The Emergence Of B F Skinner's Theory Of Operant Behavior: A Case Study In The History And Philosophy Of Science

Kristjan Gudmundsson

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LA THÈSE A ÉTÉ MICROFILMÉE TELLE QUE NOUS L'AVONS REÇUE
THE EMERGENCE OF B.F. SKINNER'S THEORY OF OPERANT BEHAVIOR:
A CASE STUDY IN THE HISTORY AND PHILOSOPHY OF SCIENCE

by

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Submitted in partial fulfillment
of the requirements for the degree of
Doctor of Philosophy

Faculty of Graduate Studies
The University of Western Ontario
London, Ontario
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ABSTRACT

This thesis is a case study in the history and philosophy of science concerned with the emergence of Skinner's theory of operant behavior. During his graduate work in experimental psychology at Harvard University during 1928-31 Skinner became committed to the reflex tradition by way of an analogical extension of the experimental problem/solutions of Sherrington and Pavlov, in the sense of Kuhn's original concept of the exemplar. The reflex tradition however, is not the monotheoretic entity Kuhn makes the paradigm out to be, but is rather a global theory in the sense of Laudan's research tradition, understood as a succession of individual theories, that along with certain commitments, ontological and methodological, constrain the possible theoretical framework of the committed scientist. Kuhn's and Laudan's models of scientific problems are examined by a thorough examination of Skinner's experimental work from 1932-38, and though Kuhn's three types of problems, that I call the extender, the predictor, and the articulator, are seen to be of explanatory value, there are also discernible problems of a non-empirical kind, in the sense of Laudan's internal and external conceptual problems. Finally, it is argued through an examination of Skinner's debate with Konorski and Miller, that operant theory did not emerge as an answer to a state of growing crisis in the field, but that the converse is true. I argue that Skinner's
experimental work is best understood as the continual attempt to solve the definition problem of the elicitation assumption and the measurement problem of one-trial conditioning. By 1936 Skinner has solved both of these problems and once he extends that solution to verbal behavior, I argue that we have operant theory in embryo. As this happened before the debate with Kornorski and Miller, the inevitable conclusion is that the emergence of a novel theory within normal science preceded the crisis in the field, caused the crisis and turned normal science revolutionary.
ACKNOWLEDGEMENTS

I would like to take this opportunity to thank my advisory committee: Professors James J. Leach and especially Ausonio A. Marras for valuable advice and criticism of my earlier work. I am especially indebted to my chief advisor, John M. Nicholas, for penetrating and detailed criticisms at every stage of this thesis, that together with advice and encouragement has made this work a rewarding experience. I would also like to thank my colleagues, Malcolm Forster and Kai Hahlowg, for many interesting discussions, and last but not least, my wife, Margrét Jóhannsdóttir, for temporarily relaxing the requirements of equal rights. Without her support and encouragement this thesis would never have been completed.
ABBREVIATIONS

For the sake of convenience I have abbreviated Skinner's books in the following way: (based on Skinner's own abbreviations in About Behaviorism, 1974, 279):

BO  The Behavior of Organisms: An Experimental Analysis, 1938.

SHR  Science and Human Behavior, 1953.

VB  Verbal Behavior, 1957.

COR  Contingencies of Reinforcement: A Theoretical Analysis; 1969


AB  About Behaviorism, 1974.


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PART ONE -- THE EXPERIMENTAL STAGE: 1928-32

PLAN OF THE CAMPAIGN FOR THE YEARS 30-60

1. EXPERIMENTAL DESCRIPTION OF BEHAVIOR. Continue along present lines. Properties of conditioning, extinction, drives, emotions, etc. No surrender to the physiology of the central nervous system. Publish.

2. BEHAVIORISM vs. PSYCHOLOGY. Support behavioristic methodology throughout. Operational definitions of all psychological concepts. Don't publish much.

3. THEORIES OF KNOWLEDGE (scientific only). Definitions of concepts in terms of behavior. A descriptive science of what happens when people think. Relate to experimental work. Include a theory of meaning. Publish late.

4. THEORIES OF KNOWLEDGE (non-scientific). Literary criticism. Behavioristic theory of creation. Publish very late if at all.

These are in the order of their importance, although 2 and 3 are about equal. By far the greater bulk of time should go on 1.

Plan for the years 60- (?)

(These are beyond my present control.)

B.F. Skinner, NOTE written Nov. 17, 1932
CHAPTER ONE

SKINNER AND THE REFLEX TRADITION
1. INTRODUCTION

1.1. Though little studied, the first thirty years of this century should provide the historian of psychology with interesting material. This whole period was characterized by controversies and ferment. Not only were fundamental questions (e.g. of the mind/body relation, the nature of consciousness, the basic unit of analysis, etc.) common in the literature, but so were the systematic answers: Structuralism, Functionalism, Behaviorism, Gestalt, and in part, Psychoanalysis. Though attacks, counterattacks, polemic and controversy filled the journals, these different and systematic answers were just that—different.

Gestalt psychology, as an example, was not much more than the systematic attempt to build up a new global theory on the basis of a few wholistic observations concerning the nature of perception. Psychoanalysis, on the other hand, was largely based on a few clinical cases of hysteria using mostly hypnosis, and Behaviorism, in the hands of Watson, was nothing more than a polemic— the promise of an objective psychology—mostly based on the inadequacies of the other approaches. Functionalism, largely through the impetus of Darwin's theory of evolution, was never a clear school of thought, and was in fact, chiefly important as a reaction against the dominant school of thought, Structuralism.

Historians have referred to this period as the Age of Schools, and with good reason, for not only were most of
these schools of thought put forward and contrasted during this period, but they were also fighting for power and prominence so typical of young, immature, and pre-paradigmatic sciences.

1.2. Wilhelm Wundt had started all this with his establishment of the first experimental laboratory of psychology in Leipzig in 1879. Psychology was to be the experimental science of controlled introspection, he had insisted, concerned with the discovery of and mechanisms, of the fundamental units of the mind. By the turn of the century however, introspection had already been heavily criticized, both as a method and metaphysics.

Due to the excitement of a promise of a science of mental life, others had followed suit and established their own research centers. Usually that was preceded by a period of study under Wundt in Leipzig, and one such apprentice, the Englishman E.B. Titchener, moved to North America, became a professor of psychology at Cornell, and was, by 1900 the most arduous defender of Wundtian structural psychology.

1.3. One of Titchener's best students, E.G. Boring, later became the head of the department of psychology at Harvard University. When Boring joined that department, psychology was not regarded as an independent science at Harvard, and was in fact, within the department of philosophy. Boring tried to separate the two, but when that happened he was not as enthusiastic about the prospects of introspective psychology as either Wundt or Titchener had been. Boring in
fact, did not play any major role in the development of scientific psychology at Harvard, although he was quite diligent in administrative matters. The department was in fact, quite diversified during those years, and though major emphasis had been on recruiting promising young psychologists, mostly from Europe, the department somehow managed to lose them pretty quickly.

H. Münsterberg, for example, had joined the department after working under Wundt in Leipzig, but he died in 1922. William James had for a time changed his title to "Professor of Psychology", but changed it back to "Professor of Philosophy" in 1897. Robert Yerkes joined the department before the war, but war time work on intelligence tests led him elsewhere. William McDougall was brought in from England to strengthen the department but he went to Duke University in 1927.

Quite an impressive list of psychologists, if they only would have stayed at Harvard. Later the department brought in Gordon Allport and Karl Lashley, but neither stayed for long. In fact, it was only with the advent of a certain B.F. Skinner in 1948 that Harvard finally got an important figure to stay, for in about ten years Skinner's brand of psychology -- operant behaviorism -- had become the dominant force in North America -- not only winning over his behavioristic rivals, Tolman, Hull, and Spence, but dominating most of experimental psychology as well.

1:4. Though diversified, the department of psychology at Harvard in the 1920's did not produce much experimental
work, and it centered in fact, around a few relatively uncontroversial areas. Boring's work was mostly administrative, and though he was an authority on experimental psychology (e.g. his *History of Experimental Psychology*, 1929), his self-characterization as "psychologist at large" is much to the point. He did not do much original research himself, although he had something to say about anyone else's.

Apart from Boring, professors Carroll Pratt and Leonard Troland taught experimental psychology. The latter was a specialist in vision, and was instrumental in the invention of Technicolor. The former studied such things as delayed reactions, or the capacity to tell the difference between two stimuli, measured as reaction time. Other members of the department in the 1920's were the curious John Gilbert Beebe-Center and over in the clinic was Henry Murray. The latter was probably the only experimentally minded professor in Harvard at that time, and was interested in such diverse things as thematic apperception, and unconscious motivation.

Harvard was not an impressive department by any means during those years, and was curiously lacking in any such new developments as concerned psychoanalysis, gestalt-theory, Watson's behaviorism, the moderate position of functionalism, or even the new ideas from physiology, such as Pavlov's work in the Soviet Union on the conditioned salivary reflex, or Sherrington's research in England on the reflexes of the spinal cord.

1.5. Still Harvard did always boast of an experimentally
minded department in psychology, but did neither have many professors, nor any one in particular that could attract new graduate students. Jointly, the department covered such areas as memory and forgetting, concept formation, language development, maze learning, habit formation, transfer of training, problem-box solution, and delayed reaction capacity.

The instruments were primarily the maze (T-maze mostly), the memory drum, nonsense syllables, and the problem-box. The principal measures were reaction time, absolute judgement, thermal sensitivity, and time and errors per trial. The laws referred to were mostly concerned with averaged group results in memory and habit formation (e.g. frequency, recency, vividness, and contiguity).

1.6. This was the situation at Harvard when a certain formerly literary minded person from the coal mining town of Scranton, Pennsylvania, named Burrhus Frederic Skinner joined the department as a graduate student. Having given up on literature Skinner was looking for a hard experimental science of behavior, but unfortunately he did not find any, at least not in the department of psychology. He did stay on though, mostly because of a fellow student, Fred S. Keller, and when he realized what was going on over in physiology, he immediately knew he had found what he was looking for. Before long he was making plans for a thoroughly scientific psychology and was soon ready to substitute the existing method and metaphysic of contemporary psychology with the very new and exciting prospect of a science through reflex physiology.
1.7. It is the purpose of this thesis to examine this attempt, for though it starts out as a rather bold, but still predictable extension of Pavlov's and Sherrington's concept of the reflex to the field of psychology, it is clear that 10 years later Skinner has somehow come up with a new theory -- the theory of operant behavior -- that is quite different and indeed the denial of any reflex theory. What happened to Skinner's early prospect of a science during the years 1928-38, that makes such a conceptual change possible? How can Skinner say with confidence that all behavior is reflexive, and only a few years and a number of experiments later (Skinner published no less than 30 experimental papers from 1931-38), that the reflex captures only a small part of the behavior of the whole organism, and that the behavior he is interested in is operant, or non-reflexive, behavior?

It is the purpose of this thesis to answer these and other similar questions.

1.8. In chapter one the early influences on Skinner are discussed, and the point made that when he entered graduate school at Harvard in 1928, he was looking for a hard-core empirical and experimental science. He did not find that however, at Harvard, that is, not in psychology. But by his thesis in 1931 he nevertheless had a promise of a science of behavior, on the basis of a research tradition of reflex physiology. The work of Sherrington and Pavlov on the reflex characteristics of specific organs, was not readily applicable to the whole organism, but with the added impetus
of the tropistic doctrine of Loeb and Crozier, Skinner thought he had found a way around that problem.

An important part of chapter one is the argument that the research tradition of reflex physiology cannot as such, direct the scientist to specific experiments, but functions much rather as a set of constraints. These constraints are methodological and ontological boundaries the scientist cannot exceed without violating the tradition he has committed himself to. These are very strong commitments, and I argue further that they only become the commitments of B.F. Skinner once Sherrington and Pavlov have served him as exemplars, the successive application of which determine Skinner's research tradition. On the basis of these exemplars, the application of which depends on the successful extension of Pavlov's and Sherrington's problem/solutions to a new experimental context, Skinner constructs his own sketch of a research program, which is to be understood as his individual exemplification of the research tradition of reflex physiology.

1.9. In chapter two the emphasis shifts to Skinner's attempt to carry out his individual research program, during the years 1932-34, when he published no less than 13 experimental papers. In reviewing the earliest of these I emphasize a number of specific changes in definition, apparatus, and measurement, that jointly have the effect of increasing Skinner's worries, although each and every one of these changes was originally introduced in response to
one worry or another.

Kuhn's and Laudan's problem-oriented models of scientific change are introduced, and I argue that apart from Kuhn's three types of experimental and theoretical problems, we can also detect non-empirical or conceptual problems in Skinner's experimental work. These empirical and conceptual problems do not disappear, but develop and transform due to Skinner's curious lack of incentive to solve them. I further argue that Skinner is increasingly worried about two major problems, the problem of the definition of the reflex and the problem of how to measure the strength of a reflex.

These problems are further amplified due to a few of Skinner's discoveries, especially of one-trial conditioning, of extinction as the opposite of conditioning, and finally of discrimination as the concurrent process of conditioning and extinction. Thus we see Skinner on the one hand increasingly worried about the role of the stimulus in the process of conditioning (the elicitation problem of the definition of the reflex as a necessary relation of stimulus and response), and about the correct measure of reflex strength in the new -- one-trial -- type of conditioning (the measurement problem of the two types of conditioning).

Chapter two ends with a preliminary discussion of crisis and incommensurability, for on the one hand we see Skinner moving back to reflex physiology proper only to find that attempt completely a waste of time as physiologists were not interested in the problems he was encountering, and
On the other we see ever more clearly that Boring's criticism of Skinner's thesis, that he was not using the reflex in any of the customary senses, is very much to the point. The issues raised are Kuhn's thesis of a crisis as a prerequisite of any change from normal science to revolutionary, and his equally controversial thesis, that we often have, in the history of science, the same term being used in different senses, such that a direct and rational comparison between the two theories is impossible. These issues are only raised in chapter two, and an answer made dependent on further developments in chapter three.

1.10. It is only in 1935 that we see Skinner actively engaged in solving his two major problems, the empirical problem of the correct measure for one-trial conditioning, and the conceptual problem of the definition of the reflex with the underlying assumption of elicitation. Chapter three starts with an analysis of Skinner's experimental papers from 1935 to 37 that lead up to the publication of his first book, The Behavior of Organisms, in 1938. On the one hand we see Skinner writing a long paper attempting to clarify further his conception of stimuli and responses, and thus engaging in the problem of the definition of the reflex. On the other hand he writes his second paper on two types of conditioning.

That second paper however, does not so much help Skinner in distinguishing between the two types of conditioning he is arguing for, as highlight his elicitation problem.
Instead of making discriminative stimuli the defining characteristic of the new type of conditioning, he ignores that in his distinction between the two types, and is consequently forced to create still another type of conditioning to account for the role of discriminative stimuli, and he calls it, for lack of a better name, the pseudo-reflex.

Interestingly enough Skinner does not seem to see any relation between his two major problems, for in that latter paper he is still not ready to give up the elicitation assumption and use instead discriminative stimuli, although he has virtually given up that same assumption concerning the other problem. Thus he says on the one hand, that we should emphasize defining properties of stimuli, instead of defining them by "the very doubtful property of its ability to elicit", while in the latter paper he is not ready to say anything like that.

Still quite a change has occurred due to these two papers, and it is best understood on the basis of some quite interesting changes Skinner is forced to make in his research program.

Another very important argument in chapter three concerns the role of external or social factors in scientific change. There are two aspects to this. On the one hand it can be argued, on purely internal grounds, that Skinner's move away from the reflex tradition, can be understood by purely internal reasons (i.e. reasons that have only to do with
conceptual or cognitive factors within his science). This whole thesis is predominantly of this kind, as it is, so to speak, an attempt to get inside the scientist's head for detection of the worries, problems, and intentions that guide his behavior. But to concentrate solely on internal factors carries with it considerable dangers, as historical work is unavoidably carried out in hindsight, so that any change that occurs in the early development of a theory always tends to be seen in terms of the final product -- operant theory proper, in this case -- which constantly stares the historian in the face, not only because of its temporal approximation, but also, in this case study at least, because of the eventual success of that theory in the 1950's and 60's.

To counter this and to show how internal and external reasons can be compatible, I emphasize that Skinner can also, and even more so, be seen to move away from the reflex tradition for reasons strictly external to his science. I show that Skinner did in fact, come to see much more prospect of financial support, power and prestige, if he gave up on the reflex tradition, and that for this reason he intentionally associated with a person he disliked both professionally and personally, while at the same time hailing him in a book review and criticizing his opponent -- the only person Skinner had been in close contact with, professionally and personally, all through his scientific career.

1.11. In 1936 Skinner writes a paper in the area of verbal behavior, that at first sight seems to have little or no
connection to any of his experimental work. I argue that there is in fact a direct and clear-cut relation between this work and his experimental work proper, and point out that this is the beginning of what I call Skinner's extrapolation stage. There are two ways to understand Skinner's excursion into verbal behavior. On the one hand we learn that this work is based on the experimental work of Pavlov and Sherrington, in the same sense as before, as he uses their problem/solutions in a new area -- verbal behavior. Thus he mimics Pavlov's imitation reflex (no pun intended) along with Sherrington's summation principle to study the properties of latent speech. But latent speech, says Skinner, is an excellent device to the study of responses that occur without any eliciting stimuli, and this is the second way to understand the relation between this work and Skinner's experimental work proper. In fact this work concentrates, as does so much of Skinner's work in 1934-36, on the role of the stimulus, and is furthermore the first time he denies the elicitation assumption altogether, thereby violating the underlying and basic assumption of the whole reflex tradition.

1.12. I argue further that once Skinner denies the elicitation assumption he is by necessity abandoning the reflex tradition -- the same tradition he had committed himself to by the exemplar use of Sherrington and Pavlov. He is furthermore developing an altogether new theory, for the reflex expresses the necessity of the stimulus-response relation, but by denying that Skinner has said both, that
stimuli can occur without responses and that responses can occur
without stimuli. Both statements break down the reflex as a
unit of analysis, but the latter statement is more important
for Skinner, as it constitutes in effect operant theory in
embryo.

When two Polish physiologists, Konorski and Miller,
criticize Skinner for having incorrectly distinguished
between the two types of conditioning, Skinner replies by
saying he agrees with much of their criticism, and that he
is now willing to go even further as he has already done
in "a work now in progress". This turns out to be a
reference to The Behavior of Organisms, where Skinner uses
the term operant, as he does for the first time in his
answer to Konorski and Miller, to mean responses that can
occur without any elicitative stimuli. The interesting
thing about this answer is the fact that it infuriates
Konorski and Miller who see no reason to introduce the term
"operant", for each and every response, they insist, "can be
said to have" its elicitative stimulus, and that this fact
"follows from general premises concerning the physiology of
the nervous system".

My basic argument here is that while Konorski and Miller
are not prepared to abandon the elicitation assumption,
Skinner is, for what he sees as an anomaly they see as a
more problem. I argue further that Konorski remained a
Pavlovian to the end, or that he tried, in his later work,
to develop further and amend the theories of his predecessors,
Sherrington and Pavlov.

1.13. Unlike Konorski and Miller, Skinner was ready to deny the elicitation assumption (and thereby abandon the whole reflex tradition). He did so because he had already solved that problem in another context (in his work on verbal behavior), for there he had explicitly studied responses that by the very nature of the experiment could not be elicited by any stimulus in the immediate environment.

I argue further that this runs counter to Kuhn's view of scientific revolutions as he insists that normal science turns revolutionary only on the condition of a severe and prior crisis. But as this case study shows, the crisis came after Skinner had denied the elicitation assumption, not before. The fact is that Skinner had already solved this problem within normal science by insisting that verbal responses were due to their own relative strength but not due to elicitative stimuli, and that only after extending that solution to conditioning did Konorski and Miller strongly object to Skinner's position. They said that there was no evidence for the existence of operant behavior, and that it furthermore violated a general premise of the reflex tradition. Only then do we have a crisis and only then do we have anything in Skinner's emerging theory that runs counter to Pavlov.

1.14. On the whole this case study of the emergence of operant theory is critical of Kuhn's monotheoretic account of scientific revolutions. It must be emphasized however,
that there is much of value in Kuhn's model, like his emphasis on the three types of empirical problems the scientist is typically preoccupied with during normal science. Essential to Kuhn's model furthermore, is his original idea of a paradigm in the sense of the exemplar, and it too proved to be of major importance in this case study. Thus I argued that the exemplar plays an essential role in the way the scientist learns his trade, and that only after the scientist has successfully extended the problem/solution of his predecessors to a new context, can the research tradition constrain his theory building.

But the research tradition is not the monotheoretic entity Kuhn would insist it is, for neither is it a sole theory dogmatically accepted throughout the whole reflex community, nor is it an unchanging dogma. The reflex tradition is nothing but a set of constraints, consisting of all those theories, like Sherrington's and Pavlov's, that have the reflex as their unit of analysis and share a commitment to the elicitation assumption concerning the necessary correlation of stimuli and responses. That tradition does change I argue, in the sense of being a succession of theories, and Kornorski provides an example as he attempts to build a new theory on the basis of Sherrington and Pavlov, by expanding and correcting his predecessors on a number of specific points, while all the time remaining in the reflex tradition.

The second point in favor of Laudan's pluralistic account
is his insistence that theories are answers to unsolved problems. While I argue that an extension of the problem/solution to a new area is also needed before a solved problem can become a theory, the point is still that Skinner's solution to the elicitation problem in the context of verbal behavior is the beginnings of a theory, or operant theory in embryo. Once Skinner extends that solution to other areas, I further argue, does he have a fully-fledged theory of operant behavior, and that also explains why Konorski and Miller react so violently to Skinner, once he answers them in terms of his emerging theory. As Laudan correctly insists, it is only when a problem has been solved by a particular theory, operant theory in this case, that the previously unsolved problem becomes a crisis-causing anomaly for other theories, like Pavlov's and Konorski's theories of conditioning.

2. B.F. SKINNER

2.1. Burrhus Frederic Skinner was born on March 20, 1904 in Susquehanna, North Eastern Pennsylvania. His father was a lawyer, but his mother was a stronger influence on young Skinner. He had a brother about two years younger, who died suddenly at the age of 16, from what was thought to have been cerebral hemorrhage.

Skinner lived a normal childhood in Susquehanna. His family moved to the coal mining town of Scranton, Pennsylvania just before he left for college -- Hamilton College -- a small liberal arts college in Clinton, New York. During his college
years Skinner seems to have been mostly interested in music and literature.

Skinner graduated from Hamilton College in the spring of 1926, apparently determined to become a writer. His father was not exactly enthusiastic about the idea, but when young Skinner got a letter from Robert Frost (a noted literary figure at that time) encouraging him to pursue that career, Skinner got all the reinforcement he needed.

2.2. In this letter Frost tries to answer Skinner's plea to tell him "if there is enough in the stories to warrant your going on". After telling Skinner his view that the most essential aspect of writing is the motivation behind it, he says that there are "real niceties of observation" in one of the stories, and that "you've done them to a shade" (ibid).

2.3. As we will see in a moment, this remark about niceties of observation later became important to Skinner, for when he turns away from literature, this is one of the reasons he chooses psychology over any other field as his new profession.

Frost's overall impression of Skinner's work must have been the main immediate effect though, as he says that "I ought to say you have the touch of art. The work is clear run., You're worth twice anyone else I have seen in prose this year" (ibid). This is high praise and Skinner moved back to his parent's home in Scranton, intending to write a novel, using the town as the setting for the story.

2.4. In his autobiography Skinner has characterized the year he devoted to his literary ambitions as "The Dark Year"
He soon found that writing a major novel was too much, but even at writing short stories and poems, he found it hard to keep his concentration.

"The truth was", he explains in retrospect, that "I had no reason to write anything. I had nothing to say and nothing about my life was making any change in that condition (ibid, 264). It became clear that the year spent to become a writer, was a "terrible mistake" (ibid). He considered other means of survival (e.g. chicken farming, being a chauffeur, and landscape architecture -- see PML, 283), but to no avail.

It was not until his father suggested a job to him, that he found something to write about.

2.5. The job was to write a digest of decisions that had been reached by the Board of Conciliation in its settlement of grievances brought by coal companies and unions. To provide company lawyers with a convenient reference, young Skinner read and summarized 1148 grievances. Not surprisingly he found the job boring, but got it done, and it was published by the Skinner's as A Digest of Decisions of the Anthracite Board of Conciliation (see PML, 286).

Writing the book also had its rewards. Not only did Skinner get to exercise his analytic skill in summarizing and classifying facts, but the sum of money made on the book enabled him to consider "fresh prospects for a career" (PML, 287). He began to consider graduate work, but in what field?

2.6. Becoming a writer did not appeal to him anymore, for -- as he said -- he had no reason to write anything.
Skinner seems at this point to have made quite an abrupt decision. I emphasize the fact that this was a sudden and unexpected decision, for the reason it makes the consideration of early scholarly influences on Skinner all the more important -- as we will indeed see in a moment.

Apparently Skinner had no clear idea at that time, where to go from here, but the clue came when he asked himself the question "I had apparently failed as a writer but was it not possible that literature had failed me as a method?" (PML, 291). He explains this by saying that he was "interested in human behavior, but ... had been investigating it in the wrong way" (ibid). As pointed out above, Frost's comment that his stories revealed some 'niceties of observation' and 'were done to a shade', probably had some effect here.

2.7. In the autobiography we further learn that a friend told him science is the art of the twentieth century, "and I believed him", Skinner quite frankly admits. "Literature as an art form was dead; I would turn to science" (PML, 291; underlining added). To see just how sudden this decision was, notice how Skinner expresses himself above. Even more telling though, is his comment that the "science that concerned itself with behavior (animal or human) was said to be psychology, about which I knew very little" (PML, 292; underlining added). Consequently Skinner quite simply proceeded to pick up books on this subject of 'psychology', and his choice of books is quite interesting.
3. EARLY INFLUENCES

3.1. The first influence, it seems, was the German physiologist Jacques Loeb. In college Skinner had been interested in biology and related fields. He had dissected some unfortunate cats preserved in formaldehyde, and made, he says, "quite acceptable slides of a chick and a pig embryo" (PML, 295). More interestingly, the teacher had called his attention to the writings of Loeb. "I read Physiology of the Brain and Comparative Psychology and The Organism as a Whole" (Loeb, 1916), Skinner reports, "and was quite impressed by the concept of tropism or forced movement" (ibid).

3.2. Loeb's work can be seen as a direct reaction to the dominant school of thought in comparative psychology. His aim was to explain in purely mechanical terms the apparently random or spontaneous movements of various organisms, his prime example being the movement of a plant towards the source of light. Whereas the prevailing mood of the day was to regard such movements as explainable by the attribution of human characteristics to the animal (Romanes had, for instance, said that the moth flew into the flame because of its curiosity), Loeb showed that a much simpler explanation was possible. The plant's movement could most simply be seen as an oriented movement in an energy field, i.e. as a forced or tropistic movement.

3.3. Loeb's influence on Skinner is at least twofold. He showed, first of all, how behavior that appeared random or capricious, was actually forced or tropistic. And notice
that this involves mechanistic explanations devoid of any reference to mentalistic or physiological terminology. Related to this is Loeb's questionable method of using experimental results obtained with simple organisms (mostly plants in fact) to reinforce his strong views concerning strict and universal determinism. As we will see later, it strikes one that this criticism can also be levelled against Skinner's extended theory -- as indeed Chomsky has done (see section 8.10 of chapter three).

Secondly, and more importantly, Loeb insisted that various organisms could be studied as a whole, with a unit of behavior such as tropism. This is very much in line with the early prospect of a science Skinner was soon to develop, although Skinner rejected that particular unit in favor of a more recent one -- the reflex -- for reasons that we will later be in a better position to comprehend.

3.4. But probably the crucial influence came through "that wonderful magazine the DIAL" (SB, 10; emphasis in original) to which Skinner subscribed in his literary days. There he had read, among other things, Bertrand Russell's review of a book that later would have great significance for Skinner, Ogden and Richards' *The Meaning of Meaning* (1923). In the review (1926) Russell praises the author's behavioristic leanings (they argued for instance that such mentalistic words as "ideas" and "direct apprehending" were not essential for a theory of meaning), and although he criticizes them on some details, Russell's main objection
was that their theory of meaning was not behavioristic enough for his own tastes. Russell referred to J.B. Watson, "whose latest book", he says, "I consider massively impressive" (Russell, 1927, 121). After reading the review Skinner bought Watson's book, and a year or so later, Russell's Philosophy. Curiously enough Skinner was much more impressed by Russell than Watson. Russell's book, says Skinner, "begins with a careful statement of several epistemological issues considerably more sophisticated than anything of Watson's (SB, 10; underlining added).

He also says that in the later chapters of the book, Russell 'rehashes' his views on the nature of the physical universe, "and I stopped reading when I reached it. I therefore missed the last third", Skinner quite frankly admits, "in which Russell undertakes to disprove the behaviorist part by talking about 'man from within!'" (SB, 11).

Apparently Skinner became quite interested in both Russell and Watson. He reports that he found them both 'refreshing', in how quickly they got around to facts (see PML, 298). But what facts? or rather, what kind of facts? The answer is clear in this quote from Russell:

The scientific study of learning in animals is a very recent growth; it may almost be regarded as beginning with Thorndike's Animal Intelligence, which he published in 1911." (Russell, 1927, 24)

3.5. The influence here -- like before -- is twofold. Let us begin with the one that is more concrete, and then go on to see what general influence all these writers (Loeb included), had on Skinner.
As was pointed out before, Skinner objects to Russell that Thorndike's experimental work on the associative processes in animals began with the monograph from 1898 rather than 1911. But while that is true, it is equally clear that the work from 1898 is rather elementary, as even Thorndike himself admits. Further, Thorndike formulated the law of effect only in the latter work, though he can be said to come close -- as in expressions like this one from the 1898 monograph:

I have spoken all along of the connection between the situation and a certain impulse and act being stamped in when pleasure results from the act and stamped out when it doesn't. (ibid, 147)

It is only in the later chapters of Animal Intelligence: Experimental Studies from 1911 (in a chapter called "Laws and Hypotheses for Behavior") that Thorndike is willing to speculate on laws governing behavior -- and still he calls them only 'provisional':

The Law of Effect is that: Of several responses made to the same situation, those which are accompanied or closely followed by satisfaction to the animal will, other things being equal, be more firmly connected with the situation, so that, when it recurs, they will be more likely to recur; those which are accompanied or closely followed by discomfort to the animal will, other things being equal, have their connections with that situation weakened, so that when it recurs, they will be less likely to recur. The greater the satisfaction or discomfort, the greater the strengthening or weakening of the bond.

The Law of Exercise is that: Any response to a situation will, other things being equal, be more strongly connected with the situation in proportion to the number of times it has been connected with that situation and to the average vigour and duration of the connections (p. 244; underlining added)

3.6. As one can never actually determine what brings
satisfaction or discomfort to animals, Russell suggests that Thorndike's laws should be reformulated. Thus one should say that the animal "tends to behave so as to [make certain results] recur" (Russell, 1927, 26; underlining added). This makes the definitions objective, he thinks, but he still prefers Watson's single principle of learned reactions. Russell restates that principle as follows:

WHEN THE BODY OF AN ANIMAL OR HUMAN BEING HAS BEEN EXPOSED SUFFICIENTLY OFTEN TO TWO ROUGHLY SIMULTANEOUS STIMULI, THE EARLIER OF THEM ALONE TENDS TO CALL OUT THE RESPONSE PREVIOUSLY CALLED OUT BY THE OTHER. (ibid; emphasis in original)

The reason Russell thinks Watson's principle (read: Pavlov's principle) much better is that it is objective and becomes verifiable over a much larger field than the older principle of Thorndike's "owing to the fact that it is movements, not 'ideas', that are to be associated" (Russell, ibid., 26).

Watson's principle is thus, he says, the modern form of the principle of association.

3.7. I have not been able to find this (or any) 'principle of learned reactions' anywhere in Watson. He does talk of habit formation and later of conditioning as principles, but never of any "reaction principles". Indeed reaction, for Watson, is just responding. What principle is Russell then restating? And where did he get it from? If we look back at the principle in question the answer is quite obvious, for the principle is clearly Pavlov's conditioned reflex. And the reason Russell sees himself as restating a principle of Watson probably derives from the fact that Pavlov's main
work had not been translated from the Russian yet, although many psychologists were already well aware of Pavlov's theory. Indeed, Watson had talked explicitly of Pavlov in his presidential address to the American Psychological Association in 1915. "The conditional salivary reflex", says Watson there, "is well known in this country, thanks to the summaries of the researches of Pawlow's laboratory..." (p. 92).

3.8. It is absolutely crucial, at this point, to realize what this amounts to. Movements, not ideas, say Russell and Watson, and movement is behavior. So what is happening here, is that Watson and Russell have a behavioral principle, or were trying, as Skinner expresses it in his autobiography:

> to interpret the Law of Effect as an example of the substitution of stimuli, the principle being, of course, Pavlov's principle of the conditioned reflex. (PML, 299; underlining added)

Interestingly enough, Skinner adds to this that it would be a long time before he saw the mistake Russell and Watson were making (i.e. in equating the law of effect with Pavlov's conditioned reflex). As will become apparent later, this was a mistake "because the course of psychology as a science was to follow the unproductive path of a stimulus-response psychology for many years" (PML, 299).

3.9. Basic to my interpretation of Skinner's experimental stage is that he is not only talking about other psychologists, but also of himself. For if my interpretation is correct, Skinner is himself working in the reflex tradition (in a sense
that will be spelled out in more detail in the following sections) at least until he explicitly realizes that most behavior is **operant**, but not reflexive.

As we will see -- if I may anticipate a little at this point -- is that in his early prospect of a science (the ontology and methodology of which he will derive from the reflex tradition) Skinner is a strict reflex theorist, and that only in his early and actual experimental research does he begin to move away from that tradition. Indeed, Skinner's persistent and unsuccessful attempts (that first show themselves as mere worries) to show that all behavior is reflexive, will eventually lead him to the discovery of the operant, but a consideration of that, at this point, would take us too far afield.

3.10. Apart from this ultimately damaging influence on Skinner to interpret the law of effect in terms of the conditioned reflex, there is another and more basic influence that needs to be considered. When Russell via Watson via Pavlov extends the domain of the law of effect to movements, the underlying assumption is that behavior **per se** can be a subject matter of scientific investigation. This simple and much ignored point is the second and more general influence I have been talking about. As I will argue for later in this chapter, when Skinner attempts to extend the reflex to the whole organism, he is accepting the reflex tradition as his **tradition**, with all that follows as a consequence. The most obvious consequence is experimental
technique. In his attempt to extend the concept of the reflex to the whole, intact organism, Skinner does so by mimicking experiments by Pavlov, Sherrington, Magnus, as the main theorists of that tradition. But in doing so the reflex tradition does much more than provide Skinner with exemplars. As I will argue for in the next few sections, the reflex research tradition dictates the metaphysics (ontology really), methodology, and most importantly (for this study at least) a specific problem-context. Thus, by accepting the reflex tradition Skinner is committed to a specific methodology (there is a special twist here as Skinner gets much of his methodology from general physiology, but more on that in the next few sections), that dictates the methods available. Secondly, the reflex tradition commits Skinner to a specific ontology, that dictates what entities a science of behavior is about. And finally, the tradition also commits Skinner to the study of specific empirical problems, and what would constitute an acceptable solution to those problems.

3.11. We have seen how various authors influenced young Skinner around the time he gave up on literature and turned to that science called psychology. Before finally deciding on that career, Skinner went back to his old biology teacher from college, asking for advice. He urged Skinner on and gave him a copy of a new translation: **Conditioned Reflexes**, by some Russian physiologist named "Pavloff" (PML, 300). Skinner also got the word that Harvard and Columbia were "the two outstanding departments in psychology" (PML, 301).
Since Watson was no longer teaching (due to his affair with a graduate student and a subsequent divorce), Skinner applied for graduate study to the department of philosophy and psychology at Harvard, and was accepted.

4. THE DEDICATED BEHAVIORIST

4.1. Skinner started graduate work at Harvard in the fall of 1928. During that summer he had followed up on his new interest by reading I.P. Pavlov’s *Conditioned Reflexes* (1927) and the Watson’s version of Dr. Spock: *Psychological Care of the Infant and Child* (J.B. Watson and C.A. Watson, 1928). Skinner was clearly a behaviorist (in intention at least), before coming to Harvard, but he soon found that the Harvard psychology department had little to offer a behaviorist by way of reinforcements. In fact, Skinner claims in his autobiography that, if not for the influence of fellow students, he would not have “resisted the mentalistic predispositions of the department” (SB, 14). What this indicates first of all, is that there was no -- in a word -- behavioristically minded professor in the department at that time. But what it also says, I think, is that Skinner was not much impressed by the professors, even in their own area of specialty, for as he often says in his autobiography, he took a bare minimum of courses in psychology proper, concentrating on research courses and soon also on physiology. In fact, it was not until he took courses in physiology that he found what he was looking for. He signed up for one, and found to his amazement, that “it actually discussed the conditioned
reflexes of Pavlov!" (SB, 17). Also discussed in that course was Rudolph Magnus' Körperstellung (1924), which Skinner struggled through in the German. He says he was more impressed by Magnus than by Pavlov, as "Pavlov's reflexes, conditioned and unconditioned, were glandular secretions, but here was physical MOVEMENT, something much closer to what was ordinarily called behavior" (SB, 17; emphasis in original, underlining added).

Actually Skinner is somewhat inconsistent here for though it is undoubtedly correct that Magnus' concept was closer to the ordinary term of behavior than Pavlov's was, Skinner nevertheless soon began to see Pavlov's contribution in the same light. The correct description is probably that Magnus' treatment of "behavior" had this influence on Skinner, that he was able to regard Pavlov's results as concerning "pure behavior". The reason I say this is that Skinner made very little use of Magnus (although he did try to make use of his postural reflex experiments and tried to measure temperature coefficients -- see section 5.5. below -- but both of these attempts failed), and was only successful in mimicking the experiments of Pavlov and Sherrington. Let us examine therefore, the prime experiments of Pavlov and Sherrington, for as we will see in the next two sections, Skinner made use -- not so much of their laws or principles -- but much rather of their exemplars -- in the sense that he mimicked their experiments so that he could extend their problem/solutions to the similar experiments he himself was conducting on
intact organisms.

4.2. Apart from Magnus and Pavlov, the courses Skinner took over in physiology also emphasized the work of Charles S. Sherrington. Skinner says he read his classic: *The Integrative Action of the Nervous System* (Sherrington, 1906) "with enthusiasm" (SB, 17). Sherrington's work was on inborn reflexes (unconditioned reflexes as Pavlov would later call them) like the scratch reflex and the flexion reflex, in cats and dogs. He experimented on, what he called, "spinal-dogs" and "spinal-cats", by which he meant animals under deep anesthesia after he had cut the spinal cord at the neck with "the Sherrington guillotine".

Sherrington was interested in the integrated action of the nervous system — as his title implies, or simpler movements that were not controlled by the higher brain centers. By integrative he meant that in every nervous reaction the animal integrates or "welds together" its behavior as a social unit "from its components, and constitutes it from a mere collection of organs" (Sherrington, 1906, 2). Thus he intended to study together as an integrated whole:

At least three separable structures — an EFFECTOR organ, e.g. gland cells or muscle cells; a conducting nervous path or CONDUCTOR leading to that organ; and an initiating organ or RECEPTOR whence the reaction starts. (ibid, 6; emphasis in original)

The means by which he aimed to do this, he says, was through the "conception of a reflex" (ibid), for it embraced, he says, all these functions above. He never properly defined the reflex however, except by saying it to be a
necessary reaction (most often expressed as the elicitation of reflexes) to a stimulus in the immediate environment. He speaks furthermore of the reflex-arc as his fundamental unit of analysis:

For our purpose the receptor is best included as a part of the nervous system, and so it is convenient to speak of the whole chain of structures -- receptor, conductor, and effector -- as a REFLEX-ARC.

The reflex-arc is the unit mechanism of the nervous system when that system is regarded in its integrative function. THE UNIT REACTION IN NERVOUS INTEGRATION IS THE REFLEX, because every reflex is an integrative reaction and no nervous action short of a reflex is a complete act of integration. (ibid, 7; emphasis in original, underlining added)

4.3. The reflex is thereby the unit of analysis

Sherrington worked with. The means by which he did that was with an electric shock to the organ being studied, e.g. if it was the flexion-reflex then he would send a current to the paw of the animal, and record the properties of the characteristic flexion that followed. Sherrington devised various measures of reflex strength by this method. Conduction in a reflex-arc he "measured by the latent interval between application of stimulus and appearance of end-effect" (ibid, 18). This measure of latency is much higher in the scratch-reflex than in the flexion-reflex. The latter is typically measured by:

the latent period of the flexion-reflex of the 'spinal' dog's hind-leg. The movement of this reflex is a flexion at knee, hip, and ankle. It is easily and regularly evoked by nocuous or electrical stimuli applied to the skin of the limb or to any afferent nerve of the limb. For measurements of the reflex latency I have stimulated with break or make shocks of regular but varied frequency. (ibid, 18)
Magnitude of response and after-discharge he measured by continual stimulation on the paw of the spinal animal, with a varying intensity of the stimulus. The following diagram with Sherrington's own explanations is adapted from The Integrative Action of the Nervous System (1906, 28-29):

![Diagram showing flexion-reflex effect of intensity of stimulus on magnitude of response and on after-discharge.](image)

Records from above downwards:
- Period of stimulation: the stimulus for each reflex was 72 brick shocks at the rate of 40 per sec; the interruptions of the electromagnet in the primary circuit are recorded.
- Myograph curves (I-VII): the vertical area on the myograph curves show the onset (S) and cessation (S') of the stimulus.
- Time in sec.

<table>
<thead>
<tr>
<th>Intensity of stimulus</th>
<th>Measure of reflex</th>
<th>Measure of after-discharge</th>
</tr>
</thead>
<tbody>
<tr>
<td>I</td>
<td>357</td>
<td>94</td>
</tr>
<tr>
<td>II</td>
<td>475</td>
<td>200</td>
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<td>III</td>
<td>690</td>
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<td>V</td>
<td>1900</td>
<td>1277</td>
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<tr>
<td>VI</td>
<td>3000</td>
<td>1765</td>
</tr>
<tr>
<td>VII</td>
<td>350</td>
<td>62</td>
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</tbody>
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Skinner was very much impressed by the rigor and details of Sherrington's work, or the way in which Sherrington was able to isolate his problem, find a unit of analysis, and
how he measured it. The time between the beginning of the shock, wrote Skinner:

and the beginning of the flexion is the 'latency'; after a strong shock, the leg continues to flex for some time in 'afterdischarge'; a stimulus will not elicit a second response during a brief 'refractory phase'; under repeated elicitations responses grow weak in 'reflex fatigue'; and so on. (SB, 18)

Adding with obvious admiration that, "this, I was sure, was the way to study behavior!" (SB, 18).

Sherrington found the scratch-reflex to be very different, as it could not, he says, "be elicited by a single-induction shock, or even by two shocks" (Sherrington, 1906, 36). We still do have a reflex, he argued, although the single stimulus does not actually elicit the reflex, for very:

feeble shocks, each succeeding the other within a certain time -- summation time -- sum as stimuli and provoke a reflex. Thus long series of subliminal stimuli ultimately provoke a reflex. (ibid, 36).

Two stimuli that cannot by themselves elicit the scratch, do so when applied in succession by this principle of summation. In a dog where the spinal cord has been transected in the neck, the scratch-reflex can be elicited at any of the points shown on Figure 2 (adapted from Sherrington, 1906, 46):
A convenient way of eliciting this reflex, says Sherrington, is by an electrode on the surface of the dog's skin, for after repeated stimulation of subliminal intensity the stimuli would summate and elicit the reflex movement. A condition for the summation effect is that the stimuli be sufficiently alike one another and that they appear in close temporal succession.

4.4. In one of the first courses Skinner took over in physiology, taught by professor Hudson Hoagland, the main text was *Recent Advances in Physiology* (1928) by C.L. Evans. It covered recent physiological research into spinal reflexes as well as physiology of the brain. It might seem that the latter was a more ambitious topic, if only for reasons of complexity, but given the way both Sherrington and Pavlov actually carried out their studies, that difference was not noticeable. Sherrington's emphasis was always on integrative activity, or on the joint function of more than one reflex (hence his many ways of measuring reflexes), but Pavlov worked nearly exclusively with one reflex using only one measure. He had stumbled on to "conditioned", as he came to call them, or learned reflexes, while studying the physiological mechanisms of the digestive (e.g. salivary) glands. He had studied the inborn reaction of flow of saliva when food or acid is put into the mouth of a dog, but was constantly bothered by an effect he could not account for. It seemed that it was enough for the dog to see the food, or even, after long hours of experimentation, to hear the experimenter coming, for he began to salivate long before the
food was there. Pavlov first called the effect "psychic", and relegated it to psychology. But "psychic secretion" (Pavlov, 1927, 6) began to interest him more, and by the turn of the century he had become sufficiently interested in that curious effect to make a separate study of it. He began to record all the external stimuli falling on the animal at the time its reflex reaction was manifested (in this particular case the secretion of saliva), at the same time recording all changes in the reaction of the animal. (ibid)

What is immediately striking about this is how Pavlov himself uses Sherrington as his own exemplar, for not only does he continue his work by using the same unit of analysis and measures of reflex strength, but the transition to a new area is made particularly easy for Pavlov, due to the particular circumstance of the discovery of conditioned reflexes. He did not have to devise new ways of experimenting, for he was already doing typical physiological work. The only thing he had to do, so as to extend the problem/solution of his predecessor to a new area, was to make the decision to study that "psychic secretion".

4.5. Pavlov's work was easily adaptable by Skinner to his own interests -- i.e. the whole and intact organism -- because Pavlov was in many ways much more explicit and detailed than Sherrington had been. Thus he explicitly defined his unit of analysis, the reflex, as "every activity of the organism as a NECESSARY reaction to some external stimulus, the connection being made through a nervous path" (ibid, 4; emphasis in original). He adds, again echoing
Sherrington, that the reflex "concerns chiefly the activities of separate organs and tissues" (ibid, 9; underlining added). Pavlov says that he has confined his experiments almost entirely to "the secretory component" of the reflex:

The secretory reflex presents many important advantages for our purpose. It allows of an extremely accurate measurement of the intensity of reflex activity, since ... the number of drops [of saliva] in a given time may be counted ... (Pavlov, 1927, 17)

To distinguish between the simpler and inborn reflexes of the spine that Sherrington had made a special study of, and the reflexes Pavlov was interested in, Pavlov makes a distinction between unconditioned and conditioned reflexes. The former are characterized by their simplicity, he says, evolutionary function, and the fact that they are stable throughout the lifetime of the individual. They are furthermore comparatively few in number, he says, and the unconditioned stimuli for them only work close up (e.g. the food or acid has to be in the mouth before unconditioned salivation can start). Such were the reflexes Sherrington had studied, and Pavlov makes the point that a "spinal dog" left alone, would not survive long with just these inborn spinal reflexes. Conditioned reflexes on the other hand, are unstable, can be learned during the lifetime of the individual, and usually disappear if not explicitly reinforced:

The complex conditions of everyday existence require a much more detailed and specialized correlation between the animal and its environment than is afforded by the inborn reflexes alone. This more precise correlation can be established only through the medium of the cerebral hemispheres ... (ibid, 16)
Consequently Pavlov's dogs were not spinal dogs, and were not surgically restricted, except for the fact that they were tied down during experimentation, and only a minor operation on the side of their mouth was necessary, so the saliva could flow directly into a tube (see figure 3, adapted from Yerkes and Morgulis, 1909, 264, which was the first English summary of "the method of J.P. Pavlow [sic] in animal psychology".

4.6. Given Pavlov's transition from Sherrington's unconditioned reflexes to conditioned ones, he had to explain the formation of the latter on the basis of the former. He first did that in a general way by saying that he has found:

that a great number of all sorts of stimuli always act through the medium of the hemispheres as temporary and interchangeable signals for the comparatively small number of agencies of a general character which determine the inborn reflexes, and that this is the only means by which a most delicate adjustment of the organism to the environment can be established. To this function of the hemispheres we gave the name of "signalization". (Pavlov, 1927, 16-17)

He then gave a "demonstration" of this all important process of signalization. A dog was put in the stand and unconditioned salivation was elicited a few times. A metronome is sounded
simultaneously with the unconditioned stimulus of food and salivary secretion measured. Finally the food was withdrawn and when:

the sounds of a beating metronome are allowed to fall upon the ear, a salivary secretion begins after 9 seconds, and in the course of 45 seconds, eleven drops have been secreted. The activity of the salivary gland has thus been called into play by impulses of sound -- a stimulus quite alien to food. (ibid, 22)

The underlying principle here is signalization, Pavlov continues, in the sense that the sound of the metronome becomes a signal for food. Signalization is thus the capacity of the cerebral cortex, he continues, to associate a new and previously neutral stimulus with an unconditioned one, such as food, given a "sufficient number" of simultaneous elicitations. The conditions under which this can occur are that the two stimuli must overlap in time, and it is equally necessary, he says, "that the conditioned stimulus [the sound] should begin to operate before the unconditioned stimulus [food] comes to action" (ibid, 27). Other conditions are that the animal be healthy and that the stimulus be neutral prior to conditioning. Otherwise no new reflex is formed, he says, and no signalization will occur. Of interest here is the fact that Pavlov, although typically very explicit, only says that signalization occurs "after a sufficient number of elicitations", and does not give a minimal number as a condition for the process to occur. In fact he does say that in some cases only one trial is needed (p. 27). I mention this here, as it will be of considerable importance later.
Another important aspect to Pavlov's theory of the conditioned reflex concerns the distinguishing characteristic of the conditioned reflex of disappearing if not reinforced. This is "experimental extinction", and is demonstrated by the same experiment as before, except for the fact that:

Stimulation by the metronome is not followed in this particular experiment by feeding, I.E. contrary to our usual routine the conditioned reflex is not reinforced. (Pavlov, 1927, 49; emphasis in original)

By measuring the interval between the beginning of the stimulus and the beginning of the salivary secretion, the following typical extinction is the result:

<table>
<thead>
<tr>
<th>Latent Periods in Seconds</th>
<th>Secretion of Saliva in Drops during 30 seconds</th>
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<tr>
<td>3</td>
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(ibid, 49; slightly adapted)

The diminishing number of responses Pavlov takes as evidence for inhibition in the progress of experimental extinction. He added significantly, that it "is often subject to fluctuation. The fluctuations of an otherwise smooth curve may be brought about both by external and internal factors" (ibid, 50). Any slight change in external conditions, he maintains, is enough to change the extinction curve significantly.

4.7. There are three essential features of the experimental work of Sherrington and Pavlov that must have
attracted young Skinner, when he first attempted to mimic their experiments. First must be the concept of the reflex, its definition, identification, and methods of isolation. This was accomplished by the experimental procedure of eliciting reactions by specific but varied stimuli. The second factor must have been how they measured the detailed reactions to stimuli of various intensity. Sherrington devised many such measures (e.g. latency, after-discharge, refractory phase, etc.), while Pavlov's single measure was in terms of drops of saliva. The third and final factor that affected Skinner was the emphasis in both Sherrington and Pavlov on inactive organisms, i.e. organisms that reacted in the same way as before to an ever increasing number of stimuli. By the process of signalization Pavlov's dogs would salivate in a way they did not have to learn, (although the association itself was learned) to all kinds of new and previously neutral stimuli. Investigations into the nature of stimuli seemed to provide the clue to future research.

