

*A VISIT TO BLOOMINGTON: THE FIRST
CONFERENCE ON THE EXPERIMENTAL
ANALYSIS OF BEHAVIOR*

JAMES A. DINSMOOR

INDIANA UNIVERSITY

Some day I should like to write a series of longer and more scholarly reports on the first meetings held by people interested in the experimental analysis of behavior. I would draw on the recollections of other participants as well as my own and would use my notebooks to reconstruct some of the material that was discussed during the sessions; but for the present, my personal recollections will have to suffice.

My first impressions of Bloomington, where I was later to make my home for a number of years, were based on what I could see through the windows of a Greyhound bus. Green walls of foliage rushed past as we approached the town along a curving highway now known as "Old 37." The last segment was especially attractive. Unexpectedly, to the right we saw a shallow stream rippling over a rocky bed alongside us, as we climbed up a ravine between two steeply sloping hillsides. Leaving the Cascades, we then continued down one of the main thoroughfares to the center of town.

It was in June of 1947, and the day was warm and sunny. The four of us—Fred Keller, Nat Schoenfeld, Fred Frick, and I—had traveled by train from New York to Indianapolis. I remember that I slept in a reclining seat on the coach, as I was desperately short of cash, while the others were able to afford berths on the Pullman. We got off the train at Union Station, now renovated to house a collection of small shops for the tourist trade but at that time a major center for rail transportation. We had breakfast at a luncheonette across from the ticket windows and then carried our bags the short distance to the Greyhound terminal. To this day I remember the men's room in that terminal as one of the most repellent I

have ever encountered: a hot, humid, crowded, grimy, and odorous underground cavern that appeared to be permanently inhabited by a collection of human derelicts. For a number of years, that room and the flatness of the surrounding blocks seemed to me to epitomize downtown Indianapolis. It was certainly not an attractive scene with which to greet the first-time visitor. But as we approached Bloomington, my spirits rose.

In those days the center of Bloomington looked very much like the downtown area of any other sleepy county seat in southern Indiana. We were let off on the square. Around the sides were the usual assortment of commercial establishments, mostly housed in buildings that revealed their 19th-century origins. On an elevated area in the middle stood the courthouse, surrounded by an expanse of lawn with some trees, two small cannon, a drinking fountain, a flag at the top of a tall pole, and a cenotaph erected by the local post of the Grand Army of the Republic. Traffic was sparse and moved at a leisurely pace. By contrast with the bustle and intensity of life in New York, Bloomington seemed quiet, friendly, and relaxed. After surveying the situation briefly, we stopped for coffee at a hole-in-the-wall down one of the side streets before returning to the square to seek directions to the university.

Although the suitcases began to feel a bit heavy before we got there, the route turned out to be simple and direct: We proceeded five blocks up the street on which we were standing, which was lined on either side with large and gracious maples, and on through the main entrance to the campus. The university library (now Student Services Building) was to our left. At this point, I think we continued up the hill and then to the right, along the sides of the thickly wooded quadrangle, rather than taking the brick-paved pathway that cut diago-

Communication regarding this article may be sent to James A. Dinsmoor, Department of Psychology, Indiana University, Bloomington, Indiana 47405.



Fig. 1. Science Hall, Indiana University, where the first and second CEABs were held, as seen from the edge of the quadrangle. The building was later renamed Ernest Hiram Lindley Hall, and in 1962 the department moved across campus to a newly constructed Psychology Building. (Courtesy I.U. News Bureau)

nally through to the opposite corner. In any case, at the top of the incline, along the eastern boundary, stretched several of the first buildings to be erected after the campus moved to its present location, back in 1885. They were constructed of rough-hewn limestone from the local quarries, with vines of ivy climbing up their walls. Science Hall was at the end of the row, just before it intersected with a row of newer buildings running along Third Street (Figure 1).

Inside the building, we later discovered, there was a small, rickety elevator with wooden doors, but it was quite obscurely located with respect to the entrance, and we climbed the broad, metal staircase, designed to handle the

flow of students to and from their classes. Two and a half flights up, we found ourselves facing the departmental office, where Skinner presided.

Looking down the corridor to our left we could see a couple of smaller offices on each side of the hall and then a flight of five steps, stretching all the way across, that led to larger rooms in the corners of the building. At the top of the steps, the room to the left (northwestern corner) proved to be the animal quarters, where the rats and the pigeons were housed. As far as I know, it was the only room in the building that had an air conditioner.

