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Editorial

It is with great pleasure that I introduce this issue of The Reasoner which features my interview with Philip Jonhson-Laird. A renowned expert of the psychology of reasoning, Fellow of the British Academy and of the Royal Society, Philip is Emeritus Professor at the Department of Psychology, Princeton University and Visiting Professor at the Department of Psychology at NYU.

Many readers of The Reasoner will at some point have come across his 1983 book *Mental Models: Toward a Cognitive Science of Language, Inference and Consciousness*. I can imagine how impatient they are to know how it all begun, so without further ado I leave you with the interview, not before thanking very warmly Phil for his time and enthusiasm.

Hykel Hosni
University of Milan

Features

Interview with Philip Jonhson-Laird

HH : Can you tell our readers what led you to study psychology?

At school, my passion was music, and one puzzle perplexed me: what makes chords dissonant? My father and grandfather had both been professional musicians. But, I left school at the age of 15, worked for five years as a quantity surveyor (don't ask!).

HH : OK, I won't . . .

As followers of Bertrand Russell's Committee of 100, my wife Mo and I were convicted for blocking the traffic down Whitehall, along with a couple of hundred others as a protest against the UK having nuclear weapons. For the same reason, I refused to do my military service, and had to work as a hospital porter. The need for an interesting job became urgent, and an obvious path was to go to University. What to study? Something leading to a stimulating career, and something for which I didn't need science exams at A-level, because I had to study on my own. After some research, I chose to study psychology – perhaps one of the few rational decisions I ever made, because one could do a BA in the subject, and it promised all sorts of jobs that looked appealing, from ergonomics to psychoanalysis.

HH : How did you become interested in reasoning?

One of my A-level subjects was logic, which I passed from reading Cohen and Nagel's excellent textbook. It treated quantifiers in a simple set-theoretic way. At University College London, one intellectual influence was A.R. Jonckheere, who worked every summer with Piaget in Geneva, and who remarked one day that the task for psychologists was to discover which logic was in the mind and how it was formulated there. Another influence was Peter Wason, who had been at the Harvard Center for Cognitive Studies, co-founded by Jerry Bruner and George A. Miller, where Chomsky's ideas had inspired the renewed study of psycholinguistics. In logic, my subsidiary subject, the lecturers were Bernard Williams, the brilliant moral philosopher, and A.H. Basson, for advanced logic. Basson lectured without notes, and gave his lecture on the deduction theorem two weeks in a row – he was twenty minutes into it before we, the handful of students in the audience, realized his mistake. No-one said anything. John Downing was my devastating but friendly tutor, to whom I read my essays on causality and other topics.

HH : You mentioned Peter Wason . . .

PJ-L : Yes indeed. Wason became my PhD adviser, and it took me years to understand the originality of his genius. But, he kindly ushered me through my thesis in two years. After my oral exam, an examiner remarked, "Now, you can do some real research". It was 1967, there were plenty of academic jobs, and so – without ever having aimed for it, I became a lecturer in psychology. My brother, Andy, became a computer programmer for a company that manufactured mainframes – the only sort of computer that existed then, and together we wrote a program that simulated rats running in a T-maze. But, what provoked my quest to understand reasoning was Wason's now well-known "selection" task.

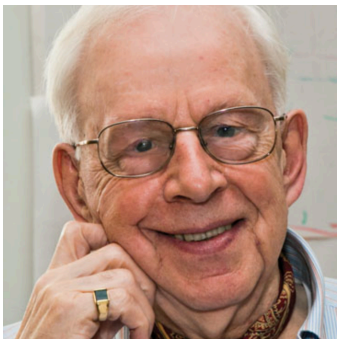
HH : Can you brief our readers on the problem?