When Skinner later would exclaim that he had finally made contact with Pavlov, or that he had finally found "pure behavior", it was the combination of these three factors that were at issue. Thus Skinner would have to define his unit of analysis, devise measures of its strength, and find various stimuli that would elicit that particular reflex-unit.

4.8. Physiology at Harvard was a branch of the department of Biology. It was headed by W.J. Crozier, a student of Loeb and quite an influential experimental
physiologist at that time. He soon became the leading influence on Skinner. Crozier encouraged individual research, and Skinner soon began to plan his own. Hoagland suggested a topic to him on whether "the conditioned reflex of a frog could be confined to one eye or possibly one side of the brain" (SB, 18). The project "sounded like Pavlov"(ibid), Skinner says with delight. He wrote to his parents about this, and though he is obviously trying to impress them with details, the letter shows well how he wanted to mimic Pavlov and Sherrington:

> I will have to measure the time which elapses between the time when a frog receives an electric shock and when he jumps. Said time [latency] will be measured in thousands of a second, and I will have to plan a long series of experiments which will run off mechanically, every move being recorded on revolving drums. (SB, 18)

Unfortunately, the experiment turned out to be much simpler than expected, as it was not a case of a conditioned reflex but merely of a lowered threshold. Hoagland was amused and suggested an experiment on simpler organisms. Skinner began to work with T.C. Barnes, a young Canadian graduate student in physiology, on geotropism in ants.

This move, from the unsuccessful study of reflex characteristics in frogs to the work on tropisms in lower life forms, had considerable consequences for Skinner's very early experimental work. The immediate consequence was that he would have to postpone the exemplar use of Pavlov and Sherrington until he had found the right reflex to work with. Skinner is furthermore allowed, due to this shift
from the reflex to tropism, to compare the two concepts, and although he never could accept the definition of tropism as "a forced movement in an energy field", he was nevertheless impressed by the methodology of treating the organism as a whole.

There was another important consequence to this shift to tropism, and it concerns the fact that Skinner came in close contact with Crozier. They soon developed a close relationship and Crozier did much to help Skinner develop individual research and to get publications. In fact, Skinner's move over to physiology was so obvious that he complains in the autobiography of lack of supervision from the psychology department. They probably thought they had lost Skinner over to physiology (and would later call him "a deserter" for that particular reason), but though he worked with Crozier and avoided courses in psychology, Skinner says he was working entirely without supervision:

No one knew what I was doing until I handed in some kind of flimsy report. Possibly the psychologists thought I was being counseled by Crozier and Hoagland, and they may have thought that someone in psychology was keeping an eye on me, but the fact was that I was doing exactly as I pleased. (SB, 35)

This statement is important because it explains how Skinner was allowed to make use of physiology in a way that no member of the psychology department would either have understood or approved of. Crozier and Hoagland even tried to persuade Skinner to change departments, and he explains, once more in a letter to his parents, the social (i.e. external to his
science -- see section 4 of chapter three) reasons involved. I quote the whole paragraph as it shows well Skinner's actual position at that time, and because we will later have occasion to see Skinner reverse his decision concerning the prospects of a scientific career:

You see the physiology of the nervous system is practically psychology and the facilities of the Department of Physiology are better. They have just received an appropriation of $6 million and are building a new $2 million building next year. Crozier is already widely known and just at present the Department of Physiology is of far more importance here than psychology. Hoagland is especially anxious to have me come over. It would mean, not only that a Ph.D. from Physiology would be a better thing, but that there would be a good chance to line up with a local laboratory under this new endowment fund and get a good position... (SB, 25-6)

He adds that Crozier is a big gun in the field and that "it would be a good chance to line up with an influential man" (ibid, 26).

4.9. Skinner's first publications all appeared in the Journal of General Psychology. His name is first mentioned in a footnote to a paper by Barnes (1930). There Barnes refers to an earlier work of theirs, to be published shortly: "this was chiefly the work of my colleague Mr. B.F. Skinner" (p. 540). That work appeared the same year, and it was Skinner's first publication (Barnes and Skinner, 1930). Further on in the same issue there is a review by Skinner: "On the inheritance of Maze Behavior" (1930a) of a paper by E. M. Vicari (Vicari, 1929). In themselves, these papers are not important as an early contribution of a behaviorist. Still they are interesting for various reasons. First, they
all appeared in the same journal. Secondly, they were all reported to come from the "Laboratory of General Physiology" (see Barnes, 1930, 547; Barnes and Skinner, 1930, 102; and Skinner, 1930a, 346), though officially Skinner was working towards his Ph.D. in psychology. Thirdly, all these papers were biological or physiological in character, rather than psychological. They are on tropisms in ants, inherited behavior in mice, done in the "Laboratory of General Physiology", and reported in physiological terms. (The recurring terms are reaction time, stimulus, tropism (e.g. geotropism, spatiotropism, etc.), response, behavior, etc.) As we will see later, Skinner is constantly moving between the departments of physiology and psychology throughout his whole experimental stage (1928-36). That makes Skinner a sort of an interdisciplinary figure, but more on that later.

Apart from these factors, there is still another one, that would later have great significance for Skinner's early prospect of a science. As we will see, Skinner chooses the reflex as the unit of that science, but not tropism. That does not mean however, that the tropism concept has no effect here. In fact it has a crucial effect, for when Skinner redefines and extends the reflex to the whole organism, his definition, and especially the measurement of the reflex (reflex strength) owes very much to Crozier. But a fuller consideration of this influence will have to wait awhile, or else it would take us too far afield (see however section 6.2 later in this chapter).
4.10. Skinner signed up for courses in both fields, psychology and physiology, so that he could "postpone a decision" -- hoping that by the end of the year he would "have a clearer picture of both fields" (SR, 26). When that time came, Skinner, it seems, was in a real conflict.

It is interesting to see just how Skinner proceeds from here on. He has not been studying psychology or physiology for long -- and already he seems about ready to unite the fields. And notice that this is not just an innocent speculation on Skinner's part, but the starting point of a one man research program. But alas, as we will see later, this attempt to bring the physiology of the nervous system into psychology ends in a nearly complete failure. Let us then proceed to examine this conflict, and see what it is in each approach that attracts him. This will lead to Skinner's early experimental work proper, at the end of which he thinks he has a clear-cut research program for the whole of scientific psychology, but our immediate concern is where he gets that research program from.

The basic and probably older influence on Skinner was the tropism doctrine. Skinner's resolution to turn from literature to science was based on the fact that he had always been interested in curious bits of behavior (although as a writer he had, presumably, been approaching it in the wrong way). Secondly, in college, Skinner had been impressed by Loeb's work on the behavior of the whole (i.e. 'intact') organism, with his emphasis on tropisms and the lawlikeness
of purportedly spontaneous movements. The third and crucial influence must have been Crozier, who -- as has been noted -- had studied under Loeb. Crozier was, essentially, following up on the work begun by his old professor, with very similar emphasis. Both "resented the nervous system" (see SB, 45), regarding it as just another organ. Both were interested in demonstrated lawlike behaviors of intact organisms. In general, Crozier had been "bitten by the bug of a new discipline", Skinner explains, "of General Physiology" (SB, 16).

In the paper "Tropisms" mentioned earlier, Crozier says:

A major aspect of the theory of animal conduct is concerned with the initiation and the control of directed movements. The tropism doctrine successfully describes the course of one general class of such directed movements. (Crozier, 1928, 234)

Clearly Crozier visions a general theory of behavior ('animal conduct') using the doctrine of tropism as the basic unit of that theory. And given this goal, it is easy to see that the work of either Pavlov or Sherrington is of no consequence here, in that their units were molecular (e.g. 'glandular secretions' and 'neural reactions'), and not on the behavioral (i.e. molar) level.

Still Skinner was impressed by Sherrington's work. Along with Magnus and Pavlov they had all found order in surgical segments of organisms. Their results were obviously limited by this fact (e.g. what could results with cats whose spinal cord was cut tell us about the behavior of intact organisms?), but that only made their results more rigorous.
in another way. For they could study the properties of
various reflexes this way in considerable detail, and vast
amounts of data were already accumulating by this approach
(e.g. all the laws of the reflex -- threshold, after-discharge,
latency, etc. -- there were no comparable laws of general
physiology).

When Skinner began his own experimental work, he was
torn between these two evils. First, he wanted to do a
rigorous experimental analysis of individual reflexes in
the spirit of Pavlov and Sherrington, and second, he wanted
to study animal behavior in the spirit of Loeb and Crozier,
emphasizing forced or directed movements (tropisms).

4.11. But how could he do both? How was it possible to
get the rigor, exactness, and reproducibility of Sherrington
and Pavlov, while working with intact, freely moving organisms?
Put differently, how was it possible to unite the reflex
tradition with a tradition that emphasized a more general unit
than internal reactions? Indeed, how was it possible to use
together, the reflex -- a term that applied on a molecular
level to physiological reactions of individual organs, and
tropism -- a term that applied to the functional relations
of individual organisms to their environment?

These were the kinds of questions Skinner was trying to
answer in his early experimental work, to be described shortly.

4.12. Skinner's dilemma, as well as the hint towards
its eventual solution, is well expressed in this passage
from his autobiography:
I did not take warmly to tropisms... the stimulating environment I cared about could seldom be described as a field of force or behavior simply as orientation or movement. Nor could I abandon Pavlov, Magnus, and Sherrington so cavalierly. Nevertheless, I began to think of reflexes as behavior rather than, with Pavlov, as "the activity of the cerebral cortex" or, with Sherrington, as "the integrative action of the nervous system". (SB, 45-46)

The crucial insight here of course, is the phrase "I began to think of reflexes as behavior". What this means will be cashed out in the following section, where we will see that Skinner is not merely choosing the reflex over tropism as his unit, but something much more than that. Choosing the reflex is not an isolated or separate decision, but carries with it some very specific commitments, metaphysical and methodological.

4.13. By this time (winter 1929) Skinner had satisfied most of his requirements in the department of psychology, for by passing his 'prelims' he could avoid taking the final exams that spring. "From this point on", says Skinner in the autobiography, "I gave my courses little further attention", and "with only one or two exceptions, I signed up for research courses" (SB, 34).

Let us now look at the beginning of Skinner's experimental work proper in some detail, to see how, exactly, he visions this new and 'independent science of behavior', given the influences discussed so far. In particular, let us concentrate on how he thinks he can unite the seemingly incompatible approaches of Loeb and Crozier on the one hand, and Pavlov and Sherrington on the other. How is it exactly, that he
thinks he can unite the emphasis on intact, freely moving organisms, with the emphasis on the detailed study of the properties of individual reflexes?

5. THE REFLEX TRADITION

5.1. In the last two sections we have seen how the early influences on Skinner were of two kinds. On the one hand there is the work of Pavlov and Sherrington that centered around the concept of the reflex, and on the other the work of Loeb and Crozier on tropisms. As I have indicated the latter are not only different in that they insist on another basic concept -- tropism -- but also in that their unit is one level higher than the reflex. The reflex typically applies to individual organs while tropism is a forced movement of the whole organism. The reflex is a property of individual organs, while tropism applies to the whole organism.

I have further indicated that Skinner attempts to unite these two approaches when he starts 'to think of reflexes as behavior' (see section 4.12. above). Whatever that speculation means ultimately, it is clear at least, that Skinner will choose the reflex as his unit (but not tropism), and further that he will extend that concept (the reflex) to a (literally) larger domain (from organs to organisms). Thus he starts to think of reflexes as a property of the whole organism.

5.2. But so far this is only a speculation. It is one thing to say that the reflex can be thus extended and quite another to show how or even whether that is possible. Let
us then ask how exactly, Skinner thinks he can unite these seemingly different approaches to the study of behavior.
And first we must realize that both Loeb and Crozier resented the emphasis on the nervous system -- it was just another organ to them. (Related to this of course, was their emphasis on organisms lower in the phylogenic scale, many of which had no nervous system.) Skinner however, saw no dilemma here. He did not see himself in the position of being forced to reject one approach by accepting the other. He thought he could well accept the scientific rigor he admired so much, of Sherrington and Pavlov, while still holding on to the insistence that it was the behavior of the whole organism that mattered. "General Physiology dealt with overall quantitative laws", Skinner correctly observes, and it "was a methodolody rather than a subject matter, and almost any data would serve if studied with the right methods" (SB, 45; underlining added).

Another way to express this is to say that Loeb's and Crozier's resentment of the nervous system "cancelled out the physiological theorizing of Pavlov and Sherrington", says Skinner, "and thus clarified what remained of the work of these men as the beginnings of an independent science of behavior" (CR, 104; underlining added).

5.3. The typical experimental setup at the time Skinner began his experimental work was to study the behavior of rats in mazes. The basic experimental training from the physiology department came in handy, when Skinner -- pardon
the expression -- made up his mind to stay in the field of psychology. The machine shop in Emerson Hall (psychology) soon became his "center of activity" (SB, 32).

Interestingly enough, and as Skinner points out in his autobiography, "the maze as a scientific instrument", did not serve his purposes, for the "animal's behavior was composed of too many different 'reflexes' and should be taken apart for analysis" (SB, 32; underlining added). This statement is highly revealing. Not only does Skinner reject out of hand the typical experimental technique of animal psychology, but he does so saying that it should be taken apart and analysed. Notice what Skinner is saying. He rejects the genearly accepted experimental method as irrelevant for his purposes: But what were his purposes?, and what different conception of method is it that makes Skinner say such a thing?

5.4. But before we answer these more general questions, let us study in some detail Skinner's very early experimental work, for it will give us more substance to what is only a speculation so far; namely that it is possible to extend the reflex concept to the behavior of the whole organism.

What we first notice is that Skinner is looking for the individual reflex. When he says that maze behavior should be 'taken apart for analysis', he means that it is composed of too many reflexes. He therefore decided to study the first part -- the way in which the rat first entered the maze. This way he hoped to be able to get at the individual reflex. He constructed a box and put a small tunnel-like structure at
the top of a flight of steps against one side of the wall
(see figure 4).

He then released the rat from the box at the rear of
the dark tunnel, from which it could emerge into the lighted
space. "I planned to study" Skinner says later, "how it moved
forward down the steps and how it pulled back when I made a
noise. These were, I thought, reflexes" (SB, 33; underlining
added). But as the rat ran from the tunnel, it "seemed to
be torn by competing reflexes" (ibid). It would move in a
forward-and-backward oscillation, and such movements did not
indicate a single reflex. He therefore gave up on his
approach saying that his effort to take "maze-behavior apart
and study separate reflexes had left [him] with data that
did not seem to be susceptible to further analysis" (ibid,
35).

5.5. The first attempt to study the properties of a
single reflex (like Pavlov and Sherrington) of an intact
organism (like Loeb and Crozier) had failed. But the failure
is revealing for our purposes, because we see Skinner put
all the emphasis on the individual reflex, and the lack of a real "measurable quantity" (see SB, 34) to work with. This is the first instance of what I will later call Skinner's two major problems, the problem of definition (of the individual reflex) and the problem of measurement (of reflex strength). I will return to that issue in the next section (and again in chapters two and three), but let us return to Skinner's very early experimental work.

As his first attempts to get at the reflex properties of whole, intact organisms, (which we now know to involve the identification and quantitative measure of the individual reflex) had failed, Skinner had no idea what to do next. He considered doing classical maze studies, but again returned to inherited behavior in rats -- at Crozier's suggestion. But this was a diversion and only had "the major result", Skinner later admits, "that some of [his] rats had babies" (CR, 104). As it turned out, the older rats did not tell him much about inherited behavior, but in the behavior of the young ones Skinner thought he "saw the postural reflexes shown in ... Körperstellung ..." (SB, 36), and he decided to repeat some of Magnus' experiments. In sheer frustration, it seems, Skinner started to study the younger rats (as their behavior was simpler, presumably) and mimicked experiments from Körperstellung to study the temperature coefficients of reflex processes" (SB, 36).

But even this failed. The temperature coefficients were too difficult to measure, and Skinner next turned to
Sherrington's flexion reflex, to see if luck would finally lead him to some kind of measurable individual reflexes of intact organisms. He soon realized that, like before, this was much more difficult than might appear at first. The reason was, of course, that Skinner was not exactly repeating or replicating the experiments of Magnus, and now Sherrington, for his organisms, but not their's, were intact freely moving baby rats. This made the task of getting reliable measurements all the more difficult. Let us now look into this experiment on the flexion reflex in some detail (before we turn to the more general question of research traditions), if only for the reason that it, finally, opened up the possibility of exact research into individual reflexes of intact organisms.

5.6. Skinner tried to suspend baby rats in belly slings, to measure the stiffening of the legs as the foot touched the ground and is pushed towards the body (i.e. Sherrington's flexion reflex) "But the movements were too delicate to record mechanically" (SB, 36), Skinner once again had to admit, so next he tried to pull the rats by their tail, to see how their leg muscles would react (see figure 5). As the table responded to the tremor of their leg muscles; "a wiggly line appeared on the kymograph" (SB, 37); (see figure 6):
Finally Skinner was in luck. He says:

I soon discovered more interesting behavior. When pulled backward, a baby rat suddenly springs forward in the air, possibly sacrificing a bit of the tender skin of its tail. My table and kymograph seemed to report such leaps fairly accurately, and I was overjoyed. Here was the kind of thing I was looking for: the reflex behavior of an intact organism, recorded with a sensitivity close to that of Sherrington's "torsion-wire myograph" (SB, 37; underlining added)

This was the lead Skinner was after, a kind of measurable reflex behavior, with a version of Sherrington's flexion reflex experiment, "but here adapted to the response of a whole organism" (CR, 106).

5.7. As we will see in some detail in the next section, this discovery of 'more interesting behavior' marks the beginning of Skinner's early experimental research proper. He will soon do experiment after experiment, slowly shaping up his early prospect of a science. That will involve a specific program of research to which I will return later on in this chapter. It will also involve two quite specific problems -- the problem of the definition of the reflex and of its measurement -- but before I continue to analyse that however, it is necessary to say something about the terminology used here to characterize this general theory that lies behind and motivates Skinner's early experimental work.

That general theory is the reflex tradition and is to be understood in the sense proposed by Larry Laudan in his book Progress and Its Problems (1977). In that book Laudan proposes a problem-oriented model of scientific change and
growth. The model is based on the work of Kuhn (1962 and 1970) on paradigms and Lakatos (1968 and 1970) on research programs. But while Laudan's concept of research tradition bears a strong resemblance to these, it has a number of distinguishing features, to which I shall return in a moment.

Laudan claims that there are two kinds of problems in science, empirical and conceptual. Individual theories, properly understood, are specific answers to certain empirical problems, Laudan insists, and resolutions of certain conceptual problems. One of the most interesting aspects of Laudan's model is that it makes all theory evaluation a comparative matter. In fact, one reason he came to insist on the distinction between individual theories and global (= research traditions), was that it enabled him to recognize many (and often rival) theories within the same tradition. As we will see later, one specific role two theories within the same tradition can play is to generate, both anomalies and external conceptual problems for its rivals.

I will return to Laudan's analysis of problems, empirical, conceptual, and anomalous, in the next section. In this section, however, the interest is not so much with this aspect of Laudan's model, but rather with this insistence of his on individual theories (that are designed to answer specific empirical and conceptual problems) and more global theories that have a very different function. The literature is ambiguous as to which of these a "theory" means, Laudan says, and the term actually "refers to (at least) two very
different] types of things" (Laudan, 1977, 71). Theory, narrowly construed, refers to individual and specific theories, like Einstein's theory of the photoelectric effect, Marx's theory of value, or Freud's theory of the Oedipal complex. Similarly I will say that Skinner has (or comes to have) a theory of operant behavior. Whether Skinner ever had a theory of reflex behavior or whether we should rather say that he only has an operant theory in embryo, is a vexed question to which I will have to return. My immediate problem however, is what this more general theory could be in Skinner's case. "Theory" in that latter sense Laudan calls a research tradition. Its character is more global than individual theories, and it serves a very different function. Thus we speak of the atomic theory, or the general theory of evolution, Laudan correctly points out, and adds that we are "referring not to a single theory [in this case], but to a whole spectrum of theories" (ibid). The term evolutionary theory, for instance, refers to a whole cluster of theories, of which Darwin's original theory (and Wallace's) is but one.

In our case I will speak of this global theory as the research tradition of reflex physiology, or reflex tradition for short. But before I accumulate the evidence so far gathered for that assertion, and add a few more, let me first characterize more clearly Laudan's concept of a research tradition, and see how it differs from Kuhn's influential concept of a paradigm.
5.8. Research traditions, says Laudan, have three major things in common:

1. Every research tradition has a number of specific theories which exemplify and partially constitute it; some of these theories will be contemporaneous, others will be temporal successors of earlier ones;
2. Every research tradition exhibits certain METAPHYSICAL and METHODOLOGICAL commitments which, as an ensemble, individuate the research tradition and distinguish it from others;
3. Each research tradition (unlike a specific theory) goes through a number of different, detailed (and often mutually contradictory) formulations and generally has a long history extending through a significant period of time. (By contrast, theories are frequently short-lived.)

(Laudan, 1972, 79; emphasis in original)

We see that the function of research traditions is very different from the function of individual theories, for while the latter are designed to answer specific problems, the former serve to provide a set of guidelines for the construction and development of those specific theories. Theories are thus articulations of research traditions. Another way to put this, although this is not the way Laudan characterizes research traditions, is to say that they provide the scientist with a set of constraints, metaphysical and methodological, as to how he can and, more importantly, cannot construct his theory (i.e. articulate the research program):

Put simplistically, A RESEARCH TRADITION IS THUS A SET OF ONTOLOGICAL AND METHODOLOGICAL "DO'S" AND "DON'TS". To attempt what is forbidden by the metaphysics and methodology of a research tradition is to put oneself outside that tradition and to repudiate it. (Ibid, 80; emphasis in original)

A research tradition, by its very generality is neither explanatory, nor predictive, nor is it directly testable.
as the theory is. It is this normative element of the research tradition that best characterizes it. In fact, this normative element, says Laudan, gives us:

a preliminary, working definition of a research tradition [that] could be as follows: A RESEARCH TRADITION IS A SET OF GENERAL ASSUMPTIONS ABOUT THE ENTITIES AND PROCESSES IN A DOMAIN OF STUDY, AND ABOUT THE APPROPRIATE METHODS TO BE USED FOR INVESTIGATING THE PROBLEMS AND CONSTRUCTING THE THEORIES IN THAT DOMAIN. (ibid, 81; emphasis in original)

It is the purpose of the rest of this chapter to articulate further these normative functions of the research tradition — the reflex tradition in this case. I will discuss these functions of the reflex tradition and how it is supplemented with the methodology of general physiology.

In section 7 I will discuss the ontology of the reflex tradition, and in section 8 the methodological commitments.

But in the next section I will again discuss, and in more detail, Skinner's very early experimental work, for it is my contention that though Laudan's model is a marked improvement over both Kuhn's model of the paradigm and especially Lakatos' research program, he still neglects one crucial element in Kuhn's theory.

5.9. Kuhn has been heavily and quite correctly criticized for being systematically ambiguous as to what originally he meant by a paradigm. He found it necessary to distinguish between two senses of the term. One sense is the disciplinary matrix, and the other is the exemplar (see Kuhn, 1969 and 1977). An exemplar, I will argue, is what directly guides the scientist in his research. The
exemplar is a concrete problem/solution, which when employed as a model or example, can replace explicit rules in directly guiding research.

As we will see in the next section, it is not enough to insist on the research tradition as normative (metaphysical and methodological) assumptions that guide research, for their generality precludes them to be more than constraints on what the scientist should not do. In addition the scientist will need something much more specific than that. What he needs in effect, is a specific and clear paradigm (in the sense of an exemplar), that enables him to literally mimic the experiments of his tradition. I will argue further, that it is this attempt to mimic experiments, which (in this case at least) is the search for a measurable quantity to work with, that originally defines (or decides) the scientist's research tradition. The exemplar is needed, and so is the successful measure, before the scientist can start considering the more general commitments of the tradition, for essentially, these normative commitments only follow once the exemplar has served its purpose. Conversely, if the scientist (Skinner in this case) would not have been able to use the exemplar (if Skinner could not have replicated Pavlov, Sherrington, or Magnus) then, by hypothesis, the reflex tradition would not have become his research tradition. But since he was able to use the experiments of Pavlov and Sherrington as exemplars, he did immediately rush into considerations of the more general assumptions of that tradition.
5.10. Let us then see how Skinner was guided to the reflex tradition by his mimicking of the prime examples of Pavlov and Sherrington (as these were described in section 4.3. and 4.5. above, respectively) and then, in section 7 and 8, see how he describes the constraints (methodological and metaphysical) of the tradition the exemplars have committed him to.

6. THE EXEMPLAR

6.1. It was suggested in the last section that the reflex tradition is to be understood in the sense of Laudan's research tradition. By the end of the section however, I indicated that Laudan fails to take into consideration one very important aspect of Kuhn's original paradigm-concept -- the exemplar. Before we go into the role of the exemplar in the establishment of a scientist's research tradition however, let me explain a few preliminary points.

The first one has to do with the relation between the models of Laudan and Kuhn. As I have hinted at before I do not really consider the models as rivals, but rather as complementary. Let me now give some substance to this claim. Laudan's model is in many ways an improvement over Kuhn's (see Laudan, 1977, 73-6 for a discussion of some of these). The most important ones have to do with the role of conceptual problems in scientific research, the role of rival theories within the same tradition as the generator of anomalies, and more generally, I will insist on the well-foundedness of Laudan's terminology, especially concerning his "problems" as neutral entities, on which one can lay
normative value by such adjectives as: solved, unsolved, anomalous, and conceptual.

Though Kuhn is right in his characterization of empirical and theoretical problems, and although Skinner's problem of the measurement of reflex strength is just such a problem, I argue (in section 6 of chapter two) that the definition problem (the problem of the elicitation assumption) is not empirical, but a conceptual problem. It is an external conceptual problem in Laudan's sense of that term, or a problem between an individual theory and a more general doctrine. The latter is the reflex tradition, the basis of which is the (believed to be) well founded elicitation assumption. The former and individual theory is Skinner's emerging operant theory, which starts out as a promise of an independent science of behavior through his sketch of a research program. The elicitation problem is the conceptual tension between this emerging theory and the tradition, and the solution to the problem is its denial, or conversely, Skinner's assertion that there is non-elicited or operant behavior.

The main reason this runs counter to Kuhn is not that the problem is conceptual in a way he can not account for, as Kuhn can still argue that this was not a "scientific" problem, or that it should not have bothered the scientist B.F. Skinner. This amounts to the claim that such conceptual problems are not typical of mature sciences, but neither am I prepared to accept the generality of that.
claim, nor willing to judge the matureness of a science on this basis alone. Kuhn's problem is that this conceptual problem did worry Skinner; and as this whole thesis attempts to show, it is in fact the crucial component in the emergence of operant theory -- from the elicited reflex to the non-elicited operant -- from normal science to revolutionary.

6.2. Another and related point that also scores in Laudan's favor is the role rival theories play within the paradigm/research tradition as the generator of anomalies. As will be argued in detail in sections 7 and 8 of chapter three, that was just the function of operant theory, for once Skinner had solved the elicitation problem, a previously unsolved and relatively uncritical problem for Pavlov's theory became anomalous and crisis-causing. It is not that normal science turns revolutionary because of an ever increasing crisis, as Kuhn argues, but the other way around. For Kuhn the revolution requires a prior crisis, but in this case study no such crisis is evident. The crisis came however, once Skinner had solved the elicitation problem within normal science, as the conclusion of the debate with Konorski and Miller will show. Kuhn's puzzle-promotion phenomenon will be shown to be quite insufficient in explaining a growing sense of crisis, for his monothetic account of normal science precludes any such crisis-causing device.

A third point in Laudan's favor is again related to the last one, and is really a generalization of that point. This
concerns his multi-theoretic account of research traditions, as he allows for the existence of many different and often rival theories within the same tradition. Once that concession is made it is easy to see how rival theories can be crisis-causing, for when a new theory provides an answer for a previously unsolved problem, there is a direct clash between it and any theory within the tradition that has not solved that same problem.

This brings me to a point in Kuhn's favor, for though Laudan's distinction between individual theories and the more global research traditions is seen to be very helpful in this case study, it is also true that Kuhn has clarified, to a considerable extent, his previously careless use of "paradigms". Kuhn's original idea moreover, of a paradigmatic use of predecessor theories, is completely lacking in Laudan's research tradition conception. In the end, it is the way each model explains scientific progress through successive theories, that is crucial. As I will argue for in detail in this section, the paradigm in the sense of exemplar has to be supplemented to Laudan's model. Without such a use of predecessor theories, Laudan can neither explain why a scientist is committed to a research tradition in the first place, nor explain how the tradition guides his research, for its very generality precludes it from being more than a set of constraints on the building of individual theories.

6.3. The final preliminary point has to do with Kuhn's original conception of the paradigm. I say original here,
as before, for though Kuhn was later to (improve on and) make a distinction between, the disciplinary matrix and the exemplar (first in Kuhn, 1969, and then more fully in Kuhn, 1977), the point is that this is only an improvement in clarity. Neither the disciplinary matrix nor the exemplar serve the purpose of adding a new aspect to the original paradigm concept, but serve rather as a further explication of what Kuhn meant all along by the paradigm concept. When Shapere (1964, 1971) and especially Masterman (1970) pointed to many different uses of the term paradigm, Kuhn felt compelled, not to add to this list, but rather to explicate the (as it turned out) two main aspects of the paradigm.

The first and more global, aspect of the paradigm, says Kuhn, is the disciplinary matrix:

'Disciplinary' because it is the common possession of the practitioners of a professional discipline; 'matrix' because it is composed of ordered elements of various sorts, each requiring further specification. (1977, 463)

The paradigm in this sense is a group commitment of a variously large group of practitioner/scientists. It is a commitment to symbolic generalizations, says Kuhn, models and values. These symbolic generalizations look like laws of nature, but have, Kuhn maintains, a stipulative element in them, and if they are strongly held, become definitions and mere tautologies. Such is it with Skinner's $R = f(s)$, or the statement that the response is a function of the stimulus (discussed more fully in section 8 below). This is the elicitation assumption in its logical form, and Skinner
shares that assumption at first, to the extent that it is, to him as to others in the tradition, tautological.

Together with the model, the symbolic generalizations amount to the same as Laudan's metaphysical assumptions of the research tradition. The model specifies the analogies and metaphors used, and in this case it is a mechanistic model of animals as being reaction-mechanisms to stimuli in the immediate environment.

6.4. Before I continue with the second and crucial aspect of the paradigm as a shared example, let me make a little more clear the general commitment to the reflex tradition. I have talked about Sherrington, Magnus, and Pavlov, about Thorndike, Watson, and Russell, and finally also about Loeb and Crozier. What all of these authors have in common is general commitment to an independent science of behavior of some sort. Whatever their individual theories, they all share the commitment, that behavior can be a subject matter of scientific investigation. That does not make all of them reflex theorists, and as we have seen both Loeb and Crozier resented reflex physiologists. Their unit of tropism was one level higher, but Skinner thought he could unite these different approaches, for the methodology of general physiology could be served with "almost any data". This is the special twist to Skinner's individual research program and it is a methodological addition to the reflex tradition.

Thorndike, Watson and Russell are not strict reflex theorists either, though the latter two came to reinterpret
their own units (habit formation and learned reaction, respectively) in terms of Pavlov's conditioned reflex. Thorndike is connected to all this in a rather complex way, for though he was certainly never a reflex theorist himself, his influential law of effect was reinterpreted as an instance of the conditioned reflex. Thorndike did not talk in terms of reflexes, stimuli or responses, but Watson and Russell still interpreted his law in those terms.

6.5. What then is this reflex tradition? If it is in need of a methodological addition from Loeb and Crozier, of what substance then is the claim that Skinner's research is crucially influenced by the reflex tradition? The answer to this is twofold and has in fact, already been given. As was explained in sections 4 and 5, when we say that there is a connection between Sherrington, Magnus, and Pavlov on the one hand, and Thorndike, Watson, and Russell on the other, it is the commitment to a science of behavior that connects them. What connects some of them even more, is the concept of the reflex. Pavlov's theory was about conditioned reflexes, Sherrington had studied unconditioned reflexes, while Watson and Russell came to interpret Thorndike's law of effect as well as their own units, in terms of the reflex. Still these are all specific theories, in the sense of being specific answers to specific scientific problems, but what unites these authors is that they all came to interpret their own results in terms of (Sherrington's unconditioned and) Pavlov's conditioned reflex.
But what does that mean? Sherrington's theory was a predecessor to Pavlov's, but did all the other authors accept Pavlov's theory? Certainly not. His theory was never accepted uncritically (except maybe in the Soviet Union), and was from the beginning heavily criticized. But that did not deter people from the genuine fruits of his theory, and what a whole group of scientists accepted was that there was something of major importance in his interpretation of the conditioned reflex. What they shared, and what ultimately defined the reflex tradition, was the commitment to Pavlov's signalization interpretation of the conditioned reflex. Signalization was the process by which new and neutral stimuli acquired the capacity to elicit reflexes, and a commitment to it instructed scientists to look for such stimuli. It was this signalization principle that was accepted, not Pavlov's whole theory.

6.6. In the Postscript (Kuhn, 1969) Kuhn admits that several of the key difficulties in his original text (1962) "cluster about the concept of a paradigm" (Kuhn, 1969, 174). Though the term is used in many different ways, Kuhn thinks that basically it comes down to two different senses:

On the one hand, it stands for the entire constellation of beliefs, values, techniques, and so on shared by the members of a given community. On the other, it denotes one sort of element in that constellation, the concrete puzzle-solutions which, employed as models or examples, can replace explicit rules as a basis for the solution of the remaining puzzles of normal science. (ibid, 175)

The first sense is sociological (e.g. "scientific community");
a disciplinary matrix as a shared commitment. I have already indicated what some of those commitments are in the case of the reflex tradition, and will say more about that later, but for now I want to concentrate on one more component of the disciplinary matrix: the paradigm as a shared example.

Kuhn points out, correctly I think, that the "paradigm as shared example is the central element of ... the most novel and least understood aspect" (ibid, underlining added) of his book. The reason philosophers of science have found this so difficult to understand, Kuhn further claims, is that they have a totally different theory of how a student learns a specific science. First the student learns the theory and some of the rules for its application, or so that theory goes, and only then can he possibly tackle the problems of that science.

Kuhn's argument is that the opposite is actually the case. He claims that the student's learning process starts with the problems, but not some abstract exposition of the science. It is the problem, or rather the problem-context, that is the starting point. It is not that the theory and its laws are of no consequence, but rather that in the absence of problem-solving; in "the absence of such exemplars, the laws and theories he has previously learned would have little empirical content" (ibid, 188).

But what is it that the student acquires when he does one problem/solution after another? He acquires the ability, says Kuhn, to see resemblances between apparently disparate problems.
That ability to recognize group-licensed resemblances is the main thing students acquire by doing problems. These concrete problems with their solutions are what I previously referred to as exemplars, a community's standard examples. (Kuhn, 1977, 471)

6.7. In section 5 above we saw the beginnings of Skinner's very early experimental work center around the search for the individual reflex and its measurement. Thus he began to think of reflexes as behavior, or put differently, as the property of the whole organism. Skinner rejected the maze, for it could not tell him anything about individual reflexes. He decided to take the maze apart, but that attempt failed (see section 5.4., above). What was he to do? The only thing he could think of was to repeat some experiments by Magnus, Sherrington, and (soon) Pavlov. But as he emphasized the whole organism, he was not really repeating or replicating those experimenters, but much rather trying to apply their solutions to his problems. Just think for a moment of the quote in section 4.3. above, about Sherrington's laws of latency, after-discharge, etc. When he exclaims that this (he was sure) was the way to study behavior, it marks the beginning of the student's learning of how to solve the problems of behavior. So when he is confronted with the problem of measuring the individual reflex of the freely moving rat, the problem—solutions of Sherrington (Magnus and Pavlov) in a slightly different context, provide Skinner with the exemplar.

Moreover, when the experiment on Sherrington's flexion reflex gave orderly results, Skinner said 'this was the kind
orderly process in the behavior of an intact organism was very much in line with Crozier's field of General Physiology. He told Skinner he wanted a couple of articles before summer -- he would help to get them published. Skinner promptly wrote up a paper, showed it to Crozier and Boring, and both agreed that it should be published. The title was "On the Conditions of Elicitation of Certain Eating Reflexes" and was published in the Proceedings of the National Academy of Sciences (Skinner, 1930b) in the summer of 1930.

6.14. As we have seen more than once in this section, Skinner exclaims that he has finally made contact with Pavlov. That means he has found a new reflex (the 'eating' reflex), on the basis of an analogy with Sherrington, Magnus, and Pavlov. Put differently, he had used their results as exemplars, or has made use of their concrete problem-solutions adapted to a slightly different context (e.g. 'intact' organisms).

It also means that Skinner was studying unconditioned reflexes, again just like Sherrington (and Pavlov in his early work), "or at best the physiological process of ingestion, but I was", Skinner goes on, "interested in learning" (SB, 62). He regarded these results as showing that behavior hitherto supposed to be free or capricious, to be just as much subject to natural laws as, say, heartbeat. In an attempt to get at behavior that was not as physiological as the eating reflex, Skinner simplified his apparatus even more. The guiding principle seems to have been to get at
6.9. The baby rats Skinner had used, first to mimic Magnus, and then Sherrington, were grown now, so Skinner thought he might repeat his own experiments on Sherrington's flexion reflex. Also, it had bothered him that in the earlier experiments he had to pull the rats by their tail, for although that would fit in well in Sherrington's experiments, it did not seem right for someone who emphasized the study of individual reflexes of intact -- freely moving organisms.

Skinner then built a runway constructed of light wood, in the form of a U girder, moulded rigidly on vertical glass plates, the elasticity of which permitted a very slight longitudinal movement (see figure 7).

Skinner soon got tired of carrying the rats back to the other end of the runway, and constructed a back alley (see figure 8):

Now the rats could run unhindered down runway B, the movement being recorded on D. At point C food was waiting, and after having eaten it, the rat would eventually turn back along A.
and the process could be repeated. But why *eventually*?

Why not right away? Surely any rat in its right mind (so to speak) could learn that by starting another run right away, it would get more food. This problem soon started to bother Skinner, as we will see in a moment.

This did not bother him right away, though he sometimes had to wait up to ten minutes before the rat started another run. The point of the experiment was to study the rat's adaptation to the sound of a 'click' that Skinner would sometimes make, which at first would often stop the rat dead in its tracks in the middle of the runway (see figure 9):

![Diagram](time_click_rat)

The experiment now seemed as developed as it could be. The purpose had been to mimic Sherrington's experiment on the flexion reflex, without restricting the rat in any way. But as explained before, Skinner was not really replicating Sherrington. In effect, he was attempting to show that Sherrington's laws applied, not only on a physiological or molecular level, but that they applied equally well on the behavioral or molar level. Still Skinner was not satisfied. He had his data, all right, but:

*This was pretty tortured stuff, and it was clear that the experiment was not getting anywhere. What was wrong? I seemed to be behaving as scientists behaved [sic] ... the tracings on my records looked like the white-on-black records I had seen in articles*
on muscular contractions. But what was I to do with them? Pavlov could quantify his results by counting drops of saliva. How was I to convert these wiggly lines into significant magnitudes? (SB, 53; underlining added)

6.10. So the problem was the lack of a measurable quantity. As mentioned before the rat would 'eventually' start another run after eating food at point C (see again figure 8). At first this was just "one annoying detail" (CR, 106), although Skinner adds, significantly, that there "seemed no explanation for this" (ibid). Soon this started to worry him however, and his first reaction was to interpret the delays as the result of conflicting reflexes. These, we remember, he had encountered before, and Skinner speculated at length how this could happen. But whatever he would say, i.e. whatever antagonistic reflex he could think up, the explanation just did not hold. The point was that:

"whereas throughout the course of an hours experimentation the running reflex (being 'reinforced'), persists, the [antagonistic behavior] ADAPTS OUT". The delays should get shorter, but in fact they got longer. 38 (SB, 55; emphasis in original, underlining added)

The annoying detail was threatening to get out of hand. Skinner had in fact exhausted his exemplar use of Sherrington's flexion reflex and still he could not explain the problem away. His next step was quite crucial. When faced with this persistent worry he first tries to explain it away with the least effort possible, until he finally gives up and starts to measure the delays (see figure 10) for their own sake.
When I timed these delays with a stop watch ... and plotted them, they seemed to show orderly changes ... This was, of course, the kind of thing I was looking for. I forgot all about the movements of the substratum and began to run rats for the sake of the delay measurements alone. (CR, 106; underlining added)

Again Skinner exclaims that this was the thing he was looking for. But what is this 'thing'? Was it the fact that he had found orderly changes in the behavior of an intact organism. Or had he found a unit of behavior? Had he found his single reflex? In the paper "The Experimental Analysis of Behavior (A History)" (Skinner, 1976b) he says that here:

was a PROCESS, something like the processes of conditioning and extinction in Pavlov's work, where the details of the act of running, like those of salivation, were not the most important thing. (RBS, 115; emphasis in original)

It seems that in the attempt to repeat his own experiment on the flexion reflex Skinner had found a quantifiable measure very close to Pavlov's. He was not sure of what he had, but he did know he could identify an individual reflex, and that he did have a way of eliciting it, and now he thought he had also found a way to quantify his results.

6.11. Skinner did not bother with sounding the click anymore, as the new measure of delay occupied all of his interest. He was just not interested in the flexion reflex anymore, but only in the new process he had found. An experiment similar to Sherrington's flexion reflex had turned into something completely different! But into what, exactly, Skinner was not sure of yet.
What he was sure of however, was that he needed a better measure of the time delay and the amount of food eaten, so he devised a food magazine (see figure 11):

The food magazine had a spindle on it, and by winding a string around it, allowing it to unwind as the magazine emptied (see figure 12), Skinner got a different kind of record (see figure 13):

With typical irony Skinner explains:

Instead of a mere report of the up-and-down movement of the runway [like A in figure 13], as a series of pips as in a polygraph, I would get a CURVE [like B in figure 13]. And I knew that science made great use of curves, although, as far as I could discover, very little of pips on a polygram. (CR, 108-9; emphasis in original)

This rather simple change in apparatus may not seem all that earthshaking. But for Skinner's purposes it was
crucial. He had failed to quantify the wiggly lines, but once he had found a systematic change in delay, the wiggly lines were no longer needed. The curve revealed the rate of responding, and changes in that rate, in a way that the polygram simply did not (compare A and B in figure 13). With a single glance at the curve, Skinner could now see how hard the rat was working:

When the rat was working rapidly, the thread would be payed out rapidly and the line would be rather steep, but as the rat slowed down, the curve would grow flatter. From the slope I could estimate the speed at which the rat was working at any moment ... and before long I was getting what are now standard cumulative records. (SB, 56)

The apparatus was totally automatic. The only thing Skinner had to do was to have the magazine full with food pellets. He did not forget to fill the food magazine, but to the same effect, the apparatus broke down, and lo and behold, he got an extinction curve (see figure 14):

Skinner first corrected the failure, but soon realized that he was on to something. Once again (and for reasons we understand better now) he exclaimed:

I had made contact with Pavlov at last! Here was a curve corrupted by the physiological process of ingestion. It was an orderly change due to nothing more than a special contingency of reinforcement. It was pure behavior! (CR, 110; underlining added)

The exemplar use of Pavlov was now complete, for not only had he found a quantifiable measure similar to his,
using a similar stimulus (food pellets), but he could now also reverse the process, like in Pavlov's extinction. Realizing that he had, at last, found a reliable way to quantify behavior, Skinner no longer needed the tilted runway. A much simpler mechanism would do just as well. The rat would simply push open the door of a small bin to get a piece of food (see figure 15):

The rat stands on platform A, and has to push a door B open to get at food C. When the door opens, the movement is recorded by D. The apparatus was made to lock the door to the tray. This was the apparatus Skinner used in the first of the series of experiments to be examined at the beginning of chapter two.

6.12. This was the apparatus Skinner used for the experiments that later would constitute half of his thesis. He was satisfied with this apparatus for it gave him an individual reflex to work with (the 'eating reflex'), and with the behavior thus reduced to the opening of a door, Skinner says he began to get more orderly results:
Under controlled conditions and with pellets of food which took some time to chew, I found that the rate of eating was a function of the quantity of food already eaten. (RBS, 115; underlining added)

Not only had he found an individual reflex, but also a stimulus (pellet of food) that elicited it, and a way to measure reactions to that stimulus. This is a combined exemplar use of Sherrington and Pavlov, for the reflex is inborn or unconditioned like Sherrington's reflexes, but the eliciting stimulus is like Pavlov's (i.e. food). And remember that this had begun as the attempt to mimic Sherrington's flexion reflex. The fact that he had not really succeeded in mimicking the flexion reflex did not bother Skinner, for he had all the essential components, the individual reflex, a way to measure it, and a stimulus that would elicit the reflex. The actual details of the experiment were not all that important, and it did not bother Skinner that his experimental apparatus was considerably different from both Pavlov and Sherrington, for he never really wanted to replicate them, but just use their problem/solutions on the experiments on the intact organisms he was interested in.

6.13. Skinner took these results to Crozier and a friend in the physics department, Cutbert Daniel, who showed him how to plot them on a logarithmic paper. What Skinner had found essentially, was that the rate at which a rat eats food, is a square function of time (i.e. the time it takes to eat).

Crozier 'was quite worked up about this', Skinner reports in his autobiography -- and not surprisingly. Finding an
orderly process in the behavior of an intact organism was very much in line with Crozier's field of General Physiology. He told Skinner he wanted a couple of articles before summer -- he would help to get them published. Skinner promptly wrote a paper, showed it to Crozier and Boring, and both agreed that it should be published. The title was "On the Conditions of Elicitation of Certain Eating Reflexes" and was published in the *Proceedings of the National Academy of Sciences* (Skinner, 1930b) in the summer of 1930.

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pure behavior, or behavior not contaminated by any physiological processes — i.e. the single reflex as a property of the whole organism. The point is the same as before, for though running around that tilted runway was undoubtedly learned, as well as the opening of a door to get at food, it was just like maze behavior, composed of too many reflexes. Skinner needed simpler behavior, and by accident really (see section 1.8. of chapter two), came up with a lever, held up by a spring, but easily pressed down by the rat (see figure 16):

Notice however, that I am again getting ahead of the story here; for the experiments to be examined in chapter two, start, not with the Skinner box (as in figure 16), but with the apparatus shown on figure 15 above. The conditioned response he proposed to study in this box was "pressing the lever" (SB, 62). Thus was born the famous Skinner box.

6.15. So far we have seen how the direction of Skinner's early experimental work is shaped by various things, from external influences to pure accidental contingencies. Finally he was able to quantify his results, and finally he had
the apparatus to study the single — first eating reflex and then — lever pressing reflex of an intact organism. It seemed that his early prospect of a science was on its way to success, for he thought he had accomplished the first step of joining together exact research into the single reflex with an intact, freely moving organism. From this point on, the experimental papers came one after the other, each describing some characteristic of that single reflex, much in the spirit of both, Sherrington and Pavlov, as well as Loeb and Crozier.

Before we go into that research however, we have to consider the two more general assumptions of the reflex tradition (Laudan's methodological and ontological assumptions), that together with Kuhn's exemplar, direct Skinner in his early search for an independent science of behavior.

7. THE PROSPECT OF A SCIENCE

7.1. The title of the paper "On the Conditions of Elicitation of Certain Eating Reflexes" — previously mentioned — "hinted at my theoretical position" (SB, 60) says Skinner, and the first sentence of that paper left no doubt: "The behavior of an intact organism differs from the reflex activity of a preparation chiefly in the number of independent variables" (Skinner, 1930b, 433). The independent variables are eliciting stimuli, and their number, he is saying, is the main difference.

7.2. There is another difference however, and it concerns the unit Skinner had just committed himself to, with his
successful use of Pavlov and Sherrington. They had defined the reflex as a necessary reaction to a stimulus, adding a reference to internal physiological mechanisms (e.g., the digestive system, and the nervous system). Skinner however, emphasized the whole organism. That influence came from Crozier and Loeb. In the paper "Tropisms" (mentioned earlier) Crozier scoffed at the attempts to define tropism as a definite response of an animal with a nervous system (Crozier shared Loeb's resentment to 'organ physiologists'), claiming that such a definition was "obviously meaningless" (Crozier, 1928, 214). A tropism, he says, "is most effectively defined as an oriented movement in an energy field" (ibid).

This emphasis on movement agrees well with Skinner, for he is looking for a definition that applies at the behavioral level -- a molar definition, to show that the term applies to the whole organism -- not just to specific organs. And of course, he had reason to be optimistic, for he had already shown the eating reflex to apply at the behavioral level.

As we saw in the last section, Crozier was quite worked up about that finding, for though Skinner found a "reflex", the point was, that Skinner had found "quantitative" or "functional interrelations" (to use Crozier's own terms -- see ibid, 213) between environment variables and the behavior of an organism considered as a whole. No wonder Crozier was pleased, for this paralleled exactly his own methodology.

7.3. Skinner had just started to look into books on
the history of reflex action, when a new book caught his
attention. This was Franklin Fearing's *Reflex Action: A
Study in the History of Physiological Psychology* (1930).
"I was curious to see what the author was doing in my field"
(SB, 62, underlining added), says Skinner, but from the first
pages it was obvious that Fearing did not share Skinner's
optimism. In the preface to the book Fearing says that the
reflex arc concept has come to play a major role in psychological
and physiological theorizing:

The stimulus-response formula embodied in the reflex
is suited admirably to the interpretation of the simpler
forms of behavior, as shown, for example, by the
artificially isolated spinal segments in the experimental
animal. (Fearing, 1930, x)

Fearing wants to argue however, that all attempts to
extend the application of this principle to complex behavior
have been unsuccessful. There are those "who do not find the
interpretation of all human behavior in terms of reflex action
wholly satisfactory" (ibid, 3-4) he says, and his whole book
is directed at showing just this. Fearing reviews the
various authors that have contributed to the study of reflex
action, and while he is sympathetic to the exposition of their
more moderate claims, he is quick to point out (and quote),
whenever he can find them, the author's own statements
concerning the dangers of extending the reflex to higher
mental faculties.

