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The roots of music therapy: Towards an indigenous research paradigm. (Volumes I and II)

Aigen, Kenneth, D.A.

New York University, 1991
THE ROOTS OF MUSIC THERAPY: TOWARDS AN INDIGENOUS RESEARCH PARADIGM

Kenneth Aigen

Submitted in partial fulfillment of the requirements for the degree of Doctor of Arts in the School of Education, Health, Nursing, and Arts Professions New York University 1991
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ACKNOWLEDGMENTS

There are individuals and institutions who have contributed to the existence of this work on a variety of levels and have become part of its fabric. I would like to take this opportunity to recognize those whose help was quite direct as well as those whose participation was less overt.

I would like to express my debt to those research theorists in Music Therapy who have come before me and to whose efforts I apply a fair amount of criticism. Though I disagree with many of your ideas, I appreciate the opportunity to sharpen my thought by applying it to your work and arguments. This work would not have been possible without yours.

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The Music Therapy community of New York University—comprising those who have been my teachers, students, colleagues and friends—has proved to be an invaluable support system in the almost ten years that I have been fortunate enough to be associated with it. The unifying thread of this study—that research in Music Therapy must focus on and be answerable to the clinical needs of the field—is a direct result of being a member of this academic, social and spiritual circle. If there is a guiding philosophy of the program and its head, Barbara Hesser, it is that academic training should be guided by the needs of the working clinician. My choice to extend this value into the sphere of research is a direct result of having developed as a person and professional within this community.

In this regard I would like to acknowledge the actual research already being done through the NYU doctoral program by such inspired and dedicated clinicians as Michele Forinash, Rosanne Kasayka and Dorit Amir. These individuals are already engaged in the research for which this study is intended to provide conceptual support and I encourage readers of this study to seek out their work.

Michele Forinash and Dean Jerrold Ross provided some last-minute editorial assistance. Thanks for
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Benedikte Barth Scheiby was instrumental in bringing me to Denmark; read and commented on many early drafts of this study; has served as a sounding board for many of my ideas, inspirations and frustrations; and has brought a jewel named Sara into our lives. Jeg elsker dig min kærste.

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PREFACE

There are a few technical remarks on the structure and content of this study that I would like to make at this point to avoid any confusion or misunderstandings.

The first relates to my use of capital letters for the profession of Music Therapy. In this regard I follow the precedent set by Carolyn Kenny.¹ My purpose here is to distinguish the profession of Music Therapy from other uses of music, such as in medicine, personal growth, new-age type healing, etc. Any use of music directed to either healing, rehabilitation, therapy or personal growth does not merely by the intent of the practitioner qualify as Music Therapy. My use of capital letters is intended to reinforce the belief that the profession of Music Therapy—involving specialized training, credentialling procedures and professional standards—is an autonomous, developed discipline.

Second, though I make ample use of historical material in this study I cannot emphasize strongly

enough that this study is not intended as history. The historical material is directed towards distinguishing what I call traditional research in Music Therapy from—for lack of a better term—new research.

In fact, these two approaches have a closer relationship than will become apparent purely from reading this study. The new research has evolved from the traditional research and it is this process which I do not examine. Such an examination would demand its own study in order to be done correctly and thoroughly. My concern throughout has been to draw upon enough historical material to 1) support my contention that there are these two opposing trends, 2) demonstrate the fundamental differences between them, and 3) construct an argument that will demonstrate the need for the new research.

Although this observation will also not become clear solely from reading this study, the theorists and clinicians identified with the new research are part of a loosely defined movement associated with New York University and the Phoenicia Group. The historical roots of the new research can be traced back—in English language publications—at least as far as a 1982 International Symposium at New York University

\[2\] See the Fall, 1989 issue of Tuning In, the newsletter of the American Association for Music Therapy for information on the Phoenicia Group and Music Therapy.
entitled "Music in the Life of Man: Toward a Theory of Music Therapy," and all the clinicians cited as belonging to this movement are either faculty members at New York University (Michele Forinash, the present author), members of the Phoenicia Group (Carolyn Kenny, Helen Bonny, Even Ruud), or affiliated with both NYU and the Phoenicia Group (Barbara Hesser, David Gonzalez, Dorit Amir).
CHAPTER I
INTRODUCTION

Any work of this type has two, very different sources. The first lies in the history and professional and personal development of the author; the work can be understood in terms of its place in this personal, evolutionary process. The second source is the need in the discipline that such a study is created to meet.

I would like to discuss both of these sources, not just to make clear my own process in coming to the point of view espoused here, but to provide the reader with a greater understanding of my perspective and conclusions. As I note in the subsequent discussion of early Music Therapy research theorists, it can be difficult to distinguish among the psychological, sociological and conceptual factors in determining why a particular individual argues for a particular point of view. In evaluating this study, the reader has the right to know both the inner and outer influences that have determined its content, as far as I can make them clear. I present this inner history and its connection to the research problem now; that the truth of this inner history is generalizable to the discipline at
large is documented in the section on external corroborations.

The Inner Source of this Study

Prior to my being trained as a Music Therapist, my primary academic pursuits included the history of science, philosophy, and psychology. In the latter two subjects I was most interested in the philosophy of science and the psychology of perception. Finding that my interests overlapped traditional academic boundaries, I pursued majors in both philosophy and psychology in undergraduate school. Although they were in different academic departments, I found that theories of perception had much in common with philosophies of science, on both structural and functional levels: both types of theories are attempts to account for how human beings organize sensory and perceptual phenomena into a coherent view of the external world.

It was this process of constructing a world that intrigued me. Whether this process was approached through questions like how two-dimensional retinal impressions led to a three-dimensional experience of space, or how observations of stellar and planetary movement led to physical models of the universe, was of secondary concern.
It was natural that I was attracted to those philosophers of science--such as Thomas Kuhn--who, in their examination of the role of theories in science, came to the conclusion that scientists' prior beliefs strongly influenced their observations and data-gathering activities. This view mirrored that of perceptual theorists who argued for just such a cognitive contribution to perceptual processes. In both arenas, I found myself arguing against the view that we construct a worldview from neutral or pure sensory impressions. In philosophy, this led me to believe that there was no such thing as neutral, purely objective, observation; in psychology, this led me to support the views of psychologists--such as Ulric Neisser--who did not see the individual as a passive recipient of perceptual information, but rather as an information gatherer.¹

My undergraduate training led me, as a Music Therapy student, to seek external systems that could provide a justification for Music Therapy treatment as well as a conceptual matrix and language in which to understand and describe clinical experience. These efforts seemed natural and necessary to my professional development. I soon adopted a Jungian frame of reference, recognizing a system where the desire for creative understanding and expression was seen as a positive human need, rather than as something to be
explained away solely through mechanisms of regression and sublimation. A master's thesis from a Jungian perspective ensued in which I investigated the use of music in ritual and its relevance for Music Therapy. In the following years, I would occasionally present Jungian material and argue for this viewpoint, without a deep sense of commitment to it.

The recognition that Music Therapists were formulating treatment rationales and methods from a variety of psychotherapeutic sources and personality models humbled me. After a time, I realized that the Jungian perspective was only one among many, and there was no reason to assume that it could make any claim as the "correct" one for Music Therapy. The more these various formulations proliferated, the more "mine" assumed a relative character. If all of these theorists could provide conceptual tools in understanding Music Therapy process, I concluded that the choice of which one to use will undoubtedly reflect the personal preferences of the choice-maker more than the actual phenomena revealed in Music Therapy. I was left feeling conceptually adrift.

Though I found myself moving away from existing clinical Music Therapy theory, my clinical skills and style nonetheless continued to develop. Crucial to this development was my piano training with Karen Nisenson, an adjunct faculty member of New York
University's Music Therapy program. The profound and immediate effects of this work had two important implications for me: It shattered some of the dichotomies regarding music that I had been working under. I came to experience that on a fundamental level the distinctions drawn between atonal and tonal music, and between improvised and composed music, no longer applied. I observed that the processes involved in learning to use free-form improvisation as a clinical tool were identical—in all of the important aspects—to those involved in learning to play, and bring to life, Bach inventions.

Second, I learned that the more that I trusted my own creative impulse, i.e., the more that I allowed clinical music to find its own form independent of prior clinical theory or therapeutic intent, the more clinically potent my use of music became. Though I had not yet formulated my viewpoint explicitly, in retrospect I see that I had begun to realize that the non-clinically oriented creation of music had much more in common with clinical uses of music than I had previously realized. Any clinical theory that did not address the role of creativity would not adequately account for my experience in the clinic.

As my clinical experience developed in a direction that I did not see addressed in the Music Therapy literature, I began to lose interest in both
professional journals and conference presentations. This was particularly disturbing as I could not understand how someone with a relatively brief professional tenure could become so easily disillusioned with the conceptual and theoretical works of his colleagues. Gradually, I realized that it was neither the relative nature of each practitioner's conceptualizations nor the fact that each viewed Music Therapy process from their own subjective "glasses" that led to my lack of interest. Rather, it was their presentation of Music Therapy process through non-musical perspectives that avoided discussion of the role of creative processes which disturbed me. With this realization, the focus of the present study became clear.

The Nature of the Present Study

What I feel is inarguable is that in the field of Music Therapy there is a dearth of research efforts directed to illuminating the underlying processes of clinical practice based on creative and improvisational methods. Existing research and theorizing has not proceeded from an effort to discover and explore the unique aspects of this work, but instead has reflected an attempt to explain Music Therapy process solely through methods and constructs derived from other forms of psychological inquiry. Consequently, the salient
aspects of creative clinical practice—including the nature of the clinician's intuitive and creative interventions, the personal meaning of the client's musical expression, and the manner in which these elements interact within the therapeutic relationship—are either overlooked or subordinated to other concepts, such as those of psychoanalytic or behavioral theory.

Certain therapists have articulated a viewpoint in reaction to this situation with which the present writer finds great sympathy: namely, because neither behavioral analysis nor various psychodynamic therapies—the two predominant ways of describing clinical process—capture the essence of clinical experience, what is needed to conduct meaningful research in Music Therapy is a theoretical and methodological perspective derived from this experience. Hence, my purpose in this study is to contribute to the construction of a clinically relevant, indigenous research paradigm. To accomplish this goal I hope to both justify and provide support for a shift in descriptive and explanatory mechanisms, as well as to set forth the characteristics requisite to indigenous research methods for Music Therapy.

Although it is being stated that current research in Music Therapy has limited applicability to creative practice, in and of itself this is not reason enough to
fundamentally change a research program. The
deficiency could be due to either poor quality research
or an inability of clinicians and researchers to draw
the connections between research and practice. What is
needed, and what I propose in this study, is to
identify the locus of effect of Music Therapy practice
and thereby argue for what the true data of the
discipline are. As the explanatory devices of the
current paradigms will be shown to be inadequate for
the nature of this data, support will be offered for a
research program directed towards an analysis of the
creative experience at the core of process in Music
Therapy.

Fundamental to this study is the belief that only
a dramatic shift in the focus of traditional research
will establish a clinically relevant, progressive
research tradition for Music Therapy. Such a shift
will require new conceptualizations of the nature of
science, research and Music Therapy practice. In order
to create an alternative, though, it is first necessary
to understand the conceptual bases of traditional
research. In Chapter III I identify the theoretical
and methodological constraints, as well as the
pragmatic professional considerations, that have guided
traditional research in Music Therapy.

The prime deficiency of this traditional research
in Music Therapy is its lack of clinical relevance.
Current models of Music Therapy practice--and orientations towards Music Therapy research--reflect the use of imported psychological and philosophical conceptions. The only way to establish a clinically relevant, progressive research model is to base it on an indigenous portrait of creative clinical practice. Since such a perspective of clinical practice is currently unavailable, in Chapter V I present elements of creative Music Therapy practice from an indigenous perspective and identify the metatheoretical implications of these elements for research.

Since the goal of this study is not to provide a purely philosophical criticism of the existent paradigm based upon its conceptual foundations, this portrait of clinical practice will serve as the real-life measure against which any adequate research paradigm should be evaluated. With this portrait in hand, in Chapter VII I will critically analyze the appropriateness of the traditional research paradigm in investigating creative Music Therapy practice, when viewed from an indigenous perspective.

It is certainly easier to criticize and tear down an existent research rationale than it is to build a new one. Yet the critique of the existent paradigm is worse than meaningless unless some alternative is suggested. Since my goal is not merely to criticize but to help build a new research program, in Chapter
VIII I conclude this study by providing a conceptual framework for studying aspects of Music Therapy process that have traditionally been ignored in the research literature.

The primary reason for this study, then, is to provide a broad-based theoretical foundation for designing research methods and building explanatory models concerned with illuminating creative processes in Music Therapy. From the principles thereby presented, it is hoped that subsequent researchers will be able to derive suitable methods for a particular area of study, whether this is a specific clinical population, Music Therapy technique or level of analysis. It is also possible that researchers wishing to employ aspects of the current models will be able to do so with a better understanding of what is needed to complement these models to enhance their explanatory power.

That there is a gulf or schism in Music Therapy among the components of practice, theory and research is subsequently documented in this paper. This gulf cannot be bridged until its underlying causes are made explicit and alternatives are suggested. In constructing a research paradigm derived from clinical practice, it is hoped that the natural, organic relationship among the three mutually supporting elements of the profession of Music Therapy--treatment,
training and research--can be discovered and articulated. Although the present effort is focused on the research component, its successful completion will have implications for theory, practice and training.

Research and clinical training exist to serve practice; research in Music Therapy acquires meaning and significance when its results can be applied to clinical work. If the present study can serve as a conceptual foundation for mapping out and providing a language for uncharted--but not untravelled--clinical terrain, training will be enhanced, which will in turn improve the quality of service that Music Therapists provide.

The activity of scientists continuously involves the discovery and creation of novel concepts and categories of experience. New words--such as evolution, relativity, the unconscious--are created to express what may have been previously considered to be inexpressible or paradoxical. It is hoped that the current effort will eliminate the conceptual barriers to investigating the most meaningful, if seemingly ineffable, moments in Music Therapy process, as well as facilitate the development of a language for this process, thereby removing these elements from the realm of the unknowable and the unknown, respectively. This is the only way the essence, power and uniqueness of this work will be preserved and built upon.
External Corroboration

At this point, the reader is entitled to wonder to what extent this author's impressions and attitudes towards traditional Music Therapy research reflect those of the profession at large. This question brings us to the second source of the study mentioned previously. Having provided some sense of the personal factors influencing my evaluation of research, I will now examine the evidence for claiming that my experience is a representative one, thus establishing the relevance and value of the current study for the profession.

The claims about the status of Music Therapy research that I will document in the first section of this chapter are as follows: current research that follows traditional standards is largely irrelevant to clinical practice, particularly that practice relying on creative techniques; clinicians are dissatisfied with the available conceptual tools; and existing theoretical attempts have been made from the perspective of imported psychological viewpoints.

Given that these three claims constitute my diagnosis of the primary problem I wish to address, the following three represent the strategy for remediating this problem: dissatisfaction with current research among clinicians is largely due to its non-indigenous basis; this non-indigenous foundation is explained by
the fact that Music Therapy is in a pre-paradigmatic stage of development; and there is a need for an indigenous paradigm which will meet the criteria of clinical relevance. The documentation for these claims—which take us beyond merely supporting the existence of the specific deficiency of the research literature that I am claiming, to supporting the strategy I have adapted to address this problem—will be offered in the section on metacritiques.

Before proceeding to this documentation I would like to re-emphasize that neither the present chapter nor the chapter on traditional research in Music Therapy is intended to be an exhaustive, historical account. A complete history of research theory in Music Therapy would demand its own study. What I am intending in these two chapters is to explicate the underlying bases and rationales for what I am calling traditional research in Music Therapy, and the new view of research exemplified in current efforts to study actual clinical work. Although I provide historical information where it is important to the general line of my overall argument, it should be understood that this historical material was selected in order to best represent the two opposing strands of research thought in Music Therapy.

I will offer one additional clarification here. The reader will notice that I have strong disagreements
with many of the authors that I subsequently cite as support for my evaluation of Music Therapy research. This somewhat unusual situation results from the fact that these authors— for the most part— feel that their investigations into clinicians' attitudes towards research are just that, investigations into attitudes. I take the further step of considering these attitudes to be a bona fide judgement on the value of this research.

While the experience of any one individual can make no a priori claim to generalizability, a consensus on the nature of individual experience is valuable in determining answers to questions like the relevancy of current research. The inclusion of the judgments of individuals, offered as support for the legitimacy of the present study, is a necessary step in this process.

Perspectives Within the Current Paradigm

It has been asserted that a substantial amount of Music Therapy research is not relevant to clinical practice, particularly to creative methods, and that this state of affairs is due to conceptual, not practical, problems. While the reasons for the latter claim will be explored in Chapter VII, evidence for the former claim will be presented here.

In a quantitative study to explore the finding that "music therapists do not consider knowledge of
current research literature to be an important aspect of music therapy practice," Nicholas & Gilbert investigated Music Therapists' attitudes towards current research. They found that 75% of the respondents to their survey agreed that "research studies . . . were not relevant to their daily functioning as music therapists" and 64% agreed that this research was not applicable to "real world problems." Additionally, the need for more explanations regarding the clinical applications of research was almost universally agreed to (98.5% of respondents). Further, 73% of those surveyed felt that research would be more applicable if carried out by practicing clinicians. The researchers also report that there was a significant difference between clinicians and educators in their views on the practicality of research, so the percentage of those actually practicing Music Therapy who are critical of the current research is higher than the reported combined average of 75%.6

These researchers conclude that the reported negative attitudes towards research are due to poor communication between clinicians and researchers. They point to a "concerted attempt"7 on the part of researchers to articulate practical implications to assert that the opinions of clinicians regarding the irrelevance of research are not supported.
This conclusion may not be warranted. An attempt to draw practical implications may not necessarily be successful, especially if the fundamental assumptions of the research preclude practical applicability. This point may be too obvious to note, yet, remarkably, it does not appear to have been investigated by Nicholas and Gilbert. In fact, in hypothesizing that therapists' inability to understand the research would explain the results, the authors shift the focus of the study from the deficiencies of the research itself to deficiencies of the audience. This tendency--interpreting data in a manner that preserves the primacy of a paradigm--reflects the resiliency of specific paradigms and the reluctance of adherents to examine its fundamental bases.

Clinicians' complaints regarding the unintelligibility of research journals are justified, according to Madsen, yet he attributes this to the specialization of vocabulary necessary to conduct research. Madsen feels that "a positive attitude towards research has not yet found acceptance within the appropriate group"--presumably clinicians--and it is this fact which can help explain the negative evaluations of research.

This observation is not supported, and is in fact strongly contradicted, by the empirical data provided by Nicholas and Gilbert and by the implications of
another of Madsen's own studies. Nicholas & Gilbert found that over 95% of Music Therapists surveyed agreed that research skills should be part of all Music Therapists' training, and 97% agreed upon the need for more research in the field. In citing this "generally positive attitude toward research," the authors distinguish it from the "expressed dissatisfaction with the current research," manifest in the results reported above.¹⁰

Among other conclusions, Madsen found that "research sessions [at professional conferences] are well-attended and deemed to be of high quality, those who attend research sessions are representative of the membership at large, and the level of research training within the profession is relatively high."¹¹

All of these factors would seem to predispose clinicians to favorably receiving research reports, if those reports relate to clinical concerns in any form. Rather than look at the research itself to understand the continued negative evaluations, Madsen attributes these negative ratings to the form of their presentation and the inability of clinicians to appreciate research whose presentation is not "entertaining."¹² Thus, he also shifts the focus from the deficiencies of the research to those of the audience, even though the empirical evidence suggests otherwise.
Given this portrait of the inapplicability of current research focus, it would seem difficult to justify its continuation. Possibly one might argue that pure research has benefits, whether or not applications to practical needs are immediately apparent. Certainly this is true in the "harder" sciences such as physics and astronomy. Yet Music Therapy, by definition, is a service-oriented field. With so little known about its internal mechanisms, and so little empirical support for its application, it is hard to justify research efforts with such limited capability for transfer to real-life situations. Furthermore, questions such as the fundamental construction of matter have an intrinsic interest, regardless of practical application. It would be difficult to maintain this "intrinsic interest" argument for the highly specific experimental research designs in Music Therapy.

Gilbert\textsuperscript{13} reports that the locations of Music Therapy research studies conducted in the years 1976-78 were equally divided between university and clinical settings. Also noted was the change from the 1973-75 period where the respective proportions were one-third and two-thirds for these two settings. These figures are cited as evidence of the "concern for applicability to the clinical setting."\textsuperscript{14}
Curiously, Gilbert does not comment on the difference between the two time periods studied where the trend is away from conducting research at clinical settings. If location of the study is evidence of clinical application, then using this criteria it would seem that researchers are becoming less concerned with this application.

Additionally, it will be argued that it is the level of analysis—not the specific attributes of the subjects in a study—that determine its applicability. As Scheiby has shown, the underlying processes exhibited by university students in clinical use of Music Therapy are similar to those seen in the analysis of neurotic and psychotic individuals. It seems that research with human subjects has a high degree of potential for generalizability across populations if the operations performed by the researcher are consonant with the nature of Music Therapy process. If these operations have fundamental barriers to illuminating this process, conducting the research in a clinical setting will not increase their clinical applicability.

The most important result of Gilbert's study, however, is one that contradicts her conclusion regarding clinical applicability. She notes that "the proportion of studies focusing upon pedagogical and physical/perceptual bases for music research has
increased in the last 3 years; the comparative focus on psychological aspects of musical behavior has decreased within the last 3 years. According to researchers, this shift is due to the growing interest in educational and medical uses of music.

To the extent that such rationales for treatment are pursued one moves away from investigations relevant to clinicians working with the emotional lives of their clients. For a Music Therapist working with a physically handicapped individual, for example, it is equally important to account for the influence of affective factors as when one is working with an emotionally-handicapped individual. I say this because the process of physical rehabilitation does not exhaust the potential of Music Therapy for the physically-disabled individual. To understand the effect of music in this rehabilitative process one must investigate factors like the enhanced motivation and increased self-esteem resulting from the this clinical use of music, factors typically associated with Music Therapy as psychotherapy. Any clinician working with either the creative or expressive potentials of music is necessarily concerned with the psychological realm. A research program with a decreasing focus on this realm is one with a decreasing relevancy for practicing Music Therapists.
In a follow-up study to Gilbert's, Codding reported an approximate return to the two-thirds/one third split between clinical and university settings, respectively, for the years 1977-1985. Although the prevalence of clinical articles in research literature decreased from 22% to 7% during that time, this change is described as a moderate decrease. Curiously, there is no discussion of the meaning and implications of such a dramatic decrease in clinical research.

Asmus and Gilbert acknowledge that a prime deficiency of the professional literature is the "paucity of clinically oriented articles," and they cite Nicholas's claim that there is a "need for quality articles dealing with clinical methodologies, techniques and approaches." The investigators hypothesize that this problem is partly due to the absence of a conceptual model for the therapeutic process, and they proceed to present a client-centered model to remedy this deficiency.

Although the "client-centeredness" of this model is questionable—the therapist identifies client needs and goals from observations of behavior prior to intervening therapeutically, and this is seen as evidence that "the client in effect establishes the goals . . . and . . . methods"—what is of greater interest here is the fact that the authors proceed to suggest a model for the therapeutic process which is
adapted from a model that describes the process of training clinicians. In other words, rather than base training methods on clinical process, a conceptualization of Music Therapy process is derived from the manner in which its implementation is taught.

Possibly, this logical inversion is due to the difficulty of discussing and training the creative and intuitive skills that a competent Music Therapist must possess. As these elements of training are not easily codified or reduced to a set of procedures for acquisition, they can lose ontological status in an academic setting where conceptual and propositional knowledge is emphasized. As a result, there is a temptation to define the process based on "how we teach people to do it," even though essential elements of the process may be unknowingly omitted.

Gfeller sought to uncover such underlying bases of theory and practice in Music Therapy through an analysis of the articles in the Journal of Music Therapy from 1964-1987. Results showed that 26% of these articles referred overtly to behavioral principles and techniques and 11% to psychoanalytic theory. There are reasons, however, to suspect that these numbers may be an underestimation of the current importance of these schools of thought to the profession of Music Therapy.
Gfeller notes that there are indirect references to these theories that are not counted by her system of data collection and concludes that "psychoanalytic theory may have greater influence . . . than direct references would suggest." Specific mechanisms are often invoked in an explanatory manner—such as the cathartic value of music—without a clear reference to a specific analytic position. Similarly, behavioral principles are used to guide research and interpret results, without this debt being made explicit.

Given these considerations, Gfeller concludes that psychoanalytic theory was dominant during the first six years of this journal's publication, its influence declining in the mid 1970s. Simultaneous to this decline is an ascendance of behavioral literature, which peaked in the years 1976-8 at 41% of all articles and declined slightly after that time.

Interestingly, Gfeller cites the claim of Feder and Feder that in addition to behavioral principles, the second predominant trend in music therapy is the "development of a structure that is independent of psychological theory . . . specific to the music therapy profession." The components of this structure involve attributing the efficacy of Music Therapy to the ability of music to impose order, enhance self-esteem, and facilitate social integration. These are referred to as "Gaston's principles."
Although these principles could provide a basis for an indigenous paradigm, in actuality approximately 40% of the articles citing one of these factors does so from the viewpoint of an external psychological theory. The pattern that emerges from this group of researchers is to acknowledge that there is a negative perception of research among clinicians, while attributing this either to clinicians themselves or to external factors, e.g., the absence of a model. The negative perception of research is seen as a problem, not as a source of valuable information to be used in the evaluation of research efforts. The difficulty in stepping out of an accepted paradigm is illustrated in the approach of Asmus and Gilbert, who go as far as acknowledging the need for research on actual clinical interventions, yet in deriving their model from one used for educational purposes, they appear to sabotage their own efforts.

**Metacritiques**

The following group of authors have all, to varying degrees, proposed fundamental changes in the way research should be conducted. The dominant concern expressed is that research methods must change to provide a context for the experience of clinicians. This material is presented chronologically in order to provide some sense of the manner in which thought in this area has developed.
The beginnings of this movement occurred in the late 1970s when clinicians influenced by European theorists promoting phenomenological viewpoints began to question the value of research not related to clinical concerns. In one of the first published precursors to the present effort, Even Ruud, a Norwegian Music Therapist, states that while it has been necessary to explain Music Therapy processes according to prevailing psychological treatment theories, the "ultimate goal" of Music Therapy research should be to establish the uniqueness of this discipline. Here, he appears to be supporting the construction of an indigenous paradigm. While noting that conflicts between the prevailing theories can be attributed to underlying philosophical positions, Ruud then argues that these approaches are complementary rather than contradictory. As a result, the establishment of a dominant paradigm in Music Therapy is considered to be counter-productive in needlessly limiting "man's potential ways of regarding himself." Therefore, it becomes unclear if Ruud is actually advocating an indigenous paradigm or a continuation of the current theoretical diversity.

A possible answer lies in Ruud's assertion that "music therapy can never establish theories and procedures separated from . . . psychology and philosophy." Ruud implies that it is therefore
necessary for Music Therapists to work within the conceptual constraints of these external systems. Yet, as will be subsequently argued, it is possible to adopt philosophical positions, e.g., an epistemological stance, and psychological methods, e.g., phenomenological inquiry, and still create indigenous theories based in clinical experience. Perhaps this type of synthesis is one not considered by Ruud.

In developing criteria for the evaluation of Music Therapy models, Ruud adopts a pragmatic approach in recommending that they be judged "not on the basis of whether they are 'humanistic,' 'true,' or 'scientific,' but rather on the basis of their consequences." This suggests that there are self-limiting elements to the current paradigms, but ones which can be overcome by combining them rather than by adopting the more parsimonious approach of an indigenous effort.

In an unpublished working paper for the New York University International Symposium on Music in the Life of Man: Toward a Theory of Music Therapy—a gathering with an important relationship to the present effort as it was "devoted to music and what makes it a unique tool in therapy"—Barbara Hesser warns that "much of the essence" of Music Therapy may be lost if research efforts are limited to the experimental model. She calls for an integration of scientific and artistic approaches that will comprise a "new paradigm of
research," and suggests that "new trends in qualitative research and phenomenological psychology" may be useful in this new paradigm.31

The research committee of the symposium reported that "one of the objections to current research in music therapy is that it is too behaviorally oriented and tends to overlook the experience of the listener."32 The committee made the following recommendations to improve the state of Music Therapy research:

1) The use of all types of research to study the experience of music in therapy should be used in order to get as full a picture as is possible of the musical experience. Specific research methods recommended included historical, descriptive, experimental, phenomenological and introspective among others.

2) More attention should be given to the uses and purposes of studies instead of minute details of experimental design.

3) Researchers should study master clinicians. The committee pointed out that those clinicians most able to ask the important research questions were not in positions to carry out clinical research.

The committee attributed the inadequacies of Music Therapy research not to the scientific method itself, but "inadequate views on the nature of scientific inquiry in the field of music therapy."33
In an important article that brought some of these emerging ideas on research to a broader audience, Helen Bonny observed that, historically, Music Therapists have based their work on prevailing educational and psychological models. Although developmentally appropriate at one time, Bonny asserts that it is now time to move beyond this "parasitic thinking" characterized by "adopting models from other disciplines" and establish a paradigm based upon the unique aspects of Music Therapy practice.34 This paradigm should be a broadly inclusive one that can embrace and provide a basis for a variety of theoretical and practical viewpoints. Bonny identifies the limits of traditional conceptions as their reliance on mechanistic and reductionist principles, and she offers a holistic paradigm as an alternative for the future.35

In a pioneering study of a particularly dramatic clinical case, Carolyn Kenny discusses the choice of a phenomenological method to study Music Therapy process.36 This new method was deemed necessary as the author "needed to find a way to describe that experience . . . without distorting or taking away from it [and the] verbal language was not available in . . . Music Therapy."37 Kenny justifies the need for a new paradigm in saying that "at this point in time the largest portion of Music Therapy literature . . . does
not really say too much about the inherent processes and experience of . . . Music Therapy."^{38} She is also in agreement with Bonny and Ruud in identifying the source of previous treatment rationales for Music Therapy: "Music Therapy . . . accommodated its own uniqueness . . . to establish a foundation of acceptance for practice. . . . Essential elements were sacrificed, particularly those dealing with creativity and individuation."^{39} This emphasis on understanding the contribution of creativity to the therapeutic process is crucial to Kenny, and no Music Therapy research method will be adequate unless it is able to assess its role.^{40} Future research must embody the "sensibilities of both artist and scientist,"^{41} because Music Therapy process takes place within an art form.

Kenny notes the statement of the Research/Client Assessment group from the previously mentioned International Symposium on Music Therapy in which she participated. Most important is the fact that "there were no conclusions drawn as to effective methods for analyzing and presenting publicly the use of clinical piano improvisation."^{42} This panel of prominent Music Therapists and theorists could not point to any methods at the present time which could illuminate the mechanisms of this primary Music Therapy method.

In a later writing, Ruud observes that "there has always been someone able to translate new findings in
any dominating philosophical or scientific thought into a new rationale for the use of music to take care of health and disease, and that in an effort to establish itself in its infancy, Music Therapy was required to adopt various "metaphysical or idealistic types of theory." His observation that the current paradigm shift in physics is a promising one for Music Therapy seems to indicate that he agrees that this type of intellectual exercise is a productive one. In sum, Ruud agrees that Music Therapy has been built on adopted premises, but his evaluation of this tendency is different from that of the present author.

In discussing Music Therapists' lack of a strong professional identity, Hesser attributes this deficiency to the borrowing of languages and models of research from related disciplines. Though developmentally necessary, this author feels that Music Therapists must now develop their "own theoretical foundation, using the language and forms of research best suited to [their] unique experience of music as therapy."

Hesser is in agreement with Kenny in saying that developing research methods that "reflect the art and beauty of [Music Therapy] work as well as the science" will best serve the current needs of the field. Lastly, Hesser suggests that the lack of research on actual clinical practice is possibly "due to the fact
that quantitative research methods do not directly relate to [clinicians'] personal experience of music therapy." ⁶⁷

Acknowledging the contribution of quantitative methods, Forinash and Gonzalez proceed to decry their inability to capture "the true power and energy that exists in clinical work." ⁶⁸ Similarly, existent qualitative methods--various psychotherapeutic interpretive systems--are seen as unable to provide a "complete representation of the music therapy process." In arguing for a phenomenological approach, these authors cite the need for a research method that does not explain musical forms of interaction through "non-musical models of human life." ⁶⁹

In another pioneering study, Forinash reports that traditional research methods do not yield results consonant with the nature of her clinical experience. ⁵⁰ She attributes the lack of clinical relevance of such research to a reductive strategy that can not capture the "complex, multidimensional human interaction, the working together of patient and therapist in the context of the therapeutic environment." ⁵¹ Forinash considers an appropriate Music Therapy research method to be one that "allow(s) the music therapy experience to be seen and heard as whole, one that would take into account the complexity and richness of the patients
with whom [she] work(s) and the sessions in which [they] interact."52

In conclusion, the present author's call for a clinically relevant research paradigm that maintains rigorous scientific standards is part of a developing tradition. Although incipient efforts have been made in the area of clinical research, progress has been slow, in part owing to the absence of an overall vision of science that can support such clinical investigations. In order for the reader to be able to evaluate the view of science promoted by the present author it is first necessary to gain a working familiarity with the basic concepts in the philosophy of science and this is the focus of the following chapter. Those readers already possessing some knowledge in this area may wish to pass over the initial discussion and go directly to the discussion of paradigms commencing on page 77.
Notes


7. Ibid., 211.


14. Ibid., 105.


20. Asmus, Jr. and Gilbert 44.


22. Ibid., 191.


27. Ibid., 71.

28. Ibid., 70.

29. Ibid., 71.


33. Ibid.

34. Helen Bonny, prologue, Music Therapy, 4(1), 1984, 2.


36. Kenny, Field.


38. Kenny, Field 11.


41. Kenny, Field 45.

42. Kenny, Field 2.


46. Ibid., 12.

47. Ibid., 12.

48. Forinash and Gonzalez 35.

49. Forinash and Gonzalez 36.


51. Ibid., 1.

52. Ibid., 2.
CHAPTER II

PHILOSOPHY OF SCIENCE

A Role for Philosophy of Science

I introduce here a slight observation which I shall call 'philosophical,' meaning simply that we could do without it.

Though throughout this paper I employ conceptual, philosophical and historical analyses, I view the study--taken as a whole--as a scientific one, or rather one whose primary use will be to alter the subsequent course of empirical research and not merely provide conceptual and historical analyses of research efforts. I would like to make clear the distinction between philosophic and scientific endeavors because philosophic efforts are often seen as irrelevant to practical concerns, and in some sense this is true as the continuing progress of science has not been inhibited by an inability of philosophers to adequately characterize scientific activity. By demonstrating that the present effort represents a practical problem, I hope to ensure its relevancy for the actual activities of clinician-researchers.

My purpose is not to argue for a predetermined research philosophy using Music Therapy phenomena as
evidence, but is instead to derive an appropriate conceptual system from the nature of Music Therapy process. The present study is more accurately considered part of the effort to establish an indigenous research paradigm, rather than an external element or a logical precursor of such an effort.

Laudan makes it most clear that the importance of conceptual problems—such as the one being addressed here—as integral parts of science has been minimized because "it does not comport well with those empiricist epistemologies of science which have been the reigning fashion for more than a century."² This conclusion follows from the history of science where "key debates between scientists have centered as much on nonempirical issues as on empirical ones."³ Conceptual problems are distinguished from empirical ones in the following manner:

If empirical problems are first order questions about the substantive entities in some domain, conceptual problems are higher order question about the well-foundedness of the conceptual structures (e.g., theories) which have been devised to answer the first order questions.⁴

Though this issue will be discussed in Chapter VII, I will briefly anticipate that argument here by noting that claims that Music Therapy process can be adequately explained by either behavioral or psychodynamic formulations are themselves theories, albeit higher order ones of the type described by Laudan. Such higher order theories are embedded within
a matrix of methodological and explanatory norms. Conflicts over these types of prescriptions for correct scientific conduct have been "the single major source for most of the controversies in the history of science, and for the generation of many of the most acute conceptual problems with which scientists have had to cope."  

A substantial portion of this study is devoted to unearthing the methodological and explanatory norms underlying behavioral (and to a lesser extent, psychoanalytic) conceptions of Music Therapy. These norms and guidelines will then be compared to those recommended by a more theory-neutral, i.e. indigenous, conception of Music Therapy phenomena. By providing a critique of—and suggesting alternatives to—the higher order theories guiding research and explanation in Music Therapy, the current study is addressing a perennial conceptual problem in science. The role of philosophy of science here is not to provide a conceptual scheme to be imposed upon Music Therapy, but merely to provide the conceptual tools by which traditional research and the subsequently established indigenous paradigm will be evaluated.