PJ-L : Four cards are put in front of you, such as A, B, 2, and 3, and you know that each of them has a number on one side and a letter on the other side. Your task is to select just those cards, which if turned over, would reveal the truth or falsity of the hypothesis, *If a card has an A on one side then it has a 2 on the other side*. Wason had made the striking discovery that most people failed to select the 3 card; yet, the combination of A and 3 refuted the hypothesis. This finding piqued my interest.

HH : Where did the research on the selection task lead?

PJ-L : The neglect of a putative piece of falsifying evidence (the 3 card) led philosophers, such as L.J. Cohen, to criticize the task in defence of human rationality. Wason and I got a three-year grant to study why people erred. We discovered two main phenomena. First, a change in contents led people to make more rational selections. As Paolo and Maria Legrenzi showed, they did very well with a deontic postal regulation. Second, if individuals had a chance to find out the consequences of their selections in a repeated version of the task, just about all of them soon started to make rational selections. I devised an algorithm to explain people's selections – it was perhaps the first "dual" process theory of reasoning, in which intuitions led to a focus on examples of a hypothesis whereas deliberations shifted the focus to counterexamples. As you may know, psychology is plagued by failures to replicate results. But, over fourteen thousand people have carried out the selection task, and its results are robust. My three years of living with the task lasted me for a lifetime – at least until last year, when my colleagues Marco Ragni and Ilir Kola showed that the dual-process algorithm gave an almost optimal fit to the results.

HH : You also did research on lexical semantics with George Miller, can you tell us briefly about it?



PJ-L : Miller invited me to visit the Institute for Advanced Study in Princeton. My Ph.D. has been on psycholinguistics, he had just completed a study of verbs of motion, and so we began a paper on the meanings of words. The paper morphed into a book. What it argued was that the meanings of words cannot be reduced to purely perceptual predicates (pace Carnap), that the mental lexicon is based on semantic fields often founded on a single semantic primitive, and that various other concepts, such as space, time, possibility, and intention, occur in many different semantic fields. The core concept of ownership, for example, cannot be reduced to perceptions, because it has a moral component (as Locke had argued). The concept of causation, which we reduced to sets of possibilities, occurs in all semantic fields. (Sunny Khemlani, Cristina Quelhas, and I are now pursuing the idea that possibilities underlie, not just causality, but all reasoning.) The weakness of the book was that we never implemented its theory computationally. After five years' work on it, we had neither energy to do so nor access to a computer. There were none at the Institute in the early 1970s.

HH : I guess most of our readers will readily associate you with "mental models". Can you tell us how it all begun?

PJ-L : Not long after my return from Princeton, Stuart Sutherland persuaded me to move to Sussex University. The work with Miller had suggested that when individuals understood an assertion, they constructed a model of the situation to which it referred. Likewise, studies of syllogistic reasoning, with Janellen Huttenlocher, implied that they reasoned from models akin to the set-theoretic ideas in Cohen and Nagel's text. I had read Kenneth Craik's remarkable book, *The Nature of Explanation*, in which he talked of people constructing small-scale models of the world in order to make sensible decisions. But, Craik had argued that reasoning depends on verbal rules. Indeed, psychologists at that time accepted the view, as presaged in Jonckheere's remark above that reasoning depends on formal rules of inference akin to those of logic. My experience with the selection task made me skeptical. It would be a singular coincidence if the vagaries of human reasoning were based on the predicate calculus. So, with Mark Steedman's help, I learned my first list-processing language, POP-2, and wrote a program that used models in order to draw its own conclusions from syllogistic premises. Once again, it accounted both for the systematic errors that individuals made and, with a search for counterexamples, for their valid conclusions. Very few people can make a correct inference from premises such as:

- None of the painters is a cubist.
- All of the cubists are sculptors.

Some people infer that none of the painters is a sculptor, some infer its converse; some infer that some of the painters are

not sculptors, and some infer that nothing follows from the premises. A valid inference, however, is:

- At least some of the sculptors are not painters.

HH : What happened next?