Fearing even wages a war against the reflex as applied
to simpler behavior, and concludes that even such simple
behavior as the knee jerk and:
perhaps all other 'simple' spinal reflexes, CANNOT BE REGARDED AS ISOLATED UNITS OF FUNCTION IN THE INTACT NERVOUS SYSTEM. (ibid, 277; emphasis in original)

7.4. This was too much for Skinner to swallow. He wrote a vitriolic review, accusing Fearing of prejudice, and showed it to Crozier. He 'tongue down a phrase or two', added his name as co-author, and published it in the Journal of General Psychology (Skinner and Crozier, 1931). This review is very interesting for our purposes. In it Skinner, for the first time, outlines his view of 'the new science of behavior', which turns out to be exactly the opposite to Fearing's view. The quote above that the reflex cannot be regarded as an isolated unit must have touched Skinner especially. Fearing also claimed to show that the reflex was an over simplification and that it was inadequate as an explanatory principle in accounting for the behavior of an intact organism.

7.5. Skinner and Crozier counter just about every point made by Fearing, arguing that the "reflex is exactly comparable to any other well established scientific concept ...", and that it "is the conceptual expression of a corrélation between certain observed events (called, in this case, stimulus and response), and has no validity beyond this correlation" (ibid, 126; underlining added). They further point out that Fearing (and others) are systematically ambiguous as to what they mean by the term reflex, and they show how Fearing endorses different definitions at different places in the book. Among the various definitions Skinner and Crozier.
mention are: that the reflex is involuntary, unlearned, unconscious, invariable, predictable and uniform, and not conditioned by consciousness (see ibid, 127).

Their argument concludes that:

It is obvious that if all behavior is reflex, then it is meaningless to define reflex as involuntary, unlearned, unconscious, or as employing limited neural tracts. The surviving condition of the definition is that of predictability and uniformity, which is, as we have seen, the statement that there is a correlation between observed stimulus and observed response which can be expressed in scientific law. (ibid; underlining added)

To this they add that theories of reflex action from Descartes to Pavlov "have never been able to point to more than some aspect of this correlation", and that it is an open question whether "reflex action can actually be established for the total behavior of the intact organism " (ibid).

It may seem curious that Crozier is the co-author to such a review, especially as it concerned the reflex, but the point of the review concerned methodology not ontology. As the quote above shows, the emphasis is on observed responses and the predictability of that response. We can thus see an emphasis on Crozier's methodology of functional interrelations of environmental variables and the movements of the whole organism.

7.6. Reviewing Fearing's book obviously helped Skinner to formulate his general position, and he now clearly visioned the science of behavior to develop around the notion of reflex (understood in this special way as the correlation of an observed stimulus and an observed response), as the basic
unit of that science.

Skinner took some time off at the end of his second academic year (1929-30); but spent the major part of the summer working on reflexes in lobsters at a Marine Biological Laboratory. Late summer 1930 Skinner returned to Cambridge, determined to defend his new thesis from the review, that the reflex could indeed be extended to the behavioral level.

7.7. Skinner started working on a long paper, quite properly titled "The Concept of the Reflex in the Description of Behavior". To defend his thesis from the review of Fearing's book, he obviously had to look into the history of the term. For that purpose he examined the experimental literature on reflexes from the middle of the seventeenth century down through Magnus and Pavlov. To do a thorough job, he even had to go to the original in many cases.

Skinner's examination of the history of reflex action was no ordinary review. In fact, it was much more of an interpretation, for rather than attempting to find out how these various reflex theorists defined the term, he was more concerned with how they actually used it, and how they should have defined it -- given their use of the term. In fact, Skinner admits this much. He says that the reader should be aware that he is not giving an exhaustive account of the history of the reflex:

Certain historical facts are considered for two reasons: to discover the nature of the observations upon which the concept has been based, and to indicate the source of the incidental interpretations with which we are concerned. (CR, 431)
The main purpose of this examination of the reflex was to insist upon the correctness of the concept in the description of behavior. And once again Skinner's old source of influence provided the clue.

7.8. Somewhere Russell had said that "the term 'reflex' in physiology had the same status as the term 'force' in physics", Skinner explains in his autobiography, adding he knew what that meant because he had discussed P.W. Bridgman's *The Logic of Modern Physics* (1927) with Cutbert Daniel, who was working with Bridgman (SB, 66-7).

Here both a course on the history of science Skinner audited and Poincaré's *La Science et l'hypothèse* (1902) came in handy. Skinner's paper is an operational analysis of the term reflex (on a par with Mach's and later, Bridgman's analysis of the term force in physics), or an attempt to evaluate the historical definition of the term. He undertakes to come up with an alternative definition, "not wholly in despite of the historical usage" (CR, 431). Skinner's argument is just the one he asserted in the review of Fearing's book, that "tentatively ... we may define a reflex as an observed correlation of stimulus and response" (ibid, 442). He further points out that the negative characteristics which describe the reflex as involuntary, unlearned, unconscious, or as restricted to special neural paths, have proceeded from unscientific presuppositions concerning the behavior of organisms.
When you say, for example, that Robert Whytt discovered the pupillary reflex ... we do not mean that he discovered either the contraction of the iris or the impingement of light upon the retina, but rather that he first stated the necessary relationship between these two events. So far as behavior is concerned, the pupillary reflex is nothing more than this relationship. (ibid; underlining added)

7.9. Skinner's survey of the history of the reflex revealed just what he had claimed in the review of Fearing's book. The reflex could be defined, he says both in the review and in "The Concept of the Reflex in the Description of Behavior" paper, as an observed correlation of two events, stimulus and response. He added the condition of the necessity of that correlation, for when a new reflex is discovered, it consists in exactly the fact that this stimulus elicits that response. So once you have a stimulus in a reflex you necessarily have a response, and vice versa. What makes this an operational redefinition is Skinner's claim that whatever the negative definitions (e.g. unlearned, unconscious, etc.) amount to, they are all consistent with his new definition. This does not mean that the reflex physiologists were wrong in their definitions, or that they should adopt Skinner's new one, but only that this is what the reflex minimally means once it is stripped of its physiological connotations.

Essentially, the point is this. Reflex physiologists have defined the reflex in physiochemical terms, and rightly so. For the purposes of the science of behavior however, the concept should be stripped of its physiological character.
The behaviorist is not interested "in the validity of that concept, but in its nature" (CR, 446). Put differently, the issue is, What do all the different definitions of (or all the uses of) the term reflex have in common? Or again, What minimal definition is consistent with the historical usage? And the answer, for Skinner, is obvious:

A survey of the history discloses no other characteristic upon which the definition can legitimately be based. The physiological investigation does not question the correlative nature of the reflex, for its data and its concepts deal essentially with the conditions of a correlation ... (CR, 448; underlining added)

But why can Skinner not just accept Pavlov's definition of the reflex (necessary reaction to a stimulus, through a nervous path)? And why does Skinner emphasize an observed correlation in his own definition? Why indeed all this fuss about a definition, for are definitions not arbitrary and tautological? Can he not define the term any way he wants, so long as he is consistent in his use of it?

7.10. There is only one way to answer these questions, and when it is noticed that Skinner substitutes "observed" for "through a nervous path" in Pavlov's definition of the reflex, we see that Skinner is directing the definition from physiology to psychology, or from organs to organisms. The reflex is not a property of organs, but organisms. This is the special twist to Skinner's research tradition, or his attempt to redefine a term deriving from Sherrington and Pavlov, to the whole organism as emphasized by Loeb and Crozier. When Skinner says that in his redefinition of the
reflex he is emphasizing the "essential continuity between reflex physiology and the special science of ... behavior" (CR, 447), he is speculating on the metaphysical commitments of the reflex tradition. He is committed to the ontological correlation of two events, a stimulus in the immediate environment, and the organism's reaction, and so as to move the reflex to the behavioral level he conveniently adds "an observed correlation", to the definition. Adding that condition to the definition may seem arbitrary at first, but it is absolutely crucial for Skinner, as he can now look for these eliciting stimuli in the immediate environment. The response moreover, is observed, and is thereby moved up a level, from being an inner physiological reaction, to being physical movement, in just the same way tropistic movement was for Loeb and Crozier.

7.11. One must not think however that the concept of the reflex is misused by this redefinition, Skinner is quick to point out, and we must not:

fail to recognize a well-grounded distinction between the two fields; which is based primarily upon a difference in immediate purpose: The one seeks a description of the reflex in terms of physio-chemical events, the other a description of behavior in terms of the reflex. It is assumed that the word REFLEX refers to the same thing in both instances. (ibid; emphasis in original)

I will later come back to this interesting claim that the concept of the reflex refers to the same thing in both cases, but at present only want to emphasize that Skinner's concern with definitions reflects on his ontological commitments. It is the influence from Loeb and Crozier that
is crucial, for he wants the reflex to apply to the movements of the whole and intact organism.

Now, that Skinner has freed the definition of the reflex of its physiological connotations, how does it apply to behavior? Indeed, what is this behavior of an intact organism? Behavior must include, for Skinner, the total activity of the organism; i.e. the functioning of all its parts. More specifically, Skinner says:

We are interested primarily in the movement of an organism in some frame of reference. We are interested in any internal change which has an observable and significant effect upon this movement ... we are interested ... in what the organism DOES. (CR, 448; emphasis in original)

8. A RESEARCH PROGRAM

8.1. With his new definition of the reflex, Skinner thinks he has finally found a solid ground for his 'new and independent science of behavior'. For when he regards behavior as the functioning of an intact organism, "the reflex is important in the description of behavior because it is by definition a statement of the NECESSITY of this relation" (CR, 449; emphasis in original). And if we further reduce, as Skinner does — at Mach's suggestion — explanation to description "and the notion of function substituted for that of causation" (ibid), then it is clear that Skinner has, not only a prospect for a new science, but a clear-cut research program for that science as well.

What Skinner really has, at this point, is his methodology of functional analysis. He does not quite say so yet, though
function is substituted for causation, and he wants to go from
description of behavior to "analysis" (CR 449). The first
step in the analysis is to isolate a response and to identify
its correlated stimulus. The function that expresses this
correlation can be written as:

\[ R = f(S) \]

where \( R \) is a response and \( S \) a stimulus. One may vary the
strength of \( S \) known to elicit a certain response, and record
the variations in the strength of \( R \). The experimental findings
indicate a threshold, for example, where it is found that
below a given value of \( S \), the \( R \) is equal to zero. One can
also investigate the temporal aspects of the function, to
measure latency and after-discharge.

But there is a second field of investigation, Skinner
points out, that involves more than one elicitation of a
reflex. Here we are concerned with "variations in any aspect of
a correlation, as they may appear in the comparison of
successive elicitations" (CR, 454). We might, for example,
repeat eliciting the reflex under a fixed value of \( S \) and
find out that there is progressive decrease in the value of
\( R \). This phenomenon Sherrington called reflex fatigue. But
this does not apply strictly to equation (1), for though
these findings:

\[ \text{do not challenge the necessity of the relationship}
\text{expressed therein (as they might well do if they}
\text{were less orderly), ... they do require that, in the}
\text{description of a reflex, account be taken of THIRD.}
\text{VARIABLES. (ibid; emphasis in original)} \]

This second equation can be written as:

\[ R = f(S, A) \]
Where $A$ is a variable designed to account for any given observed change in the value of $R$. Thus $A$ can be time (in refractory phase) or the number of elicitations at a given rate (reflex fatigue).

The physiologist would explain reflex fatigue by reference to, say, synaptic change or some other physiological state, but:

in the description of behavior, where we are only secondarily interested in these physiological inferences, reflex fatigue is nothing more than an orderly change in some measured aspect of a given correlation. A law describing the course of that change, where the independent variable is time or the number of elicitations or some other condition of the experiment, is particularly a law of behavior. It may become a law of the synapse, by virtue of certain physiological inferences, but it has by that time passed beyond the scope of the description of behavior. (CR, 454; underlining added)

8.2. Let me now clarify what Skinner's methodology of functional analysis amounts to. He is interested in behavior, and in the independent variables of which it is a function. A science of behavior, for Skinner, should not go beyond the behavioral level, whether that level be physiological or mentalistic, not because that is improper per se, but just improper for a science of behavior. We have here already the essentials of Skinner's methodology -- a position that he later further strengthens both in his attacks on physiological explanations of behavior (in The Behavior of Organisms, mostly), and especially in his frequent attacks on mentalistic explanations in later books. Skinner's attitude towards both is really forced, given this.
methodology. We will later be in a position to see more
of the details of Skinner's views on these matters, but
must add, now a minor worry he has -- a worry that later
becomes a major problem. This is Skinner's conception of
reflex strength which, surprisingly enough, has really
nothing to do with the corresponding physiological notion.

8.3. One peculiar difference that Skinner notes
between the methods in equation (1) and (2) is that in the
latter, but not in the former, a change in one variable
affects changes in other aspects of the correlation as well.
If, for example, the ratio of $R$ and $S$ in equation (2) is
changed, we may expect, Skinner says:

> to find all other ratios, as well as the threshold,
latency, and after-discharge of the reflex, likewise
changed. It is usual, therefore, to regard the
particular change which we chance to observe as a
sample of a greater process. (CR, 454-5; underlining
added)

As Skinner now has to speak in terms of these group changes,
"it is almost necessary to have a term describing the STATE
of a correlation of any given time with respect to all its
aspects" (ibid, 455; emphasis in original). The physiologist
has here an easy way out of course, for he can attribute
the change to the synapse, or some other physiological state,
but Skinner has blocked that kind of explanation out of his
science of behavior. What he can say though, is that the
reflex is strong or weak. He can attribute strength to the
correlation. He says:

"Reflex strength" expresses in a very general way the
state of a given correlation at a given time with respect
to many of its characteristics. It is a useful term,
for it permits us to deal with reflex fatigue, for
example, as a CHANGE IN REFLEX STRENGTH, without stopping
to specify the particular changes which compose it.
(ibid, 455; emphasis in original)
It is clear that the reflex, construed in this way, leads to two kinds of laws. The first describe correlations of stimuli and responses. Thus the reflex, so defined, is just such a law. Secondly there are laws (of second order) that deal with changes in any aspect of the primary correlation. These laws describe changes inflicted by third variables and may be described as changes in reflex strength. Skinner says about this latter kind of laws (clearly not anticipating the major change he is starting that in:

the behavior of intact organisms the apparent variability of specific stimulus-response relationships emphasizes the importance of laws of the second sort. (CR, 456)

And adds that conditioning, emotion, and drive, so far as they concern behavior, "are essentially to be regarded as changes in reflex strength" (ibid).

8.4. For this part of the argument, Skinner had something up his sleeve. He had followed up on his successful research on 'eating' reflexes (see the discussion of Skinner's paper "On the Conditions of Elicitation of Certain Eating Reflexes" in section 6.13. of this chapter) by a more thorough analysis. He worked backwards "from the preistalsis of the alimentary tract to swallowing, chewing, seizing food that touched the lips ..." (SB, 71). The last step of the analysis was how the rat approached the food.

Later Skinner would talk of a chain of reflexes, but for now he is just bothered by the aspect of his definition that says the correlation of stimulus and response is
necessary. We remember that the rat in the tilted runway would sometimes wait and only 'eventually' start another run and get more food. This is starting to bother Skinner, for how could this be a reflex (i.e. a necessary correlation) when the rat would not always eat the food available? Later this would become a genuine problem for Skinner, but for now he says that:

The answer ... lay in accounting for the variability [in running the tilted runway]. If the change in strength is orderly, we may "assert the necessity of the reflex relationship", as Sherrington asserted it in spite of such a change as reflex fatigue. (SB, 72)

8.5 It is not the point of this section to point to the problems Skinner first encounters in his research program, or how he first tries to explain them away. That will be done in chapter two, but still it has to be emphasized how his two major problems of definition and measurement derive, in the first place, from his research program. Both of these problems do derive from his definition of the reflex as a necessary and observed correlation of stimuli and responses, in the following way. As all stimuli and responses are necessary correlations, then all responses should always follow all stimuli, or else we would not have a reflex. But as Skinner had already begun to worry about, his rats in the tilted runway, made a special point of waiting after eating, i.e. after the stimulus was delivered.

The measurement problem on the other hand, was generated from the observed part of the correlation, for as has already been argued, that meant behavior as a physical movement.
But if that is the case, it is hard to see how any of the measures deriving from Sherrington or Pavlov could be of use to Skinner, as they were all measures concerned with specific organs. Obviously Skinner could not count drops of saliva to determine reflex strength, and as we will see more clearly in chapter two, Sherrington's various measures of reflex strength were also of little use to him.

8.6. Armed with this new prospect of a science, Skinner was convinced that this was the way to do the science of psychology. He was certain that the concept of the reflex (understood in his own idiosyncratic way) embraced the "whole field of psychology" (SB, 70). He had further drawn up a sketch of a research program, which was, effectively, to divide behavior into reflexes (postural reflexes, eating and flexion reflexes, etc.), devise measures of their strength, and then search the fields of conditioning, drive, and emotion, for the "third" variables of which that strength was a function.

Thus he had, or so he thought, all the ingredients, methodological and ontological:

(a) the functional analysis of environment/behavior relations,

(b) the definition of his reflex-unit, as the necessary correlation of stimulus and response,

(c) the experimental apparatus of the 'precurrent' behavior of pressing the lever as his reflex, and

(d) some experimental evidence that behavioral variation (e.g. of the 'eating' reflex) could be measured by changes in reflex strength.
8.7. Skinner explains his enthusiasm with this new proposal of a science in his autobiography quite humorously:

I thought this was a suitable project for a Ph.D. thesis, and when I ran into [professor] Beebe-Center one day I told him what I proposed to do. He stared at me for a moment, and then said, "Who do you think you are? Helmholtz?" ... He insisted that I had more than I needed for a thesis and should submit it at once. (SB, 70)

And so he did. The first half of the thesis was the historical analysis of the concept of the reflex, that he had already published as "The Concept of the Reflex in the Description of Behavior" (Skinner, 1931), and the second, the experimental analysis he had so far on the 'eating' reflex. Here too he relied on a publication, "On the Conditions of Elicitation of Certain Eating Reflexes" (Skinner, 1930b).

"Now ... I had", says Skinner the reflex psychologist, in retrospect, armed with the alleged fact that all behavior is reflexive, "something to say ... or so I thought" (SB, 71).

8.8. Late 1931 Skinner submitted his thesis, consisting of the historical research on the reflex and the experimental work on hunger as a drive (the 'eating' reflex), to the psychology department at Harvard. He was worried though, since none of this work had originally been intended as a thesis. He also knew that all theses submitted to the department had to go through Boring, who usually "had the last word -- and usually a great many words before that" (SB, 72). He suspected that Boring would not be very sympathetic to his approach, and expected problems.

Boring, in fact, criticized the thesis quite severely.
Skinner's redefinition of the reflex bothered him especially. "I felt that you may be distorting history", he said, and added that Skinner had "given a very broad, strange, almost bizarre meaning to the word REFLEX" (quoted in SB, 73; emphasis in original). His point was that Skinner put the word to a new use altogether, and that he did not really need a historical paper, but much rather "propaganda and a school" (ibid). Though this might seem a reasonable objection (I will have more to say about Boring's criticism later) Skinner was not impressed. His answer was clear and characteristic:

I submitted the thesis again essentially unchanged, with a note containing a couplet from Thomas Hood's "Bridge of Sighs":

Owing to her weakness, her evil behavior,
And leaving with meekness her sins to her Saviour.

(SB, 73)

Boring again advised a complete revision, and again Skinner ignored his comments. Boring could do no more; he appointed a committee (Crozier was in it) and Skinner got his degree just before Christmas in 1931. Having passed his examinations, Skinner was no longer a graduate student. But it was now January 1932 and he was still on a Walker Fellowship. He expressed interest in a postdoctoral, since he was eager to continue his research. The psychologists would recommend him for a special fellowship, but it had to be spent abroad. Again it seemed that Boring wanted to get rid of him, but Crozier urged Skinner to apply for a fellowship from the National Research Council. That fellowship came through, and once again Skinner moved back to physiology.
So for the academic year 1932-33 Harvard appointed Skinner a Research Fellow in General Physiology.

8.9. Crozier's new department of physiology was expanding and got a large share of the new Biology building that was just completed. Skinner, though only a research fellow, got an office and two soundproof rooms for experimental purposes two floors underground. He was eager to get his research going again, and built four identical sets of apparatus of 'repeating problem boxes' -- as he called them to begin with. The problem box (or Skinner box as it became known later) had a lever and an automatic food dispenser. With a cumulative recorder attached to the box, he could see, with just a glance at the curve, how the rat was performing -- i.e. he could see the rate of responding or reflex strength as he still called it.

Skinner was now in a position to study the reflex behavior of intact organisms, emphasizing the single reflex of an organism considered as a whole. Thus he had his experimental conditions perfected, his methodology of functional analysis, and his research program. He would vary numerous stimulus conditions (the independent variables) and study the (supposed) orderly changes in reflex strength (dependent variable), given the processes he had under some experimental control. So far he had studied the fields of deprivation and satiation almost exclusively, but soon added others (e.g. conditioning, extinction, etc.), as we will see in the next chapter.
9. SUMMARY OF CHAPTER ONE

9.1. This chapter has been concerned with the development of Skinner's views from the time he gave up on literature in 1927-28, all the way up to 1931-32, after he received his doctorate in psychology from Harvard University. By that time Skinner had a definite prospect of a science, concerned with the study of reflex characteristics of whole and intact organisms, and I argue, a sketch of a research program as well. The main focus of this chapter is on the developments that led up to this research program. That program is consequently understood as Skinner's own exemplification of the research tradition of reflex physiology through the exemplar use of Sherrington and Pavlov, in the sense discussed below.

9.2. I have emphasized how sudden Skinner's decision was to turn from literature to science, and how arbitrary it was too, for as he says himself, the scientific study of behavior 'was said to be psychology' about which he knew next to nothing. This makes the considerations of the early scholarly influences on young Skinner all the more pertinent, I argue, as he must have been quite susceptible to the various authors, although he seems always to have wanted a rigorous and experimental science of behavior. The reason for that was probably the fact that he wanted to get as far away from literature as he possibly could, while at the same time keeping his eye on behavior.

The discernible influences of Loeb, Thorndike, Russell,
and Watson were discussed, and an emphasis put on their shared interest in a science of behavior of some sort. Though quite different, they all regarded behavior per se as a subject matter of scientific investigation. The crucial influence however, must have been Pavlov, for Skinner read the translation of his Conditioned Reflexes the year it was published in 1927. I argue further that Pavlov's signalization interpretation of the conditioned reflex is of major importance, for not only was Skinner directly influenced by it, but so were Watson and Russell. Although both had started out with their own units of analysis habit-formation and learned reaction respectively, they both came to reinterpret their own units in terms of the conditioned reflex. Thorndike's important law of effect is also considered, for though he couched it in subjective terms himself, both Russell and Watson interpreted that law in terms of the substitution of stimuli through signalization.

9.3. Skinner started graduate work at Harvard in the fall of 1928. He was not impressed by the department however, and was overjoyed once he took courses in physiology. There he read Pavlov again, and was introduced to Sherrington's work on spinal reflexes and Magnus' postural reflexes. Skinner soon tried to mimic their experimental results, but after a few unsuccessful attempts, he was led to research on the tropistic movements of simpler organisms. He became acquainted with Crozier, the head of the department of general physiology. Crozier had been a student of Loeb,
and shared his methodology of the functional analysis of tropistic movements in lower organisms.

9.4. Skinner had found two separate and seemingly incompatible influences in physiology, Pavlov's and Sherrington's reflex physiology on the one hand, and the general physiology of Loeb and Crozier. The incompatibility stemmed from the fact that the former emphasized the reflex characteristics of specific organs in surgically restricted animals, while the latter emphasized functional inter-relationships of environmental variables and the movements of intact organisms considered as a whole.

Skinner was not impressed by the latter unit of analysis, tropism, and during his second year at Harvard again tried to mimic the experiments of reflex physiologists, first Magnus, then Sherrington, and soon also, of Pavlov. This attempt turned out to be crucial for Skinner's scientific development, for though it took him some time and effort, he finally found what he was looking for. This was the 'eating reflex', which he found he could manipulate with food (obviously) and measure, first with the delays in running towards the food, and then more simply with the measure of the rate of running.

9.5. Once Skinner found his individual reflex, and a way to control it and measure, he exclaimed that he had 'made contact with Sherrington and Pavlov at last'. I argue that this is a perfect example of Kuhn's theory of the paradigm in the sense of the exemplar, which is the analogical use of the problem/solutions of predecessor
theorists, to the scientist's own experimental interests. This Kuhn argues, is how the typical young scientist learns his trade, and not through the prior use of the more general laws and principles of the science in question.

I argue that this is exactly the case with Skinner, for he exclaims he has made contact with Sherrington and Pavlov just on the basis of the fact that he has found his own individual reflex on the analogy with their experimental problem/solutions. Skinner has made contact with Pavlov in the sense that his measure of reflex strength is analogous to Pavlov's counts of the drops of saliva, and also in that both used food as the stimulus eliciting the reaction to be measured.

The contact with Sherrington is made through the fact that the experiment started out as an attempt to mimic his flexion reflex experiment, and again the issue is not the question of whether Skinner found the same reflex or not, but much rather that he found a similar reflex on an analogy with Sherrington's flexion reflex. The similarity is that both reflexes were inborn, or unconditioned as Pavlov would say, but the difference was that Sherrington worked with "spinal dogs", while Skinner studied intact rats in a simplified version of the running maze.

9.6. Philosophers of science have long emphasized the formal structure of scientific theories as the proper tool for the understanding of science. The young apprentice learns his science, it is typically stated, through the
rules, laws, and principles of that science. This case study can not support such a view of science however, and refers the reader instead to Kuhn's more realistic theory of learning through examples. What the young scientist learns in textbooks of science, Kuhn says, is not so much rules and regulations, but much rather how to do examples. The rules and laws are there too of course, but they have little empirical content, Kuhn insists, if the examples would not come first. What the young scientist learns through such repeated examples, according to Kuhn, is to see similarity relations between different problem-contexts; he acquires the ability to apply problem/solutions of one context to another.

This is what Skinner did, I argue, when he went over into physiology, and brought the problem/solutions of Sherrington and Pavlov to bear on rats running in mazes. It is the main purpose of this chapter to show in detail how this learning of a science through the exemplar, actually happened in the case of B.F. Skinner.

9.7. There are two further points that support Kuhn's theory of the exemplar. The first one has to do with the fact that most of the development detailed in chapter one happened during Skinner's first two years at Harvard, and as has already been indicated, the year before that he knew next to nothing about virtually any science. During the years 1928-30 Skinner learns the tools of his trade, which can not be regarded a long time of training by any
standards, but still this was all he needed.

By 1931 Skinner had already published a number of papers, two sets of which became his thesis. One set was the experimental work on the eating reflex already mentioned, and the other was a historical analysis of the concept of the reflex; an operational analysis of the concept from Descartes to Pavlov.

This brings me to the second point, for his analysis is evidence of the fact that once Skinner had successfully made use of Sherrington's and Pavlov's problem/solutions to the context of his own interest, he immediately begins to consider broader issues, such as how the reflex has been defined and measured throughout the history of reflex action. This again supports Kuhn's analysis, for as he insists, the successful application of the concrete prime examples come first, and the rules, laws, and principles of the more general paradigm in the sense of a disciplinary matrix, come after that.

9.8. I do not use Kuhn's concept of the disciplinary matrix however, mostly for reasons that will be discussed in later chapters. I should say here though, that it is essentially Kuhn's monotheoretic account of the paradigm that I object to, for I do not share his conviction that during normal science only one paradigmatic theory is generally and dogmatically accepted. Nor do I believe that such a general theory is dogmatic and conservative to the extent he does.

Instead I use Laudan's conception of a research tradition.
to denote such a general theory. For Laudan the research tradition is understood in the sense of a succession of (often rival) individual theories, and though I do refer the reader to chapters two and three for some of the arguments for this decision, it is apparent that I have already talked about three theories within the research tradition of the reflex physiology: the theories of Sherrington, Magnus, and Pavlov. Laudan's distinction between individual theories and the more global research traditions is of prime importance in this case study, for it shows how a research tradition can develop through the introduction of new theories, and more importantly it shows too, how one theory can turn the unsolved problems of another one into anomalies.

9.9. The research tradition of reflex physiology, or the reflex tradition for short, is the tradition Skinner is committed to, once he has made use of Pavlov and Sherrington in the exemplar sense discussed above. His further research into the history of the reflex is evidence of the fact that he recognizes this commitment, which I argue to be a set of constraints, methodological and metaphysical. These constraints are such that once committed to the tradition, the scientist can only violate them by abandoning the tradition. This is the sense in which Skinner is committed to the reflex tradition, for when he redefines the reflex as the necessary correlation of stimulus and response, he is committing himself to the elicitation assumption. That is the fundamental assumption of the tradition, for it stipulates that there can be no stimuli without responses
(in a reflex), and no responses without stimuli. That latter claims moreover, directly connects to Pavlov's signalization interpretation of the conditioned reflex, and it is this constraint, in the end, that is crucial in Skinner's revolution from the reflex tradition. The operant, as he will come to use that term, is by definition, a response class not elicited by a stimulus in the immediate environment.

The ontological commitment is basically a commitment to the reflex as the unit of analysis, and though Skinner can talk in terms of stimuli and responses, these are only of interest to him if connected in the sense specified by the elicitation assumption.

9.10. There is one final aspect that Skinner is committed to, for in his definition of the reflex he says also that it is an observed correlation of stimuli and responses. I argue that this is basically an influence from the methodology of Loeb's and Crozier's functional analysis. Their unit of analysis, tropism, is one level beyond the reflex as used by Pavlov and Sherrington. The latter took the reflex to be a property of organs, but Skinner shares Loeb's and Crozier's emphasis on the whole organism. Consequently he emphasized observed stimuli and responses, for it enables him to extend the reflex from organs to the movements of the whole and intact organism.

To enable the reader to see more clearly the intricacy of these early influences on Skinner, and how they led him
to his sketch of a research program, I present the following diagram:

(diagram 1)

EARLY INFLUENCES:

Reflex Physiology: General Physiology:
Sherrington, Pavlov Loeb, Crozier

DEFINITION OF THE UNIT OF ANALYSIS:

Reflex \( \stackrel{df}{=} \) The necessary reaction to a stimulus through a nervous path

Tropism \( \stackrel{df}{=} \) An oriented movement in an energy field

MEASUREMENT OF UNIT OF ANALYSIS:

Reflex Strength: is determined by the intensity of the stimulus eliciting the reflex

Tropism Movement: The amount of directed movement as turning per unit length of path.

RESEARCH PROGRAM:

The reflex characteristics of specific organs in surgically restricted animals

Functional interrelationships of environment/reaction in whole, intact, organisms

SKINNER'S THESIS (FROM 1931):

The study of reflex characteristics as functional environment/behavior correlations in whole, intact, organisms.

FIRST EXTENSION OF THE REFLEX:

The physiological concept of the reflex arc as a property of specific organs is extended to the behavior of whole, intact, organism.

FIRST REDEFINITION OF THE REFLEX:

Observed and necessary correlation of stimulus and response.

FIRST MEASURE OF REFLEX STRENGTH:

Response rate during continuous stimulation.
SKINNER'S SKETCH OF A RESEARCH PROGRAM IN 1931:

Divide behavior into reflexes, devise measures of their strength, and search the fields of drive, conditioning, and emotion, for the variables of which that strength is a function.
REFERENCES

1 Skinner had met Frost briefly the previous year at a summer school of English at Bread Loaf, Vermont. At Frost's suggestion Skinner had sent him some of his work, or three short stories.

2 The whole letter is printed in Selected Letters of Robert Frost, edited by Thompson (1964, 326-27). All the quotes above are from 327.

3 I will adopt the convention of referring to Skinner's books by capital letters, as explained in the ABBREVIATIONS.

4 B. Frederic Skinner and William S. Skinner, A Digest of Decisions of the Anthracite Board of Conciliation. (Skinner and Skinner, 1928). Though this is Skinner's first publication I will keep matters simple by referring to The Behavior of Organisms (1938) as his first book, as the former one has nothing to do with any of his later work.

5 The critical reader may here be aware of a not too uncommon fallacy. To establish a point -- whether it be a particular historical fact or an integral part of some sophisticated argument -- it is always dangerous to refer, uncritically, to autobiographies. But although it is probably never completely clear to what extent the author is interpreting his own behavior or even attempting to make it intelligible, it should also be mentioned that Skinner's autobiography is very remarkable in just this respect. As many reviewers (Burke, 1976 and Baer, 1976) did point out, Skinner is well aware of this danger, and keeps all rationalizations and ascriptions of intent to a bare minimum, as a good behaviorist should. I will return to this point later.

6 Apparently Skinner believed this quite seriously. In the second volume of his autobiography he explains that in a course at Harvard called 'The Psychology of the Individual', he was not at all impressed by the professors 'Freudian or Jungian analysis'. At the end of the first lecture Skinner went up to him and said 'You are a LITERARY psychologist' (SB, 27: emphasis in original). The professor probably took the statement as a compliment, but from Skinner's point of view, 'it was the worst thing [he] could say' (ibid). Further evidence similar to this is throughout his autobiography, some of which we will encounter later.

7 By this Skinner evidently means Comparative Physiology of the Brain and Comparative Psychology (Loeb, 1900).
REFERENCES

8 This form of explanation was a direct consequence of Darwin's theory of natural selection. Since that theory implied a continuum between species, it was thought that one could discover in animals the signs of the incipient mentality. G.J. Romanes' Animal Intelligence (1882) was the main work in this area, full of anecdotes of 'intelligent' animals exhibiting humanlike characteristics. Darwin himself had started this kind of approach in The Expression of the Emotions in Men and Animals (Darwin, 1872).

9 Loeb's approach received crucial support from the British comparative psychologist Lloyd Morgan, who in his An Introduction to Comparative Psychology (Morgan, 1894) put forward this famous dictum (known as 'Lloyd Morgan's cannon'):

IN NO CASE MAY WE INTERPRET AN ACTION AS THE OUTCOME OF A HIGHER FACULTY, IF IT CAN BE INTERPRETED AS THE OUTCOME OF THE EXERCISE OF ONE WHICH STANDS LOWER IN THE PSYCHOLOGICAL SCALE. (53; emphasis in original)

10 As Skinner explains, his book "Verbal Behavior... might indeed have borne the subtitle, The Meaninglessness of Meaning" (Skinner, 1971b, 347). We will have occasion to discuss the extension of the operant to the verbal field later on in this thesis.

11 J.B. Watson, Behaviorism, 1930. (Reprinted from the original 1924 edition.)

12 B. Russell, 1927. Also published as An Outline of Philosophy that same year.

13 I find this statement quite interesting, for though Skinner does not exactly say so here, it is easy to see that he was -- as the typical 'scientist' very selective in his evidence. Indicators of this are throughout his autobiography, some of which is quite striking, if not humorous. For example, he says that Walter Hunter suggested that they jointly publish a paper Skinner did on insight (in the spirit of Yerkes and Köhler), "but, 'insight' was not a respectable word for a behaviorist, and I refused" (SB, 31).

Another incident is when, on a ship that took him across the Atlantic the summer he began at Harvard, Skinner met another graduate student in psychology at the University of Iowa; "she knew much more about psychology than I, but she was not a behaviorist, and that was that" (PML, 312; underlining added).

14 As Skinner points out, Russell is wrong here, in that Thorndike had published on the associative processes in animals much earlier, or in a monograph from 1898: "Animal Intelligence", Psychological Monographs.

15 In the conclusion to the 1898 monograph (p.153ff), Thorndike notes that he only has a "summary of the results" (153) and that his modest study of association in animals has only "given us a working hypothesis for a comparative psychology" (155).
REFERENCES

16 What Russell is objecting to is the use of mentalistic (subjective, he says) terms in an experimental science. An immediate objection might be that he substitutes one problem for another, as his proposed reformulation is clearly and overtly teleological. And how, one may ask, is it more objective to say that the animal does one thing so as to receive another (intended?), result? However, this problem need not detain us here, for as will be seen in Part Two of this thesis (when Skinner attempts to extend the operant to a larger domain), we will find that Skinner takes exactly the same line as Russell (see sections 6 and 8 of chapter three).

17 "The Place of the Conditioned-Reflex in Psychology" (Watson, 1916). The whole paper was read by Watson in December of 1915, as the address of the president before the American Psychological Association at Chicago. In his autobiographical sketch "John Broadus Watson" to A History of Psychology in Autobiography, Vol. III, edited by (Wirth and Murchison, 1961), Watson says that a large part of the material sketched in my 1915 address as President of the American Psychological Association was contributed by [K. Lashley]. I am sure I remember that he first used the term CONDITIONED EMOTIONAL REFLEX in one of my seminars (277; emphasis in original).

18 These summaries, says Watson, were made by "Yerkes and Morgulis, and more recently by Morgulis alone" (ibid), although he gives no references. That seems to indicate that American psychologists knew of Pavlov's work some years before 1915.

Dover had just published Anrep's translation that same year (1927). Notice, by the way, now no one gets Pavlov's name right!

20 Only later did Skinner find out that this was old news; for at Harvard he was not at all impressed by the department. Which is one reason he turned to physiology -- as we will see later.

21 When discussing his first impressions of Harvard in his autobiography, Skinner says that he "was a BEHAVIORIST, and for me behaviorism was psychology. I had been converted to the behaviorist position by Bertrand Russell" (SB, 10; emphasis in original).

22 Skinner's first year at Harvard was not entirely without its bright behavioristic spots however. Walter S. Hunter of Clark University drove in from Worcester once a week to give a seminar in animal behavior" (SB, 44). That course was evidently to Skinner's liking, but it lasted only one term -- as Hunter was not a permanent member of the department. Further evidence that his influence was not lasting has already been noted (in footnote 13 of section 3.4. above), when Skinner refused to jointly publish a paper with Hunter, on insight in animals.

23 See, for instance, his paper "Tropisms" (Crozier, 1928). There Crozier reports on the research begun by Loeb, referring to no less than 38 earlier papers of his own; all experimental and on tropisms or forced movements in various animals and plants.
REFERENCES

24 All of Skinner's early papers (as well as a series of Skinner's early experimental papers proper -- to be discussed shortly), appeared in the Journal of General Psychology for the simple reason that there were not many options available at that time, and more importantly because a certain W.J. Crozier was among the editors of this one.

25 Curiously enough, and probably indicative of Skinner's stand at that time, this paper is said to come from the "Laboratory of General Physiology" on its first page (p.102), but from the "Laboratory of General Psychology" on the last page (p. 112).

26 Loeb had started that field, and was the head of the division of general physiology at the Rockefeller Institute for Medical Research from 1910 until his death in 1924.

27 A part of this problem is technical, of course. How was Skinner to study the reflex characteristics of rats if he could in no way restrict their movements? Even worse, where was that single, individual reflex of the whole organism, and how could it be measured?

28 Skinner is quite frank in the autobiography about his general knowledge of psychology. I have already indicated that he was not impressed by the department, and he says he came to the four three-hour examinations that were supposed to test his comprehensive knowledge of the major areas of psychology "totally unprepared" (SB, 34). He got borderline grades, and they were barely offset, he says, by his considerably higher grades in physiology.

Passing these comprehensives enabled Skinner to concentrate on physiology. "I never learned how to read the 'literature' in psychology," he said, nor was he "ever to learn much more psychology at Harvard" (ibid). Even by 1936, when he started teaching undergraduate psychology at Minnesota, he says "the fact that [he] was keeping a jump ahead of the class that first year must have been obvious" (SB, 191) to his students.

29. Skinner tells us that his reason for not transferring to the physiology department (as Crozier had already asked him to do -- see section 4.8. above) was mostly because he was given total freedom to use the machine shop in psychology. As he puts it, he was confirmed in his "choice of psychology as a profession not so much by what [he] was learning as by the machine shop in Emerson Hall" (SB, 31). As we will see later, this decision was by no means final. Skinner in fact moves between fields a number of times.

30 Clark Hull and Edward Tolman (Skinner's two main rivals within the behavioristic tradition) both made extensive use of the maze. The same is true of course, of J.B. Watson. Even Fred S. Keller (Skinner's best friend and fellow student at Harvard) based his thesis on the maze, though later he became the first "Skinnerian". (I will have occasion to say more about Keller later).

31 All the following figures are adapted from Skinner's paper "A Case History in Scientific Method" (Skinner, 1956), reprinted in CR, 101-24, except the last two (fig. 15 and 16).
REFERENCES

32 This kind of behavior is frequently observed in squirrels. If one tosses a nut towards one, the squirrel will approach a few steps, then withdraw a step or two, approach again, withdraw, and so on. It has been pointed out to me by Professor Nancy Innis that E.C. Tolman had already reported on the same kind of behavior. See for instance "Prediction of Vicarious Trial and Error by means of the Schematic Sowbug" (Tolman, 1939). Tolman seems to have first reported on this in his "Behavioristic Theory of Ideas" (1926).

33 For Laudan's comparison of these to his own model see (Laudan, 1977), 73-8.


35 As Laudan himself admits, "there are numerous common elements between my model and those of Kuhn and Lakatos (and I readily concede a great debt to their pioneering work) ..." (1977, 78).

36 It follows that I will have to take issue with Laudan's rather unfortunate comments that because he is unable "to follow the logic of [Kuhn's] later changes of mind, [he has] been forced to characterize Kuhn's views in their original form" (Laudan, 1977, 231, footnote 1).

37 This is "the component of a group's shared commitment", says Kuhn, "which first led me to the choice of that word [paradigm]" (1969, 187).

38 Interestingly enough, Skinner starts explaining this small but annoying detail with the least effort possible -- as he had already encountered conflicting reflexes. But when he tries to identify them (whether it be Pavlov's "freedom reflex", Sherrington's "orientation", "adaptation", or even "autocoid", which was the latest idea in the current physiological theory of hunger, he always ends up in the contradiction that the delays should get shorter, whereas the opposite was actually the case.

39 The quotes here refer to Skinner's own notes, which he seems to have written and compiled all throughout his scientific career. He consults these again and again while writing his autobiography, thus lending it important objective support, as this minimizes the threat of ad hoc rationalizations so many scientists fall into, when they no longer remember why they did a particular experiment.

Skinner's colleague at Harvard, Robert Epstein, has recently started to edit these notes, confirming that Skinner seems to have written these continuously throughout his career. They were published as Notebooks: B.F. Skinner (1980), edited, and with an Introduction by R. Epstein. In that introduction, Epstein says that every time he "finished editing a stack of notes, Skinner discovered another untapped shelf or file drawer ... Even with this volume complete, I am still far from the top of the hill" (vii).

40 The reader should be warned right here that I am getting a little bit ahead of the story. Thus Skinner did not talk in terms of rate of responding at that time (1930), nor use the cumulative recorder. He did so however
REFERENCES

a few years later, but that time-lag is very important, and will be explained in detail in chapter two. As explained there the fact was that Skinner talked in terms of reflex strength, and did not realize that he was measuring no such thing. (The quote above is from his autobiography, which of course explains the experiments in terms of his final theory.) Anyway, the reason I include these things here is that I am only highlighting the use of the exemplar, not how Skinner interprets his results.

41 This diagram (slightly changed for our purposes) appears in Skinner's paper "Drive and Reflex Strength" (Skinner, 1932a), 25.

42 Boring had been on leave during Skinner's first year at Harvard. Now he was back as the head of the psychology department. He knew Skinner had some problems with his newly found reflex, the eating reflex, and warned him that he should "avoid EATING BEHAVIOR. It is potentially humorous" (SB, 62; emphasis in original). As we will see in the second chapter, this was not the only criticism by Boring that hit the mark, though Skinner finds that hard to admit.

43 This figure is slightly adapted from Skinner's (BD, 49).

44 "Because", says Skinner, "the paper needed more authority than my criticism of Miss Vicari. It was automatically published in the Journal of General Psychology" (SB, 64). Crozier was one of the editors of that journal, so the publication was automatic.

45 Accepted for publication by Crozier, in the Journal of General Psychology (Skinner, 1931), and reprinted in CR, third edition, 429-457. In this case, as always when a paper by Skinner is reprinted in CR or RBS, the reference will be to the book. This paper was later to become the first half of Skinner's thesis, though he did not intend it to be.

46 Here the Boston Medical Library came to good use, as it had many of these original texts. Skinner was reasonably fluent in both German and French -- he read, for instance, Descartes' "Traité de l'Homme", and even found a text by an engineer, Isaac de Caus, describing the hydraulic automata that so much impressed his contemporary, Descartes.

47 Daniel was, as we remember, Skinner's friend from the physics department -- the one who had shown him how to plot his results on logarithmic paper.

48 Along with the courses Skinner took in his second year from Boring and Crozier (see section 6.13. above), he also sat in on a course on the history of science taught by L.J. Henderson. Skinner remembers the lectures as very interesting, "often brilliant", and he took them "seriously" (SB, 49). He bought Sarton's Introduction to the History of Science (Sarton, 1927), the first volume of which was just out, and would later benefit from his lectures, as he taught the second term of that course. He was even more impressed by Sarton than Henderson, and readily applied their ideas to his growing interest in the concept of the reflex. For example, when Henderson and Sarton agreed that a creative idea seldom bursts upon the world fully formed, but would rather grow step by step, it led Skinner to write the following note:
REFERENCES

Similarly, it would no doubt be impossible to arrive at the belief that all behavior is reflex if a preparatory step (that some behavior is reflex) had not long ago been taken. (SB, 58)

49 Henderson had drawn Skinner's attention to E. Mach's *Science of Mechanics* (1919). In Mach and Poincaré, Skinner says, he "found early versions of what was beginning to be called operationism" (SB, 66).

50 You get a strong reflex, for example, if the threshold is low, the latency short, the after-discharge prolonged, and if the ratio R/S is large. If the threshold is high, the latency long, the after-discharge short, and the ratio R/S small, however, then the reflex is said to be weak.

51 Boring suggested, among other things, that the historical chapter be left out altogether, and that Skinner should instead examine such things as correlation vs. cause, entity vs. construct, reflex vs. habituations, instinct, consciousness, etc., and "probably half a dozen other things that have not yet occurred to me because I have not been writing this paper" (quoted in SB, 74). A big part of the problem here, as Skinner soon found out, was that the department was 'out to get him', as they felt he had gone over to Crozier and physiology. Upon hearing this Skinner quickly "put out a peace feels" (SB, 74), suggesting that an operational analysis of psychological terms was needed, and that it would take him "little more than a rather mechanical application of a few principles" (ibid). Could he not take this on instead of preparing for the defense? I took the question to Beebe-Center. Would the department excuse me from anything more than most perfunctory examination if I prepared an operational analysis of half a dozen key terms from subjective psychology? Beebe-Center was so astonished by my proposal that I did not wait for his answer. (SB, 75)
PART ONE CONTINUED -- THE EXPERIMENTAL STAGE 1932-34

CHAPTER TWO

PROBLEMS WITH THE REFLEX TRADITION
1. EARLY CHANGES FROM THE REFLEX TRADITION

1.1. One of Skinner's first experiments after his doctorate was, interestingly enough, to study 'spontaneous activity'. He reports that this was a "serious subject" (SB, 77), as Curt Richter and others had done experiments on running wheels (Richter, 1927, see also Richter, 1922 and Stier, 1930). Skinner's source here, evidently, is Crozier, for in the paper already discussed "Tropisms" (Crozier, 1926), he had quoted Richter and adds that apart from directed movements (i.e., tropistic or externally controlled) there is also:

the origination of movements from stimulation arising within the organism, which consequently appear as SPONTANEOUS MOVEMENTS, and which seem usually regarded as devoid of constraint as regards their incidence in time. (Crozier, 1928, 235; emphasis in original)

As we will see in more detail later this actually anticipates Skinner's elicitation problem (as I will call it) and paves the way to the discovery of the operant. Without going into that in detail here, let me say though, that the operant is originally defined by Skinner in just this way, i.e. as behavior devoid of constraint as regards its incidence in time, or as behavior not under the control of elicitative stimuli (or stimuli in the immediate environment). The point I wish to make here is just that this shows clearly that the influence of Crozier's general physiology is just as powerful a factor in the eventual discovery of the operant as Pavlov's reflex physiology is; it is just that the latter influence is more obvious. And the reason for that
of course, is that influences of ontology are always more obvious to trace, than influences of methodology.

1.2. When Skinner now moved back to the physiology building (as a research fellow in physiology), he brought with him a running-wheel that he had originally built for squirrels he had kept earlier. He now tried the apparatus on his rats to see whether their spontaneous running would give a satiation curve like the experiments on 'eating reflexes' had. The rats could run the wheel when they wanted, at their own speed (hence the "spontaneous"), and had free access to food. The wheel was connected to a cumulative recorder so that revolutions on the wheel were recorded on a curve.

Skinner was mainly worried about two things in the "Spontaneous Activity" paper. The first is how to measure spontaneous activity (as the title of the paper implies), for the rat, of course, does not run the wheel continuously, but only intermittently. "The problem of spontaneous activity", he says in that paper, "arises in this apparent variability" (Skinner, 1933b, 20). In solving it "we need not go beyond a quantitative description of strength as a function of some ... independent variable" (ibid), adding that no physiological correlate is necessary to supplement the explanation. Again he echoes the methodology of Crozier, and points to his experiments on 'eating reflexes' (from the thesis and to be discussed again in the following section), saying that the spontaneous activity curve is very much like his normal
eating curve. This indicates that he has again found a way to measure the reflex activity of an intact organism, considered as a whole, and also found the independent variables (i.e. eliciting stimuli) of which that reflex is a function.

But spontaneous activity, "presents a special problem" (ibid, 20) he says, and this leads us to the second worry he has in that paper. Since the activity is intermittent or variable, and since "the outstanding characteristic of spontaneous activity is the suddenness of the transition from rest to action" (ibid), then doubtless there is an external stimulus responsible for that change. But 'spontaneous' activity is usually defined as 'a response "for which the correlated stimulus is unknown"' (ibid, 19). This second worry then is the role of the stimulus in spontaneous activity, and Skinner is quick to point out that one need not assume an internal stimulus. What he emphasizes instead is the sudden increase in variability after each long delay in running, and:

\[ \text{it is supported by the reasonable assumption that the sudden change in variability indicates the operation of a factor extraneous to the group previously controlling the behavior. (ibid; underlining added)} \]

But what is this reasonable assumption? And why is it reasonable? Why indeed did he perform this experiment? In answer to this we only have to go back to the definition of the reflex, for in defining his unit as a necessary correlation of stimulus and response, Skinner is committed to look for stimuli that elicit responses, especially those
responses that seem "spontaneous".

1.3. This paper is very important for the development of Skinner's experimental work, especially when we consider how he answers these two worries just outlined. Later we will see how these become his two major problems -- the problems that lead him, eventually to the discovery of the operant -- but I call them mere worries now, because of the way he answers them.

The problem of measuring the single reflex he now thinks he can answer by appealing to the increased variability. And that further indicates a "compensatory increase in the rate of running" (Skinner, 1933b, 19-20). What he does not realize is that he is actually proposing a new measure of reflex strength. Rate of response is the new measure (i.e. as indicated by how steep the cumulative curve is or how much the variability has increased), instead of the customary stimulus intensity measure. But this is (as yet) neither a problem nor an answer to one, at least not until Skinner extends the use of this new measure to eating reflexes. He will do that however in the two "Drive and Reflex Strength" papers (to be discussed shortly), but even then he does not seem to realize the change he is starting away from the reflex tradition, so I hesitate to call this a problem.

