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Webs of Things in the Mind: A New Science of Evidence

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WEBS OF THINGS IN THE MIND: A NEW SCIENCE OF EVIDENCE

EVIDENCE AND INFERENCE FOR THE INTELLIGENCE ANALYST. By *David Schum.* Lanham, Maryland: University Press of America. 1987. Two volumes. Vol. 1, pp. xvi, 486. $38.75. Vol. 2, pp. xii, 359. $28.25.

*Peter Tillers**

I. INTRODUCTION

Contemporary scholarship in the law of evidence is awash with theorizing about evidence, inference, logic, probability, and proof, but it is not clear that either the friends or the critics of this new scholarship understand exactly what it is or where it is headed. The publication of *Evidence and Inference for the Intelligence Analyst*[1] provides a good occasion for a general review of David Schum's[2] contributions to the new scholarship. His studies do much to clarify the nature of major theoretical currents in legal scholarship in evidence. They also have broad implications for future research on proof processes in adjudication and in other legal contexts.

Interest in matters theoretical, philosophical, and logical is not new to evidence scholarship in America. The leading evidence scholars of the first half of this century had an overtly pragmatic bent, but they also had a strong theoretical and philosophical streak. These great synthesizers of the law of evidence — John Henry Wigmore, Edmund Morgan, John Maguire, and the like — devoted much attention to the logic of factual inference. These practical men were doing nothing less than examining the foundations of empirical knowledge.

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David Schum and I are friends and we are collaborators in an ongoing research project. These facts surely are part of the explanation for the friendly tone of this essay. However, I also believe that my admiration for Schum's scholarship is *merited.* Indeed, I expressed my admiration for his work long before he became my collaborator or my friend. *See, e.g.,* 1 & 1A WIGMORE ON EVIDENCE xiii, § 37.1 n.8 (P. Tillers rev. ed. 1983) [hereinafter WIGMORE ON EVIDENCE (Tillers)]. I should also note that Schum may not like or believe things I have said in this essay.

My thanks go to Judge Jack Weinstein and Professors R. Lea Brilmayer, David Carlson, Richard Lempert, Roger Park, and William Twining for their comments. However, they bear no responsibility for any flaws in the essay.

1. Hereinafter cited as EVIDENCE AND INFERENCE.

2. Professor of Information Technology and Systems Engineering, George Mason University.

These evidence scholars were philosophers of knowledge, but they were also men of action who had little interest in epistemological theories for their own sake. They wanted to put their theories to *work* and they expected that their theorizing about inference would advance reform of the law of evidence. They succeeded. These men, who dominated evidence scholarship, were also the architects of the modern law of evidence; they saw to it that their theory of inference became a set of working principles guiding the law of evidence. Following the example of James Bradley Thayer,[3] Wigmore, Morgan, Maguire, and many others argued that the principle of relevancy is a cornerstone of the law of evidence and that a variety of legal rules regulating the admissibility of evidence incorporate, or should be construed as incorporating, relevancy principles or relevancy-related principles. This thesis quickly became legal dogma[4] and, thus, a "philosophical" theory of inference became part of the fabric of the law of evidence.

The theoretical foundations of traditional evidence scholarship are remarkably homogenous. Although Wigmore and his colleagues had sharp disagreements about the nature of relevancy and inference, these differences of opinion, with only one notable exception,[5] concerned detail rather than substance. Under the rubric of "relevancy," the leaders in traditional evidence scholarship and almost all of their followers subscribed to a theory of inference impregnated with an epistemological perspective characteristic of nineteenth-century British empiricism. This theory of relevancy and inference, which I have described in detail elsewhere,[6] put great emphasis on the role of experience and on generalizations based on experience. The theory of relevancy was seen as the foundation and the central organizing principle of the modern and "rational" law of evidence.

In the late 1960s and early 1970s a new way of thinking about evidence and inference began to emerge in American law schools. This new style of theorizing, later labelled "the new evidence scholarship,"[7] did not readily mesh with more traditional theorizing about evidence and inference. Many of the early participants in this new wave of scholarship were Bayesians. They talked about the coherent integration of subjective probability estimates, conditional probability,

3. Thayer, *Presumptions and the Law of Evidence,* 3 HARV. L. REV. 141, 144-45 (1889); *see also* J. THAYER, A PRELIMINARY TREATISE ON EVIDENCE 265-66 (1898).

4. *See* 1 WIGMORE ON EVIDENCE (Tillers), *supra* note *, at §§ 9 & 10.

5. The exception was the theory of proof that Jerome Michael and Mortimer Adler developed. *See* Michael & Adler, *The Trial of An Issue of Fact: I & II,* 34 COLUM. L. REV. 1224, 1462 (1934); *see also* J. MICHAEL & M. ADLER, THE NATURE OF JUDICIAL PROOF (1931) (unpublished manuscript available in Harvard Law School library). Their theory is summarized in 1A WIGMORE ON EVIDENCE (Tillers), *supra* note *, at § 37.3.

6. 1A WIGMORE ON EVIDENCE (Tillers), *supra* note *, at §§ 37.1-37.5.

7. Lempert, *The New Evidence Scholarship: Analyzing the Process of Proof,* 66 B.U. L. REV. 439 (1986), *reprinted in* PROBABILITY AND INFERENCE IN THE LAW OF EVIDENCE: THE USES AND LIMITS OF BAYESIANISM 61 (P. Tillers & E. Green eds. 1988) [hereinafter BAYESIANISM].

prior probability, posterior probability, and various other matters, but they had little to say about the role of generalizations in inference. As the new evidence scholarship flowered, the theoretical homogeneity that characterized its early days waned. In addition to talk about Bayesianism, one now finds discussions of Baconianism, schema theory, fuzzy set and fuzzy probability theory, Shafer-Dempster belief functions, "holism," story models and theories of narrative, scenarios, statistical inference, and a great deal else besides.

Although the new scholarship is now quite diverse, it does share some characteristic features. For example, it focuses more on logic and less on law; it focuses more on proof and less on rules of admissibility; and it emphasizes rigor rather than rhetoric. Moreover, much of this scholarship grapples with fundamental problems of epistemology, apparently in the belief that they need to be reexamined. Also, a substantial part of the literature in this field employs "technical" analysis, especially mathematics and formal logic.

Nevertheless, the new scholarship is more heterogenous than homogenous. I have suggested elsewhere that this theoretical diversity is mainly a blessing.[8] It is, however, also a curse, for this diversity makes it very hard to answer two fundamental questions about the new scholarship: (1) What is it?, and (2) Where should it go?

David Schum's *Evidence and Inference* provides at least a partial answer to the first question; this book is an excellent primer on theories that figure prominently in the new scholarship. Moreover, Schum has made many important contributions of his own to the study of inference which have much to say about the directions that the new evidence scholarship should take in years to come.

Of course, neither Schum's analysis of the present state of research on inference nor his views about the appropriate agenda for future research guarantee that the centrifugal tendencies of the new evidence scholarship will not pull it apart. The primer Schum provides is a *critical* one; it is not an *apologia* for every feature of the new evidence scholarship. Hence, it is not likely that every new evidence scholar will agree with his evaluation of existing theories, or with his vision of the appropriate mission of the new evidence scholarship. Schum's view of the purpose of theorizing about inference is distinctive, even idiosyncratic. Nonetheless, his description of the nature and limits of various types of theoretical inquiry is a powerful one, and his work will likely define the agenda of an important part of the new evidence scholarship.

8. Tillers, *Introduction,* 66 B.U. L. REV. 381, 389-90 (1986) (from Symposium on Probability and Inference in the Law of Evidence).

II. BASIC LESSONS

A. *"Technical" Theory in Ordinary Language*

Schum's two-volume work, *Evidence and Inference,* restates and refines the results of more than seventy earlier works.[9] The explicitly mathematical and formal analyses are consigned to the second volume. The first volume, and large parts of the second volume as well, can be read and understood by people without any special acumen in logic, mathematics, or probability.

In the first of two basic tutorials, Schum offers an explanation of various formal theories of evidence, inference, probability, and proof. An impressive variety of theories is examined, including some with odd-sounding names such as the theory of belief-functions and subjective Bayesianism. Schum's survey also includes discussion of a variety of theoretical perspectives that have been unjustly neglected by avant-garde evidence scholars. For example, Schum has useful and interesting things to say about the implications of signal detection theory and psychophysics.

Evidence and Inference is the *only* satisfactory introduction for nonspecialists to a number of very important recent developments in theories of probability, uncertainty, and inference. For example, it is the first and only book to make the literature on Dempster-Shafer belief functions accessible to a general audience;[10] it also provides a marvelously intelligible and succinct explanation of L.J. Cohen's Baconian theory.[11] Schum's explanations of these complex theories are sometimes more lucid than those of the theorists themselves.

B. *The Role of Symbols in Formal Analysis*

It has been suggested that the new evidence scholarship may be the product of misguided enthusiasm for the application of scientific, mathematical, and logical methods to law. We know that enthusiasm for scientific method can be dangerous. In the early part of this century talk about law as a form of "social engineering" was not uncommon[12] and, as Professor Brilmayer has noted,[13] there was also once a movement for more widespread use of formal logic in law. Some of these earlier expectations about the benefits of social engineering and

9. *See infra* appendix, at 1256-58.

