

THEORETICAL NOTE

Ecological and Evolutionary Validity: Comments on Johnson-Laird, Legrenzi, Girotto, Legrenzi, and Caverni's (1999) Mental-Model Theory of Extensional Reasoning Uncertainty

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The mental-models account of naive probabilistic reasoning by P. N. Johnson-Laird, P. Legrenzi, V. Girotto, M. S. Legrenzi, and J. P. Caverni (1999) provides an opportunity to clarify several similarities and differences between it and ecological rationality (frequentist) accounts. First, ambiguities in the meaning of Bayesian reasoning can lead to disagreements and inappropriate arguments. Second, 2 conflated effects of using natural frequencies are noticed but not actually tested separately because of an artificial dissociation of frequency representations and natural sampling. Third, similarities are noted between the subset principle and the principle of natural sampling. Finally, some potentially misleading portrayals of the role of evolutionary factors in psychology are corrected. Mental-models theory, rather than better explaining probabilistic reasoning, may be able to use frequency representations as a key element in clarifying its own ambiguous constructs.

I applaud the effort by Johnson-Laird, Legrenzi, Girotto, Legrenzi, & Caverni (1999) to integrate the research fields of reasoning and decision-making. In conjunction with similar endeavors before theirs (e.g., Evans, Over, & Manktelow, 1993), the overall message that these two areas can fruitfully collaborate seems to be a growing theme. As such interdisciplinary works yield exciting research developments in both fields, it is hoped that more and more psychologists will be convinced of the value of such an approach.

One of the benefits of interdisciplinary work is that two competing conceptual frameworks sometimes lay claim to the same phenomena, and the work to resolve such debates can sharpen and refine those frameworks. In this case, the assertion that a mental-models theory is appropriate for explaining statistical reasoning and decision making is contrasted with what Johnson-Laird et al. (1999) call the "frequentist" hypothesis. It is crucial in such situations to establish, from the onset, what precisely each theoretical position asserts. Whereas Johnson-Laird et al. cursorily dismiss a frequentist model of ecological rationality as an adequate explanatory framework, it appears that several aspects of the frequentist position may have been inadequately described. In an effort to refine the issues that do and do not contrast these theoretical frameworks, this article outlines several debates and the relevant frequentist positions. Specifically, I address the following questions: (a) Can frequencies elicit Bayesian reasoning? (b) Are

frequencies a privileged representational format? (c) Do frequencies *per se* facilitate statistical judgments? (d) What is the role of evolutionary theory in supporting the frequentist model of ecological rationality?

But is it Bayesian?

The fact that natural frequencies (as compared to single-event probabilities) lead to improved performance on a number of statistical reasoning tasks is a matter of empirical fact (e.g., Brase, Cosmides & Tooby, 1998; Cosmides & Tooby, 1996; Gigerenzer & Hoffrage, 1995). A frequentist explanation, which is actually part of a larger explanatory framework sometimes called *ecological rationality* (Gigerenzer, Todd, & the ABC Research Group, 1999), claims this as evidence that "frequentist problems elicit bayesian reasoning" (Cosmides & Tooby, 1996, p. 62). Johnson-Laird et al. (1999), on the other hand, have countered this claim with the assertions of Howson and Urbach (1993) that, "with probabilistic reasoning, and especially with reasoning about frequency probabilities, Cosmides and Tooby's results have very little to do at all" (p. 422).

The position of Howson and Urbach (1993) was derived from the fact that the use of frequencies (or more specifically, natural frequencies) makes probabilistic reasoning computationally simpler (as explained quite clearly from a frequentist perspective by Gigerenzer & Hoffrage, 1995). The difficulty here is the confounding of two possible meanings of *Bayesian reasoning*. One interpretation – apparently used by Howson and Urbach – is that "bayesian reasoning" refers to the use of the classic formula for Bayes's theorem,

$$p(H|D) = \frac{p(D|H)p(H)}{p(D)},$$

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in which case natural frequencies (not being in a probabilistic format) cannot be Bayesian.¹ A second interpretation, used by Cosmides and Tooby (1996) as well as Johnson-Laird et al. (usually), is that “bayesian reasoning” refers to reasoning in a way that achieves the function that Bayes’s theorem is designed to achieve (i.e., resulting in a posterior probability conforming to Bayes’s Law). Under this interpretation, frequencies can and do elicit bayesian reasoning. Furthermore, this reading accepts the fact that reasoning that starts with frequency representations, manipulates those frequency representations, and produces frequency outputs can nevertheless embody aspects of a calculus of probability.