The way Skinner attempts to answer the elicitation problem, i.e., the problem of finding the eliciting stimuli to apparently spontaneous responses, makes it even clearer how cavalier Skinner is in his answers. He does realize that the eliciting stimulus in spontaneous activity is
nowhere to be found, but explains the problem away by some fancy rhetoric very uncharacteristic of Skinner:

The supposed unknown stimulus implied in the use of the term spontaneous must act, not to elicit progression, but only to FACILITATE its elicitation by ... stimuli. This is an important distinction. (Skinner, 1933b, 19; emphasis in original, underlining added)

1.4. Skinner also did some experiments with the so-called double-alternation maze. They were quite uneventful (and indeed a curious sidestep for Skinner to take, especially as he had already rejected the maze as an instrument for his purposes), but as they were, like the running wheel experiments, totally automatic, Skinner devoted much time of that term [winter 1932] to theoretical issues" (SB, 81).

He read further in the literature on the reflex, and tried to determine how to carry on the interesting results obtained in the second half of his thesis. He decided to expand on those results, and in two papers reporting his further experiments, we see that he was still very much committed to the reflex tradition.

1.5. In the former of these papers "Drive and Reflex Strength" (Skinner, 1932a) Skinner argues that the central problem in the study of hunger "is to account for the appearance or non-appearance of a given sort of behavior at a given time" (ibid, 22). Put differently, the problem is, he says, to account for the variability in behavior. Remember that for Skinner the reflex is an observed and necessary relation between stimulus and response, so the
question why the response is not always evoked by the appropriate stimulus is a problem for him. Earlier, he had claimed that this variability was to be explained by reference to a third variable, such as time (see section 8.3. of chapter one). He then went on (section 8.5.) to ask what it is that changes as a function of time. It was not reflex fatigue, threshold, or any singular reflex that changes, but the whole lot. "We lack a term to stand for the totality that undergoes the change" (ibid, 33). The physiologist, he then adds, may conveniently talk here of the synapse or some such physiological structure, but Skinner the behaviorist, (i.e. functionalist) has to remain at the level of observation -- hence at the level of behavior. His answer at this point is simply to say that he will use the term reflex strength, for this purpose.

This again, is Skinner's proposed change in the meaning of the strength of the reflex, or his recommendation that we use response rate as that measure. Later we will see how this differs from the typical measure in reflex physiology proper.

1.6. But apparently Skinner was beginning to have doubts about the reflex nature of his research program, for he adds, significantly, that the term reflex strength:

has never been used in any very exact sense. The strength of a reflex is given by the value of its threshold, the ratio of the values of its stimulus and response, the duration of its latency, the amount of its after-discharge, and so on. It is thus a convenient term in describing a process that involves all these factors. (Skinner, 1932a, 33; underlining added)
Skinner is really understating his case here, although he could only admit so later, for according to the reflex action tradition, reflex strength is simply determined by the strength of the stimulus. The stronger the intensity of the stimulus, the stronger the response (and so too the reflex). In the quote above Skinner says that he is using reflex strength in a new way, but then immediately reverts back to Sherrington's terminology, saying that the strength of a reflex is determined by its threshold, ratio, latency, etc. In the second paper "Drive and Reflex Strength: II" (Skinner, 1932b) he still holds doggedly to Sherrington's terminology saying that "a given rate is determined by the refractory phase of the reflex initiating the behavior" (ibid, 47). But this new insistence on reflex strength as something underlying change, was now beginning to bother Skinner, for his dependent variable was, really rate of responding. This was his most sensible and crucial variable. By varying the environmental conditions (i.e. "third variables") he could control the rate of responding. But in no way did he realize the importance of this difference in these early experiments, though later he could say:

If I had never heard of Pavlov, Magnus, or Sherrington, I should have seen that my basic fact was the RATE at which an organism engaged in a particular kind of behavior, but because of my exposure to reflex theory I wanted rate to be a measure of reflex strength. (SB, 8); emphasis in original

1.7. The second thing that was starting to bother Skinner was the requirement of elicitation of responses by stimuli. This worry can be seen to be generated by
the former problem, for reflex strength, he has just stipulated, denotes the 'underlying state' of a change in behavior, which is to say that it applies to behavior as a whole. Each reflex is elicited, Skinner still thinks, but as reflex strength is a measure of behavior as a whole, it refers to a chain of reflexes rather than an individual reflex. If that is correct, it would seem that only the second order laws (in the terminology of his previous paper "The Concept of Reflex in the Description of Behavior" -- see section 8.4 of chapter one) apply to it. The first order laws, as we remember, all make reference to elicitation, while the second apply to behavior once elicited. The question is then, to which type of law does the new reflex strength apply? The problem is not so much the question whether 'eating reflexes' are actually elicited, but rather whether the initial reflex plays any special role in a chain of reflexes.

In the former "Drive and Reflex Strength" paper Skinner could not really tell, for the apparatus was such that the rat would push open a lightly balanced door to get at food on a small tray. The behavior of the rat in approaching, seizing, and eating a piece of food, by assumption, he says, is a reflex:

or more accurately it is composed of a chain of reflexes, which are closely interwoven ... the response to one stimulus brings up the stimulus for a subsequent response. In the initial members of the chain certain visual, tactual, and olfactory stimuli initiate movements of orientation, first of the head, then of the body, followed in turn by movements of progression. New stimuli are supplied internally and externally ... (Skinner, 1932a, 31; underlining added)
1.8. To get at the question of the importance of the initial reflex, Skinner makes a crucial change in the apparatus, for the experiments reported in the later "Drive and Reflex Strength: II" paper. He says:

The plan of the present experiment is to add an arbitrary initial member to this sequence [of the approaching, seizing, and eating, chain]. The food tray is accordingly replaced by a repeating "problem box", which delivers a pellet of food into an open trough each time a horizontal lever is pressed downward. The response to the lever is thus added to the sequence. (Skinner, 1932b, 40; underlining added)

Although introducing the lever did answer his question (the rate of eating did turn out to be "independent of the nature of the particular reflex with which the eating behavior begins" (ibid, 47), it only served to increase Skinner's uneasiness with the elicitation-assumption. Due to this conclusion he is now ready to deny any importance to the initial reflex. In fact, he says:

there is no one initial reflex of this sort. We cannot control the behavior of the rat adequately enough to insure the administration of an invariable stimulus or the elicitation of an invariable response. The number of possible initial reflexes, even in the simple act of picking up a pellet of food, is therefore infinite ...

(ibid, 46)

1.9. What immediately strikes one here is this denial of any importance to the initial reflex, as well as the admission that even in such a simple case as this one, the number of initial reflexes can be infinite. That should really play havoc with his research program, for as we remember it first required that behavior be divided into reflexes. But given this admission on Skinner's part, there
now seems no way of getting that program off the ground as the first step could never be completed.

Skinner does not seem worried by that however (he will soon though, especially after Boring criticizes him on just this issue), but for now let us see just how fragile the elicitation assumption already is. The lever is the new initial stimulus and eating food the initial response. That seems problematic however, for if one response can be regarded as the stimulus for another, then how quickly the other follows should depend on its threshold and latency, and on the after-discharge and refractory phase of the former response. But as Skinner can only admit later, "the lever was the stimulus, not the preceding response" (SB, 81; emphasis in original).

Skinner was not ready to face this problem yet however, for in concluding the second "Drive and Reflex Strength" paper, he again reverts back to Sherrington's terminology, saying that "a given rate is determined by the refractory phase of the reflex initiating the behavior" (Skinner, 1932b, 47).

2. THE DISCOVERY OF ONE-TRIAL CONDITIONING

2.1. The three papers just discussed (the "Drive and Reflex Strength" papers as well as the one on spontaneous activity) are not only important in the long term, as the first evidence of an eventual and complete break from the reflex tradition. They are also important in the short term, as the changes Skinner has already made (or suggested) in apparatus (from a food tray to a lever), in definition
(from a physiological reflex arc to the reflex as a necessary correlation of observed stimuli and responses), and in measurement (from Sherrington's various measures of reflex strength in terms of latency, refractory phase, and after-discharge to the response rate measure of reflex strength) led him on to a new track. As Skinner explains in his autobiography:

in my experiments on the rate of eating I had added pressing the lever as a new "initial reflex", but I had not paid much attention to how my rats learned to press it. My main interest was conditioning, however, and I now tackled it head on. (SB, 87)

So far Skinner had studied the hunger-drive nearly exclusively. He had talked of the eating reflex and found it to depend on the (third) variables of deprivation and satiation. But as he had now introduced the lever to the experimental situation, he saw the opportunity to make a move to conditioned reflexes. The eating reflex was a typical Sherringtonian unconditioned reflex, but the rat did not press the lever as an unconditioned reflex. Pressing the lever is an arbitrary response, and not an unconditioned reflex like eating is. Skinner's experimental setup is therefore coming closer to Thorndike's (previously discussed) problem box (indeed Skinner calls his lever-apparatus a "repeating problem-box") from which Thorndike formulated his law of effect and law of exercise.

2.2. But there is an important difference between Thorndike's procedure and Skinner's. Skinner would carefully introduce the rat to the conditions before the actual
experiment began. His rationale was that this way the change in the rat's behavior (in the experiment) could not be attributed to the various external conditions prevailing (e.g. sound, odor, or whatever) as it would be used to all of that by the time the experiment began. The rat could not become conditioned upon the lever either, Skinner reasoned, for the lever was always in its lowest possible position before the experiment began. As the rat could not yet press it, the lever could not become conditioned upon food. All this was undoubtedly an influence from Pavlov, who always emphasized how important it was to control conditions. ('Control your conditions and you will see order' seems to be a cherished Soviet maxim).

Thorndike, on the other hand, had talked about the trial and error process of learning, by the stamping in of successful behavior and the stamping out of unsuccessful. He would draw a "learning curve", showing the gradual increase of the former, at the expense of the latter, so that the animal, after a sufficient number of trials, was very quick to find its way out of the problem box. But since Skinner prepared his animals in the apparatus long before the actual experiment began, the difference was this:

In carefully controlling my conditions I had eliminated almost all the unsuccessful behavior in Thorndike's "learning curve" BEFORE CONDITIONING TOOK PLACE. There was nothing to be "stamped out". The successful response did not merely survive, it was conspicuously strengthened. So was the successful response in Thorndike's experiment, but the evidence was not to be found in his "learning curve". (SB, 88; emphasis in original)
2.3. The results of Skinner's first conditioning experiments with the lever were quite startling. The rats began to respond at a high rate as soon as a single response was followed by the delivery of food. The rats "learned to press the lever in one trial", Skinner aptly explains in his autobiography, "and no learning could be faster than that" (SB, 89).

But notice that although the behavior of touching the lever should not become more frequent until the experiment has begun, it might still be argued that this approach begs the question at issue. The point is, the objection might continue, that the typical Skinner-box is quite simple -- so simple, in fact, that the rat has literally nothing to do but press the lever (and approach the food-tray). A simple rejoinder is that this is simply not correct, as the lever is down and not movable until the experiment begins, and that it is a contingent matter whether the delivery of food can become conditioned upon an arbitrary response (such as pressing a lever). Anyway, and as we will see later the linguist N. Chomsky has made much use of just this point in his criticism of Skinner's later work (see section 8.10. of chapter three).

2.4. By this time Skinner clearly knew he was on to something; but was apparently having difficulties in expressing exactly what it was. He started speculating, tentatively, that he was on to some "process of conditioning that was different from Pavlov's and much more like learning
in daily life" (SB, 89). It was first in a letter to his friend (Fred S. Keller) that Skinner proclaimed he had found a new process of conditioning. But when pressed for details, Skinner in reply reverts back to the old formulation, saying that Pavlov's:

principle of conditioning is still good with me. This is simply an interpretation of the learning curve. It has at least two novel points: (1) It assumes, thought not necessarily, that the act of conditioning, given one invariable response and one invariable stimulus, may take place completely with one occasion... (2) It takes account of the fact ... that upon two successive occasions the stimulus ... varies considerably. (SB, 89-90)

2.5. Skinner reports these new results in the paper "On the Rate of Formation of a Conditioned Reflex" (Skinner, 1932c). There he finally makes a distinction between two types of conditioning. He says that in addition to Pavlovian or Type I conditioning there is also another type (Type II), "in which the developed reflex is originally present and is only strengthened during the process of conditioning" (ibid., 274). The experiments on 'eating reflexes' and 'lever reflexes' are given as examples. In Type I (Pavlovian conditioning) a new stimulus is substituted in the elicitation of a response, and a new reflex is formed. Skinner expressed this as:

\[ S \rightarrow R \]

\[ S^1 \rightarrow R^1 \]

Let \( S - R \) and \( S^1 - R^1 \) be reflexes observable prior to conditioning (\( S \) could be food, \( R \) salivation, \( S^1 \) the sound of a bell, and \( R^1 \) some response or other -- not salivation,
though). If the two reflexes are now elicited a sufficient number of times (remember that as far as Skinner knew Pavlov needed at least seven trials), a new and conditioned reflex comes to be observed: \( S^1 \rightarrow R^1 \) (or salivation at the sound of a bell, in our example).

2.6. Now, there is a "second type of conditioning" (Skinner, 1932c, 274), says Skinner, in which an already established reflex "is only strengthened during the process of conditioning, \( S^1 \rightarrow R^1 \) requires another formula" (ibid):

\[
S^1 \rightarrow R^1 \rightarrow S \stackrel{R}{\longrightarrow} R
\]

Let \( S^1 \) be (the stimulus) the lever, \( R^1 \) the 'arbitrary' response of pressing it, while \( S \) is food, and \( R \) is eating. Both reflexes (i.e. \( S^1 \rightarrow R^1 \) and \( S \rightarrow R \)) "are observable prior to conditioning"(ibid), Skinner thinks, but the \( S^1 \rightarrow R^1 \) appears only as:

a weak "investigatory" reflex, which, if elicited alone, would normally disappear through adaptation. If the response \( R^1 \) is now made to be followed immediately by the stimulus \( S \), a change may be observed in the strength of the reflex \( S^1 \rightarrow R^1 \) ...

(ibid, 274-5)

He points to a further difference in the two types of conditioning, in that in Type I two of the original four terms (\( S \) and \( R^1 \)) may eventually be disregarded, and one (\( R^1 \)) may disappear altogether. But in Type II, Skinner (erroneously) thinks, all four terms are retained. The two reflexes "are chained together experimentally", he claims, "and they remain in that relation. \( \text{No new reflex is established} \)" (ibid, 275; underlining added).
2.7. Skinner seems thus to have given up on the attempt to use Pavlov's Formula on all conditioned reflexes. But immediately after making the distinction between the two types, he adds:

Type I and II are alike, nevertheless, in requiring as an essential condition for the appearance of the phenomenon the approximately simultaneous elicitation of two reflexes ... What is actually observed in either type is a change in the quantitative relationship of a stimulus and a response ... (Skinner, 1932c, 275)

This paper was very crucial for Skinner, for though he moves very cautiously, accumulating all the similarities he can find between the two types, he is, nevertheless, stating that there are two different (i.e. neither is reducible to the other) processes at work. Little did he know that about the same time, two Polish physiologists, J. Konorski and S. Miller, were trying to persuade Pavlov of the same distinction. They had done experiments on dogs, either shocking their leg, or flexing it by hand, 'to produce food'. They saw, just like Skinner, that Pavlov's formula could not account for these results (this will be discussed in more detail later, see section 7 of chapter three). But Pavlov, although he found the results interesting, would only respond that:

Something more has to be introduced into the doctrine of higher nervous activity, something that identifies it with the phenomena of mental life...¹²

leaving the working out of that proposal to others.

2.8. Before we move on to Skinner's more explicit rejection of Pavlov's formula (as accounting for all behavior), to the debate with Konorski and Miller, and to Skinner's
subsequent discovery of the operant, let me first mention some further aspects of Skinner's 1932 position, as these will eventually lead him to two major problems: the measurement problem and the elicitation problem. The interesting question here is whether these are problems for Skinner at this point, or whether they will only emerge as problems later. If the latter is the case, then the present question becomes one of when and how these become problems for Skinner (i.e. when he, instead of explaining the problems away, actually tries to answer them).

It should be emphasized here too, that we are not so much interested in the details of Skinner's current position, or in his actual views at any one particular time, but much rather in the relation between any two views Skinner holds at different times. The emphasis is thus upon the changes Skinner comes to make in his position, how and why he comes to make them, and how they effect his future research. The crucial change of course, is from Skinner the reflex theorist to Skinner the operant theorist, and the process of change in between. To get at this change -- this process -- I will emphasize the emergence of these two problems, of measurement and elicitation, how these become problems and how they led Skinner to the discovery of the operant. I will say for now that these two issues have started to worry Skinner, and only later become problems for him. But as will be emphasized in the next few sections, a number of very specific developments have to occur before Skinner can
realize that these are his problems that need his solution. These problems will force some drastic changes, as we will see, at the expense of the reflex tradition. Thus Skinner will reject some very basic assumptions of that tradition, and my contention is that only then can Skinner be said to have solved his problems, as opposed to explaining them away.

2.9. For this Skinner will have to realize the following:

(a) that these two worries of his will not disappear, but always come back, albeit in a different form (what I will call the transformation of worries and problems);

(b) that these are his worries/problems, but not problems for the reflex tradition from which they are though generated;

(c) that he has to realize that his own position is actually different from Pavlov's;

(d) that not even fellow workers in animal psychology will be able to help him, as they do not encounter (do not understand) his problems; and finally,

(e) that these problems will not be solved except at the expense of some assumptions that underwrite the whole reflex tradition.

Let us therefore in the next two sections see how Skinner tries to explain (get rid of) his worries/problems away, only to find that they come back (transformed) to haunt him again. Later I will raise more general issues,
as concern the question of what constitutes scientific problems; anomalies and discoveries and explain how Skinner came to realize that these are his own problems that need his solution, but for the next two sections let me emphasize the changes in apparatus, definition, and measurement, the relation between these, and Skinner's curious lack of understanding of how severe these changes really are. Essentially it is this lack of understanding that makes it impossible for us to say that Skinner is now encountering problems, as opposed to mere worries.

3. WORRYING ABOUT MEASUREMENT

3.1. When Skinner points out that in the new type of conditioning (Type II) the greater part of the change can take place upon the first occurrence of the $S^1 \rightarrow R$ $S \rightarrow R$ sequence, he mentions another rather embarrassing consequence, if one sticks to the Pavlovian terminology. In the paper "On the Rate of Formation of a Conditioned Reflex" (already mentioned) Skinner pointed out that his one trial conditioning "raises an interesting problem of method" (Skinner, 1932c, 284). He is understating the case considerably, for what he means is that we can measure the strength of a reflex only by eliciting it. We are therefore limited to two measurements -- one taken before, and one after, the event. "It is therefore impossible", Skinner goes on, "to determine the COURSE of the process" (ibid; emphasis in original). Not only that, but:
we can represent the process of conditioning
by plotting the strength of the reflex, not against
time, but against the number of elicitations (since
we can make determinations only at these points).
If the entire process takes place at one elicitation,
the graph will be discontinuous. (ibid; underlining
added).

Suitably translated this statement tells us quite a lot.
What it says, in effect, is that since the process is so
sudden, the Pavlovian paradigm cannot account for it. Put
differently, Skinner has found a process that cannot be
explained in the research tradition from which the discovery
is made.

3.2. Notice what is happening to the reflex strength
measure. In his first paper on conditioning (just mentioned)
Skinner had to admit that:

so far as the response to the lever is concerned, it
is impossible to distinguish between a conditioned
non-hungry rat and a hungry unconditioned rat. The
response is simply lacking in both cases. Between
these two extremes, moreover, it is impossible to
tell from a single observation whether a given
state of the reflex (an observed strength) is
to be attributed to a degree of hunger or to a degree
of conditioning. (Skinner, 1932c, 276; underlining
added)

But "degrees of conditioning" has no meaning in the type
Skinner just discovered, though it is a perfectly legitimate
notion in traditional reflex physiology. This is because
in Type II, conditioning may take place in just one trial,
which is to say that the reflex reaches its maximum strength
given just one trial. The problem which Skinner how faced
was how to measure 'degrees of conditioning' or better,
response strength (or reflex strength, as he still called
it). He had been led to the response rate measure of reflex
strength through his research into spontaneous activity.
As such behavior did not seem to have any external elicitative stimulus, he had been forced to emphasize the sudden change in variability as that external stimulus. That is how he preserved the elicitation assumption. Emphasizing variability in behavior meant that response rate during continual stimulation was his measure. But how was he now to measure the strength of a response that was acquired in one trial? This problem led Skinner still further from the classical reflex strength measure, and to a process distinct, though related to, conditioning — namely extinction.

3.3. Skinner must have known about Pavlov's work on extinction, as he read his Conditioned Reflexes as early as 1928. We remember too, that in his early attempts to extend Sherrington's problem/solutions to his own experiments on the intact organism, he had exclaimed as he did now, that he had "made contact with Pavlov at last" (CR, 110). In the former case the experiment had started out as an attempt to mimic Sherrington's flexion reflex, and now the process of discovery was just as accidental:

My first extinction curve turned up by accident. A rat was pressing the lever in an experiment on satiation when the pellet dispenser jammed. I was not there at the time [as the apparatus was wholly automatic], and when I returned I found a beautiful curve. The rat had gone on pressing although no pellets were received ... but ... more and more slowly as time wore on. (SB, 95)

In his earliest papers Skinner frequently talked of behavior as "adapting out", meaning something like the slow disappearance of an unconditioned reflex. Now however, he saw the relevance of this to Pavlov's process of extinction,
presumably because his measure of response rate gave an accurate curve. Skinner was quite worked up about this finding, as his curve was even more orderly than Pavlov's curves on the salivary reflex. Again we see the importance of the exemplar, and again we see that it turns around the unit of the reflex, its identification, and especially a measurable quantity. Skinner expresses his excitement with this new contact with Pavlov quite well in his autobiography:

I was terribly excited. It was a Friday afternoon and there was no one in the laboratory whom I could tell. All that weekend I crossed streets with particular care and avoided all unnecessary risks to protect my discovery from loss through my death. (SB, 95)

3.4. In the paper "On the Rate of Extinction of a Conditioned Reflex" (Skinner, 1933a), where Skinner reports his preliminary results on extinction, he claims that extinction is the reversal of the conditioning process. In conditioning, he says, the "arbitrary" response of pressing the lever is strengthened, "when the elicitation of the response is followed ... by a second stimulus" (Skinner, 1933a, 114; underlining added). In extinction, however:

the response, once conditioned, is then not followed by the second stimulus, and the strength of the reflex diminishes. Thus conditioning appears experimentally as an increase in reflex strength, extinction as a decrease. (ibid)

Adding that in both cases the rate at which "the response is elicited under constant stimulation" may still be used "as a measure of the strength" (ibid).

3.5. The rest of Skinner's first paper on extinction is occupied with explaining away a certain cyclic or
wavelike form of his extinction curves. He does note however, that although extinction is the reverse of conditioning:

the combined process of conditioning-and-extinction has left a detectable effect upon the organism, which appears experimentally, not as a change in immediate behavior, but as a modification in the rate or in the rate of change of the rate of some subsequent process. (Skinner, 1933a, 119)

What this means is that the extinction curve for a conditioned rat and a reconditioned one is not the same. The latter curve shows more resistance to extinction. This is strange, given Skinner's present notion of reflex strength, for he took care to have the strength the same in both cases. Hence, his reflex strength measure cannot help him here in explaining this resistance. At present Skinner can only say that:

resistance to extinction is properly beyond the scope of this paper. It belongs to a class of effects which have so far failed to receive an adequate experimental treatment. They present a special problem ... (ibid, 121; underlining added)

3.6. We saw, in the paper just described, that Skinner still holds on to his measure of reflex strength, but in a second paper on extinction, a number of interesting changes take place. In that paper 'Resistance to Extinction' in the Process of Conditioning (Skinner, 1933d), the interplay between conditioning and extinction gives Skinner a number of new ideas. First of all, he speaks of a reinforcing stimulus for the first time. Before he would speak of conditioning the rat, but now he reinforces a reflex. The advantages of this new term is that it suggests strengthening of the response and also the fact that whereas conditioning
supposedly takes place in the rat, "reinforcement was the sequence of events in the environment that brought about the change" (SB, 97; underlining added).

Secondly, Skinner points out that the reinforcing stimulus has two distinguishable effects. The traditional conditioning experiment, says Skinner in the 'Resistance to Extinction' paper, is "confined to one of them -- the increase in strength -- probably because it is the immediate observed change" (Skinner, 1933d, 420). This is the way he has used the term until now. But the development of resistance to extinction is equally important, Skinner now thinks. "Indeed it could be argued", he continues, "that extinction is the only appropriate measure of conditioning" (ibid; underlining added).

3.7. Skinner is here suggesting an important change from his first paper on extinction, as rate of responding begins to play a crucial role -- at the expense of reflex strength proper. Notice however, that Skinner is not ready to take the whole step, as he insists that both measures are important, "and we must ignore neither of them" (Skinner, 1933d, 420). But still, his argument is that though either reflex strength or resistance to extinction may be used as a measure of conditioning, he now prefers the latter one. "Contrary to established practice", he says, "the use of the extinction curve is here usually to be preferred" (ibid). The reason for that is the one touched upon before, that the new measure is more accurate than the other one and gives more information. To measure reflex
strength by an extinction curve is applicable moreover to both types of conditioning, while the traditional measure of the amount of conditioning by continual stimulation works on conditioning only if it takes place in more than one trial.

3.8. The resistance to extinction measure, furthermore, gives more reliable information. Instead of having to rely on the measure of reflex strength prior to, and after one reinforcement, Skinner now suggests the following procedure to measure the effect of one trial conditioning, or better, to measure the effect of a single reinforcement. After making the rat fully accustomed to the experimental situation, connect the lever, so that the first press of it is followed by the delivery of food. Then immediately disconnect the magazine, so that no more food (i.e. reinforcement) will be forthcoming:

It will be seen from the record that the rat began to respond again immediately and during the next 40 minutes traced out a typical extinction curve. We are forced to conclude that this is the effect of the single reinforcement. (Skinner, 1933d, 423)

3.9. The extinction curve is a much more delicate measure than the older one. These curves vary considerably (showing that different amounts of conditioning have taken place) and show the cyclic deviation already mentioned. They also indicate a base-line (what Skinner would later call operant-level), which is essentially due to secondary conditioning (e.g. the rat may also become conditioned upon the sound the apparatus makes in a typical experiment, etc.). After listing these (and other) deviations between extinction curves, Skinner concludes that:
the extinction curve supplies considerable definite information about the change occurring at reinforcement. So far as resistance to extinction alone is concerned, some aspect of the curve (its height or area) is a proper measure of the AMOUNT of conditioning. (ibid, 427; emphasis in original)

4. WORRYING ABOUT ELICITATION

4.1. With these experiment on extinction Skinner's doubts about the elicitation requirement became stronger. The interplay between conditioning and extinction, that had already produced a number of ideas, again proved helpful. It seemed that the combination of conditioning and extinction had some relation to the process of discrimination. This process had already demonstrated its importance, especially in experiments on animal perception. In fact it was (and still is) an ideal way to answer questions such as "Can this or that animal discriminate between colors?", or "between different shades of a color?" But in his first paper on discrimination, "The Rate of Establishment of a Discrimination" (Skinner, 1933c) Skinner is quick to point out that he is not interested in discrimination as a "measurement of a sort of capacity" (ibid, 303), but wants rather to follow the course of the development of a discrimination, and determine the properties of the process" (ibid).

As it turns out this research will all turn on the role of the stimulus, and as such, is an important part of the emergence of what I will call the elicitation problem, or the assumption shared by all reflex physiologists as well as psychologists, that every response is to some stimulus
in the immediate environment. But for the moment Skinner is not directly testing this assumption. The point is rather that in his research into discrimination, he will have to introduce new kinds of stimuli. These new stimuli will seem to function differently from the typical elicitative stimulus, so that he will have to face the question, what these "discriminatory stimuli" have in common with the typical elicitative stimulus.

What Skinner wants to test at the moment however, is whether discrimination cannot be seen as the combined result of conditioning and extinction. If discrimination is construed as the capacity of the organism to distinguish between two properties of a stimulus, then the whole process consists of a continued conditioning of one reflex and the concurrent extinction of another. Consequently the process may turn out to be but a special (if complicated) case of extinction.

4.2. Skinner's method of testing this was to use the click made when a pellet of food drops into the food tray as an extra stimulus. He also added light as a similar extra stimulus. The procedure was to reinforce a reflex only when the light was on (or the sound -- these turned out to be equivalent), but not the reflex without any extra stimulation (i.e. lever-plus-light and lever-plus-sound is reinforced, while just pressing the lever is not reinforced). And the results were clear:
It is evident ... that ... by the introduction of a differentiating stimulus, the slope of each curve begins to fall off. The strength of the reflex in response to lever-plus-light remains maximal, but the reflex in response to the lever alone gradually decreases in strength, and a very low value is eventually reached. (Skinner, 1933c, 326)

This result Skinner soon confirmed in the paper "The Abolishment of a Discrimination" (Skinner, 1933e). By showing that he could abolish discrimination by turning this procedure around (i.e. by reinforcing a press of the lever, but not the lever-plus-light), he had established that there was "no separate process of discrimination" (SB, 102).

Indeed, discrimination was just a special case of conditioning and extinction.

4.3. But although there was no separate process of discrimination to be found, Skinner soon realized that this result had some significance for the stimulus. He was already talking about reinforcing -- instead of just eliciting -- stimuli, and much later, in his autobiography, he explains how the experiments just described on discrimination further weakened the elicitation requirement:

The rats gradually stopped pressing the lever in the dark, as if I had stopped reinforcing altogether, but pressed within a few second whenever the light came on. They could be said to be discriminating between two stimuli, light-on and light-off, by responding to one and not to the other. The light was a DISCRIMINATIVE, rather than an eliciting, stimulus. (SB, 103; emphasis in original)

Skinner was not ready to take this step now, however, although it was slowly becoming clear to him that the light did not elicit the response in the sense in which the sound of a bell
elicits salivation in Pavlov's experiments.

4.4. Skinner was again starting a break from the reflex tradition -- his own research tradition, as he had assumed with other reflex physiologists, that 'every movement of an organism is in response to an immediate stimulus', or 'all behavior is reflexive' -- by denying that stimulus would invariably act as a goad to action. So far he has only expressed doubts about the elicitation assumption, with his idea of a discriminative stimulus, and in his third paper on discrimination "A Discrimination Without Previous Conditioning" (Skinner, 1934c), the problem becomes still more obvious. In the first two experiments on discrimination, extinction was made to occur after the response had initially been reinforced, while Skinner now "reduces the problem a step further by beginning the discrimination before either reflex has been conditioned" (Skinner, 1934c, 532). After adapting the rat to the apparatus, the first press of the lever was reinforced, while the light was on. Then both the food magazine and the light were turned off. The rat made only two responses during the next five minutes, and when the light was turned on again:

the rat responded after 39 seconds. Both light and magazine were then turned off [again], and two more responses in the dark occurred during the next five minutes ... By the seventh reinforcement the latency of ... [the reflex] had reached a more or less stable value of about 20 secs., which was maintained throughout the rest of the experiment. (ibid, 533-4)

He adds that this rate was much lower than previously reported (before the rats had responded after just 4 seconds
on average), and explained the effect by induction —
"borrowing the term from Sherrington" (SB, 104). The
explanation goes like this. While the response is reinforced
in the light it should become more frequent, but it is also
concurrently extinguished, as the responses in the dark are
not reinforced. Hence there is an 'inductive effect' from
the dark.

4.5. This result is important for a number of reasons.
First of all, it forced Skinner to make further distinctions
among stimuli. Not only does he now talk about a "discriminatory
stimulus" and a total stimulus, but he also distinguishes
between reflexes by $S_{AB} \text{--} R$ and $S_{AB} \text{--} L$ -- the former
meaning the bar-pressing reflex, while the latter stands
for the bar-pressing-light reflex. Secondly, it is becoming
increasingly obvious that all these experiments on discrimination
are about the role of the stimulus in the process of conditioning,
rather than about discrimination as a capacity of the organism.
And while it is clear that there still are stimuli that elicit
responses (e.g. the sound of a bell still elicits salivation,
etc.), it is equally clear that Skinner's current stimuli,
light-on and light-off somehow affect the response, while
not actually eliciting it. Of course, the lever stimulated
the rat before a response was made and reinforced, but its
effect, Skinner would much later explain in his autobiography:

was upon the probability that pressing would occur ...
I was breaking away from the traditional view of a
stimulus as a goad. (The two concepts were combined
by psychologists ... in a "total stimulus situation".)
The temporal order of stimulus and response suggested
causal action, but it was not the action of a force.
(SB, 105)
4.6. It needs emphasis again that the quote above is from Skinner's autobiography, written some thirty years after the fact. It is therefore written with the hindsight of Skinner's eventual operant theory, but as I have said before, there is no evidence as of yet that these new types of stimuli raise the probability of a response. The fact is that Skinner has just introduced the reinforcing stimulus, contrasted it to a total stimulus, and has just discovered that the processes of conditioning and extinction can work simultaneously, such that discrimination can occur. Skinner is able, at this point, to say that the rate of responding increases in the light, and decreases in the dark. But he does not say, indeed cannot say this, except metaphorically, for what he means is that the reflex strength increases in the light, and decreases in the dark. That is all the lever-pressing-light-reflex means. Another way to put this is to say that reinforcement cannot work on responses (it cannot affect the rate of responding per se) directly, as the response is not Skinner's (current) unit of analysis. Later the response will become such a unit (or rather the operant as a class of responses is such a unit), but for now the reflex is. So reinforcement works on the lever-pressing-light/dark-reflexes, however clumsy that sounds now.

5. WORRIES, PUZZLES, OR PROBLEMS?

5.1. In the first four sections of this chapter we have seen Skinner engaged in the attempt to provide a new measure
of the process of conditioning. Crucial to this development were his discoveries of one-trial conditioning and of extinction as the converse of conditioning. Of importance too, were some changes he made in terminology, from an exclusively eliciting stimulus to a reinforcing and a discriminative stimulus, and from reflex strength to response rate.

The former of these changes will have a crucial effect on Skinner's use of the stimulus concept, as he soon will be forced to admit that the eliciting stimulus is not the only kind of stimulus there is. That in fact, is one of the two major problems Skinner encounters at this stage of his experimental work, as has already been hinted at in section 4 above. The latter change, from reflex strength to response rate, is of equal importance, as we saw in the 3rd section, and it is the other major problem Skinner encounters at this stage of his experimental work.

But while it is true that Skinner is clearly entertaining the idea of an altogether new measure of conditioning, it is equally clear that he is as yet -- 1933-4 -- neither prepared to say that response rate (during extinction) is the only measure (or even a better measure) nor ready to make the conceptual change from reflex strength to response rate. I explained this above by pointing out that although Skinner actually did use a response rate measure (as opposed to any measure of reflex strength used by physiologists), he could not say so, except metaphorically, and in fact, interpreted
his measure as being a measure of reflex strength.

I should point out further, that exactly the same situation is evident concerning the elicitation assumption. As explained in section 7 of chapter one, the elicitation assumption underlies the whole reflex tradition, and it states that nothing is a reflex but a response elicited by a stimulus. But as shown in section 4 above, Skinner does already make a number of distinctions regarding stimuli, presumably in regards to how they function (e.g. reinforcing, discriminative, and total, as opposed to mere elicitative, stimuli). The similarity here with the reflex-response problem, is that while Skinner has really, denied the elicitation assumption, he cannot do so except metaphorically. For as I pointed out in section 4 above, Skinner talks of reinforcing as well as discriminative stimuli, as opposed to elicitative stimuli, but he simply avoids the question altogether, of the relation between the two. The reinforcing stimulus does not elicit a response, he all but admits, but it increases the strength of the lever-pressing-light-reflex. He should say, of course, that reinforcement works on responses, but the reflex is his unit, at the moment, or the ontological category provided by the reflex tradition.

5.2. I have been quite cavalier up to this point in my use of such terms as puzzles, problems, and discoveries. I have in fact used them in an untechnical sense (e.g. "a problem for Skinner" means just that), and for a specific reason. By now however, it is high time I address these issues,
e.g. whether the measurement problem is best explained as an unsolved problem in Laudan's sense, or maybe just a puzzle of normal science in Kuhn's sense. And the same question has now to be faced as regards the elicitation assumption: is it an anomaly in Kuhn's sense, or maybe rather an internal conceptual problem in Laudan's sense?

Moreover, there is a more general question that has to be faced, namely whether this case study of Skinner's early experimental work supports Kuhn's model of normal science turned revolutionary by a growing crisis via anomalies, or whether it is more like Laudan's model that explains the generation of anomalies in terms of rival theories. But as noted above, I have avoided these issues up until now (although I have already taken a stand, in chapter one, on research traditions and exemplars), and characterized the situation more in terms of worries, transformations, metaphorical answers, and discoveries.

All of this will be explained in due course, in terms of a selected version of both models (i.e. Kuhn's and Laudan's). The reason I have avoided these issues up until now is that I wanted to examine the generation of Skinner's research program (understood as a particular instantiation of a research tradition). And for that we had to go far back (1926-28), to a time when Skinner knew next to nothing about either physiology or psychology. Most of Skinner's very early experimental work explained in chapter one occurs well before we can say decisively, whether he faces (e.g.) a puzzle or
a problem. But as the last few sections conclusively show, it is high time I bring in models of scientific change, for not only is it quite obvious by now that Skinner is wrestling with problems of a specific kind, but also that he is encountering some quite severe problems (in a non-technical sense, again) with the execution of his research program. These will become more acute as we continue, but before we can go into that highly interesting part of Skinner's research, let me first analyse in some detail Kuhn's model of normal science turned revolutionary, as well as Laudan's attempted amendment of that model, with his emphasis on such things as rival theories and conceptual problems. Later on I will compare these models specifically, and emphasize their similarities, before I can go into the more interesting part, where they differ. As we will see then, if I may anticipate a little at this point, it is here that this case study of the emergence of operant theory proves particularly helpful, as it helps us choose between these two models on a number of specific points.

5.3. In the chapter on the nature of normal science of The Structure of Scientific Revolutions (op. cit.) Kuhn articulates what he means by normal science "or paradigm-based research" (1970, 25). *He first distinguishes between fact-gathering (experimental problems) and theoretical problems. He distinguishes further between "three normal foci" (ibid) for experimental problems: 
First is that class of facts that the paradigm has shown to be particularly revealing of the nature of things. By employing them in solving problems, the paradigm has made them worth determining both with more precision and in a larger variety of situations ...

A second usual but smaller class of factual determinations is directed to those facts that, thought often without much intrinsic interest, can be compared directly with predictions for the paradigm theory ...

A third class of experiments and observations exhausts the fact-gathering activities of normal science. It consists of empirical work undertaken to articulate the paradigm theory, resolving some of its residual ambiguities and permitting the solution of problems to which it had previously only drawn attention ...

(ibid. 25-7; underlining added)

For convenience I will call these classes of experimental problems, the extender, the predictor, and the articulator. These labels should be self-explanatory, and the first one, further, corresponds with Kuhn's later use of the exemplar.

And as I argued before that Skinner's very early experimental work was derived from just such an extension (in section 6 of chapter one), it follows that Kuhn's first class of experimental problems fits Skinner's case. Indeed, when Skinner is desperately trying to mimic (read: apply to a larger variety of situations) the results of Magnus, Sherrington, and Pavlov (see sections 5.4. to 5.7., as well as 6.6. to 6.11. of chapter one), he is using them as exemplars. When Skinner tries the postural reflex from Magnus, Sherrington's flexion reflex, and Pavlov's unconditioned reflex, he is just attempting "to increase the accuracy and scope" (Kuhn, 1970, 25) of the exemplars. As we saw further in figures I through 16, this type of research is best characterized
by "the invention, construction and deployment of ... apparatus ... designed for such purposes" (ibid), and notice that Skinner rejects each new piece of apparatus (and improves on it) just for the reason that it is not accurate enough.

5.4. The second class of experimental problems (what I call the predictors) is not evident in the bulk of Skinner's experimental work so far discussed, and with one exception, not in his later experimental work either. Kuhn does say that this class is less frequent, and without much intrinsic interest, but I can only confirm the former. For it seems that when Skinner directly tests Clark Hull's suggestion of "disinhibition" (see section 3.6. of chapter three), his own factual results are 'compared directly with predictions from the paradigm theory' (Pavlov's theory of extinction in this case). But maybe the reason this test will turn out to be so important for Skinner is the fact that its result is negative i.e. Skinner does not find any disinhibition effect, and by parity of reasoning no inhibition effect either. This negative result will play a crucial role in Skinner's reaction to the criticism of Konorski and Miller, for it (the negative result) enables him to see that he is really on to a new, different, and indeed rival theory to Pavlov's. I will not discuss this further here, but refer the reader to the relevant section (3.6. of chapter three). Let me add one point though, for Kuhn does point out, correctly I think, that this class of experimental problems, the predictor, often involves theoretical considerations, that tend to confound the experimental issues involved (see subsection 5.7. below).
5.5. Kuhn thinks the third class of experimental problems (what I call articulators) exhausts the fact-gathering activities of normal science. These are usually articulations for the determination of physical constraints in the more mathematical sciences, he further points out, and "also aim at quantitative laws" (28), "through paradigm articulation" (29) in the less formal sciences. I find this class closely related to the first one, the extender, for both resemble paradigm exploration, or further research into a new, but related area, where exemplar similarities are easy to see. Thus Skinner's idea of extending the reflex exemplar to the whole organism was no gigantic leap of the imagination, but much more an articulation via exploration of the basic exemplar. Thus Skinner could repeatedly exclaim he had made contact with Pavlov at last, as he had come to a process similar to his, and as we saw, that meant he could measure the process quantitatively, just like Pavlov could count the drops of saliva.

5.6. The theoretical problems of normal science, says Kuhn, "fall into very nearly the same classes as the experimental and observational" (1970, 30). Thus we have three classes of theoretical problems -- theoretical extender, predictor, and articulator problems. One thing that is immediately striking here is how short this section on theoretical problems is. Kuhn just provides one example, the further theoretical explorations (by European mathematicians mostly) in the eighteenth and early nineteenth century of
Newton's gravitational theory. These were interested in applying the paradigmatic theory (i.e. the Principia in the exemplar sense) to new, but related areas, like "hydrodynamics and for the problem of vibrating strings" (ibid., 32). This is the theoretical aspect of the extender problem. Why theoretical? Presumably because this involved further mathematical exploration of theory (to new areas) but no new fact-gathering. The same goes for the third class of theoretical problems, the articulators. I have already indicated how similar this problem is to the extender, and in the theoretical realm it continues to be so. The only difference, it seems, is that much of this mathematical work that Kuhn dubs "theoretical problems of paradigm articulation ... aims simply at clarification by reformulation" (33). The end product of such work is not new fact-gathering (in fact no fact-gathering takes place), but is rather to express the same results as were known before "in an equivalent but logically and aesthetically more satisfying form" (ibid).

5.7. The second class of theoretical problems in normal science, the predictors, again proves most interesting. "A part of normal theoretical work, though only a small part", says Kuhn, "consists simply in the use of existing theory to predict factual information of intrinsic value" (1970, 30; underlining added). The only difference here from the experimental aspect of the predictor concerns this intrinsic value. There is not much else to the distinction, as we will see in a moment, but let me point out here, that this too,
will be discussed further in section 3.6 of chapter three, where Skinner's test of Pavlov's "disinhibition" hypothesis is examined.

The reason the theoretical aspect to the predictor does not really differ from the experimental aspect is that Kuhn admits at the outset that "to classify [paradigm articulation] as empirical was arbitrary" (ibid, 33), for it is simultaneously theoretical and empirical. This quote is only about articulators, but as I have already indicated their similarity to the extender, and further since Kuhn has already stated that the experimental predictor problem usually involves a theoretical aspect (see p. 26), it seems that much of the force of the experimental/theoretical distinction evaporates.

But maybe that is as it should be. Maybe all that this distinction amounts to is this: There are only three classes of problems for normal science: extenders, predictors, and articulators. Sometimes they only (or predominantly -- 'no intrinsic value') have an empirical aspect (e.g. "... the problem of precision. We have already illustrated its empirical aspect", (31; underlining added)), meaning that the problem is observational and fact-gathering. But as Kuhn elsewhere argues for the theoryladenness of observation, it really comes as no surprise that most of these problems have a theoretical component to them most of the time. So the way to understand Kuhn's problems of normal science, I suggest, is to view the observational/theoretical distinction
as a continuum, such that if a particular problem critically involves fact-gathering it may be called experimental, but the more formal it is -- the more actual theory is involved in the working of the problem -- the more theoretical it is.

5.8. These three classes of problems -- "determination of significant fact [extenders], matching facts with theory [predictors], and articulations of theory [articulators], exhaust", Kuhn claims, "the literature of normal science, both empirical and theoretical" (1970, 34; underlining added). They do not, he is quick to add, exhaust the entire literature of science, for there are also, what he calls for now, "extraordinary problems" (ibid).

These he later calls crisis-causing anomalies, or contradictions of fact and theory, and to pave the way for the transition from normal science to revolutionary science he dubs all problems of normal science puzzles. The modus operandi of scientific change, for Kuhn, is the change in a problem from a mere puzzle to a crisis-causing anomaly, or the "puzzle-promotion" phenomenon. A sufficient number of extraordinary problems or anomalies, he further maintains, causes a crisis in normal science that will eventually turn revolutionary. The crisis is therefore a prerequisite to scientific revolutions according to Kuhn.

5.9. We have seen how Skinner's experimental work, especially in the years 1932-34, is clearly based on the empirical problems of normal science, that I have called extenders and articulators. I have further hinted (and
referred the reader to a future section) that Skinner can also be seen, on occasion, to directly test the paradigm to fact, and thus engage in Kuhn's third and final class of problems, the predictor.

But before I go on to Laudan's conception of empirical problems, and also to other parts of his model, let me first explain what is meant by these two "major problems" so often mentioned earlier in this chapter. And what do I mean by such locutions as "a worry" and "a problem for Skinner" (especially in sections 3 and 4 earlier in this chapter)?

5.10. The expressions so frequently used up until now (e.g. worry, problem for Skinner, discovery, etc.) to describe Skinner's very early experimental work, are just that; descriptive terms that are supposed to capture the trials and tribulations of his very early experimental work. This is done for a specific reason, for though I realize that it is not possible to write a case study in the history and philosophy of science without introducing (however implicitly) meta-level considerations, I still think it important to start a case study like this as unbiased as possible. I am not saying that it is bad method to start with models of scientific change and then do the history; my point is rather that it is the history that lights up (so to speak) the models, but not the other way around. Put differently, this case study is first and foremost about Skinner's early experimental work, against which a few models of scientific change are compared, once some of that history is known. The point is that at
least some (relatively unbiased) history ("pure" internal history comes first, as I will argue in chapter three) has to be made, before a decision can even be made as to which models of scientific change can even be considered.

6. PROBLEMS, EMPIRICAL AND CONCEPTUAL

6.1. Before I go into the model of scientific change presented in Laudan's *Progress and Its Problems* (op.cit.) in any detail, let me first make a few preliminary points, as these should help the reader to appreciate the purpose of this section.

First I should say that although Laudan's (sketch of) a model of progress (i.e. non-cumulative progress, that still makes science a rational process) is undoubtedly the most important aspect of the book, philosophically speaking, it definitely is not for my purposes. My (immediate) interest is much rather in the conceptual apparatus, i.e. the model of scientific change, through such conceptions as problems, anomalies, conceptual vs. empirical problems, solved vs. unsolved problems, theories vs. research traditions, (research traditions vs. world-views), and the idea of rival theories (within the same research tradition) that turn unsolved problems of other theories into anomalies.

Secondly, I want to emphasize that Laudan's book is much more of a synthesis (as he admits) than a definitely new model of scientific change. As I will emphasize, there is much common ground between Laudan and Kuhn (and even Lakatos and Popper if you are willing to stretch the idea
a bit), and though Laudan understandably is very specific about the differences, they are more often than not mere differences in emphasis. For example, all of the authors above share the emphasis on scientific problems as the modus operandi of scientific change, all regard the anomaly as the crucial aspect of that change, and most (not Popper) emphasize the crucial influence of global theories (i.e. paradigms, research programmes and research traditions). The main difference seems to be Laudan's emphasis on rival theories, conceptual problems, and non-refuting anomalies, much of which the other authors neglect, says Laudan, because of their empiricist (he must mean positivist) leanings.

Thirdly, I want to stress that scientific progress for Laudan is mainly replacement of one theory by a new one that has greater problem-solving effectiveness. Though not unreasonable, I will suggest (see especially section 8 of chapter three), that this picture of scientific progress seems to neglect a crucial characteristic of (twentieth century) science; namely the proliferation of, not only theories, but more interestingly whole new areas of investigation -- what some have dubbed new fields of ignorance.

6.2. For Laudan there are two types of scientific problems, empirical and conceptual. Empirical problems are lower level questions, he says, and to answer a problem is to invent a theory. To seek an explanation is an empirical problem, to give it is to theorize:
If problems are the focal point of scientific thought, theories are its end result. Theories matter, they are COGNITIVELY important, insofar as -- and only insofar as -- they provide adequate solutions to problems. If problems constitute the questions of science, it is theories which constitute the answers ... theories ... [are] solutions to problems. (Laudan, 1977, 13; emphasis in original)

Any question about a factual regularity in nature can pose a scientific problem, Laudan claims, but he is quick to point out that a problem is not given by an empirical fact. A problem of course, can be counter-factual:

A problem need not accurately describe a real state of affairs to be a problem: all that is required is that it be THOUGHT TO BE an actual state of affairs by some agent. (ibid, 16; emphasis in original)

Interestingly enough, this comes close to Skinner's worries, as I have described them in sections 3 and 4 of this chapter. As we will see later, these worries (or problems as we now use the term) do not disappear, but only change and come back transformed (i.e. as new formulations of the same general problem). We will see this more clearly in the first few sections of the next chapter, but the present point is that Skinner, at this time (1932-3), thinks these are mere worries to be explained away, once he knows how. What we have seen some evidence of already, and will repeatedly encounter for some time still, is that Skinner "solves" his problems by a careless sleight of hand, not realizing how unsatisfactory his answers really are.

The reason for this, I take it, is that Skinner did not see anyone in the reflex tradition to be working on these problems, and when he goes back to reflex physiology proper
(e.g. 'organophysiology') Skinner finds his suspicion confirmed, as they do not even understand him. As we will see in section 3.5. of chapter three one more piece of development is needed before Skinner can actually piece this puzzle together. For when he finds that he does not get any disinhibition effect in conditioning or extinction, as Pavlov's theory predicts he should, he is finally forced to the conclusion that he is on to a new theory that is different from -- indeed a rival to -- Pavlov's theory.