The room to the right (northeastern corner) was divided by wooden and celotex partitions into a suite of smaller, open-topped rooms, where the corresponding experimentation was conducted. A few years later, such rooms would have been filled with relays mounted on Bakelite panels and connected by snap leads; but back then the procedures, and therefore the circuits, were relatively simple. My visual images of the scene at the time, admittedly fragmentary, consist largely of lengths of string curving around cams cut from sheets of plywood. A photograph of some of the conditioning chambers then in use was published in an apparatus note a couple of years later by Guttman and Estes (1949). (See also one of the pictures tipped in between pages 184 and 185 of Skinner, 1979.)

Another piece of equipment, which I inherited for use in my own laboratory when I joined the Indiana faculty four years later, was what might be called a multiple or gang recorder. It had four pens, each riding in a notch cut in the top edge of a belt of heavy cloth, so that when it had been stepped all the way across the paper, it rode up on a metal incline, out of the notch, and was pulled back by the weight at the end of a string to its original starting position. The same roll of paper was used for all four pens and was drawn along by a single constant-speed motor.

One of the most ingenious devices we saw was the "grid scrambler" that Skinner and Campbell (1947) had built to shift the electrical connection of each of the floor rods in the experimental chamber and prevent the rat from finding a pair that remained at the same polarity. There were crank-shaped endings on each of the rods, permitting them to be rocked back and forth in a rotary motion, in order to

dislodge any feces that might have been deposited by the rat on receipt of the shock.

An item that we later borrowed for use at Columbia was based on a 78 rpm phonograph record, the standard at the time, with slits cut through the nonconductive surface material at varying distances around the circumference, baring the metal disk beneath. A metal point riding like the conventional needle on top of the record made electrical contact at intervals specified by the spacing of these slits. This was the first device used to program what were then called "aperiodic" (now "variable-interval") schedules of reinforcement.

At the other end of the corridor, the corner rooms were used to meet classes, and, as I recall, most or all of the conference sessions were held in the one to the left (southeast corner). There was a blackboard on the wall separating the two rooms, with a long table in front of it, suitable for conducting demonstrations, and movable wooden chairs, with arm rests for taking notes. During the meetings, a number of tall windows behind us and along the wall to the left were kept open, and the outside breezes held the temperature to an acceptable level.

That afternoon, I think, Skinner drove us through the downtown area and around the campus. I remember his pointing out a billboard sponsored by a local church organization which depicted a bearded man (not so common in those days) in biblical garb and asked whether "this man [was] crazy" in claiming to be the son of God. I suppose that if one were sufficiently firm in one's faith, the question seemed purely rhetorical and not sacrilegious. I also remember Skinner pointing to the university logo, displayed at several points on campus, which consisted of an I superimposed on a U in such a way that he could pretend it was the Greek letter *psi*. He also told us several anecdotes about his colleague, Alfred Kinsey, who was very much in the news at the time, even though his survey on male sexual behavior had not yet appeared in print (Kinsey, Pomeroy, & Martin, 1948).

I believe the faculty were housed privately, but the out-of-town students (I was teaching full-time in the School of General Studies but did not yet have my doctoral degree) spent their nights at one of the new graduate dormitories out beyond the auditorium, near Tenth and Union Streets. There was a bus that circled

the campus every 20 minutes, but it was just as fast to walk. The war was only recently over, veterans were returning to the campus in record numbers, and the university was expanding at a rapid pace. A number of trailers, quonset huts, and barracks designed for "temporary" wartime use were pressed into continued service to meet the need, and on the way to the dorm I remember passing great mounds of earth and construction materials.

I do not, at the present time, have a complete and accurate list of those who attended the sessions. A group photograph showing many, but not all of them, can be found in this journal on p. 456 of Volume 5. The same group of people also appears, with the present author cropped out, in the second volume of Skinner's autobiography (Skinner, 1979). Although in one case the date is given as 1946 and in the other as 1948, the correct year is 1947. Most of the participants were either from Columbia or from Indiana; of the twenty people in the photograph, there are only two that I cannot identify with one school or the other. Indiana had perhaps the longest experimental tradition of any department in the country (Capshaw & Hearst, 1980), but most of the faculty were new; some of what was going on at Columbia at the time has been captured in a recent article by Keller (1986).

We would hardly have qualified as an "eclectic" group—we certainly prided ourselves on our scientific rigor and in most cases our thinking was closer to that of Skinner than to that of any other theorist—but we did not wear blinders. Hull's views were frequently discussed at Columbia and Kantor's at Indiana. Spence and Guthrie, as well as Skinner, taught summer classes at Columbia while I was there. Personally, I remember browsing through almost all of the psychological journals then published as they appeared in the Psychology Reading Room. Some of the better known participants in the Indiana CEABs of 1947 and 1948 were also members of the Dartmouth Conference on Learning Theory (June 19 to August 18, 1950), which ultimately led to a book in which they examined and evaluated the leading learning theorists of the period (Estes et al., 1954). And Fred Keller and Nat Schoenfeld were already hard at work on their classic text, *Principles of Psychology* (1950), which attempted to weave experimental materials from a wide variety of sources

into a single, unified design. I do not recall any pressure toward orthodoxy, other than adherence to sound scientific principles.

