Revista Mexicana de Análisis de la Conducta • Mexican Journal of Behavior Analysis
2025 | Núm. 1 • Junio | No. 1 • June | Vol. 51, 15-39
https://doi.org/ 10.5514/rmac.v51.i1.91684

JIMINY CRICKET, THE PREPARED MIND, CHANCE ENCOUNTERS, AND BEHAVIOR-ANALYTIC RESEARCH

PEPE GRILLO, LA MENTE PREPARADA, ENCUENTROS FORTUITOS E INVESTIGACIÓN ANALÍTICO CONDUCTUAL

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Resumen

Esta reseña es una autobiografía de investigación del autor. Se organiza en torno al método inductivo de investigación modelado por B. F. Skinner y descrito en su artículo de 1956, "Una historia de caso en el método científico". Comenzando con un experimento sobre los efectos del castigo en las interacciones conductuales en un programa de reforzamiento múltiple, el seguimiento de los resultados de experimentos sucesivos ilustra cómo el método inductivo genera nuevas áreas de investigación y enriquece las existentes. A medida que el programa de investigación se expande, algunas áreas se desvanecen, desaparecen, reaparecen y se transforman en otras, pero su influencia en la investigación contemporánea permanece. A lo largo del camino se discuten incidentes y personas que afectaron el curso de la investigación. Las áreas de investigación revisadas y conectadas son las relaciones respuesta-reforzador, el papel de la dependencia y la contigüidad en el establecimiento y mantenimiento de la respuesta operante, la demora de reforzamiento, la historia conductual, el resurgimiento de la respuesta operante y su extinción. La sección final considera algunas cuestiones generales relacionadas con el método inductivo, en particular la interacción entre las evaluaciones objetivas y subjetivas de la investigación. También se analiza el papel de la estética en la investigación del comportamiento y su impacto tanto en el consumidor como en el productor de la investigación.

Palabras clave: autobiografía de investigación, método inductivo, relaciones respuesta reforzador, demora de reforzamiento, resurgencia, extinción, estética de la ciencia

1.- This article is dedicated with affectionate appreciation to my dear friend and colleague Carlos Antonio Bruner e Iturbide, whose experiments and our conversations about them stimulated and enriched my own research over the years of our association. I so wish he were alive to read it. A version of this review was presented at the Southeastern Association for Behavior Analysis meeting in November 2024 in Greenville, South Carolina. Correspondence concerning this article may be addressed to the author at West Virginia University, Morgantown, WV 26506-6040. USA. Email: kalttal@wvu.edu

Abstract

This review is a research autobiography of the author. It is organized around the inductive method of research modeled by B. F. Skinner and described in his 1956 article, *A case history in scientific method*. Starting with an experiment on punishment effects on behavioral interactions in a multiple schedule of reinforcement, following the results of successive experiments illustrates how the inductive method spawns new research areas and embellishes extant ones. As the research program expands, some areas fade into the background, disappear, reappear, and morph into other areas, but their influence on contemporary research remains. Along the way, incidents and people that affected the course of the research are discussed. The research areas reviewed and connected are response-reinforcer relations, the role of dependency and contiguity in establishing and maintaining operant responding, delay of reinforcement, behavioral history, operant response resurgence, and extinction. The final section considers some general issues related to the inductive method, notably the interplay between objective and subjective assessments of research. The role of aesthetics in behavioral research and its impact on both the consumer and producer of the research also is discussed.

Keywords: research autobiography, inductive method, response-reinforcer relations, reinforcement delay, resurgence, extinction, aesthetics of science

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"Everything is connected, like a delicate
web. Ever growing, ever changing ..."
- Chelsie Shakespeare, *The Pull*

I was surprised and honored to learn of the Mexican Journal of Behavior Analysis's plans for this special issue. I am indebted to the editors and authors who have organized and contributed to it. My connection to behavior analysis in Mexico started with a paper that I presented in 1992 at the First International Congress on Behaviorism and the Sciences of Behavior in Guadalajara, Mexico, organized by Emilio Ribes-Iñesta and Peter Harzem. There, I met the person who has been the cornerstone of my association with behavior analysis in Mexico, Carlos Bruner. Carlos and Laura Acuña subsequently spent a sabbatical year in my lab at West Virginia University (WVU) in Morgantown, West Virginia. In the years thereafter, I sponsored at WVU several other behavior-analytic researchers from Mexico: Raul Avila, Alica Roca, Rogelio Escobar, and Jonathan Buritica. Interactions with them and others through correspondence and professional meetings both in the United States and during numerous visits to Mexico, the opportunity to teach a short course at the University of Guadalajara, and service as an examiner on doctoral dissertation committees at UNAM all have contributed to my understanding of our subject matter and our discipline. Many of my professional colleagues

in Mexico also have become personal friends, whose acquaintance I will value for all that remains of my life.