Are Frequencies a Privileged Representational Format?

The above debate does unveil a legitimate issue, however, and that is the question of whether or not frequencies are a privileged representational format in the human mind. Precisely because the use of natural frequencies leads to a computational simplification of Bayesian reasoning problems (using the second sense of the term *Bayesian*), there is a confound in most previous research: The superior performance by subjects using frequencies could be due to computational simplification only, or due to both the simplification and the mesh between frequency formats and how the human mind works. Distinguishing between these two possibilities has not been explored in previous research, and Johnson-Laird et al. (1999) perceptively point out that “it is indeed crucial to show that the difference between frequencies and probabilities transcends mere difficulties in calculation” (p. 81). To that end, I have recently conducted research (Brase, in press) that assesses the relative clarity and ease of understanding for different numerical formats. Frequencies are, in fact, seen as clearer and easier to understand than single event probabilities. Because this research was already in progress when Johnson-Laird et al. was published, we apparently agree on the importance of this issue. On the other hand, as will be explained in the subsequent section, the data discussed by Johnson-Laird et al. is not an adequate assay of this issue.

Do Frequencies per se Facilitate Statistical Judgments?

Another question has to do with the claim that “data in the form of frequencies by no means guarantee good Bayesian reasoning”² (Johnson-Laird et al., 1999, p. 81). The problem is that this claim is validated only by artificially separating frequencies from the natural sampling system in which they are usually found. That is, what is called the frequentist approach actually involves one specific type of frequency called *natural frequencies*. A natural frequency is the result of the sequential acquisition of event frequencies from experience (i.e., natural sampling; see Aitchison & Dunsmore, 1975; Kleiter, 1994; Gigerenzer, 1998). This methods of information gathering by counting events as one encounters them and embedding the counts in a categorical conceptual structure. (e.g., see Figure 1b) produces frequencies that are not normalized to an arbitrary standard (such as 100 for percentages). Instead, frequencies based on natural sampling – also called *natural frequencies* – implicitly carry information about the base rates via their relative sizes. The frequentist position that seems to have been described by Johnson-Laird et al. (that all frequencies guarantee good Bayesian reasoning) is one that no present researchers

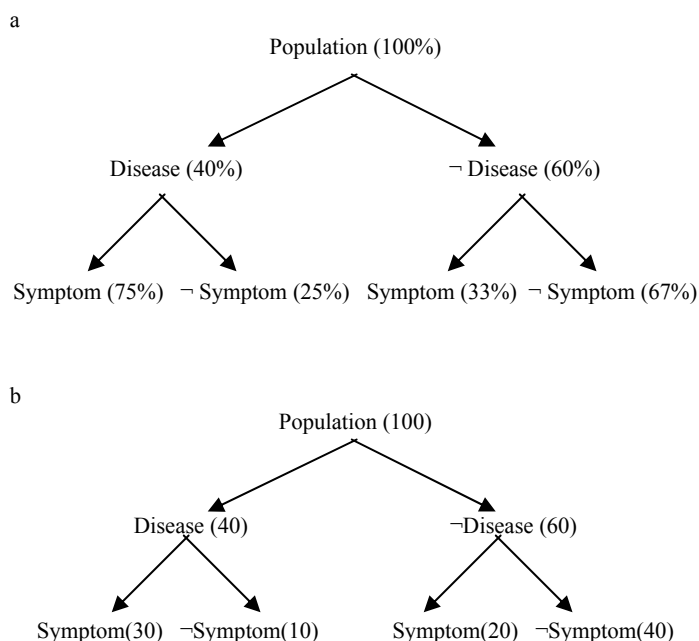


Figure 1. How to change a mental model into a natural sampling decision tree: (a) changes just the arrangement of items into a tree structure, and (b) changes the percentage numbers to frequencies (assuming a reference class of 100).

