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Book reviews



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## Book reviews

### Improving heuristic decision making by triers of fact: Anchored narratives

W.A. Wagenaar, P.J. van Koppen and H.F.M. Crombag, *Anchored Narratives: The Psychology of Criminal Evidence*. Harvester Wheatsheaf/St. Martin's Press, Hemel Hempstead, New York, 1993.

The authors have produced a veritable tour de force in their analysis of decision making by triers of fact when confronted with what are referred to as 'critical cases.' These particular criminal cases are the approximately 12 percent of cases that are dubious in the sense that they contain legal and/or logical problems. The remaining 88 percent are deemed to have resulted in safe convictions in the sense that there are no legal or logical problems: Suspects are caught in the act, or clear and verifiable confessions are obtained, or the physical evidence is overwhelming, and so on. Although the authors are careful to point out that these figures are based on a random sample of 233 Dutch criminal cases, I suspect that the figures are not drastically different for most other countries, regardless of whether they possess common law or civil law judicial systems.

Irrespective of judicial system, the body of law relevant to proof generation in criminal cases has had many centuries to develop into what legal scholars, triers of fact, and the lay public alike would regard as a system of decision making that functions relatively smoothly. As Wagenaar, Van Koppen, and Crombag point out, their book is a psychological study of the process of proof generation. One might well ask what they had hoped to accomplish by psychological analysis of a process that, in popular mythology at least, involves establishing the facts of a case (objectively and with reasonable accuracy) and then by a process of logical reasoning inferring the conclusions.

It turns out that the authors have accomplished plenty. For one thing, they have shown quite convincingly that logical inference theories of judicial reasoning (e.g., Wigmore, 1937) are deficient. First, they show that the process of proof generation is not a bottom-up process, as logical inference is supposed to be, but rather is a top-down hypothesis testing procedure. Second, intensive study of their sample of 35 Dutch criminal cases in which there was a dubious conviction of the defendant reveals that

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there is no empirical support for the notion that Dutch jurists, at least, follow the sort of logic supposed.

Wagenaar, Van Koppen, and Crombag proceed to argue that the Bayesian formulation of the hypothesis testing model is a more realistic model of trier of fact reasoning and decision making. Here a trier of fact starts with very low prior odds of the defendant's guilt and then estimating the diagnostic value of every piece of evidence as it is encountered, adjusts his/her prior odds accordingly. When all the evidence has been evaluated, the trier of fact compares the now stable odds of the defendant's guilt with a criterion of what constitutes 'guilt beyond a reasonable doubt.' Only if the odds exceed the criterion does the trier of fact convict (or vote to convict) the defendant. Despite the Bayesian model being a quantitative model of what is clearly a top-down process, Wagenaar and colleagues show that it is not terribly satisfactory, either. Any successful theory of the reasoning process by which triers of fact generate proof must account for the anomalous cases, as well. Unfortunately, Bayesian hypothesis testing would not have led to the conclusions reached by Dutch jurists in any of their sample of 35 dubious cases. Of course, others have also obtained empirical results not compatible with a Bayesian account (e.g., Pennington and Hastie, 1986). Pennington and Hastie found a major effect for the order in which evidence was presented, a finding not predicted at all by Bayesian theory. They found that both the prosecution and the defense were more readily believed when the same evidence was presented in the form of a good story, as opposed to being presented in a random order.

In fact, Wagenaar, Van Koppen, and Crombag's own theory of trier of fact decision making is built upon the notion that criminal proceedings can be viewed as story telling by the prosecution and defense (Bennett and Feldman, 1981). The notion is that the trier of fact must assess the plausibility of stories woven by the two parties. Evidence takes on meaning in the context of a story (cf. Rumelhart, 1975). Adopting Bennett and Feldman's story grammar, Wagenaar and colleagues argue that good stories are those that have a readily identifiable central action and a context that provides a straightforward explanation of the actions of the actors. Of course, stories told by both parties in a criminal action should not only be good but at least one of them must be true. The goal of each party, then, is to try to establish firmer links between their narrative and the evidence than can the other party. The prosecution, for instance, must present evidence relevant to the issues of identity (whether the defendant is the person who committed the offense), *actus reus* (what the defendant did and whether it is a criminal offense), and *mens rea* (whether the defendant acted intentionally, recklessly, or negligently). Evidence relevant to each issue forms a sub-story within the narrative, a sub-story needing its own anchor in the form of further evidence, which becomes a sub-sub-story needing anchoring. The chain of evidence is safely anchored when it can be tied to a common-sense rule "the truth of which cannot be sensibly doubted" (p. 40). Of course, either party can attempt to show that anchors for one or more evidentiary chains in the narrative of the other party are not safe.