Space precludes mentioning all the research discussed with and undertaken by my students at WVU - those who completed Master's and Doctoral degrees with me as their adviser as well as other graduate and undergraduate students and others who have been associated with my lab. Among my colleagues over the years in the Department of Psychology at West Virginia University, Don Hake and Mike Perone have been most influential. All my colleagues and students have provided sounding boards and reality checks as we have moved together through the ever-shifting sands of the research and disciplinary issues of behavior analysis.

Setting Events

I was born during the Second World War. My parents began their marriage in Panama, where my father was an anti-aircraft artillery officer contributing to the defense of the Panama Canal. Soon after the war broke out, my mother, several months pregnant with me, was evacuated back to the United States. My father did not see me until I was two years old. Trained as a chemist and metallurgist, he spent his career in the U.S. Army. At the same time, my mother taught kindergarten at our various military assignments across the United States and in Germany, where I lived for three of my formative years. My parents were intelligent, energetic people who were generous in their affection with one another and with me and in sharing with me their knowledge and insights about people and the world.

The Inductive Method

I learned the inductive method early in my professional life. It remains my polestar. Its importance is reinforced each time I read Skinner's (1956) marvelous description of the role of induction in scientific research, *A Case History in Scientific Method*. The article offers several important lessons. One is the role of what Skinner casually describes as chance ["some people are lucky" (p. 225)]. Skinner certainly understood the importance of what Louis Pasteur called a "prepared mind." Bachrach (1962) translated the expression as "a combination of stored basic knowledge and a readiness to perceive the unusual" (p. 9). Put still another way, the observer requires a particular history of interacting with the subject matter for a chance circumstance to potentially affect the course of research. Apparatus failures, human errors, and the like are not the only serendipitous events that affect the course of a research program. Chance encounters with

casual remarks of others about perhaps an unrelated topic may, given the right circumstances guide the future direction of a research program.

The overriding feature of the inductive method, however, is its approach to data. Like all of us, I have received and given much advice: a little great, some good, and some (hopefully only a little), well, undistinguished. One bit of advice that I invariably give my novice researchers —undergraduate and graduate students working with me— is a variation of the mantra of Walt Disney's reconceptualization of Carlo Collodi's character from *The Adventures of Pinocchio*, Jiminy Cricket. Fans will recall Jiminy's famous advice to a values-torn Pinocchio: "Let your conscience be your guide." Paraphrasing that advice and eliminating its unpalatable mentalist frame, I advise my students to "Let your data be your guide." This review is the story of where following my own advice has led.

On Becoming a Behavior Analyst

My initiation into behavior analysis started with Brady et al.'s (1958) executive monkey experiment. In my junior undergraduate year, I enrolled in a course identified as "Comparative Psychology" at the University of Alabama. It was taught by a young assistant professor named Steven B. Kendall, the first of many chance encounters affecting my career. Steve had been a student of Benjamin Wyckoff at Emory University. Wyckoff, in turn, was a student of Skinner's before he (Skinner) left Indiana University for Harvard. Wyckoff was the "father of the observing response," among other things (Escobar & Lattal, 2011). Observing responses were Steve's focus as well (e.g., Kendall, 1965; 1969). This particular Snark of a comparative psychology course was in Steve's interpretation of its content a Boojum (Beach, 1950), because he used the course to teach the essentials of the experimental analysis of behavior (cf. Beach, 1950). When Steve mentioned the Brady et al. experiment in class, I wanted to learn more. So, I went to Steve's Comer Hall office to talk more about it. Soon thereafter, I was putting pigeons into boxes. I haven't stopped doing so since.

I hung around the lab (dubbed "SEBAC," for "Southeastern Behavior Analysis Center"), located in a "temporary" World War II era rickety building near the side entrance to the infamous Bryce State Hospital, on and off during the remainder of my undergraduate days. SEBAC was to be home for the next several years as I remained at Alabama for my Ph.D. degree. My graduate student colleagues included Anne Clay, Bob Campbell, Dave Gibson, Guy Marlowe, John Meehan, Jesse Milby, John Randolph, and Bill Sewell. Steve was my advisor during my first three years in the graduate program, but then left for the University of Western Ontario. My role model for scholarly teaching at

Alabama was Paul Siegel, a dyed-in-the-wool Hullian with a large soft spot for the good science that he saw coming from the experimental analysis of behavior.

In addition to the foundational research and content skills I learned at SEBAC, I learned about apparatus. In my first week as a graduate student, to my surprise, Bill Sewell told me that if I wanted to do research, my first task was to build a box. I was given a 4 ft by 8 ft sheet of plywood, lights, a response key, and a pigeon feeder. My second task was to assemble onto panels the control equipment I would need to program my experiment(s). Most of the apparatus at SEBAC was built on site from a seemingly endless stock of electrical and electromechanical government surplus components. These were culled from obsolete computers and sold to SEBAC by the pound at Redstone Arsenal in Huntsville, Alabama, which at that time was the home of the United States's space program. Learning to construct and program apparatus taught me resourcefulness and creativity in the design of apparatus and, by extension, in research. Throughout my research career, my SEBAC-acquired apparatus acumen has served me well. There has been very little that I could not design and build for my research. Later in my career, those same skills contributed to an appreciation of the history of the apparatus that shaped the experimental analysis of behavior (e.g., Lattal, 2004).