6.3. But I am getting ahead of myself again. The immediate concern here is Laudan's conception of empirical problems, and how they might relate to Kuhn's experimental problems. To approach this question let us simply ask what Laudan means by empirical, when scientific problems, on his account, are not given by facts; empirical problems may not have any physical reference, he says, and still can be problems. Significantly, Laudan explains:

That assumption, of course, is a theory-laden one, but we nonetheless assert it to be about the physical world. Empirical problems are thus FIRST ORDER PROBLEMS; they are substantive questions about the objects which constitute the domain of any given science. Unlike other, higher order problems [conceptual problems] ... we judge the adequacy of solutions to empirical problems by studying the objects in the domain. (Laudan, 1977, 15; emphasis in original; underlining added)

Apart from the introduction of higher order problems, conceptual problems, to which we will have to return, the interest here is in Laudan's view of the observational/theoretical distinction. Unlike Kuhn, he downplays the distinction, insisting on the theory-ladenness of observation.
6.4. Thus Laudan is explicit about what is only implicit in Kuhn, namely the theory-ladenness of observation. As I argued in the last section, this point takes most of the sting out of the experimental/theoretical distinction in Kuhn, so Laudan's more general concept of empirical problems comes to much the same as both, Kuhn's experimental and theoretical problems. I will later return to Laudan's insistence that answering an empirical problem constitutes a (new) theory (which by the way, I can not accept, when I will later argue that some generalization of the problem/solution is needed before we would call anything a theory), and his three types of problems (unsolved, solved, and anomalous), but for now let us concentrate on his second class of scientific problems -- conceptual problems. The way I suggest we approach this issue, is by returning to the case study again. As I have already indicated, there are two problems that continually worry Skinner throughout his early experimental work, so the obvious question is what type of problems these are. Are they experimental problems in Kuhn's sense? Or would Kuhn label them as theoretical? If only one of these problems is empirical in Laudan's sense, then is the other conceptual? What indeed, is a conceptual problem? And what could it mean for Skinner to encounter a conceptual problem during his early experimental work?

6.5. The first major thing Skinner was worried about in his early experimental work, had to do with the measurement
to intact organisms. But as I have endeavored to show in the last few sections, he has increasing difficulties in using the whole system of the reflex action theory to account for his data. Not that Skinner doubts (at this point) whether the whole system works, for his difficulties can be much better expressed by saying that he has these peculiar worries or problems, that have the property of changing and growing, each time he makes the slightest change in apparatus, measurement, or definition. The change in apparatus, from a food tray to a lever, in measurement, from stimulus intensity to reflex strength during continuous elicitation, and still later, to resistance to extinction, and finally, in definition, from the reflex arc to the reflex as the observed and necessary correlation of stimulus and response, all have the combined effect of making it increasingly difficult for Skinner to explain his results in the terminology of the reflex tradition.

7.2. But why does Skinner want to go back to standard reflex action theory, if indeed he is moving away from that tradition? The answer is clear for in Skinner's application for reappointment as a research fellow in physiology, he says that he wants to "study reflexes in partially dissected animals in order to make a clearer analysis of his observations of the behavior of intact organisms" (SB, 107; underlining added). This is an absolutely crucial point in my analysis, for in the end it is the utter failure of this move back to reflex physiology that convinces him that the problems he is encountering are his problems, but not the
PROBLEM:

Spontaneous activity "presents a special problem" (Skinner, 1933b, 20). Where is the observed stimulus in spontaneous activity?

ANSWER:

"The outstanding character of spontaneous activity is the suddenness of the transition from rest to action" (ibid). Only an external stimulus can account for this variability.

PROBLEM:

But then we are not working with an individual reflex but rather "a chain of reflexes, which are closely interwoven". (Skinner, 1932a, 31)

PROBLEM:

If reflex strength is the underlying change of these various measures, then what is the correct measure of reflex strength?

ANSWER:

The rate of response during continual stimulation can be used as a measure of reflex strength.

PROBLEM:

But during continual stimulation is only the first reflex elicited or are all of them?

ANSWER:

Only the first reflex has to be elicited, which then elicits the next one, and so on.

SKINNER'S PROBLEMS WITH THE RESEARCH PROGRAM 1932-33:

"There is no one initial reflex of this sort. We cannot control the behavior of the rat adequately enough to insure ... the elicitation of an invariable response. The number of possible initial reflexes, even in a simple act ... is therefore infinite". (Skinner, 1932b, 46)

6.6: These are clearly empirical problems as they concern, on virtually every issue, the problem of how to find, experimentally, the individual reflex and how to measure, again experimentally, its strength. We see furthermore that there is a direct feedback relation between these problems of definition and measurement. The redefinition of the reflex, for instance, forced Skinner to conclude that none of the usual measures of its strength would do in his case, for if the reflex was the property of the whole organism, then the measure of its strength had to respect that fact. But
what then, could the correct measure be? Here the conclusion from the work on spontaneous activity provided the clue. Rate of response could be used as a measure of reflex strength, Skinner thought as it was relevant to variability in behavior. Both the measurement problem and the definition problem are for now (1932-33) empirical, as they are clearly of the fact-gathering kind. The redefinition of the reflex leads directly to the new understanding of reflex strength as the underlying change in the whole organism. But then, in the experiments on running wheels, the thing that strikes him is the sudden change from low response rate to high. That sudden change furthermore, can be measured, and the most obvious way to do that is by measuring response rate during continual stimulation. But continual stimulation raises the question of the role of the initial stimulus, and again that problem is empirical. Skinner simply adds a new feature to the experiment (the lever) and thus answers the empirical question by experiment. He finds out that the initial reflex has no special role, and that he is consequently, working with a chain of simpler reflexes.

6.7. The above diagram summarizes the development of Skinner's early experimental work up to the time he discovers one-trial conditioning. The result of these developments, as we have seen, is a slight change in the research program, although it will take Skinner some time to make those changes. After the discovery of one-trial conditioning however, the shape of Skinner's experimental work begins to take on a
very different picture, and we will begin to see, much more clearly, a split between the two types of problems. In fact, it soon becomes evident that the problem of the definition of the reflex and of its measurement, drift apart and develop independently of one another. This is not to say that the problems are no longer related, but much rather that Skinner did not see any relation between them.

**PROBLEM:**

What is the relation between conditioning and extinction on the one hand, and discrimination on the other?

**ANSWER:**

Discrimination is not a separate process (like conditioning and extinction are), but is rather the combined process of both. It is the concurrent conditioning of one reflex ('in the light') and extinction of another ('in the dark').

**PROBLEM:**

How can light be a stimulus in this experiment? The rat still responds to the lever as the stimulus, but now only in the presence of the light.

**ANSWER:**

The reflex is a response to (the stimulus) the lever in the presence of the light. The light is a discriminative stimulus.

**PROBLEM:**

How are we to measure the new type of conditioning? The response rate measure will not work for conditioning that takes place in just one trial, for it requires continual stimulation.

**ANSWER:**

Extinction appears as the opposite of conditioning. Use the resistance to extinction as a measure of the amount of conditioning (i.e. as a measure of reflex strength).

**PROBLEM:**

Which measure is the correct measure of conditioning? Response rate during continual stimulation or the resistance to extinction measure?

**ANSWER:**

The latter gives more information. By reinforcing only one press of the lever and then obtaining an extinction curve, we get a measure of the exact amount of conditioning.
What is of special interest in diagram 3 is not the initial empirical problems he starts out with, but much rather how they split up and develop independent of one another, and especially how the results they gather lead Skinner to two distinct problems. These are the mysteries of discriminative stimuli and reinforcing stimuli. Clearly these are related (for both question the elicitation assumption) but not for Skinner. He sees no relation between the two. On the one hand, he determines (through his new measure of the amount of conditioning), that there are these reinforcing stimuli. On the other hand, he determines (through the experimental work on simultaneous conditioning and extinction), that discrimination is not a separate process, but rather reducible to the two mentioned. But for us the obvious question is how discriminative and reinforcing stimuli relate to each other, and how both relate to elicitative stimuli.

6.8: The measurement problem is clearly an empirical problem as it concerns the problem of how to measure, experimentally, the amount of one-trial conditioning. This is a combination of an articulator problem and extender problem in Kuhn's sense, as Skinner was, in all of these numerous experiments on extinction, concerned with developing a measure with more precision (see section 5.3. above). That precisely, is the reason Skinner gives when he did eventually choose between the two measures, for the resistance to extinction measure, he said, is much more delicate and informative.

This is also an articulator problem in Kuhn's sense, as
the extinction experiments was clearly fact-gathering work, meant to articulate further the paradigm theory (i.e. Pavlov's theory of extinction is the exemplar) in a larger variety of situations (see section 5.3. again). Skinner was, in short, applying the extinction measure to one-trial conditioning.

6.9. But what about the other problem -- the problem of definition (or elicitation as I also call it)? Is that problem too, of the fact-gathering kind; e.g. an extender, predictor, or maybe an articulator problem? Is it an empirical problem in any of these senses, and if not, then what kind of worry is it? Why does it concern Skinner so much, and why does he keep coming back to the question of the role of the stimulus in the process of conditioning?

A conceptual problem can arise for a theory, says Laudan, in one of two ways:

1. When T exhibits certain internal inconsistencies, or when its basic categories of analysis are vague and unclear; these are INTERNAL CONCEPTUAL PROBLEMS.

2. When T is in conflict with another theory or doctrine, T, which proponents of T believe to be rationally well founded, these are EXTERNAL CONCEPTUAL PROBLEMS. (Laudan, 1977, 49; emphasis in original; underlining added)

We saw in diagrams 2 and 3 that the problems are empirical, but when we compare the content of the top to the bottom of both diagrams a slightly different picture begins to emerge. In the first case the clash from top to bottom concerns the research program. Originally it stipulated that behavior be divided into reflexes, but as Skinner is ready to admit in the quote from the bottom of the diagram, any sample act is
composed of a chain of individual reflexes and as initial reflexes can be infinite, it follows that we have what Laudan calls an internal conceptual problem. (Notice however, that even though Skinner has already admitted all this, it still will take him some time to change the research program in the appropriate ways. The reasons for this delay are discussed in the next two sections.)

As regards diagram 3 the conceptual character of the problems is even more obvious. The problems, again, start out as empirical, and end as empirical, but here the conceptual character is evident once we ask what Skinner can mean by a reinforcing and a discriminative stimulus. The problem is furthermore not restricted to how (or if) these are related, but much more crucially, how these are related to the elicitation assumption.

Before we go into that interesting problem however, let me analyse in more detail Laudan's conceptual problems, for we will find here the main advantage of his model.

6.10. When Laudan discusses the nature of conceptual problems, he starts -- as we have -- by defining them by exclusion (i.e. conceptual = nonempirical problems). He immediately adds that:

we must stress a conceptual problem is a problem EXHIBITED BY SOME THEORY OR OTHER. Conceptual problems are characteristics of theories and have no existence independent of the theories which exhibit them, not even ... limited autonomy ... empirical problems are first order questions about the substantive entities in some domain, conceptual problems are higher order questions about the well-foundedness of the conceptual structures (e.g. theories) which have been devised to answer the first order questions. (Laudan, 1977, 48; emphasis in original)
Later he says that external conceptual problems:

are generated by a theory, T, when T is in conflict with another theory or doctrine which the proponents of T believe to be rationally well founded. It is the existence of this "tension" which constitutes a conceptual problem. (ibid, 50-51; underlining added)

The emphasis here is on the tension between two theories within the same research tradition. A conceptual problem, says Laudan, owes its existence to a rival theory, a rival research tradition, or a conflict with the prevailing world view. So if the elicitation assumption is to become an external conceptual problem, there have to be two theories such that there is conflict or tension between the two (e.g. one adheres to the elicitative assumption while the other rejects it). We will later cash this out in considerable detail, to see if Skinner's solution to the elicitation problem (plus his generalization of that solution to other domains) constitutes not only a new theory -- operant theory -- but also a rival to Pavlov's theory. Once Skinner makes his solution public, Konorski and Miller will not accept it because it violates a fundamental assumption of the tradition, and because of the consequences for Pavlov's theory, and this is the reason why Konorski and Miller react so strongly to Skinner's answer to them.

But before we can go into all this we have to examine a number of crucial developments that together make Skinner realize that he is on to this new, different, and indeed rival, theory to Pavlov's.

6.11. This emphasis on two (or more) theories within the same tradition in conflict with one another is an
absolutely crucial aspect of Laudan's model, and is furthermore, an aspect that Kuhn just will not accept. But before we can investigate these conceptual problems further, there remains the task of explaining the development of the beginnings of operant theory, or the reasons why Skinner finds it necessary to develop an alternative to Pavlov's theory. This development is complex and has a number of important factors, but the crucial question is whether Skinner develops a new theory on the basis of a prior crisis, which in turn depends on a sufficient number of anomalies, or whether the development of operant theory in embryo happens within normal science, and for reasons independent of a growing and profound sense of anomaly.

7. BACK TO REFLEX PHYSIOLOGY

7.1. Skinner's experimental work had gone quite well the first few years after his degree. He was still a Research Fellow in General Physiology and could devote his time exclusively to his research. Now, when he had to apply for a reappointment for the academic year 1932-3, he said he wanted to work part time in the department of physiology at the Medical School. He would go back to the study of partially dissected animals, in order to study their reflex characteristics. This step may seem quite surprising at first, as all of Skinner's work had been on intact, freely moving organisms. It was understandable though, when we consider the problems Skinner was encountering. He had been working in the reflex tradition for some years now, trying to extend that paradigm.
to intact organisms. But as I have endeavored to show in the last few sections, he has increasing difficulties in using the whole system of the reflex action theory to account for his data. Not that Skinner doubts (at this point) whether the whole system works, for his difficulties can be much better expressed by saying that he has these peculiar worries or problems, that have the property of changing and growing, each time he makes the slightest change in apparatus, measurement, or definition. The change in apparatus, from a food tray to a lever, in measurement, from stimulus intensity to reflex strength during continuous elicitation, and still later, to resistance to extinction, and finally, in definition, from the reflex arc to the reflex as the observed and necessary correlation of stimulus and response, all have the combined effect of making it increasingly difficult for Skinner to explain his results in the terminology of the reflex tradition.

7.2. But why does Skinner want to go back to standard reflex action theory, if indeed he is moving away from that tradition? The answer is clear for in Skinner's application for reappointment as a research fellow in physiology, he says that he wants to "study reflexes in partially dissected animals 'in order to make a clearer analysis of his observations of the behavior of intact organisms'" (SB, 107; underlining added). This is an absolutely crucial point in my analysis, for in the end it is the utter failure of this move back to reflex physiology that convinces him that the problems he is encountering are his problems, but not the
problems of reflex physiology. This is the reason I said earlier that Skinner does not (at this point) doubt that the whole system works, for the fact that he seeks help from reflex physiology shows that he still believes in that approach. (Incidentally, the fact that this failed also shows that Skinner does not really know in what field he is working, but more on that later.)

Still Skinner does seek help and the fact that he will be working with two distinguished figures in reflex physiology, Alexander Forbes and Hallowell Davis, was probably also a contributing factor, especially as he may have thought that recent developments in the field might throw some new light on the two major problems he was encountering.

7.3. Skinner started working with another Research Fellow, Elizabeth Lambert, on spinal reflexes. Sherrington had initiated work in this area. He had succeeded in establishing a spinal block in a cat (so that its limbs were not moved by the higher brain centers) by cutting the spinal cord near the neck. Skinner and Lambert were to attempt to make that block reversible, by only freezing a section of the cord. "It would be hard to do this in an intact organism" (SB, 108), Skinner predictably points out, and they had to use anesthesia, artificial respiration, and the 'Sherrington guillotine' to cut the forepart of the cat's head away. The project was difficult enough even with such a surgically restricted animal however, and it had to be abandoned as there were too many problems involved. Forbes then suggested
an experiment on chronaxie of subordination, but though Skinner did get a publication out of this: "Some Conditions Affecting Intensity and Duration Thresholds in Motor Nerve with Reference to Chronaxie of Subordination" (E.F. Lambert, B.F. Skinner, A. Forbes, 1933) with Lambert and Forbes, Skinner did not get much out of the Medical School experience. He even says he never fully understood what they were investigating, and that the paper was mainly written by Lambert and Forbes (see SB, 119). I will return to this point in the next section.

Skinner liked the contact with the kind of physiology "that was said to be relevant to [his] field" (SB, 120), but though this was sensible physiology, he said he found:

nothing that would be particularly helpful in the analysis of behavior. On the contrary, a limitation was becoming clear. From spinal reflexes to bodily changes in hunger and emotion physiologists talked about responses to stimuli; magnitude of response was their measure.²¹ (SB, 120; underlining added)

7.4. Later, Skinner sought help in a very different place. -- he read J.M. Keynes' Treatise on Probability (1921). This must have been sometime in 1934. Although Skinner downplays the book's influence, but maybe he was already looking for ways to express the influence of a discriminative stimulus, for if it does not elicit the response, maybe it can affect its probability of occurrence?

I caution the reader once again however, as there is no evidence yet that this is the direction Skinner is going in. There is never any mention of probability in this sense during these years (1932-34), and probability of response
is not even comprehensible to Skinner at this point, as the reflex is still his unit.

7.5. As has been examined in some detail up to this point, Skinner's research had gone quite well the first few years after he received his degree. He had published rather extensively; four papers in 1930-31; again four in 1932, and six papers in 1933. The papers from 1932-33 are of special significance -- as we have seen -- for they report a continuum of research, where Skinner nearly exclusively refers to his own earlier papers.

However, that may now have started to worry him, particularly since there had not been any response to his papers, for as we will soon see, when a response finally came, it had quite an effect on Skinner. Also, the fact Skinner soon had to look for a job, may have contributed to his worries. He could not renew his National Research Council Fellowship another year, so in the spring of 1933 Skinner started contacting some of his colleagues in the hope of an academic position. He first wrote to Hunter, who replied that he could do no more than keep his 'eyes and ears open', giving the depression as his main excuse. Hunter did tell him, though, to write to Robert Yerkes at Yale and ask about:

the possibility of working in his Institute for enough pay to keep you [Skinner] alive and asking him, if nothing is available there, to keep you in mind if any position comes up for which one of his own men is not available. You may tell him that I suggested that you write to him. I should also write to any psychologists about the country whom you know and inquire about possible openings. These are hard times, and such letters will not be misunderstood. (SB, 120-21)
7.6. Hunter’s letter is quite expressive of Skinner’s academic situation at that time. As will be much more evident later (for in three years he will be in the same job-hunting situation), Skinner already seems to have somewhat of a reputation (as a competent experimental scientist). However, that reputation may have worked against him (at the moment) in at least two ways. First, although departments may have been hiring new personnel, they were usually looking for ‘beginners’ as they could be offered less money, especially as experimentally oriented scientists (like Skinner) would require extra space and an expense account to do their research. Secondly, Skinner is clearly a sort of interdisciplinary figure (what ‘interdisciplinary’ means exactly here will be thrashed out later), as it is (not yet) clear whether his work applies more to reflex physiology, general physiology or the physiology of animal learning.

Anyway, Skinner took Hunter’s advice and wrote to Yerkes and others, but invariably got the reply that no jobs were available. Hunter “backed me personally” (SB, 178). Skinner says later, although, interestingly enough, in the letter quoted above Hunter does also mention a curious “little job at the Boston Psychopathic” which would keep you from starvation” (ibid).

The fact of the matter was obvious enough. Although Skinner had already published a good number of papers, they were not in the mainstream of American psychology (or for that matter, of American physiology either). There had been
no response to any of his papers, and he was almost never cited (although Tolman, Hunter, Boring, and others had expressed "a special interest" in his work), which clearly indicates that though other researchers found Skinner's work of interest, they just could not see how it applied or related to their own. Skinner himself was partly to blame here, for as has already been noted, he had not cited others in the field, or tried to relate his work to anyone currently working in the same area. Indeed, Skinner's own department of psychology at Harvard looked at him with suspicion, as they felt he had gone over to physiology and to them he was "a deserter" (SB, 178). Skinner's research was all on the borderline of psychology and physiology, as well as being quite unique conceptually and experimentally. And as we have seen in this chapter Skinner was moving away from the reflex tradition conceptually, with his redefinition of the reflex, and his new measurements of reflex strength.

7.7. Experimentally, it was the same story; as no one else used a cumulative recorder, lever or food dispenser, with the consequence that it must have been harder and harder for Skinner to relate his work to others in the field, the more he developed it, and as I have already noted he did not even try.

For example, at the annual meeting of the American Psychological Association at Columbia University in September of 1934 Skinner gave his second lecture. He already had a problem "that would become", he says, more and more "acute
as the years passed" (SB, 147) in explaining his experimental apparatus and procedure in the limited time allotted to him, before he could even start to talk about his results. Only a few people knew about Skinner's experimental procedure, and even they, it seems, were not altogether clear about what he was doing. I say this because even Skinner's best friend during those years, Fred S. Keller, knew little of Skinner's experimental work until after the publication of The Behavior of Organisms in 1938. He did have some general idea, for sure, because they saw much of each other while graduate students, and later corresponded quite frequently and substantially (as we have already seen some evidence of) once Keller left for Colgate.

Later Keller was to become one of the first "Skinnerians", but all the evidence indicates that it was not until 1938 that he was substantially influenced by Skinner. Even as graduate students, "Skinner took more account of my research than I of his", says Keller in the paper "Psychology at Harvard (1926-1931)--A Reminiscence" (Keller, 1970, 10). This sounds plausible, and in fact explains Skinner's uneventful experiment on rats in double-alternation mazes (see section 5.5. of chapter one). Such experiments could not tell Skinner anything about the individual reflex of the intact organism, but the point is that this was the experimental apparatus used by Keller at that time. His thesis was a follow-up of Hunter's work on the temporal maze (see Keller, ibid).
7.8. After Keller graduated and moved to Colgate University in 1931, he continued doing classical maze studies for the seven years he was there. The crucial year seems to have been 1938, when Keller took a position at Columbia, and when Skinner published _The Behavior of Organisms_. Skinner confirms this in the autobiography, for he says that Keller had not followed his research closely. Keller's thesis, Skinner explains:

was on a traditional theme, and during his first years at Colgate he and his students continued to work on standard problems like delayed reaction and the sensory control of the maze. Although we promoted behaviorism together, I did not discuss with him the theoretical issues arising in my own work. "Two Types of Conditioned Reflex and a Pseudo Type" appeared early in 1935, and it was almost two years later that I wrote to explain the point. (SB, 167)

It was not long after this that Keller wrote to Skinner asking him for "one of those sound-proof boxes ... with all the gadgets" (SB, 167). He says further that although he was impressed by Skinner's first experiments, he could only see their methodological significance:

in the years between 1931 and 1938, I read his other papers as they came along, and even bought a SKINNER BOX (for $45.00), but I saw his contribution as mainly methodological. It was not until the summer of '38, when I began to read my copy of _THE BEHAVIOR OF ORGANISMS_, that I finally saw what was happening. Then, at last, I had something systematically exciting to give my classes, and a new phase of my own career began. (Keller, ibid, 11; emphasis in original, underlinings added).

7.9. Obviously Skinner worked in nearly complete isolation throughout most of his experimental stage (1930-38), for as we have seen, not even his best friend and close colleague later, Fred S. Keller, knew what he was doing,
and (as he admits himself) only saw its significance in 1938. I will return to the implications of this fact in the next section.

8. CRISIS AND INCOMMENSURABILITY

8.1. Early in 1933 Skinner applied for a renewal of the National Research Council Fellowship — though third-year appointments were granted "only in very exceptional cases" (SB, 120), but before that was rejected, he found to his surprise that he was being considered for a fellowship of another kind. Once again it was the physiologists, rather than the psychologists, that came to Skinner's rescue, for he was separately proposed by Hallowell Davis and Crozier, both from the department of general physiology, for a Junior Fellowship to the new Society of Fellows at Harvard. The appointment lends further support to the contention of Skinner's interdisciplinary status, for this Fellowship was not awarded on the basis of work already done in any one field, but much rather on the basis of the promise of a future contribution.

The three original Junior Fellows were asked to sign a "creed" at their appointment to the Society. It said, among other things, that each one had been selected as a member of the Society "for your personal prospect of serious achievement in your chosen field, and your promise of a notable contribution to knowledge and thought" (quoted in SB, 130; underlining added, see also Brinton, 1959, 67-73). Further evidence that the Junior Fellows were appointed
because of promise and prospect is the fact that both Boston
and University newspapers referred to them as a 'super brain
clique' and 'super scholars', the New York Times said they
were 'regular fellows', and the Harvard Crimson scoffed at
them as "Funny Fellows" (SB, 128).

8.2. The President of Harvard and a few professors had
been considering this new fellowship for promising new Ph.D.'s
for some time. When A.N. Whitehead joined the group, a
decision was made to draw up a Society of Fellows, on the
model of the prestigious Trinity Price Fellowships at Cambridge,
England. (Whitehead had himself been a Trinity Price Fellow).
Skinner was one of the first three selected as a Junior
Price Fellow. This was a six year appointment but as Skinner
was already 29 years old -- 28 was to be the limiting age
of appointment -- he was appointed for only three years
(1933-36). Again Skinner could devote his time exclusively
to research and the first paper he wrote as a Junior Fellow
is of considerable interest.

8.3. In that paper "The Extinction of Chained Reflexes"
(Skinner, 1934b), he investigates -- with his new technique
of simultaneous conditioning and extinction -- whether the
lever-pressing-reflex is really the single unit of analysis
he previously had thought it to be. This step is not all
that surprising, for as we saw so well in chapter one, his
whole experimental work is best described as the search for
just such a unit. He finally had found that individual reflex
on an analogy with Pavlov and Sherrington, with the lever-
pressing-reflex. This was the single unit of analysis, or so he thought, of an intact, freely moving organism.

But if all reflexes were really composed of chains of individual reflexes, then the lever-pressing-reflex is "capable of further analysis" (Skinner, 1934b, 234), Skinner now insists. He therefore divided the reflex into "the seizing, chewing and swallowing of food", plus "actual and olfactory stimulation arising from the food" (ibid). The next step could be the approach to the food-tray, a further stimulus could be the sound of food falling on the tray, and so on. He then proceeded to break the chain at various points, by simultaneously conditioning (i.e. reinforcing) one part of the chain, while extingishuing (i.e. not reinforcing) another. Skinner always found the result that:

the interruption of a chain extinguishes all members up to the point of interruption but not beyond. Since the interruption suppresses the elicitation of all members coming after it, the rule may be stated more significantly as follows: IN A CHAIN OF REFLEXES NOT ULTIMATELY REINFORCED ONLY THE MEMBERS ACTUALLY ELICITED UNDERGO EXTINCTION. (ibid, 237; emphasis in original)

8.4. Skinner's unsuccessful search for a job, and the appointment to the Society of Fellows shows quite well the attitude both psychologists and physiologists expressed toward Skinner at that time. Though both groups found his work "interesting" (e.g. the psychologists really wanted to help Skinner get a job, only not in their own departments), and full of "scholarly promise" (e.g. the physiologists recommended Skinner for the psychologist to the Society), no one
was quite sure where to place him academically.

This is for the reason that Skinner's interests were interdisciplinary, in the sense that he wanted to take his training (gained through the exemplar) from one science and use it on another. As I argued in chapter one Skinner's training was in reflex physiology through the exemplar use of Sherrington and Pavlov. He did not replicate their results, but made use of their problem/solutions in a new (but analogous) context. But what was that context? It was the experimental analysis of the reflex properties of the whole and intact rat. For that interest the maze was of no use to Skinner except in the sense of providing him with a convenient starting point. He took the maze apart, literally speaking, so as to get at the individual reflex.

8.5. Skinner's colleagues in animal psychology must have found that curious, for the maze (the T-maze) was the standard tool of analysis, especially for those interested in the experimental analysis of the behavior of the white-rat. Hunter, Tolman, Yerkes, and Hull all used the maze in this way, and when they said they found Skinner's work of interest, I suggest it means "I don't know what you are doing, but it looks interesting". None of those mentioned above could get Skinner a job, (except at a mental institution), while the physiologists supported Skinner to the extent of providing him with generous support from the prestigious Society of Fellows.

I have further argued how Skinner's emerging system
needed contact with other researchers, and that not even his best friend and colleague, Fred S. Keller, knew what he was up to. Skinner did not help matters much, as he did not try to relate his work to others in the field (except by publishing, of course). He did not even bother to explain the important "Two Types of Reflex and a Pseudo-Type" paper to Keller. Skinner's unstable and ambiguous status around this time is further indicated by the fact that he sought help in reflex physiology at the Medical School, especially since that turned out to be so useless. Skinner even admits he 'never fully understood what they were doing', and that the publication he got out of the Medical School experience 'was mostly written by Lambert and Forbes'.

8.6. Kuhn's interesting and controversial thesis of incommensurability may be relevant here, for what the foregoing discussion may suggest is that much of the interdisciplinary character of Skinner's position may be due to his (unfortunate?) combination of reflex physiology and general physiology. There was tension between these schools, as pointed out in chapter one. When Skinner thinks he can unite these seemingly antagonistic approaches to the study of organisms by his joint emphasis on reflex characteristics and on intact organisms, it may be that Skinner is doing violence to the concept of the reflex. Actually this is what Boring suggested when he criticized Skinner's thesis, and the general question may be expressed in the Kuhnian terminology of whether Skinner's reflex and Pavlov's reflex are in fact, incommensurable. If that
is the case, it may help in explaining the lack of understanding between Skinner and his colleagues.

One of Kuhn's examples of incommensurable theories is Einsteinian vs. Newtonian mechanics. Newtonian dynamics can not be derived from the relativistic because Einstein's central terms of space, time, and mass have changed meanings from their Newtonian correlates:

the transition from Newtonian to Einsteinian mechanics illustrates with particular clarity the scientific revolution as a displacement of the conceptual network... a scientific revolution is not only incompatible but often actually incommensurable with what has gone before. (Kuhn, 1970, 102-3; underlining added)

8.7. Through the history of science such basic concepts as mass, light, force, phlogiston, and even oxygen have been used in different senses, Kuhn insists, such that any rational comparison between theories containing these terms may be problematic, if not impossible. Such may be the case with Skinner's "reflex" and Pavlov's "reflex", for the former refers to the properties of the whole organism, (e.g. movements) while the latter refers to individual organs (e.g. internal reactions).

I will leave this question open for now and suggest that if one is to determine whether the two concepts of reflex actually make Pavlov's theory and Skinner's incommensurable, then we have to know more about Skinner's theory. I will return to this issue and then later to the vexed question of the emergence of Skinner's theory in sections 5 and 6 of chapter three.
8.8. Another issue that Skinner's move back to reflex physiology at the Medical School may raise, concerns Kuhn's (previously mentioned) thesis of a crisis as a prerequisite to scientific revolutions. Is Skinner's Medical School experience an example of such a crisis, especially considering the fruitlessness of that move? Then again, are the growing problems discussed in this chapter evidence of such a crisis?

Before we can answer these questions, it is imperative to realize what Kuhn means by such a crisis. If he means a mere expression of worry or something of that sort, as a piece of evidence for such a crisis, then Skinner was in such a crisis. But Kuhn must not mean merely that, though it is by no means clear what he takes an anomaly to be. The crisis before a revolution is dependent on the number of anomalies, but neither does Kuhn give us any way to measure (count?) these anomalies, nor does he say unequivocally that anomalies are counterinstances of theory and fact. Kuhn does state that of all the cases he examined:

except that of Newton the awareness of anomaly has lasted so long and penetrated so deep that one can appropriately describe the fields affected by it as in a state of growing crisis. (Kuhn, 1970, 67)

But scientists do not always "treat anomalies as counterinstances" (ibid, 77), and what is even worse:

every problem that normal science sees as a puzzle can be seen, from another viewpoint, as a counterinstance and thus a source of crisis. (ibid, 79; underlining added)

It is clear however, that if an anomaly is to be crisis-causing it must be a counterinstance of theory to fact, Kuhn says, and only after considerable time and effort
has been put into the attempt to resolve it:

if an anomaly is to evoke crisis, it must usually
[?] be more than just an anomaly ... When ...
an anomaly comes to seem more than just another
puzzle of normal science, the transition to crisis,
and to extraordinary science has begun. (ibid,
82)

8.9. Kuhn's answer to what the crisis-causing anomaly
is cannot be said to be clear. It is not too informative
either as he hedges many of the important points. The
crucial questions therefore remain: How many anomalies
(and how severe) does it take to produce a crisis? How
long can the particular anomaly be tolerated? Are all
anomalies (or even most) counterinstances? What turns
a puzzle of normal science into an anomaly?

One thing is certain, though. An anomaly may not
always be a counterinstance, but an "extraordinary anomaly"
surely is. And the latter, says Kuhn, is the crisis-causing
anomaly. And if that is the case then the question is
whether there is any such anomaly, understood as a counter-
instance of theory to fact, detectible in Skinner's case.
Is one-trial conditioning a counterinstance to Pavlov's
theory? Does the fact that such conditioning can take
place in but one trial run counter to Pavlov's theory?

8.10. Again an issue has been raised, only to leave
the reader with more questions than answers, and again
the excuse is that the issue is readdressed in chapter
three. The point in both cases is the same, for there
is not enough historical evidence gathered in either case
to decide the issue. It has been indicated that Skinner has increasing problems with articulating his research program, but whether there is a crisis yet, is simply not clear.

The reason questions of crisis and incommensurability are raised in this chapter, but not answered, is that the reader should be made aware of the candidates for the explanation of the crucial question of why Skinner eventually denies the elicitation assumption, why he abandons the reflex tradition, and why he turns normal science revolutionary. Did a growing crisis in the field cause the revolution? Was the emergence of operant theory the result of a crisis, or did it cause a crisis?

9. SUMMARY OF CHAPTER TWO

9.1. In this chapter Skinner's first problems with the reflex tradition have been examined, and an attempt made to exhibit the continuation of his research from the time of his graduate work at Harvard during the years 1928-31, up to his post-doctoral research in 1933-34. In the early sections the main emphasis was on how Skinner perfected his "simplified maze" in the never-ending attempt to get at the individual reflex and its proper measure. Finally, after a period when Skinner used a version of Thorndike's problem-box, he came up with the lever-pressing reflex in what is now generally known as the Skinner-box.

I have argued that though relatively minor, the changes Skinner comes to make in apparatus, definition, and measure-
ment, have the cumulative effect of a substantial change away from the reflex tradition. The apparatus, for instance, started out as a simplified maze, a food-tray apparatus, and finally a lever. The major change however, is in terms of definition and measurement, and I argue that Skinner's experimental work in 1932-34 can best be understood in terms of two kinds of problems, the problem of definition and the problem of measurement.

9.2. Both of these problems start out as mere worries, and the main purpose of the chapter is to explain how they grow to such an extent that we can say Skinner is becoming preoccupied with just these two problems. He does experiment after experiment, on the one hand concerned with the role of the stimulus in the process of conditioning (the definition problem of the elicitation assumption), and on the other hand he is concerned with the proper measure of reflex strength (the measurement problem of one-trial conditioning).

During 1932 and early 1933 Skinner shows a curious lack of sensitivity as to the severity of his problems. He seems to think that these can easily be explained away, for in his answers he is quite cavalier, and they (the answers) serve not so much to solve the problems in question as to push them aside. That only makes the problems come back, albeit transformed, and once Skinner discovers one-trial conditioning and extinction, his two problems are further magnified.

'Skinner speculates that one-trial conditioning is
different from Pavlovian conditioning, as it takes place
in but one trial, and the stimuli he says, seem to vary
considerably. He distinguishes between two types of condition-
ing, but moves cautiously, emphasizing all the similarities
of the two types.

Extinction he claims, is the opposite to conditioning,
and he suggests a new measure of conditioning with the
extinction curve. The relevance of this measure is considerable,
as it is the only measure available on both types of conditioning.

9.3. Though I treat scientific problems somewhat
differently from what is customary in the literature, as
I emphasize two kinds of problems as tools for the understanding
of the development of Skinner's experimental work, the
problem-oriented approaches of Kuhn and Laudan serve this
case study quite well. I begin with an analysis of Kuhn's
three types of experimental problems, which are, for convenience
labelled, the extender, the predictor, and the articulator
problems. All of these problems are seen to be of relevance
in Skinner's experimental work, and Kuhn's insight that
the theoretical predictor problem is often of intrinsic
interest is furthermore supported. I do argue however,
that once Kuhn's observational/theoretical distinction
is made explicit it reveals a continuum, that connects
his two sets of problems, experimental to theoretical.
Laudan is more explicit in his theory-ladenness of observation,
and his empirical problems cover much of Kuhn's experimental
and theoretical problems. Kuhn's analysis of the three
types of problems is much better worked out however, while Laudan's emphasis is mostly on conceptual problems.

9.4. Skinner's measurement problem of the new type of conditioning is an example of an experimental problem in Kuhn's sense, as is much of Skinner's early experimental work described in chapter one. All this work is fact-gathering research par excellence. The other problem, that reveals itself in Skinner's increasing emphasis on the role of the stimulus in the process of conditioning is likewise experimental to begin with, but once his many experiments on the process of discrimination are examined, it becomes increasingly clear that the motivation for these is the tension between the elicitation assumption of the reflex tradition and Skinner's two major classes of stimuli, reinforcing and discriminative. This is an example of Laudan's external conceptual problem, for by committing himself to the existence of such stimuli, the problem for Skinner is how they relate to ordinary elicitative stimuli. Put differently the conceptual problem is to explain how reinforcing and discriminative stimuli affect a response, if they do not actually elicit it. This tension is between Skinner's commitment to the tradition which takes all responses to be elicited, and his claim that there are other kinds of stimuli.

9.5. This analysis of the problem-oriented approaches of Kuhn and Laudan reveals, just like in chapter one, the sense in which I see their models as complementary. On the one hand I argue that Kuhn's three types of problems,
the extender, the predictor, and the articulator problem, fits this case study quite well. Laudan's empirical problem does that too, but it is the detail of Kuhn's analysis that is impressive.

On the other hand, I argue that there is some evidence of conceptual problems in Skinner's experimental work, both in the sense of Laudan's external and internal conceptual problems. I point out that Kuhn can argue that such problems should not bother the scientist, but the problem for Kuhn, is that in this case study the conceptual problem of the role of the stimulus in the process of conditioning, is the modus operandi of scientific change. There is simply no way to understand the continuum of Skinner's experimental work from 1928-38, except through his two problems, the problem of definition and the problem of measurement, the former of which is conceptual.

Kuhn might argue that this is just what he means by a crisis-causing anomaly, but the main problem with that answer (apart from the fact that Kuhn is vague in specifying what his anomalies amount to), is that he has no means of expressing that conceptual tension. The tension is not between theory and fact, but much rather between the elicitation assumption of the reflex tradition and Skinner's discriminative stimuli. These discriminative stimuli furthermore, are not factually determined to be non-elicitative, for as we will see in chapter three, Konorski and Miller saw these discriminative stimuli too, but never took them to be non-elicitative.
As we will see in the next chapter Kuhn's difficulties center around his monotheoretic emphasis, for failing to see more than one theory in the paradigm, Kuhn has no way to express a conceptual problem as the tension it is between two theories within the tradition (or between an individual theory and the tradition).

9.6. Due to the fact that Skinner's definition problem becomes conceptual his two problems split up, as can be seen from a comparison of diagrams 2 and 3. In the former there is a direct relation between many of the questions and answers while in the latter no such relation is evident.

9.7. Late in 1933 Skinner goes back to 'organ physiology' i.e. back to the study of surgically restricted organisms, in order, he says, 'to make a clearer analysis of intact organisms'. Clearly Skinner is seeking help in reflex physiology, only to find that neither does he properly understand what they were doing, nor were they interested in his particular problems of definition and measurement.

I discuss further aspects of Skinner's position, emphasizing his uncertain and interdisciplinary character. Evidently Skinner worked mostly in isolation, as neither do his fellow workers in animal psychology understand what he is doing, nor does his best friend and colleague, Fred S. Keller. The general physiologists were in fact the only active supporters of Skinner's, especially Crozier, who still accepted all of his papers for publication.

9.8. We have seen how Skinner's problems were of
two kinds. First was the empirical problem of how to measure the new type of conditioning, and second, the conceptual problem of seemingly non-eliciting stimuli. But as has been argued, Skinner still avoided directly questioning these problems, and he has, as of yet, neither said anything about the relation between eliciting and discriminative stimuli, and how the latter affect a response, nor abandoned the old measure of conditioning by continual stimulation. He has said already that the resistance to extinction measure gives more information, but just as it is with the eliciting/discriminative problem, he avoids directly confronting his two measures of conditioning.

9.9. In 1935 Skinner writes two papers that each is an attempt to answer the two problems much more directly than he has done before. The question why he attempts this now, but not earlier (or later) is not easily answered. May I suggest, thought, five crucial factors.

First, these two problems had slowly been forming in the course of Skinner's experimental work, and as has been indicated in this chapter, they occupy more and more space in his papers. Thus he was doing discrimination experiments solely for the purpose of investigating the nature of 'discriminatory' stimuli, although discrimination experiments have traditionally involved research on perception. Similarly, he did experiment after experiment trying to determine the best way to measure the 'amount of conditioning' (e.g. the resistance to extinction measure). Thirdly, Skinner
did some work in reflex physiology proper, as he thought leading figures in that area might be experiencing the same problems he was. But as he had to admit that these were simply not regarded as problems in the study of reflex properties of partially dissected animals, Skinner came to realize that these were only, and uniquely his problems. Fourthly, although other psychologists interested in animal learning (e.g. Hunter, Tolman, Hull, Yerkes, etc.) were "interested" in Skinner's work, their work was mostly with rats in mazes, which did not throw any light on the study of the single lever-pressing reflex of an intact organism. Finally, the fact that Skinner's job-situation had been so suddenly and unexpectedly solved with the appointment to a very generous society, must have forced him to look harder at his problems, as he had been appointed on the basis of prospect and promise of future scholarly contributions, rather than on the basis of work already done.

Anyway, Skinner did soon address both his problems, by writing two papers, each directly addressing one of the problems and attempting a solution.
REFERENCES

1 This paper is called "The Measurement of Spontaneous Activity" (Skinner, 1933b). There are some papers published before this one, which are discussed later. I treat this one here for the reason that it highlights well the two problems that are starting to worry Skinner, and because of the fact that this paper shows well Crozier's continued influence on Skinner.

2 Here again Skinner shows his superior mechanical ability. The standard running wheel was heavy and would swing back and forth like a pendulum for some time after the animal stopped running. Skinner built a larger wheel, but lighter. He applied a bit of friction, so now the animal would run on a nearly flat surface and could stop without the wheel swinging. An added advantage was that he could now manipulate the speed of running by adjusting the friction.

3 I have no explanation as to why Skinner did these double-alternation experiments (as opposed to the spontaneous activity research, that will later turn out to be crucial for the emergence of his two major problems). He certainly had no use for these maze experiments, given his research program, for even if they had been successful, they would not have told him anything about the single reflex of an intact organism. But maybe the reason for this was that his friend, Fred S. Keller (we will hear more of him later) had already chosen this maze for his thesis.

4 See the discussion in section 1.3. above and in section 8.3. of chapter one, for earlier developments of that same idea.

5 This can be seen from Sherrington's measure of reflex strength, discussed in section 4 of chapter one.

6 See figure 15 in section 6.11. of chapter one.

7 Like Pavlov himself, Skinner moves from the study of unconditioned reflexes to conditioned ones. Pavlov only turned to conditioned reflexes ("psychic reactions") long after he had established himself with classical studies on the digestive system (for which he received the Nobel prize). Skinner has recently acknowledged this similarity in their work in the paper "Pavlov's Influence on Psychology in America" (Skinner, 1981).

8 Startling, because as far as Skinner knew, Pavlov's "all time record holder" was said to have been seven trials before a response became conditioned, and Pavlov's conditioning had already been criticized as too slow to explain most learning in daily life. Skinner seems not to have known of Pavlov's admission that conditioning could indeed take place in but one trial, as he makes much of just this difference in his letter to Keller. This issue was discussed in section 4 of chapter one, and will be discussed again in chapter three.

9 As noted before, Skinner had four identical problem boxes. In the first experiment two of the rats began responding at a high rate immediately, while the third one needed two trials and the fourth rat needed five trials. The results with the first two rats were crucial, of course, and Skinner regarded the slower rate of responding of the other two rats as mere noise -- or a problem to be explained away later.
REFERENCES

10 In the letter to Keller he further claims that
the learning curve is NOT a conditioning curve" (ibid;
emphasis in original), and adds, with obvious enjoyment,
that this "item is good fun because it leaves the insight
boys [Köhler and Yerkes] with their mouths open" (ibid;
90) -- no doubt salivating at the fact, too. Gestalt
psychologists still emphasize the quickness of 'insightful
behavior', but Skinner thinks he can account for that now.

11 The reader should be warned right here that Skinner's
labels for the two types of conditioning can only lead
to confusion. Here Type I refers to Pavlov's procedure,
while Type II refers to his procedure. Later he will revert
the labels, and when he responds to Konorski and Miller,
their Type I equals Skinner's Type II and vice versa.
Skinner finally resolves the confusion still later (in
his response to Konorski and Miller) where Pavlovian
conditioning is called Type S and his type is called
Type R.

12 From H.D. Kimmel, "Notes From 'Pavlov's Wednesdays':

13 In section 3.6. of chapter two, it was pointed
out that Skinner (after having introduced the resistance
to extinction measure), speculates that 'Indeed it could
be argued that extinction is the only appropriate measure
of conditioning' (ibid). But this is merely a speculation,
and that argument is not given (at this point). Skinner
does take this step later of course, but many things have
to happen before he can do that.

14 Kuhn actually avoids this term throughout his written
work, probably because he does not want to say that all
observation is theory-laden, as he has so often been accused
(wrongly, I think) of.

15 I borrow this term from a paper by J.M. Nicholas:
"Puzzles, Anomalies, and Scientific Crisis" (Nicholas,
1982 , 9).

16 The point is especially important in cases like
this one, where virtually no historical work has been carried
out.

17 He correctly points out that he already has data
and results for a single rat "that were more orderly and
reproducible than the averages of large groups in mazes
and discrimination boxes" (SB, 114), adding that "a few
principles seemed to be covering a lot of ground" (ibid).

18 It should be pointed out here that this inner quote
is Skinner's own; presumably from the original application
for reappointment to the National Research Council.

19 Of Forbes Skinner says that he "had worked at Cambridge
University with some of the pioneers in the physiology
of the nervous system, Keith Lucas ... and later Adrian.
He himself was studying reflex activity, and his laboratory
owed much to Sherrington" (SB, 108; underlining mine added).
Later he adds that "Professor Forbes was as close as [Skinner]
could get to the history of nerve and reflex physiology"
(ibid; 120).

Clearly Skinner was not only looking for help, but
was also there to see what were the latest developments
in the field. Maybe, just maybe, they were encountering
the same problems as he was.
REFERENCES

20 Cronaxies, Skinner explains, were supposed properties of the nerve coming off the spinal cord, the changes of which explained some of the properties of spinal reflexes. The theory of nerve conduct by way of chronaxies had just recently been proposed by the French physiologist Lapicque.

21 Skinner adds that they "were not interested in the consequences of behavior or their effect on the probability that an organism would behave in a given way at a given time" (ibid). However, though this is correct, Skinner is clearly here telling the story in terms of his final theory (see section 8 of chapter three). While both consequences of behavior and probability soon become crucial concepts in Skinner's operant theory, there is as of yet (1933-4) no sign of a step in this direction. We have here an example of the dangers of historical research, namely the scientist's own explanation ('rationalization') of a historical fact -- long after the fact.

22 The only exception is the paper Skinner wrote with Forbes and Lambert on the spinal properties of partially dissected animals. Even that paper is of importance, though, as it demonstrated to Skinner that there was no help to be had from reflex physiology. He found, to his surprise, that they were just not interested in the problems he was encountering. I will return to this issue in the next section, and again in section 5 of chapter three.

23 Skinner had already been appointed for two years (1931-32 and 1932-33) and as he points in the autobiography third-year appointments were granted only "in very exceptional cases ... and the Depression was at its worst" (SB, 120).

24 Skinner also wrote to Carmichael at Brown University and Tolman at California (the latter had given a research course at Harvard in the summer of 1931). Skinner sat in on that course and talked about his new thesis "for more than a fair share of the time" (SB, 83), but neither Carmichael nor Tolman had any job opportunities for Skinner.

25 The question of what field we are here talking about is left open until later. The reason of course, is that it is simply not clear anymore in what field Skinner is working. He has his ideas about that for sure, but they have already changed a number of times (this started, as we have seen, when Skinner began to take courses in psychology), and will change again.

26 We remember that this is what they all said, Tolman, Hull, Hunter, Boring, and now Keller. The case of Keller furthermore is of special interest, as he explains what he means by that statement.

27 The only person that might be said to have understood Skinner at this stage was W.J. Crozier. That shows, among other things, how remarkable Skinner's criticism of him (in the Handbook -- see section 4 of chapter three) really is. How could Skinner criticize the only person who showed more than an "interest" in his work?, and how can we possibly explain that, without bringing in external causes?

28 The Society of Fellows, edited by C. Brinton, provides the following list of the six first Junior Fellows (I have not been able to find out which two were, along with Skinner, the original three):
Among these was the biochemist L.J. Henderson, whose course on the history of science Skinner had audited.

Whitehead, Henderson, A.L. Lowell (the president of Harvard), and C. Curtis (a lawyer) were the original Senior Fellows, while there were supposed to be 24 Junior Fellows. However, the first year only six were selected, three for each term. Skinner was among the first three, while Willard Van Orman Quine joined the group later. Skinner and Quine soon became good friends.

Among later scholars that have been Junior Fellows include Skinner's arch-rival Noam Chomsky and of course, Thomas Kuhn. In a sociological study of Kuhn, R.K. Merton points out that there were only three original Junior Fellows as they could not find a sponsor for the fellowships. Indeed, the society was first anonymously funded by Lowell, says Merton (Merton, 1977, 81). See also E. Brinton, 1959.

He had been examined on his 29th birthday (20 March, 1933) and been appointed a Junior Fellow about three weeks later (See SB,123).
PART TWO -- THE EXTRAPOLATION STAGE

The reader will have noticed that almost no extension to human behavior is made or suggested. This does not mean that he is expected to be interested in the behavior of the rat for its own sake. The importance of a science of behavior derives largely from the possibility of an eventual extension to human affairs.

CHAPTER THREE

THE EMERGENCE OF OPERANT THEORY
1. THE TWO PROBLEMS SOLVED

1.1. In chapter two I have tried to trace the growing problems Skinner had with articulating his research program. A research program means, as before, the particular instantiation of a particular research tradition through an exemplar, to the field of whole and intact organisms (in this case). Skinner's position has been examined from 1928 through his doctorate in 1931, and the extensive experimental work from 1931-34 has been reviewed. I have not emphasized the details of Skinner's particular position at any one time, but much rather attempted to understand the changes he came to make; and have concentrated on the following: Changes in definition, from the reflex arc to the reflex as a property of the whole organism understood as a necessary and observed relation. Changes in measurement, from stimulus intensity to response rate during continual stimulation, and later to the resistance to extinction measure. In apparatus, from a variation of the running maze to a food tray, and later to a lever, food dispenser, light as a discriminative stimulus, and a cumulative recorder. I have furthermore emphasized the crucial role of experimental discoveries, especially of extinction as the converse of conditioning, and of one-trial conditioning.