The Received View: Introduction

Before describing the philosophy of science that has guided research in Music therapy it is first
necessary to discuss the various conceptions regarding the function of philosophy of science, as well as to briefly summarize the conflicting positions philosophers of science hold. The goal here is neither to argue the relative merits of the various positions nor to provide a complete account of contrasting views, but merely to outline the basic components of the influential position referred to as the Received View—the one traditionally appealed to by Music Therapy researchers—and to establish that this account is only one of many possible accounts of the nature of scientific activity.

This latter step is necessary because adherents of the various forms of behaviorism—an approach that has dominated thinking in Music Therapy research—argue that the only way psychology, and hence Music Therapy, can be considered scientific is to follow behavioral principles. Behaviorism is unique as a scientific school because it involves not the acceptance of any specific theory, but the implementation of a specific methodological program.\(^7\) The purpose of the behavioral program was to put psychology on a more firm scientific footing through the elimination of consciousness as an object of study. Alternate approaches in psychology were then criticized not so much for being right or wrong, but for being unscientific. Therefore, it becomes necessary to establish that the philosophical
positions upon which behaviorism is based can no more ensure scientific progress than can those of other schools in psychology. If there are no other coherent accounts of scientific activity then behaviorists are correct in asserting both that behaviorist strictures are essential to any scientific psychology, and that their research program embodies not a philosophy of science but the philosophy of science.

Typically, philosophers are more concerned with issues in the philosophy of science than are researchers. Modern scientific activity does not as a rule proceed by selecting a philosophy and then building a research program on its precepts. Researchers employ methods that function well in solving problems—problems that result from the paradigm in which they are engaged.

Adherents of logical positivism were profoundly influenced by the scientific revolution effected by the advent of modern physics, especially by the manner in which it overthrew the deeply held beliefs of the previous two hundred years. These philosophers were concerned with two primary tasks: The first involved providing a formal analysis of scientific methods in order to aid the activity of scientists in not making claims that go beyond that warranted by available data; they were also concerned with providing a coherent account of the manner in which scientific knowledge
seemed to progress. The Received View resulted from the efforts of the adherents of logical positivism to rationally reconstruct the process of scientific progress, and to provide a logical description of how observations, theories and laws interact to ensure this progress.

The discovery that many historical examples of scientific achievements did not fit the Received View led to a variety of responses among philosophers of science. While some subsequently claimed that theories not meeting these formal requirements were unscientific, others took these discoveries to mean that the Received View itself is inadequate. These disputes reflect differences over the very purpose of philosophy of science and Suppe notes that this discipline has alternately "been concerned to either vindicate, evaluate, make sense of, or improve the practice of the then contemporaneous science." The debate which still continues occurs around whether philosophy of science should be prescriptive or descriptive.

Descriptive, or naturalistic, approaches focus on the actual activity of scientists in order to understand how science progresses. Philosophers employing these methods—such as Thomas Kuhn and Stephen Toulmin—have embraced the above-mentioned dissonances to create new explanations of this
progress. As noted above, others have decided to hold to their view—giving primacy to logical necessity rather than to historical veracity—of what legitimate scientific activity consists of, and criticize various theories as being un-scientific, even when the theory has established a successful paradigm and research tradition. David Hull notes that philosophers have found reasons to dismiss the theories of population biology, molecular genetics and the genetic and synthetic theories of evolution, to name a few.\textsuperscript{10} Here, biologists can remain relatively immune to the criticisms of philosophers as they can point to a fruitful research tradition, regardless of whether or not philosophers are satisfied with it.

The field of psychology, however, can not point to a similarly successful research tradition and therefore is particularly susceptible to philosophical criticisms of this sort. In fact, the continuing conflict over methodological precepts in psychology makes it more reasonable to employ philosophical analysis in a diagnostic role. Yet the analysis that behaviorists have taken to heart—and whose precepts are often invoked as support for behavioral positions—is derived from a portrait of the physical sciences.

Psychologists have historically been enamored with physics, seeing in the axiomization of physical laws keys to the progress of psychology. Consequently,
psychologists are humorously referred to as having "physics envy." It is worth noting that behaviorists are not unique here in that any psychology with claims to being scientific has attempted to base itself on physics. The fundamental irony is that psychologists have sought to build their discipline on a view of science that is too narrow and constricted to suffice even as a complete account of physics, their model of a successful science.

Without an accepted model of how to fruitfully research consciousness, emotions or aesthetic experience in psychology at large, it was natural for Music Therapists to "hitch their wagon" as it were, on the approach that has dominated psychology in the United States for most of this century: behaviorism. It will be subsequently argued that the primary reasons for this alliance have been related to the social status of music therapy, not to the exigencies of research itself—and that on a conceptual level the practice of Music Therapy is antithetical to the precepts of behaviorism.

Yet there are many forms of behaviorism that differ from each other in ways that are non-trivial. The behavioral approach comprises specific views on areas of concern in science such as the nature of observation and objectivity, the role of theories and the nature of causal explanation, to name a few. It is
then necessary to discover and explore the positions held by various Music Therapy theoreticians on these areas of concern, as a global criticism of behaviorism qua behaviorism as a basis for Music Therapy research, could leave the real culprits—the specific components of the positivist philosophy—untouched and free to re-appear in a subsequent research program.

Additionally, philosophers disagree upon which aspects of the Received View merit criticism. It is not enough to just say behavioral research is discredited because it is based on the Received View because, while "there is agreement that the Received View is inadequate, there is no general consensus what the source of its inadequacy is."\(^{13}\)

The following discussion of the Received View is necessarily brief and each of the philosophical positions are sketched in the broadest way, thereby running the risk of obscuring potentially significant divisions within each school of thought. The reader is encouraged to go directly to the literature to gain more than the superficial understanding presented herein.

**Received View: Exposition**

Observation, evidence, facts; these notions, if drawn from the "catalogue sciences" of school and undergraduate text-books, will ill prepare one for understanding the foundations of particle theory. So too with the ideas of theory, hypothesis, law, causality and principle. In a growing research discipline, inquiry is directed not to rearranging
old facts and explanations into more elegant formal patterns, but rather to the discovery of new patterns of explanation."

And so Hanson presents one of the earliest challenges to the positivist view of science—a view which emphasized the role of objective observation, deductive reasoning and theory validation. Contrasting views of science have placed more emphasis on understanding the role of theory-laden observation, non-deductive reasoning and theory discovery in creating an accurate portrait of science.

Nevertheless, the Received View dominated philosophy of science for most of this century and is still influential in determining common-sense notions of science. Though discredited in philosophic circles, the ideals it held up as models of scientific activity still determine what is considered legitimate science, especially in the softer sciences that are more insecure about their professional status than are fields like physics and chemistry.

The Received View can only be applied as a formal description of a relatively complete science (and even in this area it has serious deficiencies); it cannot model how scientists actually work. Yet, given these criticisms (which will be substantiated) it is true that one can best understand 1) current thinking in philosophy of science and, 2) traditional thinking in Music Therapy research, by understanding the view of
science that (1) is reacting against and that (2) has drawn inspiration from. This view is as follows:

1. Without the biasing element of commitment to a pre-determined theory the scientist collects and classifies facts through direct observation of the natural world.

Further, these facts are of an objective nature such that they possess intersubject reliability and involve investigations of an empirical sort—that is of experience of the world as opposed to armchair speculations. These observations should ideally be quantifiable as they will eventually need to be deduced by laws of a mathematical nature.

2. Once these data are classified, patterns are deduced which in turn suggests a theory that would account for the patterns.

The additional stipulation here is that only certain types of statements comprise legitimate theories and hence, explanations. To explain an event in this view has two components: The first is that the explanation describes a system where the event must take place given certain conditions. The theory of gravity says that Newton's apple and the earth must
move toward each other given the two objects' relative masses. In any given set of circumstances, only one prediction is allowed for the theory. A theory compatible with two or more outcomes given the same state of affairs is then an unscientific one. An example of an unscientific hypothesis would then be: All things being equal, when the apple separates from the tree it will either move towards the earth or away from it.

Second, it must be shown that the fall of the apple has been caused by circumstances external to the apple. It would be illegitimate in this view to say the apple had a desire to fall to the earth, or in some way had an inner compulsion to rest on the ground. The reason for the occurrence must not be a property of the individual (apple) in question.

It is also important to note that there is a strict demarcation implied between entities and properties that are directly observed, and those whose existence is only inferred. The former are data to be explained, the latter are only inferred and consigned to the status of theoretical entities.

3. Combining the theory with what is already known about the natural world, other facts and laws of nature, the scientist then turns to the world to see how his predictions fare, regarding what he will further observe given certain conditions, if the theory
is correct. After these subsequent observations, the theory is then either validated or abandoned based on its efficacy in making accurate predictions.

Because science is an objective enterprise all observers will agree on whether or not the theory has been validated. The language of observations and theories ensures that data is interpreted unambiguously regarding the truth-value of a given theory, and previous beliefs such as holding a rival theory will not affect what one observes.

4. The validated theory then assumes the status of a Law of Nature whose necessity, for the time being, is unquestioned. These laws are said to be universal in the sense that they are about all atoms, solar systems, or people, not specific individuals. Just as a law of nature would not refer specifically to red-headed individuals between the ages of ten and eleven, it would not refer to our specific solar system. Lastly, science progresses by subsuming increasing quantities of data under a decreasing amount of laws.

With this brief portrait in hand, it is now possible to discuss the various philosophical positions thereby implied. Again, this account is not intended to be exhaustive, but merely provides the tool for the subsequent analysis of the research tradition in Music
Therapy.

The origins of the Received View can be traced back to the positivism of Auguste Comte. Relevant aspects of Comte's positivism that survived his quasi-religious movement include the idea that "explanations of nature in terms of unobservable entities were to be abandoned." The goal of science was to provide the ability to predict, and hence control, nature. Implicit in this view is a strict determinism as it was felt only in a deterministic universe could science exist.

In our portrait above, this determinism is seen in the claim that a scientific theory is only allowed one prediction of an event given a certain state of affairs. Deterministic laws "permit the inference of all past and future states of the system, given the values for the relevant variables at any one time." On this view, then, one could only be said to explain, say, the orbit of Mars or the timing of the present author's lunch on Monday, November 6, 1989 at 12:31 p.m., if and only if, one can show that both of these affairs necessarily had to occur given the state of universe and certain operative laws of nature. The most complete statement of determinism can be seen in Laplace's claim that "were he but supplied with an account of the state of the universe at one moment, plus a list of all the causal laws, he could predict
and retrodict every other moment of the world's history."\textsuperscript{18}

Although determinism as form of explanation regarding human actions comes into direct conflict with ideas of free will or volition, it certainly embodies an important aspect of what it means to explain something. What type of explanation is offered when one says that an apple fell to the earth because of an inner compulsion to do so? What then explains the inner compulsion? Is one really saying anything of interest in this "explanation?" Most of us would probably agree that this is not an explanation that is particularly enlightening, or even correct, when applied to the apple. Other issues arise when entities such as people or species are considered, and these will be subsequently discussed.

But first it is important to see how employing the doctrine of determinism would lead one to search for external causes for events. If one must know the inner nature of either apples or people in order to explain their actions a whole host of methodological and empirical problems arise: such as discovering the nature of the apple's inner structure, determining which aspects of this structure are relevant to the question at hand, and deciding how to investigate this structure without fundamentally altering it through one's means of investigation. Yet, if only external
factors are relevant the task of the scientist is greatly simplified and these questions requiring speculations on the "inner nature" or apple or person are not relevant.

The metaphysical view of the world supporting determinism and the search for external causes is known as the corpuscularian philosophy. It pictures the world exclusively as "Moving Atoms in the Void." Since, on an ultimate physical level, all that exists are particles in empty space, the only type of change is change in positions of these particles; the only way the position can be changed is to be struck by another particle. All activities, of either nature or of scientists in the laboratory, then are caused by these mechanical processes.

Two factors bear noting here. This mechanistic philosophy does not just imply that there is a mechanism for natural processes, but implies that this mechanism is mechanical in the form of actions of externals. For example, there are mechanisms by which species adapt to environments and human children develop concepts, but these mechanisms need not be mechanical in nature, i.e., they may be described in ways other than that of external forces acting on passive individuals. Additionally, the historical context in which this philosophy arose caused its proponents to think that mechanistic laws--laws
governing matter in motion—provided the only alternative to "super-naturalism and idealism in the exploration of natural events." Therefore, any other statements offered in an explanatory function are considered to be unscientific.

The pervasiveness of mechanistic thinking throughout science resulted from the fact that the earliest scientific successes were in the field of mechanics and astronomy, fields that explained causation in terms of concepts like impact, attraction and momentum. Hence, all causes were conceived of as "impacts, attractions, pushes and pulls, and all effects the results of pushes and pulls." To explain something meant to reduce the observed phenomenon to the interactions of these mechanistic forces.

An important manifestation of the mechanistic philosophy in psychology has been in the form of behaviorism. In focusing on overt behavior in their reaction against introspectionist methods and the study of consciousness, behaviorists have been guided by mechanistic principles for behavior is no more or less than "an organism's motion in space" [my emphasis]. Construing behavior as motion—a quantifiable property—opens the way for psychology to become objective and therefore, part of science. In psychology at least, it was felt that this quantification of observations was essential to ensure the objectivity of data. This is
what is meant when it was noted above that the nature of one's data needs to be indisputable by a variety of observers. The quantification of data as a means of objectification has further implications.

In psychology, for example, this has led to the adoption of operationalism as an account of the meaning of theoretical terms. An operationalist view defines qualities by the measurements we perform to detect and evaluate them. If a psychologist wishes to investigate anger it is required then to provide a means of detecting and measuring it. Typically the behaviorist could choose something like fist-pounding on the table, or a child screaming, raising his voice, etc. The operationalist then goes beyond this move and says that the anger is the fist pounding because nowhere do we see the anger apart from some behavioral manifestation of it. Different researchers will not disagree over whether or not anger was exhibited by a specific subject, and the concept of what anger will remain immutable through time. There is no justification, in this view, for postulating a mental state that would account for the various manifestations of anger. Additionally, since there is no singular phenomenon, e.g., anger, to explain the various behaviors, each measure we use to detect anger is actually measuring a discreetly different property. It was felt that only operational definitions could "insure the possibility
of an objective test for the hypotheses formulated by means of [scientific] terms.\textsuperscript{25}

This translation of psychological terms into behavioral terms is one of the two ways that the doctrine of reductionism has been manifest in psychology. The other way is through the translation of psychological terms into physiological terms, typically brain states. Theoretical reduction, though not technically part of the Received View, is derived from the closely allied positivist claim of the unity of science, and therefore both positions bear discussion in relation to one another.\textsuperscript{26}

The concept of the unity of science consists of two closely related doctrines. Adherents of methodological unity hold that there exists a universal scientific method, applicable to all sciences at all times. The theoretical unity of science is the doctrine that all of the branches of science can be arranged hierarchically, with physics as the foundation, proceeding through chemistry, biology, psychology and sociology. Theoretical reduction occurs when the theories or concepts of one science can be adequately explained by the theories of the more basic science. In other words, if all the properties of molecules could be explained solely by reference to atoms, then it would be said that chemistry has been reduced to physics. Similarly, if one can show that
the theories of sociology can be explained solely by reference to the forces within individuals, then sociology will have been reduced to psychology.

The positivist view holds that the goal and purpose of science is in fact this type of reduction and that reduction of terms characterizes scientific progress. Again, to return to our mythical scientist, this is what is meant when it is said that the goal of science is to explain an increasing amount of phenomena by a decreasing amount of theories. If science reached the point where all events could be explained by the theories of physics, whether this is the evolution of species or the timing of this writer's lunch on the above-mentioned day, then the goal of scientific activity will have been reached.

Belief in the unity of the objects of scientific knowledge implies that all entities in the universe possess the same types of properties and that the activities of all must be explained by reference to similar forces. If this doctrine is valid, it becomes it more reasonable to assume—or at least argue for—the existence of a universal method applied to all branches of science indiscriminately. As noted earlier it was belief in the methodological unity of science that united the various forms of behaviorism, not adherence to a specific theory. Substantive concerns were subordinated to methodological ones.
There are a few problematic assumptions in these related doctrines which have worked against the implementation of the reductionist program. Primary among these is the belief that the units of interest that one encounters as one moves through the various levels of science, e.g., atoms, molecules, cells, people, societies, do not possess unique properties that cannot be accounted for by the entities of the more fundamental science. It would be misstating the reductionist position to accuse them of denying consciousness to people because this property is absent in cells. The reductionist would grant the attribution of consciousness, with the stipulation that we do not attribute any properties to consciousness (such as volition) that cannot be explained solely by the activities of individual cells and neurons. The term "consciousness" only serves as a kind of shorthand for purely physiological processes.

The corollary to this restriction on unique or emergent properties, is that one cannot invoke in an explanatory function process that would be precluded by the laws of the more fundamental level. So if one wishes to study species, people or societies, it is not legitimate to invoke in a causal explanation an attribution of a property that would be denied to atoms or molecules. Returning to our original example, it is just as illegitimate to aver that John pounded his fist
on the table because he was angry, as it is to say the apple fell to the ground because it was tired of hanging in the tree.

These latter proscribed statements are examples of teleological explanations. As a metaphysical position dating back to Greek science teleological explanations ascribed strivings, desires and needs to both animate and inanimate objects. Although as an overall metaphysical stance teleology is no longer subscribed to, both biology and psychology employ concepts such as function, purpose, and self-actualizing drives which have a teleological character. If these types of terms cannot be eliminated without sacrificing the explanatory power of the theories they are embedded in, then the reductionist program will be shown to be inadequate when applied to the sciences utilizing these terms.27

The ultimate fate of the reductionist program has implications for all of the sciences. If it is shown to be fundamentally flawed then it is important to ask whether or not there is a single philosophy of science that describes all that we as a society label scientific knowledge. Perhaps what is then required is a philosophical analysis of each science, without a concomitant effort to unify these analyses into a singular vision. Science then becomes open to a variety of types of explanations and what may be
legitimate in one domain may not be translatable to another science. It just may be that ascribing purposes to people, and not to atoms, is not a violation of desirable standards of scientific conduct!

One important tenet of the Received View that might serve to provide a unity of science is that science proceeds by an objective evaluation of the data. This occurs by employing a neutral observation language in whose terms only those facts that are directly observed are couched. The entities referred to in theories, on the other hand, are not directly observed. This observational/theoretical distinction is an integral component of the Received View. It is felt that establishing this distinction—between observables whose nature is beyond dispute and theoreticals whose existence is only inferred—is necessary to ensure that two individuals with opposing theories can have a common basis for resolving their differences via crucial experiments. These experiments test situations where rival theories predict divergent outcomes thus establishing an objective basis for theory choice and the progress of science.

The strength of behavioral methodologies depends upon the maintenance of this distinction. For a behaviorist, overt behaviors are directly observed, while psychological states are only inferred. In stricter forms of behaviorism psychological states are
not admissible, even as theoretical terms much less observation terms, because they are in principle unobservable, and "fundamental to [positivism] . . . is a repudiation of any inferred entities or processes which are in principle unobservable."²⁸

Although this seems like a common-sense consideration—that things we observe are known with certainty and things whose existence we postulate are only known conditionally—it has proved to be a difficult distinction to maintain when applied to the manner in which theoretical terms function in actual practice. The first point to consider is that there is no strict dividing line between those entities which can be said to be observed directly and those whose existence is inferred. (Even the term "observe directly" is problematic and is nowhere adequately defined by those who maintain the importance of this distinction.) Maxwell makes this point clear in saying that there is a continuous series of types of observations "beginning with looking through a vacuum . . . looking through a windowpane, looking through binoculars, looking through a low-power microscope, looking through a high-power microscope, etc."²⁹ and presumably continuing until we reach tracks of subatomic particles in a bubble chamber. Where one chooses to draw the line between that which is directly observed and that which is inferred is purely
arbitrary.

One who is dependent upon this distinction for their theoretical (or actual!) livelihood (such as a hardened behaviorist) might object that while the above examples demonstrate the difficulty of providing a strict demarcation between observed and theoretical entities, things like mental states are unobservable in principle, and this characteristic should serve to banish them from legitimate scientific discourse. The answer here is that scientists routinely invoke theoretical entities and processes considered to be "unobservable in principle" according to this criteria. Phenomena like the force of gravity, evolutionary and geological processes, black holes and viruses before the invention of the microscope, are or were in principle unobservable, yet still served important theoretical and explanatory functions.

The criteria behaviorists would use to banish the study of consciousness from psychology would also eliminate many of the concepts of modern physics, since properties like mass and force "do not refer to observables in any simple or direct way."\(^30\) What we do observe is their action upon observables. It would seem that the relevant distinction is not between objective and subjective, or theoretical and observed, but between degrees of these qualities.
In psychology there is a trade-off between publicly observable data and closeness to the phenomena of interest, when the phenomena is in the realm of consciousness. Without a strict, logical guideline to determine where to draw this line—and this writer believes there is none—pragmatic considerations come to the fore and allow for the use of private data to solve a specific puzzle. Use of private data cannot be legitimately restricted based on either logical criteria, i.e., what would function as a valid explanation, or empirical criteria, i.e., these types of properties do not figure in explanations in other successful sciences.

The fact that viruses, once considered unobservable in principle because they were too small to be seen, have turned out to be both observable and real demonstrates the tenuousness of this distinction. As a result, Maxwell concludes that "there are no a priori or philosophical criteria for separating the observable from the unobservable," and that the drawing of the observational-theoretical line at any given point is an accident and a function of our physiological make-up, our current state of knowledge, and the instruments we happen to have available and, therefore ... it has no ontological significance whatever.

One other problematic aspect of maintaining the existence of a neutral observation language is that the meaning of an observation term is usually derived from a specific theory, and therefore is not theory-neutral.
or objective. As an example, consider the term "response" as behaviorists use it. When a behaviorist observes a "response" he is not just saying that an organism was observed to do something, or acted in a certain way, but that the act of this organism was controlled by a certain environmental contingency and approximated the goal behavior the experimenter is attempting to shape. Observing an action as a "response" or alternately, as a fact that has any kind of scientific significance, is dependent upon being a behaviorist, which itself entails certain theoretical or methodological commitments.

In fact, Brian Mackenzie characterizes the training of behaviorists as necessarily interpretive, insofar as they must learn to see incipient movements toward desired behaviors in the physical activity of organisms, and thus read the meaning of these acts. He concludes, somewhat unconventionally, that "the practice of behaviourist psychology has long been based on an unrecognized and covert kind of phenomenology."33 The fact that the meaning of "response" is embedded in a theoretical system is not a criticism of behaviorism; the observation that their methodological beliefs cause them to deny this, is. Dudley Shapere summarizes the consensus of philosophers of science when he says that "the distinction between 'observation' and 'theory' has proved to be unclear, partly because what [are]
considered to be 'theories' are often treated in
science the way 'facts' or 'observations' ('the given')
are."\textsuperscript{34}

All of the previously mentioned "unobservables"
are accepted as real by scientists because they have
either provided explanations for known phenomena
(planetary orbits, the multitude of species, etc.), or
have allowed the making of predictions which have
subsequently been validated. It is worth noting here
that Newton's concepts of "attractive and repulsive
forces, action-at-a-distance, absolute space and time
. . . [were] regarded as tantamount to a reintroduction
of medieval occultisms into natural science."\textsuperscript{35} Yet
these same properties became the very embodiment of a
scientific explanation, thus demonstrating the manner
in which these conventions are, in fact, conventions
and do evolve.

It is the degree to which theories provide
accurate predictions, as well as account for the known
data, which equally determine if they are validated or
refuted. The processes of explanation and prediction
are considered to be logically equivalent in the
Received View and only "differ in so far as what is
predicted has yet to happen and what is to be explained
has already happened."\textsuperscript{36} Thus there is a symmetry
between explanation and prediction, a symmetry arising
from the logical relationship between observations,
theories and laws. A theory without predictive power is not considered to be explanatory, and is not a bona fide theory in this context.

This position regarding the nature of explanation probably conflicts more with actual scientific practice than any other aspect of the Received View. There are two objections to the symmetry thesis: one relates to the form of explanation characteristic of natural science and the other is more relevant to fields like biology and psychology. Taken together, they render it questionable whether the symmetry thesis can survive in any form.

The first objection was formulated by Sylvain Bromberger and involves showing how the logical picture of explanation (here referred to as the D-N model for "deductive-nomological") from which the symmetry thesis is derived is itself flawed, thus invalidating the symmetry thesis. Suppe describes Bromberger's account as follows:

Using geometric optics we can form a law of coexistence which correlates the height of a flagpole, the angle of the sun to the horizon, and the length of the shadow cast by the flagpole. Using this law and initial conditions about the height of the flagpole and the sun's angle, we can explain the length of the shadow in accordance with the D-N model. However, if we take our initial conditions as being the length of the shadow and the sun's angle, using the law the D-N model allows us to (causally!) explain the height of the flagpole.38

But there is no meaningful way in which the length of the shadow the flagpole casts can be said to be the
cause of its height. The D-N model, however, can not
distinguish this case from the valid causal explanation
of the length of the shadow given the height of the
flagpole. Thus the D-N model is defective and the
grounds for asserting the symmetry thesis—that
explanation and prediction share a logical form—is
invalidated.

The second objection to the symmetry thesis
involves examining the difference between causes and
reasons offered as explanations as formulated by
Robinson. The following account is taken from his
discussion:

Causes are purely natural phenomena in which the
element of 'agency' is neither discernible nor
necessary. Reasons, on the other hand, entail
'agency' and create a category of agent-causality
quite unlike the event-causality of the physical
world. 39

For example, while a causal account of Smith's
death would involve a description of the physiological
damage done by a bullet, a complete explanation must
include the reasons the assassin had for shooting him.
All social and historical events are accounted for by
human reasons, intentions and motives. Events which
can be accounted for purely by reference to the
physical properties of the people and objects involved
are not historical or social events. They are what
Robinson describes as "natural" ones.

Normally it is felt that if we give the reasons
for a social or historical event we have explained it.
The Yankees lost last night because the owner berated the players before the game causing them to lose confidence and play poorly. Although this would seem to explain the loss, the team could just have easily banded together and played their best game of the season to "show that S.O.B. owner." The explanation—though potentially correct—makes no prediction about what will happen if the circumstance is repeated. Because the explanation (as well as the owner!) lacks a logically deduced consequent—and in fact can support mutually exclusive outcomes—it would not be considered a true explanation.

However, this type of explanation is crucial to the theories of evolutionary science, geology and psychoanalysis, among others. These disciplines provide historical explanations of past events without making singular predictions about the future. Whether one considers the lack of predictive power in these theories as evidence of deficiency in either the Received View or of psychoanalysis and evolutionary theory respectively, depends on whether one is a philosopher, psychoanalyst or biologist. This difference in what constitutes a legitimate explanation will be crucial in the later discussion of Music Therapy.

Consider a typical event in a Music Therapy session with an autistic child sitting in the corner
holding his ears, or in some other way attempting to shut out the therapist's presence. If the child initially was passively ignoring the therapist, a possible explanation for the current situation would be that contact had been made with the child, as he had moved from a passive, uninvolved state to one of active avoidance. In fact, Nordoff & Robbins utilize this type of explanation in correlating their degree of resistance with the strength of the therapeutic contact.

Now it might have alternately occurred that the child manifested this increased contact by approaching the Music Therapist and sitting on the piano bench. The explanation then allows for two, mutually exclusive states of affairs. One cannot predict whether increased contact will lead the child to sit closer to the therapist or to avoid him. Because this explanation allows for varying predictions, it would be considered illegitimate or a non-explanation.

One possibility is to say that increased contact will lead the child to respond in some way to the therapist, whether this is through increased involvement or increased avoidance, and therefore the explanation is making a singular prediction. However, the attribution of contact in mutually exclusive behaviors would not meet operational criteria for a theoretical term, and therefore would be inadmissible.
Alternately, two competing theories might, in some cases make the same prediction. Consider two Music Therapists, one who is concerned with shaping behavior and plays in a minor key whenever his client comes closer to the piano bench, and a second, humanistic therapist, who is concerned with activating inner potentials and who plays in a minor key whenever she feels contact with the client. It would be reasonable for both of these therapists to predict--albeit for different reasons--that use of minor keys will engage the child. But since these theories would make a similar prediction, on the Received View they are formally equivalent and there is no meaningful distinction between them. Although they obviously consist of radically different assertions and presuppositions, adherence to the doctrine of symmetry requires that they make variant predictions in order to be considered theories that differ from each other in any meaningful way.

The doctrine of the symmetry between explanation and prediction implies a very specific theory of causation known as the successionist view. This theory—which owes its genesis to the empiricism of Hume, among others—holds that events which are said to be causally related have no connection to hold them together other than the fact that they have been observed to occur in succession. The fact that we tend
to attribute the quality of necessity to cause/effect relationships is completely due to our psychological dispositions and not due to any empirical quality of the external world. Thus, it can be said that "positivistically oriented experimentation aims at establishing functional relationships between variables and is not concerned with underlying causal mechanisms."  

In order to make this position more clear it will be contrasted with the opposing view known as the generative theory. Here the events in a causal relationship have a necessary connection because there is a mechanism connecting the two. The causal relationship is internal to the two events here, while in the successionist view the causal connection is imposed from without by the observing agency. Perhaps returning to our baseball team will make this distinction clear, as well as demonstrate how it is dependent upon the explanation/prediction symmetry.

Suppose one attributed the Yankee's loss on the above-mentioned night to the fact that the opposing starting pitcher was under thirty, and that this team has lost all season long when the opposing pitcher was under thirty. One might argue that this fact, while true, is only the result of an accidental correlation and therefore is not legitimate as explanation. Yet, on the Received View, it would be more legitimate than
blaming the owner because it both accounts for the previous losses and makes predictions that can be tested. One might argue that there is no mechanism linking the over/under thirty status of the pitcher with how well the Yankees play, yet this would be considered a spurious objection because it misconstrues the nature of what is meant by a causal connection.

Rom Harré notes that these two views of causality differ most widely over whether they admit causal powers or agents into their . . . world. [Successionists believe that] things are passive and effects are what happen to them brought about by influences from outside. . . . The generative view sees materials and individual things as having causal powers which can be evoked in suitable circumstances. 42

This distinction was certainly evident in the discussion of teleological vs. mechanistic explanation. The former position allows references to the internal states of phenomena in explanation; the latter position forbids it. We can also see strong elements in the behavioral programme of research as having its seeds in the successionist view. Behaviorists attempt to explain human actions based upon reactions to external environmental stimuli. References to internal beliefs, purposes and desires are prohibited, not only because they are in theory unobservable, but because, by definition, a cause is external to the individual which is the locus of the effect. The difference between behavioral therapies and psychodynamic ones can be seen in terms of where they believe causation lies, and
hence where they focus their intervention strategies. In psychodynamic and humanistic therapies individuals are seen as possessing the causal determinants of their actions and therapeutic interventions are directed internally.

Holding the successionist view of causation necessarily commits one to the **fictionalist** position (alternately referred to as **instrumentalism**) regarding scientific theories. This view is so named because the entities in theories are considered to be convenient fictions or instruments of calculation. The fictionalist holds that the entities and processes referred to in theories do not refer to anything with a real, independent existence. Their purpose is merely to function as calculating devices to account for past observations and to provide for accurate predictions of future observations. If the Yankees always lose to pitchers under thirty or to those with last names beginning with the letter "L", this is as valid a theory as any to explain their loss, if it makes accurate predictions. The inability to provide a mechanism connecting the age of the opposing pitcher with the outcome of the game in no way militates against acceptance of this theory. In the clinical example cited above it would make no sense to ask which theory (behavioral or humanistic) is correct. The function of both is to serve as a guide for clinical
intervention, not to model an actual process in the external world.

As can be seen in this example, one problem with the fictionalist view is that it cannot differentiate between theories that produce accurate predictions based on premises that are nonetheless false, from those theories that produce accurate predictions because they are actually true! The realist position, on the other hand, holds that the theoretical entities and processes referred to in theories really do exist. The theory is not right or wrong solely on the basis of its explanatory or predictive power, but according to how well it actually conforms to the state of the world it purports to describe.

One last issue that needs to be discussed here is the character of the laws of nature that scientists discover and use in an explanatory context. Scientific laws refer to classes of individuals that are not spatiotemporally restricted in any sense. The laws of physics refer to all atomic particles and their constituents, because these entities do not differ from one another in any way that will affect how they will react to particular conditions or forces. An electron is an electron is an electron.

The reason why classes are of interest to scientists—as opposed to individuals—is that "the main business of classical science is
generalization." Scientific interest is not generally interested in the unique, whether this is represented by an individual, or a property of an individual. Any individual, whether an atom, molecule, organism or planet, is only of interest insofar as it functions as a sample of the representative class—a sample that is interchangeable with any other representative of its class.

This universal type of law that deals with homogenous classes of individuals is called a process law because it "permits one to infer the state of a system with respect to the values of certain variables at any time whatsoever from knowledge of the state of these variables at any one time." This should recall the description of the perfectly deterministic universe discussed above.

That natural laws should refer to all heavenly bodies, not just the planet Venus, and to all molecules, not just H2O, seems reasonable—this is what is meant by saying laws refer to classes, not individuals. A problem arises when one applies this thinking to biology and psychology however. Species are unique products resulting from specific ecological factors. They are spatio-temporally restricted, being identified by a contiguous geographical and genetic relationship among members. Any one species is more like an individual than a class. Yet just as there
cannot be a law of nature restricted to the planet Venus, "there cannot be laws about particular species." This would seem to preclude even the possibility of a scientific psychology because the study of homo sapiens, as a species, could never lead to the discovery of bona fide scientific laws.

This view of the nature and importance of laws is important to the subsequent analysis of behaviorism as basis for Music Therapy research. One can control (to some extent) the environmental contingencies that people are exposed to. One cannot control how these contingencies are experienced. By focusing on environmental determinants of behavior, behaviorists attempt to treat humans as if they were an homogenous class, and therefore susceptible to the formulation of deterministic laws. If one's goal is the formulation of exceptionless laws, then differences among individuals of the relevant class will be seen as impediments to knowledge. They will be considered confounding variables to be controlled, not phenomena worthy of study in their own right.

This state of affairs brings two important questions to mind. The first is, assuming that the entities studied by biology and psychology actually differ from those studied by the physical sciences along the lines of this individual\class distinction, does this necessarily prohibit the discovery of laws?
If the answer is yes then we need to ask if these disciplines can be considered scientific, whether or not they reveal the type of natural law typical of the physical sciences. The difference is more than one of semantics, especially in respect to an applied field like Music Therapy that is constantly fighting for the legitimacy and respect typically granted to scientific disciplines.

**Received View: Postmortem**

All of the previously mentioned positions have generated a large body of literature in which there are claims of refutation of every aspect of the Received View. Only those issues that bear directly upon the philosophy of research in Music Therapy will eventually be taken up in this study.

As an accounting of all of science it is generally agreed upon by philosophers of science that the Received View is "fundamentally untenable." Yet no all-embracing philosophy has replaced the Received View because "there is no consensus of opinion as to what would constitute an adequate account of theories."

In approaching their work as a *rational reconstruction* of theories, proponents of the Received View sought to set logical criteria for what must be the case if science is to proceed in a logical manner. Since this attempt has failed it would seem reasonable
to take the opposite tack which is to examine actual scientific theories as they are used, and try to abstract a method from this historical material.

The problem here is that one must presuppose criteria for what one considers actual scientific theories in the selection of historical examples to be explained. There is a problem of circularity here that would seem to preclude the forming of any consensus on the nature of science. The philosopher who holds that evolutionary theory and psychoanalysis are legitimate examples of science will wind up with a much different picture of science from the philosopher who feels that his analysis need not account for these latter areas of inquiry.

In spite of this limitation, Suppe notes that many of the alternatives to the Received View are based on the idea that "an adequate analysis of theories should characterize theories as they are actually employed in science."49 Some philosophers who have actually looked at the variety of types of theories feel that it will not be possible to provide "a comprehensive analysis of theories which displays deep properties common to all theories."50 This observation brings us back to the question that began this chapter: One philosophy of science, or many?
Kuhnian Philosophy of Science

Paradigms

The view of scientific development associated with the Received View is termed the "thesis of development by reduction."\textsuperscript{51} This thesis holds that progress occurs when highly confirmed theories either become extended to a wider scope or become incorporated into more comprehensive theories. This picture of science maintains that progress occurs in a cumulative fashion and older theories are neither rejected nor demonstrated as false by newer theories; older theories are "superceded by more comprehensive theories to which they are reduced."\textsuperscript{52}

Thomas Kuhn's study of the history of science led him to reject this notion of development in science. He saw the unit of scientific progress in broader terms than that of an individual theory: Scientists instead group themselves around general paradigms. The view of scientific progress recommended by the Received View was precluded, however, by the nature of paradigms.