During my junior and senior years, I also took courses from two clinical psychologists in the department, Charlie Rickard and Mike Dinoff. Charlie was very committed to behavior modification, as it then was known. His doctoral adviser was Gerald Pascal, who, in his 1959 book, *Behavioral Change in the Clinic*, offered a behaviorist perspective on clinical psychology. Charlie became my advisor for the clinical psychology Ph.D. program, which I also completed while a graduate student. That aspect of my training was the foundation of my continuing appreciation for translation research. Mike and Charlie invited me to work the summer after my college graduation at their new camp for what then were called "emotionally disturbed children" in the northern mountainous (well, more mountainous than Tuscaloosa) region of the state. Two years later, the camp was the setting for research leading to my first publication in the then newly founded *Journal of Applied Behavior Analysis* (Lattal, 1969). The water safety and arts and crafts specialist at the camp during the summer of '64 was an American Studies major named Darnell Hammer, whom I had known in my earlier years when she and her family were our neighbors in Germany. The most significant event of that summer for me was that Darnell and I started a relationship that culminated in our marriage the following year. Over the past almost 60 years, we continue to grow

together both personally and intellectually. She remains today a singular influence on my views of the world, including, but not limited to, behavior analysis. Children followed —Matthew and our twin daughters Rachel and Ashley— and the five of us shared not only their formative years but, through myriad discussions —around the dinner table, camping and skiing adventures, shopping trips, vacations with grandparents, and everywhere else— the excitement, promises, and perils of research in the experimental analysis of behavior.

Another formative event in my ideas about research occurred the summer of 1965: an internship with Nate Azrin at the Behavior Research Lab at Anna State Hospital, arranged by Steve Kendall. Azrin's lab was, at the time one of the hottest spots in the behavior-analytic world and I found myself in the middle of it. As I arrived, Allyon and Azrin were finishing their seminal work on the token economy (Allyon & Azrin, 1965). Azrin's research on shock-elicited aggression (e.g., Ulrich et al., 1965) also was capturing widespread attention in and outside behavior analysis. Working with him was quite demanding, but I learned more from Nate in a summer than I did from any other experience in psychology to that point. If I had to pick the point at which the inductive method really sunk in, it was that summer working with Nate. His scientific behavior was under the complete control of the data. When the data were not as he expected, he questioned hard. What had changed? Were there glitches in the procedures? When they were as he expected, he questioned equally intensely. What were our next steps? How could we bring the behavior under even better control? What were our data telling us? It was the most exciting, stimulating place I could ever imagine being. It was Jiminy Crickett in spades: "let the data pose the next question." I had found a home.

Following completion of my Ph.D., I had a military obligation to fulfill and was fortunate to do so as a research psychologist at the U.S. Army Medical Research Laboratory at Edgewood, Arsenal, Maryland, from 1969 to 1971. I conducted behavioral pharmacology research with an eclectic group of experimental psychologists, most with a similar service obligation: Harry Avis, Dave Grover, Vince Houser, and Agu Pert, and our lab techs, George Maxey and Jerry Treadway. Jerry was a fellow behavior analyst who had the misfortune of being drafted out of his graduate program, but the subsequent good fortune to be assigned to our lab instead of Viet Nam. He was a particularly valued colleague during that time and has been ever since. Our leader was Colonel George Crampton, a contemporary and friend of Vic Laties during their graduate school time together at the University of Rochester. Geroge, whose research was about the vestibular system, was wonderful. He

vigorously supported his young researchers and protected us from the extremes of military bureaucracy while allowing, within the constraints of our broad mission statement, considerable freedom to pursue research topics that interested us,

As my time in the Army wound down, I wrote, and, happily, received a grant for a National Institute of Mental Health (NIMH) post-doctoral fellowship to study in George Reynolds's lab at the University of California at San Diego (UCSD). My dissertation (and my application to NIMH) had been on punishment contrast [inspired by both my experiences with Nate Azrin and by Brethower and Reynolds (1962)], but by the time I arrived in San Diego my interest had shifted to response-reinforcer relations, as noted below. Both George and NIMH were fine with this shift. During my time at UCSD, I benefited from interactions with my lab-mates, Ted Carr, Bob Collins, Charles LaBounty, Lynda LaBounty, and Allister McCloud, as well as with Edmund Fantino and his students, especially Dave Meyers and Steve Hursh. Dave and Steve remain colleagues and friends to the present. I particularly admired George's work on behavioral contrast because it combined a broadly inductive approach with theoretical acumen. The research described below had its roots in projects I undertook in my formative years as a graduate student at Alabama and during my subsequent time at Edgewood Arsenal and UCSD.

