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Murray Sidman's Legacy and the Scientist's Behavior

Carmen Luciano

Universidad de Almería & Madrid Institute of Contextual Psychology -MICPSY-, España

Abstract

Murray Sidman passed away in May 2019 at age 96, leaving a prolific legacy behind. He was one of the stars that more powerfully shone in the behavioral science universe, illuminating with his work the paths that would be followed by many other scientists. We are seldom aware of the details characterizing a scientific trajectory like his. My aim with this paper is to honor Sidman's enormous scientific legacy by describing some of the conditions under which he accomplished the things he did, in an effort to provide some insight into his contributions. First, this paper briefly outlines the most relevant elements in his experimental and conceptual track of record. Second, it focuses on his way of doing science, his Tactics. Then, it changes focus onto the scientific characteristics that Murray Sidman himself shared with us concerning who he was when he was doing research. The paper ends with a more personal point.
Key words: Murray Sidman, behavioral science, experimental analysis of behavior, scientist's behavior, equivalence, derived relational responding.

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Novelty and Significance

What is already known about the topic?

- Sidman's scientific contributions are widespread in psychology and mainly in the behavioral analysis perspective of behavior.
- Several special issues have been published devoted to analyzing his contributions to behavior analysis.

What this paper adds?

- The combination of Sidman's contributions hand in hand to the conditions where he did what he did and to the expression of his behavior as a scientist.
- A distilled eight points about *Tactics*, the foundation of Sidman's way of doing science.
- The enjoyment, curiosity and creativity as reinforcers in the scientific`s behavior.

Murray Sidman passed away in May 2019 at age 96, leaving a prolific legacy behind. His life was a rich and brilliant trajectory that impacted (and still impacts today) many of us in our academic research, and that will continue to impact young behavior analysts in the future. The appreciation of this trajectory requires considering the many behaviors that formed his scientific behavioral repertoire. Behaviors such as generating ideas about behavior, preparing conditions for experimental analysis, collecting and analyzing data in a parsimonious and open way, deriving and establishing principles about behavior, and clarifying horizons and opening new avenues for further experimental and applied analyses of human behavior.

There are many stars in the behavioral science universe, but only a few of them shine from time to time with enough power to illuminate the horizon and critically influence the behavior of others who will perhaps gleam in the future. Sidman's work

* Correspondence: Carmen Luciano, Departamento de Psicología, Universidad de Almería, España. Email: mluciano@ual.es
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had that kind of power. However, as he expressed on several occasions (e.g., in 2007), scientists do not frequently report the conditions in which they do what they do. Sidman grounded his insightful work in a series of repertoires that he promoted and, fortunately, shared with us. Tracking his own writings about the conditions that controlled his scientific behavior is the main aim of this paper, in an effort to provide some insight into his main contributions.

Before going into the specifics of the paper, the question arises about what kind of contingencies select those repertoires that effect a change in the understanding of natural phenomena, that is, in providing key principles about behavioral phenomena that afford influence on them. Skinner (1953) expressed it this way:

> "Science is, of course, more than a set of attitudes. It is a search for order, for uniformities, for lawful relations among the events in nature. It begins, as we all begin, by observing single episodes, but it quickly passes on to the general rule, to scientific law. Scientists have also discovered the value of remaining without an answer until a satisfactory one can be found. This is a difficult lesson. It takes considerable training to avoid premature conclusions, to refrain from making statements on insufficient evidence, and to avoid explanations which are pure invention. Yet the history of science has demonstrated again and again the advantage of these practices" (p. 13).

Skinner established the search for general principles based on the connection between behavior and environment. He explicitly stated that the scientist's behavior is one more instance of behavior and, as such, it is just as controlled as the behavior of any other organism. In the view of Radical Behaviorism, akin to that of Interbehaviorism, science, in whatever field, is one kind of human behavior, of humans whose particular characteristics are a complex myriad of behaviors under the control of those functions established in each personal history. For this reason, any advance in science is the result of specific repertoires and the identification of those repertoires is a relevant goal, provided the critical and strong effect that science has in solving problems. As it is widely known, the behavior of behavioral scientists is a repertoire aimed at understanding and influencing behavior by the experimental manipulation of variables, so that the key conditions that are responsible for it can be isolated. Thus, the scientist's behavioral repertoire is a class, an operant formed by many responses that are established and maintained by specific reinforcers. Many kinds of behavior might take part in this dance: talking, dreaming, guessing ideas, designing experiments, designing apparatus and conditions, experimentally manipulating contextual variables, carefully looking at and analyzing data, writing, arguing with colleagues, reviewers, acknowledging mistakes, deriving experimentally-based key principles, connecting behavioral phenomena, deriving theories, etc.

Even if the diversity of the range of behaviors that can be categorized as scientific behaviors is somehow clear and shared, the conditions under which these behaviors are shaped and the contingencies maintaining them are not as crystal clear as to be helpful in order to influence scientific behavior. Contrary to what happens with other kinds of behaviors, scientists have rarely shared the variables controlling their behavior, what motivations are involved, and how they acquire control for subsequent responding. Frequently, these relevant points are lost in the midst of the formal aspects of scientific work, especially when, as it currently occurs, the number of published papers and the journals where they are published become the crucial merit for any academic or research position, above and beyond any consideration of the behavioral repertoires involved in scientific behavior. For instance, the repertoire of being curious and patient, of accepting frustration in the process of positing questions and getting an answer from the collected data, of enjoying the search for something unknown, of connecting apparently unrelated things, of solving problems, with all of these behaviors perhaps resulting in the establishment of general principles that make a change in solving socially relevant practical problems.

