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FROM BOTTLE-FED CHIMP TO BOTTLENOSE DOLPHIN: A CONTEMPORARY APPRAISAL OF WINTHROP KELLOGG

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This paper details the life and professional career of Winthrop Niles Kellogg (1898-1972). While the diversity of his research is noted, special attention is paid to three major projects: the ape and child study, the work on learning and conditioning, and the porpoise research. Kellogg's work and his place in the history of psychology are assessed in terms of the contrast between American animal psychology and European ethology. It is concluded that Kellogg's research, a blend of both traditions, was a forerunner of contemporary animal-behavior science.

Winthrop Niles Kellogg was born in 1898 in Mount Vernon, New York, and died in 1972 in Fort Lauderdale, Florida. He spent approximately 40 of those 74 years actively engaged in research, a career that produced more than 130 publications, including two books assured as classics by their primacy in their respective areas, if not in fact by their quality as experimental investigations. Those books are *The Ape and the Child* (Kellogg & Kellogg, 1933) and *Porpoises and Sonar* (Kellogg, 1961) and both were ground-breaking projects in psychology. It is the purpose of this paper to explore the background of those two pioneering studies as well as other aspects of Kellogg's science that are often overlooked, for example, his numerous studies of learning and conditioning. Using Kellogg's own published writings, published evaluations of his work, unpublished notes on his research (chiefly on the porpoise studies), and correspondence and interviews with approximately 35 of his former students and colleagues, this paper provides the following: a biographical sketch; an overview of the diversity of his research interests; detailed comments on the ape and child project, the conditioning work, and the porpoise investigations; and finally, an assessment of Kellogg's place in the history of psychology. While the focus is on Kellogg as a researcher, it is hoped that the reader will also acquire a feeling for Kellogg as a teacher, a mentor, and a person.

On January 22, 1982, Florida State University named its psychology research building the Kellogg Research Laboratory of Psychology. The present essay is a combined version of talks that the two of us gave at that time on the life and work of W. N. Kellogg. Our efforts have been assisted materially by many individuals and we are most appreciative of their contribuitons. Especially critical were the letters, research notes, grant applications, biographical information, photos, personal recollections, and suggestions variously provided by Joe Grosslight, Shirley Kellogg Ingalls, Bob Kohler, Mike Rashotte, Jim Smith, and Charles Rice. Reprint requests should be sent to Ludy T. Benjamin, Jr., Department of Psychology, Texas A&M University, College Station, TX 77843.

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WINTHROP KELLOGG AS A STUDENT

Kellogg began his college days at Cornell University in 1916 but left after a year for the Great War in Europe. For 2 years he flew in England and France in the U.S. Army Air Service as part of the American Expeditionary Forces where he earned the prestigious *Croix de Guerre.* After the war he continued his undergraduate work at Indiana University where he met and married Luella Dorothy Agger in 1920. He graduated in 1922, having majored in philosophy and psychology, and for a while tried his hand at several jobs, including a brief time as a journalist. His wife's uncle, Eugene E. Agger, himself a university professor, felt Kellogg's talents and personality were well suited to an academic career and encouraged him to consider such an option. Accordingly, Kellogg enrolled in psychology at Columbia where he received his M.A. degree in 1927 and his doctorate in 1929. His dissertation, which was directed by Robert S. Woodworth, involved a comparison of psychophysical methods (Deese, 1973).

THE INDIANA UNIVERSITY YEARS

Kellogg was an active researcher as a graduate student, publishing five articles in 1928-29 in addition to his dissertation, and only one of those was coauthored. Three more articles appeared in 1930 and another five, the following year. This level of productivity was maintained throughout his academic career. He began that career at Indiana University as an assistant professor in 1929. The following year he was promoted to associate professor, and in 1937, to full professor. Kellogg remained at Indiana until 1950, although there were brief periods elsewhere—summers spent as a visiting faculty member at Columbia University in 1933 and 1934 and at the University of Southern California in 1948, 1959, and 1961. In addition, most of the 1931-32 academic year was spent at Orange Park, Florida, as the result of a research leave funded by the Social Science Research Council. It was during that period that the ape and child study was conducted. However, the time at Indiana was devoted largely to research on conditioning and learning. This research was carried out in a special dog conditioning laboratory, a facility which was completed in 1936 and proudly described by Kellogg (1938a) in an article in the *American Journal of Psychology.*

Still, he did research on other topics, sometimes with students, sometimes on his own. At least a few of these studies deserve mention to illustrate the versatility of Kellogg as scientist. As mentioned previously, his earliest work was on psychophysics and that research was essentially completed at Columbia, although one additional study was published during his time at Indiana. Single studies appeared on a variety of topics such as fear in rats, mice, and birds (1931a), advertising (1932a), emotion as it affects muscular steadiness (1932b), fetal activity (1941), and a learning curve for flying an airplane (1946). The latter paper was the result of studies conducted in 1939-40 for the Civil Aeronautics Authority as the United States prepared for the possibility of war. Kellogg would reenlist for that war, serving in South America and Trinidad.

The diversity of other research at Indiana no doubt reflected student interest. That is not to say that Kellogg was not involved in those projects; indeed, it is most unlikely that he would have supervised any research that he did not view as interesting. Included among these projects was a study on social perception in different racial groups (Kellogg & Eagleson, 1931), research on maze learning in water snakes (Kellogg & Pomeroy, 1936), and an investigation of the true-false question as an aid in studying (Kellogg & Payne, 1938). The source of the last of these studies is easily ascertained. Kellogg was a great believer in the value of objective tests and routinely gave his students in introductory psychology a list of hundreds of true-false questions from which those comprising the final exam would be selected.

There is another feature of Kellogg's publication record that should be mentioned and that is his facility for designing new scientific apparatus, improving extant equipment, designing new data collection procedures, and developing new surgical techniques, the latter primarily for chronic preparations used in his dog conditioning research. His vita includes nearly a dozen publications that deal entirely or in part with these kinds of improvements in research methods. The later pioneering work with porpoises would require that same kind of technical innovation.