Establishing a Research Program at West Virginia University

I arrived on the West Virginia University (WVU) campus in June of 1972, when I taught my first class, Introduction to Psychology, in Oglebay Hall in a very hot, un-air-conditioned second-floor classroom. I set up a lab in what I came to call the "ivory basement" of Oglebay with a few items George had given me when I left UCSD, things lying around the lab I inherited at WVU, and some borrowed pieces of apparatus from Jim Shafer, the director of our experimental psychology doctoral program. Room 13 of Oglebay would be my lab home for the next 30 years. Ned Brainerd, O. J. Sizemore, and I assembled the equipment and got the lab running in quick order that first summer. The lab has continued uninterrupted ever since (even during the 2020 Covid epidemic, pigeons were run and data were generated). In 2002, the lab moved to Room 2151 of WVU's brand-new Life Sciences Building.

Some of the research in this story started before my move to WVU, but most transpired after that transitional summer into the role of "faculty member." I already have described the seminal roles of SEBAC, Steve Kendall, George Reynolds, and, especially, Nate Azrin during my pre-WVU research days. Good role models can take one only

so far, however, and at some point, one's behavior must come under the control of the data.

Early Analyses of the Response-Reinforcer Dependency

My first post-Ph.D. research program originated in 1968 because of a chance encounter with a new faculty member at the University of Alabama that year, Paul Weisberg. Paul had conducted extensive operant research with children (e.g., Weisberg & Rovee-Collins, 1998), including an experiment in which he maintained responding of children over extended periods with reinforcers delivered independently of responding (Weisberg & Kennedy, 1969). Our discussions introduced me to this research problem, and we soon embarked on an experiment in which we examined, with pigeons, the effects of repeated exposure to a schedule of response-independent reinforcement[2]. When I met Paul, I was winding down my doctoral dissertation, related to behavioral contrast as noted above (see also Lattal, 1970). One of the burning research issues at that time was whether the increases in responding during a constant component of a multiple schedule – behavioral contrast (Reynolds, 1961)– were the result of decreases in response rate brought about by extinction or were due to the simultaneous decreases in reinforcement rate in the changed multiple schedule component. I saw in the removal of the response-reinforcer dependency a way to separate these two confounded variables. By such removal in one component one can reduce response rates considerably without simultaneously changing the reinforcement rate. Although the effects were positive, Halliday and Boakes (1974) made the same discovery and published first. I took consolation that at least I was on a path that others also considered useful. My positive results, however, led to a series of experiments in which I investigated the stimulus control of responding by the presence and absence of the response-reinforcer dependency, what might be described informally as the discriminability of the response-reinforcer relation. These experiments involved comparsions of responding maintained by the presence and absence of the response-reinforcer dependency in, first, multiple (Lattal & Maxey, 1971; Lattal, 1972) and then in mixed (Lattal, 1973) schedules. One finding from these experiments that piqued my interest,

2.- The term "response-independent reinforcement" has been criticized because it is counter to the definition of reinforcement, which requires that a response produce the reinforcer and that responding be established or maintained that operation. The term is used here because in the research described, the reinforcing efficacy of the response-dependent event is established prior to removing the response-reinforcer dependency (cf. Lattal & Poling, 1981).

and to which I returned later, was that responding under mixed variable-interval (VI) variable-time (VT) schedules (i.e., half of the reinforcers required no response) was maintained under some conditions almost as well as it was when all reinforcers were response dependent (Lattal, 1973).

Reinforcement Contingencies as Discriminative Stimuli

My interest was in the discrimination of the presence and absence of a response-reinforcer dependency. In the above series of experiments, the only index of discrimination was response rate, which was confounded by the response-maintaining properties of the two response-reinforcer relations. I therefore turned to a conditional discrimination procedure in which a choice response, rather than response rate, indexed the discriminative properties of the sample stimulus. A chance discussion one day with Lynda LaBounty at UCSD led me to consider signal-detection theory (Green & Swets, 1966) as a way of separating the biasing (reinforcement) effects of the response-reinforcer relation from the discriminative properties of the time between a response and its subsequent reinforcer (See also Lattal, 1981). In the first such experiment, I arranged for each reinforcer delivered according to a mixed differential-reinforcement-of-low-rate (DRL) differential-reinforcement-of-other-behavior (DRO) schedule with each reinforcer to be immediately followed by a choice situation in a conditional discrimination procedure (Lattal, 1975, 1979). Thus, the choice was presented, with $p = .5$, either immediately after a key-peck separated by a previous key-peck by 10 s or more (the DRL schedule) or 10 s (the DRO schedule value) since the last key-peck before the food delivery. Pecking the choice key corresponding to the peck-pause requirement in effect on that trial was reinforced. In another variation, the response or its absence for 10 s produced only the choice component. This was done to determine how the 3-s access to food, which constituted a delay to choice component onset, affected choice responses. In both experiments, signal-detection analysis allowed separation of the discriminative and reinforcing effects of the different response-reinforcer relations. Choice responding was biased by delaying the choice response for different periods of time from the response that produced the choice (Lattal, 1975) and by varying the distributions of peck- and pause-produced reinforcers (Lattal, 1979; see also Lattal, 1981; Nussear & Lattal, 1983; see also Kuroda & Lattal, 2014). The theme of distinguishing sensitivity and bias as they contribute to response maintenance remains a major one in behavior analysis.