PJ-L : The Social Science Research Council – Mrs. Thatcher later insisted that it dropped "science" from its title, and so it's now known as the Economic and Social Research Council – gave me a grant that paid my salary for two years. It enabled me to write a book, *Mental Models*, which brought together models as representations of discourse and models as underlying reasoning. It was well received except by proponents of logic as the basis of human deductions. Even Chomsky quite liked it, apart from its use of phrase-structure grammars of the sort that Stan Peters and Gerald Gazdar advocated, and of the compositionality of meanings à la Montague. The big hole in the book was a plausible account of sentential reasoning. When Thatcher cut the funding of universities – and Sussex by 20%, I found myself writing a personal cheque to pay for animal feed for the Lab. I needed to escape, and an opening came up at the MRC Applied Psychology Unit in Cambridge. (Thanks to Alan Baddeley!) Ruth Byrne came to work with me there, and together we filled the hole in the theory, showing how models could represent conditionals and disjunctions in a way that accounted for robust results.

HH : In what ways are mental models incompatible with logic?

PJ-L : You can make them compatible with logic (as Philipp Koralus has done). But, there are many ways in which everyday reasoning diverges from logic. In logic, infinitely many different conclusions follow validly from any set of premises; people often assert that "nothing follows". In logic, any conclusion whatsoever follows from a contradiction. My favorite example is Russell's riposte to a person who said, "Well, if so, prove from 1 + 1 = 1 that I am the Pope." Russell replied at once: "You are one, and the Pope is one, but one plus one equals one, so you and the Pope are one." Of course, that isn't the real reason, *ma se non e vero è ben trovato*.

HH : Classical truth-functionality is often another issue ...

PJ-L : Indeed! The truth-functional account of compound assertions doesn't work, either. Consider the inference:

- It is not the case that if the Christian God exists then atheism is correct.
- Therefore, the Christian God exists.

The premise is true and, given that its embedded conditional is a material implication, the inference is valid, and so its conclusion is true too. A short sound proof that God exists! In modal logics, the notion of "possibility" differs from its everyday sense. Most people infer, as Marco Ragni has shown:

- It's possible that Trump will be impeached.
- So, it's possible that he won't be.

The inference is invalid in modal logics.

HH : Of course. So, what would you say to either classical or non-classical logicians to convince them that mental models are on the right lines?

PJ-L : Suppose that only one of these three premises is true:

- Pence or Pompeo, or both of them leaked.
- Pence or Sessions, or both of them leaked.
- Kushner leaked.

Does it follow that Pence could have leaked? Logicians should say, "No". So, why, then, do most people say, "Yes"? Mental models predict the answer, and a computer program implementing the theory led to the discovery of these illusory inferences. Mental models represent only what's true, and so people envisage what holds if the first premise is true, what holds if the second premise is true, and what holds if the third premise is true. In two of these cases Pence is potential leaker. So, that's why people think he could have leaked.

HH : On behalf of non-logicians, what's wrong with that?

PJ-L : Only one premise is true. So, if the first premise is true then the other two premises are false, and the falsity of the second premise implies that neither Pence nor Sessions leaked. If the second premise is true then the other two premises are false, and the falsity of the first premise implies that neither Pence nor Pompeo leaked. If the third premise is true, then the other two premises are true, so neither Pence nor Sessions nor Pompeo leaked. So, in any case, the leaker can't be Pence. Didn't you know that he was an Eagle scout, and scouts are trustworthy?

HH : One fallacy doesn't make a theory, though, does it?

PJ-L : No, but such illusory inferences occur in all the main domains of reasoning. Here's another sort that concerns blood relatives:

- Ann is related to Beth.
- Beth is related to Chuck.
- Is Ann related to Chuck?

As Geoff Goodwin showed, most people say, "yes". They think of siblings or descendants, and overlook a counterexample: Ann and Chuck are Beth's parents.

HH : Does the theory apply to reasoning other than deductions?