10. Vol. 1, pp. 111-16, 141-43, 212-13; vol. 2, pp. 109-23, 145-46, 227-39.

11. Vol. 1, pp. 107-11, 138-40; vol. 2, p. 109.

12. *See, e.g.,* Pound, *A Theory of Social Interests,* 15 PAPERS AND PROCEEDINGS OF THE AMERICAN SOCIOLOGICAL SOCIETY 16, 44 (1921) ("I venture to think of problems of eliminating friction and precluding waste in human enjoyment of the goods of existence and of the legal order as a system of social engineering whereby those ends are achieved.").

13. Brilmayer, *Second-Order Evidence and Bayesian Logic,* 66 B.U. L. REV. 673, 673 (1986), *reprinted in* BAYESIANISM, *supra* note 7, at 147.

symbolic logic in law were clearly unrealistic, and it is not a bad idea to be on guard against repetition of such mistakes.

A few legal scholars assert that the new evidence scholarship is in fact another cycle of scientism. These critics say that factual inference and proof are inherently intuitive, subjective, and fuzzy processes and that the new scholarship makes the mistake of trying to make these processes scientific, quantitatively exact, and objective. Mathematical precision and quantification of evidence and inference are said to be objectionable on epistemological grounds, normative and ethical grounds, or for social reasons.[14]

The second tutorial in *Evidence and Inference,* which examines the objectives of formal analysis in the study of proof, shows that the basic assumptions underlying this critique are wrong. People who are not professional logicians or mathematicians often accord formal analysis a degree of authority that it cannot possibly have. Legal scholars who see the new evidence scholarship as a new form of scientism effectively (if unwittingly) capitalize on the weaknesses of exaggerated claims for formal analysis. The appropriate remedy for these exaggerated claims, however, is not to abandon formal analysis in legal scholarship on evidence. Instead, a more measured and sophisticated appreciation of the nature and functions of formal analysis is called for. It is particularly important to have a better understanding of the varied functions that symbols such as numbers can have in formal analysis.

Some of the criticism of the new evidence scholarship assumes that numerical notation in mathematical analysis of evidence and inference is necessarily designed to make evidence and inference either exact or quantitative, or both. As I have explained elsewhere,[15] numbers do not *have* to serve these functions in mathematical arguments about evidence and inference, and they do not serve such functions in Schum's analyses. Schum sees numbers and other symbolic notation as a special kind of grammar or language and he does not believe that the use of this special language entails a commitment either to exact quantification of inference or to objectivity in inferential reasoning.

In his book, Schum translates Bayesian concepts and other complex "mathematical" concepts into "ordinary" language (and also into transparent charts and diagrams). In doing this, Schum does not in any sense lay aside "mathematical logic" and other types of "technical" logic; as he sees it, he is simply translating that sort of logic into a

14. *See* Callen, *Notes on a Grand Illusion: Some Limits on the Use of Bayesian Theory in Evidence Law,* 57 IND. L.J. 1 (1982); Graham, Jr., *"There'll Always Be an England": The Instrumental Ideology of Evidence* (Book Review), 85 MICH. L. REV. 1204 (1987); Nesson, *The Evidence or the Event? On Judicial Proof and the Acceptability of Verdicts,* 98 HARV. L. REV. 1357 (1985); Tribe, *Trial by Mathematics: Precision and Ritual in the Legal Process,* 84 HARV. L. REV. 1329 (1971); Zuckerman, *Law, Fact or Justice?,* 66 B.U. L. REV. 487, 498-508 (1986); *cf.* Brilmayer, *supra* note 13, at 673.

15. Tillers, *supra* note 8, at 383-88.

different language. This effort to translate technical and mathematical logic into a form of discourse largely bereft of numerical notation reflects Schum's basic methodological assumption that Bayesian equations and other mathematical systems *instantiate and exemplify, rather than constitute, the logic of certain forms of inference.* By his ordinary language translation Schum tries to show that *the logic drives the mathematics, not vice-versa.*

Nonmathematicians have a tendency to think that a number must "stand for" or "represent" something. The function of numbers in mathematical arguments, however, may not be to represent any *thing* (natural phenomenon) at all, and the numbers in Schum's arguments are not used to count things in the world or to measure their size or intensity. It follows that there is nothing "statistical" about Schum's theory of inference. Instead, the function of numbers and other symbols in his analyses is to display a particular form of logic and its implications in a transparent and coherent way. Hence, Schum does not believe that numerical notation is essential to the analysis of the logic and structure of inference; in his view, the arguments made in the grammar of mathematics can also be made, if less elegantly and more arduously, without the use of any type of numerical notation.

This kind of theoretical enterprise has often been mischaracterized. Some legal observers have argued that mathematical analysts mistakenly strive for "objectivity" in evidence and inference.[16] Where this criticism is aimed at people such as Richard Lempert and David Schum, it is both mystifying and ironic because, while Lempert and Schum may have made mistakes, an excessive faith in the possibility of making inference transpersonally objective is surely not one of them. It is well known that Lempert, Schum, and others like them have been deeply influenced by the version of probability theory articulated by Bruno de Finetti[17] (and also by Frank P. Ramsey[18] and Leonard J. Savage[19]). De Finetti's theory was *not* about the objectivity of inference or about the feasibility of making inference objective by rooting it in objectively observable facts. De Finetti firmly believed that the foundations of inference are *subjective.* Indeed, de Finetti believed in a radical form of subjectivity. For example, he believed that observable regularities in the world are nothing more than a case of "stable mea-

16. *See, e.g.,* Nesson, *supra* note 14, at 1378-82 (discussion of statistical evidence and inference assumes that subjective Bayesian analysis is the same thing); Tribe, *supra* note 14, at 1361-65 (discussion of "dwarfing of soft variables" assumes that Bayesian analysis has statistical nature); Zuckerman, *supra* note 14, at 487 ("Much of the discussion of the role of probabilities in factfinding proceeds from the assumption of objectivity.").

17. De Finetti, *Foresight: Its Logical Laws, Its Subjective Sources,* in STUDIES IN SUBJECTIVE PROBABILITY 51 (H. Kyburg, Jr. & H. Smokler 2d ed. 1980) (reprint of 1937 article).

18. F. RAMSEY, *Truth and Probability,* in THE FOUNDATIONS OF MATHEMATICS AND OTHER ESSAYS 156 (R. Braithwaite ed. 1931).

19. *See, e.g.,* L. SAVAGE, THE FOUNDATIONS OF STATISTICS (1954).

surement."[20] De Finetti and others like him accord paramount importance to logic (and, to be sure, a specific type of logic) and it might therefore be said that they believe in "objective" rules of thought. Nonetheless, theorists in the tradition of de Finetti unquestionably hold to a subjective theory of probability. They are best described as "logical subjectivists" because they tend to believe that the nature of the connection between "laws of thought" and "reality" — events in space and time — is mysterious or (in de Finetti's opinion) utterly impenetrable.

It is not easy to explain why so many intelligent observers fail to appreciate the role of mathematical logic in the work of scholars such as David Schum; there is much accessible literature that both advocates and explains subjectivist interpretations of the standard calculus of probability. Perhaps some observers miss the point because they are too enmeshed within a particular intellectual and epistemological tradition. The philosophical roots of Bayesianism are found in neo-Kantianism rather than in the sort of empiricist perspective that still dominates evidence scholarship in the Anglo-American world. A rational empiricist of the English variety tends to believe that good logic offers a key to the nature of empirical reality and that objective knowledge is knowledge of the way things actually stand in space and time. While some neo-Kantians also believe in objective knowledge, the meaning imputed to "objectivity" is quite different and, from an empiricist's vantage point, quite subjective. If a neo-Kantian believes in objective knowledge at all, he believes that it consists of knowledge of the laws or forms of thought, not knowledge of the features of particular empirical events, which he views as "accidental." Correlatively, if the neo-Kantian believes in reliable empirical knowledge at all, he believes that logical thinking is the best cognitive tool we have for achieving reliable empirical knowledge because it is the only available cognitive tool. Logical ordering of credal states guarantees "mental" coherence and consistency, but nothing else; the rest is supplied by faith, intuition, opinion, hunch, or who knows what — but in any event, not by logic or mathematics.

While few contemporary Bayesians are as radically subjective as de Finetti, they are generally closer to de Finetti than to English empiricism. Hence, the appropriate critique of many Bayesians is not that they are trying to be too objective but that they are being too subjective. The great question for these subjectivist theories is whether they can effectively deal with empirical problems. The source of this problem is the premise that nothing in the world can objectively verify the validity of the logic embedded in those subjectivist theories. Excessive

20. B. DE FINETTI, PROBABILITY, INDUCTION, AND STATISTICS: THE ART OF GUESSING 145 (1972).

subjectivity — not excessive objectivity — is also the overriding problem in Schum's theoretical perspective.