THE FLORIDA STATE UNIVERSITY YEARS

Kellogg left Indiana University to accept a position at Florida State University. Apparently for some time he had wanted to move to Florida and sent letters to several universities in the state notifying them of that wish. Florida State University had only recently emerged from its role as a women's college and was eager to begin building a major university. That university made Kellogg an offer, and although it was below the salary he had been receiving at Indiana, he accepted it, moving his family to Tallahassee in the summer of 1950. That move marked the end of the dog conditioning studies and signaled the beginning of a whole new focus of research. After a single paper on conditioning in salt-water fishes (Kellogg, 1952), he turned his attention to the study of porpoises, something that would occupy much of the next 13 years of his life.

Florida State University offered Kellogg a chance to be a major force in shaping the psychology programs there, a leadership role he had not enjoyed at Indiana. Kellogg was something of a loner at Indiana, a fact dictated in part by his personality but also by the nature of his research. He spent his career, particularly the Indiana years, in a time when most of psychology was involved in research derived from one learning theory or another. For Kellogg, science was the product of natural curiosity. He spurned the value of theory because he felt it placed blinders on the scientist causing important findings to go unnoticed or at least to be misinterpreted. This atheoretical position was not very popular at Indiana, nor at many other institutions in the 1930s and 1940s, and it undoubtedly contributed to the fact that Kellogg had only a few doctoral students during his 21 years at Indiana.

Kellogg's situation at Florida State University was very different. He found himself as the senior person in the psychology department, and in terms of his visibility nationally, he was likely one of the most famous

professors on the campus at that time. He worked hard to build an excellent doctoral program in psychology and began that task by completely restructuring the undergraduate and master's degree programs. More than anyone else he was responsible for the acquisition of the matching funds from the National Science Foundation (NSF) to build the psychology research building at Florida State, the building that now bears his name. He also aided in the recruitment of new faculty, argued for the needed growth of the department, and in general used his considerable reputation as a scientist and scholar to enhance the psychology programs. At the same time he was conducting a very active research program with his graduate students on the sonar abilities of porpoises.

STANFORD RESEARCH INSTITUTE AND RETIREMENT

In 1963, Kellogg officially retired from Florida State, although he would return to that campus on several occasions in temporary faculty positions. In 1962, however, he began his association with the Stanford Research Institute (SRI) at Menlo Park, California, where he established two large research projects. One was funded by NSF and involved investigations of sonar in sea lions, while the second was funded by the National Institute of Health and involved echolocation in blind humans. The grants were for long-term projects, but it is unlikely that Kellogg ever saw his involvement in the projects beyond the first year or two. He hired two of his doctoral students from Florida State to direct the investigations—Ronald Schusterman for the sea lion studies and Charles Rice for the human echolocation studies. In February of 1965, Kellogg resigned from SRI. He and Luella spent much of their remaining days together traveling to various parts of the world. Their deaths came in the summer of 1972, his on June 22, hers on July 17.

KELLOGG AS COLLEAGUE, EDUCATOR, RESEARCHER

Before discussing Kellogg's major research areas, we provide a brief picture of the personality of this man as described to us by his former students and colleagues. To begin with, we note that the responses from these individuals have been amazingly uniform. Thus it appears that Kellogg was consistent in the way he dealt with people, and he does not seem to have been one way to one person and another way to someone else.

Winthrop Kellogg was a man of strong likes and dislikes who formed impressions of people on initial encounters, impressions that were not easily altered. He has been described as fair in his dealings with others and perhaps naive in his expectation that he would be treated similarly. He had little tolerance for those he viewed as unjust or those whose behavior he viewed as less than ethical and he had clear ideas about who were the incompetents and scoundrels in science. These negative evaluations often provided Kellogg with the impetus for his own research, thus at times giving it an adversarial quality. He was more than a little egocentric and frequently had difficulty in recognizing validity in positions contrary to his own. Kellogg was an individual with great self-confidence in his professional as well as personal life. Indeed, overconfident might not be too exagerated a

description. For example, one of his students, Paul R. Fuller, related an incident in which Kellogg suffered some financial embarrassment from an automobile accident since he did not have collision insurance on his car. "Since he was a skillful and careful driver . . . he surmised that if there were a collision he would not be at fault" (Fuller, Note 1).

Kellogg's manner was often brusque and businesslike. He had little time for casual chatter, particularly with students. He was a workaholic who demanded much from himself and from those around him, whether they were colleagues, students, or social acquaintances. He possessed a good sense of humor, although he did not necessarily appreciate humor from his students, since to him it indicated disrespect for the instructor. He was viewed as a person of great energy and intensity, who brought enthusiasm to much of what he did.

As a teacher, Kellogg received high marks from his students who were impressed by his exhaustive knowledge of the literature in his areas of instruction. He demanded respect from his students which included the requirement that they always address him as Dr. Kellogg. His well-planned lectures were enthusiastic, filled with details on numerous research studies and occasional vignettes about the researchers. While his manner in dealing with students was often curt, he was approachable and not unsympathetic to their problems. Many of his former graduate students felt that Kellogg was one of the best, if not the best, classroom teachers they had experienced. Not only did he make the subject matter interesting, but he communicated an enormous amount of information.

We have already mentioned some of Kellogg's talents as a scientist and his disdain for theory. He was also the very model of scientific integrity in the planning, execution, and reporting of his research. He took all precautions possible to see that extraneous variables were controlled; it was intended that nothing be left to chance. Everything was checked and checked again. He was almost obsessive in some of his research habits such as having extra equipment and tools on hand in the event that some repairs or replacements were needed during the course of an investigation. His laboratory was both orderly in arrangement and immaculately clean. Woe be the student who failed to maintain those standards. One of his students, Robert S. Daniel, has described him as "from the old school of researchers who did everything they could think of to try to prove that their own hypotheses were wrong before they published results" (Daniel, Note 2). Many of those trained by Kellogg acknowledged the debt they owed to him as a model for carrying out exacting, rigorous investigations. His skills in doing empirical research and the rigor of that work were no doubt partly responsible for Kellogg's reputation as a scientist and his success in obtaining grants throughout his career.