Sidman's trajectory is that of a researcher whose behavior produced key experiments that involved instances of serendipity, of finding something without being deliberately on the search for it, of connecting unrelated points and opening doors for understanding complex human behavior. In the context of his experimental pieces, Sidman formulated the philosophical foundations and the methods as open rules that he would follow when doing research, in his extraordinary book, Tactics. Furthermore, he shared with us aspects of his scientific behavior that are not typically considered. His way of finding connecting points as if composing music, as he used to say, or which motivations were involved in the specific behavioral patterns that sometimes produce insightful behavior in the scenario

My aim in this paper is to honor Sidman's legacy by going over the three abovementioned points. First, a brief outline of his experimental pieces that will set the stage for, second, some of the essential key points of his *Tactics of Scientific Research*, his way of thinking and doing science. Finally, I will turn to the aspects that Murray Sidman shared with us concerning who he was when doing research, when he came upon something new in his work and he derived order among different events. These behavioral events constituted the music of his composition, a composition that transcends his life.

Serendipity and insightful behavior around Sidman's experimental pieces

Some of Sidman's experiments had an enormous impact in terms of pointing out where and how to look at behavioral phenomena, mostly without specific instructions other than to be alert, remain curious, and let experience shape scientific behavior. According to his writings, he began his behavioral research somehow marked by his early concern about the problems in the world and how to change them. He had assumed that people would be able "to create their own world" (Sidman, 2007, p. 310) if the conditions allowing for this were known. The point was how to help in making this happen. In his words, "changes were going to have to be engineered deliberately, not left to the slow pace and uncertain outcome of natural evolution" (p. 310). As he expressed in 1989, he began early on to be worried about the violence between each other as he explicitly expressed: "…what I read about people's senseless cruelty and hypocrisy was almost unbelievable. How could human beings do the things they were always doing to each other?" (Sidman, 1989, p. vi).

The pioneering lab that Fred S. Keller and William N. Schoenfeld were running at Columbia University back in the late 1940s was the context for Sidman to begin to generate the conditions for "creating behavior" in a rat. What happened is that he realized he had found a pattern connecting the conditions he organized and the rat's responding, that is, the cumulative graphs were showing a kind of order in the operant chamber. Somehow, connections between behaviors, in and outside the lab, took place: During his student period, he became convinced (he credit Freud for that conviction) that the "psychiatric problems were built into behavioral histories of punishment and negative reinforcement" (Sidman, 2007, p. 312). He dedicated a good number of years to what he called *free-operant avoidance* and, among many other relevant details, he found a pattern relating the consequences and the rat's behavior to avoid being shocked. The systematic and replicated results produced with this procedure have allowed for its use in other domains such as in behavioral pharmacology. The analysis of aversive control and avoidance occupied Sidman early research. His inductive way of doing was very careful with the generalization from avoidance in rats to humans, and he was explicit about the need for further conceptual and experimental analysis of human avoidance. Within this context, Sidman published *Coercion and Its Fallout* (1989) with the explicit return of his early concerns about aversive interactions and providing solutions based on what behavior analysis had to offer for promoting positive each other interactions and our chance to survive. *Coercion* was a critical callout of the problems of punishment and a call for procedures based on positive reinforcement to substitute for aversive practices.

Sidman's research trajectory is linked to the area of stimulus control with a clear interest in solving practical problems encountered in teaching humans suffering from brain damage. He designed *discrimination* procedures for teaching complex discriminations to children and adults who did not learn with the typical teaching procedures. The interest in stimulus control was the scenario for his experimental pieces, including perhaps the "golden one" that was a kind of serendipity effect for the benefit of the entire field of behavior analysis. Very briefly, Sidman and his lab colleagues were having difficulties in teaching Kent (a boy with severe intellectual disabilities) to read (to relate written words to pictures) and they designed an indirect procedure consisting of, on the one hand, reinforcing Kent's pointing response to the picture of a ball when the sound "ball" was presented and, on the other hand, reinforcing the pointing response to the written word "ball" when the sound "ball" was presented. They did the same with more words and sounds and, soon, unexpected effects were revealed to them: Kent was relating the picture and the written word. That is, he was responding correctly to many untaught relations. Sidman called this phenomenon *Stimulus Equivalence* (Sidman, 1971).

Although this paper did not have a substantial immediate impact in behavioral research, it did so a few years later with multiple papers published, replicating the emergent or derived behavioral effect in many types of participants and with multiple types of stimuli. Equivalence research increased in the next two decades concurrently with conceptual developments about the concepts of stimulus class and derived stimulus relations and, mainly about the origins of equivalence. In other words, what conditions are needed for the emergence or derivation of equivalence relations? Prominent among these: the *naming theory* by Horne and Lowe; the *equivalence as a basic stimulus function* by Sidman; and the *relational responding theory* by Hayes and Barnes (see special issues in the *Journal of the Experimental Analysis of Behavior* -JEAB-, 1996 and 1997). But this was not the whole picture. All of this ran parallel to additional experimental preparations that evidenced not only relations other than equivalence (such as opposition and comparison, see Dymond & Barnes, 1995, 1996), but also the transfer of functions among the stimuli in the equivalence class (Dougher, Augustson, Markham, Greenway, & Wulfert, 1994).