III. NETWORKS

A. *Theoretical Complexity and the Complexity of Inference*

In 1971 Professor Laurence Tribe made a multipronged attack against "trial by mathematics."[21] He criticized a proposal by Michael Finkelstein and William Fairley[22] that Bayes' Theorem be used in criminal trials to deal with certain kinds of identity problems. In their article Finkelstein and Fairley made the simplifying assumption that factual questions such as the identity of the person who is the source of pertinent evidence (such as fingerprints) are decisive on the issue of the guilt or innocence of a given defendant. Tribe argued that this simplifying assumption is unrealistic and that Bayesian analysis becomes unduly complicated once this and similar simplifying assumptions are abandoned.

Tribe rightly noted that a factual question such as the identity of the (human) source of incriminating evidence is not necessarily the same as the question of guilt. To illustrate this point, Tribe examined a hypothetical homicide case involving palm print evidence. Suppose a woman's body is found in a ditch: the woman has been stabbed, and a palm print similar to the defendant's palm print is found on the knife used in the murder. Tribe observed that the issue of the identity of the originator of the palm print is not the same as the question of that source's guilt or innocence of the crime charged. The defendant might have been framed; the police might have planted the defendant's palm print on the knife. Moreover, even if the defendant in Tribe's murder case was the source of the palm print on the knife, it does not follow that the defendant was the killer. Suppose the defendant accidentally discovered the woman's body on the night of the killing, picked up the knife out of curiosity, put the knife back where he found it, and went home. The police then arrive at the murder scene and find the knife with defendant's palm print. The person who left the palm print is the defendant but the defendant is not the killer.

Tribe's argument about uncertainties of this sort was a prelude to a more general argument about the dangers of using Bayesian analysis in criminal trials. He made the uncontroversial observation that uncertainties of the sort presented in his murder hypothetical frequently arise in criminal trials. More controversially, he argued that Bayesian analysis necessarily distorts the probative value of evidence if the Bayesian equations applied by the trier of fact do not expressly incor-

21. Tribe, *supra* note 14.

22. Finkelstein & Fairley, *A Bayesian Approach to Identification Evidence*, 83 HARV. L. REV. 489 (1970).

porate every uncertainty which has a significant impact on the probative value of the evidence in question. Finally, he demonstrated that the equations used in Bayesian analysis take a rather intricate form if uncertainties such as the possibility of a frame-up are expressly taken into account and he argued that a trier without substantial mathematical expertise cannot be expected to understand such complicated equations or apply them correctly. By way of example, Tribe argued that the possibility of a frame-up alone "strain[s] the [Bayesian] system beyond its breaking point" because the Bayesian equations required to take this possibility into account are "messy" and too complex for a jury to use or understand.[23] In passing, Tribe further noted that the evidentiary facts may themselves be in dispute, "[f]urther complicating the picture . . . ."[24]

Tribe's argument had various interesting and novel features but was not entirely new. Wigmore, for example, had pointed out long before that inferences can run through a series of steps; that some factual inferences are merely intermediate steps in a series of inferences; and that in lawsuits most of the inferences about the ultimate facts in issue rest on a series of inferences.[25] Stated in Wigmorean terminology, Tribe's criticism is that Finkelstein and Fairley neglected the possibility of weak links at both the top and the bottom of inferential chains. They ignored possible weak links at the *top* of an inferential chain by neglecting to emphasize that some factual hypotheses (such as "the palm print on the knife is the defendant's") are intermediate rather than ultimate *facta probanda* — that some factual hypotheses have significance because and only because they are the way stations for further factual hypotheses which do have legal significance (*e.g.*, "*D* stabbed *V*"). Conversely, by calling attention (if only briefly) to the possibility that the matters serving as a foundation for the ultimate inference (to the final *factum probandum*) may be in dispute, Tribe effectively chastised Finkelstein and Fairley for forgetting that the *base* of an inferential chain may also be insecure. In Wigmore's terminology, Finkelstein and Fairley did not pay attention to the possibility that a proposition which serves as a *factum probans* (evidence) may itself be a *factum probandum* — that a matter that serves as evidence of something else may itself be an inference based on evidence.

In making these and similar criticisms of Finkelstein's and Fairley's proposal, Tribe (through no fault of his own[26]) was apparently unaware that David Schum had already begun to unravel how Baye-

23. Tribe, *supra* note 14, at 1364.

24. *Id.* at 1364 n.116.

25. 1 WIGMORE ON EVIDENCE § 2 (3d ed. 1940); *see also id.* at §§ 25 & 41.

26. Schum has published much of his work in rather obscure journals and often has not published his work at all. *See infra* note 43. In any event, Schum's investigations of inference were still in their early stages in 1971.

sian analysis can be structured to deal with the sorts of problems of multistage inference that Tribe considered. Schum's work did not and does not directly challenge several of Tribe's central claims. For example, Schum's intricate analyses clearly show that Tribe was right about the complexity of Bayesian equations that do take into account the many kinds of uncertainty that appear in real-world problems of evidence. (Some of Schum's *equations* run for many pages.) By the same token, the extreme intricacy of some of Schum's analyses of even relatively "simple" problems of inference makes it hard to quarrel with Tribe's view that jurors cannot be expected to understand or properly apply most Bayesian configurations of evidence problems. Nonetheless, Schum did undermine one of the foundations of Tribe's argument. As early as 1971 Schum was beginning to demonstrate that the enormous complexity of problems of evidence in the real world may not outstrip the capacity of Bayesian methods for structuring inferential reasoning. Schum's work was making it unsafe to say that complex, real-world problems of inference are inherently insusceptible of formal deconstruction and reconstruction along Bayesian lines.

Tribe had accused Bayesians of oversimplifying problems of evidence and inference in real-world litigation contexts. This charge, however, does not stick to Schum. Schum has consistently recognized — indeed, he has repeatedly and vigorously *emphasized* — the multistage character of practically all real-world inference. Indeed, Schum has seen a wrinkle in inference that some legal scholars have preferred to ignore. He has emphasized not only the vertically cascaded nature of inference, but also the "lateral" connections and interactions among chains of inference. (This analysis, by the by, goes beyond the conventional Bayesian recognition of conditionally dependent evidence because Schum's analysis recognizes the additional complications generated by the phenomenon of "source uncertainty.")

B. *Warp and Weft in Inference*

Schum was not the first scholar to see that the multiplicity of steps in inference produces complexity. A good many years before Schum began his work, Edmund Morgan also gave the legal world the following picture of multistage inference.[27]

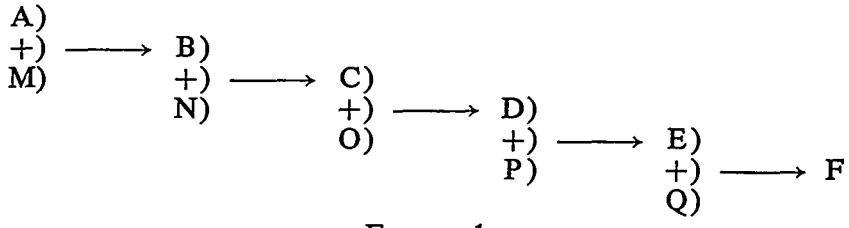

FIGURE 1

27. E. MORGAN, BASIC PROBLEMS OF EVIDENCE 185-86 (1961).

According to Morgan, Figure 1 represents the series of inferences involved in a murder case where the evidence is a love letter written by the defendant to the homicide victim's wife and the question is whether the person who wrote the love letter killed the husband of the female addressee. With the exception of *A,* which represents the love letter, the letters in the chart represent various factual inferences as well as certain supporting generalizations. For example, *B* represents the defendant's love of the victim's wife; *C,* the defendant's desire for exclusive possession of the victim's wife; and *M,* the generalization "A man who loves a woman probably desires her for himself alone." Similarly, *D* represents the defendant's desire to get rid of the victim and *O,* the generalization, "A man who loves a married woman and desires her for himself alone desires to get rid of her husband."