The concept of a paradigm as that which provides unity to scientific communities was introduced by Kuhn\textsuperscript{53} because in studying such communities he could not discover "enough shared rules to account for the group's unproblematic conduct of research."\textsuperscript{54} The term "paradigm" has proved to be an extremely plastic one
used in a variety of confusing ways by both Kuhn's supporters and critics. Subsequent analyses revealed that Kuhn himself has used the term in a myriad of ways, which he acknowledges, yet Kuhn observes that the various uses of "paradigm" fall into one of two sets.

In the first sense of this word, paradigms are "universally recognized scientific achievements that . . . provide model problems and solutions to a community of practitioners." Examples of well-known paradigms in this sense include Newton's laws of motion and Darwin's theory of evolution, though a paradigm need not exist on this grand a scale. Kuhn has alternately referred to this sense of a paradigm as an "exemplar." The second sense of "paradigm" refers to the "characteristic set of beliefs and preconceptions . . . including instrumental, theoretical, and metaphysical commitments" held by a particular community of researchers. In order to avoid confusion, Kuhn has since coined the term "disciplinary matrix" as an alternative to this usage of "paradigm."

Merton separates the metaphysical commitments—which are usually unquestioned suppositions—from the rest of the disciplinary matrix to produce a tripartite division of "paradigm" which results in a somewhat more clear conception than Kuhn's. The unquestioned beliefs in a paradigm are its most basic manifestation,
the disciplinary matrix (consisting of, among others, instrumental and theoretical beliefs, experimental techniques, formulations of laws and physical constants, and standards of explanation) is more restrictive and must be contained in the metaphysic supporting it, and the exemplar is more restrictive still and represents the most specific use of "paradigm." Kuhn feels that it is the exemplar which is most significant and should draw the attention of historians and philosophers.

Because "paradigm" has proven to be such a malleable term, some argue that its wide-spread utility has resulted not from its ability to accurately account for a wide range of historical material, but instead derives from its tendency to be all things to all people. It is the very imprecision of the term which Kuhn's critics have latched onto and to which they attribute its wide-spread applicability. Since I feel it is crucial to have an accurate sense of how "paradigm" is being used in this study, I would like to briefly elaborate on the nature and functions of paradigms.

An alternate reason for the variety of usages of "paradigm" is that Kuhn has used the term to simultaneously refer to the content of a scientific achievement as well as to the function of this achievement for the research community. In
determining the evolution and development of scientific disciplines the function

of an exemplar is to permit a way of seeing one's subject matter on a concrete level thereby allowing puzzle solving to take place . . . but puzzle solving can only be carried out if a community shares concrete puzzle solutions, or exemplars. 61

In this way, the ongoing work of science is guided by its accepted exemplars.

The three levels of a paradigm maintain a functional, hierarchic relationship of an organic nature. The content of a worldview places restrictions on the types of disciplinary matrices it will support which in turn restricts the type of exemplar developed. A rather crude example can be seen in psychology where a predominantly rational world view characteristic of western thought gave rise to a logical positivist philosophy (among others) which has been invoked as support for behaviorist methodology. A Skinnerian behavioral model, as an exemplar of sorts, is not necessitated by its metaphysical roots but is only possible given such roots.

Puzzles resistant to solution through the application of a given exemplar are known as anomalies. When such anomalies are resolved through the revolutionary overthrow of an existent paradigm by a radically different one, fundamental changes radiate outward to alter the original disciplinary matrix and metaphysical foundation. Though the structure of the
three-leveld paradigm is hierarchical, in revolutionary periods of science the forces of transformation work from the specific (exemplar) to the that which is general and more fundamental: the disciplinary matrix and metaphysical foundation.

In this study, I hope to provide the conceptual foundation for a paradigm (in the sense of an exemplar) for Music Therapy. The unity and progressiveness supplied by a paradigm can only result from empirical research and is not something that can be legislated in advance of these efforts. Kuhn makes it clear that if researchers feel "that they can improve the status of their field by first legislating agreement on fundamentals and then turning to puzzle solving, they are badly misconstruing [his] point." Such researchers would be mistaken because they would be ignoring the fact that professional consensus over research methodology "emerges from nothing more or less than concrete instances of highly successful scientific practice."62 Gary Gutting further characterizes Kuhn's advice to social scientists struggling to bring scientific legitimacy to their fields: "Forget about trying to figure out strategies for becoming mature sciences and get on with the job of doing good individual pieces of scientific work."64

I agree with Kuhn's position here and therefore I am not presenting either testable hypotheses or a
specific research method in this study. Yet within these restrictions there still exist a few tasks that a conceptual analysis like the present one can legitimately and fruitfully address.

First, I hope to free Music Therapy researchers from a necessary allegiance to any imported view of scientific method in general, and from specific conceptions of psychology and Music Therapy in particular. Though we must have standards in Music Therapy research, lacking an existent paradigm we are left with the problem of where to draw these standards from. Until the present, the answer to this dilemma has been found in the employment of alien psychological and philosophical systems—systems whose foundations are not necessarily consonant with illuminating the creative, musical and expressive processes characteristic of Music Therapy practice. The alternative argued for herein is that progress in Music Therapy will only come about from turning to our experience as clinicians as a source for methodological standards.

Second, I will present an indigenous portrait representing my clinical experience in order to suggest how we can most fruitfully construe this discipline. The concrete exemplar (and by this I do not necessarily mean a single theory but perhaps a body of interrelated ones) is more likely to emerge when researchers have
some sense of what it is essential for any successful Music Therapy theory to account for and which elements of practice are of peripheral concern. We must have some idea of where to look for the essential aspects of Music Therapy practice before we can hope to discover them. In order to further this end, a general research direction and orientation will be derived from the view of clinical experience presented herein.

Although conceptions of the components of science— theories, laws, the nature of observation and explanation, models, etc.—are constantly changing, it does seem that scientific activity is characterized by the perennial use of such structures. We have no reason to believe that a science of Music Therapy will be any different. Yet what we do not know is what conceptions of explanation, observation, and the role of theory will be suggested by a Music Therapy exemplar. We must articulate some standards here, and both work towards them and with them, knowing that their function is to become obsolete once the exemplar emerges. By establishing conceptions of explanation, theory and observation that are consonant with the indigenous clinical portrait contained herein, a general working methodology is suggested, which, though flawed from its lack of embodiment in an actual exemplar, is still superior to a method derived from extrinsic concerns.
In sum, I hope to facilitate the emergence of a paradigm for Music Therapy by 1) providing a rationale for developing an indigenous methodology, 2) suggesting a portrait of clinical practice untainted by obeisance to extrinsic descriptive and explanatory standards, and 3) discussing potentially fruitful avenues of research. I am engaging in all three of these tasks knowing that, if successful, the analysis and guidelines contained herein will be rendered obsolete through being superceded by the actual empirical efforts supported by this study.

The concept and actual existence of a Music Therapy paradigm is obviously a crucial element of this study. It is the element around which I have chosen to organize and conceptualize the activity of scientific research. Yet the existence and nature of paradigms--as described by Kuhn--is not something that philosophers of science have reached a consensus on.

In the three sections that follow, comprising the nature of research communities and the nature and necessity of an indigenous research paradigm, I have outlined some of the criticisms and elaborations of Kuhn's work, and have shown which debates directly impinge on the utility of employing the concept of a paradigm in this study. In other words, though there exists controversy in philosophic circles about Kuhn's work, not all of these issues directly effect the
usefulness of employing the concept of a paradigm as an historical/conceptual descriptor. I have tried to provide a sense of the nature of these controversies, as well as defend the use of the concept of a paradigm that is independent of the manner in which these controversies are ultimately resolved.

Research Communities

Subsequent to Kuhn's initial formulation of paradigms, there has been a proliferation of descriptive entities that have been proposed to account for both the unity of scientific communities and the progressive nature of the knowledge gained by these communities. A partial listing would include the following: research program;\textsuperscript{65} collective concept, scientific discipline;\textsuperscript{66} themata;\textsuperscript{67} research tradition;\textsuperscript{68} science as perception-communication;\textsuperscript{69} scientific domain.\textsuperscript{70} Although I do make extensive use of both Shapere's and Laudan's constructs in Chapters V, VII and VIII, it is obviously Kuhn's construct around which the focus of this entire study has been formulated. Because Kuhn's ideas are by no means generally accepted in the philosophy of science and more recent constructs build on the perceived deficiencies of Kuhn's, I would like to briefly support my use of the concept of a paradigm in light of the existence of (possibly superior) alternatives.
Criticisms of Kuhn have focused on two areas. The first of these involves the belief that if paradigms function in the manner portrayed by Kuhn, then there is no way to account for the progressive nature of scientific knowledge. That is, if commitment to a paradigm involves assuming the validity of certain meta-theoretical beliefs and the history of science involves a succession of these ontologically equivalent world views, then one cannot characterize the activity of science as representing an increase in knowledge or approach to truth. Here, it is not the character of paradigms that is in dispute, but the manner in which they replace each other. In this regard, Putnam acknowledges that "Kuhn's most controversial assertions have to do with the process whereby a new paradigm supplants an older paradigm."71

Though Kuhn's critics hold that the very definition of a paradigm implies a subjective/relativistic view of scientific knowledge, Kuhn himself does not believe the nature of paradigmatic succession to be arbitrary, and Laudan72 demonstrates that even if two research traditions are incommensurable—that is, do not possess enough of an empirical/conceptual overlap to imply variant testable predictions—one can still formulate criteria for determining which tradition is more progressive. In other words, "the jury is still out" on whether or not
"paradigms" as construed by Kuhn, imply a view of scientific knowledge as being purely subjective, irrational or relativistic.

Moreover, even if such a relativism is a consequence of an analyses employing paradigms, this is not a criticism of the concept of paradigms as a valid historical descriptor. As we shall see with Laudan (in Chapter VIII), it is possible to maintain a notion of scientific progress without taking a stand on whether this progress embodies successively closer approximations of truth. The concept of a paradigm retains its usefulness regardless of its epistemological implications.

Second, Laudan has noted the "perennial co-existence of conflicting traditions of research," thus arguing against Kuhn's claim that normal science involves the dominance of a single paradigm. If true, this observation would seem to contradict the claim of the present study that the establishment of a singular (indigenous) paradigm is a necessary precursor of progress in Music Therapy research. This dispute is ultimately over the correct view of the history of science, and I do not believe that its resolution, one way or the other, bears on the current study.

I say this because even if it is the case that a multiplicity of paradigms is more the rule than the exception (and Kuhn does not deny that certain stages
of a science contain such a multiplicity—it is a state of affairs characteristic of both incipient and revolutionary stages of development) what I think is inarguable is that progress proceeds along the lines of only one of any number of contemporaneously existing paradigms. It is Darwinian evolution and Newtonian physics which represent progress in science and whose traditions have been built upon and, in some cases, transcended. The existence of competing paradigms does not mean that it is not one of these paradigms that ultimately survives, develops and becomes the thread upon which scientific progress continues. Therefore, even if Laudan's historical point is correct, it does not follow that this supports the inherent superiority of paradigmatic multiplicity.\textsuperscript{74}

An important pragmatic consideration in employing the concept of a paradigm in an analysis of Music Therapy research is that Kuhn's work has been applied to psychology in general\textsuperscript{75} and Music Therapy in particular\textsuperscript{76} more than that of any other philosopher of science. Moreover, Guttering's representative assessment of Kuhn's contribution to the history of thought is contained in his statement that The Structure of Scientific Revolutions has had a "wider academic influence than any other single book of the last twenty years."\textsuperscript{77} Since there is a tradition of discussing the various schools and issues in these sciences in terms
of competing paradigms, Kuhn's ideas are more well known among non-philosophers than are those of any other philosopher.

As a consequence, the concept of a paradigm has taken on a life of its own and is no longer used solely in the manner intended by Kuhn. That scientists work within paradigms—when "paradigm" is used in a generic sense—has come to represent any theory that holds that the activity of science requires the employment of conceptual, metaphysical, epistemological or any other non-empirically demonstrable set of beliefs. Although I will adhere to Kuhn's formulation as closely as possible, it is the qualities of this generic sense of "paradigm" which seem common to all of the descriptors of scientific communities noted above. As such, "paradigm" is the one which best represents an amalgam of these various philosophical conceptions. And essential to this amalgam is the recognition that methodological commitments are not independent of ontological commitments and empirical claims; instead, all three of these types of beliefs maintain a mutually supportive relation in facilitating scientific growth.

Finally, Kuhn's view that science cannot be explicated by appeal to rule-governed procedures and his search for an alternative unifying ingredient are completely consonant with the view of Music Therapy practice that is promoted herein. The practice of
Music Therapy represents a highly specialized form of human interaction and knowledge acquisition, one whose postulated mechanisms must be adequate to its products. Like Kuhnian science, a portrait of Music Therapy that does not refer to the implementation of formal rules in characterizing clinical practice can only be criticized as irrational if one equates rational behavior with that which is rule-governed. The challenge for Music Therapists—as for Kuhnian and other post-positivistic philosophers of science—is to develop alternate notions of rationality.

This approach to Music Therapy research—and the explication of science in Kuhnian terms—is actually more rooted in empirical concerns (and this is contrary to conventional wisdom) than is that of the two respective alternatives; behavioral Music Therapy research and logical positivist philosophies. Kuhn requires that the mechanisms of scientific change be, first and foremost, adequate to explain the history of science; he gives primacy to adequacy to the data and not to a priori notions of rationality. Similarly, in this study I argue that the theoretical mechanisms proposed to account for Music Therapy process must be adequate to the nature of the experience of clinicians while engaged in the process; adequacy to a priori notions of what constitutes legitimate scientific, medical or therapeutic practice must be secondary.
Empiricist methodologies are criticized in this study, yet the view of science promoted is still rooted in empirical concerns.

The Nature of an Indigenous Paradigm

Though I do not believe that it is either legitimate or necessarily fruitful to derive methodological guidelines for Music Therapy research from any philosophical analysis of science, there is nonetheless an important role that can be served by the comparison of an indigenously derived methodology with existent philosophical conceptions. That is to show that the novel methodology has a plausible—though not a metaphysically inviolable—foundation. For example, it will be subsequently argued that because philosophers have demonstrated that there is no a priori distinction between observed and theoretical entities, the most important objection to mentalistic and introspectional research is invalid. This observation is used in the present study to corroborate, but neither to prove nor demonstrate the validity of, the indigenously drawn conclusion that the nature of Music Therapy process requires introspectional observation and explanation. In a formal sense, we can see that statements that make "X" true are not logically equivalent to statements that make "X" merely more plausible. It is this latter
function that non-indigenously based arguments can serve.

The distinction is drawn between deriving explanatory elements from other disciplines and showing that these elements are merely consonant with the explanatory entities (models, theories, psychic structures) in other philosophies or sciences. It is this latter operation which is a legitimate part of an indigenous research paradigm. As an example, observing that the characteristics of Nordoff & Robbins' concept of the "music child"79 are similar to Maslowian concepts of self-actualizing drives is to enhance the explanatory power of both conceptions by showing the ability of a similarly constructed concept to be broadly applied. Some might argue, however, that attributing explanatory significance to Steiner's theories concerning musical intervals80 would be an invalid component of an indigenous paradigm. It is an open question whether or not such claims go beyond that required by a parsimonious appeal to the nature of Music Therapy process. A criterion for determining the validity of applying "found" constructs should be an affirmative answer to the following question: Would I have arrived at such a construct purely from considerations of Music Therapy phenomena?

The Music Therapy researcher should not borrow explanatory mechanisms (models, theories, laws) and
aids to research (methodological constraints, research
designs and methods) from other realms of inquiry,
unless such tools are demonstrated to be consonant
with, and implied by, theory-neutral descriptions (see
below) of Music Therapy process. Using such imported
constructs restricts observations to particular
categories of experience—categories to which the
phenomena of interest may not be naturally related.

As long as a methodological principle or
theoretical entity is derived from the nature of Music
Therapy process its indigenous nature is maintained.
When citing the consonance of Music Therapy concepts
with those in related fields we must avoid the
temptation to adopt the system in which the concept is
embedded. This is the very tendency that the present
study is being undertaken in reaction to.

A major problem with this criterion is that there
really is no sense in which it can be said that
scientific activity is theory-neutral. Where one looks
for data is determined by the theory one wishes to
investigate. All possible descriptions of the natural
world are not of interest to science; only those
descriptions are of interest which bear on a particular
theory or unsolved problem. Moreover, contemporary
philosophers of science consider observation itself to
be a theory-laden activity. The very meaning of
observation terms—such as energy, mass and
stimulus/response—is inextricably linked to the theories that such terms figure in. One cannot even employ an observation term without reference to a pre-selected theory. How can we then begin to formulate criteria for legitimate indigenous methodological and theoretical guidelines? There appears to be a circularity of reasoning here that precludes such a formulation.

Perhaps Laudan's observation that there are different orders of theories can be of use here. That any type of theoretical explanation of Music Therapy process will have to deal with the nature of musical meaning is a theory of a sort—and is in fact one that will be subsequently defended in this study. But it is also possible to consider this a minimum requirement of any adequate explanation in Music Therapy. That is, one can make a plausible argument that creative Music Therapy techniques involve utilizing a (musical) language whose referents are not pre-determined by convention. Any adequate theory must account for the mechanisms by which the genesis and use of this language contributes to the therapeutic process.

On the other hand, we can envision adequate explanations in Music Therapy that do not refer to either mechanisms of repression and sublimation, or conditioning and extinction. These latter terms refer to specific components of theories having their origins
in realms unrelated to that of Music Therapy. The fact that they may solve important problems in one domain is no guarantee that they hold any explanatory relevance for Music Therapy process.

The activity of science involves making necessary assumptions that one can call theories or hypotheses, but they are not hypotheses that are being directly tested through the research effort (except insofar as the success of any research program reflects upon its conceptual foundations). The answer to the dilemma posed above is that theory-neutral descriptions are possible, but this neutrality holds only in respect to the second order hypotheses that are directly tested in the research effort. We can describe Music Therapy process independent of any particular theory of personality or psychotherapy, though that description will reflect a more basic commitment regarding the factors that any adequate theory will address. The very things we choose to describe (behaviors, images, feelings, responses, etc.) are determined by our higher order theory or assumptions. In sum, theory-neutral observation is possible; paradigm-neutral observation is not.

The Necessity of an Indigenous Research Paradigm

Inherent suppositions of this researcher are that Music Therapy research is in what Kuhn would call a
preparadigmatic stage of development and that growth in the field will be facilitated by the emergence of a dominant paradigm. This latter assertion does not find universal approval as some would argue that a multiplicity of viewpoints in a discipline is natural and even healthy.\textsuperscript{81}

Yet it is important to discuss which elements it is healthy to disagree over and in which areas a discipline requires unanimity for progress to occur. A brief discussion of the elements of pre-paradigmatic sciences will make it clear that Music Therapy is in fact in this stage. Once the characteristics of this stage are delineated, the importance of a dominant paradigm will be discussed.

Kuhn believes that "acquisition of a paradigm is a sign of maturity in . . . any given scientific field."\textsuperscript{82} In its absence "all of the facts that could possibly pertain to the development of a given science are likely to seem equally relevant."\textsuperscript{83} Consider the field of optics prior to Newton where "there was no standard set of methods or of phenomena that every optical writer felt forced to employ and explain."\textsuperscript{84} Consequently, each researcher was compelled to "build his field anew from its foundations."\textsuperscript{85}

The various schools of the preparadigm period do represent bodies of belief that can guide research, yet these belief systems are usually not derived from the
phenomena under study, but by "a current metaphysic, by another science, or by personal and historical accident." As a result, these research programs are incapable of generating the various constants and quantitative laws characteristic of progress in the natural sciences.

Areas of debate in this period are focused on what constitutes the "legitimate methods, problems and standards of solution" of the science. These debates reflect the incompatible definitions of the nature of scientific inquiry held by pre-paradigmatic schools. In contrast, when research is guided by a shared paradigm, its adherents are "committed to the same rules and standards for scientific practice."

Kuhn could have drawn his descriptions of pre-paradigmatic science directly from a study of Music Therapy. There is no received body of belief or data that various researchers are compelled to explain. Each theoretician/researcher must construct his own foundation and the field has yet to produce a progressive research tradition. I do not believe that we have significantly more insight into the nature of Music Therapy process--how and why it works--than did researchers working forty years ago during the birth of the modern profession of Music Therapy.

The differences between the various schools in Music Therapy are not reducible to which theory each
group believes better explains the same phenomena. We have not yet reached agreement over the nature of the phenomena (data), instead being occupied with debates over definitions and methodology, classic areas of concern in a pre-paradigmatic science.

Also typical of a field at this stage of development is the exogenous nature of existent theories. The sources of theories in preparadigmatic sciences described by Kuhn—metaphysics and other sciences—are exactly those employed by current research and theorizing. Behavioral theory, psychoanalytic theory, theories of vibrational healing, and spiritual disciplines all represent systems of thought not guided by the exigencies of explanation in Music Therapy. As such, their use as explanatory elements in Music Therapy represents a regressive, non-empirically guided approach to science.

Some might argue that the source of a scientific theory does not bear on its usefulness. If one finds a useful concept from another field, the argument goes, why not apply it to Music Therapy and determine its value through use, rather than banishing it because of its source in another realm of human inquiry. Yet, Hanson,90 among others, holds that theory creation is actually an important part of scientific activity. Progressive and useful theories do not just appear magically to scientists, nor are they acquired by
merely transposing a theory in one science to another, though reasoning by analogy is quite important. The emergence of theories reflects a combination of both creative and rational processes, continually guided by adequacy to the data one wishes to explain.

The answer to the previous objection is that the history of science demonstrates that successful theorizing does not come about in such a pedestrian manner as transposing theories across disciplines. Our activity as Music Therapy researchers includes the creation of hypotheses guided by our important research problems. In effect, Kuhn says that by definition, a paradigm which transforms a field into one that exhibits scientific progress is an indigenous one. As was noted previously, the early schools in a particular scientific field do employ paradigms, yet it is their non-indigenous character that necessarily limits their achievements.

Scientific inquiry is characterized by diversity of creative theoretical approaches. This is one of its strengths and guarantees that a multiplicity of research avenues are explored. Yet, unless there is basic agreement on fundamental areas of concern—such as definitions of data, methodological standards, and explanatory criteria—this diversity is counter-productive. If each researcher must expend efforts towards justifying their conceptions of data, and
choice of theories, methods, models and explanations--
in short, their overall metaphysical and methodological
context--then actual empirical work and progress is
inhibited. If our goal as researchers is to illuminate
the underlying processes and mechanisms of clinical
practice, it is clear that this will not occur by the
singular success or merging of existent (pre)paradigms,
but through the development of a body of thought whose
roots spring from the fertile ground of clinical
experience.


3. Ibid., 46.

4. Ibid., 48.

5. Ibid., 58.

6. This term was first coined by Frederick Suppe in his book *The Structure of Scientific Theories*, Urbana and Chicago: University of Illinois Press, 1977. I am not claiming that early Music Therapists have made reference to Suppe's work; just that the beliefs about science espoused by prominent Music Therapy theorists are consonant with those subsequently described by Suppe under the heading of the "Received View."


8. Suppe 3.


11. Leahey 25.


15. Leahey 145.

16. Leahey 146.

17. Hull 71.

18. Hanson 51.

20. Ibid., 131.

21. Ibid., 118.

22. Suppe 8.

23. Hanson 65.

24. Leahey 91.


27. Hull 7.


32. Mackenzie 232.

33. Mackenzie 169.


35. Mackenzie 44.

36. Harré 56.


38. Suppe 621.


42. Harré 121.

43. Leahey 470; Hull 48.


45. Hull 49.

46. Leahey 470.

47. Suppe 62.

48. Suppe 117.

49. Suppe 120.

50. Suppe 120.

51. Suppe 56.

52. Suppe 56.


63. Gutting 14.

64. Gutting 14.


68. Larry Laudan, Progress.


72. Laudan, Progress.

73. Laudan, Progress 36.

74. It is important to remember that paradigms—in the sense of disciplinary matrices—are not identical to theories. By arguing for the virtues of a singular paradigm I am not arguing against the virtues of a diversity in theoretical formulations.


76. See Kenny, Field and Ruud, Relationship.

77. Gutting v.
78. Although I have gradually come to this view regarding Music Therapy from internal, empirical reflection, there are also important conceptual reasons why adherence to procedural rules cannot explain the nature of creative clinical process (see Chapter V).


80. Bruscia, Improvisational 68.

81. See Bruscia, Defining; Ruud, Relationship.

82. Kuhn, The Structure 11.

83. Kuhn, The Structure 15.


86. Kuhn, The Structure 17.


89. Kuhn, The Structure 11.

90. Hanson, Patterns.
CHAPTER III

TRADITIONAL RESEARCH IN MUSIC THERAPY

The dominant impression that emerges from studying the literature on research in Music Therapy is that of important opportunities missed to establish a fruitful, indigenous research tradition, due to a fledgling discipline's attempts to seek validation in the external society. Positivistic views on science have predominated in Music Therapy, finding their way in through the adoption of a behavioral methodology. Although the use of behavioral principles is due, in part, to the manner in which important theorists conceptualized Music Therapy process, this conceptualization of clinical process was itself determined by a more basic methodological commitment. This commitment involved belief in such a thing as the scientific method and that through following this external recipe for progress, Music Therapy would attain status as an objective discipline respected in the medical community.

One can sense the tension in individuals such as Gaston and Sears, between their intuition about the importance of the creative and experiential aspects of
Music Therapy and the diminished role they assigned these elements in order to gain acceptance in the culture at large. Kenny supports this interpretation in noting that Gaston felt conflict between "his substantial wisdom of the authentic power of Music Therapy and pressures to lay the groundwork for accountable clinical practice,"¹ and that Sears recognized the importance of creative process and individual expression, yet he nonetheless limited his recommendations to utilizing operational measures to track socially acceptable responses in therapy.²

At times, the historical and social factors that influenced the adoption of certain philosophies have been as important as the intrinsic content of the positions themselves in determining the eventual course of Music Therapy research. Often, these various influences become intertwined so that it becomes difficult, if not impossible, to disentangle the historical and social factors from the philosophical ones. An example here involves the various definitions of Music Therapy held by important research theoreticians. It will be demonstrated that many of these definitions blurred the distinction between the processes of education and therapy and therefore made adoption of the behavioral approach seem more reasonable. What has been unclear until now is to what
extent these views were derived from (1) a priori views on the nature of therapy, (2) historical factors such as the reality that these definitions were formulated by academician/educators who were not themselves clinicians, or (3) actual empirical investigations, such as observing effective clinical work.

This distinction is of more than academic importance. Throughout this paper the attempt is made to avoid the temptation to criticize existent research in Music Therapy based on its roots in behaviorism qua behaviorism. It is a fundamental tenet of this study that a behavioral approach arising from indigenous concerns has more validity than one imposed arbitrarily or from extrinsic concerns. The historical material herein presented strongly supports the claim that the adoption of behavioral methods was in fact due more to social needs and historical accident than to indigenous considerations, i.e., to answer important questions posed by the nature of clinical practice.

The schism between research and practice in Music Therapy that was noted earlier also reflects a gulf between experimental research efforts and theories of clinical process. Music Therapy clinicians have created treatment theories primarily from psychodynamic and transpersonal perspectives, while Music Therapy research more often proceeds on behavioral lines. Thus, the formulations of important clinicians--such as
Paul Nordoff & Clive Robbins, Florence Tyson, Mary Priestly and Helen Bonny--are not directly evaluated in the research literature. This schism has led to the unusual situation that because this study is focusing on the foundations of current research, the formulations of important clinical theorists will not be directly addressed. It will be noted, however, where the critique of non-indigenous research overlaps with that of non-indigenous clinical theory.

Determining an Underlying Philosophy

At times, important research theoreticians--such as Gaston and Madsen--have made quite explicit statements regarding the conceptual foundations of the research programs they have supported. In other places, this philosophy is only implicit in their specific methodological recommendations and they therefore must be abstracted from such statements. In this chapter, the latent assumptions that have traditionally guided Music Therapy research will be made more visible, through investigating those areas of scientific activity in which one's metaphysical assumptions about the nature of science become manifest. What follows is a brief description of these crucial areas and their relationship to the present research effort.
The manner in which data has been delimited has often served to differentiate among the various schools in psychology. The same is true in Music Therapy: behavioral therapists are concerned with observable actions, analytically oriented therapists discuss the function of unconscious conflicts and historical trauma, and humanistically oriented therapists focus more on present feelings and patterns of interaction. The relative positions here reveal as much about the adherent's view of what it is possible to know, an epistemological position, as it does about which model of personality is subscribed to. It is the former, philosophical position that will be focused on in the ensuing analysis.

Similarly, the view advanced of the nature and function of theories can be an accurate barometer of one's underlying philosophy. Important issues here include whether or not theories must have predictive power, the ontological status of theoretical entities, the role of theory in treatment, and the differentiation between basic data and theoretical entities. In some philosophies the affective content of a client's music can be observed; in others it is felt; in still others it is an object of hypothesis; and lastly, it may be ignored as unreliable or irrelevant for research.
The question of what constitutes legitimate data and data-gathering methods is a crucial one. Are only public events (behaviors and verbalizations) appropriate here or can reports of subjective experience (that of the client, therapist, and third party observers/raters) be considered reliable? Related to this is the role of quantification and measurement. Must data be quantifiable to play an important role in an explanatory system, or does data of a qualitative nature take priority here?

Also important is the role and status of the clinician/researcher. Do standards of objectivity require that these roles be separate or is it legitimate (or possibly necessary!) to research one's own clinical work? Is a clinician an objective observer or do the therapist's clinical obligations require a degree of subjective perception inimical to the goals of research?

Lastly, it is crucial to examine the view of human activity implied by a particular psychological position. Specifically, whether or not it is felt that human actions are guided in a deterministic or non-deterministic manner will certainly impinge on what it is possible to know, explain and predict in this regard. It may be advocated that human actions have deterministic and non-deterministic components, and that science can only deal with the former type. The
important question will then relate to whether or not a science built on such a foundation can address the questions that are relevant to Music Therapists who deal with the entire range of human actions.

A full accounting of any research tradition would have to consider the logical, psychological and sociological forces at work in its participants. Purely logical accounts, i.e., rational reconstructions, examine only the content of a science and attempt to create a historical narrative by projecting contemporary scientific standards onto the historical personages. One only looks at the data to be explained and draws connections to the theories and methods advocated. A psychological account attempts to show how the personal, idiosyncratic histories of individuals led them to advocate for certain theories. As an example, consider how Einstein's theological beliefs led him to argue against probabilistic conceptions of the universe. Lastly, social forces external to the actual data of the science can influence and determine the theoretical development comprising its internal history.

In studying the Music Therapy literature it has become apparent that sociological factors--like the need for professional recognition, training and certification criteria--have exerted a disproportionate influence over the internal content of the discipline.
Although specific philosophic positions have been argued for, more often than not these positions resulted from social pressure and not intrinsic empirical need. Thus, the content of the present chapter is more heavily weighted to fleshing out the nature of these social factors and showing how they favored positivistic conceptions of science, conceptions that are in opposition to those suggested by a view of Music Therapy independent of these extrinsic pressures.

Three related themes will be simultaneously pursued in this chapter: The actual philosophy of science advocated by important theoreticians will be delineated in terms of the positions and categories articulated in the previous chapter. An attempt will be made to ascribe historical causes to the adoption of this program. Last, important implications of the manner in which this program was adopted will be articulated. Fundamental among these will be the fact that by using behavioral principles, Music Therapists unwittingly adopted a specific epistemology—positivism—which placed constraints on the resultant conceptualizations of therapy, personality and the role and purpose of research. It will be subsequently argued that both the manner in which it has entered the field—as an imported perspective—and the actual content—its epistemological foundation—has ensured
that the behavioral program is fundamentally unable to accomplish for Music Therapy the goal of illuminating the mechanisms of creative clinical practice.

For purposes of clarity the development of research methodology can be divided roughly into three phases. The first period begins with the years around World War II, a time where most writers place the origins of the modern profession of Music Therapy, and a time when the need for a thorough and scientific research effort was first being articulated. This first period, beginning around 1950, was characterized by a loosely psychoanalytic approach in treatment and repeated calls in the research literature for enhanced levels of systematic inquiry. In 1968, the publication of Gaston's landmark text and an article by Madsen, Cotter & Madsen, Jr., ushered in the behavioral period in Music Therapy. Behavioral theory predominated in Music Therapy in this second period, peaking in the years 1976 to 1978. The third period—typified by published dissatisfaction with purely quantitative and imported approaches—began in the late 1970s and can be seen in the statements of Hesser, Ruud and the New York University International Symposium that were discussed earlier. Since this last group of theorists maintain positions in opposition to the ones that emerged in the first two periods, their views will not be discussed in more detail here and the
present chapter should not be considered a complete history of the research tradition in Music Therapy, but an explication of the theorists adhering to traditional research standards.

**Early State of Music Therapy Research**

What was the climate in this first period, in which Music Therapists first began formalizing their research goals? Gaston describes the earliest post-war reports as being concerned chiefly with the imagined healing power of music. . . . Reports . . . of miraculous cures appeared in newspapers and some popular magazines, infrequently there was a report in a professional journal. . . . Seldom was there a description of the manner in which the music therapist approached the patient or how he used the music. 6

One of the dangers was that this misconception of Music Therapy led to clinical practices that Gaston felt were inappropriate. Obviously, a sound research base would serve to separate the charlatans invoking mysterious powers from the legitimate clinicians operating from known principles.

Fultz describes the then current literature as lacking "continuity, coherence, and the integrating values of a larger rationale and theory of music therapy . . . [with] too much bias and prejudice, too many didactic statements and not enough verified opinion." 7 Clearly, he is articulating the need for a paradigm in which to unite splintered research efforts,
as well as means for establishing empirical support for clinical claims. Fultz further argues that experimental research is needed to overcome the "confusion, poor orientation, poor delimitation, and poor organization of operational procedure with the same absence of the truth as has always existed."\(^8\)

In the context of reviewing the contemporary research literature, Michel says that the existence of Music Therapy has been demonstrated, but that few recent studies report investigation of the process. . . [and] the method of research has been clinical observation. . . . Results seem to have been interpreted largely by speculation alone and have seldom been classified for future use and replication of the study.\(^9\)

Michel could not find any current research reports on the actual use of Music Therapy. He acknowledges that while this is "the most important area for investigation it seems to be the one least subjected to scientific inquiry."\(^10\) This lack of sufficient research on actual clinical work remains a chronic problem in the literature.

In 1953 the NAMT proposed the "Long Range Design for Music Therapy Research." The stated purpose of this document was to "aid in the development of scientific knowledge in the field of music therapy . . . to show a logical, consistent sequence efficiently integrated toward a 20-year guided plan of growth."\(^11\) In expressing the hope that this plan would "crystallize and unify [Music Therapist's] efforts
along productive lines,"¹² Michel gave implicit agreement with the diagnosis of the current research as lacking unity and focus.

The plan itself does not seem to have had a lasting impact on research efforts, judged by the scarcity of future references to it. This is a somewhat confusing document that provided a classification system for studies relevant to Music Therapy, but omitted a concrete methodology or unifying theoretical perspective. The need for such a plan can be looked at in a diagnostic sense in assessing the state of research. Its authors attempted to supply, externally, the coherence and organization that a paradigm provides through its ability to suggest problems as well as strategies and standards for possible solutions.

The Research Agenda

The need to establish legitimacy in the eyes of the medical community was of paramount importance in early research efforts. As a result, researchers were exhortd to conduct more rigorously scientific studies utilizing controlled observation and experimentation.¹³ Since Music Therapists were subordinate to psychiatrists in the professional hierarchy, Pepinsky advocated the subordination of "therapeutic planning to the program of the psychiatrist."¹⁴ Elsewhere, he
quotes psychiatric authorities to the effect that patients have to be protected from the unsound medical use of music therapy and that the "musical prescription" should be written by a psychiatrist.

Clearly there was a power struggle for professional recognition and it was felt that only research following standard medical models would be acceptable to those in a position to grant legitimacy to Music Therapy treatment. In fact, gaining esteem with medical doctors was so important that the NAMT withheld reprinting of a paper entitled "Music Therapy—What and Why?" due to "adverse reaction . . . particularly from the medical community." In this regard it is important to note that the establishment of liaisons with medical and psychiatric associations—a task that might normally be assigned to a public relations or professional standards committee—was a function of the NAMT research committee.

Curiously, though Pepinsky notes that "even the psychiatrist is discovered to lack rigorous controls in the design of his experimentation," he still feels that research in Music Therapy should be limited and defined by research in psychiatry. He failed to see the irony in his suggestion that Music Therapists follow a strictly defined program of experimental research in order to placate a community that could not themselves adhere to such stringent standards. There
has been a consistent tendency in Music Therapy to adopt external guidelines, criteria and agenda in formulating research programs.