RESEARCH ON THE APE AND THE CHILD AT ORANGE PARK

No investigation in Kellogg's career brought him more attention than did the study involving the rearing of his infant son Donald with an infant chimpanzee, Gua. The study is well documented in the 336 pages that comprise *The Ape and the Child.* Our aim here is to explore the reasons behind the initiation of the study, to discuss some of the difficulties en-

countered in the conduct of the research, to describe some of the major characteristics of the investigation that set it apart from earlier and later studies, to speculate on the reasons for the eventual termination of the project, and to discuss its importance as a scientific investigation.

The idea for the study emerged in 1927 when Kellogg was still a graduate student at Columbia University. Kellogg and Kellogg (1933) give us that date for the idea but not its source. However, our guess is that it was stimulated by an article on the "wolf children" of India which was published that year in the *American Journal of Psychology* (Squires, 1927). Similar to Itard's "wild boy of Aveyron," the wolf children were two young girls found in a cave inhabited by wolves. These children behaved as though they were wolves, eating and drinking like those animals and making no use of their hands except to crawl around on all fours, which was their method of locomotion. Eventually the girls learned to walk upright, although they could never run. One acquired speech, at least a vocabulary of approximately 100 words, but the other continued only to make grunting noises. Howling noises at night were never extinguished, nor were their human teachers able to break them of the rather distasteful habit of "pouncing upon and devouring small birds and mammals" (Kellogg, 1931b, p. 162). Both girls died at an early age.

Like other feral children, the wolf children were judged to be subnormal in intelligence and it was assumed that their intellectual deficits prevented them from being able to adapt to their new surroundings. This interpretation was common in explaining the problems of adjustment in feral children and was, in fact, the explanation offered by Squires (1927). Kellogg disagreed with that interpretation, and in two replies published in the *American Journal of Psychology* (1931c, 1934), he argued that the wolf children, and others like them, were probably born of normal intelligence. Indeed, it was unlikely that they would otherwise have been capable of survival. From his environmentalistic perspective he contended that these children learned to be wild animals because that was exactly what their environment demanded of them. He believed in the strong impact of early experience and the existence of critical periods in development, and he maintained that the problem with civilizing feral children was the difficulty of overturning the habits learned early in life.

One way to test this hypothesis would be to place a human infant of normal intelligence in an uncivilized environment and to observe systematically its development in that environment. Kellogg noted that while such an experiment would be both morally outrageous and illegal, there was another way, albeit somewhat indirect, to test the environment-heredity question. That was to take a wild animal and place it in the civilized environment of a human home (Kellogg & Kellogg, 1933). Thus began the attempt to produce this unusual experiment.

A decision was made to select an ape, either a chimpanzee or an orangutan, preferably as soon after birth as possible. Kellogg was aware of earlier investigations with apes in civilized surroundings, but none of those instances met the rigorous criteria he would propose for his own experiment. As early as 1909, Lightner Witmer had attempted to teach human language to a chimpanzee, Peter, a retiree from a theatrical act. The experiment was largely a failure, but Witmer (1909) speculated that success

might be obtained in future investigations where "chimpanzees will be taken early in life and subjected for purposes of scientific investigation to a course of procedure more closely resembling that which is accorded the human child" (p. 205).

Figure 1. W. N. Kellogg holding Gua, 1932. (From Robert M. Yerkes Papers, reprinted by permission of Yale University Library.)

Kellogg wanted to use an experimental subject that was very young, before the animal could acquire a repertoire of infrahuman modes of responding. He wanted a situation that would assure that the animal was *always* treated as a human and never as an animal, particularly a pet. That is, it was not to be fed from a dish on the floor or scratched behind its ears. Interactions with the animal were to be full-time. He objected to arrangements whereby the animal was played with for several hours each day, only to be placed in a cage or otherwise ignored for the remainder of the day. One or more of these situations rendered previous investigations invalid in Kellogg's view.

The plan for Kellogg's experiment was outlined in a *Psychological Review* (1931b) article in which he wrote:

> Suppose an anthropoid were taken into a typical human family at the day of birth and reared as a child. Suppose he were fed upon a bottle, clothed, washed, bathed, fondled, and given a characteristically human environment; that he were spoken to like the human infant from the moment of par-

> turition; that he had an adopted human mother and an adopted human father . . . . The experimental situation *par excellence* should indeed be attained if this technique were refined one step farther by adopting such a baby ape into a human family with one child of approximately the ape's age. (p. 168)

Kellogg ended that article with a statement indicating that arrangements for such an experiment were currently underway. Those arrangements were the culmination of a number of years of discussion, a major issue being whether or not even to undertake the experiment. The matter is mentioned in the preface to *The Ape and the Child*:

> Indeed the enthusiasm of one of us met with so much resistance from the other that it appeared likely we could never come to an agreement upon whether or not we should even attempt such an undertaking. (p. ix)

The preface does not tell us who was who in that statement, but the enthusiasm undoubtedly belonged to Winthrop and the resistance to Luella.

That issue apparently resolved, Kellogg arranged a leave of absence from Indiana University, and with a grant secured from the Social Science Research Council, he, Luella, and infant son Donald moved to Florida, near the Yale Anthropoid Experiment Station at Orange Park. Through a special agreement with Robert Yerkes, they were able to obtain a young female chimpanzee, Gua. Gua was 7½ months old when the Kelloggs acquired her. At that time, Donald was 10 months of age. Kellogg regretted the fact that the chimp was not younger, but given the difficulties of acquiring young apes, he had little choice. At the conclusion of the experiment, he would assert that the age problem "was of less serious consequence in influencing her 'human' life than might at first have been supposed" (Kellogg & Kellogg, 1933, p. 18).

Luella Kellogg was not the only person to express some misgivings about the project. There were reactions from the public and, less directly, from some of Kellogg's own colleagues within the scientific community. Some individuals objected on the grounds that the experiment was inhumane in its rearing of a human child with an ape sibling. These criticisms also focused on the undesirability of using a young child as an experimental subject for such an extended period of time. A few people objected to the separation of Gua from her mother and the company of those of her own kind.

