Will the Next Kepler Be a Computer?

John H. Holland, Keith J. Holyoak, Richard E. Nisbett, and Paul R. Thagard, *Induction: Processes of Inference, Learning and Discovery.* Cambridge: MIT Press, 1986, 384 pp. $24.95.

Pat Langley, Herbert A. Simon, Gary L. Bradshaw, and Jan M. Zytkow, *Scientific Discovery: Computational Explorations of the Creative Processes.* Cambridge: MIT Press, 1987, 357 pp. $9.95.

Both of these books are attempts to show how inductive reasoning can be modeled on computers. *Scientific Discovery* focuses on examples drawn from the history of science, though the discussion of historical cases is brief and very limited. *Induction* discusses inductive reasoning across a wide range of phenomena, including learning, attributions of cause in social situations and, in a next-to-final chapter, scientific discovery. Neither book discusses the role of social factors in discovery. Therefore, both are particularly relevant to those interested in the cognitive psychology of science.

Scientific Discovery

This book focuses on heuristics as the main ingredient in scientific dicovery. Heuristics are "rules of thumb" that guide the search for a solution to a problem. Most of the book is spent describing heuristic-based computer programs designed to model famous scientific discoveries. For example, a program called BACON was designed to re-discover several scientific laws from data, including Kepler's third law. The earliest version of BACON used three heuristics to find Kepler's third law:

> If the values of a term are constant, then infer that the term always has that value.If the values of two numerical terms increase together, then consider their ratio.If the values of one term increase as those of another decrease, consider their product. (p. 66)

Later versions of BACON added heuristics to increase generality, e.g., ones to express the ratio in the second heuristic as a linear function and check that function by gathering further data. BACON is given data on the distance between the sun and each planet and the angle found by using a fixed star and the planet as two endpoints with the Sun as the pivot at different times. From this data, BACON finds an inverse relation between the distance (D) and the slope (s) which it eventually uses to discover that D^3s^2 is a constant.

Then the program stops, and Langley et al. claim that it has "discovered" Kepler's third law, with a simplified version of his second law embedded within. To anyone familiar at all with Kepler's actual discovery process (see Koestler, 1959 for a good account) the program falls ludicrously short. No one presented Kepler with a table of data, neatly arranged, and told him to discover an arbitrary mathematical relationship. Kepler started with a belief that geometric solids could be fitted into the orbits of the planets with the sun at the center of the solar system. When this belief did not accord with Tycho Brahe's data, he had to go through the agonizing process of altering his framework, deciding which data were worth using to frame and check his new model. His success was due, in part, to persistent calculations based on Brahe's data, but no one else would have known that such calculations were worth doing. As Koestler says,

> He had been searching for this Third Law, that is to say, for a correlation between a planet's period and its distance, since his youth. Without such a correlation, the universe would make no sense to him; it would be an arbitrary structure. If the sun had the power to govern the planet's motions, then that motion must somehow depend on their distance from the sun; but how? Kepler was the first who saw the problem—quite apart from the fact that he found the solution to it, after twenty-two years of labour. The reason why nobody before him had asked the question is that nobody had thought of cosmological problems in terms of actual physical forces. (p. 395)

To put this in the language of modern cognitive science, Kepler's mental representation of the solar system was more important than the heuristics he used to discover his laws. (For a similar argument regarding the performance of subjects in scientific reasoning experiments, see Gorman, Stafford, and Gorman, 1987.) BACON has no mental representations; it merely searches for quantitative relationships between arbitrary terms supplied by the programmers, who interpret its results to be "discovery" of Kepler's third law. One feels they have missed the point of Kepler's genius entirely.

There are several points in *Scientific Discovery* where Langley et al. issue disclaimers, pointing out that programs like BACON—and the book includes many other programs designed to model historical discoveries from different sciences—were not intended to model the history of particular discoveries. Rather, these programs present "ways in which historical discoveries could have been made with the help of a few simple heuristics and with moderate amounts of computation" (p. 170). But this disclaimer does not answer the problem raised in the Kepler example. There is no way Kepler could have found his laws "with a few simple heuristics"; he had to represent the solar system in a way that suggested where to look for lawful relations.