David Schum has used similar graphic devices to represent multistage inference. Schum's charts sometimes look like this:

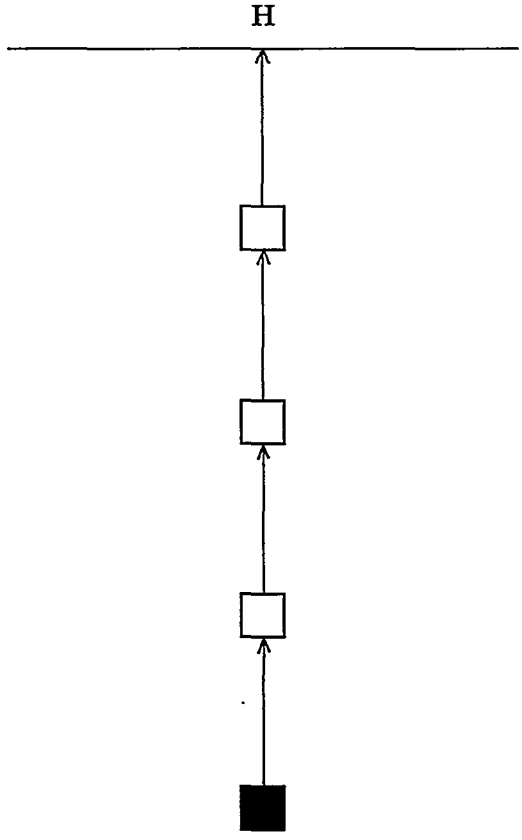

FIGURE 2

Figure 2 depicts a series of inferences built upon each other; like Morgan's chart, it depicts a *single* series of inferences based upon a *single* piece of evidence. Schum, however, makes a qualitative extension of

this sort of diagramming technique. Sometimes his charts have the following structure:

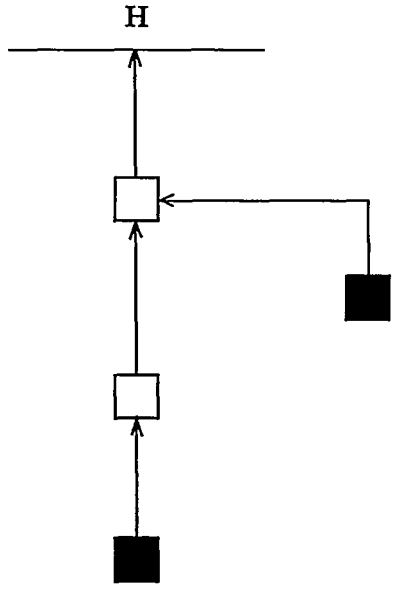

FIGURE 3

Figure 3, in contrast to Figures 1 and 2, represents *two* pieces of evidence (indicated by the two black boxes) and it represents *two* series of inferences (indicated by the arrows on the left going vertically from the black box on the left to "H" at the top and by the sequence of arrows beginning with the black box on the right-hand side, going through the open box to the left, and ending (again) at "H"). Moreover, Figure 3 represents a *lateral* interaction between two inferential chains, the chain that begins with the black box on the left and the chain that begins at the black box on the right).

Problems of evidence having this sort of structure often occur in the "real world." For example, we may interpret the black box at the left as "report or evidence of *D*'s escape from jail," the open box above it as "*D*'s escape from jail," the open box above it as "*D*'s belief in his own guilt," and the "H" at the top as "*D*'s guilt," and we may then interpret the black box *on the right* as "report or evidence of *D*'s statement, 'I did it.' " Schum's schema portrays, then, how separate lines of inference may join together to support an "upper level" inference (belief in guilt), which then forms a basis for the inference of "*D*'s guilt."

To be sure, Figure 3 is not yet intricate enough to portray all the evidence that typically interests us or all the lines of reasoning that typically occupy us when we face problems such as those involving evidence of escape. Nonetheless, the specimens of Schum's charts given here do suggest that Schum may have discovered a useful gram-

mar for portraying different *types* of complexities and uncertainties that may be found in real-world problems. If a satisfactory grammar exists, it may of course be used to construct more detailed descriptions of particular problems. Schum, for example, has constructed more intricate charts such as the following:

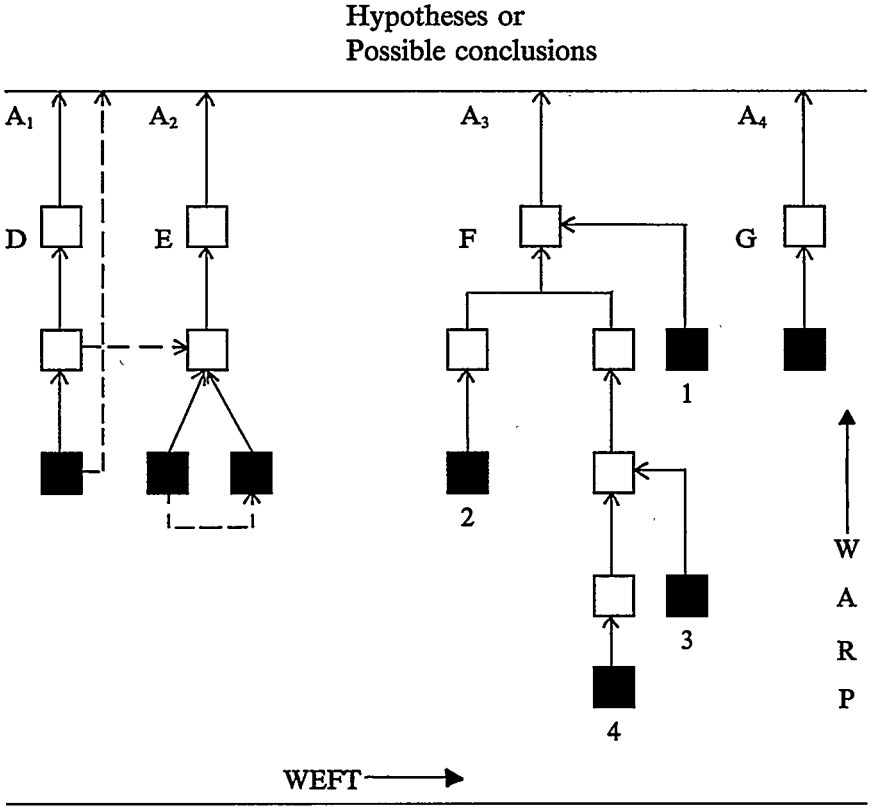

FIGURE 4

If this chart is not yet intricate enough, further detail may be added.

Almost twenty years have passed since Tribe's critique of the enterprise of Finkelstein and Fairley. While Tribe focused on a specific method of formal analysis (Bayesianism), his critique presented a general challenge to the aspirations of the new evidence scholarship: "Can formal analysis of evidence and inference deal with the complexity of problems of evidence and inference in the real world?" Thus far, Tribe's challenge has not been adequately answered. However, Schum's proposal to treat inference as a network points the way to a possible answer: web-like logical structures may be rich enough to capture the complex and dynamic thought processes we use to puzzle over evidence problems in the real world.

The notion of inference as a network has two advantages over a

number of other portraits of inference. First, a network accommodates the phenomenon of cascaded, catenated, or multistage inference. Conventional Bayesian analysis (before Schum) did pay attention to the relationships among different inferences in its references to the redundancy of evidence, but it almost entirely ignored the phenomenon of "source uncertainty," which occurs whenever there is a chain of inferences which bears on a factual hypothesis. As the example of the Finkelstein and Fairley article shows, this was a major limitation of Bayesian analysis. The network image, by contrast, practically *demands* that attention be paid to the equivocal character of most evidence.

Second, the network concept differs from portraits of inference of the sort found in Morgan's diagram (Figure 1). Morgan portrays inference as a chain connecting a piece of evidence and a fact. This chain may consist of a number of links or steps but ultimately a single chain is constructed. The network concept is richer: instead of being invited to think of inference solely as a single thread between a single piece of evidence and a single fact in issue, we are invited to think about threads that connect different pieces of evidence with different "terminal" facts-in-issue and about the many different ways that many different threads may be strung together and may interact with each other and transmit force to each other.

C. *Natural Inference and Theoretical Art*

Schum, unlike other Bayesians, has a surprisingly catholic attitude toward non-Bayesian theories. He sees merit in many theories of evidence, inference, probability, and proof. Schum is also idiosyncratic in his admiration for Wigmore's chart method; I know of no other Bayesian who has taken more than a passing interest in Wigmore charting techniques.

Schum's view of the mission of theorizing about evidence and inference is rather different from that of some of his contemporaries. Some theorists think of their investigations into the logic of inference as investigations of a "normative" logic and they draw a sharp distinction between normative and descriptive theories of the logic of inference. Schum, however, does not believe that normative and descriptive theorizing are entirely dichotomous, partly because Schum does not distrust ordinary inference and partly because he does not believe in a transcendental logic of inference. He instead tends to believe that theory should explain the structure and logic of ordinary inference in natural (real-world) settings such as law and medicine.

Schum's orientation toward the natural accounts for his theoretical catholicism because he believes that ordinary inference involves a variety of logics. His emphasis on ordinary logic, in a somewhat different way, also accounts for his interest in Wigmore diagrams. Wigmore

described his charting technique as a method for the analysis of "complex and mixed masses of evidence." Wigmore's charts are cluttered with several kinds of strange-looking symbols and, on first encounter, his charts seem far removed from ordinary thought processes. However, Wigmore was not trying to create an artificial logic; he saw the chart method as a device for keeping order in common sense reasoning about evidence. Schum approaches formal representations of inferential reasoning in the same spirit. Thus Schum is attracted to Wigmore-like charts because he believes that the function of formal theories and symbolic representations is *not to restructure our logic but to portray the logic we use* when thinking about evidence, facts, probabilities, and like matters. Schum believes that such charts are rich enough to reconstruct and mimic the sort of thinking that people are actually inclined to use when they face complex real-world problems — when they confront "mixed masses of evidence."

Schum's theoretical respect for natural inference seemingly generates a paradox. Schum does *not* imagine that he is engaged in a purely descriptive study of inferential logic; he does *not* think that the function of formal, orderly accounts of the logic(s) of inference is merely to replicate the way that human beings already think and process information. He believes that systematic logical analysis can *improve* our inferential reasoning. This conviction, however, seems open to the objection that no account of what people *actually* do when they grapple with evidence can tell them what they *should* do in order to assess evidence in a rational way.