Wagenaar and colleagues spent most of the book exploring the boundaries of application of the concept of anchoring, explorations of the parameters of safe anchoring and how they might fit the various areas constituting the domain of criminal evidence: investigation and evidence selection, confessions, identification, witness testimony,

expert testimony. At least within the corpus of 35 problematic Dutch criminal cases they have analyzed in detail, the authors have demonstrated an alarming lack of critical reasoning on the part of the triers of fact. Time and again, the only theory of judicial decision making that can account for the anomalous decisions rendered is their theory of anchored narratives, even though the narrative apparently adopted by the judiciary is at times bizarre. Though those of us from common law systems might like to think that the adversarial nature of the proceedings and a jury of peers as triers of fact would not permit such deficiencies in critical reasoning, it is not at all certain that this would be the case. Clearly, without reform, it will prove difficult to investigate jury decision making, inasmuch as juries are not required to justify their decisions. The procedure of plea bargaining, especially as practiced in the United States, would not likely result in an improvement over the jury trial, either, in terms of promoting properly anchored narratives by the prosecution.

In addition to arguing the case for their theory of anchored narratives, Wagenaar and colleagues have made a major contribution by developing a set of ten rules that would require a thorough testing of the quality of anchoring for the prosecution's narrative, a thorough testing by the trier of fact. This would require more work on the part of police and prosecution in dubious cases, but the quality of justice requires it. The authors do worry that Rule 5, requiring that triers of fact specify the narrative on which their decision is based and the accompanying anchoring, would be highly impractical, especially in common law countries where trial by jury is practiced. Juries may not currently be required to give reasons for their decisions, but I do not believe that it would be all that difficult a reform to implement the requirement.

Juries currently must either convict or acquit a defendant on each count of the indictment. The prosecution and defense each must present a closing argument to the jury. Presumably each party presents the narrative that they believe accounts for the facts of the case. It would not be requiring much additional to expect each party to present a well-shaped narrative, with essential components anchored. Both prosecution and defense could be required to supply the jury with a written version of their anchored narratives. It would not be requiring all that much to expect juries when making decisions to specify which narrative they believe, whether or not they believe defense attempts to falsify one or more aspects of the indictment's narrative have succeeded, and so on. Perhaps it is the right time to exert some influence on courts and legislatures, in the United States, at least, inasmuch as national field experiments have been conducted testing the efficacy of innovations such as juror note taking and juror questioning of witnesses during trials (Heuer and Penrod, 1994).

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R.M. Joshi and C.K. Leong (eds.), *Reading Disabilities: Diagnosis and Component Processes*. Kluwer Academic Publishers, Dordrecht, 1993. Pp. 344. ISBN 0 7923 2302 5. US \$ 134.00

In the fall of 1991 a NATO Advanced Study Institute conference was held in Toulouse, France on: Differential diagnosis and treatments of reading and writing disorders. The volume *Reading Disabilities: Diagnosis and Component Processes* is part one of a two-volume book based on its proceedings. The companion volume will also be published by Kluwer Academic Publishers and carries the title, *Developmental and Acquired Dyslexia: Neuropsychological and Neurolinguistic Perspectives*.

Twelve of the 18 contributions are based on English research (USA, Canada, UK, Australia, and New Zealand), whereas the remaining chapters originated in six different non-English speaking European countries (Germany, Belgium, Sweden, Denmark, Greece, and The Netherlands). The book consists of three parts.

The first part deals with the problem of 'differential diagnosis of reading disabilities'. Stanovich opens the discussion on the problem that has surrounded us ever since the World Federation of Neurology defined dyslexia as, "A disorder manifested by difficulty in learning to read despite conventional instruction, adequate intelligence, and socio-cultural opportunity". This definition emphasises the difference between children with an unexpected reading disability (not predicted by the child's general intelligence), the so-called real dyslexics, and those that do not show a discrepancy between their reading performance and their IQ. This latter group is often referred to as 'garden-variety' poor readers. Stanovich shows in a clear argument that this does not lead to what we are actually trying to do, namely, predicting how much growth in reading comprehension ability would be expected if the decoding deficit were to be totally remediated. The underlying assumption here, for which there is abundant evidence, is that the core of the reading problem resides in an inefficient word identification process.