In a final chapter, the authors attempt to address this problem of mental representations, including pictorial images. Their claim is that the same sort of programs that produced Kepler's third law could be adapted to form representations. But they regard these sorts of representations as relatively simple (see p. 318) and also talk very little about how a machine could form them from the kind of information and belifs Kepler had available. Certainly we could find a way to program a computer to begin with the assumption that the planets' orbits can be filled by geometric solids, then provide it with data that shows the inadequacy of that view and heuristics that would guide it to Kepler's solution. But this isn't what Kepler did. He had to decide that geometric solids must fit into the planetary orbits, figure out how to test this assumption, and develop radical new assumptions, e.g., the idea that some kind of force emanating from the sun controlled the motion of the planets. Historians and philosophers may find some of Langley et al.'s other cases of greater interest than their account of Kepler. But there is no danger that their programs, even if modified to create representations of the form they suggest toward the end of the book, will be able to make novel discoveries of the sort that Kepler made.

Langley et al. close by arguing that scientific creativity should not be regarded as mysterious, as fundamentally different from other human cognitive processes. I agree—we need to try to understand the cognitive aspects of scientific and technological creativity. But it seems to me that their account of scientific problem-solving is as limited as early behavioristic accounts of learning—a necessary and halting first step, perhaps, but one which we must go beyond as rapidly as possible.

Induction

Fortunately, more promising approaches are already at hand, and *Induction* represents one of them. Although it has only one chapter on scientific discovery, it is of far more interest to psychologists of science than *Scientific Discovery.* Whereas most of the latter book is detailed description of computer programs, the former spends only one chapter describing the authors' computer implmentation of their model and the rest applying the model to important areas of psychology, including scientific discovery.

Induction's approach relies far more heavily on mental representations than heuristics. Holland et al. try to demonstrate that mental representations are best viewed as sets of rules, including rules for transitions from one state to another. They use the example of "Jennifer" (p. 58-62), who notices that an "object in her visual field is small and black, has a long axis in the horizontal direction, and is an animal" (p. 59). Then they show how her "system" computes values for several mutually exclusive alternatives, e.g., black dog, black cat, and black squirrel; dog gets the highest value, but "the tentative characterization . . . does not reach some implicit confirmation threshold" (p. 60). So the system tells Jennifer to move her eyes "along the animal's horizontal axis," which leads to the observation that the animal has a round head. This new information adds strength to the squirrel and cat categories; cat passes the "confirmation threshold" and Jennifer realizes she is looking at a cat. One of the strengths of *Induction* is that its framework allows for competing rules or representations, whereas the programs in *Scientific Discovery* can consider only one rule at a time.

Sociobiologists hold that each organism acts as if it were a sort of "genetic calculator," calculating whether kin are sufficiently close to warrant sacrificing its own life to save their (Dawkins, 1976). Similarly, Holland et al. construct a model of thinking based on rules which involve complex calculations. Unlike the sociobiologists, there is no "as if" involved in Holland et al.'s analysis; they assume that these sorts of calculations are being carried on by a computational cognitive system. Lest the analogy seem too far-fetched, it should be pointed out that Holland et al. mention biological systems as one area in which the mathematics of induction should have its greatest application. Critics of the whole computational perspective would argue that this mathematical level of analysis is unnecessary (see, for example, Searle, 1984). For the present, it is best to say that Holland et al. provide substantial evidence that in many situations the human or organism acts *as if* it were doing computational calculations before recognizing or acting.

Holland et al.'s more sophisticated computational model comes a step closer to Kepler than BACON's simple heuristics. In fact, Holland et al. use the Kepler example to criticize BACON along the lines I sketched above (pp. 324-5—but I developed my critique independently of theirs). They also present a more sophisticated philosophy of science than *Scientific Discovery,* due in part to the fact that one of the co-authors, Thagard, is a philosopher. A quotation will illustrate:

> How do scientists arrive at new laws? A common view, misattributed to Francis Bacon, is that scientists generally start with the data and then by "induction" directly derive laws that describe those data. More typically, we will argue, the discovery of laws requires the kinds of reconceptualizations that are central to our account of problem solving; synchroic search among concepts is as important as the search for diachronic realities. We will argue that this is true even for observational laws, or laws that cover observable events; it is even more obviously so for theoretical laws that require top-down concept-formation. (p. 323)

In other words, theory formation involves searching for relationships among concepts as well as search for regularities in the data. Holland et al. incorporate both types of search in their model and they recognize that, "Although he started with relatively accurate data furnished by Tycho Brahe, Kepler's discovery of the three famous laws would have been impossible without several dramatic reconceptualizations of the problem domain" (p. 324). The amount of reconceptualization is far greater for a Newton or an Einstein; Newton, for example, took Kepler's and Galileo's laws and forged them into a single system.