Schum's approach does tend to make normative formal logic derivative of ordinary logic, but the puzzle about the normative force of Schum's "psycho-logic" is more apparent than real. Like de Finetti (and Wigmore), Schum thinks of theories of inference as devices for self-interrogation. He believes that systematic and orderly thinking about our own thinking — *i.e.,* formal reconstruction of informal logic — enables us to ask questions of evidence which, given the way we think, we should want to ask. This view is not puzzling if we assume that (1) people are, generally, already rational creatures, (2) the ratiocinative processes of human beings can work well but they can also work badly, and, (3) no matter how well we already use our reasoning processes, careful deliberation about those processes can sometimes make them work better.

As I have explained elsewhere, Schum's perspective on the relationship between theoretical art and natural inference makes a great deal of sense if one recalls that logic — including ordinary logic — is a *standard* rather than a brute "fact."[28] Appropriately enough, Schum seems to approve the distinction made by L.J. Cohen between inferen-

28. Tillers, *Mapping Inferential Domains,* 66 B.U. L. REV. 883, 933 (1986), *reprinted in* BAYESIANISM, *supra* note 7, at 277, 314.

tial *competence* and inferential *performance* (vol. 1, p. 376). Formal analytical techniques are not designed to provide fundamentally irrational people with rational methods for assessing evidence; they are instead tools that people can use to improve the inferential skills and capacities they already have. There is nothing odd about this idea. The common sense in Schum's position is obscured only by excessive devotion to the distinction between descriptive and normative theories. This distinction between "is" and "ought" does not work the same way in the field of inferential reasoning as in some other contexts.

Schum sees merit in a variety of theoretical perspectives precisely because he thinks of theoretical constructs as special tools for self-interrogation. Those tools must have different shapes and properties because our thoughts have different shapes and properties which can be honed only by different kinds of tools. For example, if we imagine that a problem involves generalizations about the world or people, no abstract logic can gainsay the importance of such "Baconian" thinking. Similarly, if we are "fixated" on the notion that our semantic categories have fuzzy edges, no abstract logic can deny the importance of the sort of vagueness that fuzzy set theory tries to address. Schum explains:

> Persons having an interest in various formal systems of probabilistic reasoning are sometimes asked: "[W]hich of these systems do you prefer?" This is rather like asking if you prefer your hammer to your saw. When you want to cut wood you prefer a saw over a hammer; when you want to pound a nail you prefer a hammer over a saw.[29]

The metaphor is quaint but effective.

I have already explained a few of the reasons why Schum admires Wigmore. Schum also admires the mental acuity of Sherlock Holmes. We can see the reason for this: Holmes was a logical fellow who knew how to make deductions. However, Schum also admires Charles Peirce who, while admiring logic, was also "pragmatic" in his attitude toward it. So is Schum. He believes that we should study the logics people actually use in various contexts.

Schum's respect for ordinary inferential reasoning is a rough analogue to the respect that ordinary language philosophy has for conventional language. Schum is convinced that human beings are already quite logical, perhaps more than they realize. Hence, he believes that in the study of "rational" inference it is important to respect the way people ordinarily think.

D. *The Atomic Structure of Inference*

Wigmore's resort to graphic representation was motivated by his

29. Schum, *Research on the Marshalling of Evidence and the Structuring of Argument*, in OPERATIONS RESEARCH AND ARTIFICIAL INTELLIGENCE: THE INTEGRATION OF PROBLEM SOLVING STRATEGIES (D. Brown ed.) (forthcoming).

awareness of the bewildering complexity of inference; he wanted an orderly way to evaluate complex arguments based on large masses and collections of evidence. His instincts told him that complex masses of evidence are more manageable if they are broken down into their basic, constituent parts. Of course, Wigmore did want to construct whole arguments based on such large collections of evidence, but he believed that we can understand "wholes" of evidence and inference only if we construct them out of their "atoms."

Although Schum, perhaps unlike Wigmore, knows that networks do not necessarily have to be represented by spatial forms, the reasons for his attachment to symbolic networks are much the same as Wigmore's. Schum's theoretical aims are also both "holistic" and "atomistic." Like Wigmore, he wants to break down global inferential assessments into their basic and primitive constituents; like Wigmore, he believes that the varieties of those constituents are finite in number; and, like Wigmore, he wants to construct (or reconstruct) chains, networks, and webs of inference by putting those constituents together.

Schum's project of constructing inference networks is in the spirit of Russell and Whitehead's *Principia Mathematica*.[30] Russell and Whitehead constructed notoriously long inferential chains and networks by defining basic elements, propositions, and operations and putting them together into complex arrangements — strings of complex arguments. Schum also believes that inference has an atomic structure — that there are basic elements, propositions, and operations in inference — and, like Russell and Whitehead, he also constructs complex inferential chains out of the basic constituents of inference.

The aim of atomic analysis is to make complex inference more understandable by decomposing it into simple parts. Although the atoms that make up complex inference can be arrayed and related in an infinite variety of ways and although very complex structures can be built out of the basic constituents of inference, inference at the atomic level is relatively simple. The project of describing complex inference is thus conceived as the project of tediously and painstakingly assembling large and complex inferential structures out of basic building blocks.

The atomic structure of inference, in its simplest form, looks like this:

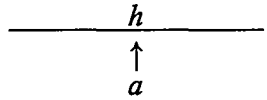

FIGURE 5

Figure 5 may be restated as "$a \longrightarrow h$" or "$a > h$".

30. A. WHITEHEAD & B. RUSSELL, PRINCIPIA MATHEMATICA (2d ed. 1927).

We may paraphrase "$a > h$" as "*a* implies *h*" — *provided* we do not give the word "imply" the meaning it has in deductive logic but take it to mean something like "indicate" or "suggest."[31]

In Schum's schema the sentence "$a > h$" merely states a possible inference. It does not assert that *h* should be inferred; it merely expresses the possibility that *h* may be inferred from *a*. Schum gives the term "inference" the sense it has in formal logic; "$a > h$" is merely an "argument." We do not yet know or assert *h* and we do not yet know by what criteria to determine whether *h* follows from *a*.

Inferential atoms can be strung together in different ways. For example, one possible arrangement is this:

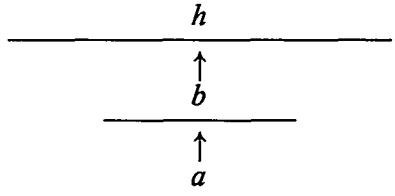

FIGURE 6

Figure 6 represents "$a > b$" and "$b > h$" or "$a > b > h$".

Figure 5 shows a single-stage ("vertical") inference. Figure 6 shows a "cascaded" inference or argument: the inference "$a > h$" has more than one step.

In constructing arguments, atoms can be arranged in an infinite variety of ways (although not in any way one pleases). Hence, we can construct more than one argument for *h*. If the arguments for *h* are unrelated, they might take the following form:

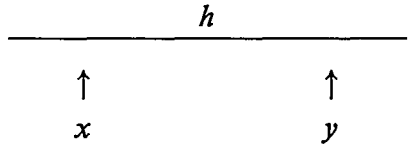

FIGURE 7

Figure 7 reads, $x > h$, and $y > h$.

Two or more independent arguments for *h* may themselves be "cascaded":

31. I add this proviso because the diagram in the text is a graphic representation with specific properties and a deductive argument cannot be constructed within or by the diagram. Schum's diagrams are directed acyclic graphs and, as Schum construes them, arguments of the form $m > n$ in these graphs are always inductive arguments. Later in this essay, however, I argue that it is enlightening, for purposes of discussion only, to endow the graphs with different properties and use them to portray deductive arguments. *See infra* note 32. It is possible, I admit, that my reinterpretation of the logic of Schum's diagrams is logically incoherent. However, my general argument about the relationship between models portraying the structure of inference and models portraying the logic of inference is not affected by this particular issue.

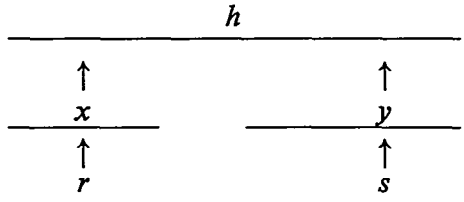

FIGURE 8

By the same token, separate arguments for h may be related. For example, we may have $r > x > h$, and $r > y > h$. This argument structure may be diagrammed in the following way:

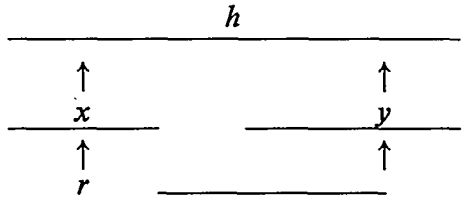

FIGURE 9

We can also have different kinds of connections. For example, we can have the following structure:

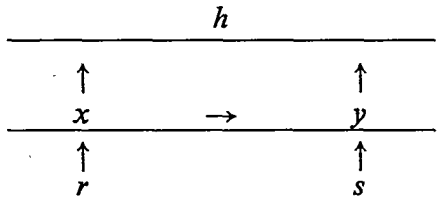

FIGURE 10

Figure 10 economically represents the following arguments (inferences):

(1) $r > x > h$
(2) $s > y > h$; and
(3) $r > x > y > h$

E. *Structure and Logic*

Figures 5-10 say both more and less about factual inference than one might think. They say *less* than one might think because they assert nothing about the nature of the connectors between the basic particles in atoms of inference: the nature of " $>$ " — and of its graphic equivalent, the arrow "$\longrightarrow$" — is left open by these diagrams, and the diagrams say nothing about the values that particles of the type "*a, b, ... n*" can assume (in atoms of inference of the form "$a > b$" or "$a \longrightarrow b$").