It is this tendency—seen alternately in the tendency to conform either to the positivist view of science, or to conceptions of what constitutes valid treatment evaluation—of allowing research to be guided by extrinsic needs, rather than by indigenous concerns, that has inhibited growth in Music Therapy. Rather than search for the inherent mechanisms of Music Therapy process, researchers are guided into a search for a justification for this practice. The value of research efforts has not been measured by the degree to which they provide understanding of actual practice, but by the degree to which they can be used as a rationale for treatment.

We can see this difference in terms of the fictionalist\realist dichotomy discussed earlier. Music Therapy researchers who are concerned with providing a rationale for treatment have selected evidence and utilized methods that conform to the world view of those whose professional sanction and approval is sought. Whether or not these methods reveal the inherent nature of Music Therapy process—the realist orientation—is of secondary concern, because what is most important is giving a plausible accounting of the data, a position conforming to the fictionalist view.
When Music Therapists adopt psychoanalytic, physiological or educational theories and language to speak with psychiatrists, medical doctors or teachers respectively, they are unconsciously adopting a fictionalist outlook. From the purely pragmatic perspective of gaining professional acceptance this step appears reasonable; from the perspective of attempting to expand knowledge of Music Therapy process it has proved to be hopelessly limiting.

The claim here is not that theoreticians were deliberately advocating a fictionalist view and were unconcerned with the nature of clinical process. What is being argued is that the subordination of research goals to extrinsic needs necessarily limited the ability of research to penetrate into the deeper and more significant levels of phenomena congruent with adopting the realist perspective.

Fultz comments that what is important is "the development of an adequate rationale for our work." Because the early research efforts were oriented towards justifying clinical practice rather than discovering its underlying mechanisms, researchers took the view that valid treatment depended upon their efforts. In this regard, George says that "the primary reason that persons engage in research is to make decisions or value judgments about individuals or treatments." Gfeller warns that clinicians "should
not be content to base practice on principles that have had limited empirical examination"\textsuperscript{22} as determined by the lack of "data-based research."\textsuperscript{23} Duerksen says that "research results provide the best source of validated suggestions available to the clinician for improved practice in music therapy,"\textsuperscript{24} and quotes Levin's statement that "when practice runs ahead of knowledge, there is the danger that arrogance, dogma and ritual may replace . . . the search for knowledge and experimentation."\textsuperscript{25}

What is reasonable and uncontroversial here is that clinical work should proceed from a state of knowledge. The further step taken by the authors quoted above--the validity of which is not so self-evident--is to equate knowledge with that which can be stated and verified in the language of experimental research. The explicit message here is that what the clinician does know does not qualify as legitimate knowledge until verified experimentally. It is also suggested that the purpose of research is to supply a body of knowledge from which clinicians can draw in order to form their therapeutic interventions.

Although this issue will be discussed later, it can be said at this point that the above account is not adequate in describing the actual relationship that exists between research and practice in Music Therapy. While clinical practice has grown significantly in the
last forty years, the development of a research base--as evaluated by clinicians--lags significantly behind. Since the profession of Music Therapy continues to flourish in the absence of an empirically validated data-base, it is apparent that the conceptualization of research as providing a justification for treatment needs to be re-evaluated.

Methodological Concerns

Part of the problem here reflects the fact that a research methodology tends to become an epistemology. Smith\textsuperscript{26} notes that Clark Hull--on of the two or three most influential behaviorists--advocated fiercely for the unity of scientific method and that these pronouncements "gradually sounded less like recommendations for scientific praxis and more like logical imperatives."\textsuperscript{27} The research tool is not seen as one possible way to acquire knowledge, but becomes the only way that valid knowledge is obtained. Thus, the preconceptions or metaphysical commitments inherent in any research method become projected on the phenomena.

An example of the way this process has occurred in Music Therapy can be seen in Asmus, Jr. & Gilbert.\textsuperscript{28} These authors present a model in order to provide "a framework for conceptualizing clinical practice."\textsuperscript{29} The steps involved here include (1) assessing
unacceptable behaviors through examining records, assessment devices, observation of the client and consultations with other professionals (2) data collection in order to (3) quantify the prevalence of the targeted behavior, leading to (4) the establishment of a baseline for the behavior for which (5) goals are established that are (6) translated into measurable behavioral objectives.

The client is rendered in completely passive terms in this model. A wide variety of sources are used in determining problematic behaviors––only omitting asking the client! The client has pre-tests administered, intervention techniques applied and behaviors modified. The direction of causality flows from the external world onto the passive client, the paradigmatic description of a mechanistic system.

What is apparent is that the steps of this procedure function better as a description of the steps of experimental research than of actual clinical practice. The form of experimental research has passed from being a vehicle for discovery into the actual process it is utilized to illuminate. Of course if clinicians actually worked in this manner it would be easy to formulate experimental designs with a high degree of relevance, yet it is of questionable value to fit the phenomena into our forms of knowing, rather than expand these forms to accommodate a wider or
deeper range of phenomena. A point is then reached where the presentation of clinical process assumes a particular form because it is amenable to empirical investigation, not because the form reflects actual clinical practice.

An analogous situation would consist of an automobile driver who is using a convex mirror in order to get a better angle for viewing the cars behind him. The curve of the mirror allows for seeing otherwise hidden areas, although it distorts the shapes and spatial relations of the objects it reflects. If the driver then began describing the world itself in terms of these distorted shapes, his position would be akin to the researcher who "discovers" that clinical process involves procedures akin to those of experimental research. In both cases it is the tool being described, not the phenomena.

Strict reliance on behavioral methodological dogma can lead to the establishment of constructs in this manner. Mackenzie\textsuperscript{30} notes that conditioned behavior chains are frequently interrupted by behaviors that were never reinforced, when an animal is removed from the experimental milieu and placed in a complex situation more similar to its natural environment. In addition, animals have proved to be resistant to being conditioned at all, except in the relatively simple experimental apparatus employed by the researcher.
Since behavioral constructs only function well as explanatory devices in the highly controlled experimental milieu, it is reasonable to conclude that they are created by the experimenter through his manipulations.

Experimental research is more amenable to investigating these environmental determinants of behavior than to dealing with qualities of experience. Therefore, one begins to consider the environmental contingencies to be the only determinants worth investigating because they are the only ones that can be objectively described, quantified and dealt with in existing research paradigm. Eventually, other determinants lose ontological status and researchers talk as if the environmental determinants are the only ones, forgetting that their particular methodology will not allow others.

This process occurred in the history of behaviorism in relation to the status of psychological terms. First they were banned as observable data but still functioned in an explanatory context in concepts like Hull's "intervening variables" or Tolman's "cognitive maps." An attempt was then made to banish cognitive terms altogether from science, although their existence and importance for the processes of the arts and religion were not doubted. Eventually, in the strict materialism of Skinner, mental terms lost all
ontological status, because in a positivist scheme, whatever cannot be formulated in operational terms is meaningless and has no claims to existence. This process can be seen as reflecting the evolution from methodological behaviorism to metaphysical (or radical) behaviorism.

The Behavioral Paradigm

A paradigm is unconsciously adopted in this fashion. Both Gaston and Sears succumbed to this sort of thinking. In the introduction to his landmark text Gaston says, "rather than explaining music therapy in terms of a particular psychological theory, we have made an effort throughout to show how music therapy can bring about desirable changes in behavior and adaptation."\(^{31}\) Sears echoes this claim in discussing his use of behavioral descriptions. He argues that while his "classifications and descriptions are consistent with pertinent and accepted psychological principles and theories,"\(^{32}\) he is deliberately avoiding "the terminology of any particular psychiatric or psychotherapeutic school of thought."\(^{33}\)

Neither author considers their focus on behavior to reflect any particular psychological theory. In their view, all scientific psychological theory places primacy on behavior, and to do so is not to make any theoretical commitment. In a strict sense they are
correct because behaviorism is defined by adherence to a specific methodology, not a specific theory. But while Gaston and Sears both believe their positions reflect an explanatory neutrality, in fact their choice of methodology rules out a whole class of explanations: those whose causal forces are a property of the person, as opposed to the environment. The decision to focus on behavior is only theory-neutral in terms of distinguishing between competing behavioral theories of Music Therapy, it is not theory-neutral regarding the higher order theories that determine the nature of a particular discipline.

The adoption of the behavioral perspective is one way of avoiding "value issues concerning 'what ought to be'" according to Madsen. In contrasting behavioral approaches to "philosophical, emotional, or aesthetic" ones he argues that since "behaviorists are primarily concerned with what behaviors are present within the environment . . . and how this environment can be manipulated to change behavior," questions of values are irrelevant.

Yet in specifying the targeted behavior to be changed, and the behavior which shall replace it, the behaviorist is just as concerned with questions of "what ought to be." In fact, because in the behavioral scheme it is the therapist who determines which behaviors are changed--and which behaviors they are
replaced by--through environmental manipulation, the behaviorist has an even weightier responsibility regarding these questions of values. If the ultimate outcome of therapy is a result of client choice--as is advocated in humanistic therapies--then therapist's values play a smaller role in determining the nature of this outcome. The behavioral approach does not allow one to elude the problem of values imposition in therapy, in fact exacerbating it. Gaston and Sears were mistaken in considering behaviorism to be a theory-neutral position; Madsen is wrong in assuming it is a values-neutral one.

What these writers did not acknowledge was that "to reject one type of metaphysics [or epistemology, or philosophy of science] was to already have accepted another, wittingly or not."\(^{37}\) Although every aspect of the scientific activity of important behaviorists such as Tolman, Skinner and Hull was derived from "underlying metaphysical views" and "pre-theoretical conceptions,"\(^{38}\) the anti-theoretical and anti-metaphysical stance of classical behaviorism caused these metaphysical beliefs to be obscured or denied. Sears', Gaston's and Madsen's claims to theoretical neutrality are reasonable seen in the light of the general denial of such commitments in the behavioral tradition.
To be fair to Sears, in particular, it must be said that his thinking came close to the present movement in Music Therapy advocating for indigenous research and explanatory mechanisms. He made a deliberate attempt to establish descriptions and classifications independent of specific theory and flowing from the specifics of Music Therapy. Yet as soon as he takes this step, Sears backs away from its implications—the establishment of indigenous theory—and reassures the reader that his intention is "not to create . . . a new school of thought nor to claim any special status for music therapy, but to permit the fitting of what music therapy has to offer into various orientations." 39 In other words, Sears saw his own efforts as amenable to all theoretical orientations, as opposed to the foundation for a new and unique one. Sears' own claims notwithstanding, Bonny assessed this 1968 article of Sears as making a "masterful contribution" in developing a paradigm "to define and expand the uniqueness of music as therapy." 40 In retrospect, it is unfortunate that subsequent researchers focused more on Sears' stated intent than on his actual accomplishment.

It is somewhat ironic that Gaston, Sears and Madsen—three of the most important research theoreticians—all recognized the importance of creativity, meaning, aesthetics and human experience,
in explaining the efficacy of Music Therapy. Yet nowhere are suggestions made in order to facilitate the investigations of these factors. In fact, the research paradigm all three chose to advocate is inimical to exploring these internal, psychological factors.

Gaston seems to be arguing for a paradigm which can investigate internal, emotional factors when he observes that there are "few reports of a scientific nature about esthetics" and that "most of what makes life worth living, the feelingful aspects, have been omitted from scientific investigation." These hardly sound like the pronouncements of a hardened behaviorist. Yet immediately following these statements Gaston concludes that since "little is known of what happens inside man when he is engaged musically, the only recourse is to observe and study his overt behavior." Why Gaston chose not to advocate actually studying what happens "inside man" when he is engaged musically can only be speculated upon. Having already committed himself to the importance of this locus of causation it must have been the case that he was either unaware of the introspectionist tradition in psychology—a highly unlikely assumption given the breadth of knowledge exhibited in his writings—or more likely, that he dismissed investigations of experience as not meeting the needs of the profession, such as the validation of
clinical practice.

Gaston also recognizes the importance of the question of meaning to understanding Music Therapy process and acknowledges the inability of the behavioral approach to provide illumination in this area. His conclusion is that the Music Therapist must acquire knowledge of "the nature and meaning of man's production and participation in music" from studying "other sciences and fields of knowledge." Rather than adapt--or abandon--his preferred methodology to include these issues as a bona fide aspect of the discipline of Music Therapy, Gaston consigns them to external study.

Sears' avoidance of an existential approach is equally puzzling considering that his descriptive system was built upon and soaked through with the concept of experience. His "three classifications that underlie the constructs and processes of music therapy are (1) experience within structure, (2) experience in self-organization, and (3) experience in relating to others." Additionally, some of his categories reflect more humanistic concerns than purely behavioral ones. Consideration is given to the recognition that: music provides for successful experiences; music provides for feeling needed by others; music provides for self-expression; music provides for developing self-directed behavior; and, music provides opportunity
for individual choice of response in groups.\textsuperscript{45}
Although creative, self-expressive needs are obviously important in Sears' system in determining the quality of the client's experience, nowhere is an allowance made for studying the influence of these factors, except as how they impinge on behavior.

This same pattern—recognizing the importance of creative and experiential factors without providing either a theoretical or methodological means for their investigation—is apparent in Madsen's writings on research. Through his writings and academic position Madsen has been arguably the single most important individual in influencing the course of Music Therapy research. As evidence, consider that Florida State University, which Madsen has been associated with for over twenty years, produced almost thirteen percent of all articles published in the Journal of Music Therapy for the years 1964-1983, close to double that of the next most influential university.\textsuperscript{46}

Recently, Madsen has acknowledged that "research is sometimes defined very broadly to include creativity and other more subtle aspects not generally associated with objective facts"\textsuperscript{47} and that the "perceived gap between what constitutes science and what belongs to the art of music therapy begins to narrow with attempts that include both pursuits."\textsuperscript{48} Here, he also seems to be arguing for a new type of paradigm that will include
qualitatively different aspects of Music Therapy process.

Yet, he has elsewhere stated that therapists "are constantly 'reinforced' for phenomenological experiences" and that "environmental control and overt responses are, of course, the cornerstones of science." While Madsen is recognizing the importance of experiential and creative factors, he simultaneously seeks to preserve the essence of a behavioral scheme based upon deterministic and mechanistic factors.

Madsen takes the questionable position of saying that humans are reinforced for the quality of their experience. In this way he allows the entrance of phenomenological experience into a scientific system, but only with the restriction that its contents are determined by the same considerations as determine the form of overt behaviors. This strategy reflects reductionistic considerations in that it admits inclusion of a new property, consciousness, while saying that existent (behavioral) mechanisms are sufficient for its explication.

But this move fundamentally corrupts the explanatory purpose served by the use of the terms "experience" and "reinforcer." Consciousness and mentalistic explanations are invoked when classical and operant conditioning principles are inadequate to explain a given phenomena, e.g., language acquisition,
writing a symphony that is not sold in the composer's lifetime, etc. No explanatory power is gained by invoking mental terms that are restricted to the conditioning principles guiding overt behaviors. Considerations of parsimony would militate against the use of mentalistic terms, unless one thereby gained a deeper level of understanding. If conditioning principles are inadequate in explaining the processes involved in language acquisition or creative Music Therapy on a behavioral level, they will be just as inadequate when mental terms are substituted for the behavioral ones.

Humans either possess the inner ability to control their own actions or they do not; they either exhibit behavior that is independent of environmental contingencies or they do not. Nothing is gained in trying to have it both ways by acknowledging the importance of consciousness, without attributing to it the power of intentionally-directed causation.

The Schism Between Research and Practice

As this discussion of the various theorists proceeds it should become apparent that, in relation to therapy, there are many ways in which one can be said to be a behaviorist. The choice can be made for purely methodological reasons, from a belief that therapy is concerned with changing behavior and that these
behaviors are learned with environmental consequences serving as primary determinants, or from a belief that therapy involves adaptation to external, societal demands, and it is the type of behavior exhibited which indicates successful adaptation.

That all of the research theorists were behaviorists—in one or another of the above uses of the word—can be seen in their definitions of therapy. Gaston says that the "ultimate goal [is] to bring about desirable changes in behavior"\textsuperscript{50} which help the client achieve "a healthier adaptation to society."\textsuperscript{51} Curiously, he ascribes as a prime force here the "purposeful persuasion"\textsuperscript{52} of the therapist. It is ironic that he is attributing the quality of purpose to the agent of change—the therapist—yet advocates a system that traditionally disallows the concept of purpose used in an explanatory manner. Apparently, only references to the client's purposes are proscribed.

According to Duerksen, "an individual is regarded as having been helped when he exhibits certain specified behaviors . . . [that] can be observed and described objectively."\textsuperscript{53} And Madsen: "Music therapy is a method of behavioral manipulation . . . music therapy could take its place in the development of the behavioral school of clinical applications . . . by the very virtue of its type of behavioral manipulations."\textsuperscript{54}
The prevalence of behavioral thinking here contrasts dramatically with the fact that none of the major Music Therapy clinicians practice from a behavioral perspective. Out of a total of fifteen models of improvisational clinical practice cited by Bruscia, only one claims a behavioral orientation. What might account for this difference and what are its implications?

Most importantly, this state of affairs has resulted in the schism between practice and research that is at the heart of this study. Researchers and clinicians cannot communicate effectively because, for the most part, they are operating in completely different conceptual spheres. Clinicians utilizing creative techniques that emphasize novelty and unpredictability will never be able to apply the findings, or satisfy the empirical standards, of researchers primarily concerned with establishing a means of prediction and control.

A compelling statistic offered by James suggests a possible explanation for the manner in which the theoretical lines have been drawn regarding research and treatment. In studying the sources of articles published in the Journal of Music Therapy during its first twenty years, he found that while in the first decade of publication sixty percent of the articles were published by authors with an agency affiliation
and forty percent by those with university affiliations, in the second decade these percentages underwent a dramatic reversal to nineteen percent for agency-affiliated authors and eighty-one percent for those with academic affiliations. James notes that this reversal occurred in 1974 and has continued to the present.\textsuperscript{57}

The predominance of professional articles from those authors with university affiliations coincides with the time period when behavioral approaches became predominant in the research literature.\textsuperscript{58} The conclusion advocated here is that it is primarily academicians who advocate for behavioral approaches, while clinicians adhere to humanistic, psychoanalytic and transpersonal ones. All of the research theorists advocating a form of behaviorism—including Madsen, Gaston, Sears, Duerksen and George—are strongly associated with various universities.

Although there are a variety of factors that have determined this orientation of theorists—including the fact that behaviorism has dominated academic psychology for a large part of this century and it would be surprising if academic Music Therapy was not affected by this trend—it is herein argued that educators, in their capacity of training students, are subject to a different set of exigencies than are clinicians who are primarily guided by the need to conceptualize their
clinical experience. These factors uniquely support the adherence to behavioral principles in academicians who lack the ongoing clinical experience that would conflict with their advocacy of behavioral theory.

Without question, it is easier to educate students in a discipline that involves the application of rules and procedures based upon an objective knowledge base, than it is in fields where subjective factors play an important role. When spontaneous, creative, aesthetic or other subjective factors are admitted as being of relevance to a specific discipline, the process of training becomes more a reflection of the values of the individual trainer and is increasingly guided by intuitive factors. This is why one might find more diverse philosophies among universities in creative writing departments than in engineering departments. The former primarily involves notions of aesthetic judgments; the latter involves the application of known rules.

If the practice of Music Therapy involves the implementation of validated interventions resting on an objective data base, then the training process is one of learning techniques and their rules of application. Removing or minimizing the effect of subjective factors--such as aesthetic or intuitive considerations --simplifies training and enhances the status of the field as one operating according to objectively
validated principles. Additionally, if subjective factors are not salient to therapeutic outcome, then the need for the teacher or academician to have actually engaged in clinical work is lessened or eliminated. One need never have baked a cake personally in order to teach others the rules involved in following a recipe—unless tasting it as one goes is essential.

In allowing "educators who have directly to do with Music Therapy either administratively [or] pedagogically" to become registered music therapists via a grandfather clause, the Music Therapy establishment was giving tacit agreement to this view of therapy. It makes sense to register a Music Therapy educator as a Music Therapist, only if academic knowledge of a field is the primary attribute of a competent clinician. The NAMT decision to accredit universities based on the curriculum offered reinforces the idea that competent therapists are judged by what they know, in contrast to the AAMT competency based approach, which evaluates clinical ability based upon what one can do.

It is important to realize here that most Music Therapy programs are housed within departments of music education, and that the important research theorists cited thus far tend to either equate the processes of therapy and education, or to blur the distinctions
between them. Michel overtly states that "modern concepts of therapy very closely relate, and sometimes equate therapy and learning."\textsuperscript{60} In a discussion of the alleged lack of relevance of research to Music Therapy practice Madsen cites five studies that he feels were successful "at bridging this 'communication gap,'"\textsuperscript{61} yet all of the articles cited are oriented to educational settings. In fact, Madsen often uses examples from educational contexts to illustrate points about Music Therapy, and again seems to equate the two processes when saying: "Manipulation actually begins when any teacher/therapist interacts with students/patients."\textsuperscript{62} And Duerksen writes that "music education and music therapy are similar in many ways . . . have similar goals [and] similar techniques."\textsuperscript{63} Bonny agrees with this characterization of traditional Music Therapy, saying that "in both training and practice [it] has closely followed the music education model"\textsuperscript{64} and her evaluation is that this tendency has limited the potential of Music Therapy practice.

Whereas other schools in psychology have focused on areas like perception, memory or problem solving, it is experimentation on various types of learning that has characterized behavioral research. This stems from the belief that all behaviors are learned, and are to be explained by associative learning principles. Therefore, it is natural for Music Therapists working
under behavioral principles to adopt teaching strategies and frames of reference in conceptualizing the therapeutic process. Since all human actions are the result of associative learning, the parallel processes of therapy and teaching how to conduct therapy are fully accounted for by learning principles.

Most therapists would agree that learning of some type is an important component of any therapeutic process. What may not be warranted is (1) the assumption that the type of learning characteristic of therapy can be adequately explained through the conditioning principles invoked by behaviorists, and (2) that all of the salient processes involved in therapy can be subsumed under terms of any learning theory. If creative, affective and transformational processes are at the core of Music Therapy process, then investigations proceeding along behavioral principles will either be blind to, or fundamentally distort, the important mechanisms underlying clinical process.

Another factor supporting this schism between research and practice is that research is often undertaken for reasons other than the enhancement or illumination of clinical process. Graham says that

... most of that which is often referred to as "research in music therapy" has been done by graduate students in quasi-clinical settings where the primary motivation has not been music therapy with the ill, but the completion of a thesis or dissertation requirement. The almost total lack of any effort to replicate any of these studies
leads one to question the relevance of many of these research efforts to the practicing music therapist.65

In addition to research done by students primarily to fulfill degree requirements, university professors are faced with the "publish or perish" dilemma. James66 mentions that this is one reason to explain the shift in source of publications from authors with agency affiliations to those with university affiliations. Regardless of the reason, research that is undertaken to either "demonstrate experimental psychology techniques"67 or promote one or another psychological persuasion, will not interest clinicians who are more concerned with their every-day functioning as Music Therapists.

The plea for clinically relevant research articulated by Graham has been a perennial one throughout the brief history of this field; it is a plea that continues to the present day. In 1954 Pepinsky advocated an approach to research that would "bridge the gap between the studies of specifics of basic research and an evaluation of the changing dynamisms in the patient."68 Eight years later Schneider voiced the "need for a quantity of current information which can be useful immediately to the practicing music therapist."69 Graham's article appeared in 1969 and the Nicholas & Gilbert study documenting clinicians' dissatisfaction with current
research was published in 1980.\textsuperscript{70} In 1983 Wheeler observed that "for too long, these three facets of music therapy [practice, theory, and research] have been separated," and she expresses the hope that a "more successful integration of theory, practice, and research will occur."\textsuperscript{71} And most current are the methodological objections raised by Kenny\textsuperscript{72} and Forinash and Gonzalez.\textsuperscript{73} Obviously, this is a problem that will not go away.

Most of the early research was done under Gaston at the University of Kansas. In both his academic and professional capacities Gaston "set the tone for the profession for 20 years."\textsuperscript{74} As such, his thoughts on the role of research in general, and on the relation between pure and applied research, permeated the field and maintain their influence to the present. Fortunately, Gaston was quite specific here and we can see the seeds of the current research dilemma in his initial formulations.

Gaston argued that it was necessary to first conduct "basic, non-clinical types of research"\textsuperscript{75} that would "make more secure the springboard for further, more complex research."\textsuperscript{76} Although Gaston says that many of these studies only demonstrated "that which was already apparent,"\textsuperscript{77} they were necessary "in order to show that music did have influence on human behavior."\textsuperscript{78} Moreover, Gaston felt that because of
their respective resources the university setting was best suited for basic research whereas the hospital was better oriented for "research . . . that specifically related to clinical situations."  

While Gaston seemed to rationalize the conduct of this basic research as being needed by the profession, the more important reason seems to have been the need to both train students in the techniques and necessities of research, as well as to foster a positive attitude to such endeavors. In any case, it is clear that Gaston only intended this basic research to "provide a more stable platform from which to launch the more complex, clinical research."  

This division of convenience--conducting applied research in the field and basic research in universities--has become institutionalized over the years to the detriment of relevant, applied research. Researchers in academic settings can claim that this is a sensible division of labors, yet since most of the research conducted at all is now carried out in the university setting, the net result of this division of labor is that most research is what is called basic research, which may just a euphemism for research without application.  

Graham supports this interpretation in saying that the pure research reflected by most of the studies now in the literature were, for the most part, not undertaken out of any clinical need. . . . Some of the problems treated by pure research are created in . . . artificial, contrived situations or stem from problems not considered by the music therapist to be of sufficient importance to
warrant a thorough investigation.\textsuperscript{81}

Gfeller's assessment that "research efforts in the profession have appeared as a series of loosely related or unrelated studies"\textsuperscript{82} can be directly attributed to the tendency noted by Graham. If a community of researchers is addressing a common problem of inherent interest, their efforts will be naturally and organically related. If the motivation for their efforts is an extra-scientific one, not related to any inherently interesting or important problem, the resultant efforts will have no natural relationship or consistency.

In a strongly worded characterization, Graham decried the "scientism and the considerable amount of pseudo-knowledge" contained in the Music Therapy journals, and expressed concerns that a conception of Music Therapists as "behavioral scientists" would inhibit students' clinical training.\textsuperscript{83}

The field of Music Therapy has been inhibited in graduating to conducting the complex, clinical research that Gaston envisioned evolving from basic research. Students trained in basic research continue in it, if they conduct further research at all. Clinicians with the expertise to supervise clinical research do not have the resources; academicians with the resources tend not to have either the interest, appropriate methodology or clinical expertise.
One last situation bears looking at in investigating the historical factors behind the clinical/research rift. This involves the manner in which naturalistic research—a style whose raison d'être is preserving relevance to real world situations—was denigrated and subsequently under-utilized in Music Therapy research.

The argument here is that research theorists have tended to pay lip service to the value of naturalistic research, while simultaneously extolling the superior virtues of experimental research. The theme that should now be familiar is that while there was recognition of both the real value of naturalistic research and the methodological problems of experimental research, extrinsic concerns led theorists to advocate the latter. Pepinsky agreed it may be necessary to investigate some forms of behavior as they "occur naturally," i.e., without control, if we are able to study them at all. It may be impractical, or even impossible to bring such studies into the laboratory without greatly distorting them. . . . A trained observer can derive considerable insight into the patient's behavior in this way.84

George concurs that naturalistic research—involving the activities of description, classification and observation of relationships—forms the foundation for case study, survey and experimental research.85 Most early researchers apparently agreed as clinical observation was utilized almost exclusively in the early stages of Music Therapy research.86
Working against the vigorous pursuit of naturalistic research was the feeling that "true research" consisted of "controlled experimentation, reliable measurement of basic phenomena [leading to] prediction of results" and the establishment of causal relationships. Clinical observation is to be mistrusted because

the observer is likely to be untrained . . . interpretive elements are likely to be confused in the report with factual elements . . . the observer [is] without full knowledge of the genetic and personality antecedents which would explain [observations] . . . [they] represent highly selected and atypical behavior that can have little or no statistical validity.  

Surprisingly, in the same article where Pepinsky acknowledges that it may be impossible to study Music Therapy process in the laboratory because of distorting elements, he simultaneously discredits the naturalistic research supported by his observation. For Pepinsky, this emphasis on experimental research was "evidence of a new emphasis in the quest for truth in music therapy." Certainly Gaston's contemporaneous emphasis on basic research reinforced this drive against studying actual clinical process.

In sum, Music Therapists have internalized a need to move beyond naturalistic, clinical observation in order to become more scientific. This has been a prime factor in establishing and maintaining the gulf between research and practice. This splitting could be looked at in clinical terms as the pathology of our field.
Somewhere, naturalistic research became equated solely with anecdotal reports which in turn were seen as being arbitrary and invalid. Remediation of this situation will involve a more healthy re-integration of the clinical phenomena of Music Therapy process into our research activity.

**Specific Philosophy of Science Positions**

According to Solomon, "research has been universally recognized as the focal point for the development of the music therapy profession." Encouraging and supporting research projects was the primary objective listed by the NAMT in its first volume of annual proceedings and "the only standing committee designated by the original Constitution of the [NAMT] was the Research Committee." As was made clear previously, early Music Therapy researchers did not have the luxury of slowly evolving an appropriate research tradition, but instead were pressured to produce immediate results to placate bodies external to the profession of Music Therapy, such as the medical community, funding sources for both research and treatment, accrediting agencies and the academic hierarchy.

Given these exigencies it is not surprising that theorists accepted the Received View of science, as--with all of its flaws and inadequacies--it remains the
most complete accounting of the activity of scientists. Here was a recipe for scientific success. All one need do was learn and apply a relatively small number of principles and procedures.

In their application of these positivistic principles Music Therapy theorists have been guilty of a naive reading and presentation of relatively complex philosophical positions. This is particularly true in relation to empiricist dogma and the meaning and role of objectivity in science.

**Issues of Objectivity**

There are two contexts in which issues of objectivity arise in the Music Therapy literature, both of which are discussed by Duerksen.\(^4\) Regarding what he calls the impartial nature of scientific method, Duerksen asserts that its utilization does not involve the assumption of self-evident axioms, and that "scientific investigation does not set out to prove particular points."\(^5\) An approach where "facts can be selected to support the thesis, and facts that contradict it can be suppressed, ignored, or explained away" is considered to be a "sales approach"\(^5\) and antithetical to true scientific discourse.

Objectivity in this sense refers to the relationship between a scientist and his theories. Ideally, the scientist dispassionately advances various
hypotheses, with careful control of experimental conditions yielding results that unambiguously validate or refute these hypotheses. The problem here is that the history of science tends to invalidate both of Duerksen's claims.

Scientists, being human beings, constantly assume self-evident axioms. Statements like "The laws of nature remain constant from day to day," and "Causes always precede their effects," are self-evident axioms whose validity we all assume. It might be objected that these are metaphysical or logical claims and Duerksen is referring to statements of fact about the world. Yet these distinctions cannot be so easily drawn. The beliefs that space and time are absolute, and that parallel lines never meet, were once considered to be both inviolably metaphysical and true. Relativity theory and the discovery of alternate geometries have shown that they are actually (false) empirical claims.

In addition, it is not characteristic of scientists to dispassionately advance theories without hunches, intuitions and deep personal commitments as to their validity. The degree to which one is committed to a theory determines how one looks at variant data. It is more typical for scientists to either tinker with a theory, or question the validity of data, when faced with disconfirming evidence.
Some of the most important philosophers of science such as Kuhn, Hanson, Toulmin, Popper and Bohm all advance one or another versions of what Suppe\textsuperscript{97} refers to as "Weltanschauungen (Worldview) Analyses" of science. Common to these alternatives to the Received View is the recognition that psychological, social and historical factors all influence the advancement and acceptance of new theories. Kuhn in particular takes pains to show that the elements of Duerksen's "sales approach"—suppressing, ignoring or explaining away variant data—is quite typical of science, particularly one in a transitional phase of development. Mackenzie observes that disconfirming evidence or "anomalies" can be treated in a variety of ways and "only rarely do they serve to refute a well-attested and ongoing theory."\textsuperscript{98}

Duerksen goes on to say that "scientific research is empirical. It looks at the world for information, following Aristotle's empiricism rather than Plato's intuition."\textsuperscript{99} Again, this appears as an uncontroversial portrayal of scientific activity. Where theorists have gone astray here is in how they define or delimit the "world" that is the legitimate domain for scientific investigation. In order to make this clear, it is necessary to indulge in a short digression into the meaning of introspection (as a philosophical and psychological method) and rationalism
and empiricism (as philosophical positions).

Rationalism—as epitomized by philosophers like Plato and Descartes—holds that the search for truth involves purely logical operations of the mind. Here, the power of reason to divine the nature of the world is trusted and followed more than the powers of observation. In contrast, the doctrine of empiricism—as advocated by philosophers such as Locke, Berkeley and Hume—holds that all that we can know about the world comes through our sensory experience of it. Truth is gained by experience, not through the discovery of innate ideas (Plato) or through logical deductions of what "must be."

Descartes was an introspectionist because he felt that truth resided within his conscious mind. However, it is mistake to equate an introspectionist method with a rationalist philosophy. As Leahey notes, "after Descartes . . . introspection became the major philosophical tool of rationalist and empiricist alike." Rather than reason to how the world must be, empiricist philosophers examined the content of their experience of the world. This necessarily involved an introspectionist method. In fact, the creation of psychology as a modern science occurred when scientific methods, as opposed to armchair speculations, were applied to the study of consciousness.
Wundt, often referred to as the father of modern psychology, was well aware of the pitfalls of casual, i.e., philosophical, introspection and created a host of objective experimental methods to circumvent these difficulties. Although he was the ultimate introspectionist, Wundt "was as antimetaphysical as any modern experimental psychologist." The important point here is that one can believe in the efficacy of introspectionist methods, without being committed to rationalism.

What has happened in the field of Music Therapy is that introspection has become incorrectly equated with rationalism, and therefore is discredited as a valid scientific approach. In this regard, Duerksen equates empirical methods with public ones, ignoring the fact that examinations of experience are empirical, though private. Madsen characterizes phenomenological investigation, a method utilizing introspection, as "not a mode of research as much as ... a mode of inquiry ... [as it] does not derive its import nor its criticism from empirical discussions." Again, this is patently false. As phenomenological inquiry is directed towards discovering meanings through an analysis of the manner in which phenomena are presented to consciousness, not an analysis guided by a priori constraints, it is very clearly a method with both empirical content and
import. Mackenzie makes this distinction clear:

Phenomenology is a way of looking, it does not specify the direction of looking. Introspection can be, but need not be . . . phenomenological, just as 'extrapspection' or looking at the world can be, but need not be . . . phenomenological."\textsuperscript{104}

Just as empiricism can spawn thinkers as different as Berkeley—who denied independent existence to the material world—and Skinner—who denied existence to mind, it can offer equally substantial support to behavioral and mentalistic methodologies.

Moreover, Duerksen's characterization of research ignores the fact that addressing conceptual problems "has been at least as important in the development of science as empirical problem solving."\textsuperscript{105} Such admissions conflict with empiricist philosophy of science, but current views hold that "key debates between scientists have centered as much on nonempirical issues as on empirical ones"\textsuperscript{106} and indeed this observation was noted as one of the fundamental rationales for this study. It may be that intuitive abilities turn on their own unique form of logic, or they may not be so reducible. In either case, the use of intuition in determining research avenues and those theories worth pursuing is a fundamental part of science. The Music Therapist's intuition plays a similarly important role in determining clinical interventions and any research paradigm will need to understand and employ these mechanisms, Duerksen's
claims to the contrary notwithstanding.

The other manner in which discussions of objectivity arise in Music Therapy theory is in the context of legislating that which constitutes legitimate scientific data. Usually, concerns for objectivity are invoked to equate data with observable behaviors, and to equate observation with measurement.\textsuperscript{107} Some of the problems here can be traced to variant uses of the term "behavior".

Gaston says that "music is human behavior."\textsuperscript{108} Insofar as musical activity is something that humans engage in he is merely stating the obvious. Yet it is equally true to say that "Music is human expression," "Music is entertainment" and "Music is an art form." These statements are complementary, not mutually exclusive, as long as one seems them in a purely descriptive light.

What has happened in Music Therapy is that theorists have taken the statement "Music is human behavior" as an identity statement, or as an exhaustive definition of music, not as one of many possible categories in which to conceptualize and describe musical activity (at least not for purposes of research in Music Therapy). Because music is behavior (and nothing else), and all behavior can be accounted for through reference to environmental contingencies, all that is needed to understand music (and hence, Music
Therapy) are the methods of behavioral science. Madsen makes a similar leap of judgment when he says that "music therapy [treatment] is a method of behavioral manipulation,"\textsuperscript{109} in order to justify a behaviorist methodology. This also appears as an identity statement, or an exhaustive definition, of a therapeutic process, which in turn is utilized to justify a preferred methodology in researching Music Therapy.