His proposal is to use listening comprehension as an alternative measure for discriminating between dyslexics and garden-variety poor readers. Given that we want to distinguish between the two types of reading disabilities, I find his argument insightful. This, however, is the issue. Why do we want to distinguish between these forms of reading disabilities if the real problem exists in word identification. This is exactly the argument put forward by Siegel. Her passionate call against the use of IQ test to

distinguish between subgroups of reading disabled children holds four main arguments. The first is theoretical, the debate on what IQ tests are supposed to measure is still on, secondly, IQ does not contribute independent variance to word reading, thirdly, no statistical differences occur among the two types of reading disabled children at each IQ level (Siegel, 1988), and finally a practical argument, there is no real evidence that either of the two groups gain more from remedial teaching.

In the second part of the book, 'Access to language-related component processes', Tunmer and Hoover discuss three variance models of language-related factors in reading disabilities. The shared premise of each of these models is the so-called 'Simple view of reading', which states that differences in reading comprehension are a function of word recognition (i.e., decoding) and listening comprehension (Hoover and Gough, 1990). Phonological recoding and listening comprehension and the interaction between the two account for 75% to 90% of the variance in reading comprehension in children from Grade 1 to Grade 4.

The first variance model is called the 'environmental model' of which Ehri is the leading proponent. She assumes that reading difficulties are the result of experiential factors, that is, inadequate exposure to print-related activities prior to schooling and inadequate instruction. Phonological deficiencies are not a pre-existing problem, but are the result of these extrinsic factors. The model that is the antithesis of the environmental model is the 'phonological "g" model'. Liberman and Stanovich are, amongst a larger number, proponents of this model, in which intrinsic factors, namely, constitutional differences in the phonological processing component, are the main determinant of reading problems. Not surprisingly, the solution to this dichotomy is the synthesis formulated in Tunmer and Hoover's 'cognitive development model' which proposes that both extrinsic and intrinsic factors contribute to the development of reading problems. There is not only abundant evidence indicating that phonological problems are primary in reading difficulties, but there also appears to be a reciprocal relationship between the already existing problems and inadequate instruction or treatment (Matthew effect, Stanovich, 1986). Although it is a clear chapter, it suffers from the same problem as most of the earlier chapters, old wine in new bottles.

This is not the case for the article by Morais, because he previews some new data, which will be published elsewhere. He makes a case for distinguishing between phonemic awareness and phonological awareness. Phonemic awareness is the set of *conscious* representations of the individual phonemes of a language (italics is mine), whereas phonological awareness is the more general ability of being aware of perceptual representations of speech (knowing which words rhyme or which of two words is the longer one). Morais claims not only that phonological awareness does not imply phonemic awareness, but also argues that phonemic awareness depends on receiving instruction in the alphabetic code (a similar view is expressed by Olson, 1993). He states that phonemic awareness and knowledge of the alphabet are acquired (see the seminal study by Morais et al., 1979), lost (indicated by four case studies of severe dyslexics) and regained again (shown by the re-education of a deep dyslexic patient) together.

Olofsson, on the other hand, argues, that it is possible to train phonemic awareness in children without explicit instruction in the alphabet, which is subsequently beneficial for later reading and spelling. Unfortunately, however, Olofsson uses the terms phonological

and phonemic awareness interchangeably, it therefore remains unclear whether he refers to the same kind of conscious knowledge as Morais was talking about.

Dodd, Russell and Oerlemans present data which suggest that children with a past history of phonological speech disorders, thus after the impairment is corrected, show poorer reading and spelling skills than a control group. Although the statistical proof is somewhat weak (only in 2 out of the 8 tasks a significant effect emerged), I think this an interesting finding. It not only corroborates previous research, but the issue also deserves further investigation, because it seems to indicate that the origin of reading difficulties (i.e., the phonological problem) can be traced back to a much earlier stage in life than school. This in turn, may be very useful information for treatment and/or prevention.