How the meaning of "$>$" or "$\longrightarrow$" is specified and what sort of variable "*a*" is matters because variations in the nature of the connec-

tors and the particles in atoms of inference make the reasoning different within each structure and may also produce different conclusions.

Consider, for example, a property of the complex of arguments portrayed by Figure 10. There we can see that even if the argument $s > y > h$ does not hold, we may nevertheless "get" h by reason of $r > x > h$. More generally, Figures 7, 8, 9, and 10 are argument complexes with this property: if one argument for a hypothesis or conclusion (such as h) drops away, it is possible to "reach" that hypothesis or conclusion by a different argument. For example, Figure 7 shows that if h cannot be "gotten" from the argument $x > h$, it is possible to "get" h from the argument $y > h$.

This type of argument complex means rather different things as the nature of the connections between the particles of atoms of inference varies — as the meaning of ">" or "⟶" varies — and as the characteristics of the particles vary. Consider again the argument complex shown in Figure 7, which looks like this:

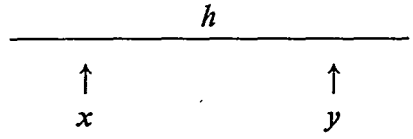

FIGURE 7

On the one hand, suppose that "⟶" is taken to denote the sort of implication found within traditional syllogistic logic. Although there is a sense in which this interpretation does violence to the logic embedded in the sort of graph shown,[32] let's assume that I can redefine the logic of the graph to accommodate my deductive reading of the graph. Alternatively, assume that x offers *conclusive* support for h, and so does y. On either interpretation, then, if x is "true" and $x > h$ is "true," h is "true" and the argument or inference $y > h$ can do nothing to "enhance" or "add to" the truth of h. Conversely, if y is "true" and $y > h$ is "true," the argument or inference adds nothing to the truth value of h.

On the other hand, suppose that h can take degrees of value and suppose that those degrees are either degrees of truth or belief in truth. Here the arguments $x > h$ and $y > h$ are not necessarily redundant even if a truth value is imputed to both arguments and a truth value is

32. *See supra* note 31. For the sake of the argument here I assume that the chart no longer has the property of being directed or of being acyclic. My argument, rephrased this way, poses the *question* of whether inference may be construed deductively. Schum takes the view (and I agree with him) that problems of factual inference cannot be construed deductively because we can then no longer meaningfully speak about one thing being "evidence" of another thing. However, I find it analytically useful to extend his thesis (beyond what he intends or accepts) so that I can leave open the question whether a deductive argument structure is a useful way of dealing with a problem of evidence. If this much is granted me, I can freely concede (for the sake of clarity) that "technically" I am no longer speaking of "inference from evidence," "inductive inference," or even "evidence."

imputed to the premises of the arguments; in this case, the two arguments taken together can produce a greater truth value for *h* than the two arguments taken separately — even though the two arguments taken separately show (to some degree) the truth of *h*.

Although the last two arguments fall within the same argument structure, they are arguments with quite different properties. In this example, the nodes in the argument networks have different properties. (The "fundamental particles" in the "atoms of inference" behave differently.) The result is that forces in these networks are transmitted differently even though the structure of these networks is the same.

The example just adduced may suggest that it would be just as well to dispense with argument structures with an indeterminate or fuzzy logical architecture and replace them with logical systems that ascribe specific properties to the connectors and the variables in inference networks. For example, a Bayesian might argue that factual inference clearly involves conditional probability and uncertainty and that interpretations of argument structures along the lines of traditional syllogistic logic should therefore be abandoned; Ockham's razor makes talk about argument structures in general unnecessary and it is better to translate them into argument structures with Bayesian properties.

This Bayesian might argue that the networks should be read as graphic expressions of assessments of the conditional probability of hypotheses (and he might thank Schum himself for showing how graphic expressions can incorporate Bayesian logic). He might also point to the argument in Figure 7 to support his thesis. The logic of Figure 7, you will recall, looks like this:

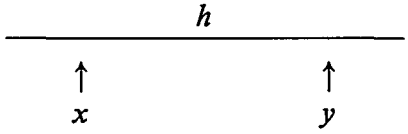

FIGURE 7

In a system that assumes that *h* has some value between 0 and 1 — *i.e.*, has a probability of more than zero and less than 100% — the arguments $x > y$ and $y > h$ may stand in an additive relationship — which is to say, the probability of *h* given *x* and *y* may be more than the probability of *h* given only *x* or only *y*. For example, the probability of rain if there are clouds in the sky and the temperature is low may be greater than its probability if there are just clouds in the sky. A Bayesian might argue that this sort of interpretation of argument structures is the only appropriate one. (The Bayesian achieves this interpretation of the arguments by construing expressions of the form $x > h$ or $x \longrightarrow h$ as equivalent to the notion of conditional probability, which is usually expressed by the notation P(X/H).)

My hypothetical Bayesian might argue that the explanatory power of inference networks is questionable if we do indeed posit that infer-

ences of the form $a > h$ are arguments within traditional syllogistic logic. For example, in Figure 7, if we read h as a factual hypothesis it may seem to us that the argument or inference $y > h$ has the capacity to add something even if we accept the argument $x > h$ and affirm x. Syllogistic logic, however, seems to deprive $y > h$ of any force or practical significance once h is inferred from x. For example, if we wish to know whether Socrates is mortal, and we know that all men are mortal and affirm that Socrates is a man, the additional premise that all animals are mortal seems to add nothing. A Bayesian (or another kind of probabilist) might argue that in reality we never encounter problems of factual inference with this kind of structure because in the real world we never encounter evidence that supports a conclusive argument for a factual hypothesis.

Schum has done more than any human being alive to show how complex inference can be interpreted along Bayesian lines. Nonetheless, he rejects the thesis that argument structures should always be reduced to a specific type of logic, even a Bayesian type of logic. General argument structures with open logical architecture have independent value, according to Schum, because he believes that the nature of the logic applicable to a problem of evidence or inference is often a *problem* with an unclear answer and that it is important not to prejudge the solution to such a problem by always giving argument forms a particular logical architecture. Hence, although Schum himself has evaluated inferential arguments on the basis of the premise that factual hypotheses — or, at least, beliefs about factual hypotheses — may take gradients of truth and falsity rather than just the values "true" and "false," and although he has also made the methodological assumption that those degrees of value have cardinal properties (which Bayesian analysis posits), he does not suppose that these suppositions are always mandatory. For example, he believes that an irrevocable decision in favor of universal use of cardinal gradients of probability improperly prejudges the question of the weight of an a on an h. In short, Schum wants to leave open the nature of the connection between evidence and hypothesis and believes that we can and must consider how different theories and logics visualize the connection between an a and an h.

Schum of course knows that argument structures with open logical architecture cannot solve for specific hypotheses, precisely because the question of the logic embedded in those argument structures is left open and undetermined. Schum nevertheless believes that networks with an open logical architecture should be retained. In his view, networks with diffuse logical properties are useful because they invite us to consider the question of which logic to use. Indeed, networks without specific logical properties have value precisely because they do not tell us how to solve for our hypotheses; their open logical structure

reminds us that logic-commitments for the analysis of evidence may remain relatively (if not wholly) open.

The import of Schum's position is best understood in relation to debates about the coherence of L.J. Cohen's Baconian theory of probability and proof. Glanville Williams and others have argued that Cohen's theory of factual proof is nonsense, in part on the grounds that there are degrees of uncertainty and that any theory that refuses to acknowledge this simple fact is incoherent and intuitively implausible.[33] Schum, while agreeing that there are often degrees of uncertainty, has shown that Cohen's theory is not therefore incoherent,[34] and that the question of sense and nonsense is more complicated than critics such as Williams think.

The open logical architecture of Schum's networks serves as a salutary reminder of a lesson that mathematicians and logicians supposedly learned long ago: there are different logics and some of them are incommensurable with each other. That is to say: there are logical and mathematical systems with different properties, and these distinct systems of logic must not be understood as incoherent from the standpoint of any particular system of logic, but rather simply as different. If we understand that principles of commutation, transitivity, and the like may differ in different logical systems, we can also understand that there is nothing *incoherent* about a system which, for example, asserts that b is uncertain in the argument $a > b$ but that when a decision has been made in favor of b (rather than in favor of some other hypothesis) it is to be taken as true for purposes of the argument $b > c$. As Schum has shown, Cohen's reasoning is fully coherent in a logical system with particular properties (*e.g.*, in a system that assigns ordinal rather than cardinal properties to uncertainty and that construes negation in a noncomplementary way).