Combinations of Response-Dependent and Response-Independent Reinforcers

In seeking other ways to assess discriminative control by the presence and absence of a response-reinforcer dependency in the conditional discrimination procedure described above, I investigated the effect of, instead of blocks of VI and VT reinforcers alternating as mixed schedule components (Lattal, 1973), arranging the two response-reinforcer relations to occur in an irregular sequence. My plan was to use the irregular presentation of the two response-reinforcer relations as the samples in a conditional discrimination procedure like that described above. The initial results of the combinations, however, were sufficiently interesting that, consistent with Skinner's (1956, p. 223) observation that "when you run onto something interesting, drop everything else and study it," I put on hold my plan to substitute this combined schedule for the mixed DRL DRO I used in the earlier experiments. In its stead, a series of experiments ensued examining how different combinations of the two response-reinforcer relations controlled behavior (e.g., Craig et al., 2014; Imam & Lattal, 1988; Kuroda et al, 2013; Lattal & Abreu-Rodrigues, 1997; Lattal & Boyer, 1980; Lattal & Bryan, 1976; Lattal et al., 1989). Lattal et al. (1989), for example, placed a block of four response-dependent reinforcers in different temporal locations during a session in which all other reinforcers occurred independently of responding. Responding across individual sessions tracked the location of the response-reinforcer dependency, a finding later investigated as contingency tracking (Keely et al., 2007; Williams & Lattal, 1999).

Contributions of Response-Reinforcer Dependency and Contiguity to Response Maintenance

The research described in the last two sections suggested other questions about the controlling variables operative when a response-reinforcer dependency is imposed. Under the latter condition, two variables operate simultaneously: the dependency of the reinforcer on the response and temporal contiguity between the two. When reinforcers occur independently of responding, the dependency is removed, and the response-reinforcer temporal interval is free to vary. To isolate the effects of disrupting temporal contiguity while retaining the response-reinforcer dependency, responding was compared when it was maintained by response-dependent and response-independent reinforcers to its maintenance by an unsignaled delay of reinforcement procedure. We (Sizemore & Lattal, 1977) selected, rather arbitrarily since the procedure at that point was largely uninvestigated (see below)

an unsignaled delay value of 3 s. Once the VI interreinforcer interval lapsed, the first response started a 3 s unsignaled delay that terminated with the reinforcer, which was delivered independently of any *further* responding. This arrangement maintained responding at an intermediate rate relative to the high-rate responding maintained by immediate (VI arranged) reinforcement and the low-rate responding maintained when the response-reinforcer dependency was absent i.e., the VT schedule). Thus, disrupting temporal contiguity reduced responding but did not eliminate it, so long as the response-reinforcer dependency remained intact.

What if the maximum nominal or programmed unsignaled delay were longer, like 30 s? A response would be required for each reinforcer, as it was in the delay condition of Sizemore and Lattal (1977), but the maximum obtained delay would be considerably longer than they reported. Would such a long unsignaled delay be sufficient to maintain responding above that in a yoked schedule delivering equivalently temporally distributed but response-independent reinforcers? Gleeson and Lattal (1987) found that, in some pigeons, there continued to be a slight but consistent difference in response rates maintained by the delayed and yoked but independently delivered reinforcers (with higher rates with the delayed but response-dependent reinforcers) for up to 60 1-hr sessions, but for others the response rates converged at some point during the 60-sesssion experiment. Thus, there was a point at which the differential effect of the response-reinforcer dependency may be overshadowed by increasingly long delays of reinforcement and the response rates maintained in the presence and absence of the dependency become indistinguishable.

Delay of Reinforcement

The experiments in the previous section piqued an interest in delay of reinforcement itself, independently of its implications for the contributions of dependency and contiguity to response maintenance. At the time Sizemore and I started our 1977 experiment, only Skinner (1938) and Azzi et al. (1964) had investigated unsignaled delays of reinforcement. The observations in Sizemore and Lattal (1977) and Gleeson and Lattal (1987) shifted my research in two directions. One was to examine the relative efficacy of signaled and unsignaled delays in maintaining responding (e.g., Lattal, 1984, 1987; Richards, 1981; Reilly & Lattal, 2004). The other was to examine “pure” delay of reinforcement gradients, that is, gradients unconfounded by changes in the rate or distribution of reinforcers during the delay imposition relative to that during the immediate reinforcement baseline condition (Lattal, 1987). or by the presence of a signal during the delay. Sizemore

and Lattal (1978) scheduled unsignaled delays before each reinforcer arranged by a VI schedule. In different conditions, the programmed or nominal delays varied from 0.5 to 10 s. Based on the mean obtained (as opposed to programmed) delays between the last response and the reinforcer it produced, the delay of reinforcement gradients were unconfounded by an immediate, response dependent stimulus change indicating the completion of the schedule requirement and onset of the delay before reinforcement. Elcoro and Lattal (2011) and Jarmolowicz and Lattal (2013) replicated these results when responding was reinforced according to fixed-interval (FI) and fixed-ratio (FR) schedules, respectively. Lattal (2010) reviewed these and other findings related to delay of reinforcement.