Hulme and Snowling show that the reading behaviour of a phonological dyslexic boy (JM) can also be explained in terms of a subsymbolic framework as suggested by Van Orden et al. (1990). JM, who had severe problems in learning to read, acquired a level for reading words that was almost indistinguishable from reading age controls, but remained severely impaired in reading pseudowords. According to standard 'Dual-route' theory we have to conclude that JM solely reads word via direct access, because his indirect or phonological (or assembly) route appears to be impaired. More in accordance with a non-information processing theory is that JM lacks phonological representations (actually an inappropriate term in this context) at the sufficient level of specification to allow the creation of mappings between phonology and orthography.

The main theme of the final part of the volume is 'Reading/spelling strategies', and only contains empirical studies. I will only discuss the most interesting findings. Elbro falsifies the hypothesis of distinct subtypes of dyslexia. Reading strategies in both normals and dyslexics appear to be unimodally distributed, suggesting that the assumption of two categories of dyslexics, namely letter-by-letter readers and whole-word readers is unjustified.

A rather surprising and promising result in the field of instruction is the finding of Uhry. Sounding out and playing games on the computer, which mainly involved spelling training, appear to have a beneficial effect on the reading of nonwords in dyslexics. Not only are transfer effects hard to get, but it is also rather difficult to have reading-disabled children read nonwords. Most dyslexics finally manage to achieve a sufficient word reading level, but remain at a loss when they have to read nonwords. We have to be cautious not to be too over optimistic, because the effects were established with 3 dyslexic subjects. Nevertheless, the result is very interesting and deserves further investigation.

Porpodas performed three experiments and observed effects in Greek-speaking children that were similar to those obtained with English-speaking subjects. More advanced Grade 2 children showed better employment of phonetic coding for linguistic information than less advanced readers. Porpodas did not conclude from his data that less advanced readers were actually using non-phonological information to perform the task, but suggested that the less advanced readers phonologic representation is less well formed or less stable than that of the more advanced readers. As he already mentioned himself, the issue is not new, but it is important to know whether phenomena established with the English orthography are also apparent in languages other than Latin-based alphabets. The Greek language is particularly interesting, because it is also an alphabeti-

cal writing system, and its orthography is like the English rather deep, but unlike the English language it is a Greek-based alphabet. Cross-linguistic research on word recognition is important, because the decision of theoretical issues should not be limited to the language under study.

In the final chapter of the book van den Bos and Spelberg discuss the results of an experiment with two groups of children: one group from a regular primary school and one from a special primary school. Their conclusion is that reading comprehension of children from special schools is predicted by the same factors as those of the normal population. Therefore, a distinction between school types does not seem relevant.

The general picture emerging from this volume on reading disabilities is that trying to distinguish between the real dyslexics and garden-variety poor readers, between poor readers from special schools and those from regular schools, and between whole-word and letter-by-letter readers is a pointless enterprise. Not only, because there is hardly any empirical evidence that supports subtyping, but also because most researchers seem to agree that the main problem in developmental dyslexia is a deficiency in the phonological component of the word identification process.

The issues discussed in this book are indeed today's current topics, but I cannot escape the impression that that is all there is to it. The theoretical problems raised are not new, and the results of the empirical studies cannot be interpreted properly, because in most cases their description is incomplete. All in all, I do not think we are getting enough scientific run for our money here.

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### Shanon on Cognition

Benny Shanon, *The Representational and the Presentational: An Essay on Cognition and the Study of Mind*. Hemel Hempstead, Harvester Wheatsheaf, 1993.

Historically, we can view what is now called cognitive science from three perspectives. The prehistory of the field includes thinkers such as Hobbes, Leibniz, and Turing who first proposed mechanistic models of thought. The modern intellectual origins of cognitive science occurred in the mid-1950s, when Allan Newell, Herbert Simon, Marvin Minsky, John McCarthy, Noam Chomsky, and George Miller laid the foundations for what has become the representational-computational approach to the study of mind. The organizational origins of cognitive science occurred in the mid-1970s, when Allan Collins, Donald Norman, Roger Schank and others established the Cognitive Science Society and the journal *Cognitive Science*.

Cognitive science has long since supplanted the behaviorist approach to the study of mind, but its current dominance of the intellectual scene is problematic in two respects. First, there is no single computational theory of mind, but rather a bevy of approaches, with different theorists emphasizing different kinds of representations (e.g. rules, schemas, analogies, images, distributed networks) and processing systems. Second, a variety of challenges have been made to the representational-computational approach, arguing for example that it has little to say about consciousness and gives a misleading view of other key aspects of thinking.