In using the term "behavior" in this global fashion Music Therapy theorists are in good company with most of contemporary behaviorists. Mackenzie notes that behavior, for theorists like Hebb, Broadbent, and Eysenck is "mainly a metaphor in terms of which human activity as a whole can be elliptically described."\textsuperscript{110} He goes on to say:

It is hard to explain why theorists investigating concept formation, personality dynamics, and esthetics avoid any direct involvement with the personal (subjective) experience of their human subjects . . . except by means of the unjustifiable assumption that behaviour data are the only scientifically valid ones and that introspective and impressionistic data are incorrigibly untrustworthy. . . . The reliance of these psychologists on the behavioural metaphor cannot be considered more than a personal preference, without any particular scientific justification.\textsuperscript{111}

Mackenzie is not criticizing these theorists for the adoption of behavior per se as a convenient metaphor, as all researchers need an operative conceptual system. What they are criticized for is
using an inappropriate one, given the exigencies of research in their particular domains. And what is preventing them from using more veridical models is their extra-scientific, methodological commitment to avoiding the study of consciousness. This is precisely the type of criticism that is at the heart of the present study.

The Unity of Science

Because the important Music Therapy research theorists all advocated the adoption of an external methodology, they were committed to supporting the notion of the unity of science. This is the only way in which to justify importing methods and constructs from other fields of inquiry. One might argue that someone like Madsen was deriving behavioral methods from the phenomena itself because he considered Music Therapy treatment to be a form of behavioral manipulation. Yet nowhere is empirical support offered for this characterization of therapy. It has more of the character of an a priori declaration.

Fultz believed in the "existence of 'common denominators' in the relations between events in different realms."\(^{112}\) Pepinsky argued that "the scientific status of music therapy must ultimately depend on its methods of research . . . [which] are basically the same as those of the other sciences."\(^{113}\)
and that "investigations . . . undertaken in music therapy must have as their aim the determination of general principles or laws." Like Pepinsky, Gaston's belief in the unity of science caused him to advocate focusing on lawful behavior, as well as on the biological reduction of psychological terms.

Gaston often made the statement that man was a biological entity, part of the natural world and subject to the laws of nature, particularly biological ones. He believed that the desire for aesthetic expression can be traced to physiological needs, and that aesthetic experience has as a primary value its ability to facilitate environmental adaptation. In a quite direct statement of support for reductionism Gaston argued that

the neural mechanisms are the same whether a patellar reflex is elicited, a beautiful sunset enjoyed, a symphony listened to, or the fragrance of a rose scented. In the final analysis of mentation, we have only biochemistry and physics.

However, Gaston's observations about neural mechanisms can as well be used to support an anti-reductionist position. One might just as easily conclude that if such different mental experiences share common mechanisms, then studying the mechanism will be of limited use in understanding the experience. The fact that brain states are not iso-morphic with psychological states can be used to support the notion that the laws of psychological organization will
function independently of biological or neurological ones.

What Gaston seems concerned with is encouraging researchers not to advance theories that conflict with what is known about an organism's biology. Since we are biological entities, Gaston argues, all of our actions must follow the dictates of biological laws. Again this would seem to place a large restriction on the scope of legitimate theorizing in Music Therapy.

The first objection to this dictate involves asking the question that if Gaston is correct why should we stop at the level of biology? We are also comprised of atomic and sub-atomic particles as well as of living cells. Does this mean that admissible statements about musical activity must also follow the same restrictions placed on the behavior of these constituents of matter? Following this line of thought would lead one to conclude that if electrons do not possess free will, then humans can not.

Secondly, it is not true that we always act in accordance with biological laws. Consider the example of altruistic behavior: if a biological law is that living entities always act to maximize their survival and self-interest, how can one explain examples of altruism, either in animals or humans? If one does not want to grant altruism to animals then how do we explain the actions of humans acting in accordance with
various values, fears, feelings and other psychological qualities? It would seem that humans often engage in behaviors--such as charitable or self-destructive ones--that contradict what one would predict if we could only follow purely biological considerations regarding personal survival.

Certainly the researcher's life is easier when one assumes the validity of both the unity of science and the reductionist program. There is then no need to engage in the philosophical struggle to discover the nature of a new field and create a new methodological approach. Early theorists in Music Therapy had enough difficulties in utilizing existent methods without beginning to create their own. Duerksen was aware that "there has been some dispute about whether there is one scientific method or man"\textsuperscript{120} but since he considered it "to be a dispute about labels,"\textsuperscript{121} he was not compelled to investigate the implications of this conflict for Music Therapy. Similarly, Gaston characterized the desire for aesthetic (and therefore musical) expression as meeting a biological need in order to make more legitimate the application of experimental research methods to Music Therapy.

\textbf{The Laws of Science and Uniqueness}

Because of his importance to the research tradition in Music Therapy it is instructive to examine
Gaston's attitudes towards scientific laws in depth. The belief in the unity of science led Gaston, and others, to argue that Music Therapy researchers should orient their efforts to the eventual discovery of laws as this is the process characteristic of scientific progress. Gaston also dealt with the problem of the establishment of laws regarding humans who are unique individuals, differing from each other "in thousands of ways," and therefore wholly unlike the entities studied by the natural sciences.

Citing Allport, Gaston noted that "scientists are interested not in uniqueness, generally, but in broad and if possible, universal laws." He goes on to argue that this quality of uniqueness should not preclude the scientific study of man, but that the "generality" or qualities common to individuals should be in the domain of science, while the illumination of individuality has its proper place in the realm of art. This was reflected earlier in Gaston's consignment of the meaningful aspects of musical expression to realms external to Music Therapy.

Gaston's thinking here was inconsistent and at times contradictory, possibly reflecting the conflicting desires and purposes engendered by the manner in which he felt his artistic awareness to come into conflict with what he perceived to be the needs of the professional of Music Therapy. He says that "our
nonverbal awareness is as much a part of us as the
verbal or mathematical . . . [and] feelingfull
awareness is inextricably bound up with the so-called
intellectual. They cannot be separated." 124 Yet this
separation is just what Gaston has proposed in
eliminating the study of the unique from the science of
Music Therapy! If understanding the effect of
individual and unique factors is part of understanding
behavior, and science is proscribed from explorations
in this realm, then the purely scientific study of
humans--and of human actions in the Music Therapy
milieu--will be woefully incomplete. Gaston took pains
to emphasize the esteem in which he held the
expressional and aesthetic aspects of music;
unfortunately, he took equal pains to eliminate their
study in Music Therapy because of a belief that the
establishment of universal laws characterized
scientific activity, and that the expressive qualities
of music were not amenable to lawful exposition.

In addition to the methodological and
philosophical difficulties involved in the scientific
study of the unique attributes of individuals, the
widely held conception of Music Therapy as oriented
primarily towards facilitating the individual's
societal and group adaptation supported the focus on
general, as opposed to unique, properties. While
humanistic and some analytic therapies have
traditionally conceived of therapy as activating inner potentials and developing uniqueness, behavioral therapies are more concerned with changing an individual's behavior in order to ensure a more comfortable fit within the social structure. If one envisions the goal of therapy as the shaping of more socially appropriate behavior, then the establishment of, and conformity to, norms becomes the data one is interested in, with individual differences functioning as an obstacle for scientific practice. The fact that most research theorists have advocated for behavioral conceptions of Music Therapy can explain why they sought a philosophy of science that would support an emphasis on the general as opposed to the unique. These two approaches are radically different as the former sees individual differences as "noise" or a barrier to scientific progress—in the latter approach these differences are the actual data of interest.

A litmus test for successful behavioral theory is the ability to both predict and control behavior. For both Skinner and Watson "control [was] the ultimate test of the scientific adequacy of observationally determined functions between antecedent variable and behavior variables." This last consideration was yet one more factor encouraging behavioral Music Therapists to argue for efforts to establish lawful statements regarding the relationship between humans and music, as
it is through the application of scientific laws that we gain the power to predict, and hence control, nature.

As was previously discussed, from a positivist viewpoint theories acquire explanatory significance from their ability to generate accurate predictions. "To explain an event was to show it could have been predicted from the preceding circumstances combined with some scientific covering law." Behavioral Music Therapists were driven to focus on laws because of their belief that acquiring the ability to both predict and control behavior would mean that Music Therapy had finally arrived as a legitimate science.

Conclusion

The primary reason for the eventual decline of behaviorism as an influential psychological school is the fact that it considered adherence to methodological dogma to be more important than substantive concerns—or more succinctly, that it placed method before content. While commitments of both sorts are necessary for the activity of science, adherence to rigid rules regarding decision procedures in science "can guarantee neither the development of a scientific field as a whole nor theoretical convergence of competing positions within the field . . . it can prevent theoretical development and convergence." In
advocating various aspects of the Received View, positivist philosophy and behaviorism, Music Therapy theoreticians were implicitly—and as the historical material shows, at times explicitly—agreeing with this commitment to the primacy of decision procedures over intrinsic research exigencies. Perhaps, the clearest and simplest statement of this view can be seen in Fultz' assertion that "the music therapist can learn to use the framework of research literature as readily as he can learn to drive a car, or operate an automatic tape-recorder."129

Other individuals, such as Gaston, Sears and Madsen, who also subscribed to this rule-based view of scientific activity, simultaneously recognized the importance of aesthetic and other experiential factors in clinical process. Their adherence to Received View principles led them, however, to adopt one or another forms of behaviorism, a methodological approach inimical to the inclusion of experiential factors. This theoretical contradiction—while leading to the various cognitive contortions and inconsistencies documented earlier—makes clear the factors surrounding the emergence of behaviorism in Music Therapy. That is that those individuals who advocated for behavioral methods of research and treatment did not do so because of either an a priori or empirically-based belief that the therapeutic process naturally embodied rule-
governed actions. Rather, their prior commitment to positivist methodology led to a behavioral research program and definition of therapy. As such, the adoption of behaviorism in Music Therapy was not guided by intrinsic need, but instead by extrinsic pressures.

The current state of the Music Therapy profession—including the schism between practice and research and the lack of theoretical development or convergence—is exactly what Mackenzie\textsuperscript{130} concludes will occur when method is placed before content, and can be directly attributed to this tendency. The primary value of the preceding analysis is to show that not only must the methodology for a specific field be derived from the problems in that field, but that this methodology must also be a flexible one that is able to respond and adapt to the empirical discoveries and conceptual novelties characteristic of scientific activity.

Behaviorism's restriction of data to that which is "directly observable"—though addressing the problem of the unreliability of introspectional reports—was not justified by any specific puzzle in psychology. It involved stepping out of normal scientific procedure and legislating rules of acceptable conduct. The adoption of behaviorism in Music Therapy proceeded from similar considerations. Regardless of the nature of the new conceptions of Music Therapy that emerge from subsequent research efforts, it is imperative to fully
understand the manner in which this faulty conception of science has functioned to inhibit the progress of Music Therapy research.

By promoting the idea that a behavioral approach embodies the only scientifically valid one, Music Therapy theorists have obscured both their own metaphysical assumptions, and the fact that the choice of fruitful research paradigms will ultimately be guided by personal preference. Given sufficient attention to intellectual rigor and honesty, no approach is inherently more scientific than another, though certain approaches may have practitioners who tend to be more rigorous or thorough.

Because methodological guidelines must be derived from the nature of a discipline's unexplained aspects, the following section of this study will be devoted to providing just such a global portrait of Music Therapy. The criticism of previous research conceptualizations has been a purely internal one until this point, focusing solely on its inherent limitations and contradictions. The analysis which follows the presentation of indigenous clinical concerns will assess the suitability of this research paradigm in relation to actual clinical process. What has been established is that current research has proceeded from a non-indigenous view of science as consisting of rule-based behaviors; what remains to be seen is how this
view conflicts with the nature of creative clinical practice.

2. Ibid., 36.


10. Ibid., 228.

11. Ibid., 221.

12. Ibid., 221.


14. Ibid., 236.


22. Gfeller 192.

23. Gfeller 192.


25. Ibid., 409.

26. Smith, Behaviorism.

27. Smith, Behaviorism 178.

28. Asmus, Jr. & Gilbert, Client-centered.

29. Asmus, Jr. & Gilbert, Client-centered 44.

30. Mackenzie 162.


33. Ibid., 31.


35. Ibid., 173.

36. Ibid., 173.

37. Smith 302.
38. Smith 302-303.
40. Bonny, Prologue 2.
41. Gaston, Man 7.
42. Gaston, Man 7.
44. Sears 32.
45. Sears 33-34.
47. Madsen, Research and Music Therapy 51.
48. Madsen, Research and Music Therapy 54.
49. Madsen, No One 176.
52. Gaston, Man 7.
53. Duerksen 412.
54. Madsen et al., Behavioral Approach 70.
55. Bruscia, Improvisational 505-506.
56. James, Sources.
57. James, Sources 89.
58. Gfeller 189; Solomon 374.
59. Boxberger 142.
61. Madsen, Research and Music Therapy 52.
62. Madsen, No One, 173.
63. Duerksen 95.


66. James, *Sources*.

67. Graham 94.

68. Pepinsky, *Introductory* 236.


70. Nicholas & Gilbert, *Research in Music Therapy*.


72. Kenny, *Field*.

73. Forinash & Gonzalez, *Phenomenology*.


76. Ibid., 238.

77. Ibid., 238.

78. Ibid., 238.

79. Ibid., 238.

80. Ibid., 238.

81. Graham 94.

82. Gfeller 191.


85. George 415.

86. Michel; Pepinsky, *Applications*. 
87. Pepinsky, Applications 222.
88. Pepinsky, Applications 223.
89. Pepinsky, Applications.
90. Pepinsky, Applications 222.
91. Solomon 1.
92. Solomon 1.
93. Boxberger 161.
97. Suppe 126.
98. Mackenzie 121.
100. Leahey 93.
101. Leahey 184.
102. Duerksen 410.
103. Madsen, No One 179.
104. Mackenzie 168.
105. Laudan 45.
106. Laudan 46.
109. Madsen et al., 70.
112. Fultz, Research Literature 231.
113. Pepinsky, Applications 224.
114. Pepinsky, Applications 224.


120. Duerksen 409.

121. Duerksen 410.


125. Such as those of Rogers, Maslow and Jung.

126. Leahey 382.

127. Leahey 313.

128. Mackenzie 144.


CHAPTER IV
THEORETICAL FRAMEWORK

In Chapter V, I present a highly personalized portrait of creative Music Therapy practice. From this clinical stance, many claims are made regarding areas of concern such as the nature of music in the therapeutic milieu, the epistemological implications of musical knowledge, and the nature of the musical therapeutic relationship.

While the conclusions reached in these areas are derived from the nature of my clinical experience, they are also consonant with and, in some cases, intimately connected to, writings in aesthetics, theories of music, symbolism and epistemology. In the present chapter, I would like to make clear some of the conceptual debts and foundations in these areas of human inquiry. In this way, the reader can gain an understanding of the more general intellectual currents with which this analysis is compatible, as well as the ways in which the present analysis departs from these antecedents. Most important, however, is for some of the novel claims made in the following chapter to be seen in terms of their place in the larger sphere of human inquiry.
Theories of Music

Copland writes: "The ideal listener is both inside and outside the music at the same moment . . . in order to write his music, the composer must also be inside and outside his music, carried away by it and yet coldly critical of it." Here, Copland is stressing the importance of both subjective and objective factors in gaining a complete understanding of the significance of music. To fully appreciate any piece of music is to be able to live in the tones themselves, while maintaining the detachment necessary to appreciate how the tones chosen by the composer meet and diverge from the expectations set by both the chosen form and previous melodies (for example).

To anticipate the importance of this view for the present study, we can see that any adequate clinical theory for creative Music Therapy will have to account for the importance of both types of processes: the therapist's ability to "let go" into the music, while simultaneously maintaining a perspective that allows for evaluating the clinical efficacy of the music thereby created. As the former process has been virtually ignored in the professional literature, it is this area which I will focus on in Chapter V.

Noting that a musical composition is like an organism in that its nature is not static but is instead constantly changing, Copland goes on to say
that no piece of music can be performed without the musician "adding something of his or her own personality." It is this personal contribution of successive generations of conductors, musicians and audiences which contributes to the evolutionary nature of music. The individual's interpretation does not serve to obscure the meaning of a piece, but is essential to the realization of this meaning. Thus, the significance of any piece of music is contained within the psychological, social and historical matrix in which it is embedded.

These considerations will emerge in the discussion of the nature of musical meaning in the clinical milieu. The observation that musical meaning can be embedded in the psychological realm—and thus cannot be reduced to the purely quantitative properties of tones—will be used as a rationale for studying the processes by which clinicians generate their clinical music.

Copland supports this view further in saying that "a theme is, after all, only a succession of notes. Merely by changing the dynamics . . . one can transform the emotional feeling of the very same succession of notes." Since altering the rhythm of a melody can transform the very same succession of notes from a "war dance" into a "lullaby" the meaning of a melody is not found in the quantitative relations represented by the
its intervallic motion, but by the dynamic qualities these building blocks are endowed with. 4

In searching for the locus of the dynamic quality of melodic tones, Zuckerkandl argues that "nothing in the physical event corresponds to the tone as a musical event." 5 Instead, "musical tones are conveyors of forces. Hearing music means hearing an action of forces." 6 Again, the conclusion here is that to glean the meaning of music one's analysis must penetrate beyond purely quantitative descriptions. The purely physical event, i.e., the tone's frequency and intensity, is devoid of meaning, as is the sum of physical events when separated from its underlying psychological matrix or musical context.

This psychologizing of musical analysis is apparent in Copland's comment that "the sonata form, properly understood, is essentially a psychological and dramatic form." 7 The patterns of tension and release, and the ebb and flow of juxtaposed musical elements reflect basic human patterns of experience and response. From this perspective we can see that an analysis of a piece of music can simultaneously reveal fundamental patterns of psychological organization and thus function as a research tool in understanding the inner person.
Music and Symbolism

Equating self-expression with "giving vent to intense feelings," Langer rightly observes that "sheer self-expression requires no artistic form." If one's goal is to understand the nature of music—as is Langer's—then it is apparent that one must formulate a rationale for the use of artistic forms that goes beyond the mere facilitating of self-expression.

Since the nature of music is not to be found in (mere) self-expression, Langer asserts that music is the logical expression of feelings and that if music "has an emotional content, it 'has' it in the same sense that language 'has' its conceptual content—symbolically." It is the nature of the symbolic relationship between musical forms and human feelings that Langer addresses in saying that is the morphology of feeling that music expresses, rather than evoking specific instances of particular feelings.

The most difficult—and perhaps most original—aspect of Langer's thought to grasp is that music is a symbol whose "significance is not logically discriminated, but is felt as a quality rather than recognized as a function." In her analysis of music, Langer maintains the traditional distinction between a symbol and the thing symbolized, but introduces a novel conception of the qualities of the symbol that bind these two entities together.
One may agree with Langer's initial assertion that one must look beyond self-expression—as she uses the term—and into the aesthetic realm to understand the significance of music. What might require further clarification is her use of the term "self-expression" to mean a sort of involuntary, automatic expression of an emotional state. Dewey terms this "emotional discharge" which is distinguished from self-expression in that the latter results from the impetus of the former being ordered and transformed through synthesis with one's prior experience. Thus, "emotional discharge is a necessary but not a sufficient condition of expression."\(^{11}\) For self-expression to take place, there must be some shaping of materials or contact with objective conditions outside the sphere of the individual.\(^{12}\)

Returning now to Langer's argument—while using Dewey's terms—it is her judgment that understanding the significance of music lies in a realm beyond the mere ventilation of transient feelings. Since any rationale for Music Therapy treatment must work within the parameters of a conception of music, it is apparent that an adequate treatment rationale must go beyond the manner in which music facilitates discharge (Langer's "self-expression") and account for how the client's affective, cognitive and spiritual needs and capacities are addressed by the creative and aesthetic elements of
music creation and interaction.

There is a question as to Langer's belief that music can be **fully** described as a symbolic form that reflects the morphologies of feelings because of certain parallels between the properties of music and the properties of human experience. Langer applies the traditional dualistic perspective of symbol and thing symbolized to music because she believes that

> until symbolic forms are consciously abstracted, they are regularly confused with the things they symbolize. This is the same principle that causes myths to be believed . . . and sacraments to be taken for efficacious acts."[13]

For Langer, it would seem that any theory that promotes an identification between actual feelings, emotions or individuals and pieces of music would be guilty of an overly primitive and naive view. It would be analogous to arguing that a totem is a living god or that a flag is the nation it represents.

Yet, it is precisely this position which opposes Langer's that will be argued in the following chapter. A state of being while engaged in clinical improvisation will be discussed that is best described as an identification of the participants' being with the world of tones and vibrations. What will be briefly supported in this study is the notion of the unity of music and musician—as opposed to the dualistic view implied by standard symbolic analyses—that results not from an unreflective, primitive
consciousness, but from a reflective, self-analyzing perspective.

While the self-reflective source of this point of view is discussed in the following chapter, here is offered a supportive conceptual context. In this light, Dewey's writing on music and symbolism will be referred to as, in his aesthetic theory, he seeks to maintain the import of the symbol for the symbol-user, thereby preventing his intellectual analyses from distorting the inner significance of the symbolic, aesthetic form.

In contrast to Langer's position, Dewey holds that through music "sounds have the power of direct emotional expression. A sound itself is threatening, whining, soothing, depressing, fierce, tender, soporific, in its own quality." While agreeing that music "achieves generality, detachment from particular objects, events," Dewey engages in no talk regarding symbolic forms and morphological similarities. The significance of music lies on a more basic level--for Dewey it can be found at the core of his aesthetic theory:

For the uniquely distinguishing feature of esthetic experience is exactly the fact that no such distinction of self and object exists in it, since it is esthetic in the degree to which organism and environment cooperate to institute an experience in which the two are so fully integrated that each disappears."
Dewey notes the same similarities between the dynamics of music and human experience as does Langer when saying that "music . . . expresses in a concentrated way the shocks and instabilities, the conflicts and resolutions, that are the dramatic changes enacted upon the more enduring background of nature and human life."\(^{17}\) Whereas Langer notes these similarities, the utility of music stems from the ways in which it still maintains its separateness from human experience; it is this distance which affords music its expressive power and allows it to carry meaning.

Dewey, on the other hand, emphasizes the homologous relationship between music and human experience and attributes the significance of all aesthetic objects--and, by extension, music--to the way in which they "exemplify in an accentuated and perfected manner the union characteristic of many other experiences through finding the exact qualitative media that fuse most completely with what is to be expressed."\(^{18}\) For Langer, it is the manner in which music forces us to consider its separateness--in spite of the observed structural parallels--that provides it with expressive potential; for Dewey, it is the extent to which music breaks down the arbitrary separation of act from actor, of quality from meaning, and of aesthetic form from communicative purpose, that it fulfills its essence as an aesthetic form. The
clinical considerations presented in Chapter V—insofar as they stress the unity of musicians (clients and therapists) with the music in the clinical milieu—are more consonant with those presented by Dewey.

Aesthetic Perception

Implicit in the present study is the idea that Music Therapy research must focus on our clinical tool—improvised music—as an aesthetic object. The argument is drawn that since this therapy takes place within an art form, to understand its clinical efficacy we must understand the contribution of the elements that define it as an art form. This consideration explains why those theorists focusing on artistic symbolism are discussed rather than those who analyze symbols from a logical or anthropological perspective.

It is further argued that no analysis of clinical music that focuses on its purely formal and structural properties (modes, intervals, etc.) can capture the aesthetic dimension that is most relevant for understanding clinical processes. The assertion is made that proficient clinicians develop a capacity for understanding the expressive/aesthetic value of their clients' music that is not circumscribed by those qualities of the music considered more public, such as its key or tempo.
Yet these judgments need not necessarily result from a mysterious or purely intuitive capacity, though this is certainly a question to be addressed empirically. The issue for the Music Therapy researcher is as Virgil Aldrich characterizes it for the aestheteic: this is "to distinguish experience of things in the aesthetic mode of perception from experience of things in the perceptual modes that ground nonaesthetic characterizations." In attacking this problem, Aldrich suggests that it will be a fatal mistake either to get away from perception in toto or to suppose that basically there is but one way to take a good, objective look at things. . . . Our talk in aesthetic terms . . . is not going to turn out to be the literal one in rapport with, say, things as "physical objects."

Since "the same material thing may be perceptually realized either as a physical or as an aesthetic object," the aesthetic perceiver is able to discern qualities of objects that are not identical with their physical properties. One and the same material thing can be perceived in different perceptual categories, with no category functioning as the ultimate or primary one. Aldrich's argument is directed towards carving out a niche for a theory of art, where the qualities of an object that define it as a work of art are neither relegated to the purely subjective realm, nor limited by its purely physical--and therefore traditionally objective--properties. He seeks a third way between
these traditional distinctions.

The primary value of Aldrich's analysis for the present study is twofold: He establishes that theories of aesthetics are not "merely" about subjective experience, but instead relate to the manner in which we can talk about making objective judgments about material objects that emerge from—-and yet are not defined by—-their physical properties. To aver that clinicians' perceive qualities in their clients' music that are not apparent to even the highly trained musical listener is not to say that these judgments are necessarily subjective; the objective entity is the clinical music perceived as a clinically-aesthetic object. To say that it is only available as an objective entity to the clinically trained ear is to say no more than that the perception of the aesthetic properties of a work of art are primarily available to those with aesthetically developed modes of perception. Thus, we can investigate the creative and aesthetic aspects of clinical music and our conclusions, though of an aesthetic nature, are not thereby relegated to the realm of the merely subjective.

Indirectly, Aldrich also provides a foundation for the claim that clinical Music Therapy techniques function as legitimate tools for gaining knowledge with epistemological implications, such as is provided by microscopes, telescopes, advanced mathematics, and
other tools of human inquiry. This means that use of these clinical techniques does not merely add to our knowledge in a quantitative sense, but instead enhances our ideas about what it is possible to know.

In holding that aesthetic judgments are about the real world beyond one's phenomenal experience, while maintaining that this "world" is not limited by conventional definitions of objective properties, Aldrich supports the notion that the nature of our experience of aesthetic objects—for perhaps more accurately, our experience of the aesthetic dimension of material objects—represents a unique form of perception, the content of which is not reducible to the information gathered through other perceptual modalities. This is why the consideration of aesthetic objects can force revisions in theories of knowledge and, by extension, why clinically-aesthetic perception represents a bona fide source of knowledge.

**Epistemological Foundations**

The epistemological principles consonant with many of the proposals for Music Therapy research advanced in this dissertation are most fully articulated in Michael Polanyi's book, *Personal Knowledge*. Interestingly, Polanyi's account of the growth of knowledge within the individual parallels Kuhn's account of the development of scientific communities. As Kuhnian principles were
used to support an assessment of the relevance of the present study for the community of music Therapy researchers and the profession at large, this brief look at Polanyi's epistemology will support the corresponding personal judgments regarding clinical practice that are presented in Chapter V.

Because he considers the acquisition of knowledge to be an active process requiring the application of a learned skill, Polanyi rejects "the ideal of scientific detachment" and the corresponding characterization of the objects of scientific knowledge as impersonal.\textsuperscript{23} The very title of his book indicates the degree to which Polanyi considers knowledge acquisition to be dependent upon the personal contribution of the knower, rather than on impersonal procedures.

The novelty of Polanyi's position becomes apparent when one realizes that he is by no means a philosophical idealist. Polanyi goes to great lengths to demonstrate that the "\textit{personal participation} of the knower in all acts of understanding . . . does not make . . . understanding \textit{subjective}."\textsuperscript{24} Personal knowledge is objective because "it claims to establish contact with reality beyond the clues on which it relies."\textsuperscript{25} It is the individual's complete and total intellectual commitment to his vision of reality which facilitates the objective quality of knowledge and "setting up objective criteria of verifiability, or falsifiability,
or testability"²⁶ only serves to obscure the extent to which the objective nature of knowledge is actually dependent upon the skill of the knowledge acquirer.

A criticism of Polanyi might be that just because knowledge acquisition is characterized as a skill, it is not therefore implied that there can be no objective formulations (or formalizations, for that matter) that can comprehensively account for the application of the skill. It is this set of procedural rules that would constitute the objective check on what Polanyi is terming "personal knowledge."

Yet, in discussing various skills (his examples include riding a bicycle, creating poetry, golfing) Polanyi says that maxims (a term standing for the "rules of an art") "cannot be understood, still less applied by anyone not already possessing a good practical knowledge of the art."²⁷ Procedural rules are only useful if they can be applied to an already existent skill; they cannot replace this practical knowledge. Polanyi asserts further: "Rules . . . do not determine the practice of an art."²⁸ The conclusion that follows from Polanyi's analysis is that since knowledge acquisition—both in the scientific and non-scientific realm—results from the skillful application of an art, this knowledge cannot claim a foundation in any impersonal testing procedure.

In this view, the tools of the knowledge
acquirer--including actual physical objects as well as various conceptual frameworks--do not remain as external objects in the traditional sense. This distinction is transcended when, as users of tools, "we pour ourselves out into them and assimilate them as parts of our own existence. We accept them existentially by dwelling in them." 29

As gaining knowledge involves the revising of existent conceptual frameworks to adapt to new information, the knower himself is irreversibly changed in a fundamental manner. This is why Polanyi says to "modify our idiom [of understanding] ... is to modify ourselves." 30 This growth in knowledge is mediated by our personal judgment because "it entails a conversion to new premises not accessible by any strict argument from those previously held." 31

The consistent epistemology underlying the current study consists of the areas where the ideas of Kuhn and Polanyi overlap and include the following considerations: Kuhn asserts that no system of procedural rules can account for the unproblematic conduct of science and Polanyi generalizes this tenet to all acts of skillful knowing; traditional accounts of knowledge acquisition have mistakenly stressed the role of impersonal testing procedures for the validation of knowledge; the role of the personal judgment of the individual in knowledge acquisition has
been minimized; the factors that go into making these types of judgments cannot be formalized in systems of rules; and, lastly, none of these considerations militates against the objective nature of the knowledge thereby obtained.

Polanyi's emphasis on personal knowledge with objective claims provides a firm foundation for the concept of aesthetic perception discussed previously. It is supporting the validity of knowledge claims that result from just such a cognitive contribution of the perceiver as is embodied in aesthetic perception towards which Polanyi's account is directed.

In the following chapter, I discuss clinical Music Therapy skills as the type not reducible to any formalization or system of rules; they represent forms of skillful knowing, precisely in Polanyi's sense of the term. It is because these techniques represent a form of knowledge acquisition that Polanyi's analysis appears to be relevant in characterizing their nature.

As improvised music is the tool of the Music Therapist most fully addressed in this study, and as the efficacious use of this tool involves a merging of musician and music, Polanyi's description of the internalization of the tool is most germane. It provides the epistemological analogue for the merging of object and perceiver characteristic of Dewey's aesthetic theory.
Polanyi's observation that knowledge acquisition involves a personal transformation of a fundamental nature is taken seriously in addressing the elements of clinically relevant research. The claim that fruitful investigations into the nature of Music Therapy process will necessarily tap into the transformative capacities characteristic of successful clinical practice—and function to transform the researcher—can be seen as not such an unconventional claim when Polanyi describes all knowledge acquisition as involving the irreversible modification of one's world view.
Notes


2. Ibid., 136.

3. Ibid., 21.

4. Ibid., 21.


6. Ibid., 37.

7. Copland 107.


9. Ibid., 176.


12. Ibid., 62.

13. Langer, Philosophy 199.


15. Dewey, Art 239.


20. Ibid., 7.

21. Ibid., 21.

23. Ibid., vii.
24. Ibid., vii.
25. Ibid., 64.
26. Ibid., 64.
27. Ibid., 31.
28. Ibid., 50.
29. Ibid., 59.
30. Ibid., 105.
31. Ibid., 105-106.
CHAPTER V
THE DOMAIN OF MUSIC THERAPY PROCESS

Creative Music Therapy Practice
from an Indigenous Perspective

In this chapter, I present a global portrait of creative clinical practice that does not rely upon pre-existing psychological theory for either an explanatory vocabulary or a theoretical justification. What follows is a brief discussion of the need for, and function of, this portrait in the present study.

Kuhn writes that "the decision to reject one paradigm is always simultaneously the decision to accept another, and the judgement leading to that decision involves the comparison of both paradigms with nature and with each other." It makes no sense to argue for the merits of a given paradigm, in and of itself, without an evaluation of how well that paradigm can address the data and important questions in the field of interest. Yet to accept one or another of the existent descriptions or models of Music Therapy as the one against which a research paradigm must be evaluated is to bias the evaluation in favor of the conceptual bases of the chosen clinical model.
There can be no paradigm-neutral descriptions of data, in principle, because particular facts only become scientific data within a particular paradigm and rival paradigms reflect "incommensurable views of the world and of doing science in it."² The employment of any paradigm requires making non-empirical assumptions and adherents of competing paradigms refuse to accept each other's non-empirical assumptions. Analytic Music Therapists assume the validity of a Freudian personality model and behavioral Music Therapists assume that the methods of mentalistic psychologies inhibit scientific growth. Each group "talks through" the other because their basic differences are of an epistemological nature.

In providing support for a paradigm shift one is then arguing for the appropriateness of one epistemological viewpoint over another, not solely on its own virtues, but as the basis for a specific discipline. In this argument one must judge the appropriateness of a given paradigm against as pure a description as is possible of the phenomena to be explained. Because, as was noted above, existing descriptions are embedded within formal systems external to Music Therapy, and the very purpose of this study is to present a research paradigm adequate to clinical practice, it becomes necessary to present a portrait of this practice meeting indigenous criteria.
The problem for this study is where to draw such a description from. By necessity, its specification is prior to the establishment of directly testable hypotheses; indeed, it is what makes such hypotheses possible. I have chosen that strategy that seems best suited to the general goals and values of this study: the only possible source for such a description can be my own experience as a clinician. It is the quality of this experience which has led to my dissatisfaction with existent research strategies and clinical models. Thus the alternative must spring from that which is the source of my dissatisfaction.

This is not such a radical strategy when one acknowledges that the purpose of research in Music Therapy is not to explain behavior but to explain experience. As a therapist I am not interested in the actions (behaviors) of my clients (and myself for that matter!) in their own right, but only in the meaning of such actions— that is, how they are experienced and what they indicate regarding the implicit forms of knowledge maintained by clinicians. If one accepts that research in Music Therapy should be oriented towards explaining meanings and phenomenal experience (which will be argued for in Chapter VI), then it is this experience which must be the source of the explanatory mechanisms, and to which such explanations must be adequate.
Rogers writes that "science always has its beginning as an inner subjective hypothesis, highly valued by the investigator because it makes patterned sense out of his experiencing"—psychological and historical investigations into the nature of scientists' activity reveal as much. My choice is to embrace this truth, rather than to deny it by hiding behind a methodology that is primarily a rationale (though not necessarily a good reason) for theory choice that is more often guided by the typically aesthetic considerations of symmetry, elegance and simplicity. While "science is practiced by individuals, scientific knowledge is intrinsically a group product." The efforts of individuals contain intuitive and non-empirically demonstrable components; the judgments of the community are more empirically constrained.

It would be mistake to read the foregoing as advocating that the resultant science of Music Therapy is "merely" about subjective experience. By communicating to other clinician/researchers the nature of our experience we begin the task of inter-subjective validation about a real entity: the process of therapeutic transformation. What I am advocating for, then, is that explanatory criteria be formulated according to the nature of our experience of these processes, not by a priori notions regarding theories
of personality and psychotherapy. Therefore, there is no alternative but to articulate the nature of this experience.

It is certainly possible that some of the elements of Music Therapy process presented here are idiosyncratic to this author, and reflect more on my unique perceptions, than on truly universal, or at least general, characteristics of the process itself. Such observations do not function as a criticism of my strategy but instead support it! Our goal as individual researchers should be to articulate our personal judgments and intuitions just so they can be evaluated by the community. Only in this way can the shared, tacit knowledge possessed by creative Music Therapists be made explicit. Discarding certain notions as not being of universal application is as important as validating those notions that are part of our shared legacy.

Though scientists employ subjective and intuitive factors in both theory generation and validation, the rational nature of science is not therefore challenged. Being intuitive does not imply that these factors are therefore arbitrary. They are the "tested and shared possessions of the members of a successful group, and the novice acquires them through training as a part of his preparation for group-membership." The community establishes the usefulness of such notions and, though
arising from individual experience, they do not depend on the vagaries of the experience of single individuals for their validation.

Kuhn's notion that the unity and evolution of scientific disciplines is not reducible to the implementation of shared rules, but is instead accounted for by the transmission of a tacit form of knowledge, is shared by the view of Music Therapy process articulated here. Much of this chapter is devoted to an extended discussion of the inability of rules of practice to adequately account for the mechanisms of Music Therapy process. The argument is made that since practice cannot be explicated by appeals to formalizable methods, guidelines and rules, research in Music Therapy must be directed towards illuminating the tacit form of knowledge possessed by creative Music Therapists. No other research strategy can address the real mechanisms of contact, growth and transformation characteristic of Music Therapy process.