All these creative experiments and conceptual developments would hardly have exploded at the time without Sidman's insightful study, as it has been acknowledged in several papers that have appeared highlighting the great significance of Sidman's research. A review of this is beyond the scope of this paper, and I strongly recommend reading this literature (e.g., Critchfield, Barnes-Holmes, & Dougher, 2018; Johnson, Iversen, Kenyon, Holth, & Souza, 2020; and the specific issue tributing Sidman in JEAB). The big and diverse current picture of *relational responding* is indebted to this first study on equivalence. It served as the occasion for the generation of variability in behavioral research, so that more experimental adventures began in the path for a functional analysis of complex behavioral phenomena. Sidman's early papers were the ground for many behavioral activities. First, for the analysis of language and cognition and the conceptualization of stimulus relations developed in Relational Frame Theory (Hayes, Barnes-Holmes, & Roche, 2001). Then, for the experimental analogs of clinically relevant phenomena (such as the work on derived aversive and avoidance functions, false memories, analogies and metaphors, rule-following, and many others). Also, for expanding behavioral applications, mainly for designing programs to establish language and to increase IQ, as well as for developing clinical interventions based on the functional impact on clinically relevant behavior (for a review, see Chritchfield *et alia*, 2018).

The relevant pieces of Sidman's experimental work can only be understood in the context of the contingencies that shaped his behavior as a scientist, the conditions under which he learned to do what he did. His whole repertoire of doing experiments was linked to the ideas about behavior-analytic research that were established hand in hand between private and public behaviors shaped by contingencies. This way of conducting research was explained by him in 1960, in his extraordinary book on behavioral science. I am referring to Sidman's *Tactics of Scientific Research*, a book about playing the piano and listening when novel and vigorous symphonies sound.

Tactics of Scientific Research, the foundation of Sidman's way of doing science

Murray Sidman not only produced experimental research, but he left us his writings about general procedures for increasing precision in the analysis of behavioral phenomena. He became aware of the prevailing chaos in psychological research and somehow this set the occasion for writing one of the most relevant books ever written about research in behavior analysis. *Tactics of Scientific Research* was published in 1960. *Tactics* was a call for science, in general, and for the experimental analysis of behavior in particular. It was a contrast either to the mainstream correlational and to the average data analysis that were the main actors in the academic and research scenario. Unfortunately for a natural account of behavior, it still is.

Sidman described that science is cumulative and integrative so thus appreciating the data of other investigators will have an important bearing upon the value of the own contributions (Sidman, 1960, p. v). He described from the very first pages that the book is dedicated mostly to the students and caution them "to not expect a set of rules of experimental procedures to be memorized… The pursuit of science is an intensely personal affair" (p. vi). It is not a systematic number of rules of experimental practice but, ironically, an open guide with hundreds of details for developing functional research. His book was considered "a kind of Bible" whose goal is not to tell the students and researchers what, where, and how to do. It is a kind of open Bible. Skinner wrote in his third piece of autography, *A Matter of Consequences*, the messages he sent to Sidman when the book was written:

> "A useful literature was in the making. In 1960, Murray Sidman published Tactics of Scientific Research. I sent him a telegram… and I wrote: "You have written a remarkably good book, and one which is going to be useful for all of us…. What is more important is that young people in the field will now have a chance to consider quietly and thoughtfully the many practices which have become a standard part of our own behavior." The book became a kind of Bible among operant conditioners." (Skinner, 1984, p. 266).

Reading this book is an extraordinary opportunity to open the eyes and ears of those behavior analysts interested in looking for the variables controlling behavior, in asking and looking for orderly interactions about causality. It is not a step-by-step guide telling the reader how to act and where to go. No, it is a prompt for scientists to let their behavior be shaped by contingencies, actions and rules hand in hand with natural contingencies of the research process: when moving through the manipulation of contextual variables, when guessing and carefully looking at the subject's responses, when analyzing data in the entire context of the present and the historical conditions of the subject responding. Sidman left the readers with an open field to explore, to observe the impact of their experimental manipulations, and to go back and forth between their thinking and acting in such conditions.

Sidman's *Tactics* was rooted in the philosophy of Radical Behaviorism and, consequently, its reading reveals the keys of this approach when being applied in the particular domains of analyzing the principles of behavior. What follows in the next pages is not intended to be a summary or a review of the book (but see the Special Section Conmemorating the 30th Anniversary of *Tactics of Scientific Research: Evaluating Experimental Data in Psychology* by Murray Sidman in *The Behavior Analyst*, 1990, volume 13, 2, pp. 159-197). What follows is very personal and brief outline of some of the characteristics that I have distilled from *Tactics*.