Further, the study was characterized as sensationalism and publicity-seeking. Kellogg felt his scientific integrity was in question from some of the reactions he received regarding the study. When the book was reviewed by *Time* magazine ("Babe and Ape," 1933), the reviewer referred to the project as a "curious stunt." Writing in 1968, Kellogg reviewed the studies that had reared chimpanzees in human homes and claimed that this kind of research required an investigator of high determination and dedication who could face those who would ridicule the experiment from their base of misunderstanding (Kellogg, 1968a).

Finally, there was another variety of criticism that was not unique to Kellogg's study but was (and is) a common experience for those scientists

who chose to work with apes. Robert Yerkes had faced the problem in establishing his anthropoid research station (Hahn, 1971). The objections came from people who could be labeled creationists or anti-Darwinists. They were suspicious of the Kellogg experiments which they viewed as against nature. Some people believed that the Yerkes station was involved in secret crossbreeding experiments between apes and humans, a belief that has persisted into modern times (Riopelle, Note 3). Recall also that Kellogg's experiment was not too distant in time or place from the famous Scopes trial in Tennessee, and any study that purported to look at similarities between apes and humans was no doubt viewed by some as evil. Despite the criticisms, the study was begun on June 26, 1931, when the Kelloggs brought Gua to their home.

For the next 9 months, Winthrop and Luella served as experimenters in a project that demanded 12 hours a day from the two of them, 7 days a week. With a few exceptions necessary "to meet the indispositions of the infants or experimenters," the schedule remained unchanged. Winthrop Kellogg was concerned that the experiment measure up to his demands. There was nothing he could do about the age differential between Donald and Gua, nor about the fact that Gua was not obtained shortly after her birth. Nevertheless, he would conduct his experiment as no other prior investigation with apes. He would maintain identical rearing conditions for his two experimental subjects. Further, he would use a variety of tasks to test his infants, not only on a comparative basis but also in looking at developmental sequences within each of them. Lastly, he would maintain sufficient scientific detachment to be able to evaluate objectively the data he was collecting.

So for 9 months, Donald and Gua were tested daily on such things as blood pressure, memory, body size, scribbling, reflexes, depth perception, vocalization, locomotion, reactions to tickling, strength, manual dexterity, problem solving, fears, equilibrium, play behavior, climbing, obedience, grasping, language comprehension, attention span, and others. The tests were exhaustive, and likely exhausting for the experimenters if not for the subjects. There were occasional baby sitters which gave the experimenters a brief respite from their duties, but those were rare occasions.

The scientific rigor and ingenuity of these tests is readily apparent from the detailed descriptions provided in the book. The reader is also likely to notice the impersonal style in which these tests are reported. If the preface and initial chapter of the book were omitted, the reader might not realize that the authors were describing studies involving their own child. At times one gets the impression of an overzealousness in the pursuit of knowledge. For example, consider the following passage from a chapter that deals with physical differences and similarities:

> The differences between the skulls can be audibly detected by tapping them with the bowl of a spoon or with some similar object. The sound made by Donald's head is somewhat in the nature of a dull thud, while that obtained from Gua's is harsher, like the crack of a mallet upon a wooden croquet or bowling ball. (Kellogg & Kellogg, 1933, p. 25)

While this example is extreme in terms of the specific difference being

assessed, it is characteristic of the detachment of most of the authors' descriptions. Indeed, the book, as well as the experiment it reports, is very different from a later publication, *The Ape in Our House* (1951), a report by Cathy Hayes about her chimp-rearing experiences. Her book is filled with anecdotes and makes no pretense of being a scientific investigation.

The Ape and the Child is clearly a book about an ape. It was the chimp who was the primary object of study; she was the experimental subject while Donald served as the control subject. This was a study designed to answer a question that was beyond the scope of other investigations. There were other works that described the young chimpanzee in considerable detail such as the naturalistic observations in *The Great Apes* (Yerkes & Yerkes, 1929) or the physical and physiological studies of Jacobsen, Jacobsen, and Yoshioka (1932). Other researchers, such as Wolfgang Köhler, had looked at problem-solving abilities in chimpanzees, and work on learning and memory had been going on at the Orange Park station since it had opened. But these studies did not permit answers of the kind the Kelloggs sought. At the simplest level, their investigation was an attempt to discover how human a chimpanzee could become when reared in a human environment. In fact, Kellogg's (1931b) *Psychological Review* article published prior to the project was entitled "Humanizing the Ape." But the experiment was much more than that; it was designed to be the definitive investigation explicating the interaction of heredity and environment. As such, it probably succeeded better than any study before its time in demonstrating the limitations heredity placed on an organism regardless of environmental opportunities as well as the developmental gains that could be made in enriched environments.

Our final concern is why the project ended when it did. *Time* magazine's review ("Babe and Ape," 1933) said the following:

> At the end of nine months the Kelloggs demonstrated that environment, particularly psychological environment, is necessary for the development of an individual's inherent abilities. Gua, treated as a human child, behaved like a human child except when the structure of her body and brain prevented her. This being shown, the experiment was discontinued. (p. 44)

However, *Time's* reason, while plausible, is not explicit in the book. Nor is a reason given in two articles that Kellogg would write about the subject toward the end of his career (1968a, 1968b). We are told only that the study was terminated on March 28, 1932, when Gua was returned to the Orange Park primate colony through a gradual rehabilitating process. But as for why, the Kelloggs, who are so specific on so many other points, leave the reader wondering. Several possible reasons, in addition to the one suggested by *Time,* come to mind. First, the schedule that the Kelloggs maintained for the 9 months was so grueling that they may have quit for reasons of fatigue. Second, they may have wanted to use the time remaining to them on leave from Indiana to prepare the book manuscript for publication. Third, Gua was maturing, gaining in strength and, according to Kellogg, becoming less predictable and more difficult to manage. It is possible that the Kelloggs feared Gua might inadvertently harm Donald.

A fourth possibility is suggested by material in the book having to do

with the acquisition of language, as well as by comments from several of Kellogg's students. Although the Kelloggs spent considerable time trying to teach Gua some words, she was never able to master them. She was quite adept at vocalizing and possessed a number of distinguishable and meaningful sounds. Gua was an excellent imitator in many respects, but at vocal imitation she was a failure. The Kelloggs noted that she never went through anything resembling the period of babbling common to human infants (see also Kellogg, 1968b). While Gua did not imitate Donald's sounds, the opposite was not true. When Donald was 14 months of age, the Kelloggs first observed him imitating the food bark that Gua would use in the presence of food. Initially he would mimic Gua's calls while she was engaged in such vocalizations, but later he would initiate the sounds wholly on his own.