In their training, scientists acquire implicit explanatory standards and creative research strategies, not explicit rules for conducting science. Much the same is true in Music Therapy training. When students participate in an experiential Music Therapy group, listen to Nordoff & Robbins' "Edward" tape, or, as interns, observe their supervisor work, they are internalizing the unverbalized essentials of clinical
process. They are learning things like how to discern levels of musical expression, how to perceive the individual meaning of their client's music and how to best work with the affective nature of this expression on a musical, verbal, and bodily level. The investigation of the transmission of these skills in training, and the mechanisms of their effect in practice, requires investigations into their subjective manifestation in the experience of the participants.

One may remain unswayed by this argument and still feel critical towards my position of not offering evidential support for this portrait of Music Therapy process. This demand for evidence can be lessened when it is understood that my aim, in this study at least, is not to present a directly testable theory or model of Music Therapy process which makes explanatory claims. This can only emerge through empirical efforts, not through a conceptual one, as this study is characterized.

Rather, my intent is to present a meta-theoretical perspective from which models, theories, criteria for explanation and research designs holding promise for a deeper understanding of Music Therapy process can be derived. All paradigms rest on unproven assumptions and the goal here is to articulate those assumptions derived from the experience to be explained. In this way, the conceptual basis researchers are working under
will have a greater likelihood of articulating productive avenues of investigation.

Though not making empirically testable claims, this portrait of Music Therapy can nonetheless be evaluated. The criterion for its evaluation should be the degree to which it possess heuristic value in conceptualizing Music Therapy process. This will become apparent by the extent to which it is successfully utilized by subsequent researchers. Additionally, the extent of its overall coherence and consistency—which can be evaluated in advance of such research efforts—will increase the validity and usefulness of the individual elements.

This portrait of clinical practice is not intended as a prescription for how to practice, but functions as a description of Music Therapy process. It is presented as a self-supporting network of principles around which clinicians and researchers with similar experiences can coalesce. Its function here is to ensure that the present effort results in a system with both internal consistency and firm roots in the living phenomena of Music Therapy practice.

I believe that a scientifically rigorous conceptual system can be created in Music Therapy without a dependence on pre-existing personality models. Though I hope that the system articulated herein reflects the experience of accomplished
therapists who wish to engage in research, failing this it is possible that the present study can still be of value. This is possible if the method deriving these principles and synthesizing them to create an indigenous paradigm is found to be a valid one. Of course, it is hoped that the argument contained herein is true as well as valid, but if only valid it is still valuable. Its internal consistency will determine its validity; its ability to function well as a heuristic for other clinician/researchers will determine its truth value.

Clarification of Terms

Since I have stated that this study is applicable to creative and improvisational methods in Music Therapy I would like to briefly establish some criteria here for what I am considering bona fide examples of such methods.

Music Therapy treatment is employed in a variety of settings with widely differing goals. It would be presumptuous to claim to be able to provide an exhaustive definition as Bruscia has recently devoted an entire book to the subject. Nevertheless, it is important to provide some sense of which of these multitude forms of clinical practice I believe the conclusions of this study to hold relevance for.
This specification is necessary because a paradigm is not a discipline-wide entity (there is no "chemistry paradigm" or "physics paradigm") but instead has a more narrow realm of application, uniting researchers in sub-disciplines such as particle physics or genetics. We should not expect all Music Therapists to find unanimity in a common paradigm; only those whose work has a common focus will benefit from the consensus provided by a paradigm.

This study is directed to those forms of Music Therapy where the affective experience of the client is acknowledged to be the desired level of intervention. Clinical work with this focus can be employed regardless of a particular client's need, handicap or debilitating condition. We should not expect Music Therapy paradigms to be differentiated by clinical population.

That much said, I would like to add that, in reality, all forms of Music Therapy practice evoke affective reactions from clients. For therapists to ignore this because their goals are not of an affective nature or because their conceptualization of Music Therapy process is not a psychotherapeutic one, is professionally irresponsible and raises serious ethical questions. Regardless of one's job description or treatment philosophy, as a Music Therapist one must take the responsibility of helping a client to manage
the feelings evoked by clinical interventions.

Clients in Music Therapy—regardless of the treatment milieu—are either in a chronic or temporary state of exacerbated emotional need. This renders them particularly vulnerable and susceptible to having feelings, attitudes and memories elicited which, though holding the potential for growth, can also be problematic if not treated with care by the therapist. This is true even in a purely pedagogical milieu. If this is doubted consider the number of people who either become Music Therapists, or seek Music Therapy treatment, because of damaging experiences with music lessons, either as children or adolescents.

The point here is that there is a real sense in which all Music Therapy techniques involve an affective response from the client, whether this response is a goal, tool or incidental result of the therapist's intervention focus. Clinical uses of music that are not necessarily classified as psychotherapeutic—such as facilitating clients' physical rehabilitation through the rhythmic and motivational impetus provided by a favorite song, or using singing to effect speech therapy goals—nevertheless function on an affective level. Conceived in this manner, a strong argument could be made to support the notion that the primary conclusions of this study hold relevance for the entire domain of Music Therapy practice. Though I will not
make that argument here, I do believe that it would be a valid one.

I consider Music Therapy process to be the aggregate of internal processes (imaginal, affective, cognitive, etc.) and external activities (speaking, playing music, expressive movement, etc.) that are directed by the intent behind the therapeutic meeting. Silence and verbalization are included in the Music Therapy process because their use acquires a qualitatively different significance in the context of a Music Therapy session.

The intent referred to above can consist of, but is not limited by the following goals: supporting appropriate emotional development, addressing specific emotional conflicts, engaging in activities oriented towards self-growth (as opposed to remediating pathology), and allowing interpersonal contact with individuals for whom alternate means are limited or unavailable, such as severely psychotic, language impaired or comatose patients.

Music Therapy takes place in the context of a human relationship, the nature of which serves an integral function in determining its efficacy. Therefore, the inner experience of both client and therapist are vital components of the therapeutic process and each is considered to be a valid and integral area of inquiry.
This definition is necessarily vague because the nature of Music Therapy process is something that should be empirically discovered, as opposed to being defined in an a priori manner. As will become apparent, it is my belief that the essential mechanisms of this process are unknown, with existent theoretical rationales representing post-hoc analyses rather than true, empirically-derived hypotheses. Considering a process a candidate for definition rather than discovery is to imply that the nature of the process is determined by convention. It is one of the core assumptions of this study that the nature of Music Therapy process is a real phenomenon, existing in its own right, and is not something whose nature can be represented equally well in a variety of psychotherapeutic or other formal systems.

Creative and improvisational methods in Music Therapy are those whose content is determined in a fluid and adaptable manner, guided by the in-the-moment interaction of client, therapist and music. While most forms of Music Therapy treatment do have creative components, use of this term is being reserved for those methods where the creative components play a necessary and essential role in accomplishing the therapeutic objectives. Henceforth, the term "creative method" will be used to designate a clinical approach that is not limited to, or defined by, the concrete
activities (improvising, listening, composing, etc.) in which the approach is embodied.

Although Bruscia acknowledges that "improvisation is a creative activity which commonly occurs in everyday life," he nonetheless distinguishes improvisational Music Therapy methods from those employing "performing, composing, notating, verbalizing and listening to music" all of which presumably have creative aspects. Yet, all of the goals that Bruscia lists in his exhaustive survey of improvisational theorists can be addressed through non-improvisational means. Thus, these alternate activities are herein considered to be functionally equivalent to pure improvisation. Composing music, interpretively performing a pre-composed work, or receptively experiencing music are all seen as having the potential for tapping the same spontaneous creative and expressive processes as pure improvisation.

Research Domains in Science

In the present chapter, the domain of Music Therapy process is delimited. The common definition of a "domain" refers to a "sphere of influence" and a "field of thought or action." Both of these descriptions figure in the sense with which the word is being used here. Shapere has taken the concept of a domain and specified the conditions that establish a
"scientific domain." His specifications serve to further illuminate the factors that bind a community of researchers in a common effort, and thus define a scientific domain.

The first consideration is that the factors that render a variety of disparate phenomena part of a common scientific domain are highly complex and not at all obvious. Apparent relation among phenomena is not by itself reason to consider them part of a common domain. Shapere observes that

in more primitive stages of science . . . obvious sensory similarities or general suppositions usually determine whether certain items of experience will be considered as forming a body or domain, this is less and less true as science progresses. . . . Differences which seemed to distinguish items from one another are concluded to be superficial; similarities which were previously unrecognized or, if recognized, considered superficial, become fundamental."

For example, at first glance it may seem that the study of objects falling towards the earth is more closely related to magnetic phenomena than to the orbits of the planets. In observing the motion of falling and magnetized objects we see two bodies attracting each other, while in planetary motion we observe elliptical orbits around two central points. Yet as science developed, it became clear that the problem of falling bodies and planetary motion were unified by the force of gravity, whereas magnetic phenomena reflected a fundamentally different force. Thus the superficial similarity between magnetic
phenomena and falling bodies was a spurious basis for considering them to be part of a scientifically relevant domain (until, of course, they are united by a successful unified field theory).

One way to look at the progress of science, then, is to see the manner in which seemingly disparate phenomena are shown to be consequents of more basic, unifying processes. The correct theory of electricity unified seemingly disparate observations regarding lightning, voltaic cells, magnets, and static electric phenomena. Each of these areas was one means of studying the more fundamental process. Establishing a domain correctly, i.e., in a way that reflects an underlying unity, is both the vehicle and goal of scientific activity.

It is not enough that certain items be related for them to constitute a domain, but that there must also be a specifiable problem, a gap in our knowledge regarding the way in which the items are related. A domain "is not merely a body of related information; it is a body of related information about which there is a problem, well defined usually and raised on the basis of specific considerations."\(^{13}\)

Not only must there be a unifying problem to consider a realm of knowledge a scientific domain, but there must also be a significant relationship among the elements so construed. This significant relationship
is defined as one that suggests the existence of "more comprehensive or deeper relationships." Establishing a correlation between variables or properties is not itself a strong indicator of the existence of an underlying, deeper relationship, and is not itself sufficient for considering them part of a domain. There must be further reasons for considering the covarying properties to be significant ones.

For example, the astronomer Kepler labored for many years to demonstrate that planetary orbits were governed by the harmonic ratios of musical intervals. Eventually, he discovered a statistic which conformed to the law of harmonic ratios. Here is a correlation of properties which nonetheless does not delineate a scientifically relevant domain; Kepler's reasons for thinking so stemmed from what would now be considered non-scientific concerns. But in his time Kepler's effort was considered to be legitimate.

The moral for Music Therapy is that it is not obvious how we should construe our research efforts in order to render them adequately scientific. Deciding what exactly we need to explain and where we should look to draw these explanations are both matters for conjecture. Behavioral Music Therapists assume that we must correlate observable behavior with formal (preferably quantifiable) properties of music; psychodynamic Music Therapists assume that correlations
need to be drawn between various psychic structures and their musical manifestations; and more esoteric minded therapists seek to establish that specific elements of music (typically intervals) contain universal significance, and research should correlate clinical outcome with these universal meanings. All of these global approaches embody particular hypotheses about where the significant relationships in this discipline lie.

Both the lack of theoretical development and the negative evaluation of research given by clinicians suggest that none of these accounts is capturing the essential elements of a truly significant research domain. They are each deficient in criteria of either "readiness" and "importance"\(^{16}\)--qualities that Shapere says are necessary for the establishment of a fruitful domain. As an example, behavioral research meets standards of readiness--problems can be formulated with sufficient precision, and research tools exist to investigate these problems--but fails in criteria of importance. The precise research problems thus formulated are not relevant to those clinicians for whom the research is carried out.

In the present chapter, I propose that viewing the creative process as the primary, unifying point from which to explore and evaluate Music Therapy treatment establishes the domain of Music Therapy research in the
most fruitful manner. The basic phenomena and most important research problems in Music Therapy cut across conceptions of clinical technique, client population and even treatment philosophy. These are the more obvious "sensory similarities" or "general presuppositions" referred to by Shapere. And I am not advocating for this stance because of a priori beliefs regarding creative processes, but from a recognition that an explanation of all bona fide forms of Music Therapy necessarily involves coming to terms with the contribution of these creative processes—processes that I consider to be defining characteristics of Music Therapy.

Establishing an indigenous research paradigm focusing on the creative source of clinical interventions ensures that criteria of "importance" are met. The research problems thereby defined are derived from the every day activity of Music Therapists. If there is one irreducible premise that this study is based on it is that researchers must research what clinicians do. This is the only way that our understanding of Music Therapy process will develop. The researcher's task is to decide which clinicians' work should be investigated, and which aspects of this work define it for purposes of research.

By directing researchers towards clinicians whose work reflects the primacy of creative processes, this
entire study is devoted to providing an answer to the first question. The present chapter fulfills the second task, delimiting the domain of Music Therapy process in the experiential awareness of client and therapist. Because the processes involved in creative clinical practice cut across client and treatment categories, their investigation represents the most potentially generalizable—and hence, fruitful—research program for the profession as a whole.

Traditionally, investigations of experience have suffered not from a lack of relevance (importance) but from a deficiency in Shapere's other criterion, "readiness." We are now, as a culture, developing the tools for these types of investigations so that the domain delineated herein is ready for investigation. These tools include research methods (such as phenomenology), theoretical constructs (like fields and systems) and world views (holism, as an example) which are supportive of investigations of creativity, aesthetics and other aspects of human experience relevant in the Music Therapy milieu.

A few words of explanation are necessary to discuss why most of the statements in this section refer to the specific Music Therapy technique of clinical improvisation, when earlier it was asserted that this study was directed to all forms of creative
Music Therapy practice. There are two reasons: one involving theoretical concerns regarding the universality of creative process, and the second involving more personal concerns regarding the present author's experience and area of competency.

Just as the clinical improviser makes certain, spontaneous musical choices, the GIM\textsuperscript{17} guide selects a piece of music, the therapist with a group of autistic children selects a musical activity, and the therapist accompanying the singing of an adult with schizophrenia chooses a specific type of accompaniment, emphasizing certain lyrical phrases and notes of the melody. On a basic level the intuitive actions of these various clinicians are quite similar.

Yet it is within clinical improvisation that the creative process at the core of successful clinical practice is laid most bare and exists in its most pure form. In composing, listening to pre-recorded music, or playing composed music, external forms --which can potentially obscure the contribution of the spontaneous, creative process--are relatively more prominent. Of course this judgment is by no means absolute, and clinical improvisations are also restricted by internal, psychological and physical constraints, as well as by external constraints regarding the possible sounds and manner of bringing forth sounds from any given instrument. The argument
here is merely that often the underlying process common to all creative techniques is most readily apprehended in clinical improvisation—and thus this technique can best represent all of creative Music Therapy practice.

Also of relevance here is the purely personal, historical consideration that it is through clinical experiences in improvisation that this author has glimpsed the deepest potential of Music Therapy, and it is the technique about which I can speak with the greatest authority. Although it is presumed that the statements about clinical improvisation apply to all forms of creative clinical practice, only subsequent empirical investigations will differentiate the universal aspects of this practice from those which uniquely comprise the process of improvisation.

The Research Problem for Music Therapy

Theories identify certain problems, questions and empirical situations as what Laudan\textsuperscript{18} refers to as archetypal ones, thus endowing them with greater importance. They are archetypal "because the theory indicates that they are the primary or basic natural process to which other processes in the domain must be reduced."\textsuperscript{19} In identifying the creative process as such a fundamental process for Music Therapy research I am suggesting that understanding 1) the source of the therapist's creative interventions, and 2) the effect
of involvement in creative processes upon the client, are the most important research problems. Though these may seem to be two separate problems, in fact they are unified in a manner that goes to the heart of this study. To anticipate my position: I will argue that these two questions cannot be separated; the key to understanding the clinical efficacy of Music Therapy treatment is intimately connected to understanding the process generating the clinical interventions constituting this treatment.

In creating a portrait of clinical practice for purposes of researching its creative aspects, it is necessary to specify what we would need to know about this process that would allow us to say that we understand it or have gained some insight into its inner workings. We must begin from questions like: Where are the gaps in our knowledge? What puzzles us about this process as regards the way it is actually used in practice? Only then can a research program be formulated that will penetrate to deeper layers of this practice in a manner that preserves clinical relevance.

Though it is arguable that the most important question for any Music Therapy theory to answer pertains to the source and nature of the clinician's creative interventions, empirical investigations into this aspect of practice are almost non-existent! I believe that the dearth of empirical research in this
realm reflects an underlying, implicit feeling that creative interventions either do not require, or are not amenable to, explanations.

Typically, clinical theories assume the ability of practitioners to create such music, and therapeutic mechanisms are then postulated to connect formal or structural aspects of the music with observed clinical outcomes. It will be subsequently argued, however, that this strategy of considering the source of music to be a mysterious, impenetrable "black box" is actually serving to deflect attention from the area of investigation most likely to reveal the operative mechanisms responsible for the therapeutic value of Music Therapy. It is through explication of the processes involved in the actual creation of clinical music that researchers will discover the key to understanding its clinical efficacy.

The most important question for Music Therapy research to address involves how the therapist makes his or her musical choices. What type of knowledge, skill or process is involved in musical interactions with clients? The view argued for herein is that it is necessary to discover and explore the creative, generating principles of accomplished clinical improvisers in order to satisfactorily answer this question. This will necessitate research on the experiential aspects of this process, from the
perspectives of both client and therapist. Descriptions of clinical practice by objective, "third-party" observers are necessarily limited to its formal and structural aspects, qualities which, it will be argued, cannot adequately explain the therapeutic efficacy of music. Establishing the causal relationships that are operative in practices like clinical improvisation requires the discovery of those elements of the process that not only exist contiguously and co-temporaneously, but can be shown to be empirically linked to therapeutic outcome.

Creative Clinical Practice

The Nature of Clinical Skills: An Argument Against Rule-Governed Theories

It is additionally proposed that the quality which renders improvisation therapeutic is not to be found in the therapist's conscious implementation of validated propositions, but rather depends upon the degree to which the therapist abandons conscious intent and enables a creative, transformative process to emerge through musical creation. Research and theorizing regarding the efficacy of clinical improvisation, then, should focus not on what facts the therapist knows, but upon what knowledge is implied by the skills that the therapist demonstrates. Focusing on this tacit knowledge is reasonable when it is accepted that a
successful therapist is evaluated as such by their actions and therapeutic outcome, not by their "book" knowledge nor by the quality of the post-hoc rationales that clinical results are attributed to.

Traditionally, Music Therapy clinicians have limited their accounts of the source of clinical interventions to reports in the form of rules and methods guiding musical interventions. Thus, Nordoff & Robbins say that "with a hyperactive and 'explosive' child . . . create a calm, undemanding mood [and] with a troubled, obstinate, or restive child, music more active in rhythm or mood may be called for."\textsuperscript{21} And Priestly suggests that the task of returning a client from an intense emotional experience is accomplished by "changing to tonal music and containing the frenzy in strong major common chords."\textsuperscript{22} Thus, the misimpression is conveyed that clinical music originates in the deliberate implementation of verbally formulated principles or, in other words, that the therapist's actions are rule-governed.

Now it is obvious that Nordoff & Robbins' Creative Music Therapy is as far from a rule-governed technique as can be. Consider this statement of their relationship to music in the clinical milieu:

Only music itself can convey the meaning of its experience. . . . The statement of music is made moment by moment; what it expresses comes to life as it moves in time. . . . When we live in the movement of a melody we become identified with it. . . . When we live in the tonal and temporal structures of a musical composition . . . our
participation integrates our responding faculties. It is out of this completeness of the relationship between music and the human being that music therapy in its truest sense arises.

In actuality, neither Priestly nor Nordoff & Robbins rely on the rigid application of rules or techniques in their actual practice, and in fact I have used these theorists specifically to show how traditional standards for clinical reporting serve to distort the real nature of clinical work. This type of reporting reflects more on the absence of a paradigm oriented by creative processes than it does on the actual view of clinical process held by these authors. Part of the purpose of this study is to expand the range of descriptions considered professionally acceptable in order to get closer to the actual clinical phenomena. The distorting effect of traditional standards is no more apparent than in the Nordoff & Robbins example, where the originators of the use of improvisation in Music therapy are forced to describe their work in a manner that does not do justice to the creative process, the very foundation of their work.

To continue the explication of how the term "rule-governed" is being used here, an action can be said to be rule-governed when an individual possesses an inner representation of a rule (such as "Stop at the corner when the light is red"), the external condition is met (the light is in fact red), and the individual
engages in the act indicated by the rule (the individual stops). A less strict interpretation here would hold that the individual need not be aware, nor possess a representation of, the rule but merely has to act in accordance with it in order for one to say that his behavior was rule-governed. In arguing against this approach as one holding promise for Music Therapy, the present writer is considering the former, stricter interpretation.

Other rules which might be said to guide clinical improvisations could take the following form: "Provide a left-hand bass ostinato for an individual whose playing is disorganized" or, "Improvise in a minor key for a sad individual." If effective clinical improvisers actually create music in this way, then understanding how to do clinical improvisation would merely involve possessing both the requisite "rules" and the musical skills to implement these rules. Researching clinical process would then involve discovering the nature of these rules, the conditions under which they are operative, and the extent of their effectiveness and generalizability.

This type of explanation is typical of the information-processing approach in psychology, modern linguistic theory and those philosophies of science equating rationality with rule-governed behavior. Hence, there is a strong cultural impetus to explain
all perceptual, cognitive and behavioral processes—including language acquisition and conceptual development—as reflecting the implementation of various rules and decision procedures. This has become one of the new paradigms of an adequate psychological explanation.

Considering therapists' actions to be rule-governed in this sense provides certain advantages: It appears to give an adequate explanation of these actions, it provides a language in which to discuss musical phenomena that are traditionally difficult to express verbally—a necessary component for both training and inter-disciplinary communication—and it gives the impression that clinicians' musical interventions are guided by rational and deliberate considerations regarding client goals. In short, it de-mystifies the practice of Music Therapy by adapting a traditional medical model, as musical interventions assume the character of medical "prescriptions."

Yet the problem is that it still does not "get us off the ground" in deciding what to play in a therapy session! At some point the therapist must make the creative, intuitive leap to producing tones, and no amount of verbal theorizing can ever produce a musically deduced consequent. In the example of the sad individual cited previously, one can ask which minor key to improvise in, what melodies to play, which
harmonies to use, what sort of rhythm, which instrument to use, what note to begin on, etc. There are a plethora of variables to be considered in any musical creation, and existent rules--while providing guidelines for general contexts--cannot be used to infer specifically what music to play for various clinical purposes.

Like artistic creation, Music Therapy process does not follow extrinsic rules or guidelines, but is instead determined by the emotional experience taking form in the clinical improvisation. The music is shaped by molding to the experience in order to move the experience to an eventual (if temporary) consummation or stasis.

The Importance of the Generating Processes of Clinical Music

For while the rule-based approach is a convenient one for talking about clinical improvisation, it does not model the actual generating processes involved in clinical improvisation. Rules and decision procedures can serve as plausible calculating devices, i.e., they can provide a post-hoc accounting of the phenomena in a fictionalist sense, but they do not represent the actual processes involved in creating music in clinical improvisation in a manner that meets realist criteria. If one's research goal is to model the actual creative processes utilized, and not merely establish formal
rules or propositions into which the data can be fit, then it will be necessary to investigate the essentially non-rational elements involved in musical creation. Rational reconstructions of clinical process—within any conceptual system—will be blind to these elements.

There are two possible objections, that I can see, to this emphasis on the necessity of understanding the generating principles (as I have characterized them) of therapeutic music in order to understand clinical improvisation. The first one involves purely methodological considerations. That is that these creative processes are both private and non-rational, and no scientific method of investigation can, by definition, enlighten us as to their nature. The second involves not a methodological constraint, but holds that the source of the therapist's music is irrelevant to understanding the efficacy of clinical improvisation. That is, all we need to do to understand clinical improvisation is correlate the actual music produced with therapeutic outcome. In this way we will learn the effect of the various elements of music and can formulate musical prescriptions and rules as detailed above. How the music is produced has no bearing whatsoever on its usefulness. Just as some philosophers hold that we do not need to know the source of a particular theory to
ascertain its validity, this view on therapy holds that we do not need to know the source of a particular clinical intervention in order to ascertain its efficacy.

In answering this first objection I would note that the whole history of psychiatry and psychotherapy has been one of drawing explanations for seemingly irrational actions. By investigating the causes and underlying contexts of dreams, everyday acts, neurotic and psychotic behavior patterns, psychologists routinely propose mechanisms and processes to account for these types of acts. In fact, the rule-based approach criticized above seeks to provide just such a formal basis for the intuitive act of musical creation. It is not wrong in proposing a mechanism for this process, just in the assumption that the mechanism must be of a certain type in order to be considered legitimate. This argument is really an argument against the possibility of psychology in general, and makes no specific claim against researching the creative aspects of Music Therapy process.

Also, to proscribe these processes from study because they are private is placing a methodological concern before a substantive one. Mackenzie's analysis has shown that this is not defensible on either metaphysical or historical grounds. It would seem counter-scientific to stipulate, in advance of
actual attempts, that certain types of processes are not amenable to scientific investigation. Certainly it is possible that researchers can devise new methods of investigation and novel ways of utilizing current methods. It is not clear what is gained by advocating the avoidance of scientific exploration of any domain.

The second objection is a little more subtle and does seem answerable by empirical considerations. What it assumes is that it is the actual music used—not the process of creation—that is essential to achieving therapeutic results, and hence that the source of clinical music is irrelevant. In other words, it is the unique combination of scales, intervals, melodies, tempi, rhythms, etc., embodied in a specific improvisation that does the therapeutic "work." Here, the therapist is merely a vehicle for music which, if the appropriate rules could be formulated, could just as easily be produced by a computer, for example.

This view carries with it some theoretical baggage that should be made clear in order to more clearly contrast it with the one argued for in this study. While it is difficult to conclusively demonstrate that this is a "wrong" view of clinical improvisation, by elaborating on its implications I hope to show that it is a less reasonable one than the alternative. My goal here is to demonstrate the importance of understanding the generating principles of the music in clinical
improvisation, by showing the difficulties caused by considering them to be irrelevant.

If it is the unique combination of musical elements that accomplishes the therapeutic objectives, then it follows that for each clinical meeting there exists a single, "ideal" improvisation which will best serve the client's needs at that moment. One is further committed to a view of music where its elements convey specific, universal meanings, and whatever quality defines music as such, is embodied completely within the physical characteristics of the tones employed, such as their frequency, duration, intensity and harmonic relationship to previous and subsequent tones. For if there is a single best improvisation at any given time, it must be the case that this improvisation is not communicating something which could be equally well communicated through a variant musical instantiation.

One consequent is that the therapist's creative contribution is minimized, if not entirely eliminated here. It is as if the clinical milieu is a puzzle with one correct answer, and the therapist's task is to figure out this answer, musically. The only room for creativity is in the activity of puzzle solving, not in the solution itself.

Also minimized is the importance of the therapeutic relationship. If it is specific attributes
of the music accomplishing the therapeutic goals, and the meaning of this music is independent of the interpersonal context in which it arises, then the nature of the therapeutic relationship is no more important to therapeutic outcome than it is when one receives an inoculation from a trusted family doctor or from a total stranger.

In sum, to advocate for the irrelevance of understanding the source of music in clinical improvisation is to support the following notions: the elements of music possess literal, universal meanings; engagement in the creative process itself is relatively unimportant to therapeutic outcome; the dynamics of the therapeutic relationship are not relevant to understanding Music Therapy process; and lastly, verbally-formulizable rules embody the extent of knowledge and skills possessed by accomplished clinical improvisers. Obviously, this constellation of beliefs represents a "difficult pill to swallow" and appears to undermine the very reasons for utilizing a creative, aesthetic medium in treatment.

Although these considerations illustrate the conceptual difficulties inherent in the view that the generative principles of clinical music are irrelevant to clinical outcome, it is primarily this author's experience--as both client and therapist--that has led to the focus on these principles in the current study.
When listening to recordings of sessions I can say things like, "Yes, here I am providing a left-hand ostinato bass while mirroring the client's scatteredness in the right hand, and using a minor key to match the client's depression." Yet to say that these actions are rule-governed is to say that I am consciously enacting these rules and procedures at the moment of musical creation. Though the descriptions can be fitted into certain rules, the actions are not rule-governed ones because the nature of the clinical music is not being determined by the conscious implementation of rules. To aver that the creation of improvised music is rule-governed is to misrepresent the process of improvisation in a fundamental manner. It would suggest that the salient healing aspects of the improvisation result from the following of rules, an inaccurate conclusion that would obscure the fundamental mechanisms of Music Therapy process. The rule only exists as a post-hoc projection upon the phenomena, and, while maintaining some measure of descriptive truth, is not enlightening in an explanatory sense.

Music Therapists certainly utilize guidelines in their improvisations, and I do not mean to deny this. What I would like to distinguish is the concept of rules as determining actions from the situation where rules function to limit possible actions. In following
a recipe we allow formalized procedures or rules to determine our actions, and these actions can be fully accounted for by reference to these rules. Yet, in a game of chess knowing the rules of the game is not enough to account for a player's moves. These are dictated by a strategy, which though limited by rules, is not determined by them. Merely understanding the rules of how pieces move is not to understand the process by which the player's successive moves are generated.

Needless to say, I believe the situation in Music Therapy is more akin to that of chess than to cooking. As such, rules and guidelines can still play an important role in both describing clinical improvisation and in teaching this skill. Providing students with verbally articulated guidelines is a way of shaping desired action that will later be guided and determined by a more intuitive, internal mode. In this way the guidelines or rules function like training wheels for a child learning to ride a bicycle. They mimic the experience of riding with balance, but are not identical to it, though externally the result is the same: not falling! In fact, just as with these training wheels, the rules and guidelines offered to students hopefully will render themselves obsolete.

As my own clinical skills have developed they have become decreasingly dependent on external guidelines
and guided more by intuition. The change has been a gradual one, moving though a series of continuous steps, yet resulting in a qualitatively different mode of conducting therapy. Just as the skill of riding a bicycle involves relinquishing the need for external aids, the skill of clinical improvisation is attained when external considerations (rules, guidelines) no longer determine the nature of music produced.

Thus, what is not capturable by the language of rule-based decision procedures is the intuitive, creative component of Music Therapy process. It bears repeating that Music Therapy takes place within an art form, participation in the creative aspects of which are not incidental, but integral, to receiving its benefits. Any conceptualization of the process which cannot account for the contribution of its creative aspects will not fully capture its essence. The deterministic nature of rule-based procedures mitigates their appropriateness for use as explanatory devices for creative clinical processes that demand novelty, spontaneity and a flexible engagement in a constantly changing, organic process.

All of this is not to say that therapists will not exhibit patterns and habits in their improvisations. Individual Music Therapists possess musical predilections that reflect their musical selves. All music contains form, and in improvisations these forms
emerge from combining present feelings, intuitions and expectancies with past experiences. Yet, the most clinically productive and meaningful aspects of Music Therapy process occur when these habitual musical patterns are transcended and client and therapist join in forming a unique creation. This creation takes a certain musical form, but this implies neither that what renders the creation therapeutic is specifically that form, nor that the therapeutically salient qualities are communicable in the language of scales, rhythms and harmonies.

Levels of Experience in Music Therapy Process

Another fundamental tenet is that any model of clinical improvisation must account for all the various levels of interpersonal contact and emotional intensity typically experienced within effective clinical improvisations, including those peak moments normally resistant to verbalization. These moments are not seen as unexplainable aberrations, but as the actual goal of the process. They embody the most powerful and therapeutically salient aspects of clinical improvisation, and a model that cannot account for them will not only be incomplete, but incorrect.

Realistically though, every therapy session does not reach such transcendent heights, and indeed not every clinical encounter demands such intensity of
experience. It may be that significant aspects of Music Therapy process are explicable by the very rule-oriented procedures that have been criticized thus far. Yet—to take an example from physics—the entire edifice of Newtonian mechanics was overcome by relativity theory, based upon phenomena existing only in highly specific and rare conditions, far removed from daily experience. Einstein's famous "thought experiments," wherein he imagined the effects on the observed speed of light by an observer in motion at that velocity eventually led to the view that Newtonian assumptions of absolute space and time needed to be abandoned. It was the necessity of including the aberration, the rare event, in the explanatory system that led to scientific progress, indeed revolution, in this instance.

I would argue that the peak moments in Music Therapy process are analogous to the rarely occurring conditions envisioned by Einstein. They are an essential part of Music Therapy process and any model that can make claims to a realistic portrayal of this process must account for such moments.26

Though it is appropriate and necessary that these aspects of creative clinical practice be researched systematically, it is in the purview of the present study to discuss them in a global sense. It is hoped that the more fully detailed picture of clinical
process that emerges will help to provide clinically relevant constraints and directions for researchers, through participation in the dialogue among Music Therapists seeking to communally establish the essential elements of creative clinical practice.

It has been my experience that these most powerful moments in clinical improvisation consist of pure music-making, not constrained or described by categories or concepts like health, illness, client, therapist, goals or clinical intent. These elements are the supportive social edifice that allow and enable the transformative process to occur, but they do not provide the impetus upon which the process turns. Thus, explanations of this process must penetrate to a level of human interaction beyond that typically provided by these concepts and roles.

**Musical Awareness in the Clinical Setting**

At the moment of creation what I am aware of is the music and feeling what it needs. I respond to the client completely via the music. What this means is not that I am translating the client's music into words, (this feels like angry music), formulating a verbal response (I think I will support this expression of anger), and then translating this verbal intervention into a musical realization (to support the anger I will play dissonant tones on the bass end of
the piano).

What is occurring is that I am becoming aware of the music as a unique manifestation of the client. The duality of person and act disintegrates and I experience the music as the person, not as a symbol or representation. I am living in my music in the same way as I am perceiving the client within his or her music, and while words can be used to later describe what occurred, the entire process takes place on a non-verbal, musical level. Therefore, whatever knowledge I possess as an accomplished clinical improviser is encoded in musical form. Any translation to verbal language will necessarily involve an alteration or distortion of nature of this information. Any attempt to research this process must take into account the manner in which the chosen language will affect what is revealed about the process.

As an example, consider the traditional psychodynamic view of music as a symbol of various inner processes. The use of the term "symbol" implies a triadic relationship among the symbol user, the symbol itself, and the referent of the symbol. Yet, as Dewey rightly observes, this conceptualization is a product of reflection upon direct phenomena [and is] not a description of what happens when so-called symbols are potent. For the feature which characterizes symbolism is precisely that the thing which later reflection calls a symbol is not a symbol, but a direct vehicle, a concrete embodiment, a vital incarnation.
The integration or interpenetration of the person with the music that I describe as being characteristic of clinical improvisation more accurately captures the nature of this event than does the dualistic or triadic relationship resulting from the post-hoc, psychodynamic analysis. This non-dualistic view better maintains the quality of my clinical experience through the various stages of analysis characteristic of research; it better represents those elements of the experience that define it and render it of interest to begin with.

Some may find the notion of musically-encoded thought to be a problematic one, or at least difficult to grasp. In this regard, Dewey comments that

thinking in terms of colors, tones, images, is a different operation technically from thinking in words. But only superstition will hold that, because the meaning of paintings and symphonies cannot be translated into words, or that of poetry into prose, therefore thought is monopolized by the latter.\textsuperscript{26}

Accepting the possibility of knowledge that is gained and encoded musically prepares the ground for the epistemology of Music Therapy discussed in Chapter VIII. This is an important notion because if one believes that knowledge is only considered as such when encoded verbally then there is no pre-existing content to which the verbal accounts of Music Therapy process must be either adequate to, or evaluated by. Once this musical knowledge is accepted as legitimate, standards are generated to which our verbal renditions of musical
process must be accountable. The verbal report becomes not just a convenient metaphor functioning equally well (or poorly!) in a variety of conceptual systems, but is instead measured for accuracy against the musical knowledge.

**Intentionality in Creativity**

The personal realizations recounted above have led me to the somewhat paradoxical conclusion that the most clinically effective experiences have been those where I have not been consciously acting as a therapist, if being a therapist means the conscious application of deliberate, verbally articulated interventions oriented towards desired ends. Not attempting to research and understand the mechanisms of clinical improvisation is to live with the paradox. The commitment to understand this deeper level of practice requires an openness to discovering what it is that Music Therapists actually do. Our definitions of therapy will then reflect actual practice, and not result from a priori values or previously held theoretical commitments. We must **discover** what Music Therapy is, not proclaim it.

