

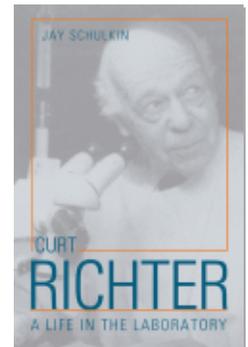


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Curt Richter

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Conclusion

Curt Richter envisioned himself and his work as part of the scientific enlightenment. In Richter's words, "We are participants in a great revolutionary movement." The contents of science matter for the human condition; science has practical consequences. Again in Richter's words, "We as scientists are beginning to hold the responsibility for the future in our own hands" (Richter 1953f, p. 91). Although Richter was not a public intellectual, certainly not in the sense of Meyer or Watson or Cannon, he did seem to understand his work in the context of the larger social milieu.

Richter assumed a broad view of biological adaptation. For him, the concept of instinct was a behavioral category for animals' orientation to the world in which problem solving takes place. He thought, as he would always say, with his hands, with instruments of measurement. Richter exemplified a research program richly conceived within regulatory physiology and biological adaptation.

An important conference that Richter attended toward the end of the 1950s provides a context for Richter's insistence on staying focused on the concrete, practical application of scientific inquiry while remaining apart from the intellectual and theoretical debates of his day.

AN INTERNATIONAL CONFERENCE ON INSTINCTIVE BEHAVIOR

A conference on the concept of instinct and metabolism was organized by French scientists and held in France a little more than a decade after World War II. This international conference brought together an interesting group of European and American investigators. Participants included Konrad Lorenz,

Daniel Lehrman, K. Von Frisch, T. C. Schneirla, and Desmond Morris, among others. This lively group submitted papers that resulted in the publication in French of *L'instinct dans le comportement des animaux et de l'homme*.

As I have indicated, instinct is a fundamental concept in psychobiology. It was also an important concept for Richter; one of his review articles, "Salt Appetite of Mammals: Its Dependence on Instinct and Metabolism," featured instinct as a central factor in salt appetite (Richter 1956d). The concept of instinct, in fact, pervades all of Richter's work.

At the conference, the stage was set for a confrontation between Lorenz and a young, vociferous critic of his work, Daniel Lehrman. Lehrman had earlier published "A Critique of Konrad Lorenz's Theory of Instinctive Behavior," in which he argued that Lorenz's notion of instinctive behavior, as innate releasing patterns of behavioral expression, was far too narrow. Moreover, Lehrman argued that innate constraints are not the same across species, and that the concept of instinct used to explain behavior was easily abused. Rather, there were variations in the degree to which behavior and responses to specific stimuli were fixed among different species. How much they varied, as well as how much they were a characteristic of a given species and its evolutionary ascent, had not been answered by ethology. Lehrman argued for greater plasticity at the level of the brain and behavior, which is certainly a feature of human beings (Lehrman 1953).

Lorenz, in his paper at the conference, presented "the objectivist theory of instinct" and defended his position against Lehrman, particularly his hydraulic model of the build-up of drive and its release to specific stimuli. Lehrman challenged Lorenz's ideas on specific points, particularly on issues of ontogeny (Lorenz 1956). Others also spoke out. There is no record of how Richter may have weighed in on this discussion.

Other papers presented at the conference were less theoretical. Although many of the speakers at the conference asked questions during the discussion, there is no record of comments from Richter. Finally, after a talk by Desmond Morris on "the function and causation of courtship ceremonies," Richter asked a question: "Is a surgical attack on a behavior problem possible in the fish, as it is in mammals, for instance, the rat? It would be of considerable importance to know how removal of the various endocrine glands would alter the behavior of your fish; or how isolated brain lesions would alter them. Do fish have a sufficiently high resistance to infections to permit surgical interferences to be made without endangering their lives?" (Grasse, p. 285).

Other papers followed, some of which were very empirical discussions of the physiological and behavioral regulation of different forms of adaptation. Many of these papers were in French or German. Recall that Richter understood German and could also speak it. A paper by Schneirla, "Interrelationships of the 'Innate' and the 'Acquired' in Instinctive Behavior," again emphasized ontogeny, as the American naturalists tended to study each species on its own ecological terms. Schneirla stated in his conclusion that he wanted to "retire" the concept of instinct from the scientific lexicon (Schneirla 1956). This conference is one of the few instances in which there was direct interaction between the American naturalists from the Museum of Natural History (Schneirla, Lehrman), and Lorenz, a German soldier from the war, distinguished naturalist, and eventual Nobel Prize winner and one of the grand promoters of the concept of instinct. This was not just about science; it was personal.