Remember that Donald was 10 months old when Gua arrived. It was only a short time later that he was able to say her name, one of only three words in his vocabulary at 11½ months. By the age of 19 months, Donald's vocabulary still consisted of three words. Actually, he had used six words to that point, but it seemed that as one new word was acquired, one of the other words would be lost. With regard to Donald's expressive vocabulary, the Kelloggs wrote:

> he was therefore less in advance of Gua than he might have been. Indeed it can be safely said that neither subject really learned to talk during the interval of research.
>
> No doubt the necessity of spending so much time with tests of various sorts was to some extent responsible for this retardation. In addition the opportunity of associating with other children, an advantage possessed by most infants, was in view of the confining nature of the work of comparatively infrequent occurrence. (Kellogg & Kellogg, 1933, p. 281)

In short, the language retardation in Donald may have brought an end to the study. Of course, it is also possible that the project could have been halted for all or some combination of the reasons mentioned.

While on the subject of language, we would like to correct a common misconception of the ape and child study that has been magnified in recent years by the various studies on communication in primates using sign language or other symbols. The Kellogg project is sometimes conceptualized as an effort to teach language to an ape. But as our description indicates, this was not an objective of the study. Language and communication represents only one chapter out of 13 in *The Ape and the Child,* and most of that chapter deals with the development of receptive language.

We have reviewed the ape and child project in some detail because we believe that it played a significant role in Kellogg's professional career, primarily through the public recognition it brought. Some of Kellogg's colleagues in psychology and in the larger scientific community seemed little impressed with the study. Yet the project caught the imagination of much of the public, and as a result, Kellogg was something of a celebrity, famous to some, infamous to others. He is said to have lamented often the popularity of the research, particularly because he felt it caused much of his other work (for example, the dog conditioning studies) to go unnoticed. Following

publication of the book in 1933, he did not write on the subject again until the late 1960s, and then in response to the work of the Gardners with Washoe. Nevertheless, he would be queried about those 9 months in Orange Park for the rest of his life.

RESEARCH ON CONDITIONING AND LEARNING AT INDIANA UNIVERSITY

Kellogg's research in conditioning and learning while at Indiana resulted in some 50 published articles. These papers, many of which describe work carried out in the Indiana Conditioning Laboratory, cover a broad range of topics. Among them are the bilateral transfer of conditioning, the necessity of making a motor response in order to condition that response, learning in dogs suffering varying degrees of cortical loss, the effects of various drugs on learning, the relationship of forward and backward conditioning, spinal conditioning, and the nature of the response in flexion conditioning. As well, there are reports describing methods and apparatus that Kellogg used in his conditioning work. It is outside the intent and scope of this essay to review all of Kellogg's research in this field. Instead, we shall attempt to present its general nature and purpose. This will serve to illustrate further the kind of scientist that Kellogg was and to help us understand what posterity's judgment of him is likely to be.

From the array of problems that Kellogg investigated, one may get the impression that his conditioning research had no focus. However, such an impression is mistaken. His chief concern was the nature of learning, and virtually all of his conditioning work was aimed at illuminating that issue. Kellogg's views on the matter are set out in five articles published in the *Psychological Review* from 1938 through 1940. In retrospect, one can see how his empirical research flowed from the ideas expressed in these papers. This is not to say that they contained a comprehensive theory of learning. Rather, they presented what Kellogg considered to be a scientifically useful conception of learning, that is, one that did not stray far from what could be observed, namely, behavior, and one that generated scientifically researchable questions. Moreover, the articles nicely illustrate Kellogg's atheoretical bias that we mentioned earlier.

The first three papers are the most important. In "An Eclectic View of Some Theories of Learning" Kellogg (1938b) attempted to minimize the differences among four different theories of learning by showing that each simply emphasized different parts of the learning situation and by highlighting continuities among them. Another paper (Kellogg, 1938c) criticized Cason (1937) for defining learning in part as the strengthening of neural connections. The themes of this article were sounded later at greater depth by Kellogg and Britt (1939). They argued for a definition of learning that stressed function (behavior change) and not structure (changes in the nervous system), as Cason had proposed. The major part of the essay raised objections to the structural viewpoint. Essentially, these were that the structural changes underlying learning were hypothetical and that the role played by the nervous system in learning was uncertain. In discussing the latter point, Kellogg and Britt mentioned the evidence of learning in dogs

when the cerebral cortex was missing and the possibility of conditioning in spinal animals. Rather prophetically, both topics were ones that Kellogg was later to investigate in depth himself. In any case, it was concluded that a physiological definition of learning was far too speculative and should yield to one that emphasized changes in behavior or function. Behavioral changes were factual and observable; neurological changes amounted to little more than hypothetical inference.

The two final articles in the series (Kellogg, 1939, 1940) were brief replies to criticisms of the earlier papers. Neither reply is particularly substantive, although the later one is characteristically Kellogg. Chappell (1940) had attacked Kellogg and Britt's (1939) behavioral definition of learning. In response, Kellogg simply reiterated that the existing scientific data did not warrant a definition of learning in terms of changes in the nervous system, and, he concluded, "As far as I am concerned, that is all there is to it" (1940, p. 97).

We have said that a review of Kellogg's empirical work in conditioning and learning is beyond the scope of this essay. Nevertheless, it is appropriate to describe briefly one of the more significant of the learning projects, specifically, conditioning in spinal dogs. For one thing, Kellogg's interest in this problem may be clearly linked to his conceptual concerns about learning since, as noted above, spinal conditioning is explicitly mentioned in the Kellogg and Britt (1939) article. Second, major textbooks of the 1950s and 1960s (Kimble, 1961; Osgood, 1953; Stevens, 1951) gave prominent attention to Kellogg's spinal conditioning work. Third, the work brought Kellogg into controversy. Finally, the spinal conditioning publications indicate how accomplished Kellogg was as an empirical scientist.