actually hold. From this viewpoint, however, Johnson-Laird et al. then reintroduce the basic relevant principle of natural sampling as their “subset principle.” They summarize their position by stating that

The real burden of the findings of Gigerenzer and Hoffrage, (1995) is that the mere use of frequencies does not constitute what they call a ‘natural sample.’ Whatever its provenance, as they hint, a natural sample is one in which the subset relations can be used to infer the posterior probability, and so reasoners do not have to use Bayes’s theorem. (p. 81)

Giroto and Gonzales (2000; cited as in press in Johnson-Laird et al., 1999, p. 81) provide an example of how one can separate the representation of information in frequencies from a system of natural sampling:

According to a recent epidemiological survey:

Out of 100 tested people, there are 10 infected people

Out of 100 infected people, 90 people have a positive reaction to the test

¹ If one is using frequencies—and not converting them to some other format—the traditional form of Bayes’s Theorem is inappropriate, much like entering binary numbers into a regular calculator is inappropriate. The utility of Bayes’s Theorem in its conventional form is that it expresses a set of relationships between different probabilities that is agnostic as to whether they are derived from frequency information or single-event confidences. However, this utility is purchased at a cost of power (as explained in a subsequent section).

² One must assume here that Johnson-Laird and colleagues use “bayesian reasoning” as referring to reasoning that achieves the same functionality as Bayes’s Theorem (estimating the posterior probability), rather than to the actual use of Bayes’s Theorem.

Out of 100 non-infected people, 30 have a positive reaction to the test

Imagine that the test is given to a new group of people. Among those who have a positive reaction, how many will actually have the disease? ____ out of ____.

The “mere use” of frequencies here, in fact, does not constitute a natural sample and that is an important part of the problem’s difficulty. The solution to this problem requires using the traditional form of Bayes theorem, which requires converting the numbers into probabilities, which means this problem is actually *more* difficult than a problem expressing data in terms of probabilities. Presenting frequencies outside of the context of a natural sampling system (i.e., as “mere” or more specifically, normalized, frequencies) is a very unnatural situation. In the real world, outside of casinos, racetracks, and decision-making research labs, information tends to exist in a way that is closely aligned with a system of natural sampling (hence the name). In other words, studying decision-making abilities with frequency information that fits with a natural sampling system is not so much a burden as it is a matter of ecologically validity.

Other than its ecologically validity, what about natural sampling is so important? Two more advantages of a natural sampling system, as compared to single event probabilities are that it conserves valuable information and that it is more flexible. To illustrate the first of these points, consider the above problem used by Giroto and Gonzales (2000). From a natural sampling perspective, as well as in other ways, it is confused. If one changes the frequencies to fit with a natural sampling system (and clears up semantic ambiguities and phrasing inconsistencies), however, the problem becomes clearer:

According to a recent epidemiological survey on a particular disease, 10 out of every 100 people have the disease.

A test exists to detect this disease, but this test is not perfect. It does not always detect when a person has the disease, and at other times the test indicates that a perfectly healthy person has the disease (called a “false positive”).

Specifically, only 9 out of every 10 people who have the disease get a positive result from the test for this disease.

Additionally, 27 out of every 90 people who do not have the disease also get a positive result from the test for this disease.

Suppose the test for this disease is given to a random sample of 100 people.