At the end of chapter two I summarized five main reasons why Skinner finally realized that he was on to a new theory, that somehow was not only different from Pavlov's and Sherrington's in some crucial respects, but that there was also some tension discernable with some of the under-

---
lying assumptions of the whole reflex tradition (e.g. the elicitation assumption).

Let us now examine the period from 1934 to 38 -- that very crucial period in the emergence of operant theory -- not only because its end product is a new theory, but also because of the more immediate result, namely Skinner's explicit attempt to solve his problems with whatever tools and means available.

1.2. Before I go into that interesting aspect of Skinner's research, let me first say some more about scientific problems, especially Laudan's distinction between solved, unsolved, and anomalous problems.

Unsolved problems, on Laudan's account, are those problems that have not been explained by any theory within or outside the tradition. The class of solved problems, conversely, are those that have been adequately solved by at least one theory. This sounds innocent enough, but Laudan's original step here is to define anomalous problems in terms of the other two:

(1) UNSOLVED PROBLEMS -- those empirical problems which have not yet been adequately solved by ANY theory;
(2) SOLVED PROBLEMS -- those empirical problems which have been adequately solved by a theory;
(3) ANOMALOUS PROBLEMS -- those empirical problems which a PARTICULAR theory has not solved, but which one or more of its competitors have. (Laudan, 1977, 17; emphasis in original)

Clearly solved problems count in favor of the relevant theory, anomalous problems constitute evidence against a theory, and unsolved problems make up, so to speak,
the potential subject matter of a particular theory, which
might be taken to be relevant for those problems. I say
"might be" for Laudan points out, correctly I think, that:

UNSOLVED PROBLEMS GENERALLY COUNT AS GENUINE
PROBLEMS ONLY WHEN THEY ARE NO LONGER UNSOLVED.
Until solved by some theory in a domain they
are generally only "potential" problems rather
than actual ones. (ibid, 18; emphasis in original)

Using the terminology of solved, unsolved, and
anomalous problems "we can agree", says Laudan, "that ONE OF
THE HALLMARKS OF SCIENTIFIC PROGRESS IS THE TRANSFORMATION
OF ANOMALOUS AND UNSOLVED PROBLEMS INTO SOLVED ONES" (ibid;
emphasis in original).

1.3. Clearly Laudan's unsolved problem is closely
related to Kuhn's puzzle, as his anomalous problem is like
Kuhn's anomaly. This is because the puzzle, as well as the
unsolved problem do not constitute any threat to the relevant
theory, but serve rather to define its problem context, or to
direct research. They are alike further, in that both can
turn into anomalies, by the puzzle-promotion phenomenon.
There are some major differences, however. First and fore-
most is the fact that a problem for Laudan is a neutral
term, of which one can say: unsolved, solved, anomalous,
empirical, conceptual, puzzling, etc., for normative value.
For Kuhn this is all less clear, as he never explains well
enough how a puzzle becomes an anomaly. Are all puzzles
empirical for Kuhn? Does a counterinstance make a puzzle
anomalous? How many anomalies are there needed for a crisis?
Are all anomalies counterinstances? What exactly makes a
puzzle become anomalous?

This last question is the crucial one, I believe, and have referred to it before as the puzzle-promotion phenomenon. There is something very paradoxical about this, for the puzzle is a characteristic of normal science for Kuhn, while anomaly and crisis characterize revolutionary science. It is the transformation from normal science to revolutionary that needs to be explained, and it is not enough to say that a puzzle-turned-anomalous does the job. On the one hand, a discrepancy between theory and fact is what generates a puzzle for normal science, and a discrepancy (of another kind?) makes for the collapse of normal science. Indeed, the discrepancy that makes for an anomaly, for Kuhn, "must usually be more than just an anomaly" (1970, 82). Similarly an anomaly has to be "extraordinary" (ibid) to cause a revolution, and a real anomaly is an "extraordinary problem" (ibid, 34). But this does not explain how a puzzle is promoted to an anomaly, and that in turn into an extraordinary one.

Laudan's answer here is much clearer and more informative. An empirical problem is only anomalous for one theory if it has been solved by at least one rival theory. That is, the problem is anomalous to one theory, only if there exists another one that has solved it. Consequently, if no theory has solved a particular problem, that problem is simply unsolved, and does not count heavily against the theory in question. The unsolved problem can then be explained away, for example, by doubting the validity of the result, or by
insisting the problem belongs to another domain.

1.4. When Skinner gave a psychology colloquium at Harvard early in 1934, explaining his interpretation from the thesis (and further developed in the two "Drive and Reflex Strength" papers, 1932a and 1932b) of the hunger-drive as the operation of deprivation and satiation, Boring wrote him an interesting letter. He was still unhappy about Skinner's redefinition of the reflex (see his criticism of Skinner's thesis in section 8 of chapter one), complaining that if one can strengthen a reflex by increasing a hunger drive (i.e. by depriving the organism of food), we seem left with an infinite number of reflexes. "Not only may you have a sugar-reflex as distinguished from a salt-reflex ...", he says, but "a grandmother might be a different stimulus from an aunt" (quoted in SB, 145). Skinner replied that one should examine the properties of typical reflexes, and those phenomena (drive, learning, etc.) which can be expressed as changes in reflex strength. He added significantly, that:

I am not at all in sympathy with the lack of rigor that permits a grandmother to be a stimulus, although I must confess that I am still trying to define the proper systematic use of such a term, especially where the intact organism is in question. (SB, 146; underlining added)

Notice that Skinner's answer only involves the problem of stimuli, and in March 1934 he gave a colloquium at Brown University on "Studies in the Definition of Stimulus and Response". He expanded that study somewhat and published it as "The Generic Nature of the Concepts of Stimulus and
Response" (Skinner, 1935a). There he examined how to define stimulus and response, and thus finally faced the problem of elicitation. He points out, first, that the problem is not limited to the stimulus, but applies equally to the response. The problem, in a nutshell, is this. If the reflex is the unit of a science of behavior, then it has to be reproducible. But "it is very difficult", Skinner has to admit, "to find a stimulus and response which maintain precisely the same properties upon two successive occasions" (CR, 458). Further, the reflex is not just a correlation of properties, as one cannot produce defining properties of a reflex without also affecting other properties. The problem is that stimulus and response are defined in terms of one another:

A stimulus or a response is an EVENT, that is to say, not a property; we must turn, therefore, to a definition on the principle of classes ... both the stimulus and the response must be taken ... as class terms, each of which embraces an infinitely large number of particular stimuli or responses but is sufficiently well defined by the specification of one or two properties. (CR, 460; emphasis in original)

1.5. This is the essence of Skinner's solution to the definition problem and he contrasts it with a more narrow definition. The idea here is from the "The Extinction of Chained Reflexes" paper (see section 8.3 of chapter two), for as the chain can be broken at any point, each part of the chain, strictly speaking, may be regarded as an individual reflex. Clearly, there would be an infinite number of reflexes on this account, but if the wider definition is used, there is instead an infinite number of stimuli and responses. We may contrast these two views,
Skinner goes on:

by saying that either a reflex is a broad term expressing the correlation of a class of stimuli with a class of responses ... or it applies to any one of a group of particular correlations ... our problem may be stated in the following form: is a reflex a correlation of classes or a class of correlations? (CR, 460-61)

The latter (and narrow) definition may be very useful for the study of the reflex properties of surgically restricted animals, but in accounting for the behavior of intact organisms, says Skinner, the definition must be wide, or generic, at least to some extent. Notice here that Skinner is no longer afraid to emphasize the difference between his system and reflex physiology -- presumably because of the part-time experience at the Medical School -- discussed earlier.

But just how wide was the definition to be? The answer lies somewhere between the two extremes, Skinner now thinks, and proposes a criterion of orderliness, which gives us:

A unit which is in no sense arbitrary ... A REFLEX, THEN, IS A CORRELATION OF A STIMULUS AND A RESPONSE AT A LEVEL OF RESTRICTION MARKED BY THE ORDERLINESS OF CHANGES IN THE CORRELATION. (CR, 471; emphasis in original)

The decision to use the criterion of orderliness or simplicity is consistently made at the expense of exact reproducibility of all reflex properties, but Skinner now thinks enough reproducibility of defining properties (thus claiming that some non-defining properties of reflexes are not important) is retained when we realize that the stimulus and response
"are not to be identified with particular instances appearing upon some given occasion but with classes of such instances" (CR, 473).

1.6. A crucial part of Skinner's argument for the generic nature of stimulus and response, is that he hopes to be able to do away with exact specifications of stimuli and responses by emphasizing certain **defining properties** of the two. That is to say, stimulus and response correlations are not to be identified with particular instances appearing upon some given occasion, but with the class of such instances. "Pressing the lever" is thus a reflex unit just in case it enters into "a correlation of a stimulus and response at a level of restriction marked by the orderliness of changes in the correlation" (CR, 477; emphasis added). The orderliness of the processes we observe justifies the practice of thus defining the reflex.

But what are the defining properties? It is difficult, Skinner now has to admit, to find these properties, for we:

frequently define the stimulus by the **very doubtful property** of its ability to elicit the response in question rather than by any independent property of the stimulus itself ... we often cannot describe the actual stimulating energies, but we assume that, whenever a response is elicited, some member of the class of effective stimuli has acted. (CR, 466; underlining added)

But this assumption is not warranted Skinner is now bold enough to claim, for an:

exception is the case ... of the type of conditioned reflex in which we cannot define the stimulus except by ability-to-elicit or by appeal to the history of the organism. (CR, 466; underlining added)
1.7. Skinner thus uses his discovery of a new type of conditioning (with its corresponding second-order laws) to attack the elicitation assumption. He does not actually deny it, for as he says, we still assume a response to be elicited, but he does deny that the eliciting stimulus is a fruitful defining property. This is the way he solves his first problem. Notice though, that he has not taken the whole step yet, because he has only said that responses are not to be defined in terms of eliciting stimuli. Whether there actually are some non-elicited responses is left open, although it certainly is suggested that there are some. Just how non-elicited responses might occur is left totally unexplained.

Still, quite a change has occurred in this paper, which can best be seen when this is contrasted with Skinner's early prospect of a science. In chapter one that prospect was described by a research program in terms of a functional analysis. The program was to 'divide behavior into reflexes, devise measures of their strength, and then search the fields of conditioning, drive, and emotion, for the variables of which that strength was a function'.

Now, the first part of this program has been abandoned, as we no longer are to divide behavior into reflexes (whether it be flexion, postural, and eating reflexes or sugar and salt-reflexes), but rather to study the typical reflex. This involves an important new idea in discovery by exploration. To find the functional relation of the reflex, Skinner now
thinks, one has, first of all, to find a defining property. That defining property:

appears on the side of the response in the first step toward what is called the discovery of a reflex. Some aspect of behavior is observed to occur repeatedly under general stimulation, and we assign a name to it which specifies (perhaps not explicitly) a defining property ... When a defining property has been decided upon, the stimuli which elicit responses possessing it are discovered by exploration. (CR, 465-6)

The exploration here referred to is to vary the stimuli to determine which property of the stimulus is the defining property of the reflex. It is "the part common to the different stimuli which are thus found to be effective" (CR, 446).

This is a process of exploration because we may well hit upon the wrong property (i.e. a non-defining property), but if that is the case the process simply will not go through (i.e. the stimulus, thus defined, will not be effective), and a new property has to be selected. Once this property of the stimulus has been experimentally determined, we have a functional relation (to be indicated by the orderliness of change or smoothness of curve), as the correlated stimulus-response class. Our reflex, then, is just that functionally related stimulus class and response class.

1.8. In the second paper, "Two Types of Conditioned Reflex and a Pseudo-type" (Skinner, 1935b), Skinner tackles the other problem -- i.e. how to measure the new type of conditioning. He now approaches this problem by attempting to clarify the distinction between the two types, but unfortunately this paper is much less clear than the paper.
on the generic nature of stimulus and response, and for a number of reasons:

First of all, Skinner now refers to Pavlovian conditioning by Type II:

(5) $S'_0 \rightarrow R'_0$

$S'_1 \rightarrow R'_1$

where $S'_0$ could be light, $R'_0$ some response or other, $S'_1$ food or shock, and $R'_1$ could be salivation or emotional change.

Skinner’s new type is now Type I:

(6) $S'_0 \rightarrow R'_0 \rightarrow S'_1 \rightarrow R'_1$

where $S'_0$ could be lever, $R'_0$ pressing, $S'_1$ food or shock, and $R'_1$ could be eating or emotional change. This is, of course, to reverse the names from the previous paper. Type I becomes Type II, and vice versa, which can only lead to confusion.

There is a major difference between this paper and the previous one on the two types of conditioning. Whereas before he listed all the similarities he could find immediately after proclaiming a new type of conditioning (in the paper “On the Rate of Formation of a Conditioned Reflex” (Skinner, 1932c) discussed in section 2 of chapter two), he is now ready to list all the differences he can find. This fact indicates clearly that Skinner is now actually trying to solve the problem, but again he confuses matters, this time for a more complicated reason.
1.9. In arguing for the differences between the two types of conditioning, Skinner points out that Type II (now the Pavlovian type) can be re-written as:

(7) light $\rightarrow$ food $\rightarrow$ salivation

But the result, he says:

is not to reduce the two types to a single form. Both kinds of conditioning proceed simultaneously but separately ...

In the special case in which [the response r is also salivation (i.e. light $\rightarrow$ salivation $\rightarrow$ food $\rightarrow$ salivation) making the two responses] of the same form, the two kinds can apparently not be separated ...

[But this] is a very special case and is also in no sense a reduction to a single type. (CR, 480)

The reason Skinner is now ready to claim that we have two irreducible different -- although simultaneous -- processes at work, is that if the response of, say, 'turning towards the light' is reinforced immediately after turning, then the 'light-turning' reflex will increase according to Type I (the new type), but decrease according to Type II (the Pavlovian type). This is certainly a major difference, and Skinner now explains the result in each case in terms of differences in responses. Thus he says that in the new type of conditioning, reinforcement can only work once a response has occurred, while in the Pavlovian case a new reflex can be created at will. In the new type, Skinner points out, the:

reflex-to-be-conditioned must be elicited at least once as an unconditioned 'investigatory' reflex ... the STATE of the reflex is "conditioned" by the occurrence of the reinforcing sequence, but its EXISTENCE is not. (CR, 481; emphasis in original)

This is clearly an answer in terms of differences in responses, as opposed to differences in stimuli, for Skinner.
continues his explanation by claiming that a major difference between Type I and II is that the former (the new type) the response R_o "Necessarily intervenes between the stimuli" (CR, 480), while in the latter the response "R_o is ignored" (ibid).

1.10. This is the essence of Skinner's explanation of the differences between the two types, and it cannot be said to be very clear. The major problem is that his explanation -- although basically sound -- does not go far enough. What he should say, of course, is that since the response R_o in Type II is ignored, the difference between the two types is not really in terms of responses, but in terms of stimuli. Given the example above, the change that occurs in Pavlovian conditioning is from a 'food-salivation' reflex to a 'light-salivation' reflex, i.e. by the process of conditioning the organism comes to salivate in response to the light, whereas before it only salivated in response to the food. On the face of it this is a process concerning a new kind of response, for the organism comes to salivate (a response) to light, whereas before it did not. But really the actual learning that takes place is a stimulus-stimulus correlation, as the organism responds to a new stimulus (light) in the same way as it did before to another stimulus (food). The response is not new (the organism salivated before the experiment began) but the stimulus (i.e. light) is. Put differently, the point can be expressed by saying that in Pavlovian conditioning no new behavior is learned, but that the animal
learns to associate an unconditioned stimulus with a new one by the process of signalization.

In the new type, on the other hand, the conditioning process involves an increase in the 'light-turning-reflex', as the response of turning comes to be correlated with the reinforcing consequence of food. The correlation in this case is a response-reinforcing stimulus correlation, and again we see that the major change taking place is not in terms of the response (the turning-response occurs before the experiment began -- indeed the experiment cannot begin until one such 'investigatory' response has taken place) as the organism learns to respond in the same way it did before to a new stimulus (food). "Conditioning of Type II", Skinner points out, "is not a device for increasing the repertory of reflexes; R₀ continues to be elicited by the one stimulus with which it began" (CR, 482; underlining added).

1.11. This quote is very important, as it provides a clue as to why Skinner's explanation is in terms of differences in responses, rather than stimuli. For how can Skinner say both, that we have to wait for a response to occur before we can condition it (by Type I), and that the response 'continues to be elicited by the one stimulus with which it began'? This is especially surprising as he has just denied that elicitation is a defining property. He has furthermore indicated (or suggested) that some responses are not elicited, or put differently, that not all stimuli are elicitative. There can only be one answer possible for this
apparent contradiction on Skinner's part; namely that he does not see the relation between his two major problems. He treats them as two separate problems, not seeing the obvious (in hindsight) relation between them. As we have seen, in attempting to clarify the latter problem, Skinner does not use his newly acquired distinction between elicitative and discriminative stimuli. Instead he "creates" differences in responses, not realizing that the differences he creates are a direct consequence of his former distinction between elicitative and discriminative stimuli.

2. THE FUNCTION OF PROBLEMS

2.1. We have seen Skinner actively engaged in attempting to solve both of his major problems; the elicitation assumption and one-trial conditioning. But maybe 'to solve' is too strong a verb here, as in both cases he still has his problems -- indeed arguably even greater problems than before. What he has done essentially, is to attack both problems (in two separate papers) and managed to change them. Superficially, it can be said that Skinner has solved his two problems (e.g. he did deny that the elicitation assumption is a fruitful defining property. He had further insisted that there are these "discriminator", i.e. non-elicitative, stimuli), but really all that has happened is that he has changed them into new problems. These problems do not function as nuances or puzzles that he has to 'straighten out' or 'complete' before he can move on to something else, but function rather as an important part of a continual struggle -- part of dynamic
interaction between problems, discoveries, solutions and new problems. That this is a continual struggle can be seen from the fact that Skinner will continue to study both stimuli in various forms and attempt various measures of one-trial conditioning, for quite some time yet. In fact, as we will see most clearly in the following sections of this chapter, he spends more and more time on these -- so much time in fact, that he can be said to be preoccupied with just these two problems. But notice again, that these problems change, and Skinner can not be said to be preoccupied with exactly the same problems, but rather with two kinds of problems.

Thus Skinner is now ready to deny any importance to the defining property of stimuli to elicit responses; i.e., he 'solves' the problem of the elicitation assumption, but the important point is that he has not indicated in any way whether or how non-elicitative stimuli work. Later he will say that they raise the probability of a specific response class, but there is absolutely no indication as of yet, that this is what he has in mind now. It is thus a completely open question for Skinner, how non-elicitative stimuli work, or in a word, it is a new problem. Exactly the same thing has happened to his second major problem, for it is also questionable to say that Skinner has 'solved' the problem of the two types of conditioning. He certainly has clarified the distinction between the two types in many ways, as he is now willing to amass all the differences (e.g. signalization) he can find between them, and he even
speculates — at the end of the paper just discussed — that they function differently "in the economy of the organism" (CR, 487). He explained this by pointing out that the essential change in Type II (Pavlovian) is preparing the organism by substitution, or signalization, as Pavlov called it, while in Type I there is no such substitution. Moreover, Type I plays the more important role, Skinner further speculated, in that the organism selects from its repertory of unconditioned responses, and in that the "conditioned response in Type I does not prepare for the reinforcing stimulus, it PRODUCES it" (CR, 487; emphasis in original).

2.2. So far so good. But notice that Skinner has not yet treated the problem of the relation between discriminative stimuli and conditioning. It has already been suggested (with the advantage of hindsight, of course) that he should have used discriminative stimuli to clarify the differences between the two types, but still he did not do that. How does Skinner explain discriminative stimuli then? Given that in 'solving' his first problem he has just 'created' discriminative (i.e. non-elicitative) stimuli, and that in 'solving' his second problem he has just 'created' differences in responses, what indeed can he say about discriminative stimuli in conditioning? Phrasing the question this way suggests only one clear answer. Given the route he has already taken, in 'solving' both his problems, there is but one way he can go. Skinner has no option but to create still a new type of conditioning. He calls it, for the lack of a better name, the pseudo-reflex.
2.3. The pseudo-reflex is the relation between light and the response to the lever. Although it may be represented as:

\[ \text{light} \xrightarrow{r} \text{lever} \xrightarrow{\text{pressing}} \]  

(8)

The similarity to Type I and II, Skinner insists, is only partial:

In many ... respects it differs from both types. A reinforcing reflex is not included in the paradigm, but must be added as a third or fourth reflex. The response is not principally to the light, but to the lever; the light is only a component member of the whole stimulus, and "light-pressing" is not legitimately the expression of a reflex. (CR, 483)

Hence the name pseudo-reflex. A pseudo-reflex is set up with a discrimination, in a way we have encountered before (in the discrimination experiments discussed in chapter two).

Pressing the lever is conditioned, after which a discrimination is established by reinforcing only in the light (pressing-plus-light). The effect, as we have seen, is that the organism remains unresponsive in the dark, but presses the lever once the light is on. However, the response is not really to the light, but rather to the lever in the presence of the light. The pseudo-reflex is just this relation between the response to the lever and the discriminative stimulus.

To see that Skinner has here created a new problem or changed his old problem into a new one -- just like in the elicitation-discrimination problem-change -- notice what he has done. He certainly has clarified the distinction between the two types of conditioning, especially when he
pointed out that signalization is only a property of classical conditioning. But as he failed to see the relevance of discriminative stimuli in his distinction, the clarity between the two original types of conditioning is made at a high price. He is forced to create a new type. To complicate matters even further the new type is not really a new type, it is a pseudo-type. Skinner obviously does not literally treat the pseudo-reflex as a new type of conditioning, but then, what exactly is the relation between it and the two types? This, I suggest, is Skinner's new problem, or rather the new form of the problem. It is as if Skinner has taken all the problematic aspects (e.g. "discriminatory" stimuli) out of the two types, bundled them up, and given the bundle a name -- the pseudo-reflex.

2.4. This move on Skinner's part is very important for the development of his position, and for a number of reasons. First, by inventing the pseudo-reflex Skinner is able to -- pardon the expression -- see his two types of conditioning in a new light. Although not a full-fledged third type of conditioning, the pseudo-reflex is still a helpful heuristic, as it is both alike and unlike the two types. What it does, in effect, is to cast doubt upon Skinner's current distinction between the two types -- although it takes him some time to realize that.

Secondly, the pseudo-reflex may seem a weak reflex, compared to the others, but as Skinner explains in his autobiography, it is of major importance. Appearances to
the contrary, the greater part of the behavior of an organism is under the control of discriminative stimuli:

which are effective only because they [are] correlated with reinforcing consequences. The control they [exert is] more subtle than "elicitation" and capable of modulation over a much wider range. (SB, 143)

The final, and crucial reason that the pseudo-reflex is so important for Skinner's subsequent theory is that it is the entering wedge to a new formulation of conditioning, where the characteristics of the pseudo-reflex are not used to create a new type, but rather to clarify directly the two basic types, Pavlovian conditioning, and Skinnerian operant conditioning. This is a most important step in the development of operant theory, and it was initiated by a sudden and unexpected response to Skinner's "Two Types of Conditioned Reflex and a Pseudo-Type" paper, and will be discussed in section 7 below.

2.5. Due to the fact that Skinner's problems, empirical and conceptual, have become quite involved, it is only fair that I try to summarize the results so far. In diagram 2 the development of Skinner's problems was summarized up to the period of his discovery of one-trial conditioning and extinction (1932). Diagram 3 summarizes the development after those discoveries, up to the year 1934. In the following diagram the developments up to 1935 are summarized:

**SKINNER'S RESEARCH PROGRAM FROM 1931:**

Divide behavior into reflexes, devise measures of their strength, and search the fields of drive, conditioning and emotion, for the variables of which that strength is a function.
df
REFLEX = The observed and necessary correlation of stimulus and response

Boring: If you strengthen a reflex by increasing the hunger drive, you are left with an infinite number of reflexes. "Not only you have a sugar-reflex as distinguished from a salt-reflex ...", but "a grandmother might be a different stimulus from an aunt" (letter to Skinner in 1934, SB, 145).

The "problem [ ] is a reflex a correlation of classes or a class of correlations? ... a reflex is ... a broad term expressing the correlation of a class of stimuli with a class of responses". (Skinner, 1935a)

"A stimulus or a response is an EVENT ... not a property ... behavior is observed to occur repeatedly under general stimulation, and we assign to ... it ... a defining property ... when a defining property has been decided upon, the stimuli which elicit responses possessing it are discovered by exploration." (ibid)

REFLEX = "IS A CORRELATION OF A STIMULUS AND A RESPONSE AT A LEVEL OF RESTRICTION MARKED BY THE ORDERLINESS OF CHANGES IN THE CORRELATION". (ibid; emphasis in original)

REFLEX STRENGTH: Response rate during continual stimulation, or in the case of one-trial conditioning, response rate during extinction.

"We cannot control ... behavior ... adequately enough to insure the ... elicitation of an invariable response ... The number of possible initial reflexes, even ... a simple act ... is therefore infinite" (Skinner, 1932b)

"Both kinds of conditioning proceed simultaneously but separately ... But the result, is not to reduce the two types to a single form". (Skinner, 1935b).

"The essence of Type II is the substitution of one stimulus for another, or as Pavlov has put it, signalization ... In Type I there is no substitution of stimuli and consequently no signalization". (ibid)

PSEUDÔ-REFLEX = "The relation between the light [the discriminative stimulus] and the response to the lever might be called a pseudo-conditioned reflex ... The response is not principally to the light, but to the lever". (ibid)

SKINNER'S AMENDED RESEARCH PROGRAM IN 1935:

The collection of reflexes is futile, so study the properties of the typical reflex. Devise a measure of its strength, and search the fields of drive, emotion, conditioning and learning for the variables of which that strength is a function.

CONCEPTUAL PROBLEMS:
REINFORCING STIMULI:
How do reinforcing stimuli elicit a response? Or are they non-elicitative? How do non-elicitative stimuli work? WHAT IS THE RELATION BETWEEN NON-ELICITATIVE STIMULI AND THE ELICITATION ASSUMPTION?

DISCRIMINATIVE STIMULI:
Why does the rat respond more frequently to the lever "in the presence of" light? HOW DOES THIS PSEUDO-REFLEX RELATE TO OTHER REFLEXES? IS THE PSEUDO-CONDITIONED REFLEX A THIRD KIND OF CONDITIONING?

ONE-TRIAL CONDITIONING:
If response rate during extinction is the only available measure for one-trial conditioning, what happens to the other measure? WHAT IS THE RELATION BETWEEN PAVLOVIAN CONDITIONING AND ONE-TRIAL CONDITIONING?

(diagram 4)

As the diagram shows, conceptual problems are second-order problems, or questions one step removed from actual experimental questions of fact-gathering nature. Typically the conceptual problem is a relational one, as it asks for the relation between two separate entities, an aspect of Skinner's current position in most cases, and an assumption of the research tradition (e.g. Skinner's reinforcing and discriminative stimuli vs. the elicitation assumption), or between two aspects within Skinner's position (e.g. can he say both, that there are two kinds of conditioning and a [third?] pseudo-kind?). The former are questions of tension between an individual research program and the tradition, what Laudan calls external conceptual problems, while the latter are questions of consistency and ambiguity, or internal conceptual problems.
2.6. Another important aspect of the above diagram concerns the relation between the problems of definition and measurement on the one hand, and the problems of the two types of conditioning on the other. As I have suggested earlier in this chapter, it was open to Skinner to use the pseudo-reflex (being the relation between the light and the press of the lever) on his one-trial conditioning. Still he did not do that. He does not seem to have seen any relation between these problems, for he comes very close to denying the elicitation assumption in the paper on the generic nature of stimulus and response, only to uphold it strongly in the paper on the pseudo-reflex. He did not actually deny the assumption however, but only insist that elicitation was not a defining property. That is probably why he failed to make discrimination a characteristic of one-trial conditioning, for if he actually denied the assumption in one context, then surely he would have done so in the other. We see however, that neither does he deny the elicitation assumption in one-trial conditioning, for he argues that we have to wait for an "investigatory reflex" before the new type of conditioning can take place. This assumption of an investigatory reflex exhibits well the extent to which Skinner is still committed to the reflex tradition, for otherwise it would have been easy for him to say simply, that a response just occurs spontaneously. Instead he assumes a reflex, i.e. assumes a prior stimulus-eliciting-response relation, to guarantee that the new type of conditioning works on
a reflex, not on an individual response.

2.7. A final aspect of diagram 4 that merits discussion is the consequence of the development of Skinner's problems, empirical and conceptual, for Laudan's conception of a problem. In the section on the nature of scientific problems Laudan says:

If problems are the focal point of scientific thought, theories are its end result. Theories matter ... insofar as -- and only insofar as -- they provide adequate solutions to problems. If problems constitute the questions of science, it is theories which constitute the answers... I speak of theories as solutions to problems. (Laudan, 1977, 13; underlining added)

A problem for Laudan is a fact (or what we take to be a fact) in need of an explanation. And a fact in need of an explanation, most simply, is a question. Questions of fact deserve an answer, and such answers, says Laudan, are theories. A theory solves a problem, Laudan says in a recent paper, "when it entails, along with appropriate initial and boundary conditions, a statement of the problem" (Laudan, 1981, 148).

But is that all we need for a theory, a solution to an empirical problem (along with appropriate initial and boundary conditions)? Surely something more is needed before we speak of individual scientific theories. I would not deny that theories start out as solutions to scientific problems, but I do insist however, that this is not enough. Or are we to say that Skinner has already advanced a number of theories, in his various answers to individual questions of fact? Are all the answers in diagrams 2 to 4 individual theories?
2.8. The problem here might be entitled the problem-problem, as it concerns the question of how to individuate and identify problems. As a quick look at diagrams 2-4 will demonstrate, it is not at all clear how to individuate problems, how many problems there are, or in what sense they are solved. As I have emphasized there are two main kinds of problems that run through Skinner's experimental work from 1928 to 1938. These are the problems of definition and measurement. He has already tackled these problems a number of times, first in the attempt to explain them away, and by 1935 in the attempt to solve them. But even then it is not so clear in what sense he has solved these problems, for though he has clarified a number of things (e.g. the distinction between the two types of conditioning) and amended his research program (e.g. from dividing behavior into reflexes to the study of the typical reflex), I argued that Skinner is faced with new and even greater problems than before. The point is not that any one solution generates new problems, but rather that it is essentially the same problem that keeps bothering Skinner, although in a new form. As we have seen some evidence of already, and will see more clearly in the following sections, the definition problem of the elicitation assumption does take up more and more of Skinner's time. By 1934-5 he can reasonably be said to be preoccupied with that problem. And notice too that in the paper on the generic nature of stimulus and response, Skinner has changed his definition of the reflex. He has solved the problem
of the charge of infinite reflexes by insisting that the reflex is now to be understood as a correlation of stimulus and response classes. Surely that constitutes a solution to a scientific problem, and just as surely, that (by itself) does not constitute a new theory.

2.9. In the following sections I will illustrate what I take it to mean to say that Skinner develops a new theory. For now let me just say that Skinner does not have (the beginnings of) a new theory, in any reasonable sense of that term, until he has both, solved his two problems, and generalized (extended or extrapolated) that solution to other areas. In sections 6 to 8 below I argue that this is just what he did when he denied the elicitation assumption and extended that solution (i.e. non-elicited responses or operants measured by the rate of response) to the field of verbal behavior.

3. EMPIRICAL PROBLEMS AGAIN

3.1. Apart from the two papers just described, where he finally attacks directly the two major problems he was facing, Skinner published one more paper in 1935. It is called "A Discrimination Based Upon a Change in the Properties of a Stimulus" (Skinner, 1935c) and is a direct follow-up of his previous papers on discrimination. Here Skinner is essentially interested in showing that discrimination may be obtained with "a single property" (ibid, 313). The basis of the discrimination may even be "a DURATION or a CHANGE from one property to another" (ibid; emphasis in
original). While this approach shows more sophisticated technique and apparatus than the earlier papers on discrimination had, there is nothing essentially new happening here -- indeed, Skinner even predicts beforehand the results in main outline -- from the data he already has on discrimination (see Skinner, 1935c, 315). The paper was therefore, a direct continuation of experiments already reported, and the main purpose was to clarify further an issue already dealt with to some extent. As we will see in a moment, Skinner's next few papers are also predominantly occupied with the experimental problems we have called articulators.

3.2. In 1936 we see five further experimental papers, that all seem to share the characteristic with the paper above, that they are, more than anything else, a further experimental report and refinement ("articulations") of work that is already well under way. In the first of these, "The Reinforcing Effect of a Differentiating Stimulus" (Skinner, 1936b), Skinner, in effect, says just this:

The present paper continues a series in which the investigation of a selected example of behavior has been reported. The example is a chain of reflexes ... and we are now concerned with adding another member to this chain. (ibid, 263)

Here the continuation with previous experiments on chaining is obvious, the main concern being the rate at which a new member can be added to the chain. The purpose was not to see how far chaining can be carried out (i.e. how long a chain the rat can learn), but simply to see at what rate a new member could be added -- thus continuing the research
on the measurement problem.

In a second paper, "The Effect on the Amount of Conditioning of an Interval of Time Before Reinforcement" (Skinner, 1936c), Skinner is interested in introducing various new intervals of time between the elicitation of the response and its reinforcement. The purpose here is to further analyse the effect the different intervals have "as a modification of the rate of elicitation under periodic reconditioning" (ibid, 279). Again we see that the main point is a further analysis and clarification of a process already studied, and the same can be said for still another paper published in 1936, "Conditioning and Extinction and Their Relation to Drive" (Skinner, 1936d). Here the consideration is the reflex as a lawful relation between stimulus and response. In the case of hunger, "the strength of the reflex is a function of an inferred variable ... 'hunger'" (ibid, 296), where the hunger could simply be operationally defined as the time the organism has gone without food. It is possible to express the strength of a reflex at any given time "as a function of a number of such variables"(ibid), Skinner continued, and this "has already been done in a number of simple cases" (ibid). The point of this experiment is simply the attempt to do this using two variables at once, and Skinner's choice of variables was (not surprisingly) the effect of conditioning and extinction on the hunger-drive. In still another paper on the functional analysis of drive, "Thirst as an Arbitrary
Drive" (Skinner, 1936e) Skinner simply changed his example from hunger to thirst. The experiment was therefore exactly the same as the one on hunger, except for the inevitable change in apparatus (i.e. a water container instead of the more usual food tray). As Skinner points out in that paper, thirst may be a better choice than hunger, for it resembles hunger:

and apparently possesses one advantage of its own. Where hunger is in reality composed of many different specific hungers (salt, sugar, and so on), which make a formulation difficult, thirst ... is presumably uniquely related to the ingestion of water. The experiments reported here test the resulting supposition that thirst might be supposed to be preferable as an arbitrary basic drive. (Skinner, 1936e, 205)

3.3. This is interesting, for two reasons. First, we seem to have a clear reference to Boring, as he criticized Skinner's definition of the reflex, for implying an infinite number of reflexes (e.g. sugar-reflex, salt-reflex, etc.). Secondly, Skinner seems also to be addressing another criticism (although he does not say so explicitly) -- this time from a paper published the year before, in the same journal, called "The Experimental Control of the Hunger-Drive" (Bousfield and Elliott, 1936). This is the first instance I have found of a direct reference to Skinner's work, and the authors raise several:

serious objections ... against [Skinner's] method. Not only does it imply the possibility of measuring motivation on the basis of a single aspect of behavior but the choice [of the hunger-drive] is particularly unfortunate. (ibid, 330)

In response Skinner is attempting to show that his work applies, with minor modifications, just as well to thirst
as a drive, and by implication, to other drives as well. Of interest here is how direct Skinner's response is (this is, after all, the first published criticism of his work), and as we will see later, the reaction is no less direct when two Polish physiologists, Konorski and Miller, publish a paper criticizing Skinner's new idea of the pseudo-reflex.

3.4. Apart from minor differences, all the papers from 1936, so far discussed, have this same characteristic, that they are more than anything else a direct follow-up of work that is already well under way. All these papers are experimental, and all are further refinements, or articulations of empirical problems that have bothered Skinner before. It seems that these are perfect examples of Kuhn's process of normal science (although this is, so far, but a one man's paradigm); where the scientist involved is typically engaged in solving or further articulating a cluster of minor problems, or puzzles.

However, apart from the fact that the case is much more complex than this, there are two major exceptions to this simple picture of Skinner's experimental research in 1936. Both exceptions will later be seen as centering around Skinner's aim of developing a new and independent science of behavior, the first because it involves the physiological aspect of that science, and the other because it is the start of a new development to make the simple model system of behavior into a general theory of behavior. Let us treat the first exception in the next sub-section and
the second one in section 6.1. below.

3.5. In the paper "A Failure to Obtain "Disinhibition" (Skinner, 1936a), also published in 1936, Skinner directly tests Pavlov's and Sherrington's concept of disinhibition. This research seems to originate in Clark Hull's visit to Skinner's lab in 1932, for when he showed Hull his latest results on extinction, Hull suggested that he better study the effect of introducing an extraneous stimulus to the situation during extinction. According to Pavlov's theory this should significantly disturb the extinction process via disinhibition. Sherrington had a similar view, but expressed it differently as he was more interested in the coordination of reflex properties than in the properties of individual and isolated reflexes. Inhibition, in his view, was the opposite of stimulation, and enabled the organism to receive stimulation to one muscle while inhibiting another muscle -- especially when one muscle was responsible for forward movement and the other for a backward movement. Although clearly not the same concept as Pavlov's, disinhibition for Sherrington, as well as for Pavlov, would mean that the second muscle would not be inhibited and thus would hinder or slow down the stimulated counterpart.

3.6. Before I go into this test of whether Pavlov's disinhibition effect would come through on the extinction curve, it is important to realize just what Skinner is doing here. In section 5 of chapter two it is argued that Skinner, in his early experimental work 1932-34, was engaged in both,
extension and articulator problems, in Kuhn's sense of experimental problems. We have seen further such problems earlier in this section, but the question at this point, concerns the third class of empirical problems, the predictor problem. It is a "smaller class of factual determinations", says Kuhn, and "can often be compared directly with predictions from the paradigm theory" (Kuhn, 1970, 26). It seems that this is clearly what Skinner is doing here, but one specific point needs emphasis. Kuhn claims that the predictor problem is "often without much intrinsic interest" (ibid), but I have to disagree with that, and for two reasons. First, the fact that the ultimate outcome of this particular prediction is negative, i.e. Skinner does not find any disinhibition effect, and second, Kuhn does claim that when we enter the theoretical side of the predictor problem, the scientist uses the "existing theory to predict factual information of intrinsic value" (ibid; underlining added).

Now, Skinner is using the Pavlovian theory to predict factual information, and that, plus the fact that the result is negative, makes the problem theoretical, according to Kuhn, and of intrinsic value. As we will see in a moment, this case study supports Kuhn's analysis here, for both are the results of intrinsic and theoretical value. In fact, as I have already hinted at, this result is one of the major factors (along with the futile attempt to get help from the reflex physiology of Adrian and Forbes) in making Skinner realize that he is on to (with his solutions to various
empirical and conceptual problems) a theory that is different from Pavlov's. But before we go into that any further, let me return to the predictor problem, and how Skinner plans to examine it.

3.7. Skinner proceeded in testing Pavlov's prediction by first introducing a relatively weak stimulus, but found that it had no permanent effect on the extinction curve. He then gradually increased the strength of the extraneous stimulus, until finally "the rats were quickly removed from the apparatus and tossed into the air" (Skinner, 1936a, 131). He even tried pricking their tail with a needle, but to no avail, as there was no permanent effect to be seen on the extinction curve (i.e. it always returned to normal).

This result was of major importance to Skinner's formulation of the processes of conditioning and extinction, as well as to his view of the independent nature of his research program. (I still hesitate to say "Skinner's theory" for though it is Laudan's view that providing a solution to an empirical problem is sufficient to the establishment of a theory, I have already argued that we also need the scientist's explicit attempt to extend the solution to other areas, before we can legitimately claim to have a full-blown theory.)

3.8. Returning to the actual experiment, let us ask what would have happened if an extraneous stimulus would in fact have significantly disturbed the extinction curve, as Pavlov's theory predicted. It would have meant, says Skinner,
that the extinction curve could not be:

the mere exhaustion of the effect of conditioning
if the strength of the reflex can be restored by
an event which in itself has no reinforcing value.
(Skinner, 1936a, 129)

But as the extraneous stimulation had no such effect, Skinner
kept on insisting that the strength of the reflex:

particularly of a reflex characteristic of the intact
organism, varies with the drive upon which the reflex
is based, where a drive is defined as a variable, the
value of which is modified through experimental operations
peculiar to drive. (ibid, 127)

Furthermore, in the case of conditioning, and extinction,
as those experimental operations:

we require a statement of the relation of the strength
of a reflex (or of the inferred variable [e.g. hunger]
of which the strength is said to be a function) to
the experimental operations of reinforcement and of
elicitation without reinforcement, respectively.
Experimentally this reduces to a description of how
a reflex originally of low strength is strengthened
by reinforcement and how through lack of reinforcement
its strength returns to its original value. (ibid, 129)

3.9. These series of quotes are of considerable
importance. What Skinner is saying is that extinction is
indeed the opposite of conditioning. If disinhibition had
the effect Clark Hull predicted, then presumably such an
effect would also be found in the conditioning process itself.
That effect would be called 'inhibition'. If Skinner would
have found these effects of inhibition and disinhibition,
then he would have been forced to add still others -- like
'facilitation' -- where inhibition is defined as any decrease
in reflex strength due to an extraneous stimulus, and
facilitation is any increase due to such a stimulus.
But Skinner does not find any disinhibition effect, and by implication, no inhibition or facilitation effect either, and in the quotes above he really turns this test around, by arguing against the introduction of Pavlov's terminology to his system. There is nothing needed over and above conditioning, extinction, reinforcement, stimulus, response, and reflex, he is in effect saying, in accounting for the behavior of the intact organism. This conclusion not only reinforces [sic] Skinner in that he is indeed on his way to the new and independent science of behavior (distinct from reflex physiology proper), but also further strengthens his research program. He has now an elaborate system of studying the reflex behavior of intact organisms, and has supported his initial rather general statement of a program that the processes of conditioning and extinction, as well as the states of hunger and thirst, are to be explained as changes in reflex strength due to reinforcement. Notice further that he is quick to point out, as before, that even though these states and processes intervene between stimuli and responses, they are mere "third variables", and do not have causal status beyond their stimulus-response connections.

Put differently, although hunger (e.g.) as a state is said to intervene between stimulus and response, 'hunger' is itself only operationally defined as time gone without food. Hunger as a state, on Skinner's account, cannot serve any explanatory role in the system.

3.10. Before we see the effect of this negative conclusion,
that Pavlov's prediction does not come through, let me elaborate further on the more general aspect of these issues; namely the question of how independent Skinner's emerging science of behavior really is, and what relation it has to other theories. Once we have done that, we will be in a better position to examine the direct confrontation with Pavlov's theory, when Konorski and Miller criticize Skinner's pseudo-reflex.

4. INTERNAL VS. EXTERNAL REASONS

4.1. Looking back over the last few sections we see that the central issue is how independent this new science of behavior is. As we have seen Boring argued that Skinner's system involved an infinite number of reflexes, while Bousfield and Elliott argued that he could not measure the state of the organism by one drive (i.e. hunger) alone. The former complains that the system is too wide, while the latter claims that it is too narrow. Clark Hull, on the other hand, suggested that the system was merely a version of Pavlov's or that it lacked, but needed, some of his technical terms.

The crucial question here, of course, is relations, or contact between Skinner and other scientists -- between the system he was developing and other theories. Skinner's position is still very vulnerable, as we have seen from it's near collapse into Pavlov's theory, and is more in need of support than criticism. The support his position needs, presumably, is contact -- with the experimental approach
and results of others working in the same or similar area, and more generally, with other theories.

So far we have seen Skinner working mostly in isolation, attempting to build up his system of behavior on the basis of the reflex arc concept. As the system gained in factual support and generality, as he published more, it became increasingly likely that Skinner was making contact with some scientist or other. We have also seen how Skinner reverts back to reflex physiology (i.e. the part-time job at the Medical School), as he was having increasing problems with extending that physiological paradigm to behavior.

4.2. At this point we enter a very important issue that has not been addressed so far. When explaining Skinner's Medical School experience, it was said that he was surprised reflex physiologists were not interested in the two problems he was encountering. Thus I said that he found himself drifting away from physiology, but is that really true? Did Skinner deliberately move away from physiology, and if so, for what reason? Were his reasons purely internal in that he was forced away from reflex physiology proper, by reasons pertaining to his research? Or were his reasons more external, in that he saw something going on in both fields, physiology and psychology, that indicated better prospects of an academic position, funding, publications, or prestige in the latter? There is considerable evidence for both alternatives. The former possibility, that he moved away from physiology from purely conceptual (internal) reasons, has been hinted at
throughout much of the text so far. There is no need to repeat
that here, except to say that I have endeavored to explain
each step in that intricate conceptual development from the
formulation of Skinner's initial sketch of a research program,
as outlined in his thesis from 1931, all the way up to the
years 1935-6.

4.3. This development, as explained so far, is internal
in a strong sense. It has been emphasized at virtually every
step of the gradual build-up of his system of behavior, that
Skinner is motivated by some internal factor or other. The
reader might further conjecture, at this point, that this is
presumably done at the expense of a sociological alternative.
One would think, on the face of it, that once a convincing
internal account is given, a corresponding external story
would simply be irrelevant if not worse. But while we will
come to that in a moment, let it first be emphasized just how
internal the story has been so far.

It was emphasized early in this thesis, just who had an
influence on Skinner, how he slowly came to a sketch of a
research program, first through the exemplar use of Sherrington's
and Pavlov's problem/solutions, and then a compromise between
the methodology of Loeb and Crozier on the one hand, and the
ontology of Pavlov and Sherrington on the other. Even more
compelling was the gradual change in apparatus, definition,
measurement and method in Skinner's unending struggle for a
measurable aspect of the reflex behavior of an intact organism.
An example here is a rather major change he came to make in
his research program. Skinner had originally thought that the identification, collection and experimentation of new and different reflexes was a crucial element of his program. Thus he did distinguish between the 'eating reflex', the postural reflex, Sherrington's flexion reflex and so on. But as he was criticized (by Boring and others) for being stuck with an impossible research program (as it suggested an infinite number of reflexes), he had to change the emphasis from identifying and individuating different reflexes to the study of the typical reflex.

The reasons for this change were purely internal, I argued, as he saw, on the basis of this conceptual criticism, that he would never have been able to realize that original research program.

The moral of that whole story of Skinner's early experimental work is that while Skinner (erroneously) thought it straightforward to substantiate his program, these two major problems slowly emerged, that forced him to reinforce the foundations of his own program by reverting back to physiology proper. But he found, surprisingly -- as I have insisted -- that he had somehow lost contact with that field and was all on his own. The collective cause, then, for this major change in Skinner's theory is therefore internal, as every small step in that chain is.

4.4. The other possibility, that Skinner may have moved away from the reflex tradition for reasons external to his scientific research, has not been examined in any detail yet.
The fact, however, may be that he did deliberately make that move, for example as he saw a development indicating more promise (in terms of prestige, funding, publications, etc.) in psychology than in physiology.

There is some clear evidence that this was the case. We have seen that the only person that had any direct input to Skinner's original research program was W.J. Crozier. He encouraged individual research, and guaranteed Skinner all his first publications. Crozier even tried to make Skinner move to the department of physiology while a graduate student. Though that failed, he was the one who got him the National Research Council Fellowship (and later recommended Skinner to the Society of Fellows), as well as a laboratory and access to experimental apparatus within the physiology department, after Skinner got his degree. Notice too that all this happened just after Skinner graduated from psychology, and that he failed to get financial support from psychology at the same time he got the NRC fellowship.

It has been suggested, that together with the internal story told so far, there is considerable evidence for the inclusion of non-internal factors; i.e. factors not pertaining to Skinner's actual research. While that is probably in accordance with a growing conception -- that for any real understanding of a scientific development both internal and external explanations are needed -- it is simply not enough to assert that. We need to say more, for it is not clear how these approaches can work together. In
particular, it needs explaining which approach is more basic, which one is overriding in cases of conflict, and last but not least, how this comes about in the actual case study at hand.

4.5. The story so far shows that while Skinner got much more support from the physiologists at Harvard than from his own department, a number of interesting developments took place, in both fields, around 1934-36, that might best be described as a power struggle. In the first place Skinner indicates that around 1934 "Crozier's star had fallen" ... and that it was not long until "Crozier's great empire [General Physiology] was reduced to his own office and laboratory" (SB, 171). It seems this had much to do with the fact that in 1933 Conant succeeded Lowell (the latter, along with Whitehead and Henderson had established the Society of Fellows -- see section 8 of chapter two), as the president of Harvard. E.G. Boring explains in his autobiography that "Lowell had not cared much about psychology, but Conant did", and Skinner adds that as soon as Conant became president "General Physiology was in trouble ... as Conant never liked [Crozier]" (SB, 171).

While the demise of Crozier does not singlehandedly explain Skinner's move away from physiology, it is clear that what physiology lost, psychology gained. There are further facts that support the suggestion that at least a part of the reason Skinner moved away from physiology to psychology was a deliberate attempt on his part to further his prospects.
One such is the fact that while all of Skinner's earliest papers were "accepted for publication by W.J. Crozier of the Editorial Board", that changed suddenly in 1935 when they were accepted by Carl Murchison, even though Crozier was still on the editorial board and still accepted papers (but fewer than before). Murchison was the founder of the journal, along with Titchener, and interestingly enough, Skinner's next publication (Skinner, 1935d), comes between those accepted by Crozier and Murchison, is a review of the second edition of the Handbook of General Experimental Psychology, edited by none other than Carl Murchison (1934). In that review Skinner praises the book as a "standard reference ... for anyone interested in the science of psychology" (Skinner, 1935d, 239). He finds time however, to criticize individual authors, and interestingly enough, his main target is none other than Crozier.

4.6. Skinner first criticizes Crozier's article "The Study of Living Organisms" for concentrating solely on tropism, and consequently for not being the introduction to the Handbook it was supposed to be. Crozier's methodology, he says, is "really quite simple" (ibid, 239). His project is to select typical:

"elements of behavior" and treat them as functions of whatever variables may be most convenient or significant in a given case. Professor Crozier prefers a tropism as such a variable. An important part of the plan is the proposal to treat variability by testing it for lawful properties. (ibid, 239-40)

This criticism is strange however, especially when it
is considered that this is Skinner's own methodology in exact detail (as we have seen in section 8 of chapter one). The point that this is not the introduction to the book it should be, is even less to the point as Skinner blame Crozier -- not the other contributors -- for the fact that elsewhere in the book, Crozier's (and Skinner's) "methodological recommendations are subsequently often ignored, and [even] ... flatly contradicted" (ibid, 240). The irony here is that this is just as much Skinner's concern as Crozier's, in fact the only consistent criticism Skinner can level at Crozier is the one about the concept of tropism as a unit of analysis. Here Skinner does correctly point out that the concept is not even mentioned again "on any of the thousand pages which follow ... [n]or could it" (ibid).