An unexpected effect of imposing unsignaled delays on VI-maintained responding was that such delays actually increased pigeons' key-pecking at the shortest delay values we investigated, 0.5 s. This seemingly anomalous finding led to a series of experiments to assess the generality of the effect and examine its controlling variables. Our choice of .5 s, like our choice of 3 s as the delay value in Sizemore and Lattal (1977), was somewhat fortuitous. Had we not shortened the delay to .5 s, we would have completely missed an interesting phenomenon. Similar increases in response rates were found when 0.5-s unsignaled delays were imposed on VI, DRL, (Lattal & Ziegler, 1982; Arbuckle & Lattal, 1988), variable-ratio (VR; Holtyn & Lattal, 2013), and FI (Elcoro & Lattal, 2011) schedules. These findings suggested an interesting property of retaining but slightly loosening the response-reinforcer dependency. This loosening, at least with the pigeon's key-peck, reorganized the structure of key-pecking, allowing bursts of several successive responses to occur between the response that produced the reinforcer and its appearance a half-second later (Arbuckle & Lattal, 1988; Lattal & Ziegler, 1982). The result suggests that the functional operant changed from a single key peck to a burst of several successive key-pecks. The result of this change was an increase in the total number of responses, reflected as a response rate increase. Isolating the behavioral mechanisms of this effect through systematic analysis is an inductively based strategy for understanding what might remain otherwise mysterious effects of contingencies: look beyond the structure of what is arranged to the interface of the contingencies with responses.

Response Acquisition with Delayed Reinforcement

Except at the shortest delay values, degrading response-reinforcer temporal contiguity by introducing unsignaled delays of reinforcement reduced response rates previously maintained by immediate positive

reinforcement. These findings begged the question of whether a "new," never previously reinforced response, could be established with reinforcement that always was delayed from the response that produced it. Shaping or otherwise training the response was precluded, so Neuringer's (1970) procedure of simply placing the pigeon in the chamber and waiting until a response occurred was used. Any response operating the microswitch on the operandum then would initiate an unsignaled delay, followed by food delivery.

The first time I tried this procedure was on a weekend morning when the lab was quiet. I set up the program, placed an experimentally naïve, but magazine trained and food deprived, pigeon in the chamber and left. When I returned about 8 hours later, nothing had transpired other than using up a lot of (flat-lined) cumulative recorder paper. Disappointed, I was preparing to remove the pigeon from the chamber when I heard a very faint clicking sound that I knew well. It was the characteristic click of a device called a pulse former that converted (translated) each key peck of the pigeon into a 50-ms pulse that, in this experiment, activated the delay period followed by the reinforcer (feeder) timing clock. I went to the relay rack and listened more carefully. I heard it again. And then again. It could be heard, but not was not sufficient to fully activate the pulse former and, thus, the other programming components. When I crept into the experimental room and looked through the peephole in the chamber wall, I saw the pigeon vigorously swiping across the key with its beak. But nothing was recording. Had I relied solely on the flat cumulative record, I would have concluded that the procedure would not work. Instead, when I replaced the pulse former with one more sensitive (a "hair trigger" type device), the responses began recording immediately. Responding thus developed and sustained for 30 sessions or more with unsignaled, long-delayed reinforcement (Lattal & Gleeson, 1990). My good fortune, according to Pasteur, was my "prepared mind" in noticing (hearing) a subtle change that differed from baseline.

On this note, it also is the case that when something unexpected happens in the lab, there often is a simple explanation that is discoverable with a little systematic exploration. Some of my favorite research glitch stories revolve around panicked students showing up in my office because the apparatus has failed. As often as not, I go to the lab and solve their problem by flipping a switch to the "on" position or unclogging a feeder. Careful, practiced observation is a critical element of the inductive method.

From these initial findings, the questions flowed about controlling variables of what we came to call RADR, our acronym for "response acquisition with delayed reinforcement." The result was an exciting

series of experiments in which we failed to find variables that would not produce the effect! These included the learning of response sequences (Bruner et al., 2002) and of responses with delayed conditioned reinforcement (Bermúdez, et al. 2013) and the role of body weight (Lattal & Williams, 1997).

In most of the research, each lever press or key peck was accompanied by slight "click" when a pigeon pecked a key or a rat's lever press activated a microswitch. To eliminate this immediate auditory feedback that, was in fact, paired with reinforcement, we established the breaking of a photobeam with delayed reinforcement, where the response had no auditory accompaniments (Critchfield & Lattal, 1993).