How many people, out of those who have a positive result on the test, will actually have the disease? ____ out of ____

Instead of using totally new reference classes in each of the sentences the problem now has one reference class of 100 people tested, with various subsets identified. This is the sort of natural sampling situation one is likely to encounter in the real world (e.g., you have 100 friends, who can be segregated into various subtypes...). It should be no surprise that when Giroto and Gonzales (2000) gave the original problem to people “not a single participant inferred the right response” (Johnson-Laird et al., 1999, p. 81). My own research using the revised version of this problem (with volunteers from a regional university) yielded correct answers from 20% of participants (N=30).³ There are several reasons to expect better performance on the revised problem: (a) the natural sampling structure corresponds to the way that infor-

mation is typically encountered in the real environment, (b) also as a consequence of the natural sampling structure) the computational requirements for solving the problem are less severe, and (c) the problem is more clearly stated, including a consistent syntax for both presenting data and answering the problem. Clearly, however, the conclusions of Giroto & Gonzalez (2000) overstated the implications of their results, especially results with 0% correct responses that may suffer from undeterminable floor effects.

The information about the *natural* frequencies of different events within this population is conserved in natural sampling, and this leads to another advantage of a natural sampling system, its flexibility. Because natural sampling tracks the simple frequency of occurrences, independent of any other events, one can in theory start from any event and calculate the conditional probability of any other event. This flexibility circumvents the major difficulties that the mental-models approach has with partitions (i.e., determining the appropriate set of exhaustive and mutually exclusive hypotheses in the denominator of Bayes’s theorem). As Johnson-Laird and colleagues point out, “The problem with the principle of indifference... is that it yields inconsistencies depending on how one chooses to partition possibilities.” (p. 68). With natural sampling, however, the problem of partitioning is addressed by the context of the real world situation being evaluated, and is not an issue of procedural inconsistencies. Specifically, Johnson-Laird et al. used as an example the following information: “The suspect’s DNA matches the crime sample. If the suspect is not guilty, then the probability of such a DNA match is 1 in a million. Is the suspect likely to be guilty?” (Johnson-Laird et al., 1999, p. 78).

They argue that the mental-models people tend to construct are the following (Johnson-Laird et al., p. 78):

		Frequencies
¬guilty	DNA matches	1
		999,999

On the other hand, a natural sampling system tracks the frequency of occurrences, independent of one another. This means that one could start from one of two origin points: knowing guilt versus non-guilt or knowing a DNA match versus nonmatch (Figures 2a and 2b).

On the basis of the structure of the given statement (beginning with “There is a DNA match”), and the usual contexts of real-world situations dealing with these events, one should use the system shown in Figure 2b. That is, the fact that there is a DNA match (or non-match) is usually known first, and the conditional probability question is over guilt or non-guilt. In fact, even if one uses the alternative model, the question that becomes most prevalent is the size of the pool of potential suspects (i.e., the total

³ As one reviewer notes, 20% correct responses is still not a spectacular performances. It should be noted, however, that the present participants were unpaid and from a regional public university. Better performances have consistently been found with participants who are paid or where from more selective institutions (e.g., Cosmides & Tooby, 1996; Gegerenzer & Hoffrage, 1999, Footnote 1).

⁴ In an abstract sense, one could argue that guilt should be considered first, since the individual achieved the status of being guilty prior to the DNA test being conducted. However, this is not the situation that people in the real world usually have to deal with; the test results are generally obtained first and then a verdict of guilt or nonguilt is decided.

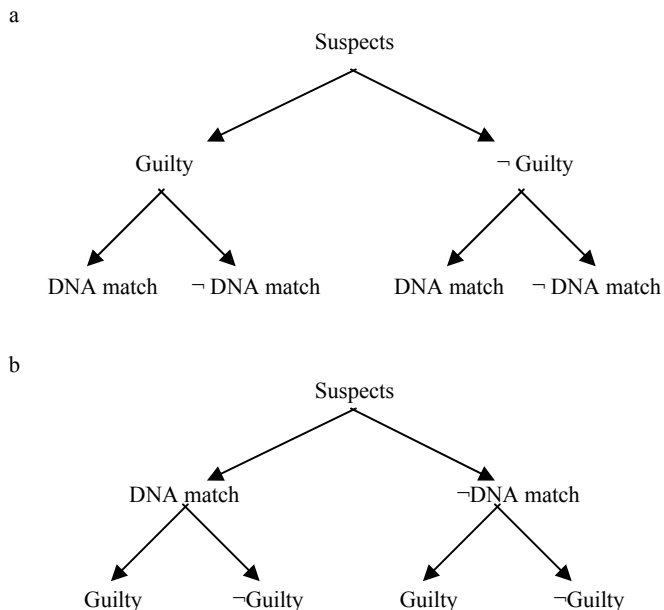


Figure 2. Two different ways of combining the frequency information of Guilty/-Guilty and DNA match/-DNA match, using natural sampling trees.