The other contributors mostly get a positive comment. Among these are Walter B. Cannon, Alexander Forbes and Hallowell Davis -- all professors at Harvard. Finally Skinner criticizes Crozier again, for his chapter on "Chemoreception". That chapter is "practically unchanged" (ibid, 245) from the original edition, Skinner points out, adding "except for the withdrawal of Professor Parker as co-author" (ibid).

4.7. While Skinner later admits that this one was considerably below the belt, the point is that the only author substantially criticized is Crozier. He regrets this in his autobiography, saying that he owed Crozier "too much to allude in such a way to his changing fortunes" (SB, 171).
There must have been a motive for this however, and I am suggesting that it was the same one that made him praise Murchison. The fact was that Crozier was losing power and prestige, while Murchison was becoming a powerhouse.

The vindication of this idea that Skinner deliberately went for Murchison was that Skinner always liked Crozier, but not Murchison. He later expresses regret at having criticized Crozier, and says, "Murchison was an enterprising publisher but not much of a psychologist" (SB, 164). This expression of regret is further evidence that Skinner's original motive for criticizing Crozier was external, especially as much of the criticism was clearly beside the point. There was open warfare between Hunter and Murchison, and Hunter had always been on good terms with Skinner. Skinner tells stories of Hunter warning him of Murchison, indicating that he was in psychology for the power and prestige of it.

In a letter to his friend Fred S. Keller (March 1935) Skinner says:

The last issue of the [Journal of General Psychology of which Carl Murchison was the chief editor] made me sick ... Murchison grows repulsive. He held up the [latest] issue for three weeks to revise his article ... It's Carl's first article, I believe, and he shows it. Changed the style of the journal to get his name at the top of every page. Previously it was the name of the Journal. Oh-hum! (SB, 164-5)

4.8. Clearly Skinner did not like Murchison personally or professionally but evidently he needed his connections, for at the same time Skinner agreed to join a group, under the heading of Murchison, to define the basic subject matter of
social science. Murchison had been asked by Warren Weaver of the Rockefeller Foundation, "to spend money during the next twenty-five years to advance the social sciences" (SB, 165). Surely Murchison was the right kind of friend to have, if only for that reason. If ever there was an external reason for a scientific change, then surely this one must count.

5. INCOMMENSURABILITY EXPLAINED

5.1. In the last section the question was raised why Skinner moves away from reflex physiology. Did he do so deliberately, or was that change forced upon him? This is like asking whether Skinner moved away from reflex physiology for reasons pertaining solely to his research, or whether there were other, more social, factors involved. Clearly we are here faced with the question of internal vs. external reasons for a scientific change.

It is true that the earlier sections of this thesis strongly suggest an internal answer, as already pointed out, for the analysis has for the most part remained within the historical perspective, concentrating as it does nearly exclusively on scientific ideas, problem areas, and experimental techniques. In that sense the emphasis has been internal, and an effort has been made to climb into the scientist's head, so to speak, to understand his research in terms of the related areas of that time-period, to isolate the forces that caused changes in definitions, measurement, and apparatus, and finally to discover how and when these
changes took place.

This thesis is historical, internal, and will remain so. But an attempt has been made in the later sections to include some factors that cannot be labelled internal.

There has been an increased emphasis on Skinner's job situation, his publications, his relation to other scientists, the reputation he has among them, his extra-scientific relations with Boring, Hunter, Crozier, and most importantly, with Murchison. These considerations are external, i.e. external to the strictly scientific environment Skinner is working in. In the last section we saw clearly one such extra-scientific factor, as Skinner -- for no apparent reason -- criticized heavily his most intimate colleague, W.J. Crozier, in a review of the Handbook, edited by a person Skinner explicitly disliked, personally as well as professionally, Carl Murchison. Even the fact that Skinner writes that review is quite strange, given the fact that he had always worked within a most restricted area of experimental psychology/physiology.

5.2. These extra-scientific developments cannot be understood given a purely and strictly internal history of science. One simply has to introduce external factors at some point in every comprehensive case study, and it is around 1934-36 in Skinner's case. But notice that while it is asserted here that external questions have to be raised at some point of a historical case study, the point is still that this is a historical case study. The point is not that
one approach (external/internal) is better than the other, but rather that both kinds of questions have to be raised at some point in any case study, if one is to arrive at a full understanding of the developments of a certain period in the history of science.

Notice also that inherent in this account is a certain structural relation between the internal and external approach. This thesis starts with nearly exclusively internal considerations, and this is as it should be, I believe, especially in the case of a mostly unexplored piece of history, like this one. Thus it is an essential part of this thesis that at least some case history in internal terms is always needed before some of the more relevant external questions can even be raised.

5.3. So far we have seen Skinner working mostly in isolation, trying to build up and substantiate his early sketch of a research program, expressed as early as 1931 in his thesis. He wanted to unite the methodology of Loeb and Crozier on the one hand, and the ontology of Pavlov and Sherrington on the other. As we saw, Skinner was impressed by Loeb’s emphasis on the organism as a whole, and Pavlov’s and Sherrington’s detailed and accurate experimentation. He thought he could extend the reflex unit of the latter, so that it applied to the methodology of the whole organism of the former. Thus Skinner hoped to be able to accurately research the reflex properties of whole, intact organisms, (the eating reflex in a rat), and began to study its reflex
characteristics. As the major part of chapter two endeavored to show, Skinner ran into difficulties after he had a specific reflex under control, difficulties that slowly crystallized as two major problems, the measurement problem, and the elicitation assumption. In the hope that reflex physiologists might be encountering the same or similar problems as he was, Skinner moved back to physiology for a while, but found, to his surprise, that they were not concerned with these problems. These were not even problems for the reflex physiologist, but only for the special system Skinner was in the process of developing. As a result, Skinner began to ponder what to do next, where he was going, and what people he might (and might not) be establishing contact with. Thus Skinner argued that his system was not merely a version of Pavlov's, while at the same time attempting actually to solve his problems.

5.4. Notice here that it is only because of these problems, and especially the fact that these were his unsolved problems, that Skinner began to consider these broader issues. Similarly, it would not make any sense for us to investigate these broader issues first, before we had at least a rudimentary analysis of the empirical and conceptual problems he was facing. But once we have that internal piece of history, we can -- and indeed must -- introduce external questions, like what reputation Skinner had early on in his career, how that affected his job situation, his prospects of publishing and funding, why he criticized the only person that shared his methodology, and still further such questions
will be raised as we go along.

At last, let me end this section by a concrete rewording of this whole story, joining together these internal and external factors, into what I believe to be, a consistent whole, for I take it that some such cooperation of the internalist and the externalist approaches has often been suggested, but seldom fleshed out in any detail. This should, to carry the metaphor one step further, provide some flesh (and blood) to the historical and social skeleton so far erected.

5.5. I have been suggesting that in one sense (the internal) it is true that Skinner found himself to be drifting away from reflex physiology proper, and that in another (the external) it is true that he deliberately moved away from that tradition, as he saw greater possibilities of funding, research, publication, and prestige in the direction of psychology. How can both of these be true? If they both are, then which one is the correct explanation? And last, but not least, even if Skinner did find himself to be drifting away from the reflex tradition, and even if he did also make a deliberate move in that direction, the question is still: How could he? Have I not shown how Skinner is still committed to the reflex tradition, even though he is encountering some conceptual problems? Was it not the whole point of the exemplar to show how Skinner became committed to the reflex tradition? How can Skinner even contemplate abandoning the reflex as his unit?
My answer, in a nutshell, is this: It is not really true that Skinner was extending the concept of the reflex to psychology, by redefining it to the whole and intact organism. Skinner thought he was doing that, and even by 1938, after the discovery of the operant, he still refers to operants as reflexes, even though a few years later he used the two terms as mutually exclusive. The operant by definition, is non-elicited behavior, or so Skinner says even in The Behavior of Organisms. This just goes to show how the originator of the revolution is not able to take the whole step. He was after all, raised up in the reflex terminology, and cannot give it up so easily. As we will see in section 8 below, Skinner will find it hard just to say that there are non-elicited responses, and as the review of Skinner's later work shows, he continues to hover between the ontological claim that there are responses literally devoid of (elicitative) stimuli, and the mere epistemological claim that such stimuli can not be identified, or that it is unfruitful to look for them.

5.6. My claim that Skinner is not really extending the physiological concept of the reflex to psychology, is not as far fetched as it might sound at first, for Skinner never did actually define the reflex as any physiologist did, nor did he take it to be the physiological concept they understood it to be. Skinner in fact claimed this much, for in his historical analysis of the concept of the reflex, he only claimed to be giving an operational redefinition to
the term. He had argued that the reflex had always meant the necessary correlation of stimulus and response, regardless of what any physiologist actually took the term to mean. Skinner's argument, basically, was that this is what the concept came to once it was stripped of its physiological excess, i.e. once the nuances and particularities of each physiological definition were dispensed with. He had, we remember, reviewed the history of the concept, and given it a Machian analysis, or a new and operational definition.

Pavlov's definition of the reflex was in terms of a necessary reaction to a stimulus, with the addition that the connection was made through a nervous path. Skinner kept the former part of this definition, but not the latter, and in fact, substituted it with an emphasis on observed stimuli and responses, so that his reflex would refer to the movements of the whole organism.

5.7. As touched upon in section 8 of chapter two, this is not enough for incommensurability between Pavlov and Skinner, for Kuhn's term carries with it a stronger claim. Incommensurability makes reference to different meanings and to differences in measurement. The difference between Newtonian "mass" and Einstein's concept of mass is not merely in terms of a change in meaning, but also in terms of the impossibility of co-measurability. Kuhn does not exactly express incommensurability in this way, and though the term is usually taken to mean incompatible or untranslatable, I believe Kuhn originally took the term from mathematics where
it means having no common measure or divisor.

So it is not the mere fact that Skinner has extended the term reflex, from being a property of organs to organisms, but also the later addition of a completely different measure of reflex strength that makes the two terms (i.e. Skinner's "reflex" and Pavlov's "reflex") incommensurable. The former is a mere change in meaning, or an extension of a term, which by itself, need cause no difficulties, once the difference in meaning is kept in mind. But once Skinner starts speculating that reflex strength refers to the underlying change of all the individual measures devised by Sherrington and Pavlov, as he did already in 1932 — see section 1 of chapter two, we have something more than meaning-change. Later he changed the measure to response rate during continual stimulation, and after the discovery of one-trial conditioning, to response rate during extinction. That final measure shows how resistant the animal's behavior is to extinction once reinforcement has been cut off, indicates how different Skinner's "reflex" has become from Pavlov's "reflex" measured by drops of saliva.

5.8. What I am suggesting here is that Skinner may not have been right in claiming that his operational redefinition of the reflex was consistent with whatever the physiologists took the reflex to mean. Now, this suggestion does not sound so implausible anymore, for when he went back to reflex physiology, Skinner found, to his surprise, that even though the physiologists used the same word he did, they just did not mean the same thing. Thus Skinner honestly admitted
that he, "never understood what they were doing" and that
the publication he got out of the Medical School experience,
"was written primarily by Lambert and Forbes" (SB, 119).
Skinner expresses the point even more clearly in the following
statement:

I found nothing that would be particularly helpful
in the analysis of behavior. On the contrary, a
limitation was becoming clear. From spinal reflexes
to bodily changes in hunger and emotion physiologists
talked about responses to stimuli; magnitude of response
was their measure. (ibid, 120; underlining added)

5.9. Another way to understand my point is through
Boring's criticisms. We saw earlier that he did not accept
Skinner's thesis on just this basis, for he claimed that
the first part of his thesis (published separately as "The
Concept of the Reflex in the Description of Behavior" (Skinner,
1931)) was historically inadequate. He argued that Skinner
was not reviewing the history of the term, but rather "distorting
history" by giving a "very broad; strange, almost bizarre
meaning to the word REFLEX" (SB, 73; emphasis in original).
I am suggesting that Boring's criticisms were correct, and
his further comments are equally perceptive:

You are making an argument for keeping the word REFLEX
and giving it a new, broader, and relatively strange
meaning. No one would guess this to be your goal as
you start it, and you yourself may not think of it in
that way. (quoted in SB, 72; emphasis in original,
underlining added)

Boring even suggested that Skinner drop this "historical"
analysis from his thesis and that he needed much rather
'propaganda and a school'. I believe this criticism is
altogether sound and it is rather remarkable how late Skinner
realized that fact. There is some evidence that he did so in *The Behavior of Organisms*, but even there the whole discussion is permeated by this psychology/physiology ambiguity. We must look in some detail into that in section 8 below, but first we must face a new and rather unexpected side to the experimental system Skinner has been developing.

6. THE FIRST EXTENSION: OPERANT THEORY IN EMBRYO

6.1. The first exception to the simple picture of Skinner's experimental work in 1936, was discussed in section 3 above. As we saw in that section most of Skinner's papers from that time were rather uninteresting articulations of experimental work that was already well underway. Kuhn's third class of empirical problems, the articulator, fits here quite well with all but two papers. The first paper was Skinner's test of Pavlov's disinhibition effect (on extinction), and was a clear example of Kuhn's interesting predictor problem. The fact that the result of this prediction was negative, made the result even more important, as Skinner realized that the view he was developing; the system -- not a theory yet, as I will insist in this section -- was very different from Pavlov's theory.

6.2. The second exception to the simple picture so far presented of Skinner's experimental research in 1936 is somewhat more involved than the first one. The sixth paper Skinner published that year was "The Verbal Summator and a Method for the Study of Latent Speech" (Skinner, 1936f) and it is, to all appearances, a new subject altogether.
In fact, however, this was not the first of such papers by Skinner, and most certainly not the last either. In 1934 he had published a paper "Has Gertrude Stein a Secret?" (Skinner, 1934a), that had caused quite a controversy. There Skinner discusses the phenomenon of automatic writing.

After reading a research report by Stein and L.M. Solomons in the Psychological Review (1896), on the subject of spontaneous automatic writing, Skinner argued that one of Stein's subsequent books, Tender Buttons (1914) was in fact a clear example of such writing. Partisans of Gertrude Stein's literary work (many of whom thought Stein was 'the Picasso of literature' -- which means, presumably, that she introduced abstract or impressionistic factors into literature) were of course appalled by such an idea, while others apparently found Skinner's argument quite convincing.

6.3. Skinner's paper on Gertrude Stein, as well as the paper "The Verbal Summator and a Method for the Study of Latent Speech" (to be discussed in a moment) is evidence of a development in Skinner's research that we have not encountered before. It seems that it has no clear relation to Skinner's main research, as this concerns the psychology of language or linguistics even, but as I will argue in some detail later, there is in fact a direct and clear cut relation between the two kinds of research. The former paper (on Gertrude Stein) is clearly an influence from Skinner's 'literary days' (see section 2.2. of chapter one), and is evidence, as Skinner says himself, that he "was drifting
back to literature" (SB, 138). He adds that he "was still suppressing [his] own small part in it" (ibid).

However, while that may be true of the paper on Gertrude Stein, it certainly was not true of the paper on the verbal summator. Here we see a complex and well researched paper by Skinner, on an apparatus he made, called the verbal summator. It is a device, he says there:

for repeating arbitrary samples of speech obtained by permuting and combining certain elementary speech-sounds. One of its uses is comparable with that of ink-blot and free-association tests ... it functions as a sort of verbal ink-blot. (Skinner, 1936f, 71)

What Skinner had created was a phonographic device that was made to emit various sounds, of different kinds, volume, pitch, intonation, and emphasis, in such a way that it resembled the sound of (unclear) spoken English. The actual sound heard could be quite vague and of low pitch, so that when subjects were asked to report what they heard, very different answers were received. In general, the more unclear the sound was, the more varied were the answers. Apparently the subject reported hearing words or sentences that he himself had experienced on another occasion, or rather one that resembled something he has heard before. In this way the device functioned as an ink-blot test, or as a Freudian free-association therapy session.

At first sight it seems Skinner is here entering a new field altogether, and attempting to make a contribution to a more 'psychological' field than before -- psychotherapy, psychoanalysis, psychology of language, or linguistics even.
And as this is a report of quite an extended research, the question must be faced whether this bears any relation to Skinner's experimental work proper. Is this related somehow, to his research into the reflex properties of single, freely moving organisms, or is Skinner here entering a new field altogether?

6.4. There are two very different ways of approaching this question. The first one is to concentrate on the experimental procedure involved, in order to see if and how it might be related to Skinner's other research. Pursuing this approach we might first point out that the verbal summator has **two underlying principles**:

The summator is based upon two familiar principles of non-verbal behavior, which may be extended without trouble to the verbal field. (a) either as an inherent mechanism or as the result of a very common form of early conditioning, we possess imitative reflexes ... (b) The second principle is that of summation. (ibid)

The first principle is from Pavlov, and the second, the principle of summation, is from Sheprington. This principle comes to work, thinks Skinner, when two responses that are of mere subliminal (latent or preverbal) strength, summate. The first principle, the imitative reflex, provides the raw data, so to speak. Simply put, it says that the presentation of a vocal stimulus tends to evoke a response resembling it. This principle is of obvious use in the verbal summator, for when subjects are asked to report what they hear coming from the device, they have nothing to go by except the fact that the sound-pattern may resemble many different sound-patterns they have heard before, and the fact that the same patterns are repeated over and over again.
But which of these sound patterns does the subject then report? It is clear that the imitative reflex cannot do the choosing among different sound patterns -- it only provides a set of possible responses -- i.e. the raw material mentioned previously. This is where the summation principle comes in. The point is that the summative effect works on the different possible responses, in such a way that it raises the strength of a particular response, if and only if it is of the same form as the sound heard before -- much like different waves resemble each other, but only summate when two of the waves have (exactly) the same form.

6.5. Actually the verbal summator does not rely on Sherrington's principle of summation, but rather on an extension of it, for less than identical (i.e. merely similar) responses also summate, presumably in proportion to their resemblances. This is exactly as it was in Skinner's very early experimental work, when he finally exclaimed that he had made contact with Pavlov. He was then, as he is now, using Pavlov and Sherrington as exemplars, in such a way that he sees group-licenced (i.e. licenced by the reflex tradition), resemblances between Pavlov's imitative reflex and Sherrington's principle of summation on the one hand, and their use in a new but related area (verbal behavior), on the other hand. Skinner is applying their problem/solutions to a new but related area.

This is actually the general characteristic of later extensions of operant theory, as we will see later, when
further extrapolations of the concept of the operant (see section & below) will be considered.

6.6. There is one major difference between Skinner's current use of Pavlov and Sherrington as exemplars, and earlier uses. The difference is in terms of complexity. He does not use Pavlov's imitative reflex, or Sherrington's principle of summation individually, but essentially combines the two. As Skinner says in the "Verbal Summator" paper:

we may summarize the action of the verbal summator by saying that it evokes latent verbal responses through summation with imitative responses to skeletal samples of speech. (Skinner, 1936f, 73; underlining added)

By repeating the skeletal sample again and again one "may evoke the response" (ibid, 71), he says, adding that "since it is not evoked by any stimulus acting at the moment, its emergence may be said to be due to its own relative strength" (ibid; underlining added).

This quote is very important for our purposes, as it indicates the second way to approach the question of if and how this research relates to Skinner's experimental work.

The first way to show how these are related was to emphasize the fact that the verbal summator is, as we have seen, based on two underlying principles -- one originating from Pavlov (i.e. the imitative reflex) and the other from Sherrington (the summation effect). Thus the verbal summator is based upon principles deriving from reflex physiology. Skinner even says that though this research is given "in the loosely
defined but familiar terms, ordinarily used in discussing
test the principle of the summator after it had
language... [a] much more rigorous formulation is available
in reflex terminology" (ibid; underlining added). He adds
been arrived at theoretically in setting up a
that the apparatus and procedure used were designed to:
system to deal with language purely as behavior,
and that a more rigorous presentation of the data
will be given in an exposition of that system now
in preparation. (ibid; underlining added)
Here we have a statement that not only is this research
expressible in reflex terminology, but that such an exposition
is now in preparation. Skinner is saying no less than this:
This system where verbal behavior is treated as any other
behavior is based upon—indeed an extension from—his
earlier (and simpler) system of animal behavior. This is
the first indication of what I call Skinner's extrapolation
stage, where he deliberately extends his simple model system
to a larger field. This simple system is a descriptive
model, if you will, whereas the extended one goes beyond the
data (of animal behavior) from which it derives, and is
thus a theory in a stronger sense of that term.

6.7. But the quote above, that the response to the
verbal summator is not evoked 'by any stimulus acting at
the moment', tells us much more -- as I have already
indicated. For not only is this research related to Skinner's
experimental work by being a deliberate extension from it,
but also by being a direct and clear-cut follow up to his
earlier attempts to solve his two major problems. The hint
in the quote above has to do with the stimulus, or the claim
that by the nature of the creature, a response to the verbal summator cannot be evoked by any stimulus acting at the moment:

The verbal summator does directly what a study of word-frequencies in normal speech does indirectly. The summator elicits responses IN THE ABSENCE of appropriate stimuli ... With either method we arrive at a description of the relative strengths of verbal responses. (Skinner, 1936f, 36; emphasis in original)

With this quote we see that Skinner is engaged in explaining what he had not explained in any way before; namely how non-elicitative stimuli work. Remember that he 'solved' the problem of the elicitation assumption before by showing that there are discriminative stimuli. Remember too that though Skinner says frequently in the autobiography that non-elicitative stimuli raise the probability of response, he can only do so with the aid of hindsight, as there is no evidence yet that this is how non-elicitative stimuli work.

But with this work on the verbal summator we finally see the beginnings of Skinner's eventual operant theory, for he is now attempting to explain -- to solve -- how non-elicitative stimuli work. Thus he now says we arrive at the relative strength of a verbal response in the absence of appropriate stimuli. He contrasts the relative frequency of response with overall frequency, as when he compares the verbal summator to normal speech:

In normal speech the responses 'refer to' external stimuli -- to whatever is being 'talked about'. In the case of summated behavior these stimuli are eliminated so far as possible. The resulting difference is that where the particular form occurring in normal speech can generally be accounted for
by pointing to a particular stimulus, in summated speech the occurrence must be attributed to the special strength of the response itself. (Ibid, 103; underlining added)

6.8. This quote is very important as it is the first instance of Skinner's denial of the elicitation assumption. In normal speech, he says, the response refers to an external stimulus. In the verbal summator the response has no such stimulus. It is a verbal response without an eliciting stimulus. By the very nature of the experiment -- in fact the whole point of the verbal summator -- was to produce a response that could not (by the very nature of the summation effect) be elicited by a stimulus in the immediate environment.

This is a solution to the elicitation problem, for not only does Skinner deny that all responses are caused by elicitive stimuli, but he is now also able to explain how non-elicitive stimuli work. Thus a verbal response is not explained by a reference to an external stimulus that elicits the response, but rather by the relative frequency of the response itself (to which his new measure of response rate is of good use). The solution here proposed to the elicitation problem constitutes a new theory, according to Laudan, as any solution to an unsolved problem is a theory. I have already argued that such a broad construal of problem/solutions leaves us with the absurd conclusion of having as many theories as we have solutions.

6.9 Arguing this way is not to deny that the solutions to unsolved problems have anything to do with the generation
of new theories. It is rather to insist that something more is needed before we have a new theory. This "something more" is the claim that the problem/solution is extendable or generalizable to other areas, or the claim that the entities proposed by the solution (non-elicited responses in this case) are of a kind found more generally. And indeed, Skinner says just that:

Aside from its use as a test, the summator is valuable in the study of other aspects of verbal behavior. (Skinner, 1936f, 71)

He does not elaborate the point in any detail, but says that we can approach normal speech in one of two ways. If we are interested in a certain number of expressions, or in the general frequency of a particular word, we emphasize "vocabulary". But a vocabulary does not exist in a uniform state of strength, he says, and adds that:

A verbal response [not to be identified with "word", he adds in a footnote] may be so weak as to be evoked by its appropriate stimulus only after a considerable period of time, as when we have difficulty in recalling a name. On the other hand, it may be so strong as to be evoked upon practically any occasion, as when we mention the name of a favorite person at every opportunity. A science of verbal behavior must deal with the conditions of latent speech ... (ibid, 72; underlining added)

6.10. So we see that Skinner's work on the verbal summator is very much related to his earlier experimental work. There are three points to this relation. The first is that the verbal summator is based on the paradigmatic use (in the exemplar sense) of two principles of the reflex tradition, Pavlov's imitative reflex and Sherrington's summation effect. These are analogical extensions to a new field,
verbal behavior. The second point is that the work on the
verbal summator centers around the concept of the stimulus,
for:

The summator is designed to obtain verbal responses
IN VACUO, so to speak. Stimuli which dictate the
elicitation of one response rather than another
are eliminated so far as possible.* (ibid, 90;
emphasis in original; underlining added)

As the whole point of the research is to study non-elicited
stimuli, Skinner turns the elicitation problem into a solved
problem, with his emphasis on the relative strength (frequency)
of a response for its causal efficacy. This is a solution
to both the definition and the measurement problem, for on
the one hand Skinner now denies that responses are necessary
reactions to elicitive stimuli, and on the other he says
these non-elicited responses are due to their own relative
strength. The former claim denies the elicitation assumption,
which effectively, splits apart stimulus-response correlations,
and the latter statement shows how these "free" responses
are to be measured by response rate (during extinction).

The third and final point to the verbal summator is
that it is an explicit attempt to extend the problem/solution
to a new area -- verbal behavior. Thus Skinner already talks
of "a science of verbal behavior" (ibid), and of a whole
system "to deal with language as behavior" (ibid), adding
that this work is now in preparation. The reference here
is to Verbal Behavior which was published in 1959, although
it appears to have been ready long before that. But
before we can examine that book, and the sense in which
non-elicited responses are extended to the verbal field, let us have a look at the debate with "Pavlov's pupils" -- Konorski and Miller -- for it is there that Skinner first introduced the term operant.

7. THE DEBATE WITH KONORSKI AND MILLER

7.1. Early in 1937 two Polish physiologists, Jerzi Konorski and Stefan Miller, published a paper: "On Two Types of Conditioned Reflex" (Konorski and Miller, 1937a), where they discussed Skinner's attempts to distinguish between two types of conditioning. To begin with they say that, in their opinion, Skinner's main analysis is correct. He correctly points out, they go on, that signalization is the main characteristic of Pavlovian conditioning, and that it is absent in the other type. But they are not happy with Skinner's treatment of the new type, and point out, as a matter of priority presumably, that in 1928 they had "made a discrimination between the ordinary reflex and a new type... which, by all appearances, corresponds to Skinner's" (Konorski and Miller, 1937a, 265).

In that former paper: "Sur une Forme Particulière des Réflexes Conditionnels" (Miller and Konorski, 1928), they had demonstrated that a dog shocked lightly in one foot would eventually lift it before the shock was administered. As they point out, a certain new phenomenon occurs in this conditioning process:

which is not considered by the Pavlovian laws. the specific stimulus eliciting the movement... becomes superfluous for the animal starts to
respond to the experimental situation ... by the movement [of the leg]. In other words, a conditioned reflex of a NEW TYPE makes its appearance. (Konorski and Miller, 1937a, 269; emphasis in original, underlining added)

7.2. Konorski and Miller are thus criticizing Skinner for not recognizing the fact that in the new type of conditioning (the discovery of which they claim priority) a new conditioned reflex is formed. This is contrary to Skinner's claim in the "Pseudo-Reflex" paper from (1935b), for there he said that only in classical Pavlovian conditioning is a new reflex formed (see section 1.9. above).

Skinner's erroneous interpretation they argue, is due to the lever. It plays a double role in his experiments:

On the one hand, it is $S_o$, as far as it elicits an investigatory response $R_o$ (pressing). On the other hand, it is also a prominent component of the whole experimental situation, $S_g$. Since the true mechanism of the new type of conditioned reflex consists, as we have shown, in the replacement of $S_g$ by $S_o$, this substitution in Skinner's experiments could not have been noticed, since $S_o$ and $S_g$ were represented by the same object. The only effect he could have recorded was an increase in frequency of pressing the lever, a fact which he erroneously attributed to the increase in strength of the investigatory reflex. (Konorski and Miller, 1937a, 267; underlining added)

The argument is that the investigatory reflex ($S_o - R_o$) is not the one that is conditioned, but that the $S_g - R_o$ is. But what is this $S_g$? It is the correlation of a new stimulus, $S_g$, which denotes the whole experimental situation, to lever pressing. This is what they mean by the creation of a new reflex in the new type of conditioning. The stimulus $S_g$, they say, "is here not merely a determining factor for the elicitation of $R_o$ by $S_o", but the very stimulus eliciting
Consequently Konorski and Miller never really denied that there was an eliciting stimulus in the new type of conditioning. It is just that due to the special circumstances of the experiment, a new stimulus $S_G$ comes to elicit the response, $R_o$. The old stimulus becomes superfluous, but only in the sense they insist, of being replaced by the stimulus of the total experimental situation.

The crucial issue in this criticism, is the status of the stimulus responsible for the conditioned movement. What Konorski and Miller demonstrated, in effect, was that at the beginning of the experiment the response is elicited by one stimulus, while later the response occurs prior to that particular eliciting stimulus (i.e. when the dog moves its leg before the shock).

As Konorski and Miller point out, Skinner missed this point by holding on to the erroneous distinction between Type I and Type II, while at the same time developing another type — the pseudo-reflex. Instead of distinguishing between Type I and II by the claim that in the Pavlovian case a new reflex is formed, while none is in the other type, Skinner should have realized, they think, that this was false, and used the characterization of the pseudo-reflex on the new type. This is because the light came to be 'the prominent feature of the whole experimental situation', which means for Konorski and Miller, that it becomes this $S_G$, or the new eliciting stimulus.
7.3. When their original paper was published in 1928, Konorski and Miller wrote Pavlov a letter, reporting their results. Pavlov responded promptly, took the results to be important, and asked for details. He even invited them to come to Leningrad, but they could not do so until 1931, when they had finished their medical degrees. Konorski stayed in Leningrad for nearly two years, and became -- as he says himself -- "one of Pavlov's pupils", with all the privileges of this little clan and with all its advantages and shortcomings" (Konorski, 1974, 191; the whole article is quite interesting especially Konorski's discussion of Pavlov's influence, the interaction between Pavlov and his "pupils", Pavlov's dictatorial tendencies, etc.).

After settling down in Leningrad, Konorski and Miller were given the task of replicating their 1928 experiments, this time under much more rigorous conditions. That they did, and confirmed their earlier thesis that Type II conditioning was different from classical Pavlovian conditioning. But what was Pavlov's reaction? Would he accept their conclusions, or had he just hoped that they would see the error of their ways, once in Leningrad? I quote the whole paragraph from Konorski's autobiography:

To end this description of my almost two years' stay in Leningrad, I would like to say a few words about my relations with Pavlov. As a matter of fact, they were far from being simple. There was no doubt that Pavlov highly appreciated the importance of our contribution to the field of conditioned reflexes, which, according to his own words, led to "physiological understanding of volitional movements". However, he strongly
opposed our thesis claiming the existence of two
types of conditioning and failed to see any difference
between them. He was so sensitive about this point
that when writing the above-mentioned paper for his
journal [J. Konorski and S. Miller (1936) in
Transactions of Pavlov's Laboratories] we simply
did not dare to use our own terminology and called
Type II conditioned reflexes "motor conditioned
reflexes" or "conditioned reflexes of the motor
analyser." Both of these terms were misleading.
(Konorski, 1974, 195; underlining added)

Thus, Pavlov would not accept their results, and when
pressed, could only admit that something "more has to be
introduced into the doctrine of higher nervous activity,
something that identifies it with the phenomena of mental
life ..." (see section 2.2. of chapter two), in effect
denying that any change was necessary in his theory.

Konorski elaborates on the effect of Pavlov's reaction
as follows:

It should be noted that this negative attitude of
Pavlov toward the specificity of type II conditioned
reflexes had a detrimental effect on the development
of the study of these reflexes in Russia. In fact,
had Pavlov accepted this specificity, the situation
would have been clear and the experimental work on
this type of conditioning would certainly have
developed in Russia as it did develop, quite
independently of our work, in the United States,
where type II conditioned reflexes were called
"instrumental" or "operant" responses. However,
when the greatest authority in the field stated
that type II conditioned reflexes simply do not
exist, this was decisive and meant special
investigations along this line, with insignificant
exceptions, were not undertaken in Russia. More-
over, when in 1949 the orthodox Pavlovism was
introduced in the Soviet Union, the term "type II
conditioning" was denounced as a manifestation of
my revisionistic tendencies and disapproved of.
(ibid; underlining added)

7.4. Pavlov could well ignore this result, for it
merely showed that, in certain circumstances, the original
eliciting stimulus might become superfluous, when the stimulus of the total experimental situation elicited the response before the original stimulus could. This was much like Skinner's first paper on conditioning (1932c -- see section 2.5. of chapter two for the discussion of that paper). In both cases the results were merely empirical and did not carry any immediate theoretical implications. The results were thus not counterinstances to Pavlov's principle of the conditioned reflex, although they are not explained by the theory either, but merely an indicator that the movement could -- under certain circumstances -- become conditioned sooner than Pavlov had thought. Why did Pavlov react so strongly to Konorski, the curious reader might ask? It is because Pavlov was not reacting to the result, but merely to Konorski's interpretation of it. The former, but not the latter, saw no reason to postulate a new type of conditioning to account for these facts.

In Skinner's case the rats responded at a high rate as soon as the first press of the lever was followed by food. A high rate meant a strong reflex (given Skinner's measure of reflex strength at that time -- 1932), so the result is merely that a reflex can become conditioned upon a new stimulus in 4-6 less trials than in the classical Pavlovian procedure. Pavlov's signalization interpretation of the conditioned reflex (see section 4 of chapter one) does not state that conditioning is impossible in one trial. It merely asserts that a conditioned reflex can be established
by "any stimulus ... previously ... applied a sufficient number of times" (Pavlov, 1927, 27; underlining added). Given the special circumstances of Skinner's experiment, one trial was a sufficient number of times.

Pavlov continues, in fact, to point out that reversing the order of stimuli (so that the conditioned stimulus (e.g. light, or the sound of a bell) is presented first) no conditioned reflex is established:

With another dog the loud buzzing of an electric bell set going 5 to 10 seconds after administration of food failed to establish a conditioned alimentary reflex even after 374 combinations, whereas the regular rotation of an object in front of the eyes of the animal, the rotation beginning before the administration of food, acquired the properties of a conditioned stimulus after only 5 combinations. The electric buzzer set going before the administration of food established a conditioned alimentary reflex after only a single combination. (ibid; underlining added)

Quote obviously Pavlov can well accept one-trial conditioning, as he even reports it himself, which shows that it is not the experimental result itself that causes problems, but rather the interpretation one puts on that result. The fact of one-trial is therefore merely an unsolved empirical problem, and as such does not pose any major threat to Pavlov's theory.

7.5: This is why Skinner did not see his original result as anomalous to Pavlov's theory, but much rather as a new fact that should be incorporated into the theory sooner or later. Skinner saw of course, that the discrepancy in the number of trials between his results and Pavlov's needed some explanation, but that was just one more unsolved problem.
He did speculate too that he was on to some "process of conditioning that was different from Pavlov's and much more like most learning in daily life", but these I characterized as mere worries. These worries transformed and increased as was explained in chapter two, especially when Skinner tried to measure one-trial conditioning. Slowly they crystallized into two problems, of definition and measurement, but it was only after the negative result of the predictor test of disinhibition, and the fruitless attempt to seek help from reflex physiology proper, that Skinner came to regard the two problems as his problems, that needed his own solution. I further argued that only when Skinner "solved" these problems, first rather unsuccessfully and then more clearly in the verbal summator paper, and prepared to extend that problem/solution to other fields (e.g. the verbal field), does he have the beginnings of an operant theory, or operant theory in embryo. By that time he had solved the problem of one-trial conditioning, and it was only then that the unexplained problem became anomalous. But anomalous for whom? Laudan emphasizes that it is only when one theory has turned an unsolved problem into a solved one that it becomes anomalous for any other theory that has not solved that same problem. Is it true that it became imperative for Pavlov and his pupils to explain the unsolved problem, not because it contradicted anything in the theory, but rather because there had appeared a new theory -- operant theory -- that explained the result? Or did operant theory turn up as the result of
the growing state of crisis in the field?

7.6. Let us now look more carefully at the later work of Konorski and Miller, for the interesting question now is why they (or Konorski rather, as he was the one to pursue these interests further) were 'Pavlov's pupils' to the end. That fact has to be explained, for Pavlov never accepted Konorski's Type II conditioning, and as we saw before, helped in labelling him "a revisionary". Why did Konorski not abandon Pavlov and his theory, and why did he not propose a new theory on the basis of his results, like Skinner did? How could Konorski do both, maintain that there was a new type of conditioning (i.e. agree with Skinner), and not see that result as anomalous for Pavlov, as Skinner did? Phrasing the question this way, we see that the issue is what the difference is between Konorski and Skinner, that made only one of them see an anomaly.

First, let me show that Konorski was a Pavlovian to the end, or put differently, that he always belonged to the reflex tradition. He did not start a revolution against that tradition like Skinner did, for presumably he merely had "revisionistic tendencies". In the 1928 paper, Konorski and Miller start by saying that "The work presented below is based on Pavlov's theory of conditioned reflexes" (Miller and Konorski, 1928, 182; Skinner's translation). Later in the monograph: Conditioned Reflexes and Neural Organization (Konorski, 1948), Konorski says he is interested in attempting a unified theory based on the work of Sherrington and Pavlov.
In a later book, *Integrative Activity of the Brain* (Konorski, 1967) he points out that the experimental approach of Pavlov "was exactly the same as that of Sherrington" (ibid, 1), and that their main difference was that Sherrington's work centered around the spinal cord, while Pavlov's concerned the physiology of the brain. Sherrington's theory is still "sound and fruitful" in general outline, he continues, although "a number of his generalizations proved to be erroneous" (ibid, xv). Greater strides have been made in the work on the physiology of the brain, he further points out, which have made Pavlov's theory rather obsolete.

Konorski's task then, is to:

attempt to extend the Sherrington conception of the functioning of the nervous system to the field of higher nervous activity. This has been done on the basis of the enormous experimental material collected by the Pavlov school over almost forty years of research. (Konorski, 1948, xvi-xvii)

Konorski's later work is in the reflex tradition, and is an attempt "within the framework of the tradition, to improve and correct ... predecessors" (Laudan, 1977, 81), as Laudan expresses his view that a research tradition is a collection of individual (and often rival) theories in succession.

Konorski's work is moreover an attempted synthesis of these two predecessor theories, or the attempt to correct both theories in view of the vast experimental material collected, while keeping their general framework in touch. The dedication in the *Conditioned Reflexes and Neural Organization* expresses well the extent to which Konorski relied on his two predecessors:
DEDICATED TO
I.P. PAVLOV AND C.S. SHERRINGTON
IN THE HOPE THAT THIS WORK
WILL DO SOMETHING TO BRIDGE THE GAP
BETWEEN THEIR RESPECTIVE
ACHIEVEMENTS

(Konorski, 1948)

7.7. Skinner was able to reply to Konorski and Miller in the same issue they criticized him in, and in his reply: "Two Types of Conditioned Reflex: A Reply to Konorski and Miller" (Skinner, 1937a), he says quite simply that they are right. He points out though, that he has already remedied the problem in "a work now in progress", so that his answer is therefore, less "a comment on the points raised by Konorski and Miller, than a revision-in-process of his earlier formulation" (Skinner, 1959, 489). This quote is from Skinner's introduction to the reprint of his (1937a) paper, and he claims not to have known of the work of Konorski and Miller prior to their (1937a) publication. The operant is probably discovered independently by Skinner, as he emphasizes the work now in progress (The Behavior of Organisms).

Clearly Konorski and Miller's criticisms of the pseudo-reflex is very important to the development of Skinner's views, for he first introduces the concept of the operant in his reply to them (as we will see in a moment). As to the question of priority, Skinner points out that "behavior
characteristic of Type R [operant behavior] was studied as early as 1889 (Thorndike)" (Skinner, 1937a, 496), Thorndike thus preceding both Konorski and Miller and himself. This is a little unfair of Skinner, for though Thorndike did study the consequences of behavior, and did propose his laws of effect and exercise, he certainly was not bothered by the same problem (the elicitation assumption) as Konorski and Skinner clearly were. Notice further how close the solutions are, for Konorski and Miller discovered behavior whose eliciting stimulus "became superfluous" and Skinner discovered "non-elicited" behavior.

But what exactly is Skinner agreeing with when he says that Konorski and Miller are right in their criticisms of his position? He is agreeing essentially, that he should not have emphasized differences in responses in his 1935 paper, and that the lever does indeed play a double role as a stimulus, in his experiments:

The distinction between an eliciting and a discriminative stimulus was not wholly respected in my earlier paper, for the reflex (lever-pressing) was pseudo. As a discriminated operant the reflex should have been written (S + LEVER - PRESSING). Since I did not derive the two types from the possible contingencies of the reinforcing stimulus it was not important that R, in Type R [operant] be independent of an eliciting stimulus. (Skinner, 1937a, 493; emphasis in original)

The lever was the $S_0$ for Konorski and Miller, and the lever plus the light was the $S_G$, or the total experimental situation. But instead of widening the concept of an eliciting stimulus to such an extent that it applies to the whole experimental situation, Skinner takes the opposite line, because:
the treatment of the lever as ELICITING an unconditioned response has proved inconvenient and impracticable in other ways, and the introduction of the notion of the operant clears up many difficulties besides those immediately in question. It eliminates the implausible assumption that all reflexes ultimately conditioned may be spoken of as existing as identifiable units in unconditioned behavior and substitutes the simpler assumption that all operant responses are generated out of undifferentiated material (ibid; emphasis in original, underlining added).

Instead of assuming that either the lever or the light (or both) elicit the lever-press, Skinner says that "$S_L$ [the lever] is contingent upon $R_o$ [the press of the lever] in the presence of a stimulus $S_D$ [the light]." (ibid, 492).

Pressing the lever is an operant in the sense that it is not an elicited response to the lever or the light, but made more probable in "the light of the lever" so to speak.

Operant behavior, says Skinner: cannot be treated with the technique devised for respondents (Sherrington and Pavlov) because in the absence of an eliciting stimulus many of the measures of reflex strength developed for respondents are meaningless. In an operant there is properly no latency (except with respect to discriminative stimuli), no after-discharge, and most important of all no ratio of the magnitudes of R and S. (ibid, 493)

In spite of repeated efforts, he continues, the magnitude of the response in an operant is simply "not a measure of its strength" (ibid). This point is important for it shows how Skinner extends his solution from the work on the verbal summator.

He denies the necessity of the stimulus-response correlation by defining behavior as non-elicited (and thus solves the definition problem of the elicitation assumption), and says that some:
measure must be devised, and from the definition of
an operant it is easy to arrive at the RATE OF OCCURRENCE
of the response. This measure has been shown to be
significant in a large number of characteristic changes
in strength. (ibid.; emphasis in original)

Again the solution comes from the verbal summator work, for
there he had already said that the relative frequency of
response was the correct measure of non-elicited responses.

7.8. Skinner finally corrects the confusing use of
'Type I' and 'Type II', by saying that he shall "refer to
conditioning which results from the contingency of a
reinforcing stimulus upon a STIMULUS as of Type S" (i.e.
classical Pavlovian conditioning), "and as to that resulting
from contingency upon a RESPONSE as of Type R" (i.e. the
new type) (1937a, 490; emphasis in original).

The two new types can now be represented as follows:

(9) Type S:

(10) Type R:

\[ S' \xrightarrow{r} r \]

\[ S'_1 \xrightarrow{R_0} S_1 \xrightarrow{(R_1)} \]

The change in the new formulation is the lower case r
in Type S and the lower case s in Type R. These are written
like that because, in the process of conditioning, they
"(a) cannot be identified, (b) may be omitted, or (c) may
disappear" (ibid., 491), as Skinner says in the (1937a)
paper. Notice what he is actually saying: On the one hand
he is claiming that in Type S (Pavlovian) there may be no
original reflex \((S'_o - r)\) as the \(r\) may be omitted. On the other hand, Skinner is claiming that in Type R (Skinnerian one-trial conditioning) there may be no original eliciting stimulus. The first claim, I take it, is Skinner's rejection of the reflex as the basic unit of analysis, for if there may be no response (no small \(r\)), then obviously the initial stimulus \((S'_o)\) may stand alone, as a smaller, more basic unit than the reflex.

The second claim, is more crucial, for our purposes. It is the converse admission that there can also be responses without stimuli, or put differently, that not all responses are caused by elicitative stimuli. Apart from again breaking down the reflex as a unit, we can see a relation here to the work on the verbal summator (see especially section 6.10. above), for Skinner now extends that conclusion to conditioning. This is probably the reason Skinner insists he had come to this conclusion before the Konorski and Miller criticism appeared, because the work on the verbal summator was published in 1936, a year before the Konorski and Miller paper was.

7.9. By this new formulation Skinner is clearly admitting to the correctness of Konorski and Miller's criticism, but he does not stop there. He goes on to point out that the new formulation:

depends upon the statement that there are responses uncorrelated with observable stimuli... it is a necessary recognition of the fact that in the unconditioned organism two kinds of behavior may be distinguished. There is, first, the kind of response which is made to specific stimulation,
where the correlation between response and stimulus is a reflex in the traditional sense. I shall refer to such a reflex as a respondent and use the term also as an adjective in referring to the behavior as a whole. (Skinner, 1937a, 491; underlining added)

There is also another kind of behavior, Skinner goes on, which is spontaneous in the sense that it is not elicited. That does not mean the response is not caused, but merely that it is impossible to find a stimulus that elicits such behavior at the moment it is emitted:

It is the nature of this kind of behavior that it should occur without an eliciting stimulus, although discriminative stimuli are practically inevitable after conditioning. It is not necessary to assume specific identifiable units prior to conditioning, but through conditioning they may be set up. I shall call such a unit an OPERANT and the behavior in general, operant behavior. (ibid, 491-92; emphasis in original, underlining added)

This was the first time Skinner used the term operant, and he says that the distinction between operant and respondent behavior and the special properties of the former "will be dealt with at length in a work now in preparation" (Skinner, 1937a, 492). This is of course, a reference to The Behavior of Organisms, which was published a year later. But before we consider that book, and the sense in which it is a new theory, let me first say something more about the discovery of the operant, how it solves Skinner's two major problems, and why Konorski and Miller react so strongly against it. It would seem that they should be quite happy with Skinner's reply, for he accepts much of their criticism and
accepts their claim of the priority to the discovery of the new type of conditioning.

As argued above, Konorski remained devoted to Pavlov, and committed to the research tradition of reflex physiology. Evidently his discovery of behavior whose elicited stimuli could become superfluous was not enough to shake his conviction. But how was that so? Why did he not see (or come to see) the discovery of non-elicited behavior as anomalous for Pavlov's theory, like Skinner did? To answer this question let us look in more detail into Konorski and Miller's first publication, for maybe it is in their characterization of Type II conditioning that we will find a crucial difference from Skinner.

7.10. In their 1928 paper, Konorski and Miller say that in their attempt to replicate Pavlov's experiments on compound stimuli (see Lecture VIII of Pavlov's Conditioned Reflexes), their starting point was the following fact:

> when a compound consisting of two stimuli, A and B, is accompanied by an unconditioned stimulus R, which reinforces only the whole of the compound without reinforcing either of its components presented separately, a conditioned reflex is established only to the compound AB, whereas the conditioned response is not evoked by either of the two stimuli presented separately. (Miller and Konorski, 1928, 187; Skinner's translation)

With a tone from a piano and the lifting of a dog's leg (passive or active) as the A and B respectively, the presentation of food as R, the AB alone would function as the conditioned stimulus according to the principle of the conditioned reflex, after a certain number ofelicitations.
But Konoński and Miller found something more:

at the same time a new phenomenon will appear
which is not predicted by Pavlov's theory:
after some time the lifting of the leg, whether
reflexive or passive, becomes superfluous,
because the sound of the piano itself will
elicit the movement. (ibid; underlining
added)

This new reflex is still a conditioned reflex, they
think, for it shares all the general properties of conditioned
reflexes (e.g. it originates in the cortex, is not innate,
but formed during the experiment, and can disappear easily):

It is for this reason that we regard them as
conditioned reflexes, but their mechanism
is different from that of the conditioned reflexes
of Pavlov, hence we call them conditioned reflexes
of the second type. (ibid, 188; underlining added)

They further point out, just like Skinner, that in this new
type there is no signalization, and that the stimuli may
vary (see Skinner's letter to Keller section 2.4. of chapter
two for the same point), and that:

The reinforcing stimulus ... can be a positive or
negative stimulus which evokes a specific reaction,
in opposition to the conditioned reflexes of Pavlov
where the response to a reinforcing stimulus must
be the same as that of the conditioned stimulus.
( Ibid )

For these reasons Konoński and Miller are unable to reduce
the reflexes of the second type to the conditioned reflexes
of Pavlov, and they "consider them consequently as the second
fundamental mechanism of the function of the cerebral cortex"
( ibid ).

7.11. So we see that Konoński and Miller claim, just
like Skinner, to have found a new type of conditioning,
irreducible to Pavlov's conditioned reflex. They further
agree on a number of distinguishing features of the new type, though Konorski's (and Miller's) treatment of them is much clearer than Skinner's, in the "Pseudo-Reflex" paper from 1935. We have further seen that Skinner agreed to their criticisms, once the problems were pointed out to him, and corrected his distinction between the two types. The first he called respondent, while the new type was operant, he said, claiming further that the distinction reflected two kinds of behaviors.

But interestingly enough, Konorski and Miller found reason to continue the debate, although Skinner had answered their criticisms effectively, by agreeing with them. This is where we finally see a crucial difference between Konorski (and Miller) on the one hand, and Skinner, on the other, for in that paper: "Further Remarks on Two Types of Conditioned Reflex" (Konorski and Miller, 1937b) they say that they cannot agree with Skinner's new answer. They do point to the convenience of Skinner's new labels, respondents and operants, but cannot accept their consequences. Why call the behavior operant, they ask, and why does Skinner claim it is without "an elicitory stimulus"? This statement does not altogether deny the relevance of stimuli, they further point out, or that such eliciting stimuli could be found, but only that the original eliciting stimulus is not operating at the time the behavior is observed. This they must admit of course, as they had themselves claimed to have experimentally manipulated reflexes in such a way that the original elicitative stimulus "became superfluous". But they do not
want to call such behavior operant.

We see no need and no basis to assume the existence of such behavior. We think that by sufficiently inclusive interpretation of the term "stimulus", which is dispensable even for the conventional S type, every behavior could be said to have its stimuli. This statement follows from general premises concerning the physiology of nervous system. There is no reason whatever to assume anything contrary, unless there were strictly established facts pointing to that effect. But such facts are lacking. (Konorski and Miller, 1937b, 405; underlining added)

7.12. Quite an interesting statement. It shows clearly how Konorski and Miller differ from Skinner. Both agree that there is a new type of conditioning and that it requires another mechanism for its explanation. In that sense the statement of the existence of a new type of conditioning is an unsolved problem (e.g. How does it relate to classical Pavlovian conditioning?). But as Konorski and Miller say so clearly in the quote above, there is no need to assume the existence of a new kind of behavior. There is no such operant behavior, they insist, for with a "sufficiently inclusive interpretation" of the concept of the stimulus, all behavior "could be said" to have its stimulus. Konorski and Miller are therefore not ready to solve the problem by denying the elicitation assumption, for as they say so clearly, it "follows from general premises concerning the physiology of the nervous system."