PJ-L : Yes. For instance, it explains how individuals induce informal algorithms that are recursive. As Monica Bucciarelli, Robert Mackiewicz, and Sunny Khemlani have shown, it applies to algorithms – containing while-loops – that 10 year-old children formulate for rearranging the order of the cars in a train of arbitrary length. It is on a railway track equivalent to an automaton with two stacks. The kids are not allowed to touch the cars: as their gestures corroborate, they use a kinematic mental model to simulate the sequence of moves.

HH : How about probabilistic reasoning?

PJ-L : The idea that probabilities enter into reasoning is quite popular at the moment: theorists want to replace logic with the probability calculus to account for reasoning. I'm skeptical. People are unlikely to adduce probabilities in reasoning unless the task itself suggests that they do. Two pilots asked me to adjudicate a disagreement between them. They were arguing about the likelihood that both engines of a twin-engined plane failed. The pilot who flew jets in Vietnam said: double the probability of one engine failing, whereas the light-plane pilot said: halve it. In a spirit of compromise, I told them that they were both wrong. What this anecdote confirms is that people don't know how to calculate the probability of a conjunction of two events. The model theory predicts that when

the two probabilities differ they take the average. Sunny Khemlani and Max Lotstein corroborated this procedure, and similar ones for disjunctions and conditional probabilities. We are all duffers about probabilities until we have mastered the calculus, and most of us remain so afterwards. You're not a duffer, if you can answer this question: what three probabilities fix the complete joint probability distribution for two events, and no matter what numerical values you guess for them, its sum is always 100%?

HH : Did you ever research anything other than human reasoning?

PJ-L : Oh, yes. When my weekday job was quantity surveyor, my weekend job was jazz pianist. That experience helped me many years later to develop an algorithmic theory of creativity, and to model it in a program that improvizes jazz. What makes chords dissonant turned out to be the oldest problem in cognitive science: Pythagoras circa 500BCE proposed a geometric explanation. My solution only took fifty years to formulate. My friend Keith Oatley and I developed a theory of emotions, and we have published about a dozen papers on the topic. Emotions, we argue, are a cut-price version of rationality, evolving first in social mammals. They are a guide towards appropriate actions, and you can't control your feelings. Thanks to Plato many people think of emotions as inimical to reason. Mental illnesses (not psychoses) are emotional disorders. And their best treatment, so far, is cognitive therapy, which presupposes that their cause is faulty reasoning. But, Francesco Mancini and Amelia Gangemi, both cognitive therapists, have shown that patients with mental illnesses reason better than control participants, but only about matters pertaining to their illnesses. ... We seem to have arrived back at reasoning.

HH: Never mind, you're talking to the right audience! And since many readers have an interest in algorithmic reasoning, can you tell us why you think it's important to build computer models?

PJL: Psychology is infected by theories that take too much for granted. The flow of thought moves through a series of planes (Vygotsky). It depends on structural operations of centering (Wertheimer), and of equilibration, which is a compensation that annuls a transformation (Piaget). Such accounts seem little more than crutches on which these great psychologists lean in order to point the way. But, a theory expressed in a computer program ... well, it's not likely to be taking too much for granted. And, as in the discovery of illusory inferences, it may lead to unexpected consequences of a theory. The actual process of devising a program can even lead to ideas about how to test the theory. As Mark Steedman told me years ago: "You should do your own programming." The other day a reviewer in rejecting a paper of mine asked, "what's the point of the computer program?" So, my case for programming, which goes back to Mark's advice, has been about as effective as the case for abstinence as a method of contraception.

HH: A final question: any advice to students just starting their careers?

PJ-L: Three tips. There is no one right way to do research: people differ. Everyone gets papers rejected, so, unless you think the reviews were right, keep submitting until a journal accepts your latest effusion. My personal worst: six journals until acceptance. Only your research matters; so stop doing it if you don't enjoy it.