1. *Tactics is not a series of rules about doing experiments to test hypotheses. Tactics is not as series of rigid rules to be followed. Tactis is oriented to experimental manipulations and to derive rules about how the world works.*

From the very first pages of the book, Sidman expressed the impact of experience to shape the behavior of scientific. As clearly said in many of the pages of this book and many others of Sidman's contributions. He emphasized:

> "I do not claim to be either a systematizer or even a classifier of the rules of experimental practice.… Neither the practice of experimentation not the evaluation of its products can be bounded by any specific rules" (1960, pp. vi-vii).

And he was clear about the distinct effect when the focus to perform experiments is to evaluate hypotheses versus to indulge the investigator`s curiosity about nature (Sidman, 1960, p.7). Needless to say that he emphasized the latter focus throughout his contributions as it will be express in the next points.

2. *Being curious, a key behavioral repertoire in science.*

The class of behaviors forming the cluster of curiosity is not a trait, but a repertoire established throughout the personal history of interactions. In his words:

> "At some time or other, everybody asks the questions, "Why? What? How?" The child asks, "Where do babies come from?" Parents ask, "Why does he behave like that?" Samuel Johnson remarked that curiosity is one of the permanent and certain characteristics of a vigorous intellect. The scientist might be defined as a person whose indulgence of his curiosity is also the means by which he earns his living. What are the consequences of placing one's curiosity under the discipline of science? There are differences between everyday and scientific curiosity. A child, for example, notices a large number of bees flying about the rose garden. He asks his father, "Why are all those bees there?" The father replies, "They are gathering pollen from the roses so that they can make honey from it".
> The nonscientific child will stop here, his curiosity satisfied. The boy with a slightly greater scientific potential is likely to continue his questioning. "What is pollen? How do they make honey out of it? Isn't there any pollen in grass? Why do roses have pollen?" If the father hasn't yet lost patience, the budding scientist

> will come out with a real back-breaker: "How do you know?" Here then, is the first distinction between scientific and everyday curiosity. Scientific curiosity is concerned with the methods by which the answers to its questions are obtained. The curiosity is not satisfied simply by a demonstration that flowers are always present when bees congregate, and that flowers bear pollen. Perhaps the bees are attracted by certain colors. Or perhaps the shape of the petals is important. Perhaps the pollen that sticks to the bees`s legs is only incidental to their search for some substance that makes them attractive to bees of the opposite sex. These possibilities can be resolved only by controlled observation and experiment..." (1960, p. 7)
> "Everyday curiosity will subside once a direct answer to questions is obtained. Scientific curiosity is characterized by a chain reaction. Instead of quieting it, the answer to a question only arouses scientific curiosity further. It has been said that any experiment worth its salt will raise more questions than it answers" (1960, pp. 7-8).

3. *Behavior as a natural phenomenon, and science as the path to follow.*

The functional approach was clearly stated in this book, emphasizing that any behavior is only one more example of natural phenomena that are linked to the rest of natural phenomena. Even more, when the purpose of understanding behavior is controlling or influencing it, then, the approach has to be experimental above and beyond any other ways of knowing. Accordingly, this book highlights the science approach, the experimental analysis of the behavior. In his words:

> "Sometimes, when a commonly observed type of behavior is demonstrated in the laboratory, we hear the remarks, "So what! Everybody knows people behave like that. Who cares if rats or monkeys or college freshmen can do it too?" Such a statement assumes beforehand that common observation is an adequate substitute for controlled observation. The two may, at times, be in agreement, but there is no predicting this before experimental studies are undertaken. Everyday observation of human behavior is notoriously unreliable. In our impressions and interpretations of behavior as it goes on around us, we tend to overlook many properties of the behavior and of its controlling variables" (1960, p. 29).

4. *On individual behavior as the subject matter, and single-subject experimental methodology.*

Sidman's *Tactics* is a fruitful trip to the analysis of the variables controlling or influencing responding. He emphasized that an organism's behavior is a unique phenomenon consisting of the individual responding to the function provided by the present variables and always within the context of the individual's psychological history. For the analysis of the variables controlling responding, this book provides a good number of open procedures and measures to increase experimental control and to derive rules about the principles of behavior based on individual replications. Sidman's approach is contextualized in the radical behaviorism perspective and, consequently, in the idiographic analytic view to behavior. His position about the emphasis on the analysis of individual's behavior versus the average of punctuations from different individuals is remarkable; the former coherent to a replication focus versus the latter coherent to nomotetic and statistical significance. The many occasions he talked about this, he pointed that the average performance –and statistic inference- of a group cannot give account for any individual member of the group. It clearly established the limits of both types of designs when the goals are clearly set. He came back to these issues in the 30 anniversary of the book:

> "*Tactics* did not 'condemn' inferential statistics, but rather put them in their place. Group statistics can have great utility, but that utility does not reside within a science of individual behavior" (Sidman, 1990, p. 191).

5. *On the precise manipulation of variables, replication, and finding hierarchical keys among different behavioral events.*

He emphazised the manipulation of antecedents and consequences in order to observe the organism's responding as the target, always within the context of the organism's history. In this process, he insisted in not implementing more than one manipulation at a time so that the change in responding from one stimulus condition to another could be evaluated with regard to the differential conditions. As he explicitly wrote:

> "We must be able to classify our variables in such a manner that we can recognize similarities in their principles of operation, in spite of the fact that their physical specifications may be quite different" (1960, p. 27).