Interestingly, in Lehrman's essay "On the Organization of Maternal Behavior and the Problem of Instinct," he included a good deal of discussion on the hormonal regulation of maternal behavior (Lehrman 1956). In this paper Lehrman emphasized the interaction between hormonal regulation of behavior and experience and how experience modified behavior (*experience* here referred to a concept larger than that of associative learning).

This paper indicated that Lehrman was well acquainted with some of Richter's work; he cited both Richter and Kinder on the behavioral regulation of temperature through nest building. Lehrman also cited the work of Wada and Richter and that of Richter on stomach contractions and the regulation of activity and food intake. Lehrman's main focus, however, was Lorenz. Although one wonders what Richter thought of it all, there is no record of this. Perhaps he was right to stay out of the ideological debate. At the conference, Richter would stick to discussing one of his favorite topics: the appetite for salt.

Richter, appearing unmoved by the theoretical shouts at this conference, gave one of his best presentations on the innate mechanisms that underlie salt ingestion. He showed that salt appetite was dependent on two factors: instinct and metabolism. By *instinct* he meant innate behavioral expression, and by *metabolism* he meant the physiological mechanisms that generate behavior and preserve sodium balance.

The issue of innateness had long been settled for Richter. Although clearly he knew of the debates about the role of innate and learned behaviors and their interactions in ingestive behavior, as well as the ideological debates that plagued the use of the concept of instinct, he no doubt was happy not to

engage. Perhaps, although he never really asserted this, he would happily dissolve the distinction that some held between instinct and intelligence, the former blind and automatic and the latter adaptive and subject to trial and error (see Boakes 1984).

The argument Richter put forth was that “studies on salt appetite which we have carried out in my laboratory during the past 20 years illustrate many of the principles involved in the production and modification of other specific appetites” (Richter 1956d, p. 378). Although he may not have been right about this, it illustrated what Richter and others understood as instinctive or innate behavior. Most of us in the field, myself included, would suggest that sodium is unique; there are few hard-wired ingestive systems such as the appetite for sodium (see Rozin and Schulkin 1990). But Richter, who contributed so much toward understanding ingestive behavior and the appetite for sodium in particular, led with his best intellectual punch.

Richter closed his talk with a discussion of the contrast between wild and domestic rats’ regulatory and behavioral capacities. Even in this review of his work, Richter always stayed close to his data. He reveled in discussing his instruments and the rats’ behavioral choices. The rootedness of the experiments in Richter’s biological orientation to behavioral design was evident throughout the review. The one theoretical point that figured in his presentation was the concept of innate behavior, or what he called instinctive behavior.

In a conference packed with individuals who were theoretical by predilection and forthright in their criticism, Richter was methodological in his orientation (though grounded in the concept of instinct). After Richter noted an interesting observation about salt taste psychophysics and the putative role of the peripheral gustatory thresholds in several experiments, Lehrman asked Richter to comment on the psychophysical experiments demonstrating that rats that were not sodium hungry could discriminate salt as well as sodium-hungry rats could (e.g., Koh and Teitelbaum 1961). Lehrman cogently and, as it turned out, correctly asserted that “these data suggest that the effect of depletion may not be to changes in the sensitivity of the taste buds, but the role of salt as a reward.” He continued, “in general, would you not agree that the experiments you describe are designed only to elucidate the facts of diet selection and its relationship to nutritional needs, and that the problem of the physiological and psychological mechanisms involved is still to a considerable extent an open one?” Richter briefly referred Lehrman to his first experiments with gustatory thresholds, saying, “The problems of the mechanisms

underlying dietary self-selection are still unsettled, I quite agree.” But he also said, “I would not agree that our experiments are designed only to elucidate facts of dietary selection and their relation to nutritional needs—certainly we try whenever possible to get an answer to the more fundamental problems of the underlying mechanisms” (Richter 1956d, p. 630). Another individual at the conference, J. Haldane, also queried Richter, saying that “I share Mr. Lehrman’s skepticism on changing peripheral thresholds” (Richter 1956d, p. 630), as did a third attendee, H. Pierone (Richter 1956d, p. 631).

There were other queries after Richter’s talk. One participant mentioned studies on African Pygmies in the forest and “the craving for salt by certain populations” (Richter 1956d, p. 629). Other investigators (J. Haldane, M. Klein) addressed the social value of salt. K. Von Frisch mentioned studies on the appetite for fat, suggesting that it is quite different from the appetite for salt as a model of appetitive behavior (Von Frisch 1956). M. Fontaine asked Richter how adrenalectomy and giving deoxycorticosterone could result in the same enhancement of salt ingestion. To this Richter replied, “To my knowledge we do not have an adequate explanation of these apparently opposing results” (Richter 1956d, p. 631).

Lehrman was concerned about the abuse of the concept of instinct and its potential misuse in understanding human motivation, creating false and misleading analogies between animals and humans. But there was a larger worry that goes back to the reification of the natural state.