The issue was whether dogs whose spinal cords had been transsected could acquire a conditioned response in a limb below the point of transsection. Observations pointing to this possibility had been first described by Culler (1937). Later, Shurrager and Culler (1940) reported data which they felt met the criteria of true motor conditioning and extinction in spinal dogs. The specific response conditioned was a muscle twitch in the exposed semitendinosis muscle in the dog's hind leg. Kellogg's first reference to the Shurrager-Culler data was in an article (Pronko & Kellogg, 1942) that described a muscle twitch in a limb when an electric shock was delivered to another limb. The animals involved were not spinal dogs, but the observation suggested to Kellogg the possibility that Shurrager and Culler's semitendinosis muscle twitch was not a true conditioned response.

Kellogg does not seem to have acted on this possibility immediately. In the years 1946 through 1949, however, he and his students published seven articles and delivered three oral presentations on the problem. The upshot was that spinal conditioning in dogs could not be produced in the Indiana laboratory. The most extensive reports were by Kellogg, Deese, Pronko, and Feinberg (1947) and by Deese and Kellogg (1949). In both articles, Kellogg concluded that the muscle twitch observed by Shurrager and Culler (1940) was actually a basic response to a conditioned electric shock stimulus applied to another part of the body (either to another limb or to the tail) and that an unconditioned electric shock stimulus applied to the limb in

question was unnecessary. It was argued further that changes in this muscle twitch with training should be regarded simply as sensitization of a reflex.

Although Shurrager responded to Kellogg's claims (Shurrager, 1947) and continued his interest in the topic (e.g., Dykman & Shurrager, 1956), we shall not follow the story further. Our intent has been to describe the nature and extent of Kellogg's participation in the controversy. One point worth noting is that Shurrager's work was carried out with acute preparations whereas Kellogg's research employed chronic preparations. While it is possible that this was the reason for the difference in their results, we mention the point also as a tribute to Kellogg's laboratory skills. It was no simple matter to keep dogs alive for months after their spinal cords had been transsected. The data published with Deese in 1949 were Kellogg's last word on the problem. Convinced that he had done all the experimental work necessary and that spinal conditioning was not to be found in his animals, he turned to other concerns.

RESEARCH ON PORPOISES AT FLORIDA STATE UNIVERSITY

Shortly after his arrival at Florida State University in 1950, Kellogg began his well-known investigation of the sonar capacity of porpoises. This project was completed in 1956. But after an interlude of research on other topics Kellogg returned to the study of porpoises (1961-63), this time to their visual and problem-solving abilities. This was his last serious involvement in research. As in our discussion of Kellogg's other research interests, our review of the porpoise investigations is intended to highlight the important features of Kellogg's science and to help us evaluate his position in the history of psychology.

In his investigation of the echolocating capacity of porpoises, Kellogg followed his characteristic ways. To begin with, his interest in porpoises was motivated by simple curiosity about how they were able to navigate so well. Believing that they used sonar, he set out to resolve the matter through experiments and careful observations. Three questions were asked. The first was whether porpoises (actually, bottlenose dolphins) emit sounds that can serve as sonar signals. The answer was that they do, namely, rapidly repeated clicking noises and bird-like whistles. The second question was whether porpoises can decode the echoes from such sounds. Two main things convinced Kellogg of this. One was the structure of the dolphin's ear and the related neuroanatomy. The second was the upper limit to the dolphin's hearing. Kellogg's procedures indicated this limit to be in the neighborhood of 80,000 Hz, although it is now known to be closer to 200,000 Hz or roughly 10 times that of the human.

Our principal concern here is with the third question, namely, the limits of the dolphin's sonar system and whether it is actually used by the animal in navigating and orienting. To obtain data about such matters, a special porpoise pool was constructed on the coast of the Gulf of Mexico and two porpoises were procured for study. We shall sketch the results of a number of experiments that were carried out with these animals.

First, the capacity of the dolphins to detect stimuli was found to be

remarkably acute. For example, they emitted sound signals to a stimulus as slight as a single BB shot dropped into the pool. As for sounds on the surface of the water, the minimal stimulus for a similar reaction was a half teaspoon of water dropped from a height of 1.5 m. Objects that were immersed silently, for example, a fish, were sensed in the course of periodic trains of sound pulses emitted by the animals every 15-20 sec.

In testing discriminative capacity, Kellogg capitalized on the porpoise's preference for spot to mullet as a food fish. Using spots that were half the size of mullets and pitting the two in a choice-discrimination situation, it was shown that the dolphin quickly learned to go directly to the spot. The conclusion preferred by Kellogg was that the animal was discriminating size by means of echolocation.

Further experiments were conducted to buttress this conclusion and to rule out the possible involvement of other sense modalities, notably vision. The latter was considered a possibility even though human visibility in the pool was restricted to about .5 m, the water was quite turbid, and some of the testing sessions were conducted on moonless overcast nights. One of these additional studies offered the dolphin the choice of two fish, both spots. They were lowered into the water simultaneously, and the porpoise began its approach toward them from a distance of roughly 2 m. However, one of the fish was held behind a plate of clear glass. Thus if the porpoise were acting on visual clues, as often as not it should swim toward the obstructed fish and strike the glass. But if it were echolocating, it should unerringly approach and select the unobstructed fish. The results were unequivocal. There were no errors in 202 test trials, and the time to go from the start point to the target point declined over the course of testing.

While other examples of such straightforward experiments giving equally unambiguous outcomes could easily be given, what we have described to this point suffices to illustrate the empirical and nontheoretical character of the entire project. It is emphasized also that this investigation represented pioneering research that demanded a considerable measure of methodological innovation. Kellogg warmed to such demands and functioned best when so challenged. The gathering of data for this project ended in 1956 and a complete account of the work was published as *Porpoises and Sonar* in 1961. Intended for the interested layperson as well as the scientist, the book was more than just an engaging account of the methods, results, and implications of a series of scientific studies. It also successfully captured the spirit and puzzle-solving nature of empirical science.