He insisted on replications in the same individual and across individuals and behaviors, so that the conditions under which a particular effect occurs might be generalized, that is, in finding regular patterns of behavior. In this regard, he insisted on looking for the conditions in which an effect was not replicated and on looking for the conditions that might be responsible for the behavior observed in both cases. And, then, back to additional experiments so that the pattern between conditions and responding would be clarified. He emphasized the replication of manipulating variables to account for establishing common properties across behavioral variability. His description of his learning process of looking for common elements is remarkable as he expressed in regard to his first experiences:

> "Systematization of data by exposing the similarities among their determining variables may seem an uninspiring pursuit to the ambitious student. As a young graduate student, for example, I felt that my work had to be different, that it had to produce something new that would startle the world. Along these lines, I once wrote a paper describing some of my work, in which I emphasized how different my experiments were from anything else that had ever been done. One of my teachers, W.N. Schoenfeld, agreed that the data were very interesting. But he went on to add that I had written the paper from a peculiar point of view. I had emphasized the differences between my work and everyone else's. But science does not ordinarily advance that way. It is the job of science to find orderly relations among phenomena, not differences. It would have been more useful if I could have pointed out the similarities between my work and previous experiments. Although the task he set for me was not an easy one, I reached a higher level of scientific maturity when I finally accepted his advice" (1960, p. 15).

6. *On the interpretation of data.*

Data are not floating in a vacuum. To the contrary, they belong to the conditions under which they have occurred and, without such conditions, data are out of context and their interpretation is misleading. This rule might seem logical but it is frequently forgotten in psychological research, and data are presented and analyzed as if tree branches might grow separated from the trunk they belong to. For instance, this is the case when interpreting data on the basis of the average response of many individuals when, however, their behavior is necessarily linked to their history. It is also the case when the rate of responses is presented without being related to the function that antecedents or consequences might have in the experimental manipulation. Even more, it is also the case when responses (as thoughts, emotions, and actions) are analyzed as if being separate events instead of analyzing the conditions under which each of them occurs as well as those conditions that establish relations among them. The former is the case when, for instance, thoughts are categorized in a list, emotions in another one, and actions in yet another list, as occurs in taxonomic systems. The latter is the case when behavioral units are analyzed taking into account that one behavior might acquire control over other responding because of the historical contingencies. Furthermore, the same might be said when one behavior is analyzed as if being independent from other behaviors -based of having different topographies- when they might belong to, or form part of, a functional class built of the same functions.

Sidman's *Tactics* is a constant reminder to link the data to the historical and present conditions under which the organism responds; it is a prolific field of experimental methodology to increase internal and external control and interpreting accordingly. On the one hand, designs and measures to increase validity and reliability. On the other hand, for the generalization and extrapolation of data based on the conditions under which they were obtained which necessarily include taking into account the historical conditions of each organism. In this track, he defended experimental control and replication instead of statistical interpretation to establish functional relations between conditions and behavioral changes.

7. On the *induction process to formulate laws, the principles of behavior.*

Tactics is full of information oriented to induction as formulating behavioral laws based on the contingencies while doing experimental analysis and multiple replications, in contrast to deduction without an experimental analysis. After 30 years of *Tactics* he renewed these keys points:

> "Our own behavior in relation to our data differs vastly when we derive knowledge by inductive rather than statistical inference..." (Sidman, 1990, p. 191).
> "Inductively derived general principles are so derived by virtue of evidence from many sources and many instances of observation and are only contradicted by *independent* evidence. They hold true by observations, rather than by procedure..." (Chiesa, 1990, p. 99-100 as quoted by Sidman, 1990, p. 191).

The book is explicit to promote that scientific behavior should be shaped by contingencies . That is the case when formulating questions and arranging the conditions for being alert and observing the answers that the experimental conditions allow; that is, deriving rules on the basis of the contingencies produced by the observation of the organism's historical and actual responding in the manipulated conditions. Sidman expressed this core aim in the scientific analysis of behavior. His very frequent appeal for the researcher to be more attentive to his own behavior of discovering than to his guesses and hypothesis is remarkable. The next paragraphs illustrate these issues:

> "Psychologists must recognize, as do other scientists, that advances in knowledge come from many unexpected quarters. A man may have a guess about nature, and the proof or disproof of his guess may indeed mark an important contribution. As Skinner has noted, 'There are doubtless many men whose curiosity about nature is less than their curiosity about the accuracy of their guesses…' (Skinner, 1938, p. 44). Such experimental activities can result in the piling up of trivia upon trivia" (1960, p. 4).