In another experiment, we examined RADR with Siamese Fighting fish, *Betta splendens*. As a graduate student, when taking a(nother) comparative psychology course, I had written a review of learning experiments involving this interesting species. In my oral presentation of my findings, I included a demonstration, using a *Betta* that I had bred (I remain a tropical fish enthusiast to this day) and then trained to swim through a small aperture by presenting a mirror, to which it could display, following each such response. Lattal and Metzger (1994) returned to that demonstration to ask whether the RADR results were limited to appetitive reinforcers. As experiments do, this one raised many new questions about the controlling variables of visual reinforcement in Siamese Fighting Fish that spun off into another series of experiments derived from following the data from one observation to the next (e.g., da Silva et al., 2014; Elcoro et al., 2008; Wirth et al., 2003).

From Behavioral History to Resurgence

My early interest in behavioral contrast, in the form of the effects of punishment on unpunished responding, was at the root of another research area that became a focus for my students and me: operant response resurgence. The path to resurgence, however, ran through the analysis of behavioral history effects. In an early experiment conducted while in the Army, I trained rats to lever press under a multiple VI VI schedule before changing it to multiple VI DRO. Then, when extinction replaced the DRO schedule, responding in that former DRO component immediately increased substantially before declining to zero over a few sessions. Response rates also increased in the constant, VI, component, which, depending on whether schedule interactions are defined by the reinforcement or response rate changes in the changed component, was either positive contrast or positive induction (see Lattal & Miles, 2024). By either definition, this increase in responding in the former DRO

component was unexpected. It warranted follow-up, but there were other projects to complete, and the finding lay fallow.

In the late 1980s, yet another chance encounter changed the course of my research program. One of our department's clinical psychology doctoral students, Tim Freeman, approached me about conducting an experiment with me. He was quite specifically interested in behavioral history effects. A small project developed from an earlier experiment by Franks and Lattal (1976) evolved into his doctoral dissertation. In it two different reinforcement schedule histories established simultaneously in a single organism were shown to be differentially affected subsequent responding on identical FI schedules (Freeman & Lattal, 1992). Several other experiments on this topic followed. Among other things, Freeman and Lattal (1992) attracted the attention of Professor Hiroto Okouchi of Osaka Kyoiku University, and Sergio Cirino, then a graduate student at the University of São Paulo in Brazil. Okouchi inquired about completing a sabbatical in my lab and Cirino about conducting his dissertation research there. Both spent the same year in my lab, and both returned, at different times, for a second sabbatical at WVU. Okouchi and I have shared a 25-year collaboration on many experiments related to behavioral history (e.g., Doughty et al., 2005; Hira et al., 2011; Okouchi & Lattal, 2006; Okouchi et al., 2014; 2021). Cirino has become a respected historian of psychology with whom I share many interests related to that topic.

Around the same time Okouchi and Cirino were at WVU the first time, Greg Lieving began considering dissertation topics. Given the zeitgeist in the lab in this period, Greg and I had been talking about behavioral history. During one conversation, I had a "peanut butter and chocolate coming together" moment by connecting my old DRO finding described above with behavioral history. My analysis was as follows. Before the DRO schedule was emplaced, responding was reinforced on a VI schedule, so it now made sense that when reinforcement was eliminated from the DRO component, the previously reinforced (according to the VI schedule) would resurge à la Epstein's (e.g., 1983) earlier experiments on resurgence. This observation led to Lieving's careful and thorough analysis of resurgence as an instance of behavioral history. Because little was known about resurgence beyond Epstein's demonstrations, we settled on an exploration of the controlling variables of resurgence for the dissertation. The next few months were as exciting as the period of research on RADR as we followed our data from one new resurgence finding to another (Lieving & Lattal, 2003). Some of the subsequent resurgence research was summarized by Lattal et al. (2017), the conclusion from which was that the myriad of conditions giving rise to resurgence could be considered

as a worsening of conditions relative to the baseline conditions of reinforcement of the target response (cf. Nighbor et al., 2020; Oliver et al, 2018).

Resurgence has proved to be a rich patch for both student research projects and professional contributions for more than two decades. These included analyses of resurgence of time allocation (Cançado et al., 2017) and response hierarchies (Lattal et al. 2019); resurgence as a function of response and reinforcement rate (Cançado & Lattal, 2013; da Silva et al., 2008; Fujimaki & Lattal, 2015; Nighbor et al., 2020); temporal response distribution (Cançado & Lattal, 2011; Yensen et al., 2022); alternative response topography and reinforcement schedule (Doughty et al., 2007); reinforcement magnitude (Oliver et al., 2018); responses and stimuli present during response training and extinction (Kincaid et al., 2015); and, in a nod to a previous research area of interest that was imported into resurgence, resurgence when reinforcement is delayed (Jarmolowicz & Lattal, 2014; Nighbor et al., 2020). Cook and Lattal (2019) also developed a method for assessing resurgence of a pigeon's key peck response within individual sessions. Based on the above-cited experiments and others, Oliver and I questioned the value of the often-used control response in assessing resurgence (Lattal & Oliver, 2020). In 2024, Miles and I brought resurgence and behavioral contrast together by suggesting that operant response resurgence might be considered an instance of behavioral contrast (Lattal & Miles, 2024).