The second porpoise project (Kellogg & Rice, 1966) investigated the visual and problem-solving abilities of these animals. What makes this project notable are the unpublished research notes that were written as the study progressed. Not only do they allow a richer account of the work, but they afford considerable insight into the Kellogg brand of animal psychology.

By way of an overview of the research, we consider a single porpoise (Paddy) who was trained on a series of form discriminations. The stimuli were always two white patterns each set against a black background. When these patterns were presented below the surface of the water and an errorless training procedure was used, Paddy was typically able to learn the

correct member of each pair. To assess the problem-solving ability of the animal, numerous transfer tests of previously learned discriminations were made. A transfer task involved a change in the positive member of an earlier learned discrimination, in the negative member, or in both. In 71% of the transfer problems, Paddy responded correctly on all test trials on the problem. It was therefore concluded that the dolphin can not only use vision but is capable of generalizing from previously learned visual discriminations to the solving of new ones.

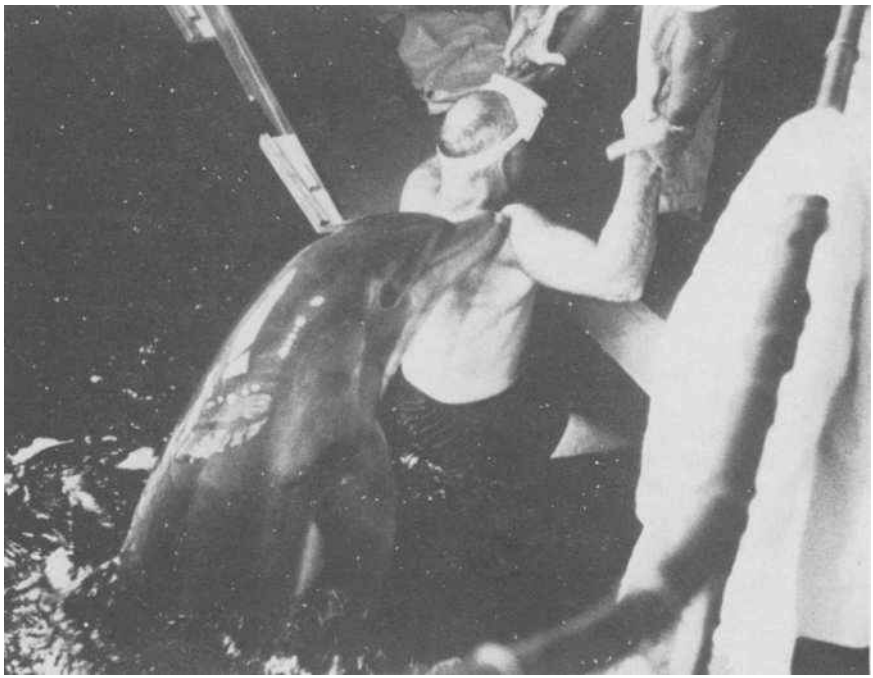

Figure 2. W. N. Kellogg and Paddy. Photo courtesy of Shirley Kellogg Ingalls.

We consider now the unpublished research notes. In general, they contain qualitative observations, insights, testing suggestions, and an idea of what happened during the course of the project. Most of them were written by Kellogg and indicate that he played a dominant role in the data gathering phase of the research. Moreover, these notes are not merely cryptic scribbled affairs. They are typed, single spaced, and typically a page in length. In short, they mirror the care and thought with which the project was conducted.

When the published reports and the unpublished materials are taken together, certain features of the project stand out. First, it had a strong comparative slant. In the original grant application, one of the stated objectives was to compare the dolphin's problem-solving ability with that of infra-human primates and human children. Comparative work on a

chimpanzee was actually conducted, although in a collaborative manner rather than directly by Kellogg. Unfortunately, the correspondence between the methods of the chimpanzee and Paddy studies was not sufficient to permit the intended comparisons. The point, though, is that the research was distinctly motivated in part by comparative interests.

A review of the research notes reveals that the project was more than a set of experiments. It was also a qualitative, descriptive exploration of the visual and problem-solving behavior of a single, intensively tested animal. The notes are replete with comments about the qualitative aspects of Paddy's activity. The animal's way of solving discrimination problems is often described and not simply its success or lack of success in such problems. Many of the descriptions are quite anthropomorphic, although Kellogg was well aware of this and typically used quotation marks around such comments. These kinds of comments did not appear in the published accounts of his research. There are also interpretations of Paddy's behavior from an emotional and a communicative perspective. In sum, the notes show that Kellogg was an earnest student of the dolphin's behavior.

In a similar vein, he often tried to understand things from the standpoint of the porpoise. Occasionally, this meant getting into the pool with the animal to see how things looked from his angle. In fact, this was how Kellogg discovered a possible source of Paddy's difficulty in solving pattern discrimination problems when the stimuli were presented *above* the waterline. In notes dated July 14, 1962, Kellogg wrote:

> For the first time we made some dives to see what the apparatus looked like from Paddy's standpoint. This probably should have been done on the first day. What we found out was astonishing and I am reluctant to admit we could have gone so far and been so damned dumb.
>
> When there is the slightest ripple in the water, the angle of entry of the refracted light rays is so garbled as to prevent any clear image of objects in the air whatever . . . .
>
> In contrast to all this, the stimulus objects when held a few inches under the water are perfectly clear even without an illuminated background . . . .
>
> Obviously, what we must do is present the stimuli underwater without lights.

Thus, Kellogg often acquired in a first-hand way an understanding of why Paddy behaved as he did.

Our review of the porpoise research has been an extended one. But we have felt this was necessary to convey an idea of the substantive nature of the research and, more importantly, further understanding of the characteristics of Kellogg's projects in animal and comparative psychology and of Kellogg the scientist.

AN ASSESSMENT OF KELLOGG'S PLACE IN THE HISTORY OF PSYCHOLOGY

To review, Kellogg had two main scientific roles during his career. First, he was a comparative psychologist and a student of animal behavior. This is best exemplified by his research on the chimp and the child and by his study of the echolocating capacity of the porpoise. Second, he in-

vestigated conditioning and learning. Particularly notable in this respect was his work on spinal conditioning in dogs. In both of these roles, Kellogg was recognized as an empirical scientist who did well-controlled, thorough experiments and who had a knack for mechanical inventiveness. His interest in theory was minimal and his research was stimulated by fundamental albeit broad questions (e.g., the contributions of heredity and environment to the development of chimpanzee behavior), by previous data (whether from his own or other laboratories), or by plain curiosity.