> "Curiosity may, of course, be guided by hypothesis and by theory, but the history of science reveals many discoveries that resulted from the inquiry, 'I wonder what will happen if…' Great experiments have been performed without the experimenter having the slightest inkling as to the probable results. In testing a hypothesis in which he believes, a scientist is surprised only if the data *do not* support his guess. A scientist hostile to a hypothesis is surprised only if it *does* receive support from the data. When an investigator performs an experiment to test *no* hypothesis, his life is full of surprises.
> There is a distinction to be made here between having a hypothesis and performing an experiment to test that hypothesis. We often make guesses about the outcome of our experiments—even those who feel themselves to be bedrock empiricists. But often the experiment may be planned and begun before the guess if formulated. The experiment is performed for other reasons than to test the adequacy of the hypothesis. Nor will the outcome of the experiment be judged a success or failure in terms of its agreement or disagreement with the prediction. This point emphasizes an important property of experiments that are designed to answer the "I wonder what will happen if…" type of questions. Such experiments, if they meet adequate criteria of reliability and generality, *never produce negative results*. Data can be negative only in terms of a prediction. When one simply asks a question of nature, the answer is always positive. Even an experimental manipulation that produces no change in the dependent variable can provide useful and often important information" (1960, pp. 8-9).

The point is that the scientist should treat his own guesses (his derivations of relating things) lightly and giving credit to what is occurring in particular conditions; as if being like a curious child. Sidman's book is likely to provide a major dose of humility reactions in front of the experimental manipulations. And this is particularly useful when the experimental conditions and the participants' responses sound somewhat incoherent according to even the least explicit guess. If so, keep tracking in the analysis of the manipulated conditions. Perhaps something will unexpectedly emerge and will give the opportunity for additional insightful questions and further experimental manipulations.

8. *A horizon of research possibilities for experimental analyses*.

Tactics is not telling what and how to do except for offering open rules for experimental control and for any focus of interest for the researcher. He was again very explicit about this later on:

> "… I knew that I was not writing about strategy at all. I was convinced then, as I am now, that the tactics I was describing were general, not restricted to the study of behavior" (Sidman, 1990 p. 187)…

More specifically he emphasized one more time that

> "an experimental design is empty until it is applied to a problem… It is sometimes difficult to brush away the uncomfortable impression that … instead of fitting designs to problems, investigators are devising problems to fit the designs" (1990, p. 188).

Sidman wrote long after about the variability of research somehow linked to Tactis. His words:

> (...) "My own research and that of many other shifted into radically new directions –transitions rather than stable states, human subjects both handicapped and normal, behavior so complex that we were for many years not prepared methodologically or conceptually to look at it, and new experimental questions that raised problems

> of technique, control, measurement and procedures specification which Tactics had never explicitly cited. Yet we have been able to proceed under the same general rubric –analyzing the behavior of individuals, carrying out experimental analyses of variability, arriving at principles by induction from cumulated observations, and gaining new insight into relationship between the behavior of our subjects and our own behavior as scientists" (1990, p. 190).

Sidman's *Tactics* is a present for behavioral scientists to gain perspective by opening their repertoire to the systematic experiences of the experimental manipulations and the organisms/participants responding in such conditions and its connections to problems in other areas. It is a reminder of being willing to observe, think about, experiment, observe again, change, experiment again, and change again. More specifically, to be willing to see order one day and variability the next, and to do new experiments; to be willing to see order in one part and not in another, to see things differently, and to gain insight into the experience of connecting points in the diversity of the behavioral universe. The scientist's behavior and the consequences, provided by the participants' responding to the experimental manipulations, is an amazing interplay, a kind of playground where many games can be applied to different areas and where new games are likely to be invented when attending to what is going on. Sooner or later, this might give the opportunity to connect with other playgrounds and the history will continue.

This book, now sixty years old, is for many of us an extraordinary source for research in behavioral science. It has been signaled as one of the most relevant in behavioral psychology and it was the focus of a special issue at the 30 Years of Tactics (see the eight papers, including Sidman's replies, 1990). Hopefully, this book will continue to be useful for the future of science and for the science of behavior.

Enjoyment, curiosity and creativity as reinforcers in the scientific`s behavior

Skinner postulated that the scientist's behavior is one more behavioral target to be analyzed and understood in the context of the reinforcing conditions that control it. Sidman provided abundant explicit information in this regard, writing about his own observations of the conditions in which he developed his scientific behavior. It is unusual that scientists share the private and public behaviors involved in the process of doing science, however, Sidman gave some examples in the paper entitled "The analysis of behavior: What's in it for us?" (Sidman, 2007). He asked "Why are we so unwilling to let people know that laboratory work can generate such reactions?" (p. 314). Perhaps more explicit than in other moments, he let us know about who he was when doing research, when finding something new on the way and when deriving order among different events. It is interesting to see that he described his own behaviors when putting on the table the meaning of the term "experimental analysis of behavior." He described the experimental analysis of behavior not just as a set of methods and theories but as "the behavior of behavior analysts," pointing to the need for identifying the reinforcers of their acts, and he asked, "What keeps them going?" (p. 309). What reinforcing functions are controlling the behavioral scientists' behavior when, in the act of analyzing other organism's behavior, they become aware of an orderly pattern of behaviors? When they are looking at data and the conditions under which they are produced, and suddenly they "discover" connections among points, like an insight showing up on stage. When they feel as if a light is illuminating order where there previously was chaos, and then establishing a higher level law, a principle of behavior. I think there is no better way to illustrate this than Sidman's own words on the matter:

> "In our scientific writing about behavior, we fail to transmit the excitement of doing research. We rarely describe the thrill of finding out things no one knew before. Although the prevalent public conception is that scientists are cold, logical creatures, it is easy to demonstrate that scientists are also lovers of worldly pleasures…" "What scientists seem reluctant to acknowledge, however, is the poetry in what they themselves do, the poetry that is intrinsic to the process of discovery" (2007, p. 309).
> "We also fail to reveal the passion with which we try to distill orderliness out of chaos, and the exhilaration we feel in the discovery of such orderliness. And although we try to avoid superstition and unverifiable doctrine, we nevertheless come close to religious fervor when we succeed in placing the conduct of human beings -what humans do and why they do it- within the realm of natural phenomena, thereby bringing the behavior of living beings, including ourselves, into the grand scheme of order in the universe" (2007, p. 310).