Skinner solved the problem by denying that assumption, and as it is the general premise of the tradition, he is revolting against it. Konorski on the other hand, remained a "revisionary", and tried in his later works to explain the new type of conditioning by other means (i.e. without denying
the elicitation assumption).

But why did Skinner see the existence of one-trial conditioning as anomalous, when Konorski and Miller saw the same thing as merely an unsolved problem? The major characteristics of the new type were the fact that it could take place in one trial and that the stimuli could vary considerably. The latter led Skinner to the solution by the denial of the elicitation assumption, but what of the former characteristic? Strangely enough that fact (that conditioning could take place in but one trial) did not cause the crisis, for not only does Pavlov himself report such a result, but Konorski and Miller too. "The taking place of conditioning of Type S after one reinforcement", they rather awkwardly point out, "is no less possible than of Type R" (Konorski and Miller, 1937b, 406).

7.13. We are left with but one possibility, for it is simply not correct that one-trial conditioning was a crisis causing anomaly. We have seen that the fact it could take place in one trial was no counterinstance to Pavlov's principle, and that neither Pavlov nor Konorski (and Miller) saw it as a counterinstance. The problem was simply how to interpret the new type of conditioning. Konorski insisted that we should not abandon a fundamental assumption of the tradition -- the elicitation assumption -- in order to explain the problem, and he has interestingly enough, later taken back many of his original claims. In the POSTSCRIPT to Skinner's translation of the original 1928 paper, he says
that "almost every single thesis of the [1928] paper is more or less erroneous," and that:

the sharp distinction between, not only the procedural side of type I and type II conditioned reflexes, but also between their physiological mechanisms seems to me now largely exaggerated. In fact, further investigation shows with increasing clarity that both types can be explained on the basis of the same general principles of connectionistic processes. (Konorski, 1969, 189; underlining added)

We have seen how the simultaneous discovery of a new kind of conditioning by Konorski and Miller and by Skinner led them in very different directions. For the former the normal science within the reflex tradition did not turn revolutionary, for at no point did they see the existence of a new type of conditioning as an anomaly (let alone an extraordinary one) until Skinner had answered their criticism. Konorski even tried to minimize the problem later, as he insisted that nearly every thesis of the original 1928 paper was in fact, incorrect. He further maintained, in his attempt to develop a successive theory based on his predecessors, Pavlov and Sherrington, that the difference between the two types of conditioning was largely exaggerated.

7.14. Skinner on the other hand, saw (or rather came to see) the existence of a new kind of conditioning as an anomaly and an extraordinary one at that. Kuhn's account of the puzzle-promotion phenomenon, that a crisis is a prerequisite to any transformation from normal science to revolutionary, seems supported here. But that support quickly evaporates, for the search for that crisis was
unsuccessful and only led to the existence of a rival theory within the reflex tradition -- operant theory in embryo. The work on the verbal summator was done before the debate with Konorski and Miller. Consequently, the crisis was not the prior existence of a counterinstance or anomaly. As we saw, the fact that the new kind of conditioning could take place in one trial was no counterinstance to Pavlov's theory, as the signalization principle itself merely expressed "a sufficient number of trials". Pavlov could well say that, in certain circumstances, like Skinner's, one trial was simply a sufficient number. This is what he in effect did, and so did Konorski and Miller in their final reply to Skinner.

Why then did Skinner solve his problem in such a radical way, i.e. by postulating a new kind of behavior -- spontaneous non-elicited operant behavior? For what reason was he, but not Konorski, ready to abandon the fundamental assumption of the reflex tradition -- the elicitation assumption? The only possible answer now is that he had already found reason to doubt that assumption, and had in fact already abandoned it in the work on the verbal summator. In that work furthermore, he was already speaking of a "science of verbal behavior", so the inevitable conclusion is that it was not the crisis that caused the revolution, but the other way around. In other words, operant theory did not start as an answer to a growing crisis within the tradition, but did cause a crisis once it was developed within normal science.
Skinner had no reason to abandon the elicitation assumption, given Konorski and Miller's criticisms, for they did not do so. But he had independently and for other reasons, and within the normal science of his individual research program, already developed the beginnings of a new theory -- operant theory in embryo. As he says himself, the debate with Konorski and Miller made him realize the step he had already taken, so he merely, in his reply to them, extended the problem/solution of that work to conditioning, and thereby solved the elicitation problem for conditioning. Only after he did that, did Konorski and Miller find reason to object to Skinner, as they rejected Skinner's answer (i.e. solution) even though he agreed with them. The conclusion therefore, is that one-trial conditioning was merely an unsolved problem until Skinner proposed a solution in terms of an independently developed new theory, which only then turned the unsolved problem into an anomaly for Pavlov's theory. And that conclusion does not support Kuhn's monothetic account of crisis-causing anomalies, but rather Laudan's account of puzzle-promotion by way of an emerging rival theory.

8. THE SECOND EXTENSION: OPERANT THEORY PROPER

8.1. In the last few sections I have tried to show how Skinner slowly came to realize that most behavior was not reflexive in any sense of that term. The reflex slowly disappeared from his main theoretical framework, while the term operant, understood as the spontaneous activity of an intact organism, took its place. This is not to deny of
course, that there are reflexes, but rather to insist that
the reflex is not the basic building block from which to
derive a comprehensive theory of the behavior of organisms.

Let us now pursue this matter further to see how
Skinner finally rids his theory of any mechanistic reflex
associationism, by extending (or extrapolating as he says
himself) the crucial concept of the operant to complex human
behavior.

8.2. Skinner could not finish The Behavior of Organisms
by the end of his Junior Fellowship. He had been selected
for three years, beginning in 1933, so by 1936 he again had
to look for a job. He was in much the same situation as
before (see section 7.5. of chapter two), but after being
turned down by Northwestern, and a few other universities,
Skinner got a position at Minnesota before the year's end.
He said he did not have a heavy teaching load, and was soon
back into research. He found time to make graphs of the
data not published, he says and "to put together not only
an account of [his] research but a 'system'" (SB, 201).

The Behavior of Organisms was eventually published in
1938. It bears the subtitle of "An Experimental Analysis",
as he says he is interested in "setting up a system of
behavior in terms of which the facts of a science may be
stated and, second, in testing the system experimentally
at some of its more important points" (BO, 5). The book is
therefore an attempt to describe the system he had slowly
been developing, and Skinner's new distinction between
respondent and operant behavior became a crucial component. He repeats his arguments (from the debate with Konorski and Miller) for this distinction, and insists upon the operational definition of the reflex.

To prepare the way for a clarification of how *The Behavior of Organisms* represents a new theory of behavior -- operant theory -- let me quote a letter Skinner wrote to Keller sometime in 1937. Keller had been expressing added interest in the details of Skinner's views (as already pointed out in sections 7.7 - 7.9 of chapter two), and after advising Keller that "the central problem" is conditioning, he adds:

I've made some rather sweeping changes in my system, preparatory to getting out a book this summer. Have two kinds of behavior OPERANT and RESPONDENT. No elicitory stimulus for the first (there may be discriminative stimuli). An operant is a cagstrated reflex with no stimulus. (SB, 182; emphasis in original)

The lever is an example, he goes on, for it is a "discriminatory" stimulus. That is to say, in the presence of certain stimuli (from the lever), certain responses will be reinforced. We see that Skinner is finally clear in how to talk about reinforcing and discriminative ("discriminatory") stimuli. In fact this use of these terms comes quite easily to him, once he has solved the elicitation problem. Thus he now says that:

The lever is the OCCASION upon which the reacting response gets an effect but it does not elicit the response in the ordinary sense of elicit. The thing is quite different from a RESPONDENT (e.g., flexion reflex or Pavlovian conditioned reflex) ... (ibid, 182-3; emphasis in original)

8.3. The most obvious sense in which we see a new
theory in *The Behavior of Organisms* is through Skinner's solved problems. He solved the problem of the elicitation assumption by showing that there were responses controlled by discriminative and reinforcing stimuli, and those stimuli are not elicitative. He had further solved the measurement problem by claiming that such responses were effected by their own relative frequency, which he had all along been measuring, by his response rate (during extinction) measure. The operant as we saw in the last section, combined these two solutions into one coherent whole, as they were response classes controlled by discriminative stimuli, and made more probable by reinforcing stimuli. The operant furthermore, denies directly the elicitation assumption, by definition, and by measurement, denies all the classical measures of reflex strength.

But so far this is but an operant theory in a weak sense, as this is but mere answers to particular previously unsolved empirical and conceptual problems. The best way to see how we get operant theory in the full sense of that term, is to compare the operant to the reflex tradition, from which it is thought derived.

Looking at the first 20 pages of *The Behavior of Organisms* one gets the strangest feeling, for on the one hand we see repeated references to Sherrington and Pavlov, along with an explication of some of their laws, and on the other we read about the operant. Both discussions are reasonable given the history of the case, but somehow they do not belong
together. Let me explain.

8.4. Chapter one of *The Behavior of Organisms* starts with an insistence on behavior as a proper scientific datum, which I argued in section 3 of chapter one to be the first thing that unites the reflex tradition. The first sentence reads:

>Although the kind of datum to which a science of behavior addresses itself is one of the commonest in human experience, it has only recently come to be regarded without reservation as a valid scientific subject matter. (BO, 3)

And behavior he defines as "the movement of an organism or of its part in a frame of reference provided by the organism itself or by various external objects or fields of force" (ibid, 6). The environment, he further observes, enters into a description of behavior when it can be shown that:

>a given PART of behavior may be induced at will (or according to certain laws) by a modification in part of the forces affecting the organism. Such a part, or modification of a part, of the environment is traditionally called a STIMULUS and the correlated part of the behavior a RESPONSE. Neither term, may be defined as to its essential properties without the other. For the observed relation between them I shall use the term REFLEX, for reasons which, I hope, will become clear as we proceed. (BO, 9; emphasis in original)

But to gather information about reflexes, Skinner now thinks (as he has changed his research program -- see sections 1 and 2 above), is not useful. We are not interested in the mere collection of reflexes. The discovery of the reflex was an historically important event, he admits, but to:

>believe that the study of behavior is concerned primarily with topographical prediction of stimuli and response ... is a mistaken, and fatal, characterization of its aim. (ibid, 11)
Skinner goes on to list the various "Static Laws of the Reflex" (BO, 12-14), much in the spirit of Sherrington. These constitute the familiar laws of latency, the threshold, magnitude of response, temporal summation, etc., and are in effect, his operational redefinition of Sherrington's laws of the synapse. These laws are static in that they prescribe a change in one (dependent) variable, given the manipulation of some independent variable. To these he adds "Dynamic Laws" (BO, 14-19), like the laws of reflex fatigue, facilitation, and inhibition. These laws are dynamic in the sense that the operation performed upon the organism effects "a simultaneous change in the values of all the static properties" (ibid, 15).

These two kinds of laws function really, as an indicator of Skinner's debt to Sherrington (e.g. threshold, reflex fatigue, summation, etc.) and to Pavlov (e.g. magnitude of response, facilitation, inhibition, etc.), for after having listed them Skinner goes on to discuss the laws he is now interested in; laws that turn out to be quite different from these static and dynamic laws of the reflex. In fact there is little or no mention made of these laws later on in the book (i.e. after he introduces the operant), and they are nowhere to be found in any of his later books.

8.5. Among the dynamic laws listed is the "Law of Conditioning of Type S", or Skinner's formulation of classical Pavlovian conditioning:

THE APPROXIMATELY SIMULTANEOUS PRESENTATION OF TWO STIMULI, ONE OF WHICH (THE 'REINFORCING' STIMULUS) BELONGS TO A REFLEX EXISTING AT THE MOMENT AT SOME STRENGTH, MAY PRODUCE AN INCREASE IN THE STRENGTH
OF A THIRD REFLEX COMPOSED OF THE RESPONSE OF THE
REINFORCING REFLEX AND THE OTHER STIMULUS.
(BO, 18; emphasis in original)

He then starts the discussion of operant behavior, saying
that with the discovery of the stimulus and the collection
of a large number of specific relationships of eliciting
stimuli and responses, many writers (and here he has some
experience himself) came to assume that all behavior could
be accounted for in this way. Many elaborate attempts have
been made to establish this (witness his own original
research program). Skinner goes on, but they have not, he
now admits, proved convincing:

There is a large body of behavior that does not
seem to be ELICITED ... The original 'spontaneous'
activity of the organism is chiefly of this sort,
as is the greater part of the conditioned behavior
of the adult organism, as I hope to show later.
(BO, 19; emphasis in original, underlining added)

Skinner then gives the -- by now familiar -- definition
of the operant as behavior not under the control of an
eliciting stimulus, and points out that the static laws of
threshold, latency, etc., are meaningless for the operant.
Most dynamic laws are inapplicable as well, but these two
laws are essential:

THE LAW OF CONDITIONING OF TYPE R. If the occurrence
of an operant is followed by presentation of a
reinforcing stimulus, the strength is increased.

THE LAW OF EXTINCTION OF TYPE R. If the occurrence
of an operant already strengthened through conditioning
is not followed by the reinforcing stimulus, the
strength is decreased. (BO, 21; emphasis in original)

8.6. All these former laws are meaningless for the
operant for the simple reason that they make reference to
reflexes, and eliciting stimuli. But what is so special about the operant, one might ask? Certainly, Konorski and Miller discovered the same entity, but thought not too much of it. In fact, it was only after Skinner had responded to them (and referred to The Behavior of Organisms as work in progress) that they openly disagreed with him. That meant, as we saw in the last section, that by proposing a new theory, Skinner turned an unsolved problem for Pavlov and his pupils into an anomaly.

But I still have not clarified adequately why or how the operant was so important to Skinner. I have indicated the curious status of the laws deriving from Sherrington and Pavlov, but a careful reading of the latter part of the book reveals even further how meaningless for Skinner these laws really are. "The most outstanding aspect" of The Behavior of Organisms, Skinner is bold enough to claim at the end of the book, is "the shift of emphasis from respondent to operant behavior" (BO, 438). Operant behavior has a unique relation to the environment, he goes on, and represents a separate important field of investigation" (ibid; underlining added). He even speculates whether respondent behavior, with its Type S conditioning, reflexes, and emphasis upon the internal economy of the organism, "may not reasonably be left to the physiologist" (ibid).

This is the step Skinner has taken, virtually declaring a new field of ignorance. Thus, his field is "the operant rather than the respondent" (SB, 201) he would explain later.
adding that he had thought in the thesis from 1931 that the concept of the reflex was all that he needed for a science of behavior:

I knew better by the time I began to write my book *The Behavior of Organisms*. My field was the operant rather than the respondent, and my measure of strength was probability (or at least rate) of responding rather than magnitude of response or latency or after-discharge. (Ibid)

But to see that the full step is not yet taken, one has just to point out that this would have been "the right time to abandon the 'reflex'" (Skinner, 1976b, 19), as he expressed it much later, though the reflex is still used on both respondents and operants in *The Behavior of Organisms*, "even though in its original meaning it applies to respondents only" (BO, 20-21). Skinner justifies this unfortunate decision by saying that "the notion of a reflex is to be emptied of any connotation of the active 'push' of the stimulus" (Ibid).

8.7. Thus Skinner outlines his "system" in 1938. I have tried to show how his views developed up to this point, and how Skinner all but declares a new field of investigation. But notice that he confounds matters, first by redefining these laws from Pavlov and Sherrington, although he really has no use for them, and secondly, by his unfortunate decision to hold on to the concept of the reflex. To see however, that these are mere leftovers from the reflex tradition, notice that by defining behavior as that part of the functioning of the organism "which is engaged in acting upon or having commerce with the world"
(BO, 6), Skinner has really made that declaration, since respondent behavior is usually not 'acting upon the world' in this way, and may thus be left to the physiologist. But before I finish this section let me indicate further how Skinner has indeed left the reflex to the physiologists, and concentrated on developing further (i.e. extended) the concept of the operant.

8.8. At the end of The Behavior of Organisms Skinner says:

The reader may have noticed that almost no extension to human behavior is made or suggested. This does not mean that he is expected to be interested in the behavior of the rat for its own sake. The importance of a science of behavior derives largely from the possibility of an eventual extension to human affairs. (BO, 441; underlining added)

He goes on to point out that the book "represents nothing more than an experimental analysis of a representative sample of behavior. Let him extrapolate who will" (ibid, 442; underlining added). While Skinner would probably deny he had any will, his subsequent books, especially Science and Human Behavior (1953), Verbal Behavior (1957), Contingencies of Reinforcement: A Theoretical Analysis (1969a), Beyond Freedom and Dignity (1971a), and finally Reflections on Behaviorism and Society (1978) made it pretty clear who had the will to extrapolate. In fact, it is the main theme of all these books that they are based on, and ultimately, extrapolations from, the basic experimental work.

In his first book Skinner further points out that the justification for this extrapolation is an open question:
We can neither assert nor deny discontinuity between the human and subhuman fields so long as we know so little about either. If, nevertheless, the author of a book of this sort is expected to hazard a guess publicly, I may say that the only differences I expect to see revealed between the behavior of a rat and man (aside from the enormous differences of complexity) lie in the field of verbal behavior. (BO, 442)

Since then of course, he has written Verbal Behavior. As we have seen, this was in fact the first theoretical work he engaged in after the publication of The Behavior of Organisms (and as the work on the verbal summator shows, this research must have started even earlier than that, 1934-5) -- see section 6 above, even though it was published after Science and Human Behavior. As for verbal behavior we are told that our first responsibility is simple description:

Once that question has been answered in at least a preliminary fashion we may advance to the stage called EXPLANATION: what conditions are relevant to the occurrence of the behavior -- or what are the variables of which it is a function? Once these have been identified, we can account for the dynamic characteristic of verbal behavior within a framework appropriate to human behavior as a whole. (SHB, 10; emphasis in original)

Again we come back to Skinner's functional analysis, which always means for him, the investigation of the independent variables of which behavior is a function.

The relationship between independent and dependent variables, he says in 1953, are:

the "cause-and-effect relationships" in behavior -- are the laws of a science. A synthesis of these laws expressed in quantitative terms yields a comprehensive picture of the organism as a behaving system. (SHB, 35)
Verbal behavior is also amenable for a functional, analysis he thinks, for:

What happens when a man speaks or responds to speech is clearly a question about human behavior and hence a question to be answered with the concepts and techniques of psychology as an experimental science of behavior. (VB, 5; underlining added)

He defines verbal behavior as "behavior reinforced through the mediation of other persons" (WB, 2), and later adds that "the listener must be responding in ways which have been conditioned PRECISELY IN ORDER TO REINFORCE THE BEHAVIOR OF THE SPEAKER" (ibid, 225; emphasis in original). In this functional analysis the unit of verbal behavior is the dependent variable -- the verbal operant -- defined as responses "of identifiable form functionally related to one or more independent variables" (ibid, 20).

8.9. In a functional analysis the term word will not do -- though it is the one that comes closest -- for the unit of verbal behavior has to be defined both from the point of view of form and meaning. The term response will not do either, Skinner says, for that is usually identified by form alone (e.g. the rat pressed the lever with its paw at 9:00 a.m. yesterday, or the man said "goodbye" at 10:00 p.m.). In a functional analysis we need a concept that has the meaning of lever-pressing or saying goodbye, to capture behavior -- not so much as an instance, but as the type of behavior it is. (e.g. form and meaning joined together). The unit then, has to be a type via the operant, of which the response is but a token. This is the problem for the
functional analysis of verbal behavior.

Skinner has only one answer to this problem, and it depends, crucially, on the idea of extrapolating the basic system as reported in his first book:

The analysis of nonverbal behavior has clarified the nature of such a unit under laboratory conditions in which the expediency of the unit may be submitted to rigorous checks. An extrapolation of this concept to the verbal field is central to the analysis represented by the rest of this book. (VB, 20)

He further talks about a verbal repertoire, in the sense that responses of various forms appear in behavior from time to time. A repertoire, then, is a collection of verbal operants, and describes the potential behavior of a speaker.

Skinner says:

To ask where a verbal operant is when a response is not in the course of being emitted is like asking where one's knee-jerk is when the physician is not tapping the patellar tendon. A repertoire of verbal behavior is a convenient construct. The distinction between 'verbal operant' and 'word' is matched by that between 'verbal repertoire' and 'vocabulary'. (ibid, 21-22)

8.10. There is no point in going in more detail into Skinner's theory of verbal behavior, for the present point is only to show how this is an extension of his new unit of analysis -- the operant. But before I leave the discussion of verbal behavior, I should point out that Skinner has on many occasions, expressed his opinion that Verbal Behavior will "turn out to be [his most] important book" (Skinner, 1976b, 112). This is quite interesting because, of all the books Skinner has written, Verbal Behavior is probably the one least read. The reason he thinks the book so important
is that it is "the missing link between the animal research
and the human field" (Skinner, 1977, 280). In other words,
the book is important because it is to bridge the gap between
explaining relatively simple behavior and more complex
behavior.

This view is well confirmed by a careful look at the
best known criticism of Skinner's position; namely N. Chomsky's
review of Verbal Behavior (Chomsky, 1959; see also his review
of Beyond Freedom and Dignity (Chomsky, 1971), and "Psychology
and Ideology", 1972). In the paper "The Skinner/Chomsky
Debate: A Reinterpretation" (Gudmundsson, 1983) I have argued
that Chomsky's basic argument -- the dilemma argument -- has
been rather badly misunderstood. Chomsky's review has been
taken to mean many things, but I argue that it is basically
an argument against the extrapolation of Skinner's theory
as it is in The Behavior of Organisms (or Science and Human
Behavior -- as that is the book Chomsky makes more use of)
to Verbal Behavior. He sets up a dilemma such that either
we take Skinner's theory as it is in the first book (in which
case we have a restricted experimental system, says Chomsky),
or we extend the explanatory concepts (e.g. stimulus, response,
reinforcement, and operant) to 'real-life' (in which case
the concepts become much too broad, he claims). Chomsky
expresses this dilemma argument well in the last section of
his 1959 review of Verbal Behavior:

My purpose in discussing the concepts one by one
was to show that in each case, if we take [Skinner's]
terms in their literal meaning, the description
covers almost no aspect of verbal behavior, and if
we take them metaphorically, the description offers
no improvement over various traditional formulations. The terms borrowed from experimental psychology simply lose their objective meaning with this extension, and take over the full vagueness of ordinary language. (Chomsky, 1959, 54)

8.11. It is not the point here to either support this interpretation of Chomsky's criticism or to evaluate it. However, I take it to be significant that Chomsky does pinpoint his criticism on the extension of the operant to the verbal field. In essence his point is that we can take Skinner's terms narrowly or literally, in which case they are trivially true, but inapplicable outside experimental situations, or we can, after their extrapolation to the verbal field, understand them in a wider sense. The narrow interpretation is true of course, of Skinner's original concept of the reflex, but once he realized (as he was well on his way of doing in his first book) that most behavior is not elicited by stimuli in the immediate environment, then the reflex became a minor part of his theory. The behavior Skinner was interested in was not reflexive, but rather emitted (un-elicited) by the organism. Thus Skinner leaves open (unanswered) the question of the initial causes of operant behavior, but claims that once emitted, it can come under stimulus control via discriminative and reinforcing stimuli.

8.12. Skinner has on many occasions, insisted that operant behavior "is essentially the field of purpose" (Skinner, 1966, x; AB, 55). This is a bold statement, but unfortunately not explained. The answer however, involves the shift from the reflex to the operant, so let me explain.
As we saw in the amendment to the research program (see sections 1 and 2 of chapter two), there does not seem to be any one physical property characteristic of any two instances of an operant. (The same is not true of the reflex of course; the elicitation assumption guarantees that). We can see this from his discussion of a defining property. It appears, he says:

> on the side of the response in the first step toward what is called the discovery of a reflex. Some aspect of behavior is observed to occur repeatedly under general stimulation, and we assign a name to it which specifies (perhaps not explicitly) a defining property. . . . when a defining property has been decided upon, the stimuli which elicit responses possessing it are discovered by exploration. (Skinner, 1935a, 465-66)

The exploration here referred to is to vary the 'stimuli to determine which property of the stimulus is the reflex.

That defining property of the stimulus is "the part common to the different stimuli which are thus found to be effective" (ibid, 466). Once this property of the stimulus has been experimentally determined we have a functional relation (to be indicated by a 'smooth curve') as the correlated stimulus-response class. Our reflex then, is that functionally related stimulus class and response class. Clearly, Chomsky's criticism of the narrowly defined stimulus and response, is exactly to the point here, for neither the stimulus nor the response can be defined independently of the other.

However, once Skinner discovered the operant, he rejects, by the extrapolation of the concept to a larger field, the fruitfulness of the search for that defining property,
and claims that the only intrinsic property of the operant may be the fact that it achieves a certain result. The concept of the operant means action not movement. When a rat presses the lever its behavior is operant in the sense that it operates on the environment (i.e. achieves a certain result). What makes it an operant is not the topography of the movement, or its detailed physical description, but rather the fact that the action operates on the outside world. The operant then, is not characterizable formally or physically, for topography will not define it. It follows that no one physical property may define it, except the fact that the act achieves something -- the operant is purposive.

To illustrate what I mean, it may be helpful to distinguish between intrinsic and extrinsic properties. The former are properties the definition of which requires no reference to a result or effect. These may be either primary or secondary qualities (e.g. latency, frequency, or color). Extrinsic properties, on the other hand, are properties the members of a class have in virtue of the effect they produce (e.g. 'causing cancer'). The characteristic thing about extrinsic properties is that they are teleological or relational, for they involve both a member of the class and the effect produced.

It should be clear by now that an operant is just such a relational term. (The reflex, on the other hand, was to be characterized intrinsically). An operant is only characterizable by the effects it produces. Consider the operant most frequent in Skinner's early experimental work;
the pressing of the lever in a Skinner-box. One can describe the lever-pressing topographically by restricting it to a certain kind of movement. Thus we could say that only when the rat presses the lever with its snout, or paw, or whatever, would we have a case of an operant. But for Skinner's purposes, the only distinguishing property of the operant 'pressing-the-lever' is the effect it achieves. Thus it does not matter in what way the rat presses it, but only that it does operate upon the environment. Consider this statement by Skinner:

The number of distinguishable acts on the part of the rat that will give the required movement of the lever is indefinite and very large. They constitute a class, which is sufficiently well-defined by the phrase 'pressing-the-lever'. (BO, 37; underlining added)

That Skinner wants to define the operant extrinsically, is clear from the above quote. Later he says that the term operant:

emphasizes the fact that the behavior OPERATES upon the environment to generate consequences. The consequences define the properties with respect to which responses are called similar. (SHB, 65; emphasis in original, underlining added)

Any adequate formulation of the interaction between an organism and its environment, Skinner claims, must always specify:

(1) the occasion upon which a response occurs,
(2) the response itself, and (3) the reinforcing consequences. The interrelationships among them are the "contingencies of reinforcement". (COR, 7)

This is Skinner's final formulation, and we see clearly how far away it is from his original reflex, where behavior consisted in purely mechanistic S-R associations. Now the
theory is really dynamic or teleological, as it concerns (1) the discriminative stimuli in the immediate environment that set the occasion of the response (but do not elicit it); (2) the response itself; and (3) the reinforcing consequences. This is a three term relation, not so much concerned with the question of what elicits behavior, but rather with what can affect the probability of its future occurrence.

9. SUMMARY OF CHAPTER THREE

9.1. Chapter one was concerned with the very early scholarly influences on young B.F. Skinner. I argued that he became committed to the reflex tradition through his successful use of Sherrington and Pavlov as exemplars, understood in Kuhn's original sense of the paradigm. Skinner made an analogical extension of Sherrington's and Pavlov's experimental problem/solutions to the context of his own interest: the reflex behavior of the whole and intact organism.

After having found his individual reflex, first the 'eating' reflex and then later the lever-pressing reflex, and a way to measure it through the manipulation of elicitative stimuli, Skinner exclaimed that he had made contact with Pavlov and Sherrington at last.

I argued further that this committed Skinner to the research tradition of reflex physiology, and that by 1931, when he graduated from Harvard, he not only had a prospect of an experimental science of behavior, but a sketch of a research program as well.
9.2. In chapter two Skinner's attempt to instantiate the reflex tradition by way of his individual research program is examined, and I argue that some relatively minor changes in definition, apparatus, and measurement, have the cumulative effect of moving him away from that tradition. Skinner did not immediately realize that fact however, but his experimental work from 1932-34 shows, I further argue, that he was having increasing difficulties with the fulfillment of his research program. At first I express his difficulties as mere worries, as Skinner shows a curious lack of sensitivity towards the severity of his experimental problems. He seems to have thought, at that time, that he could easily explain his problems away. His problems could not be answered in such a manner however, and I argue furthermore, that Skinner's discoveries of one-trial conditioning (as a new type of conditioning) and of extinction (as the opposite process to conditioning) served to increase further the severity of his problems. I emphasize two kinds of problems, the definition problem and the measurement problem. These problems are, to begin with, of the empirical kind (i.e. of the fact-gathering kind articulated by Kuhn's three types of scientific problems), but the main thrust of chapter two is to explain the generation of non-empirical problems; what Laudan calls internal and external conceptual problems.

9.3. In 1935 Skinner wrote two papers that separately function as his first attempt at the solution of his two problems. I suggest five main reasons for this, the most
important of which is the fact that Skinner came to realize that these two kinds of problems were uniquely his own, in need of his own solution. Neither did the reflex physiologists at the Medical School measure the reflex in the way he did, nor did his fellow workers in animal learning show more than an "interest" in Skinner's work.

In the former of these two papers Skinner makes an important change in his research program, as he attacks the problem of identifying individual reflexes by insisting that the elicitation of responses by stimuli was not a defining property. This is of considerable importance as Skinner thereby abandoned the search for individual reflexes and emphasized the study of the typical reflex instead.

This is Skinner's first attack at the definition problem, for though he does not actually deny the elicitation, he nevertheless opens up that possibility by denying it a defining status.

In his second paper on conditioning Skinner attacks the measurement problem of one-trial conditioning, by clarifying the distinction between the two types, emphasizing all the differences he can find. He messes things up however, due to the fact that he does not see any relation between his two problems. Thus he emphasizes differences in responses in distinguishing between the two types, while ignoring the role of discriminative stimuli. He has to say something about those however, and suggests that the relation between the discriminative stimulus of light to
the response of pressing the lever is a "pseudo-reflex". But that does not so much solve his problem, as to give it a name, for the obvious question is what these "pseudo-reflexes" are. This is not really a reflex he says, but neither does he explain what it is then, nor explain how these discriminative stimuli affect responses, if they do not actually elicit them, like they do in proper reflexes.

9.4. We see that Skinner's two problems are still with him after he has attempted their solution. He does not so much solve them as change them, for the same problems still bother Skinner, albeit transformed. The pseudo-reflex functions now both as a question concerning discriminative stimuli (in relation to ordinary elicitive stimuli), as well as questioning his measure of reflex strength (or response rate) for the problem is still how these discriminative stimuli do affect the response.

9.5. During most of 1935-36 Skinner does more work on his two problems, as he makes further inquiries into both, the nature of discriminative stimuli, and the resistance to extinction measure. On the one hand Skinner is becoming preoccupied with the role of the stimulus in the process of conditioning (the definition problem of the elicitation assumption), and on the other hand, he investigates how to determine the proper amount of conditioning (the measurement problem of one-trial conditioning).

Theré are however two pieces of development that do not seem to fit this simple picture of Skinner's work in
1935-36. One is Skinner's direct test of Pavlov's disinhibition effect, and the other is the work on the verbal summator.

9.6. I argue that the former research is an example of Kuhn's interesting predictor problem, and that it is of major importance for the understanding of Skinner's move away from the reflex tradition. He does not find any disinhibition effect in his extinction curve. Pavlov had said that the introduction of any extraneous stimulus during extinction would disturb the curve significantly, but Skinner finds no such thing.

A positive result would have forced Skinner, I argue, to look further into Pavlov's theory of inhibition and facilitation, but as the result was negative, Skinner used the opportunity to argue against the introduction of Pavlov's terminology in to the system he was developing.

9.7. I say "system" here, but the other exception to the simple picture of Skinner's experimental work during the years 1935-36, is the first evidence of the emergence of a new theory -- operant theory in embryo. The paper is: "The Verbal Summator and a Method for the Study of Latent Speech". The verbal summator is a phonographic device for the study of preverbal (sub-vocal or latent) behavior, and though this seems, at first, to be in no way related to any of his other experimental work, I argue that there is in fact a clear relation.

The first way to understand that relation is by
emphasizing the fact that this research is based on the joint use of Pavlov's imitative reflex and Sherrington's principle of summation. Skinner is thus making use of their experimental problem/solutions, in the sense of the exemplar, just like before, by extending their solutions to a new (but analogous) field -- verbal behavior. This research is therefore no different from the rest of Skinner's experimental work, which is a conclusion that is of quite some relevance to Kuhn's idea of the crisis-causing anomaly.

But there is another way to understand Skinner's research into the verbal summator, and it makes the point even more clear that this is just normal science for Skinner. The verbal summator, by the very nature of the thing, is a device for the study of (verbal) responses that are not due to elicitative stimuli. In fact these responses are not elicited by stimuli in the immediate environment says Skinner, and he proposes furthermore, to estimate the strength of such non-elicited responses by a measure of their relative frequency, with his measure of response rate during extinction.

This is a solution to both of Skinner's main problems, for in one move he solves the elicitation assumption by denying it, and the measurement problem by the use of the response rate measure on such non-elicited responses.

9.8. On the basis of this joint solution I argue that Skinner has the beginnings of a new theory, or operant theory in embryo. It is not the mere fact that Skinner
solves these problems that constitutes a new theory, but much rather that fact plus his extension of that solution to the verbal field. This shows both how important verbal behavior is to Skinner's theory, which is a fact both relevant (e.g. Chomsky's criticisms) and ignored, and it shows also, I think, how inadequate Laudan's claim that individual theories are solved problems really is.

I have before (i.e. chapter one) used Laudan's insightful distinction between the more global theories in terms of research traditions and individual theories as answers to specific scientific problems. There are two points of relevance here. First is the fact that the identification and distinction between individual scientific problems is not at all as easy a matter as Laudan makes it out to be. As we have seen, especially in chapter two, the structure of Skinner's empirical and conceptual problems is quite complex, and I have emphasized two kinds (definition and measurement) of problems that run through the experimental stage from 1928 to 34. These problems function not so much as questions of fact deserving an answer in terms of a theory, but much rather as the units of analysis for the working scientist. They are eventually solved (sometimes), but their fruitfulness depends, I suggest, more on the ability of the problem to change -- to transform -- and thus direct research.

I argue furthermore that it is not the mere solution of a problem that constitutes a theory, but much rather
the extension of that solution to other areas. Laudan's emphasis on the solution of problems is thus a starting-point for a theory, but not every such answer is a theory.

9.9. The basic conclusion of this thesis concerning the two models of scientific change examined, is that while the emphasis on problem-context is seen to be a fruitful way to approach a case study like this one, both models are seen to be wanting in crucial respects. Kuhn's exemplar plays a major role in the explanation of how the young scientist comes to learn his trade, and how he comes to be committed to the research tradition in question. The research tradition is not the same as Kuhn's paradigm in the sense of the disciplinary matrix however, for a number of distinct and even rival theories were seen to be within the research tradition of reflex physiology (e.g. Magnus, Sherrington, Pavlov, and later, Konorski).

Thus while Kuhn's insights concerning both the exemplar and the three types of scientific problems prove to be of value in this case study, the same can not be said for his emphasis on dogma, (or the general adherence to a single theory during normal science) and in general on his emphasis on the monotheoretic character of normal science.

Here Laudan's insight concerning the distinction between global non-explanatory theories that provide ontological and methodological constraints to the committed scientist, and individual theories, is of more value. This model is especially valuable when we are concerned with the causes
of the emergence of novel theories, for while Kuhn's thesis of a crisis is seen to be both unclear and unexplanatory, Laudan's interplay of individual theories allows him an effective way to both, define anomalies, and to explain how a crisis occurs in science. As this case study conclusively shows, operant theory did not emerge as a response to a growing crisis, but was rather a natural development within normal science, as the work on the verbal summator was seen to be a direct continuation of Skinner's earlier experimental work.

9.10. Once Skinner had developed the beginnings of an operant theory in one context, he could readily apply that solution to conditioning. After Konorski and Miller criticized him on those grounds. What the debate indicated furthermore, was that it was only after Skinner had revealed his solution to them, that we can discern a crisis in the field. Konorski and Skinner were compared and both were seen to be preoccupied with the same unsolved problem (the new type of conditioning). They went in completely separate ways however, and the point was that although Konorski and Miller clearly discovered operant behavior prior to Skinner, they never saw the problem as an anomaly, until Skinner, due to his solving the problem in another context, tells them that there is this operant, meaning non-elicited behavior.

Consequently Konorski and Miller never saw the new type of conditioning as indicating non-elicitative stimuli,
and when Skinner suggested such a thing, they rejected that solution out of hand. Neither was the fact that conditioning could take place in one trial a counterinstance to Pavlov's theory or to the tradition, so we must conclude that operant theory was not the result of a crisis, but rather the other way around.

The crisis then, is generated by Skinner's solution, for it is a rejection of the fundamental assumption of the reflex tradition, that the reflex is the necessary correlation of a stimulus and a response. Skinner solved the previously unsolved problem of the new type of conditioning, and thereby turned that problem into an anomaly for all other theories within the tradition, that had not solved that same problem.

And that does not support Kuhn's idea of a crisis-causing anomaly as a prerequisite to the emergence of novel theories and revolutions, but rather Laudan's analysis of the unsolved problem turned anomalous through the emergence of a new theory that provides a solution to that problem.

The essential problem for Kuhn is that given his emphasis on a strict adherence to a single theory during normal science, he does not have the conceptual apparatus to explain the transformation from normal science to revolutionary. Indeed, it is not clear at all how normal science can ever turn revolutionary on his account.

9.11. An important argument in chapter three concerns the relation between internal and external history of science. Once it became evident that Skinner was moving away from
the reflex tradition, the external question of the social motive behind that move became pertinent. I do argue that Skinner clearly had such a motive, and a pretty strong one at that, but my point is simply that the external question is raised because of or on the basis of, that piece of internal history. That is, one must first determine that such was the case, and it is only once enough of that historical puzzle is put into place, that the external or social questions can even be raised. "Pure" internal history of science may be blind (to steal a used metaphor), but without it, external history of science is empty.

This argument could be extended to the relation between the history and philosophy of science, for there does not seem to be much point in drawing morals of philosophical relevance from the structure of historical science. The point is the same one as before, and is made further relevant by the examination of a period in the history of science mostly unexplored like this one, for it is imperative that we know of that piece of history before we can raise questions of a more philosophical relevance. After all this is not a case study in the philosophy and history of science, but the other way around.
REFERENCES

1 The mere collection of reflexes (the botanizing of reflexes), says Skinner in the letter to Boring, "is to me futile and uninteresting" (SB, 146).

2 The reader has already been warned about this confusion in terminology in section 2 of chapter two. As was pointed out there, Skinner does not clear up this confusion until his answer to Konorski and Miller.

3 I owe this location to Thomas Nickles. See his "Scientific Problems: Three Empiricist Models" (1980), for an interesting discussion of the problem-problem.

4 The complexity involved is, as we have seen, the use of a paradigm (in the exemplar sense) in the reflex tradition to develop an independent science of behavior; the attempt to put that science on an experimental basis; the empirical and conceptual problems that emerged as a result; the move back to reflex physiology proper for help in dealing with these problems; the failure of that attempt; plus the realization that no one understands his problems; and finally Skinner's attempt to solve these problems -- which lead, interestingly enough to new problems.

5 Skinner's first paper on extinction was published in 1933, but the results were undoubtedly ready some time before that.

6 That apparently, is the worst thing you can do to a rat, or so I have been told.

7 It must be realized that much of the force of the interest in Skinner's system would have evaporated rather quickly if Hull's prediction of disinhibition would have come through. Skinner would then have been forced to look more carefully at Pavlov's theory (as I argued that introducing one term -- disinhibition -- would have led to others), and though he would undoubtedly have made some original contributions, it would then have been to that theory, but not to his own and independent science of behavior. He would, in fact, have become one of Pavlov's students. Later in this chapter we will see two such students, Konorski and Miller, and how Skinner (on the basis of this result, no doubt) argued that his own theory is not like their version of Pavlov's theory.

8 W.J. Crozier is the only one that Skinner had any substantial contact with throughout his early experimental work. They had jointly published the review of Fearing's book, that had done so much for Skinner's early prospect of a science. Crozier was the one who had encouraged Skinner to write his earliest experimental results up in a paper, and helped him publish. Crozier in fact, was the one who accepted them for publication. Although clearly influenced by Crozier's methodology of functional analysis, and his emphasis on the behavior of the whole and intact organism, Skinner never accepted his fundamental unit of analysis -- tropism -- and went instead for the reflex. The corroboration with Crozier seems to have come to an abrupt end however, around 1934-35 as will be discussed in a moment.

9 The fact (if it is a fact) that Skinner was surprised that the physiologists were not interested in the problems he was, may be explained away, for example by insisting that Skinner just wants to give us this impression in his autobiography, as he may (e.g.) find it embarassing to be moved by purely external factors. Then again, he may simply be wrong here, as he may just be rationalizing to himself why he actually made that move.
REFERENCES

10 P. 55 in E.G. Boring's Psychologist at Large: An Autobiography and Selected Essays (1961). This is an expanded version of (Boring, 1952). He further notes that Conant "thought that the day of the physical sciences had passed and that the day of psychology was dawning" (Boring, 1961, 55). While the former part of this prediction turned out to be wrong, Conant made every effort to further the latter part.

11 Elsewhere Skinner explains that Conant was not pleased with the psychology department as it was, so in 1934 (he became president in 1933) Conant decreed that all temporary positions should be terminated and search made for "the best psychologist in the world" (SB, 155) -- and found Karl Lashley.

12 See all of Skinner's papers (first page) that appeared in the Journal of General Psychology from 1930 to 1933, all in 10 experimental papers and 2 book reviews. In 1934 Skinner published no experimental papers in that journal. Proceedings of the National Academy of Sciences (see BIBLIOGRAPHY), and in Atlantic Monthly. In 1935 however, Skinner again publishes in the Journal of General Psychology, but this time the papers were accepted for publication by Carl Murchison.

13 There is an interesting discussion of Clark Hull's contribution "Conditioned Reflexes" and of Hunter's "Experimental Studies in Learning". Skinner criticizes the former's analysis of Pavlov's idea of inhibition and Hunter's habit as a basic unit of analysis. Neither point is surprising, given Skinner's view however, and Pavlov's "not unduly worshipped" (243) Skinner is happy to add. Pavlov's "results are not always useful in a simple formulation of behavior" (ibid). Skinner predictably points out, and although he must still share Hull's view that all the material on learning is reducible to Pavlov's conditioning, Hunter's selection of a unit, the concept of habit, "is not an ultimate analytical unit" (ibid).

14 Skinner here tells the story of Murchison being pressured to publish some work of his own, as well as so much of the work of others. "He cooked up an experiment" says Skinner -- not without some irony -- on "social relations": Two fighting cocks or roosters were released in such a way that they ran aggressively toward each other. The weight of each bird multiplied by its speed gave, said Murchison, a measure of its aggression in centimeters per gram per second." (SB, 164)

15 I was led to this insight by Kuhn himself, for in an answer to criticisms by J. Hattiangadi at the conference on Language and Learning here at The University of Western Ontario, London, Ontario, May 1983, Kuhn replied that he was one who introduced the term incommensurability in the first place. He said that it derived from mathematics, where it meant the impossibility of measuring two things at the same time.

16 See, for instance, Skinner's "The Distribution of Associated Words" (1937b), a further paper on the same issue coauthored with S.W. Cook (Cook and Skinner, 1939), and "The Alliteration in Shakespeare's Sonnets: A Study in Literary Behavior" (Skinner, 1939), to name but a few early ones.
17. According to Miss Stein, the fact of spontaneous writing was evidence for the elements of a second personality in a normal person, and she claimed it could be found in anyone. One way to produce such a split personality was by automatic writing, which can occur when one attempts to write something down while actively attending to something else (like singing, reading, etc.). The effect was typically a string of words, grammatically correct, while devoid of any clear meaning. Stein was reported to be able to produce such writing herself quite easily, after some practice, by reading her automatic writing as it occurred, but following a few lines behind.

18 Gertrude Stein, a noted literary figure by then, had earlier studied psychology at Harvard. Her work with Solomons on automatic writing was done under the supervision of Münsterberg, says Skinner in his autobiography (SB, 134), although other sources say it was William James.

19 The editor of Atlantic Monthly said Skinner's paper was a "small classic of the dissecting-table" (SB, 135). Similar views were expressed in the New Republic, American Spectator, Boston Sunday Herald, New York Herald Tribune, and even in an editorial for the New York Times.

20 Skinner explains how he came to the idea of the verbal summator in his autobiography. One fine Sunday morning, he says, he was working in his laboratory -- which was, as we remember, soundproof and two floors underground. After connecting a Skinner-box for an experiment on conditioning, he noticed that the apparatus emitted:

   a rhythmic pulse: di-DAH-di-di-DAH -- di-DAH-di-di-DAH. Suddenly I heard myself saying "You'll NEVER get OUT. You'll NEVER get OUT". Evidently the rhythmic stimulus had had that effect Sherrington called summation. An imitative response had joined forces with some latent behavior, which I could attribute to a rather obvious source: I was a prisoner in my laboratory on a lovely day (SB, 174; emphasis in original).

21 Summation for Sherrington, is the process by which weak -- subliminal stimuli, i.e. each too weak to actually cause a response -- can by "each succeeding the other within a certain time -- summation time -- sum as stimuli and provoke a reflex" (p. 36) of his Integrative Action of the Nervous System (Sherrington, 1906). An example is the scratch-reflex, as already explained in section 4.4. of chapter one.

22 Skinner himself later translated that paper as "On a Particular Form of Conditioned Reflex" (Miller and Konorski, 1969). That translation is interesting mainly for the reason that in the translator's opinion, expressed in an introductory note to the paper, this is an "important paper" (Skinner, 1968, 187). Konorski supplies a postscript however, where he denounces that "every single theory of the above paper is more or less erroneous" (Konorski, 1969, 189). I will return to this in a moment.

23 Stefan Miller only stayed in Leningrad for a few months, after which time he returned to his family in Poland (see Konorski, 1974, 191)
REFERENCES

24 The original studies in Poland had been "somewhat amateurish" (Konorski, 1974, 195), but in Leningrad Pavlov was in charge of two major experimental centers, occupied by about 80 scientific personnel. Konorski got one sound-proof chamber and five dogs, previously used for other conditioning experiments. That might be regarded as a confounding variable, but Konorski and Miller "received their entire biographies, their age, the dates of their coming to the laboratory, the whole story of their conditioned reflex careers, and the list of all positive and negative conditioned stimuli used in their training" (ibid, 194).

25 Konorski summarizes these results as follows: What were the main achievements during my stay in Leningrad? First, confirmation under more rigorous experimental conditions of the results we had found in Warsaw, to the effect that positive type I (classical) alimentary conditioned stimuli completely inhibit type II response, whereas negative type II conditioned stimuli may even increase this response. Second, we performed important experiments in which a given stimulus was reinforced by food, but this stimulus accompanied by passive flexion of the leg was not. As a result, the dog learned to extend his leg in response to the conditioned stimulus, in this way resisting passive flexion. Third, by applying as an unconditioned stimulus introduction of acid into the dog's mouth, we established avoidance conditioned reflexes and could study the relations between motor and salivary responses in these rather unusual conditions. All these results were presented in an extensive paper published in Transactions of Pavlov's Laboratories (Konorski and Miller, 1936), and they laid the foundations for the further development of my ideas concerning the mechanisms of type II conditioning. (Konorski, 1974, 195)

26 Pavlov's record holder for a reflex to become conditioned upon a new stimulus (tone, bell, light, etc.), was variously said to have been 5 to 7 trials, which meant that he had to present both stimuli about 5 to 7 times before the new and conditioned stimulus could alone elicit the response, and thereby create a new and conditioned reflex. But as we will see in a moment that is not true.

27 Due to the fact that Konorski and Miller had sent him a reprint earlier -- along with their monograph in Polish (Konorski and Miller, 1933), "in which they had entered marginal notes in French explaining the graphs and tables" (SB, 183).

28 As a result of a chance visit to the lab he realized how he "had moved away from a reflex formulation", and also, he adds, from Konorski and Miller (Skinner, 1977). I will get to that latter difference in a moment.

29 Indeed the fact that Skinner does this translation is quite ironic as he is thereby admitting to the historical importance of the discovery of non-elicited behavior (as well as admitting to Konorski and Miller's priority to that discovery), while Konorski, in the postscript, in effect denies any relevance to that discovery.

30 The most probable reason Skinner got that job at Minnesota was the fact that Walter Hunter was teaching summer school there during that time. Hunter must have mentioned Skinner to the department, and once negotiations started Skinner asked Carmichael to write in his behalf.
REFERENCES

Skinner told Boring that fact, and he also wrote a very strong letter in his support. Only later did Skinner know of that letter and the fact that it was in fact Boring's letter that "turned the trick" (see SB, 186-87).

31 Skinner has later repeatedly explained this by such statements as:
   In my reply [to Konorski and Miller (Skinner 1937a)]
   I used the term "operant" for the first time and
   applied "respondent" to the Pavlovian case. It would
   have been the right time to abandon "reflex", but
   I was still strongly under the control of Sherrington,
   Magnus, and Pavlov ... (Skinner, 1976b; underlining
   added).
   It is really incredible (and rather boring) to see how
   consistent Skinner is, for even in these autobiographical
   explanations, he is able to regard himself as just another
   organism.

32 Skinner explains this in the PREFACE to Verbal
   Behavior, where he says that the completion of the final
   manuscript "was postponed in favor of a general book on
   human behavior (Science and Human Behavior) which would
   provide a ready reference on matters not essentially verbal"
   (VB, p. vii). We know further that "several hundred mimeo-
   graphed copies" of the William James lecture at Harvard
   were circulated in 1947 (ibid), and that by 1942 he refers
   to VB as a manuscript (See SB, 252).

33 Chomsky's argument is much more complex than this
   of course. He carefully compares Skinner's key explanatory
   concepts, stimulus, response, reinforcement, and operant,
   one by one, emphasizing how they are defined -- first in
   The Behavior of Organisms -- and then in Verbal Behavior.
   Chomsky accepts the definitions in the first book (and
   cannot therefore be said to refute Skinner's basic theory,
   as many have claimed, but questions their extrapolation
   to the verbal field.

34 I owe this distinction to R.F. Kitchener. See
   his "Behavior and Behaviorism" (1977), 57.


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