosis in their day-to-day practice. Adequate detail as to the specific hypnotic interventions employed is simply not given. The psychoanalytic orientation of the author adds to the confusion in cases dealing with phobic behaviour where behavioural techniques were combined in treatment. The author appears to be familiar only with Wolpe's original (1958) approach while oblivious to Wolpe's later work (1960, 1962, 1966, 1968, 1973) and nearly 5,000 relevant research titles in the literature appearing during the past two decades. The dynamic interpretations,

the efficacy of which has never been demonstrated, detract from the value of the book by obfuscation and unnecessary protraction. While the major operative component of therapy in the phobia cases is likely to be the result of reciprocal inhibition, (Wolpe, 1960, 1962) the author's conclusion that desensitization can be accelerated by hypnosis is supported by the literature, (Barabasz, 1977).

Arreed F. Barabasz.

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TITLES OF PAPERS PRESENTED AT THE ANNUAL
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