In the 1970s, as part of the investigation of the effects of delivering response- independent reinforcers, Lattal and Maxey (1971) and Franks and Lattal (1976) investigated the reinstatement of extinguished operant responding by the delivery of response-independent reinforcers. Following in part those earlier experiments and in part based on a more general interest in the research community interest in the recurrence of extinguished operant responding, we conducted several experiments relating resurgence to renewal (Kincaid et al., 2015; Nighbor et al., 2018) and relating renewal and reinstatement to resurgence (Alessandri et al., 2015; Kincaid and Lattal, 2018).

The Extinction Burst

Extinction was one of the first research problems I encountered. In conducting my first applied research, on the management of brushing teeth in the children's summer camp I mentioned above, I had made swimming after lunch dependent on each boy brushing his teeth during a pre-swim rest hour. The swimming area, a dammed-up creek, disappeared toward the end of the camp session when the creek dried up. This imposed a natural extinction condition that eliminated the

heretofore well-maintained brushing of teeth by each camper (Lattal, 1969).

My interest in extinction thus primed, it became of interest later for three other reasons. The first was related to resurgence in that it usually involves extinction of the target and then the alternative response. The second was that eliminating the response-reinforcer dependency, one of the earliest research topics of this review, is a form of extinction, with similar but not identical behavioral effects (Lattal, 1972; Rescorla & Skucy, 1969). The third was the questionable status of the extinction burst in applied behavior analysis research (Lerman & Iwata, 1995; Lerman et al., 1999). Experiments by Katz and Lattal (2020) and Lattal et al. (2020) burst the burst's bubble. In the latter two experiments, we found, in the most generous terms, only mixed evidence for extinction bursts under controlled laboratory conditions. This led to an analysis of how the extinction burst had been implemented and measured (Katz & Lattal, 2021). Jerome Alessandri, a colleague whom I first met during a sabbatical leave at the University of Lille in northern France in 2004, expanded the analysis of the extinction burst from one focusing only on response rates as its index, to the force with which a response occurred with human participants. a force response of humans. Our findings were consistent with those found with the key pecking of nonhuman subjects: the effects depended on how the burst was defined and measured, but in general there was minimal evidence of a reliable, consistent extinction burst (Alessandri & Lattal, 2021). The evidence thus far suggests that the extinction burst is far from the universal phenomenon it sometimes has been implied to be. We continue the search for its controlling variables with both humans and other animals.

Conclusion: Objectivity, Subjectivity, and the Inductive Method

The research reviewed to this point illustrates several features of the inductive method. Observations in one area led to further questions in that area but also led to branching off into other areas in an expanding universe of inquiry. Some areas reached dead ends, either because there was not much there worth pursuing or alternative problems become more compelling. Others morphed into new questions and areas of investigation. Still others become cyclical, with periods of intense investigation followed by dormant periods, but later resurging as new questions arise or a new context for the research arose. In each case, however, the data were the guide. Both serendipity and chance encounters also advanced the research agenda. Skinner's observation about dropping everything requires context: one cannot pursue every interesting lead all at once, but observations, like the multiple VI DRO

experiment mentioned above, can be stored away for future exploration. This also holds for serendipitous observations.

Research generated by the inductive method is evaluated, as in any other scientific enterprise, by specific generally agreed-on criteria that have evolved over time. These criteria are objective and relate to the reliability, generality, and impact of the findings (Sidman, 1960). Not all criteria of evaluation, however, are objective. Terms like "elegance," "balance," "symmetry," "beautiful," and the like –heard commonly in evaluating the fine and performing arts – are applied by scientists to experimental designs, individual experiments, research programs, and conceptual frameworks for experimental findings. Thus, research is evaluated by both objective and subjective criteria.

When an individual investigator subscribes to the inductive method, the criteria guiding that individual's research also are objective and subjective. One attempts to objectively evaluate procedures and data using the agreed-on criteria noted above. Such adherence is not entirely objective in evaluating one's own research efforts any more than it is in evaluating the research of others. Behavioral histories always come into play. These histories, in fact, play a critical role in scientific decision making when following the inductive method. Scientists are called upon to make "judgements" of stability and of differences in effects between conditions. An investigator speak of shaping a response or selecting a parameter as being "more art than science." Although the primary evaluative criteria for one's own work and that of others should be, and are, objective, the aesthetic qualities of designs, data, and interpretations of one's own work also enter into the meaning of "let your data be your guide."

I started this review with my first experiment and traced the evolution of a research program based on following what I considered to be interesting and useful observations from one experiment to the next and from one research area to the next. My research has been guided by several considerations: adherence to those agreed-upon objective standards for evaluating scientific research; my own judgement as a scientist, which I have learned through contact with the subject matter; and my personal sense of what constitutes aesthetically pleasing designs and solutions for research problems. It also has been guided by a seldom mentioned but constantly present consideration noted emphatically by Edward Chase Tolman (1959, p. 152): "In the end, the only sure criterion is to have fun." And, like Tolman, I have had fun.

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Received: September 26, 2024
Final acceptance: October 12, 2024