The value of a theory (such as one that does not speak of grades of uncertainty, but only ranks them) depends on the features of inference the theory in question is capable of clarifying. In debates about the logic of inference, probability, and proof the central question is often not whether a logic is *wrong,* but rather, what it is *for.* Of course it may still be true that a theory such as Cohen's is *unenlightening* or *uninformative* — and I confess that I myself am troubled by many of the uses that Cohen has made of his theory — but *this* question is not the same as the question of the logical or formal coherence of a theory. Moreover, the question of the proper *domain* of a logically coherent theory is generally the *right* one to ask.

33. Williams, *The Mathematics of Proof — I & II,* 1979 CRIM. L. REV. 297, 340.

34. Schum's most recent discussion of Cohen's theory is Schum, *Jonathan Cohen and Thomas Bayes on the Analysis of Chains of Reasoning,* in RATIONALITY IN HUMAN REASONING (E. Eells & T. Maruzewski eds.) (Poznan Studies in the Philosophy of the Sciences and the Humanities) (forthcoming). See also his earlier analysis in Schum, *A Review of a Case Against Blaise Pascal and His Heirs,* 77 MICH. L. REV. 446 (1979).

IV. INFERENCE AND TIME

Theorists often simplify real-world problems to make them more manageable; some properties of the problem under investigation are isolated while others are provisionally ignored. This strategy has been used (sometimes unwittingly) by students of evidence and inference. However, if the ultimate aim of theorizing about evidence and inference is to clarify the properties of proof processes in real-world settings such as litigation, the "true" character of proof processes cannot be ignored indefinitely. Theories that ignore salient realities of proof processes may turn out to be either useless or positively dangerous.

Time plays a salient role in evidence, inference, and proof but neither the newer nor the older evidence scholarship has said much about it.[35] The focus of analysis instead has been on the effect of a known body of evidence on a known set of issues, which Schum calls "relational analysis."[36] Schum does discuss the relationship between time and inference, however, and this paves the way for a more realistic analysis of the structure and logic of inference in litigation.[37]

The role of time in inference presents several problems. Schum shows that analysis must deal with three separate relationships involving time: inference in time, time in inference, and evidence in time.

To begin with, time destablizes the environment for inference. In extreme cases changes brought by time may sweep away the lacework of complex inferential reasoning; inferential labor may go for naught because of changes in information, because of the mutation of the factual hypotheses in issue, or simply because of the decisionmaker's revisions in his own beliefs. In less extreme cases the changes in the inferential environment are less than global and we then wish to structure our thinking to cope with those changes and with changes yet to come. This is the problem of inference in time.

However, Schum shows that time and inference are related in at least two other ways. First, the matters in issue may themselves be in time and, if they are, they are also part of some sort of causal order, which means that prior events may affect the probability of later events. Second, evidence itself occurs in time: evidence is received at

35. The sparse legal literature on factual inference in a nonstationary environment consists almost entirely of literature on trial and clinical practice. Unfortunately, this literature is generally unsystematic and it generally offers little more than maxims and recipes, often of a rather dubious sort.

36. The methodology of relational analysis not only ignores the dynamic qualities of the decisionmaker's legal and evidentiary environment, it also assumes that the evidence at hand is not causally related to the temporal and causal connections among the events whose occurrence is hypothesized.

37. *See* vol. 1, pp. 284-354; *see also* Schum, *Probability and the Processes of Discovery, Proof and Choice,* 66 B.U. L. REV. 825 (1986) [hereinafter Schum, *Probability and Processes*], *reprinted in* BAYESIANISM, *supra* note 7, at 213; Tillers & Schum, *Charting New Territory in Judicial Proof: Beyond Wigmore,* 9 CARDOZO L. REV. 907, 907-08, 947-49, 951-63 (1988) [hereinafter Tillers & Schum, *Charting New Territory*].

a particular point in a temporal order and thus, it may itself be caused by prior events and it may also affect or cause the occurrence of subsequent events. Although I cannot do justice here to Schum's discussion of the implications of these latter two relationships, several of his points about evidence in time and about time in inference deserve to be highlighted.

Schum's views about evidence in time place conventional subjectivist analysis of evidence in an important light. He makes the point that the methodology of relational analysis and subjective Bayesianism ignores the possibility of a causal relationship between the hypothesized events in issue and the evidence bearing on those events and that subjective Bayesian views the temporal sequence of the appearance of evidence and the hypothesized events as immaterial. He notes, however, that it certainly is not counterintuitive to assume that evidence appears in time and has a causal relationship with both prior and subsequent events. This insight generates a number of fascinating observations and important analyses.

Schum's insight that the temporal order of the appearance of evidence is inferentially significant allows him to tease out a number of important implications. He demonstrates, for example, the possible rationality of a decisionmaker (such as a juror) who, unlike a conventional Bayesian, believes that the order in which evidence is presented is probatively significant. He also shows that it may be quite important for a decisionmaker to think very carefully about the relative temporal order of (1) the appearance of evidence and (2) the events that this decisionmaker hypothesizes may have happened. A great deal may ride on whether the evidence appeared before or after the event whose probability is being assessed. Similarly, Schum uses his basic premise to argue that the behavior of a processor of information may be inferentially and probatively significant.

Schum's talk about time in inference also merits mention. Schum's recognition that it is important to think about chronology when evaluating the probability of an event brings inferential theory into close contact with empirical studies[38] of the importance of stories in decisionmaking. Stories have become an important theme in much recent theorizing about evidence and proof. The participants in these discussions, however, may not always make clear which version of the relationship between time and inference they are discussing or supposing. Schum's analysis shows that story analysis is generically different from the other two relationships between time and inference and it thus paves the way for clear-headed debate about the role of "stories" and "narratives" in proof and also for empirical research with an adequate analytical foundation.

38. *See, e.g.*, R. HASTIE, S. PENROD & N. PENNINGTON, INSIDE THE JURY 22-23, 163-65 (1983).

Schum's analysis of the workings of inference *in* time is equally subtle. The analysis in *Evidence and Inference* focuses on the way in which various argument structures might be adjusted to accommodate new information, new suppositions, and new questions. This analysis is useful because it serves as a reminder that the problem associated with nonstationary decision-making environments is *not* the problem of how one copes when everything changes — for if everything changes, nothing that was done before, thought before, believed before, or heard before matters. The only problem of interest is how we manage changes that may be significant but are not wholly unconnected with the past. The charting techniques Schum recommends in *Evidence and Inference* serve in part as devices a decisionmaker can use in an effort to *identify* the extent of the changes that time has brought.

Schum makes two further points in *Evidence and Inference* about the importance of thinking about inference as being in time. His first general message is that it is dangerous to adopt the strategy of supposing that the problem at hand remains essentially the same. Schum argues that we cannot cope with nonstationary problems by holding stationary beliefs. That strategy invites all sorts of disasters. ("The Russians do not have atomic weapons"; "Asbestos does not cause asbestosis.") Schum notes that a strategy of making guesses only about more manageable localized sequences often is not available. (This, I might note, is likely to be true in litigation, where "global" factual assessments must often be made whether one feels up to it or not.) Schum further observes that even slight changes can produce dramatic changes in factual inferences from evidence. It follows that an ideal decisionmaker never sits still:

> [T]he structure of arguments we make from evidence [has] to undergo revision if we are to keep abreast of recognized changes in the world. Structural revisions . . . involve changes in our hypotheses or possible conclusions, changes in our assumptions, premises, and generalizations, and, of course, changes in the amount and kind of evidence we obtain. [vol. 1, p. 318]

Schum's most general message about time in inference is probably the most important of all: that *all inference is dynamic.* Schum notes that we cannot stop the flow of time and freeze the moment, even for purposes of evidentiary evaluation. The effects of time are not occasional: *"[A]ll* of the ingredients of [an] inference task (hypotheses, evidence, and assumptions) are in various ways contingent upon time and the flow of events" (vol. 1, p. 292; emphasis in original).

Schum has done other research that significantly extends the analysis presented in *Evidence and Inference.* In his Boston symposium article,[39] he presents an elaborate and stunningly original schema that

39. Schum, *Probability and Processes, supra* note 37.

views a lawsuit as a play with various acts and scenes, and suggests that certain theories of inference portray some of those acts and scenes better than other theories do. The argument taken as a whole is provocative because of the underlying assumption that the logic of proof activity in time changes as that proof activity progresses, that different logics describe what is going on (predominantly) in different parts of the proof process.

The apparent corollary of this thesis is that there may be a logic that describes shifts in the logic of investigation, inference, probability, and proof in litigation and that such shifts are, roughly speaking, tied to the chronological development of a lawsuit. While the Boston article may have overstated the degree to which there are relatively clean breaks in argument styles and structures in different phases of litigation, the paper defines some very important questions meriting careful investigation.

Schum is now investigating these and other questions, with the feeble assistance of this reviewer, using a simulation of a sequence of events that ends up, finally, in a lawsuit.[40] This research may produce some very important results and, even if it does not, it may identify the problems that need to be addressed to provide a model of the dynamic structure of proof processes. It already seems apparent, for example, that stories centered on individuals are a good way of storing information for later retrieval in a changed legal and factual environment. This finding intersects in interesting ways with empirical and conceptual research suggesting that stories are an important device for the assessment of evidence.