Holland et al. don't say how exactly their model would explain Kepler's discoveries, but clearly numerical computations concerning the relative strength of various rules would be involved on some level. For example, Kepler's system of geometric solids might be represented as a set of rules; the system would then seek data, e.g., Brahe's, to see if the geometric-solids model reached a "confirmation threshold." Inconsistencies between the data and the model would lead to rules that embodied exceptions and also to reconceptualizations until Kepler's set of rules gradually yielded his three laws.

Perhaps Kepler did sit in his study poring over Brahe's data while his computational system assigned weights to different rules, then sent him back to look for more data. Certainly, subconscious or semi-conscious processes of this sort do go on in scientists—and all of us—much of the time. But this model still misses the heart of what it meant to be Kepler. While these calculcations may be relatively automatic in a case where a child perceives a cat in the distance (although even that is debatable), they are not automatic in a case like Kepler's, where a scientist feels he or she has seen through Nature's veils and is trying to transform the ecstasy of discovery into sober fact. He did not compute passively; he made conscious decisions about where to seek information and what to do with it. The program outlined in *Induction* cannot be a Kepler until it develops metacognitive awareness—until it can consciously evaluate alternatives and not just automatically perform calculations.

For example, Kepler's decision to abandon the Copernican assumption that planets had circular orbits was not a simple, rational response to calculations based on the evidence that Mars' orbit could not be a circle. Instead, Kepler had to cope with a result that was in drastic conflict with his representation of the way the universe worked: the orbit of Mars was an oval.

> The oval ... has an arbitrary form. It distorts the eternal dream of the harmony of the spheres, which lay at the origin of the whole quest. Who art thou, Johan Kepler, to destroy divine symmetry? All he has to say in his own defence is, that having cleared the stable of astronomy of cycles and spirals, he left behind him "only a single cart-ful of dung: his oval." (Koestler, p. 329)

To understand Kepler, one must do more than construct a complex set of competing rules. One must understand how his emotional reaction to the "cart-ful of dung" drove him on to a year of fruitless calculations, lead him even to repudiate his own second law—all while using ellipses in his calculations without realizing that the ellipse was the key to the orbit of Mars. I don't see how even Holland et al.'s computer program could replicate this "wild goose chase" unless it were programmed in advance to follow these exact steps, in which case it would be modeling nothing but the programmer's ingenuity in setting up the steps.

Conclusion

From *Scientific Discovery*, we learn the strengths and limitations of a heuristic-based approach that virtually ignores mental representations. From *Induction*, we learn the prospects and problems associated with translating mental representations into sets of rules. My own view (see Gorman, 1987) is that a complete view of the creative process will have to include both mental representations and heuristics; a unique mental representation is a major ingredient in scientific genius, but creative scientists also utilize or develop powerful heuristics—including some of the ones discussed by Langley et al.—that allow them to test and modify their representations.

Like many congitive scientists, the authors of both books are working up from lower-order cognitive processes, e.g., in *Inducation*, how you recognize a cat in the distance and in *Scientific Discovery*, how you perceive relationships in data already ordered so as to make the discovery of the relationships easy. What the psychology of science needs is more cognitive psychology of higher-order processes. (Holland et al. briefly recognize this problem on p. 350.)

Specifically, both books ignore the problem of consciousness, or what Searle (1984) calls intentionality. Cognitive scientists are not alone in their failure to adequately address this issue; sociologists and social historians, for example, frequently treat the individual as merely a product of social forces. Certainly, social factors play a major role in shaping an individual scientist's representations and choices of heuristic; Kepler's preference for circular orbits is, in part, a result of his times. However, Kepler is no more a mere product of social circumstances than he is a complex calculator or a set of heuristics. A successful psychology of science must incorporate consciousness as well as mental representations and heuristics. Computer models may be useful in other ways, but even the most sophisticated parallel-processing architectures do not incorporate consciousness.

One of the most hopeful aspects of the book *Induction* is that the authors come from different disciplines. Only through genuinely interdisciplinary collaboration can we hope to recapture the whole Kepler, including his conscious intentions as well as the subconscious social and cognitive factors that shaped his discoveries.

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