sample size). Given, as is usually done in such court situations, a suspect pool of people with motive, opportunity, and ability to commit the offense, a 1 in a million frequency of not guilty suspects having a DNA match could plausibly be considered by most people as “overwhelming” evidence of guilt (i.e., it is overwhelming because the estimated number of viable suspects is something less than a million). More to the point, Gigerenzer (1998) discussed at some length some of the specifics of how statistical information can be misunderstood in the legal arena along with ways to use natural frequencies to clarify those situations, and Hoffrage, Lindsey, Hertwig, & Gigerenzer, (2000) provided experimental results showing that natural frequencies lead to more accurate legal inferences and verdicts. In summary, the natural sampling approach is more information rich, more flexible, and more powerful than a mental-models approach. Nevertheless, Johnson-Laird and colleagues avoided using natural sampling, and favored instead the use of mental-models, even in situations that clearly would be better served by natural sampling. For instance, at one point, Johnson-Laird et al. (1999, p. 80), show a diagram to illustrate how a mental-model is difficult to construct for a particular problem:

	Probability		Conditional probability
disease	40%	symptom	75%
		¬symptom	25%
¬disease	60%	symptom	33%
		¬symptom	67%

This diagram is actually a close approximation of a natural sampling tree (Figure 1a). Because the data are presented as percentages, rather than frequencies, Johnson-Laird et al. (1999) note that “it is not obvious how to calculate the posterior probability” (p. 80). If one changes the data to frequencies (assuming a reference class of 100, because that information was already lost), the difficult mental model is now a clear and uncomplicated

natural sampling system from which the posterior probability can readily be calculated (Figure 1b). To gain the benefits of natural sampling without accepting the concept, Johnson-Laird et al. offered the subset principle, which specifies two possibilities: One rests on the problematic assumption of equiprobability (equiprobability – outside of casinos and laboratories – usually cannot be assumed; e.g., J.L. Gould & Marler, 1987), and the second is essentially a restatement of how to use natural sampling to derive a probability (in fact, the subset principle is actually identical to Equation 2 in Gigerenzer & Hoffrage, 1995).

The Role of Evolutionary Theory in Supporting Ecological Rationality

Johnson-Laird et al. (1999) also raised the issue of the proper role for evolutionary considerations in the fields of reasoning and decision making. They stated that:

Intuitions about evolution are an interesting heuristic for generating hypotheses about how the mind solves ill-posed problems, that is problems that would be insoluble without innate constraints on the process, such as the stereoptic recovery of depth information from disparate visual images (Marr, 1982). However, it is hard, if not impossible, to test intuitions about the mental processes of our evolutionary ancestors (Lewontin, 1990). Hence the claim that frequencies trigger an inductive module needs to be examined on its own merits. (p. 81)

The first sentence of the preceding quotation concerns the scope of applicability for evolutionary thinking, both more generally and presumably in the specific case of human statistical reasoning. The assertion is made that evolutionary considerations are relevant for “ill-posed problems,” and this raises a good point: what problems are ill-posed? More immediately, is statistical reasoning an ill-posed problem, thereby making evolutionary considerations relevant by common agreement?

Regarding the first question, the above passage might be taken to imply that ill-posed problems, and hence problems for which evolutionary considerations are significant, are a specific and severely limited class of problems (“such as the stereoptic recovery of depth information from disparate visual images”), thus placing a burden of proof on those who would like to consider evolutionary factors in psychology. This would be a mistake. As Herbert Simon (1973) pointed out, “It is not exaggerating much to say that there are no WSPs (well structured problems), only ISPs (ill structured problems) that have been formalized for problem solvers” (p. 186). So I agree with Johnson-Laird and colleagues; evolutionary considerations are relevant to ill-posed problems, which is to say, relevant to nearly all aspects of life.