V. Atomism and Holism in Inference

Schum sees inference as a network and he believes that networks of inference are extremely intricate. Hence, the webs that Schum weaves around problems of evidence and inference typically consist of many delicate threads, which crisscross in various ways. These threads are sometimes difficult to keep in mind and almost seem to vanish from sight.

Schum's microscopic analyses of evidence and inference may seem unduly intricate; it is natural to wonder whether an entirely different approach to evidence and inference might work better. There has been discussion (although not quite a debate) about the value of fine-woven analyses of evidence. I myself have sometimes wondered if people might do a better job of drawing inferences if, instead of analyzing or dissecting evidence, they would just look at a mass of evidence "as a whole," try not to think too much about it, and then grunt out a re-

40. This research is supported by NSF grant SES-8704377 to George Mason University.

sponse from somewhere within themselves to the undifferentiated mass of stuff they see in front of them.

This kind of "holistic" alternative to microscopic analysis is practically its own refutation. It is hard even to imagine what it means to take evidence "as a whole." We perceive slices and various features in almost everything we see — and if we don't, perhaps we can't see anything at all. Moreover, it is hard to imagine how we can imbibe the evidence we "see" without performing *some* sort of mental analysis, which by definition seems to involve some sort of dissection. In short, it is hard to imagine how we can think holistically even if we want to do so. The admonition not to analyze and dissect almost seems tantamount to advice not to think too carefully about the way you think. One might as well advise you not to think about elephants. You may not have been thinking about elephants before, but once you are told not to do so, you cannot stop thinking about them. Hence, if we are to believe that holistic assessments play a part in inference, we must have a more subtle concept of "holism." Any theory that assumes an absolute dichotomy between holistic thinking and nonholistic thinking is thoroughly implausible and any theory that admonishes people to think globally rather than locally is vacuous.

Ironically, David Schum, with his eye for microscopic detail, indirectly advances an argument for a more credible version of holism. Schum, of course, rejects any sort of holism that denies the value of thinking about parts of "wholes" of evidence; he plainly thinks that it is important to think about details. Nonetheless, he does not believe that details in evidence work autonomously to generate conclusions, nor does he believe that theoretical analysis of evidence can provide a recipe or algorithm for summarizing the implications of pieces of evidence which simply happen along. A decisionmaker plays a creative and constitutive role when he assesses evidence, and formal analysis, like any other kind of mental dissection, must be seen as just another form of this constitutive activity.

On Schum's premises, it is *impossible* to construct an algorithm to compute the probative value of evidence because it is impossible to identify every detail that affects the force of evidence in a real-world situation. Although Schum puts evidence under a microscope and believes in the value of atomic analysis, he does not believe that it is possible to incorporate every significant evidentiary detail into a set of theoretical statements. Moreover, he does not believe that there is an objectively correct rendition of the structure of a problem of evidence and he does not even acknowledge the primacy of any particular type of inferential logic. Hence, although every subjectivist is a holist to a degree, Schum's subjectivism and holism run deeper. Not only does he acknowledge that different people may rationally draw different conclusions from evidence even when they use the same logic, but he

also believes that rational people may configure and construct problems of evidence in very different ways.

The human actor takes center stage in Schum's theory of evidence and inference. Schum has profound respect for the way that ordinary people ordinarily think. The purpose of formal analysis of inference is not to straighten people out or to show them how dumb and irrational they are, but instead to give people devices for sorting out their own thinking better. He believes that this sort of methodical analysis — which practically amounts to self-reflection — enables people to identify better the course of their reasoning, facilitates identification of the weaknesses and strengths in their inferential reasoning, and makes it easier for people to identify the nature of their disagreements about evidence. In all cases, however, the human actor remains the final arbiter of his thoughts and the final judge of the structures he should use to clarify and order his thinking about evidence. If, for example, the actor gains more insight by washing out details — or if the details confuse rather than enlighten him — he should wash out the details and think at a coarser, but not necessarily less reliable, level.

Schum talks about inferential "arguments." He uses the term "argument" in the sense that logicians do, but I think he also likes the word "argument" for a different reason. It serves as a reminder that inference is a human construct and that complex formal analyses of evidence are also constructed, assembled, and constituted by human actors. Nonetheless, Schum does believe that we gain knowledge by constructing inferential arguments and he takes it for granted that overall arguments must be constructed out of smaller pieces. It follows that there can be no domination of wholes by atoms, or vice versa, and that there is no choice to be made between "holism" and "atomism." The rational assessment of evidence and inference involves an interaction between wholes and parts.

Schum devotes little if any attention to any general logic governing the interaction of parts and wholes. He evidently believes that not much can be said about any such logic because we have no celestial vantage point from which we can describe such a logic. The attention Schum devotes to detail suggests that he thinks that we can ascertain the general "feel" we have for a problem only by seeing how we are moved to single out parts and assemble them. All we can do is try to describe and explore our thinking in an orderly fashion, and when we find we can go no further, we know that our overall sense of a problem has been satisfactorily exemplified. After this, no more can be said.

For my own part, I think the value of deconstructing and reconstructing evidence and inference remains uncertain, and if I have any criticism to make of Schum it is that he seems too sure of the value of analysis. But uncertainty is everywhere. We live with it and take our chances. We could do much worse.

VI. MAINSTREAMS AND SIDESTREAMS

After graduating from high school in 1950, David Schum did not go directly to college. In the two years following his graduation, he played semiprofessional basketball in the old Amateur Athletic Union, he played jazz piano — well enough, it seems, to make a bit of money doing it — and otherwise, he tells me, he "bummed around." Then he went to college at Southern Methodist University. There he played football, briefly, but gave up this enterprise when it became apparent that his bones might be crushed by much bigger and stronger adversaries. After graduating from college in 1956, Schum did not go directly into a graduate program. Instead he entered the Air Force and became a navigator on a B-47 in the Strategic Air Command. In 1959, at the age of 27, he entered a master's program in psychology at Southern Methodist University. In 1961, he entered a Ph.D. program in psychology and statistics at Ohio State University.[41]

I am struck by two things in this brief story of Schum's years as a young adult. First, whether by accident, choice, or necessity,[42] the path that Schum took in the decade after his graduation from high school was not the sort that would or should have been taken in the 1950s by a young adult with a single-minded determination to become a prominent academic. Schum's path to graduate study was rather indirect and he got to his destination rather late. Second, his academic work was intertwined with an unusual mix of other activities.

The pattern of Schum's activities as a mature scholar is not entirely different. Schum is a prolific scholar and he has always had his admirers, but the widespread professional esteem he now enjoys came rather late. This was partly his own doing; his work and activity as a scholar do not evince a single-minded determination to gain status in the academic world and in many ways he seems oddly indifferent to professional recognition.[43] Moreover, Schum's scholarly work is unconventionally and surprisingly varied; he has drawn on an unusual mix of disciplines in working out his own approach.

Schum apparently paid a price for his theoretical eclecticism and

41. Schum received his doctoral degree in 1964. He did postdoctoral work at Ohio State University, in the Laboratory of Aviation Psychology, from 1961 to 1966. In 1966 he took a faculty position at Rice University. He left Rice in 1985 and he now teaches at George Mason University.

42. I cannot shake the feeling that Schum's own life-choices are not entirely to blame for his relative (but hardly complete) obscurity in years past. I see Schum as a truly original thinker and I cannot help but wonder whether the academic world in America had and still has invidious features that account for his earlier obscurity. It may be noted that few children with privileged socioeconomic backgrounds would have chosen to play semiprofessional basketball in the early 1950s.

43. For example, Schum has quite deliberately chosen to publish much of his work in relatively obscure journals and quite often he does not bother to publish his work at all. He also remains maddeningly silent at conferences at which people say foolish things and when he does criticize them he does it so gently that they do not feel at all chastised.

pluralism. Psychologists in the main didn't think of him as a psychologist; mathematicians didn't think of him as a mathematician; philosophers didn't see him as a philosopher or epistemologist; statisticians didn't think of him as a statistician; and lawyers certainly didn't think of him as a legal scholar. They were all right, but they were also all wrong. He was none of these things and he was all of these things. Schum has concocted something new and important out of a large variety of disciplines. He has created, almost single-handedly, a new science of evidence and inference.

Schum's work demonstrates that a theory of evidence must be a science[44] and that a science of evidence must provide a map of the mind. If the new evidence scholarship fails to adopt this perspective, it will become either an academic epiphenomenon or a technology that masquerades as a theory of evidence and inference. To be sure, if the new evidence scholarship takes either of these two directions, all is not lost; there is value in both contemplative theorizing and technological knowledge. However, the new scholarship began with a grander ambition: to advance our understanding of the fundamental structure of uncertain knowledge about the world. It should stick with this grand ambition.

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44. It is arguable that Schum's work is not scientific in a strict sense because his theories arguably are not "verifiable." I think this criticism is wrong. *See* Tillers & Schum, *Charting New Territory, supra* note 37, at 911 n.8.

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