During his lifetime, Kellogg appears to have been recognized more for his conditioning and learning research than his comparative and animal behavior studies. To understand this, we need to consider the dominant ideas and practices of American animal psychology during the period in which Kellogg was active, 1930-65. These commitments are perhaps well known for they have been written about frequently (e.g., Beach, 1950; Gottlieb, 1979; Lockard, 1971). Accordingly, we mention them only briefly. To begin with, the subject of main interest was learning and the method for its investigation was laboratory experimentation. Kellogg's conditioning and learning projects and his laboratory skills squared perfectly with this tradition. In contrast, there were many other features of American animal psychology with which Kellogg was out of step. The preferred organism for study was the Norway rat. Similarly, the range of behaviors examined was quite limited. For the most part, rats pressed levers or ran in alleyways or mazes. A prominent casualty of the emphasis on learning was inquiry into the sensory capacities of animals. The aim of animal learning research was a theory, preferably mathematical, of behavior or learning in general. Qualitative differences among species were given short shrift. Instead, it was held that species differed mainly quantitatively and that such differences could be recognized simply by changing the constants in the equations of general behavior theory (Hull, 1945). Finally, in keeping with the emphasis on theory, it was felt that empirical science should be guided by the hypothetico-deductive method. In other words, one should generate a hypothesis from a theory and then carry out a laboratory experiment to test the hypothesis and hence the theory. Given this set of commitments, it is clear that Kellogg's professional reputation during his lifetime would have rested on his conditioning and learning research and that his comparative and animal behavior investigations would have placed him out of the mainstream of American animal psychology. Indeed, they may even have diminished his eminence in the eyes of American psychologists. We have noted previously that the professional reception of the ape-child project was lukewarm.

At the same time, much of Kellogg's research was compatible with the methodological practices of ethology, the science of animal behavior that prospered in Europe beginning with the 1930s but which failed to make much headway in America until the 1960s. The approach of ethologists, the ethological attitude, has been summarized by Burghardt (1973) as follows: (1) studying animal behavior that is meaningful given the animal's natural existence; (2) beginning with descriptive studies of an animal's behavior; (3) examining a broad range of species and behaviors; (4) comparing similar

behaviors in related species; and (5) avoiding concentrating research on domesticated animals.

One may find evidence of all of these commitments in one part or another of Kellogg's research. Thus, far from concentrating on any one domesticated species, Kellogg worked with a wide range of species (fish, snakes, birds, mice, rats, dogs, porpoises, chimps, and humans), not to mention a variety of behaviors (reflexes, various expressions of sensory capacity in the porpoise, various learned behaviors, and a vast array of developing behaviors in the chimp and the human child). The ape-child investigation is also a prime example of a comparative study, and one that was as much descriptive as experimental. Similar claims could be made for the porpoise research. Moveover, the latter research well illustrates the examination of something meaningful in the natural existence of the porpoise, namely, its sonar capacity.

There are other features of Kellogg the scientist that have a decidedly ethological flavor. In the case of the dolphin, at least, he tried to acquire as thorough a knowledge as possible of the animal's behavior. Often this meant trying to see things from the porpoise's perspective. The earlier-mentioned incident of Kellogg getting into the pool to determine how a visual stimulus appeared to the dolphin is only one of a number of such examples. This sort of thing plus Kellogg's anthropomorphic comments about his animals, his taking a chimp into his home and raising it like a human for 9 months, and his ape-child and porpoise books that were written for the layperson are all reminiscent of the practices of ethologists.

At the same time, Kellogg was far from the complete ethologist. What set him apart from this tradition was exactly what gained him recognition from American psychologists, namely, his investigations of conditioning and learning, his preference for laboratory experimentation, and his zeal for the principle of control. Furthermore, Kellogg did not share the ethologists' proclivity toward instinct. All of these points are plainly evident in his work, even in those projects which were most ethological in character. Thus, to the ape-child project, Kellogg brought a considerable environmentalistic bias; and in the porpoise research, he used many learning tasks to assess the sonar and problem-solving abilities of these animals. Finally, control and experimentation marked not only the porpoise research but also the ape-child study; raising a chimp in one's home is by any measure an experiment.

In sum, Kellogg was neither entirely an American animal psychologist nor a European ethologist. Rather, he represented a blend of some of the best commitments of both groups. Interestingly, the contemporary science of animal behavior represents a similar fusion of classic American animal psychology and classic ethology (Dewsbury, 1978). The excesses of both traditions are clearly declining, that is, the American overemphasis on learning and the pursuit of general behavior theory, and the ethological stress on the concept of instinct. As a result of this, a genuine comparative psychology has reasserted itself. Accordingly, from the vantage point of today's science of animal behavior, it is Kellogg's comparative research that seems most significant and enduring, especially his ape-child and porpoise projects.

Of these two, we believe it is the chimp-child study for which Kellogg will be best remembered. We find this quite understandable, for in our opinion, scientists are remembered for their ideas and not their empirical science no matter how good the latter might have been. And while both the ape-child and porpoise projects were major pioneering efforts of empirical science that were done with care, objectivity, thoroughness, concern for control, and ingenuity that few other than Kellogg were capable of, the ape-child work was more than that. It was fundamentally a good idea. First, it tackled a significant problem, namely, the nature-nurture issue. Second, it did so in an imaginative and extremely fruitful way. With the Kelloggs' son Donald serving as the control subject, the experiment was able to answer definitively the question of how human a chimp could become when raised like a human child in a human environment. When one adds in the anthropocentric flavor of the idea and its basic bizarreness in the eyes of many, it is not surprising that the ape-child study still commands recognition. We expect such recognition to continue and to ensure Kellogg a place in any serious history of comparative psychology. We would hope, though, that posterity will also remember him as one who performed important comparative and animal-behavior research in America during a time when it was not very fashionable to do so.

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