I would go further to suggest that, although ill-posed problems may be particularly fertile areas to look for the influence of evolutionary history, this condition is not a necessary constraint. Some problems may be sufficiently well posed that they are computationally solvable in principle, but nevertheless are not realistically solvable for the computational machinery of the human mind. For example, there is the important issue in the real world of temporal constraints on human computational abilities (as when a person reacts to a looming object without the time to consciously compute the appropriate reaction). For this reason, mechanisms can be designed by evolution to take advantage of

situations in which there have been statistical regularities across evolutionary history. Roger Shepard (1984, 1992) has pointed out the existence of these “ecological constraints” in the field of perception:

The objects that have been important to us over evolutionary history have been informationally complex (requiring vast numbers of degrees of freedom for their characterization) and, furthermore, have changed over the eons. In contrast, the rigid displacements of those objects have been constrained for all time to the same six degrees of freedom. (Shepard, 1984, p. 441)

The second, more specific question was if statistical reasoning problems are generally ill-posed, thus making evolutionary considerations relevant in this specific domain. Yes, Bayesian reasoning involves ill-posed problems, because the proper use and interpretation of probabilities is ill-defined across many contexts. This very point is made several times in Johnson-Laird et al. (1999): (a) in the introductory paragraph (p. 62); (b) where the correct answer to the “three prisoners problem,” is given, “granted certain plausible assumptions.” (In other words, the problem is ill-posed to some extent; p. 65); and (c) where the black marble-red marble problem is explicitly noted as being “ill-posed” (p. 70). Artificial problems, such as games with book bags and poker chips, may be well-posed, but Bayesian reasoning in the real world is ill-posed.

The second sentence of Johnson-Laird et al.’s (1999) consideration of evolutionary factors brings up a question regarding the extent of our knowledge of human evolutionary history. This is, indeed, a question that has been considered extensively within the evolutionary biology literature. Evolutionary approaches entail a consideration of the environment of evolutionary adaptation (EEA) for a particular trait. The EEA of an adaptation is a statistical composite of the adaptation-relevant properties of the ancestral environment encountered by members of ancestral populations, weighted by their frequency and fitness consequences, and averaged across the time that it impacted on ancestral fitness. Reconstructions of the EEA for a particular trait, as has been previously pointed out by others (Crawford, 1998; Tooby & Cosmides, 1990), is an inferential process. Some things about the EEA are very certain: there was a sun, there were seasons, animals ate, some plants were edible, some plants were inedible, there was gravity, women gave birth to babies, and so on. Other aspects of the EEA are derived from multiple, independent, and converging lines of evidence, such as studies of existing hunter-gatherer societies, archaeological evidence, and phylogenetic studies. Just as no physicist has actually seen a photon, no one has actually seen the EEA. Once a hypothesis has been formed (not basis of “intuitions” or “heuristics” but on a thoughtful consideration of the EEA), it is supported or not supported by research with modern humans. The focus – as with most psychologists – is still on understanding the mental process of modern humans. The evolutionary history of our species is used to suggest ways that human ancestral circumstances involved statistical regularities (constraints) and ill-posed problems that would have been well served by evolved default parameters.

Whereas it is, as a matter of time and space, true that we cannot test the mental processes of our evolutionary ancestors, we can and do test the mental processes of our fellow modern humans. Scientifically rigorous theorizing from an evolutionary perspective can and often has led to very nonintuitive psychological theories

(e.g., Cosmides, 1989; Cosmides & Tooby, 1992; Silverman & Eales, 1992) as well as clear and exact models (e.g., the theory of kin selection, Hamilton, 1964, as applied within psychology; e.g., Burnstein, Crandall, Kitiyama, 1994; Smith, Kish, & Crawford, 1987). It is unfortunate that Lewontin’s criticisms of adaptationist (i.e., evolutionary) approaches in the social sciences (e.g., S. J. Gould & Lewontin, 1981; Lewontin, 1990) may apparently lead some readers astray in thinking that this is a valid objection. It should be noted, at the very least, that a number of people have already spent significant amounts of time pointing out the problems with the views espoused by both Lewontin and S. J. Gould (e.g., Borgia, 1994; Buss, Haselton, Shackelford, Bleske & Wakefield, 1998; Queller, 1995; Tooby & Cosmides, 1992).