Functional descriptions (those where the musical act is described by its role in effecting the clinician's intent) are not as rigid as rule-based analyses and can produce statements regarding clinical activity that have a degree of reliability and
validity. Because their claims are weaker it may be thought that functional analyses can circumvent some of the difficulties of the rules approach. Again, I can listen to a recording of my own clinical work and make comments like "Now I am reflecting certain sound forms," or "Here I am helping to expand the client's expression by stretching beyond previous boundaries," and these observations are true, as post-hoc analyses. Yet, it bears re-stating that at the moment of creation what I am conscious of is purely the music and feeling what it needs. Possibly the client is reaching a new level of emotional involvement in their own expression as a result of my musical contribution to the improvisation, but this occurs not from my conscious intent—though it may be a general goal—-but as a secondary benefit or consequent of involvement in a more primary process: the creative one. Thus, it is invalid to portray the therapist's actions as exclusively guided by guidelines connecting specific musical forms to clinical intentions, whether these guidelines are considered to be rules, or less rigidly characterized as purposes, formulated at the moment of creation.

Any model which purports to represent clinical improvisation must account for the contribution of involvement in the creative process, a notoriously difficult area to define and research. But these
difficulties are no reason to avoid exploration here as clinicians function in this realm on a daily basis. For a research program to be applicable to clinical practice then, a conceptual foundation must be established which can support investigations of creativity. This is the central problem posed by Music Therapy practice and any methodology or philosophical orientation that precludes its study needs to be abandoned, for its maintenance cannot be justified by indigenous considerations.

Possible Objections to this Clinical Portrait

Perhaps it would be fruitful to pause here and acknowledge that the view I am proposing is a fairly radical one and examine the areas in which it most starkly contrasts with traditional views.

Probably most novel—and potentially problematic—is the recognition that therapeutic progress occurs when the therapist abandons conscious and deliberate action and places trust in an external, creative process. This might be taken as an abrogation of professional responsibility on the therapist's part and seem to relegate Music Therapy to the status of a mysterious, intuitively practiced discipline, at best akin to artistic/creative activities outside of the clinical milieu, at worst, analogous to scientifically discredited activities such as astrology. Accepting
this analysis would mean that the very purpose of this study—providing a basis for scientific research—would seem to be subverted.

However, I do not believe the situation is so bleak. The first step is to empirically ascertain whether or not accomplished improvisers actually work in the way that I have described. Though it is my suspicion that non-formalizable, intuitive factors play the fundamental role in effective clinical improvisation, adherence to professional conventions which do not recognize the clinical legitimacy of such factors have probably led clinicians to minimize their importance in the past. In any event, such musings are certainly subject to empirical investigation, and it is one of the prime purposes of this study to encourage just such research efforts.

It is in the very essence of scientific activity to explore a process whose workings are unknown—such as clinical improvisation—regardless of the implications of such study. It is typical for novel research efforts to be resisted because of their conflict with current social, religious, academic or even scientific values. One can see this type of resistance in the social reaction to evolutionary theory, a reaction that does not so much dispute the facts upon which the theory is based, but which rails against the theory due to its "relegation" of humankind
to a status equivalent to the rest of the animal kingdom—one of its implications. If systematic study of clinical improvisation leads to discovering the prominence of creative and intuitive factors that conflict with professional orthodoxy, then any accurate model and theory must weigh and account for their influence, regardless of the perceived social and professional implications.

What is probably the case is that each clinician utilizes a unique combination of rational, intuitive, verbal, imagistic and musical processes while improvising. Each experiential mode provides different types of information, thus playing a unique role in determining the music thereby produced. Some clinicians may utilize a sequence of these types of processes as an improvisation progresses, while others may process information on a variety of levels simultaneously. All I am arguing for here is epistemological tolerance in determining the clinically salient processes utilized by accomplished improvisers. This means not banishing intuitive, creative and other non-rational processes from study, because of a priori concerns, whether of a theoretical or value-laden nature.

It might also be taken that if musical interventions are ultimately the product of non-formalizable processes, then Music Therapy as a
treatment form is somehow discredited. I do not think that the latter statement necessarily follows from the former. A scientific theory, for example, can arise as a deduction from the contemplation of known facts, from a thought experiment never actually conducted in the laboratory, or from an image arising in a dream. The source of the idea—though an integral part of the scientist's activity—does not strictly determine the reasonableness of its acceptance. The important consideration is that the theory functions well in solving the problems it addresses.

By the same token, if it turns out that Music Therapy treatment ultimately springs from intuitive, non-rationally reducible processes, this would in no way mitigate the reasonableness of its use in treatment—if its efficacy is demonstrated. And this efficacy is not dependent upon having a neurological, psychoanalytic, physiological or behavioral rationale, but by the results of clinical work. The opinion that only rationally explicable processes can promote health or well-being is a conceit of our technological society, one that may need to be expanded upon as we discover more about the underlying processes involved in creative therapies. It may seem paradoxical, but what I am advocating here is that clinical experience can render it reasonable to implement a process whose inner workings may not be logically deducible. This
need not involve an abandonment of our critical and skeptical faculties.

One last issue to discuss is that this view might seem to denigrate the importance of theory and training for the effective practice of Music Therapy. I do not believe this to be the case. In saying that creative and intuitive processes are the most clinically salient ones, I am saying neither that theory is irrelevant, nor that these creative and intuitive processes cannot be trained and developed. If we look at other creative domains we find that art students spend many hours studying and copying classic paintings and jazz musicians are equally committed to learning scales, chord sequences, and the important solos played by master musicians. A specific body of knowledge is passed on in this manner, which may or may not be verbalized. Creative acts, however, demand that the rules and principles thereby gleaned become the tools of creation and not its master.

Clinical improvisation, insofaras it involves a similar measure of creativity, demands a similar measure of commitment to theory and principles, as well as an abandonment or suspension of such rules at the moment of creation. The theory can help us to understand why the music was effective, but cannot be used to directly generate the clinically effective music. Just as music theory enables us to better
understand the genius of a Beethoven without helping us to compose with the same level of genius, Music Therapy theory helps us to understand our clinical music without necessarily providing guidelines for its production.

In the same vein, some of what I am saying can be construed as a criticism of the social structure of therapy as it currently exists. One might think that I am advocating that concepts like client, therapist and clinical intent are transparent or unnecessary conventions when I say that I am not consciously acting as a therapist and implementing specific goals while improvising. Again, nothing could be further from the truth. As I said earlier, this social structure is the supportive edifice which allows and enables the transformative experience to emerge. By providing feelings of safety, regulation and a context of professional responsibility, this structure helps to ensure the integrity of Music Therapy treatment. What I am arguing for is a recognition that the components of the enabling social structure are not themselves the agents of change.

The Nature of Music and the Therapeutic Relationship

Many of these views on clinical improvisation are inseparable from a specific view on the nature of musical meaning in the clinical milieu—specifically,
that the music created in Music Therapy process does not possess absolute meanings or universal significance. It must always be understood in reference to the individual whose emotional life is its source, and the therapeutic relationship from which it is born. These are the elements which provide the context in which the meaning of the music is embedded.

To make this more clear consider a musical example. Observe that the "D" note above middle "C" acquires a dynamic quality when it is sounded in the context of a "C" tonality that is absent when it is sounded alone. Further, this quality is enhanced when the key of "C" is one that has been modulated to or when the "D" note becomes part of an important theme or motif, and is changed dramatically when the contextual key is altered. In all of these cases, the note is infused with a dynamic quality that is not explicable in terms of its physical characteristics, which do not change and cannot be correlated with the changing dynamic quality. The tone carries and conveys a dynamic quality which has a separate and independent existence. As Zuckerkandl rightly notes: "Nothing in the physical event corresponds to the tone as a musical event."27

Holding this view of music is consonant with a view of clinical music where it is not the specific musical tones which do the therapeutic "work" but
instead some other force or quality which is conveyed through the tones. This dovetails with the position expressed earlier that no number of rules regarding tonal content can mirror the generating process of clinical improvisation. If the tones themselves only serve as carriers of some other quality, then the therapeutically salient aspects cannot lie in the tones themselves, nor in the specific musical instantiation. In fact, it is proposed here that a multitude of musical instantiations can have the same clinical meaning, just as a similar clinical intent can be conveyed through a variety of musical realizations. If one's clinical intent is independent of the musical form used to convey that intent, then formulating rules correlating musical content with clinical meaning is to establish purely fortuitous and correlational relationships that have a minimum of clinical and causal relevance.

Any attempt to understand clinical improvisation must then explore its context in order to glean its meaning. And the appropriate context is not other musical events, but the interpersonal matrix in which the therapy takes place. This involves not only looking at the process as an act of communication among individuals, but in fact recognizing that it consists of an even more basic step: this is the very establishment of a language, one which reflects the
unique properties of the individuals and relationships involved.

Assuming this perspective of music highlights the limitations of the behavioral approach. Consider an example where the therapist would like to work on communication skills with a self-abusive, autistic child. If one wants to establish communication then it follows that each of the child's actions must be perceived and responded to as an expressive, communicative act. The self-abusive behaviors can mean any number of things, some mutually contradictory, including the following:

I'm angry and if you try to stop my head-banging I'll do it even more when you are not around.

The only way I get physical contact is when I am restrained from hurting myself, so by holding me, know that you are encouraging this behavior.

I'm angry now and have no other way to express it.

This is an involuntary act. Please help me to stop.

Though the truth status of any one of these statements can only be inferred, this is no reason whatsoever to exclude them as valid data, whether for purposes of research or treatment. If the history of
philosophy has shown anything, it is that no statements about experience can be deductively validated. To hold observation statements to this criteria is unnecessarily limiting.

So if the therapist focuses solely on extinguishing or eliminating the behavior, then all of the various potential meanings of the act are, in effect, rendered equivalent or meaningless, and the child does not experience an act of communication. He does not learn that the quality of his inner life, as it is expressed in external behaviors, is received and responded to by others. If one wishes to establish communication, then this approach is counterproductive.

Rather than controlling behaviors, the view expressed here is that therapy should be conceived of as providing enhanced choices. No "problematic" behavior can be addressed in therapy unless it is experienced as such by the client. Isolative or self-abusing behaviors are voluntarily engaged in by many individuals considered to be psychologically healthy. The behavior is only a problem addressable through therapy if it results from compulsion and causes the client distress. What I address as a therapist is not the isolative behavior, for example, but the fear that keeps someone isolated who wants more contact. The behavior itself has no meaning—for purposes of
therapy—apart from reference to the individual's attitude towards it.

We can move the same example into the musical realm. Consider a child who pounds a drum loudly in a steady, unvarying tempo for extended periods of time. This tendency—which would usually be described as compulsive or perseverative—can also result from a variety of factors, and therefore can be communicating a variety of feelings. It can result from anger, fear, joy, an attempt to shut out the therapist or an attempt to engage the therapist. The therapist's musical response must depend on the dynamic or feeling quality of the music, which is independent of its literal, purely physical aspect.

The same is true whether the drum beating is replaced by the continuous use of an interval of a fifth on the piano, a repeated melody or any other musical element. The very process of Music Therapy involves the creation of a mutually understood language between client(s) and therapist, not the implementation of one whose referents are pre-determined. Since this language of experience is created among the participants in the clinical process, its referents are best understood by them and are independent of the language's purely physical characteristics. Investigation of the participants' experience is therefore the only way to research the nature of
clinical music, and thus understand Music Therapy process.

One additional clinical example will serve to demonstrate the necessity of taking into account the personal meaning of the music from the client's standpoint. Consider a behavioral Music Therapist who wants to increase the frequency of an autistic child's social behaviors. The therapist improvises music—or provides some other musical reinforcement—whenever the child moves away from the door of the session room and towards the therapist. Now the problem is that we do not really know if the child is being conditioned to avoid doors or approach therapists! Or perhaps the child is being conditioned to approach the piano, not the therapist, in which case the social goal is being subverted.

The behaviorist gets around this difficulty by saying that it really does not matter or make sense to ask what the child is "really" being conditioned to do. All that matters is that the child is physically approaching the area in space where the therapist is. This is how the goal of "increasing social behaviors" is being operationally defined. And as long as we only consider the treatment or experimental milieu—with consistent schedules of reinforcement—the therapist may have a valid point.
Yet what happens when the child is in the classroom, home with his family, or in any other setting. Even if one grants the behaviorist assumptions that all behavior is learned, and that these conditioned behaviors can generalize beyond the experimental/treatment milieu, what behavior is actually going to be generalized? Is the child going to approach people more, approach pianos, or just avoid doors? These questions are not merely rhetorical as a major problem with behavioral therapies is that conditioned behaviors tend not to generalize outside of the experimental milieu. Part of the reason may be that ignoring the experiential awareness of the client prevents the researcher from really knowing what behavior was actually conditioned. The judgment that the behavior conditioned is the one the therapist was focusing on is just that, a judgment. As such, behavioral evaluations of therapeutic outcome are necessarily subjective, and have no inherent advantage over more experientially-oriented research and treatment methods.

Granted that it is essential to understand the meaning for the client of clinical music, it is not enough to seek this meaning in the purely physical instantiation of the music, as this can lead to a stereotypical view of expressive acts which are, in fact, inherently unique. The deeper significance of
the act can be obscured through the category it is placed in. This can happen when, for example, an autistic child beats a perseverative drum pattern which is described as a defense mechanism or an example of therapeutic resistance, by virtue of its perseverativeness. Yet, just by picking up the drum beater the child is altering the environment that the supposed defense is said to be maintaining the same-ness of. Some aspect of the child's inner state is brought into the outer world where it becomes a public declaration. There is a communicative significance in this act, the deciphering of which demands that one perceive beyond the boundaries implied by the pejorative psychological description.

The weakness of putting musical actions into these psychological categories can also be seen when the positive aspects of musical interaction are explained by psychoanalytic mechanisms such as circumventing defense mechanisms. It is not clear what explanatory power is gained by attributing the efficacy of music to the avoidance of a process (defense mechanisms) not itself observed. This tells us what the musical expression is not, e.g., it is not an example of "defended expression." This still leaves open what the music is and what mechanisms or processes it does activate.
Further, to explain the efficacy of Music Therapy in terms of its ability to circumvent defenses is to say that the music is communicating something that would better be communicated through words, if the client was healthier; it is to make it a deficit expression. The need for Music Therapy (or any non-verbal, symbolic expression) is then only an indicant of illness or some sort of deficit in functioning. This is an orthodox Freudian approach where symbols only convey that which is too threatening to be directly felt or expressed.

Yet, when it is recognized that music expresses the truly ineffable then music is seen as a bona-fide symbol, not overlapping with articulate thought or feeling. It exists in its own right and no verbal translation can fully capture its significance; no amount of translation rules can exhaust its meaning.

Each of our musical histories—though sharing common elements—is completely unique, and therefore each of our perceptions of, and reactions to, another's music is unique. To experience the deeper levels of meaning embedded in clinically improvised music the therapist cannot act merely as a mirror or neutral observer, but must react instead from a human, emotional response to the client and the therapists's music. This is an essential characteristic of true therapeutic work. To create music with clients—music
which flows from the emotional fabric of their fears, desires, joys, present conflicts and historical trauma—it is necessary to respond to its emotional tone. Our own emotional capacity is the instrument through which we experience and become aware of another's emotions, and hence the meaning of their music. Therefore, as therapists it is not that our own affective processes interfere with our perception of the meaning of a client's music. Rather, it is the actual vehicle through which we apprehend this music.

We cannot merely observe the emotional quality of music the same way that we can observe that it is in a major key, or that it is in a rondo form. The emotional quality of music is something that we feel. This does not mean that we have to be joyful, for example, to perceive this quality in a piece of music. What it does mean is that perception of musical meaning is a necessarily subjective experience. And to join in the creation of music is to participate in an even deeper way, demanding complete emotional involvement in the present moment. In Music Therapy, therapists allow the music into their beings, simultaneously extending their being through infusing their selves into the musical domain.

In this way, tones function as an extension of one's being—indeed are extensions of our being—as they penetrate our psychological, social, and physical
environment, much as a microscope or telescope provides biologists and astronomers with information and experiences otherwise unattainable.

The difference is that in music the method of discovery (musical interaction) is simultaneously the experience which is sought. Musical interaction is not analogous to sonar with the participants sending out vibrations into the world and interpreting the subsequent echoes. Although this is part of the musical method, an analysis that stops here treats music solely as sound and ignores the aesthetic dimension. This dimension demands an analysis of the actual experience of "information gathering" because the information gathered is the individual's experience during the musical interaction.

It should be emphasized that the inability to experience or produce music in a neutral, purely objective fashion, is not by any means a limitation placed on the therapist. Since sounds only acquire meaning and significance, i.e., become music, through their relationship to inner, subjective states, it is not clear that access to musical sounds as they exist in and of themselves, prior to interaction with our subjective experience, will be of any use to the therapist.

The therapist's emotional response to the client is not seen as a barrier to be overcome, or only as a
counter-transference to be avoided or analyzed, but as a necessary component of the therapeutic process. Since music acquires its meaning on an affective level it is impossible for the therapist to engage in the creation of therapeutically effective music without maintaining a significant level of involvement in her/his own affective process. This means being aware of one's own feeling reactions to the complete therapeutic milieu and playing from this awareness.

Since the therapist's subjective reactions are not seen as incidental, avoidable aspects of the therapeutic process, but instead are an essential component of this process, they must be included in any complete accounting of Music Therapy process. This is true whether the accounting is for clinical, research or training purposes. The data that the Music Therapist can be said to be responding to, i.e., the observation base of this discipline, coalesces or emerges on the level of the therapist's subjective experience and is only marginally connected to the physical, publicly objective, character of the music.

The concept of clinical objectivity, if it is then to be preserved, must undergo a radical re-definition in the context of Music Therapy. It cannot mean the detached, dispassionate, and singularly objective evaluation of a client. The demands of sharing a musical creation, involving the emotional participation
of both parties, preclude such a view. Objectivity can be maintained in the sense of ensuring that the mutually created music is meeting the client's needs, and springs from his or her present reality and the therapist's reaction to this reality.

We then see the therapeutic relationship as just that—a real human relationship, created on the strength of a unique personal bond. Any attempt to understand the mechanisms of therapeutic interventions, and the manner in which they facilitate personal development—in other words to understand Music Therapy process—must take into account and study the dynamics of this relationship. It is the very context from which the events of a therapy session derive their meaning.

The Nature of Objective and Subjective Judgments in Music Therapy

The four factors discussed above—the meaning of music not being iso-morphic with its physical structure, the importance of the generative process involved in clinical music, the value of the therapeutic relationship in providing a context for understanding clinical music, the necessity for therapists to act from their own feeling response—all underscore the necessity of studying the subjective experience of both client and therapist in order to illuminate the mechanisms involved in creative clinical
practice. Any research paradigm with claims to clinical relevance must be conceptually supportive of this type of investigation.

Just as naturalistic research has been denigrated because it has been equated with anecdotal, and therefore invalid, reports, empirical investigations of experience have also been considered un-scientific and to be mistrusted for the same reasons. Usually the criticism here takes the form of saying that since the mechanisms involved in these subjective judgments are private there is no objective criteria for evaluating their relative validity. Also, since they arise from historical accident or the facts of a particular individuals's psychological make-up, they are not generalizable for purposes of science. While each of these premises is true to some extent, neither of the conclusions necessarily follow.

These issues will be discussed on a more in-depth conceptual level in Chapter VII of this study. It should be noted here, however, that both the context of Music Therapy process, and indigenous research considerations, offer unique perspectives in considering the validity of these objections to the study of subjective processes.

All scientific activity involves inference at some point. The Received View held that this step occurs between recording observations that are objectively
known for certain, and the postulation of theories to account for these "facts." Yet an argument was presented that endeavored to show that the observational/theoretical distinction was somewhat of an arbitrary one. If so, it is reasonable to utilize pragmatic considerations to draw this distinction, i.e., allow inference as a legitimate scientific maneuver, at a point that makes sense given the nature of a particular research problem. If it is agreed that investigating subjective experience is necessary to understand the mechanisms of clinical improvisation, this should outweigh objections based on claims of objectivity, claims which are themselves based more on convention than necessity. Whether one considers the contents of these subjective reports to be observed or inferred is of secondary importance. What does matter is that they be included in the research effort.

The fact that these subjective judgments are necessarily individual and unique is also not evidence for their exclusion as sources of scientific data. Much of scientific activity involves the discovery of patterns in necessarily unique events. Each individual organism has a unique genetic pattern, each snowflake its own pattern of construction, yet neither of these considerations preclude the scientific study of the processes giving rise to their unique structures. The inherent and universal processes of clinical
improvisation—if there are any—will only come to
light through studying and comparing their unique
manifestation in each individual practitioner.

Some of the criticisms of utilizing reports of
personal experience in research hinge upon a
necessarily vague definition of the term "subjective."
In one sense "subjective" is used to describe all of
the contents of consciousness, regardless of their
source or varying degrees of independent existence. In
another, more limited, sense "subjective" refers to
those contents of consciousness that are acknowledged
to be completely idiosyncratic to the experiencing
individual—and thus having no claim to an external
reality.

Consider the following series of statements:
Chocolate ice cream is better than vanilla; The opening
to Beethoven's Fifth Symphony is ominous; Blueberries
are closer to purple than blue; and, The thermometer
reading is closer to eighty degrees than to eighty-one.
All of these reflect personal judgments, though there
appears to be a progression from the first to the last,
in a direction that has been traditionally described as
increasingly objective. Yet, there really is no strict
line between "objective" and "subjective" perception,
and even the most "objective" perceptions (meter
readings for example) involve a cognitive contribution
from the perceiver. Thus the sequence above is not a
series of increasingly objective, i.e. veridical, assertions, but a series of assertions where the contribution of the perceiver is increasingly less controversial, though not absent entirely.

Giorgi introduces a distinction between objectivity and intersubjectivity which is more useful than the traditional distinction between objective and subjective:

Sometimes subjects grasp . . . objects appropriately and in harmony with others, and then we have objectivity and intersubjectivity; sometimes a single subject will grasp an object appropriately, but is out of step with others, and we have objectivity and not yet intersubjectivity; sometimes, we can have a grasp in harmony with others . . . but it is not grasped appropriately, and so we have intersubjectivity but not objectivity; and, lastly, one can grasp an object both inappropriately and out of tune with others and we have the "merely subjective," or perhaps "impressionistic."{28}

It is only through empirical research that we can determine if the art of clinical improvisation is more like ice-cream tasting or more like meter reading, in terms of the criteria of intersubjectivity and appropriateness (veridicality). If clinicians' reports about the nature of their experience during clinical improvisation achieves no intersubjective agreement, then we may conclude that this is a purely mysterious, intuitive, unexplainable skill that is not amenable to systematic investigation. If, on the other hand, patterns and areas of agreement emerge, we can assume that this process has an essence which can be
represented in some form.

Similarly, the information that therapists acquire—through a technique like clinical improvisation—about the inner world of their clients can be either appropriate or inappropriate, to use Giorgi's terms. Again, only empirical efforts can make a determination here; hence there is no grounds for dismissing research of experience on the grounds that it is necessarily "subjective" and beyond the pale of science.

Though clinicians' judgments about clients' music reflects personal factors, they are nonetheless grounded in an external event, which is the actual music. As was argued previously, the significance of music (clinical and otherwise) is not iso-morphic with its objective, physical structure, and therefore is not amenable to explication through a system of translation rules. Yet, recognizing that subjective, interpretive factors play a role in understanding music, does not imply that the mechanisms of this process of understanding are necessarily unique to each individual.

Towards a Language of Clinical Experience

In researching Music Therapy process, then, one must acknowledge that "subjective" judgments—as used to describe all of the contents of consciousness—are,
therefore, not all arbitrary. Obviously, if there exist individuals with varying competency in clinical improvisation, and this difference is not the result of blind luck, then accomplished clinicians must have a way of making judgments that are more grounded in the external world, i.e., the musical/emotional needs of their clients, than are those of beginning clinicians.

For the present author, the form that this awareness takes is fundamentally a musical one. For other clinicians, the primary form may be poetic, a visual image, patterns of energy or any other symbolic representation. What is important to realize here is that one must penetrate beyond the surface level of various verbal reports in order to discover their common elements. These can be discovered if practitioners are, in fact, engaged in a common process that they have merely labeled differently. The varying forms of verbal report utilized can potentially obscure actual similarities among practitioners, especially when the language used is a non-indigenous, theoretical one. It might be that two clinicians can listen to a piece of clinical music, describe it differently and describe how they would work with it differently, and yet still produce a musical contribution that would function in a similar manner regarding the effect upon the client.
Because in this example the verbal translation of
the more primary musical or imaginal awareness is
creating the illusion of a basic difference, any
research effort must analyze the effect of the chosen
language and find a way to minimize its potential for
distortion. Any investigation of clinical
improvisation must allow for practitioners to convey
the nature of their clinical experience in a form that
is as close as possible to that of the experience.

What I am saying is no more than that the
observing or measuring device should share some aspects
of the phenomena in order to detect its essential
characteristics. If the phenomena possess poetic
characteristics and these poetic characteristics play
an important role in its understanding, then one must
employ a poetic language or method at the level of
observation or data-gathering. In investigating Music
Therapy process there must be an epistemological
overlap, an homologous relationship, between our
research methods and language, and the essential
characteristics of the process.

This is why one must begin from some view of this
process in advance of creating a theoretical system for
its investigation. Only in this way can the most
appropriate "measuring device" be selected. And
although the tools of this research are specified prior
to the actual research activity, this does not mean
that they are arbitrarily selected. Only clinical experience can provide the necessary empirical guidance needed to build the foundations for a research program. The lack of clinical relevance of Music Therapy research indicates that the wrong language and conceptual systems have been selected, and therefore need to be supplanted, not complemented.

Using the terms of any non-indigenous theoretical system—such as those of psychoanalytic theory—to describe clinical process would not meet the criteria being set here for a valid observation language. Terms such as ego, repression and sublimation represent theoretical processes and entities that, by definition, cannot convey the nature of the direct experience that must comprise the observational base of an indigenous clinical model.

Understanding a musical event does not require translation into a verbal form. In the clinical milieu this understanding is direct and expressed in the musical response: it is self-evident. What we do need to effect this translation for is to communicate about it and therefore establish the elements of the communally held, tacit knowledge. Utilizing non-indigenous theoretical and language systems that are imposed on Music Therapy process assumes that we understand this process in its own right, and that there is common agreement upon the manner in which the
aspects of clinical experience are translated into the non-indigenous language systems, e.g., psychoanalytic or behavioral ones. Neither of these assumptions are justified.

Reductive Analysis and Creativity

Reductive analysis in science attempts to understand phenomena by breaking them into parts and studying how they function in relation to each other. Psychodynamic explanations and personality models are reductive in the sense that they attempt to explain an individual's actions and experience based upon the interactions of personality constituents, or parts. Interestingly, behavioral explanation is built on a similar conceptual foundation in that complex behaviors are analyzed into the more simple behaviors constituting their "building blocks." This type of theorizing in psychology mirrors one of the perennial thematic elements of physics: the search for the ever more basic particles of which the physical world is constituted.

Utilizing this approach in psychology results from combining the atomist philosophy just described with a concept of explanation that holds that we have adequately explained some phenomenon when we have fully articulated its structure. This component of the corpuscularian philosophy (see Chapter II)
"determine(s) when explanation stops."^{29}

Both of these conceptual supports for traditional psychological theorizing, though providing a research direction and rationale, nonetheless result from metaphysical beliefs whose validity cannot be unequivocally demonstrated. They embody only one type of possible theorizing, and should be abandoned when the indigenous needs of a particular research program suggest other possibilities.

The view advocated here is that clinical music is a phenomenon arising on the level of the whole individual, and it is in relation to this level of organization that it can best be understood. A personality theory that takes as its starting point an individual as a complete and unitary being is seen as being more conceptually in line with understanding an aesthetic process that springs from the need to create--a need that is a property of people, not egos or ids.

It follows that viewing Music Therapy process in terms of pathological categories does not provide a relevant dimension for analysis. If the holistic position just described is adopted, there is no rationale for reducing musical expression to the interactions of personality constituents, whether these interactions are healthy, i.e. normal, or destructive, i.e., pathological, ones.
What I have found as a clinician is that I am not treating schizophrenia, manic-depression, the inability to trust or the creative block, to name a few examples, but that I am working with an individual with various universal, human needs. Usually these involve the need for meaningful human relationships, a rewarding vocation or avocation, an existential sense of purpose in life, and the ability to expressively relate to others in order to meet these needs.

Psychiatric disabilities can place obstacles in the way of achieving any of these goals, just as less severe, non-pathological factors can. Yet the nature of the process that has facilitated clients' growth has been remarkably similar in a wide variety of clinical settings. In working with a variety of individuals I have found that clinical need is a more meaningful distinction to draw than that of disabling condition.

The need to create music is an intrinsic human activity, not necessarily better understood when reduced to, or explained by, other drives, needs or deficiencies. It is fundamental to psychological well-being because of its essential characteristics, not because of incidental, fortuitous, i.e., non-aesthetic, benefits. Hunger is an attribute of people, not stomachs, and an explanation of food-gathering behaviors is more fruitfully engaged in by referring to the person's awareness of hunger and motivations, than
by an account of the chemical reactions of the bodily organ. Just as one need not refer to hungry entities residing inside the person, to explain human hunger, the explication of music need not incorporate either non-aesthetically based processes, nor musical homunculi residing in our psyches.

Music Therapy process is then seen not as a reflection or symbol of something more basic, but is the actual phenomenon of interest. The therapeutic relationship is formed musically, and inter- and intra-personal conflicts are expressed and resolved musically. This does not mean it is correct to say that the conflicts and feelings are represented musically. This is to maintain a dualistic outlook that subordinates the musical process to a non-musical one. It would be more correct to say that a certain process of merging or identification takes place where the duality of symbol and thing symbolized is temporarily transcended. The identities of client and therapist become bound with their expression.

Since these peak moments are more accurately captured through a language that eliminates the dualistic distinction between person and act, we need a level of analysis that views Music Therapy process as a function of being as opposed to doing. Pointing to a musical interaction and saying: This can function as a symbol of our relationship, is different from saying:
This is our relationship. It is this latter statement which better captures the perspective on Music Therapy process advocated here.

Viewing Music Therapy process from the primacy of its creative components can provide the requisite, non-dualistic, level of analysis. However, since it is the creative element which is typically viewed from an external viewpoint, e.g., other psychological or philosophical belief systems, seeing it as primary involves a conceptual reversal akin to that of moving from a geo-centric to a helio-centric astronomy. As such, its usefulness can tend to be overshadowed by its novelty.

Yet the effect of this novel perspective can be softened if we acknowledge that Music Therapy process involves genuinely creative activities—literally the bringing into being of something—and must be described as an emergent property. Phenomena embodying creation-like the emergence of life from inanimate matter, consciousness from cells, and the bringing forth of aesthetic experience from raw materials—are all inexplicable solely through reference to the structure and properties of their constituents. Recognizing that Music Therapy involves creative processes of this character reduces the possibility of its explication through reductive, structural analyses. Thus, it becomes more reasonable to support the conceptual
primacy of creative process in establishing an orientation point from which to view therapeutic process. Any research paradigm for Music Therapy must then support investigations into the general nature of creativity, as well its specific applications to therapy.

Creation and Creativity: A Foundation for Music Therapy Process

This connection between creative processes (normally associated with artistic activity) and creation (typically of life) is not merely a metaphorical one. Genuine creative activity, if supported therapeutically, serves to establish, restore and maintain both the participant's sense of inner balance, as well as his/her feelings of existing in harmony with the external world. The paradigmatic act of all creative activity is the creation of the world, and subsequently, life. Thus, creative activity—whether in the artistic, scientific, or therapeutic realms—is intimately connected with the processes conducive to life, and hence the health of organisms.

Unfortunately, this thesis demands its own study and the argument can only be drawn in a rough form here. What I would ask of the reader is to accept the assumption that creative activity promotes physiological, emotional and spiritual well-being due to its homologous relationship with very basic
processes of the universe. Robinson identifies creativity with those processes that involve a reversal of entropy. Certainly the creation of galaxies, planets, biospheres, ever-evolving complex forms of life, cultural systems, and technological and artistic achievements, can be seen in this context. Each of these manifestations of creativity involves succeeding complex forms of organization, reversing the movement towards randomness characteristic of entropic processes.

Taking note of these relationships is not to say, however, that psychological health is reducible to biological or physical factors. What is being advocated is that the same forces that maintain say, the stability of an eco-system in a state of dynamic balance, are also operative within and among individual organisms to maintain their well-being. Creative activity is the manifestation of this force on a non-material plane of analysis, whether one conceptualizes this plane as a cognitive, logical, spiritual, or affective one.

The primary data relevant for investigating Music Therapy process, though possessing a physical manifestation, exist in a fundamentally non-material form. We apprehend the various manifestations of this process through sensory means, but is important to keep in mind that these images, symbols, pieces of music,
and other human artifacts, are specific, localized realizations, not the phenomena itself.

Though it may appear unconventional to say that the science of Music Therapy research must address itself to non-material processes, in fact science routinely investigates such processes, and uses them for explanatory purposes. Entities such as waves, fields, forces, inherited behavioral predispositions, archetypes and drives, though manifest and transmitted on the physical plane, are themselves non-material entities. In fact, information processing, the dominant explanatory paradigm in psychology, is based upon functional analyses of cognitive abilities, analyses that define and recognize processes independent of their physical realizations. The functional equivalence exists purely on a formal, non-material, level of analysis.

The basic problem in investigating creative processes is therefore not methodological, but conceptual. As was noted previously, creation involves the coming into being of a property not present in its constituents or antecedents. The examples noted before included matter emerging from a void, life from inanimate matter, and artistic creations capable of inducing aesthetic experience from raw materials lacking this capability. In each of these cases there appears to be a disconnection or unexplainable gap
between the phenomenon of interest and its constituents or antecedent conditions.

Yet these processes do occur! Symphonies get written, scientists devise novel theories and Music Therapists create music that facilitates the growth and development of their clients. If these phenomena are unexplainable given our present conceptions of what constitutes legitimate explanation, then it is more reasonable to revise our concept of explanation than to deny the obvious (their existence), or to define them as outside the pale of rational discourse.

Survival, health and well-being require a balance between the forces of stability and change, between predictability and novelty. Species must pass on successful adaptive mechanisms to succeeding generations, as well as possess a mechanism for developing new ones to cope with changing environmental conditions. To survive and develop, individual people must maintain a sense of identity or continuity, as well as be open to changes in their self-image. A conception of science which values predictability above explanatory power will deal better with the forces of stability, those that maintain same-ness, than it will the forces of novelty, those whose very virtue and adaptive value is due to their unpredictability.

Creative activity also partakes of this balance of forces. Every artistic masterpiece and important
scientific theory is simultaneously built upon its predecessors, while breaking from them in novel and unpredictable ways. In the same way, the therapist's task in clinical improvisation is to create music that reflects both the client's current state of being as well as that of the therapeutic relationship, while simultaneously suggesting and offering new expressive and relational possibilities. In all of these cases the connections to stability, stasis and the past are more easily analyzed than are the novel contributions supporting change and transformation. Yet it is the novel aspect of the creative act that is its defining characteristic. Ignoring its importance is to miss out on what makes the act of interest in the first place.

Conclusion

We have actually come full-circle in this analysis of the indigenous elements of Music Therapy process. Beginning with the idea that a clinically relevant research paradigm must address the problem of the generative process of clinical music has led us back to this same conclusion. What I hope is more evident now is the necessity of this line of investigation. Understanding this creative process is the key to understanding the efficacy of Music Therapy. Participation in the creative process is the therapy.

In this light, the two problems with previous
conceptions of Music Therapy--1) focusing on the musical product rather than the act of creation and, 2) explaining creative musical process from non-indigenous conceptual systems--share a common fault: That is the relegation of the creative process to a subordinate role. In order to understand this process of creativity--and thus Music Therapy process--we must approach it on its own terms, and with reverence.

The following statements summarize the view of Music Therapy process presented herein. They are not intended to function as a sequential argument, nor is any one statement contingent on any other statement. Instead, they are a constellation of mutually supportive beliefs, adherence to which this author believes will generate a meaningful, clinically relevant research program.

1. To teach the use of, utilize in treatment and understand the process of clinical improvisation, it is necessary to explore the source of music, its generating principles, within the client/therapist dyad.

2. Effective clinical improvisation is not amenable to explication by rule-governed decision procedures nor by functional analysis.
3. Effective clinical improvisation is a skill more like riding a bicycle than knowing how to do geometry. It is a knowing how—not a knowing that. Actions of improvisers are not adequately explained by any set of propositions.

4. An effective model of clinical improvisation must be able to account for the most powerful aspects of clinical contact, as well as those occurring more commonly. The peak moments are not aberrations to be ignored, but are the actual goal of Music Therapy process, thereby providing direction to clinical efforts.

5. The most powerful moments in clinical improvisation consist of pure music-making, neither constrained nor described by concepts of health, illness, client, therapist, goals, etc. These are the supportive edifice that allow and enable the transformative process to occur.

6. The knowledge possessed by superior clinical improvisers is encoded in a musical or otherwise non-verbal form. Translation to verbal language necessary involves a degradation of the contents of this knowledge.
7. To fully understand clinical improvisation it is necessary to have insight into the general nature and importance of creative activity.