Lehrman argued that Lorenz followed a line of thought in which the “effects of domestication” resulted in “the involution or degeneration of species-specific behavior patterns and releaser mechanisms because of degenerate mutations” (Lehrman 1953, p. 354). Lehrman criticized Lorenz for his flight into Nazi mythology and its destructive consequences (but see also Rosenblatt 1995). Richter does not seem to have been part of this conversation.

The nature versus nurture controversy had an intellectual paralyzing or polarizing effect then, as it does now. Richter tended to see everything “in the genes,” even using the expression “the release of his genes” to depict his orientation. Richter believed that his work demonstrated the primacy of the biological point of view, but it was a laboratory, not a field, point of view.

World War II changed the intellectual landscape of the United States as well as of Europe. The concept of instinct, for some investigators, was tied to the eugenics espoused by the Nazis and their abuse of biological thought (Lehrman 1953). Many individuals were not entirely consistent in rejecting

the concept of instinct, or at least a narrow notion of it (e.g., Lehrman 1954; Rosenblatt 1995). Still, there was legitimate concern about the abuse of the concept of instinct for behavioral explanations and about its vagueness as a scientifically meaningful construct. Frank Beach, in an interesting review titled "The Descent of Instinct," outlined the history of the concept of instinct from prescientific times to the modern period. Beach argued that the focus should be on the development of behavior and the stimuli that facilitate the behavioral expression of genes (Beach 1955). He believed that researchers should stay close to the behavioral analysis. Of course, Richter would be sympathetic to this.

In the field of ethology, Lorenz embraced ideological battles while Niko Tinbergen, who won a Nobel Prize with Lorenz, continued to reinforce and integrate various behavioral findings. Tinbergen's view was dominated by a common conception of the buildup of instinctive energy and its discharge in response to external stimuli. The greater the buildup, the more likely the discharge. Tinbergen suggested that "there are close parallels between the mechanisms underlying locomotion and that underlie an instinctive act" (Tinbergen 1951/1969, p. 71; see also Lorenz 1981). The nervous systems orchestrate the priorities of behavioral options. Tinbergen detailed the "reproductive instinct, and their hierarchical organization of behavior." He went on to suggest that "the various instincts are not independent of each other" (Tinbergen 1951/1969, p. 111). Although there was much discussion of innate systems in Tinbergen's work, there was very little discussion of learning. But Tinbergen did talk about motivational systems, a hierarchical organization of behavior that underlies instinctive behaviors (see also Thorpe 1948). W. H. Thorpe defined instinctive behaviors as comprising three features: inherited, specific, and stereotyped patterns of behavior (Thorpe 1948).

Both the internal buildup of drive and the configuration of external eliciting stimuli came to dominate ethological depictions of instinct (Lorenz 1981). Tinbergen published his book *The Study of Instinct* several years before the conference that he and Richter attended. In the book, Tinbergen carefully carved out the conceptual and experimental contexts for understanding the concept of instinct. As Tinbergen saw it, the motivation, or drive, aspect of instinctive behaviors is hydraulic in nature. The expression of instincts reflects both appetitive and consummatory behaviors (see also Craig 1918), and these behavioral features are hard-wired. Tinbergen emphasized rule-governed behavioral responses to specific sensory information. Instinct was

embedded in this innate hardware, in terms of both the signals detected and the fixed action patterns emitted (Tinbergen 1951/1969).

Instinct and learning are inextricably linked, and although many ethologists emphasized fixed action patterns for the innate aspect of behaviors (Thorpe 1948) and motivational behaviors (Lashley 1938/1960; Stellar 1954), the concept of instinct can be tied to learning in which there is plasticity of expression (J. L. Gould 2002). An interesting array of prewired components are inherent in all behavior (Rozin 1976b). The question is how fixed and how flexible are the behavioral options? How easily can behavior be modified? We know from the strong link between gustatory information and visceral discomfort, for example, that stimuli are not of equal value (Garcia, Hankins, and Rusiniak 1974). In the concept of prepared learning, learning is steeped in innate factors and there is no rigid separation between what is innate and what is learned. Instinct is not on one side and learning on the other; many forms of learning are themselves instinctual.

Disparaging criticism of the stronghold of narrow behaviorism was leveled from many intellectual bastions, but perhaps the loudest and most ferocious came from ethologists. Lorenz and Tinbergen claimed that behaviorists “just don’t know animals.” This was a recurring theme in the autobiographical reminiscences of both Lorenz and Tinbergen (which, interestingly, appeared in the same volume as Richter’s reflections). Ethologists were out to study behavior in the wild or to simulate the conditions of natural adaptation in the laboratory. They thought that innate hardware and built-up hydraulic drives converged to respond to specific configurations of external stimuli. Spontaneous behavior reflected the activation of hormones on central states of the brain (e.g., on motivational systems that underlie hunger). This is, of course, similar to Richter’s ideas, minus his emphasis on biological clocks. There is little room for learning in this view. And that, I suggest, is a mistake, because innate predilection goes hand and hand with specialized forms of learning, adaptation, and multiple kinds of information processing, such as the phenomenon of human language (Pinker 1994).