There are many instances of this behavioral class that he made explicit in his description of his trajectory as a scientist. His interest and confidence in science, as the tool for finding an understanding of behavior and for finding solutions to problems, was signaling the path for the experimental analysis of the variables controlling behavior. Sidman shared that type of scientific behavior in his writings: being curious when observing behavior, trying to solve problems, making changes and observing the effects of those changes; all these behaviors forming part of the natural contact with the world when doing the job. He explained his early experiences and the excitement of doing something new, such as when he organized the data by drawing a cumulative curve that showed a pattern between the timing of shocking and the rat's avoidance response. For instance:

> "Does anyone think I looked at those data dispassionately, that I just coldly entered numbers into a table and then into a graph? Was I just mechanically going through the standard routines that the textbooks say differentiates scientists from nonscientists? No, you can probably empathize with me when I say I was floating on cloud nine for the rest of my vacation" (Sidman, 2007, p.313).

A similar kind of emotional reaction showed up when the procedures designed for children and adults with learning difficulties gave amazing results, that is, when the consequences of designing and implementing such discrimination procedures were on the stage. Those types of private and public behaviors, commonly named emotions and feelings, were part of the contingencies of his scientific behavior when unexpected outcomes showed up after many, many attempts to teach Kent how to read (as described in the first part of this paper). Here is Sidman's own description:

> "Trial after trial, Kent correctly matched the 20 pictures to each of the 20 printed names, and the name to its picture. The lab technician, sitting behind Kent in the experimental room, could hardly contain himself. At the end, he leaped up, grabbed the boy in a bear hug, and shouted, 'Dammit, Kent, you can read!' Outside the room, where the rest of us were watching through a one-way window, I was dancing the twist; my son, who happened to be in the lab at that moment, said to me later, 'Dad, I've never seen you like that before'" (Sidman, 2007, p. 315).

The enthusiasm arising from this outcome of relating previously unconnected points is, in my view, hard to describe in words. These are the emotional functions, experienced in one's own skin, when finding commonalities among different events. This enthusiasm might function as an example of positive reinforcement in terms of its significance to the scientist's personal life -a kind of higher order or overaching hierarchical reinforcer. In Sidman's words:

> "Achieving that kind of systematic integration involves more than the quiet satisfaction of getting papers published, or the economic advantages of academic promotion, or even the gratification that comes from professional recognition. Reasoning is akin to singing; the logical progression of thought in planning and carrying out an integrated research program resembles, to me, the composition of a piece of music (...) that seemed to me to match what had just happened in the lab" (Sidman, 2007, pp. 313-314).

This is creative behavior, and I do not mean anything mystical, but the confluence of responses such as exploring, being curious, precise in the manipulation of variables and attending to unpredicted outcomes, and being sensitive to the consequences of those behaviors. Depending on one's personal history, these functions might become inclusive or dominating over other functions that frequently show up in the inductive process of experimentation (such as frustration). However, it is the former that allows the scientist to keep going. Sidman was that kind of scientist. A source of inspiration in the precise description of his careful way of doing research in many domains of the process, especially when he wrote about the conditions under which he did what he did. More specifically, focusing on the functions that different stimuli have acquired in the experimental contexts; taking into account the likely history of the organism; tracking the organism's responding in such conditions and, then, letting his whole repertoire of scientific behaviors be shaped by the contingencies which finally permit deriving general laws of behavior based on experimental control. And, then, back again to more design and more experimentation.

Human beings are frequently, and early, taught to be aware of the impact of their own behavior on themselves, on others, and on the evironment around. In the experimental domain, as Sidman indicated, this experience is not frequently shared, which precludes a complete understanding of the complex interactions taking place in the shaping and maintenance of the scientist's behavior. One more example is given in these sentences:

> "…when we publish our research findings, we are not allowed to communicate the thrill of research, the poetry in the discovery process, or the exhilaration in the discovery of order… the expression of the emotional "vibes" that research generates…" (Sidman, 2007, p. 312).