There is no question that Skinner was the dominant figure. He was a youngish-looking 43 at the time, with a high "intellectual" forehead. (Pictures taken in 1946 and in 1948 appear in Skinner, 1979.) But although he was at the height of his creativity as an experimental scientist and was well known among his fellow psychologists—within the year he was to be elected to the presidency of the Midwestern Psychological Association—he was not as yet a major public figure. As I recall the spirit of the conference, we regarded him more as an especially shrewd and resourceful member of our expedition into the unknown—*primus inter pares*—than as an authority whose thoughts were to be treasured because of their origin. No one, I believe, would have cited him in the almost scriptural way that is sometimes done today.

The conference sessions were relatively informal. Both students and faculty presented recent or ongoing research and were welcome to participate in the subsequent discussion. From my notes, it is clear that Skinner was by far the most active contributor to the proceedings. He devoted quite a bit of time to the formulation of a standard set of definitions (theoretical concepts), classifying behavioral functions in terms that were ultimately reducible to physical description. Whenever there was an empty spot in the program, he offered some more definitions or treated his audience to selected tidbits from his large store of unpublished research.

At Columbia, none of us had ever worked with pigeons. As subjects for behavioral research, they were entirely novel at the time. Consequently, we were fascinated when Skinner opened the conference with a demonstration, at which he was so adept, of the shaping of key pecking. In the beginning, he had made use of pigeons captured outside the building. To trap them, he laid down a trail of grain which led the pigeon under an overturned box. One edge of the box was propped up with a stick to which a string was attached. When the string was pulled, the box descended, enclosing the bird. Unfortunately, these pigeons, like other birds living in the natural environment, were infested with various parasitic organisms, and some of these organisms migrated to the rats. By the time we arrived, however,

Skinner had discovered Wendell Levi and the Palmetto Pigeon Plant as a more satisfactory source of supply.

A detailed consideration of what was said at the meetings will have to await another occasion. Individual presentations were loosely grouped around different topics or themes at each morning and afternoon session, but irregularities in their sequence suggest that alterations were made to accommodate personal schedules or to fill in otherwise empty periods of time. In the first session, following his demonstration, Skinner presented a variety of material relating to the problem of selecting and defining an appropriate unit of behavior. In the second, Martin Tolcott, Bill Estes, Burton Wolin, and Bill Daniel described experiments on the interactions among the "drives" produced by such operations as depriving the subject of food or water or exposing it to bright light, low temperature, or electric shock. Next, Ralph Hefferline and Sam Campbell told us about their work on negative reinforcement. Under the heading of discrimination, we heard reports from Fred Frick on his doctoral dissertation and from Bill Estes on the effect of a food-paired tone on food-reinforced bar pressing. Quite a variety of work was reported under the heading of "response interaction": Skinner described some work with concurrent schedules (sometimes using compatible responses and at other times recording from each of two keys), his work on matching, and some exploratory work on the problem of reaction time in the pigeon. The final session was devoted to the topic of "emotion." This included Skinner's early "Parthenon" experiment, work on the production of bursts or pauses in the occurrence of one response during the extinction of another, concurrent response, and discussions by Doug Ellson and Fred Keller on possible differences in the nature of the reaction to different levels of aversive stimulation. It may be that we were naive in our optimism about the future, but much of our excitement stemmed from the fact that we were tackling the broadest and most basic problems, as we perceived them, of a systematic science of behavior.

Although most of the research was conducted with nonhuman subjects, a presentation by Nat Schoenfeld dealt with the effects of the experimenter saying "right" or "wrong" following the subject's response in an experiment on group conformity, and Kay Estes described

some of her work on the reinforcement of certain words in the utterances of preschool children. Also, at either the first or the second of the Indiana conferences we spent an afternoon at the outdoor pool in Brown County State Park, some 20 miles away, followed by an evening session held, I believe, at the top of one of the watch towers. There Skinner talked about the possibility of using subjects from prisons, mental institutions, or the military services. Unfortunately, I had not brought my notebook and was not able to record what was said.