Shanon's book is a comprehensive compendium of complaints against what he calls the representational-computational view of mind (RCVM). The value of this essay depends on answers to three questions:

- Does it give an accurate description of the current status of RCVM in cognitive science?
- Does it describe serious problems for RCVM?
- Does it propose a plausible alternative to RCVM?

The answer to the second question is yes, but the overall strength of the case is limited because of the negative answers that must be given to the first and third questions.

In Chapter 16, 'Why representationalism?', Shanon states a set of eleven substantive and eight metatheoretical/methodological assumptions that he claims underlie RCVM. I doubt that even Jerry Fodor, whom Shanon oddly takes to be the chief representative of RCVM, would endorse most of these, and am sure that leading researchers in cognitive psychology and artificial intelligence would also have difficulty recognizing most of the assumptions as part of their approach to cognition. For example, one of the substantive assumptions is "the precedence of entities relative to relations" (p. 244), but relations are taken very seriously by many researchers on such topics as knowledge representation and analogy. In his metatheoretical list, Shanon states that "RCVM assumes that the modelling of cognition can be made without considering time" (p. 246), but there has been considerable work in artificial intelligence on how to represent time in planning systems. The weakest part of Shanon's characterization of RCVM is that he sees connectionism as an alternative to it rather than as a variant of it. By concentrating on Fodor's narrow, dogmatic view of representation and computation, Shanon ignores the

role of connectionists such as David Rumelhart and Geoffrey Hinton as adherents to the basic idea that thinking is a matter of computations on representations, even though the sorts of representations and computations they propose are quite different from those originally proposed by Newell and others.

The most valuable part of the book is Part I, where Shanon compiles a list of challenges to cognitive science that deserve to be taken seriously. He argues convincingly that various aspects of thought have been neglected by researchers who subscribe to the RCVM: the role of context, the importance of the body and interaction with the world, social aspects of knowledge, emotions, and temporality. But Shanon exaggerates the extent to which these problems have been dogmatically excluded from consideration by proponents of RCVM. Fodor may advocate 'methodological solipsism' and assume that the study of thought can ignore the world and the brain, but many cognitive scientists are currently pursuing such questions as the neural underpinnings of thought and the ways in which robots can interact with the world.

Ultimately, the acceptability of RCVM depends on whether a plausible alternative can be found for it. In the history of science, dominant theoretical frameworks are never rejected simply because they face anomalies that they cannot explain, but they can be forced to bow out when a more powerful explanatory framework becomes available. Drawing on such thinkers as Gibson, Maturana, and Vygotsky, Shanon presents a sketch of an alternative position, but it is neither clear nor convincing. He rejects the mechanistic idea that psychological explanation is to be concerned with covert, underlying structures and processes, and astonishingly suggests that "the subject matter of psychology more or less coincides with the domain of the conscious" (p. 340). Contrast this restriction with the demonstrable success that cognitive science has had in illuminating the nature of the thought processes involved in problem solving, explanation, learning, language use, and memory.

Rather than abandon RCVM for phenomenology, cognitive science needs to expand its explanatory resources to deal with some of the problems that Shanon and other critics have identified. Current work in cognitive science is rapidly expanding beyond the traditional representational/computational approach in two directions, the biological and the social. Brain scanning techniques have provided a new method for investigating the neural basis for thinking, and social aspects of knowledge are being taken more seriously by researchers working on natural and artificial distributed cognition. Just how RCVM is to be integrated with work on brains and societies remains a key theoretical question, and there is no guarantee that an emerging synthesis will be able to deal with the elements on Shanon's list of hard problems for cognitive science. Advocates as well as critics of cognitive science can benefit from reading Shanon's trenchant attack on RCVM, but will draw the conclusion, I hope, that it points to the need for new theoretical work rather than to relinquishing the historically most successful approach to the understanding of mind.

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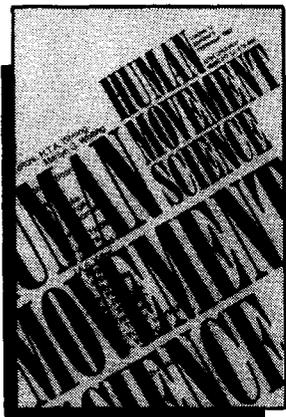
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