Amid the raging debates on this concept of instinct and the legitimacy of psychology as a science, Richter went about generating experiments, taking the body apart and putting it back together again, and linking his research to broad-based behavioral and biological adaptation. He held to his data, his instruments, and his functionalistic explanations that bridged behavioral and physiological adaptation. He embraced Darwin, Bernard, and Cannon but

understood the study of behavior as an engineer discerns the mechanisms that underlie adaptation. He was not part of the psychologists' search for the learning that underlies behavior.

RICHTER: FEARLESS AND FREE

Drawing on material he had presented to a committee at the National Research Council, Richter published an article in *Science* that drew heavily on his philosophical side. In this article, he advanced his views on what he called "free research versus design research." Richter stated his position clearly: "There is a choice before us between free and design research, or as I see it, between supporting the man or the experimental design" (Richter 1953f, p. 91). He shunned the modern culture of science that had evolved after World War II, having never really adapted to it. This was the culture of writing grants, of rigorous design in laying out one's plans. For Richter, this culture thwarted the process of investigation. He said, "We pick out the more tangible part of the application—the experimental design—how the man plans to work out his project." He worried greatly that by focusing almost exclusively on experimental design, science would lose sight of the researcher, and he felt that good researchers were being undermined. "It is not the design of the research," he said, "but the person, the individual scientist and their track record; one grant proposal may not reveal as much as the individual will, or one committee will not be able to know where it fits and what it will amount to." He went on to say that we must have faith in scientists and must recognize their past accomplishments: "The researcher has come to play a less and less important part; comparatively little is known about his background, setting, facilities, his sincerity [and his] determination and ability to carry on independent research. He is gradually being reduced to the status of a technician who must follow out in detail a definite plan of research" (Richter 1953f). For Richter, the individual gave validity to science, to experiment, and to a laboratory reproduction of ideas (see also Shapin and Schaffer 1985).

As the *Science* article illustrates, Richter was not of the era of experimental design. In later years, science would come to be dominated by statistics and design, but Richter had none of this. He explored in science. Discovery, he rightly understood, was based in part on what Kant (1790/1951) called the "free play of the imagination" and what Peirce called "abduction," or hitting on the right idea (Hanson 1971). Richter described a state of mind that involved "puzzlement of discrepancies in findings." This was, in fact, one

condition for inquiry; inquiry often begins when discrepancies are noted (Peirce 1877). Modern learning theory is deeply tied to this view (Rescorla 1988). Richter also added another parenthetical remark, that “there are researchers who do not work on a verbal plane, who cannot put into words what they are doing” (Richter 1953f, p. 92). Richter, by his own account, thought not in terms of words or ideas, but instruments. He had what most interests investigators and artists: a capacity to play and to spend long periods puzzled, tinkering and discovering. There is no blueprint, no laid-out agenda in this approach. This is what he wanted protected and what he lamented as big science, in his view, lost track of the individual.

Richter captured a kernel of truth, but he was too steeped in a mythology that did not allow him to adapt to the burgeoning art of experimental design. In the *Science* article, he rightly recognized the importance of statistical design but asserted that it “should not substitute for ideas” (Richter 1953f, p. 92). Most of Richter’s contemporaries, whatever their theoretical orientation, did not absorb statistical methods. An important contribution from behavioral psychology would be the logic of experimental design (e.g., the work of Neal Miller and Robert Rescorla). Experimental design would come to figure in all aspects of the behavioral sciences, as would the use of statistical analysis.

When Richter published the *Science* article, he was several years from emeritus status. He would continue doing research for another twenty-five years, but he was already behind the times. He was fortunate to have been supported in the way that he was. For example, some of his support came from the Rockefeller Foundation (1923–40, via Adolf Meyer), the National Institutes of Health (1952–65), the National Science Foundation (1956–77), the National Research Council (1937–45), the National Council on Alcoholism (1959–60), and the Commonwealth Fund (1964–77).

Richter probably always sought to escape the endless debates taking place within academic psychology. He remained a steadfast experimentalist, one who perhaps exaggerated innate behavioral solutions. He focused on the practical side of knowledge, the clinic, and patients. In these contexts, the engineer in Richter could come forward, extending and simplifying devices to measure physiological events that could be used to discern disease and dysfunction. Even at the very end of his life, Richter published papers on growth hormone (1980) and cortisol secretion (1983) in rats, guinea pigs, and monkeys and remained productive, active, and engaged in his scientific pursuits.