There was no published report of the conference, as such, but it did lead to the initiation of a series of mimeographed notes, straightforwardly entitled "Conference on the Experimental Analysis of Behavior—Notes." Their purpose, as outlined by Skinner in the first issue "prepared on October 9, 1947," was "to continue throughout the year the interchange of ideas and data which began at the meetings" (Figure 2). If the Indiana and Columbia conferences could be said to be the precursors of such organizations as the Society for the Experimental Analysis of Behavior, Division 25 of the American Psychological Association, and the Association for Behavior Analysis, then these notes might be considered an early version of the present journal. But they were clearly less formal. Individual notes were ordinarily submitted in the form of a stencil to Bill Estes, who had offered to supervise the mimeographing and mailing, and were then set out at irregular intervals to "conference members" and to other interested parties who were willing to send a dollar toward the costs of publication. They featured material that was not appropriate for the archival journals, such as "preliminary reports, reports of pilot experiments, tentative theories of [sic] formulae, or suggestions, which were thought to be valuable." It was suggested that each participant at the June conference prepare a one- or two-page summary of the presentation, but unfortunately for the historical record, no one did that. A second CEAB, with somewhat larger attendance, was held at Science Hall in 1948, and a third, with 43 people showing in a group photograph, at Schermerhorn Extension, Columbia, in 1949. By then Skinner had moved to Harvard and Estes had turned to mathematical models. The last issue in my files, No. 22, is dated April 26, 1951. It was about then that Indiana invited me to come

CONFERENCE ON THE EXPERIMENTAL ANALYSIS OF BEHAVIOR -- NOTES
B. F. Skinner prepared October 9, 1947 Number 1

CONFERENCE NOTES At the last meeting of the conference in Bloomington in June 1947, a strong sentiment was expressed for some sort of informal publication which would make it possible to continue throughout the year the interchange of ideas and data which began at the meetings. The journals were felt to be too slow, and in any case they would not publish the preliminary reports, reports of pilot experiments, tentative theories of formulae, or suggestions, which were thought to be valuable. It was therefore planned to issue a series of one-page mimeographed notes from Indiana University to members of the Conference and others interested. Dr. Estes offered to supervise mimeographing and mailing, but all editorial matters are left to the authors. If possible a note should be submitted in the form of a stencil not exceeding "X" of printed matter. This is especially desirable when drawings are included. When necessary, stencils will be out at Indiana. Serial numbers will be added as notes are received. Follow this general heading and format. Arrangements have been made to send the Notes free to conference members. Others interested should send \$1.00 to Dr. Estes at Bloomington, in return for which he will receive notes until the dollar is gone.

Several members of the Conference meeting later in New York agreed that the first notes might well be summaries of the reports given at the June meetings. Accordingly it is requested that each participant prepare a note (one or two pages) for each topic which he presented and forward to Dr. Estes as soon as possible. This will be the only published report of the Conference.

As a starter:
Problem: To define punishment without prejudicing the experimental question of its effect upon behavior.
Solution: Define a positive reinforcer as any stimulus which strengthens a response when presentation is contingent upon the response, and a negative reinforcer as any stimulus which strengthens a response when withdrawal is contingent upon the response. (These are both rewards.)

The experimental question then remains: what happens when a positive reinforcer is withdrawn or a negative reinforcer presented?

Related Definitions (Columb please correct): aversion is an act which withdraws a neg. reinf.; avoidance is an act which prevents the occurrence of a neg. reinf.

Note on the set ending: The stimulus itself is a reinforcer. Presentation or withdrawal, contingent upon a response is a reinforcement. The change in behavior is conditioning. E.G., food is a reinforcer; presenting food when a response is made is a reinforcement; the increase in strength of response if conditioning.

Fig. 2. A photographic reproduction of the first in the series of mimeographed notes designed to keep members of the Conference on the Experimental Analysis of Behavior in touch with each other's work.

there to teach seminars on learning and (my choice) verbal behavior at their summer session. This time it turned out to be more than a visit.

REFERENCES

- Capshaw, J. H., & Hearst, E. (1980). Psychology at Indiana University: From Bryan to Skinner. *Psychological Record*, 30, 319-342.
- Estes, W. K., Koch, S., MacCorquodale, K., Meehl, P. E., Mueller, C. G., Jr., Schoenfeld, W. N., & Verplanck, W. S. (1954). *Modern learning theory: A critical analysis of five examples*. New York: Appleton-Century-Crofts.
- Guttman, N., & Estes, W. K. (1949). A modified apparatus for the study of operant behavior in the rat. *Journal of General Psychology*, 41, 297-301.
- Keller, F. S. (1986). A fire in Schermerhorn Extension. *Behavior Analyst*, 9, 139-146.
- Keller, F. S., & Schoenfeld, W. N. (1950). *Principles of psychology*. New York: Appleton-Century-Crofts.
- Kinsey, A. C., Pomeroy, W. B., & Martin, C. E. (1948). *Sexual behavior in the human male*. Philadelphia: Saunders.
- Skinner, B. F. (1979). *The shaping of a behaviorist*. New York: Knopf.
- Skinner, B. F., & Campbell, S. L. (1947). An automatic shocking-grid apparatus for continuous use. *Journal of Comparative and Physiological Psychology*, 40, 305-307.