Sidman made a revealing and very relevant scientific contribution when, perhaps in his most intimate paper, he made public the conjunction of his private events and public responding in the class of behaviors that, as operants, defined his scientific demeanor. Private events are behavioral events that are established to have specific motivational and discriminative functions as antecedents and consequences of other behaviors of the scientist. Sidman advocated for letting people know about this. Especially important, he wrote, is to "let the students know" (Sidman, 2007, p. 316). Those kinds of private events, private behavior, cannot occur in the absence of the experience of moving from words to actions and vice versa. His concerns about the importance of preparing conditions for the shaping of these behaviors were meaningful then and they are now. He defended, as Skinner did, the importance of deriving thoughts and actions through the contact with the natural contingencies instead of promoting these complex behaviors merely under the control of instructions. As we know by experimental analysis, the latter becoming more inflexible and insensitive to changes in contingencies and, thus, quite likely to become a barrier for the most effective scientific repertoire in the experimental analysis of behavior. Let the former repertoires be promoted so that behavioral researchers can contact the still unknown areas in human behavior, and that the principles governing them surge to surface for the sake of effective applications. As he expressed at age 84: “Here was a whole new universe opening up for exploration” (Sidman, 2007, p. 311).

In *Tactics*, Sidman wrote about the end of a scientific trayectory as follows:

> “At the end of a lifetime of work, the scientist may well look back upon his career not only with pride but with astonishment at the results of innocent inquiries begun many years before” (Sidman, 1960, p. 8).

A strong coordination is clear between his words in 1960 and the highlighted effects that his scientific contributions has generated in the science of human behavior.

A final point

Lastly, a more intimate and brief point dedicated to honoring Sidman through my direct experience with him. *Tactics* was the first light of Sidman’s trajectory that reached me in 1978. I was writing my undergraduate thesis, and Ramon Bayés, a professor at the University of Barcelona, advised me to read Sidman’s *Tactics*. The impact of Tactics was an extraordinary opportunity to be more precise in my understanding of the functional perspective and impacted my behavioral research repertoire. At that time, I was also beginning my career as an academic and I used *Tactics* to organize the introductory course in Psychology at the University of Granada. I must confess that I was advised to the contrary by the cognitively-oriented professor in charge (since he considered Sidman’s book old-fashioned, useless material). However, I was in love with Behaviorism and his advice did not exert the intended control on my behavior (I was explicitly asked not to use that material for the sake of my own academic promotion). Needless to say consequences were effectively in place and I did not gain promotion at that time. Years later, I was really grateful for those initially aversive consequences, because they constituted an opportunity to open new avenues: they set the occasion for my application to a postdoctoral Fullbright scholarship, thanks to which I had the chance to meet the author of this magnificent book face-to-face. I met him in Boston in the fall of 1985, at the Shriver Center, with Harry McKay and others. It was an extraordinary moment. Being in touch with them and having the opportunity to read and comment with them on the equivalence relations papers I had been introduced to. This really helped me better understand my own data on the emergence of untrained intraverbal behavior. It also helped me to look carefully at the conditions under which I was working at the time on the emergence of problem-solving in pigeons. These conversations with Sidman helped me gain much more perspective in behavior analysis research. I can remember him in the audience while I was doing my presentation on problem-solving in Liège, in 1988, being just there, making eye-contact with me during the presentation, which gave me peace in the middle of presenting controversial data regarding the pigeon formally speaking “insight” in “problem-solving.” But this is another story.

Beyond academic interactions, Sidman was that kind of person with a broad array of interests that made it possible for us to talk about many different issues and to gain perspectives in many diverse areas. I specially remember his attentive, caring, and loving way of readings stories to my 6-year-old daughter in his Boston home that kept her so absorbed, a very nice picture. It was very easy to feel that you were in front of a person who cares about you and about the work you are doing.

The impact of Sidman's lessons has been present in my lab as a light in the back that has guided the way of designing, implementing, analyzing, and interpreting data, either in experimental or clinical work. Sometimes the impact has been more conspicuous, such as with the first studies on errorless discrimination procedures for establishing complex discriminations as intraverbals, and the series of early equivalence studies we conducted in the middle 1980's. And later, on studies combining avoidance and relational responding; or those aimed at establishing derived symmetrical and equivalence responding in babies; or on the analysis of analogical reasoning based on equivalence; or on experiments to produce untrained functions to stimuli in relations other than equivalence, such as temporal and hierarchical relations. Overall, Sidman's contributions has been -and it is- an overaching influence, a foundation for the experimental manipulation of variables and the perspective from which to analyze the results either in basic and applied preparations.

In concluding, Sidman's experimental studies and *Tactics* are the compound context in his trajectory of analyzing behavior. His observations and curious interest in looking for common factors, instead of differences, was a constant early on, as he precisely acknowledged. Sidman's trajectory is a gift for the experimental analysis of behavior. His emphasis on generating the conditions for students to directly experience the contingencies of manipulating variables, so that their thoughts or rules and their actions were shaped in such context, is extraordinary. Sooner or later, some of these students will connect unconnected points in the behavioral field and will provide more light to the horizon of behavioral science. Because science is no more and no less than the system built through the behaviors of particular human beings, and because human behavior is fortunately the kind of natural phenomenon that shows variability, then it will provide examples of insight and darkness during the inquiry process. From time to time, this behavioral variability will make a change, as if touching the heart of something unknown, or as if opening a door that was previously closed, or as if projecting light on an area and finding a door that nobody knew was there. He was one of the scientists with these characteristics. In a nutshell, Sidman was a master in the experimental analysis of behavior, and a star that has sparkled for years. Hopefully, his legacy will continue to shine and more insights in behavioral science will probably occur.

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