On Mental-models and Ecological Rationality

Having two competing conceptual frameworks entails not only critical appraisals of alternative views, but also defense of one’s own position. Recognizing this reciprocal requirement, Johnson-Laird et al. (1999) turn to their final issue:

A separate question is whether problems that concern unique events guarantee bad Bayesian reasoning. In fact... reasoners *can* infer posterior probabilities for unique events provided that the probabilities are stated in simple numerical terms, such as ‘3 chances out of 5,’ and that they allow easy numerical calculation in the use of the subset principle (p.81)

It seems suspicious to say that subjects are truly reasoning about unique events, not utilizing cognitive mechanisms designed for dealing with frequencies and not using natural sampling systems, when the probabilities are stated as de facto frequencies (i.e., “3 chances out of 5”) and the proposed subset principle is de facto natural sampling. It can just as easily be argued that this format yields better Bayesian reasoning because it does manage to tap into a form of natural frequency representation. Similarly, the paper-and-pencil exercise of constructing models, was done throughout the Johnson-Laird et al. (1999) article, is helpful in dealing with these problems because the models are again de facto frequencies (e.g., the relative chances of 0.25, 0.25, and 0.50 were modeled as 2, 2, and 4 out of 8 total models; p. 83). The cases used in Girotto and Gonzalez (2000) were even less able to sustain the illusion of being single-event probabilities rather than frequencies. For example:

Mary is tested now [for disease H]. Out of the entire 10 chances, Mary has ___ chances of showing the symptom E; among these chances, ___ chances will be associated with the disease H. (p. 274)

How many times was Mary tested? Once or ten times? If tested once, there is one “chance” for a result (about which we could discuss subjective confidences, but that is a different issue). If tested 10 times, then this is an example of frequency information.

A few years ago, Cosmides & Tooby (1996) pointed out a way to relate a mental-models approach with frequentist reasoning:

In his mental-models theory, Johnson-Laird (1983) has suggested that people also solve syllogistic reasoning problems by representing category information in the form of discrete individuals. Moreover, he has claimed that syllogistic problems in which the category information is represented as discrete countable individuals are easier to solve than problems using representations that map finite sets of individuals

into infinite and continuous sets of points, as in a Venn diagram. This is similar to our claim about bayesian problems. Indeed, both syllogistic and bayesian problems require one to understand the overlapping relationships among different categories of information – for example, people who have a disease, people who test positive for it, and people who are healthy. It may be that the same set of manipulation procedures underlie both kinds of reasoning, and that these procedures require representations of discrete individuals to map the relationships among categories properly. On this view, the distinction between inductive and deductive reasoning would begin to dissolve at the mechanism level (although not at the computational theory level). (p. 61)

Although Cosmides and Tooby (1996) was cited by Johnson-Laird et al. (1999), and this explicit discussion of mental models is clearly and directly relevant, the later did not address this suggestion. One is left with the perception that this proposal was not satisfactory, although it is not clear why. It would seem that the mental-models theory could benefit from some clarification as to the actual nature of the mental representations it posits exist in the human mind (Rips, 1986, 1995).

One of the common results of competing theoretical frameworks is that the final outcome is not the extreme version of either position. Rather, the eventual conclusion is a more accurate (and perhaps more complex) understanding of the research area that combines aspects of both positions. As a summary, and in the spirit of such conciliation, I pose the following question: If the mental-models theory of extensional reasoning looks like it uses natural frequencies (e.g., in many of the numerical tags in actual models, and expressed as n chances out of N possibilities), and it acts like a version of natural sampling (i.e., using the subset principle), yet has no corroboration from evolutionary theory, why is it still necessary to posit it as a different and distinct computational theory of statistical reasoning?

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