8. The music created in Music Therapy process has no absolute meaning or universal significance. It must always be understood in reference to the individual whose emotional life is its source, and the therapeutic relationship in which it is born. The client's musical expression must be treated as an act of communication whose referents are established within the bounds of this relationship.

9. In clinical improvisation the therapist acts not merely as a mirror or neutral observer, but acts from his/her own human/emotional response to the client. This must be taken into account in exploring the importance of the therapeutic relationship as a mechanism for achieving the goals of therapy.

10. The defining characteristics of Music Therapy process are emergent properties not reducible to, nor captured by, isolating the elements of this process.
11. Understanding the nature of a therapeutic relationship, formed through musical interaction, is essential to understanding Music Therapy process.

2. Ibid., 4.


6. A cassette tape provided with Nordoff & Robbins, *Creative Music Therapy*.


9. Ibid., 5.


11. Dudley Shapere, Scientific Theories and Their Domains, In Frederick Suppe (Ed.), *The Structure of Scientific Theories*.

12. Ibid., 521.

13. Ibid., 521-522.


15. After attempting to correlate such measurements as the distance of each planet from the sun, and the relative sizes of the planets, Kepler discovered that the difference in the maximum and minimum angular velocity of each planet as viewed from the sun conformed to the harmonic ratios.


19. Ibid., 34.

20. Of course this is not strictly true. Third party observers viewing therapy sessions have also available to them the same non-traditional, experiential data as do clinicians and clients. My point here is that the most important source of data is more accessible to the participants in the process—clients and therapists—than it is to any external observers.


24. At the time of this writing, Dorit Amir is conducting just such a study at New York University.


THE ROOTS OF MUSIC THERAPY: TOWARDS AN
INDIGENOUS RESEARCH PARADIGM

VOLUME TWO

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CHAPTER VI

PHILOSOPHY OF SCIENCE: INTERLUDE

Current Developments in Philosophy of Science

Chapter II provided the conceptual tools to evaluate the historical material that was presented regarding theories of research in Music Therapy. Yet philosophy of science has developed further than the point reached at the end of that chapter. Here, I would like to briefly summarize that development in order to provide a foundation for 1) the subsequent comparison of the exigencies of researching creative clinical practice with the underlying tenets of traditional Music Therapy research theorists, and 2) the conceptual alternatives that will be offered in place of traditional research guidelines.

Many of the objections raised to the Received View were so damaging that it can now be said that "virtually all of the positivistic program for philosophy of science has been repudiated by contemporary philosophy of science."1 It is important to realize that this means that many of the cherished, common-sense views about science--especially those regarding the possibility of neutral, objective
observation, the nature of the logical relationship between explanation and prediction, and the ontological status (or lack thereof!) of theoretical entities—have been abandoned.

The Received View represented an attempt to create a purely formal account of scientific progress where all sociological and psychological factors were purged. These included things like "the psychological activities of observation and inference, the belief systems of individual scientists, and the collective and individual use of images, metaphors, and analogies."² With the failure of this account, other philosophers created a view of science where these factors played a central role. Yet the Weltanschauungen analyses (those where the meaning of scientific terms are dependent on a specific conceptual perspective or world view) of authors like Kuhn, Hanson, Toulmin and Bohm, which at one time were the primary alternatives to the Received View are themselves now judged by some not to be "serious contenders for a viable philosophy of science."³

The primary difficulty has been to reconcile the rational development of science with characterizations of its method that rely on idiosyncratic, subjective and other unformalizable criteria. As an example, consider a common criticism of Kuhn's position that theoretical disputes in revolutionary periods of
science are really disputes between incommensurable worldviews. If this incommensurability thesis is true—meaning that no empirical evidence can resolve the dispute—then the process of science does not consist of movement towards greater knowledge or truth, but merely involves the succession of ontologically equivalent worldviews. One is stuck with a relativistic notion of scientific knowledge that most philosophers (including Kuhn himself) prefer not to argue for. The burden for Kuhn, and the direction his later works take, is to show how his paradigms or disciplinary matrices can replace each other in a manner that allows for the rational growth of knowledge. Current criticisms of Kuhn hold that he has not been able to do this.

Although Suppe judges that the various world view (Weltanschauungen) analyses do not offer "promising avenues for the development of an adequate understanding of science," he does acknowledge that contemporary conceptions have been strongly influenced by them. This influence is primarily seen in the manner that actual scientific activity—historical and contemporary—is used as criteria for judging the worth and value of current philosophical conceptions of science.

Contemporary philosophy of science, according to Suppe, differs from the both the world view analyses
and the Received View in three important ways:
1) attention is given to the role of rationality in
suggesting theories and fruitful areas of study; 2) an
acknowledgment that the goal of science is to gain
knowledge with ontological significance, thus
conflicting with the operational and
sociological/consensual views of scientific knowledge
seen as consequents of the Received View and the world
view analyses respectively; 3) belief in an
epistemological realism, where the nature of the world
plays an actual role in the development of science.

In addition to this added focus on rationality,
current philosophy of science admits as relevant to
understanding science the very

factors that were banned from the logical
positivist account: the perceptual, cognitive, and
social psychology of the scientist; the sociology
of scientific groups; the ideological values of
scientists and their culture at large; the
scientist's metaphysical beliefs; the process of
discovery; and the colorful, sometimes alarmingly
quirky, history of scientific activity.  

Admitting the relevancy of these factors has
changed the focus and method of philosophers working in
this area. They are generally concerned more with
empirical/historical investigations than with formal
ones, the problem-solving aspect of science has
replaced the focus on the search for knowledge with
absolute certainty, and science is being recognized as
an organic activity, not a "static commodity to be
dissected by post hoc analyses."  In sum, pragmatic
factors have overcome formal, logical ones in determining the approach in this field.

Smith, in discussing the decline of logical positivism as representing the accepted view of science, refers to the ascendancy of a constellation of ideas that represent post-positivist thought. First among these ideas is that "the new image admits as relevant to the task of giving a full account of science a host of factors that were banned from the logical positivist account." As philosophers of science discover that the activity of scientists actually reflects psychological considerations of a perceptual, aesthetic and intuitive nature—in addition to logical operations—a door is opened for Music Therapy research weighed more towards these factors, yet still maintaining the rigor to be called a science.

Also noted by Smith is the primary view shared by the post-positivist philosophies that "science is fundamentally a human activity rather than a linguistic product of such activity." Philosophers must then account for the actual actions and judgments of working scientists, and not limit their accounts to post-hoc, rational reconstructions of such activities. Such efforts have minimized the deductive character of scientific activity and emphasize the variable pragmatic standards employed by scientists in solving problems. Traditional standards of valid scientific
inquiry are not abandoned here, but are subsumed and developed in response to, pragmatic concerns.

In advocating the subsuming of method and research strategy to content and clinical need, the present author is arguing for just such a pragmatic approach. The new view of science recognizes that progress results from mechanisms other than purely objective methods and deductive forms of reasoning. Rather than lament this fact it may be possible for the Music Therapy researcher to embrace it in exploring new terrain.

An objection to this pragmatic approach holds that if "the ultimate decision on scientific questions rests with the scientific community, rather than with an impersonal testing procedure," then it might be said that subjective factors are introduced which render questionable the claims of scientific knowledge to rest on a sound basis. However, this criticism itself rests on the spurious assumption that "judgments made on the basis of presuppositions are dubious," which Brown answers by saying that "the objection is pointless once it is acknowledged that all judgments require presuppositions." To argue against the Received View is not to automatically argue that science is not objective; it is to take the responsibility of providing an alternative notion of how this objectivity can be ensured. A unifying element of the current
efforts in this direction is the provision of mechanisms for the establishment of knowledge through both individual judgments and community consensus that have a rational basis, but are not bound by formal rules. Even Shapere—a noted critic of the relativism that some feel is suggested by Kuhn's work—can acknowledge that "observations in science are 'theory laden' ... though ... the objectivity of science is not thereby endangered." \(^\text{13}\)

**The Foundations of Scientific Knowledge**

Current philosophy of science recognizes that methods of science and theories of knowledge both evolve. Laudan summarizes this viewpoint:

Virtually all the scholarly literature on the history of methodology shows unambiguously that such components of rational appraisal as criteria of explanation, views about scientific testing, beliefs about the methods of inductive inference and the like have undergone enormous transformations. \(^\text{14}\)

The relationship between theories of knowledge and philosophy of science is such that "we change our interpretation of science when we alter our epistemological presuppositions, and that we change our presuppositions when the problems about science that they generate are judged intractable." \(^\text{15}\) Philosophy of science has a dialectical character analogous to scientific activity itself. This realization has three important implications for the current study.
First, it should be obvious that since conceptions of science evolve, basing an actual empirical science on any philosophical conception will be a risky business. It is to link the fate of that particular scientific discipline to the fate of the philosophical analysis serving as its foundation. Consequently, the value of the research program so constructed will not be dependent upon considerations of fruitfulness in solving problems, but according to the whims and evolving epistemological beliefs of scientists and philosophers. It is to introduce non-scientific considerations to the process of the growth of scientific knowledge. And because this imported foundation cannot ensure the inviolability of the knowledge thereby gained, the only rationale for its use is invalid.

Second, the current swing of the dialectical pendulum in the philosophy of science places much importance on areas of study like perception, cognition, pattern recognition, patterns of discovery and methods of problem-solving. In short, philosophy of science has become psychologized and is looking towards results in this discipline as a foundation for its own knowledge claims. For Music Therapists to build research programs on philosophies so constructed is to allow non-indigenous processes and constructs in "through the back door." It would subvert the very
purpose of an indigenous paradigm by limiting our research methodology to those judged appropriate for other domains in psychology. In short, it would repeat the very mistake that has previously served to limit the applicability of Music Therapy research.

And last, none of the current positions in philosophy of science assume, a priori, a common scientific method but instead look to scientific practice to see if there is one. The practice of Music Therapy involves a form of gaining knowledge, through musical interaction, that could quite possibly have unique characteristics. Music Therapy researchers should do what they need to in order to research their discipline and let the philosophers characterize the method subsequently evolved, on its own terms. To look to philosophy to legislate a method is akin to holding a mirror to a mirror, since it is the indigenous needs of the discipline that take precedence over its philosophical explication. Music Therapy research is a skill just as Music Therapy practice is. If the present analysis has shown anything it is that neither one can be guided by extrinsic rules or guidelines.

Implications for Music Therapy

Manicas & Secord identify those aspects of the new view of science which are particularly relevant for psychology. These include: advocating a realist view
of theories; de-emphasizing the need to explain behavior based on laws; and, an anti-reductionist recognition of research in psychology as representing an autonomous mode of inquiry. They go on to say that "adopting this new perspective would resolve many of the tensions and conflicts which have plagued modern psychology from its beginning."\textsuperscript{17}

Ironically, though I am in strong agreement with their meta-theoretical beliefs, I believe that the efforts of these authors is misguided. In proposing a metaphysical justification for concerns that should be guided by the empirical and pragmatic every day needs of the researcher, Manicas & Secord are repeating the mistake of behaviorism; they are just choosing an alternate epistemology to enact. If one's methodology is supported by appeals to a metaphysical position, contrasting methodologists can always point to an alternative foundation. There is then truly no ground for a rational choice between the two.

In assessing the relevance of philosophical analysis for psychology, Koch says that "the need for testing, culling, transforming, supplementing, adapting philosophical insights within a context utterly controlled by responsiveness to the indigenous is absolute."\textsuperscript{18} But to then say that philosophical analysis is irrelevant to science is as mistaken as to say it should provide the foundation for science. The
key is to find the proper and most fruitful relationship between the two.

The discipline of Music Therapy will have to build a research tradition without either a clear idea of how to do this, or a pre-determined notion of what the successful tradition will ultimately look like. Given these circumstances, philosophy of science can still play an important role.

The first of these is to clearly demonstrate—as is being attempted in this study—that no a priori conceptual foundation is available, recommended or necessary for this effort. Initially, internal conceptual consistency and correspondence to the truth of clinical experience is the only criteria we can impose on a research program. Philosophical analyses can penetrate beneath the surface of clinical experience and help to identify those research methods whose foundations make them more likely to be fruitful, given the nature of this experience.

To the extent that philosophy of science is philosophy, it can never model an actual procedure of generic scientific activity. Each philosophy will have metaphysical roots, unverifiable through experience. This is why I am arguing for certain scientific procedures of Music Therapy research, not on their own merits, but in terms of how well they meet the exigencies of Music Therapy research. Philosophical
methods can help us sort out the various approaches and identify those that contingently (but not metaphysically or absolutely) hold promise for Music Therapy. Identifying the conceptual foundations of fruitful methods here can help other researchers to develop methods more likely to be successful—methods with a similar foundation.

In sum, I am advocating using philosophy in a diagnostic sense, to help us discover the reason why traditional research efforts have limited applicability to clinical practice. Philosophical analysis can also reveal the common elements of fruitful research efforts. What it cannot do is suggest a specific cure, in a vacuum. The present study represents an attempt to get us off the ground, so to speak. Yet it is done with the recognition that the only healing must be self-healing, indigenously drawn.
Notes


5. Smith 319.


7. Smith Behaviorism.

8. Smith 319.


11. Ibid., 154.

12. Ibid., 154.


17. Ibid., 400.

CHAPTER VII

TRADITIONAL RESEARCH FOUNDATIONS AND INDIGENOUS CLINICAL CONCERNS: A COMPARISON

We now turn to the task of critically analyzing the appropriateness of the traditional research paradigm for investigating creative Music Therapy practice. Here, the philosophical and methodological positions that emerged in Chapter III will be analyzed to determine their adequacy in providing the metatheoretical guidance and methods of inquiry that the conclusions of Chapter V suggest will be required for an indigenous research paradigm. Given the working portrait of creative clinical practice, we will look at whether or not the current paradigm is an appropriate one for illuminating the underlying mechanisms suggested by such a portrait. If it is not appropriate, the specific conceptual elements that prevent it from being so will be articulated.

Providing this conceptual analysis is a crucial element of this study. If, for example, one is studying the manner in which the therapeutic relationship manifests itself in the musical interaction, the available tools consist of psychodynamic personality formulations and behavioral
analysis. The phenomena discovered in Music Therapy process will necessarily reflect the specific theoretical content and conceptual foundations of these systems.

Of course, it is neither novel nor particularly undesirable to say that the nature of theory influences how data are perceived. Presumably this effect will be a positive one, bringing hitherto unknown factors to light and revealing the existence of previously unknown relationships among the phenomena under study. Yet it is possible that the method utilized will limit or alter what is discovered in some essential way so that its very nature is distorted.

The only way to determine if such distortions are in fact built in to the application of a specific paradigm to a given set of phenomena is to compare the conceptual bases and meta-theoretical recommendations of the paradigm, e.g., behaviorism, psychoanalysis, with those derived from more indigenously-based theories. It might be, for example, that a behaviorist methodology is not antithetical to an indigenous Music Therapy methodology. For purely fortuitous reasons, behavioral methodology might be successfully employed in Music Therapy research because its conceptual bases just happen to be consonant with those required by the phenomena. Therefore, demonstrating that a paradigm has a non-indigenous origin is not itself sufficient to
abandon that paradigm. One must further demonstrate that the characteristics of such a paradigm are antithetical to those of the phenomena one wishes to study.

Only by a conceptual comparison can this evaluation be made. The areas in which the comparison will take place include: the nature of scientific observation; the role of scientific laws; the nature and meaning of explanation; the nature and function of theories; and finally, epistemological considerations. Though these issues will be primarily addressed in the present chapter, some aspects of this analysis are further developed in Chapter VIII.

The important aspects of the traditional approach in Music Therapy research that will be evaluated here are as follows: a working belief in the unity of science; rigid adherence to the public observability of data; a focus on generating laws to account for musical behavior; a reductionist approach towards explanation; and a focus on controlling and predicting behaviors characteristic of belief in the deductive relationship between prediction and explanation.

According to the Received View, laws, prediction, explanation and the public nature of data fit together in a consistent conceptual portrait of science. Possessing scientific laws allowed for predictions regarding natural phenomena, and prediction and
explanation were felt to be logically related such that only those explanations that allowed for singular predictions were considered legitimate. The public nature of data ensured that the predictions of any particular theory, and of competing theories, could be tested in a way that would ensure mutual agreement among researchers as to their adequacy.

In this section I will argue against all of these notions as being appropriate for Music Therapy research, given the nature of clinical process. The fact that there are indigenous considerations against adopting each of these individual positions is to be expected since they are intimately connected components of the Received View, an eminently consistent account of science. If maintaining the public availability of data is shown to be impractical for Music Therapy research, this would make it difficult to formulate testable predictions. Then we would want to formulate a view of legitimate explanation that did not require singular predictions for theory validation. Further, if we do not accept the validity of the explanation/prediction symmetry then the need for universal quantitative laws is lessened.

Observation, Perception and Objectivity

In Contemporary Philosophy of Science

Earlier, it was shown that the most basic premise
of traditional research in Music Therapy has been that since music is behavior, and behavior can provide the only data-base which is publicly verifiable, then research efforts must be oriented towards explaining behavior. The meaning of this behavior lies outside the pale of science, and therefore speculations in this regard are not part of Music Therapy research.

Any alternative must show how the practice of Music Therapy renders this view of the domain of Music Therapy research and treatment untenable. The fundamental assumption in the current argument towards this end is that it is the researcher's obligation to explain the actual phenomena arising in clinical practice, not merely those emerging in the experimental constructs created by the researcher. After all, a researcher can always specify a variable which may, in principle, be publicly observable; yet in such specification there is no guarantee that the variable reflects a domain with any interest outside the confines of the experimental contrivance.

The present argument will have two components: I will first articulate the purely philosophical problems associated with maintaining the possibility of a neutral observation language. The purpose of this argument is not to prove the converse is then necessary; it is merely to be establish that there are plausible alternatives. Next, an argument will be
presented from the particulars of Music Therapy process which shows that abandoning the ideal of a neutral observation language is not only plausible, but more reasonable given the nature of this process. Once the rationale for this ideal is shown to be limited, we can move beyond the conception of music as behavior.

I previously argued that the line between what is observed in science and what is inferred is a grey one. Yet just because there is a similar lack of a distinct boundary between life and death does not mean that we cannot assert with certainty that Aristotle is dead and the person sitting next to us is not. Just as the absence of an unambiguous boundary here does not mean that the distinction is not a real one, acknowledgment of the grey area in science does not mean that there is not a real difference between observation and inference.

However, philosophers have not just noticed that the observational/theoretical line is a blurred one, but have also come to the conclusion that observation itself is theory-laden. What this means is that how the scientist perceives data is necessarily determined by the theory and conceptual matrix under which he/she is working. In this way, the activity of science itself is seen as an extension of the everyday acts of perception from which we all construct our phenomenal world out of the elements of sensory stimulation.
In representing this approach, Brown asserts that "in response to the view that perception provides us with pure facts, it is argued that the knowledge, beliefs and theories we already hold play a fundamental role in determining what we perceive."\(^1\) He continues: "If our knowledge and beliefs play a central role in determining what we perceive, then the scientific theories that a scientist holds should play the same sort of role in determining what he observes in the course of his research."\(^2\)

In the study of perception, the phenomenon of the "gestalt shift" has been used to demonstrate how the same sensory stimulus can be variably perceived, as when an image is first seen as a white vase on a black background, and subsequently as two human profiles on a white background, depending on which color is assumed to comprise the figure and which the background. Other examples include the duck/rabbit figure which constantly projects a shifting perception, though the sensory stimulus remains constant. In science one can see the manner in which the same sensory stimulus can lead to different perceptions in considering how Kepler, who believed in a heliocentric astronomy, and Tycho Brahe, who argued for a geocentric one, alternately perceived the sunrise. It is argued that "when Kepler saw the sun, he saw the stationary center of the universe around which the earth revolved."
Brahe, on the other hand, saw a celestial body which moved around the stationary earth."³

Further, it would not be accurate to say that Kepler and Brahe are seeing the same thing but are merely interpreting it differently. Hanson rightly notes that "seeing is an experience. A retinal reaction is only a physical state."⁴ Whether Kepler and Brahe do or do not see the same thing cannot be determined solely through reference to their physiological states.

What we then perceive is influenced by our prior knowledge and belief. In fact, the only way we gain knowledge from our sensory experience is to relate what is perceived to this prior knowledge and belief. For a philosopher like Kuhn, the unit of scientific organization--in his case the disciplinary matrix--serves the scientist as the mechanisms of perceptual organization serve the individual. The scientist's data is not comprised of the sensory given any more than the units of our perception are identical with the sensory stimulation giving rise to them.

Rather than being characterized as the sensory given

data are the results of classifying phenomena, associating phenomena into similar groupings, performing measurements . . . data are expressed in the language of the science, and the meanings of the data expressions are different under different disciplinary matrixes.⁵

It is not that the world is experienced directly
and is subsequently interpreted "from within one's disciplinary matrix; rather one sees the world through one's disciplinary matrix." For our sensory experience to have any relevance for science, i.e. for it to convey information of any kind, it must be inextricably bound with necessarily personal, subjective judgments. Thus, scientific activity is not just a search for facts, but often involves creating conceptual novelties so that our perception of the world shifts, as in the example of the gestalt figures noted above.

Ruling out private mental events from scientific consideration cannot then be justified on the basis that observation is a purely objective activity. Insisting on public verifiability, is only admissible if one believes that which constitutes scientific knowledge is determined solely by consensus and not by conformity with a real state of the world. Einstein's special theory of relativity represented a significant advance of scientific knowledge, though it consisted primarily of a gestalt shift of the kind we have been discussing—a shift which Einstein's genius allowed him access to. This theory represented an advancement of knowledge that was not dependent on the assent of other scientists. Its degree of accuracy or conformity with the real world—hence its truth value—did not change as more scientists came to support this theory. In
fact, history shows that this theory was widely accepted in advance of evidence unequivocally demonstrating its validity. It was the force and beauty of Einstein's conception which recommended itself to physicists.

The emphasis on objectivity of data and the requirement that scientific data be publicly available were argued for because it was felt that only in this way could science rest on secure and completely objective foundations. Yet we have seen how pursuing these goals implies a conventional or consensual view of knowledge antithetical to the very purpose for which they are being invoked! In addition, the only level on which data can be said to be neutral is on the level of sensory stimulation. Most scientists and philosophers would prefer to argue, though, that science gives us knowledge about the external world and is not merely a formal description of sensory patterns. Thus, to advocate for a pure objectivity is to be trapped in a type of idealism that cannot account for the rational growth of scientific knowledge. Harré argues for a similar position:

The only facts which seem to be genuinely independent of any scientific theory are those of the present experiences of touch, taste, smell, hearing and sight. . . . If facts are truly independent of theory they are private and do not form part of the public domain of knowledge; if they are public facts they are affected by all sorts of influences.

He then concludes that "for science, there are no brute
facts . . . there is no knowledge altogether independent of theory."^9

Applications to Music Therapy

In assessing the nature of Music Therapy process a variety of factors emerged that bear on the question of the desirability of an objectively neutral, publicly-verifiable data base. These factors include: the necessity of taking in to account the meaning of clinical music in understanding this process; the fact that this meaning is not to be found solely in the publicly observable characteristics of the music; that the meaning must be ascertained against the background of the clinical relationship from which it emerges and, the observation that creating clinical music involves necessarily personal, subjective factors and, consequently, that understanding clinical process will involve researching these factors. All of these assertions require that research into Music Therapy process, at some point, involve investigations of both the client's and therapist's existential experience in the clinical milieu.

Thus the locus of effect of Music Therapy process—the domain where the mechanisms of the process are revealed—is in the experiential realm. Traditional behavioral methodology holds that this realm is private and unobservable by all might care to judge the claims
made about its nature, and it cannot function in truly scientific accounts. Therefore, this plane of analysis is abandoned as a source of data.

Yet this is not the only possible step. Assuming, for the moment, that qualities of experience are private and not publicly observable, one can alternately abandon the requirement that the data be unqualifyingly observable and thereby gain access to the real data. After all, placing this qualification on legitimate data results from personal judgment, not metaphysical necessity. Pragmatic concerns could dictate its abandonment when they have become more of a burden in getting closer to the phenomena of interest than their use justifies. Just as a mountaineer abandons potentially useful, but overly weighty, equipment when scaling a peak, and a mathematician abandons "common-sense" notions (such as parallel lines never meet) when exploring non-Euclidean geometries, the Music Therapy researcher must abandon those conventions which, while helpful in one domain, become cumbersome or unworkable in another.

Of course none of this is to say that observables are irrelevant to Music Therapy process. They are akin to the sensory stimulation from which we build up our experience of the world. Without them there would be no experience, notwithstanding the claims of advocates for the reality of psychic phenomena (though,
presumably if psychic phenomena are real they are observable to other psychics).

What is being argued for here is that the operationalism employed by researchers insisting on the primacy of observables is taken to an extreme. They endow the indices (observed actions) with the status of the phenomenon of interest or, in some cases, equate the two. A pressure gauge is an index which can measure steam pressure in an engine. Just as one can move the needle on the gauge with a magnet, and yet not affect the actual pressure, an experimenter can alter behavior without affecting the client's inner organization or capacity to respond to the world of which the behavior is a manifestation.

The behaviorist might say that he nowhere sees the "weak ego structure" (for example) that the supposed index (anti-social behavior) is supposed to be an epi-phenomenon of, and therefore no explanatory significance is gained by invoking its presence. Yet this is tantamount to saying—to return to our steam engineer—that it is not an actual problem of some hypothetical entity called "high pressure" that is dangerous (because he nowhere sees this pressure), but it is the movement of the meter needle into the red zone that is the real danger because these movements have been previously correlated with explosions. In the case of the engineer, we recognize the absurdity of
focusing the intervention on the index, i.e., the
gauge, rather on the unobserved phenomena, i.e.,
releasing the pressure through a valve. Yet by
conceptualizing therapy as altering behaviors the Music
Therapist is advocating a similar position. What is
being asserted is not that behaviors cannot be
controlled by external contingencies (magnets or
reinforcers), just that in so doing one is not
necessarily affecting the conditions that make the
index of interest to begin with.

In fact, acknowledgment of an underlying
personality that is transformed through therapy is the
only possible rationale for Music Therapy treatment.
Presumably, musical behaviors do not have a high
adaptive value outside of the musical interactions in
which they take place. Focusing on growth in this area
is only reasonable if one assumes a unifying,
psychological realm that the musical interaction is
allowing access to. Otherwise, it is not clear why a
behaviorist would be interested in shaping musical
behaviors that have no relevance outside of the Music
Therapy session. To say that these behaviors
"generalize" is to beg the question of the mechanism of
generalization. It is providing just such a mechanism
that is the function of the underlying personality
variables.
I have so far been advocating that one of the most crucial research questions for Music Therapy is how the process of improvising music facilitates human contact and growth. To answer this question it was proposed to explore the means by which qualified clinicians create music in the clinical milieu. Perhaps it would be fruitful to then address how these issues regarding observation and objectivity bear on this paramount research question.

One way of addressing this question would be to utilize interview or questionnaire techniques in which improvisers could comment on actual examples of their clinical work. Once enough of these accounts were gathered by the researcher it would be reasonable that the patterns underlying clinical technique would emerge which would lead, in turn, to more generalized statements regarding predominant modes of improvisation. These generalized statements could take the forms of theories whose validity and usefulness could be tested through the manner in which they account for observed elements of clinical process.

An objection to this strategy might be that the nature of the generative processes reported by improvisers is not deductively known by the researcher. That is, there is a crucial assumption that clinicians can be reliable reporters of their own inner processes. Since it is possible that these processes could be
either deliberately or unintentionally misrepresented through the act of reporting, the researcher must infer that the reports he gathers are reliable.

Now this observation of inference is not itself damaging. We have seen how inference plays a role in all scientific knowledge. The behaviorist infers that the behavior he is intending to shape is the one actually shaped, and focusing on formal descriptions of music in Music Therapy process involves the assumption that their manipulation can account for clinical results. The real question is whether or not this inference is justified. Thus, rather than objecting to this act of inference, per se, we can reformulate the objection in the following way: Given the nature of our research question, is inference at the level of data-gathering a reasonable strategy to adopt?

It would not be difficult to formulate criteria to answer this question. We could ask how likely it is that the data gathered will be distorted, what direction might this distortion take, and what procedures could be used to determine if we were gathering valid data or not, or at least minimize the risk of gathering invalid data. Certainly, experimental research in psychology constantly deals with such questions and creates structures to maximize the reliability and validity of their investigatory procedures. It is reasonable to assume that the
relative advantages and disadvantages of utilizing this sort of private data in Music Therapy could be ascertained, and the question of its use could be determined pragmatically for each individual research situation or question, not globally applying to all such milieus.

To take this line of thought one step further, I would like to suggest one possible criterion for determining the validity of the private reports gathered in the hypothetical study discussed above. This is for the researcher to compare the reports to his or her own experience as a clinician. This is not such a radical or controversial move if one acknowledges that all acts of knowing entail a similar comparison. Michael Polanyi\textsuperscript{10} creates the basis for an epistemology where all valuation of expressed verbal truth must ultimately judged by the non-verbal, inarticulate component of knowledge; hence, all verbal truth rests on some non-verbal affirmation.

The standard objection to employing this valuative criterion in scientific procedure would be that the only way for scientists to choose rationally between theories is for the data to be public in the sense of equally available to all. If one must have the experience of being a clinical improviser to judge the validity of statements regarding its nature, then scientific knowledge becomes equated with religious
faith, in the sense that religious arguments only convince the "faithful" or those with a prior religious experience. Scientific claims, on the other hand, are supposed to be convincing, regardless of one's prior experience. In short, their validity should not vary nor depend on any one individual's state of mind.

There is a spurious element to this argument, however, which relates to the notion of data as being available equally to all observers. As was argued previously, data is not equated with pure sensory stimulation but is gleaned from the meaning that these sensory elements give rise to. I can read a series of numbers on a computer print-out which, to a trained physicist, would function to confirm a particular hypothesis about the structure of matter, and yet not experience the physicist's sense of confirmation. The numbers are available but their meaning--that which defines them as data--is not. I have no idea how they fit into any particular model, nor how they are functioning to discredit one or another theory. In this case, though I have access to the same sensory stimulus as the physicist, i.e. the lines on the page comprising the numbers presented, I have no access to the data because of my lack of training in physics.

One might argue that in the Music Therapy example it is a certain subjective experience that is necessary, while in the physics example it is a system
of propositions or laws of nature that is relevant, thus invalidating my comparison. Yet this assumes that the result of the physicist's training is fundamentally different from that of the Music Therapist. Contemporary philosophy of science, however, recognizes the presence of a tacit component to scientific knowledge, transmitted via laws, theories and experiments, but that goes beyond that which is articulated in these exemplars. This knowledge is wholly analogous to what I believe the Music Therapist internalizes through training and clinical experience. Thus, the physicist's judgments regarding the significance and validity of data rests on an inarticulable, tacit component of knowledge. To suggest that Music Therapists not be allowed to use their clinical experience in this valuative function regarding data, is to accept the burden of proving that the two forms of knowing detailed above are fundamentally different.

Scientific knowledge is not a fully formalized, experience free, axiomatic system. It takes the visual experience of color to suggest a theory for its production as manifesting various electromagnetic wavelengths. It is not clear what a blind person's optical theory might be, or even if it makes sense to speak of one. Indeed, it is the experience of vision that suggests theories of optics in the first place.
Just as only those who can see can conceive of a theory for colors, only those with experience in clinical improvisation can formulate and evaluate theories to account for its clinical efficacy.

The Nature of Scientific Laws

Plato described a world of pure and absolute forms of which our phenomenal world was only a shadow. Beginning with Galileo—an avowed Platonist—a certain approach towards scientific laws evolved from this perspective. Galileo formed his laws of motion to apply to conditions of a perfectly round sphere rolling on a frictionless plane. The quantitative scientific law was oriented towards an abstract type of existence that the real world only approximated. The real world of imperfect spheres, with plane surfaces providing varying degrees of friction and resistance, was too "messy" to provide for exact calculations. Following in this tradition, scientific laws have not been traditionally conceived as referring to actual entities, but to 1) immutable individuals, with 2) idealized properties, in 3) structurally homogenous classes, envisioned in 4) perfectly closed environments, that are 5) not necessarily unique events.

In the physical sciences, laws usually provide for good enough predictions of actual events because the
unaccounted ways in which real entities and situations deviate from the ideal either cancel each other out or produce effects that are not large enough to matter. Here, the paths of the planetary orbits can be calculated precisely enough only taking into account the gravitational attraction of the sun and nearby planets, though in actuality every stellar body in the universe exerts a force on every other and to calculate the orbits precisely would mean taking into account this multitude of forces. To calculate them sufficiently accurate for space travel, it is permissible to ignore all these effects. The application of Newton's laws actually required that the mass of bodies be concentrated at a single point, a condition Newton knew not to exist, but representing a difference which he felt would only negligibly affect his calculations. All scientific laws assume this sort of closed system when the actual world is really represented better by the model of an open system.

Earlier it was observed that there are problems with applying the concept of scientific laws adopted from natural science to biological and psychological science. This is because "the currently accepted paradigm of a scientific law is a process law"11 and process laws assume the five conditions mentioned previously, conditions which may not be applicable to biology and psychology. What I would like to do for
the present is to assume that there is no convincing argument that would preclude there being laws of psychology, in principle. Since it can be argued that Music Therapy exists as a sub-discipline of psychology any argument against the possibility of psychological law would probably hold for Music Therapy. Instead, in keeping with the spirit of this study, I would like to examine the nature of data in Music Therapy and see how they fit the criteria demanded by laws, irrespective of what is true for psychology as a whole.

Scientific laws are empirical generalizations with an added quality of necessity. Though they do not possess logical necessity (no canon of logic would be violated if the energy of a body did not equal the product of its mass and the square of the speed of light) they do possess a sort of universality that is not restricted in a spatio-temporal sense. The specific values and constants referred to in laws are not necessary, but the predictions they make are. Laws exist in a kind of in-between, nether world, leading to the determinations they imply being characterized as "contingent" ones. It is this quality, though, that allows laws to explain observed empirical regularities. Note that it is because laws refer to idealized situations that are acknowledged to be conventions that they are endowed with a quality of universality.
In Chapter III, Gaston's thoughts on scientific laws were explored. He felt that Music Therapy research should be oriented towards the discovery of such laws, and that they would need to be in agreement with the laws of biology. One consequent of this type of research focus is that the meaningful and expressional aspects of Music Therapy would necessarily be excluded as these elements manifest the unique aspects of Music Therapy, and scientific laws deal primarily with that which is generalized.

Of course this is not the only alternative. One can begin with the necessity of studying the expressional aspects of Music Therapy and then see how the concepts of scientific laws apply. Gaston chose the commitment to the establishment of laws as his fundamental assumption and let those elements of Music Therapy process which did not fit this methodology fall by the wayside. By evaluating the five characteristics of laws noted above in terms of the indigenous portrait of clinical process that emerged in Chapter V, I am taking the opposite approach.

I will first consider the concept of immutable individuals. In a mechanistic world view there are two types of allowable change in the world: these are change of position and change of structure, itself reducible in some sense, to change of position. The motion of the earth around the sun is an example of the
first, and a chemical reaction embodies the second. Yet, even this chemical reaction, involving a rearrangement of molecules, could also be described in terms of changes in the positions of the constituent atoms. The important point is that in the mechanistic view, the world consists of immutable elemental atoms whose various arrangements explain the apparent diversity of the world.

When forces do act on entities it is essential that they not fundamentally alter the existential integrity of the object so acted upon. Gravitational forces are constantly impinging on the earth, but the earth maintains its identity throughout its orbit. Only its position changes. If entities are changed by the action of forces—as when one drops a glass on a brick floor—then the resultant change is into the structural components of the original entity.

These two considerations must be the case for laws to lead to predictions. If gravity somehow changed the essence or identity of the earth how could we make predictions about the "earth" for it would not maintain its identity through changes of location and into the future. Only forces whose action affects the body in ways such that the only changes are ones of structure or position can function in making predictions.

In Music Therapy, however, it was proposed that the nature of change is that of transformation—a
change that is not explicable in terms of position (behavior) or structure (new psychodynamic forms). What is changed is a person's capacity to respond to situations and other people. This capacity cannot be operationally defined because it exists only as potential—it represents an increased freedom of action which may or may not be drawn upon. It therefore defies attempts to have its nature and manifestation predicted. Unlike the mythical atom in the void struck by another atom, the individual affected by the forces in Music Therapy process is changed in a fundamental manner. In fact, the efficacy of the process is measured by the extent of this change.

What does remain constant in this process is both the physical identity of the individual—though Music Therapy certainly has important effects on this level—and the sense of personal identity or selfhood. Yet the individual's self-conception is fundamentally altered. I know that I am the same person on a day-to-day basis, but what it means to be me has changed through the forces accessed in Music Therapy process. It is not clear how a science that is built upon the immutability of the individual (in order to create order, build laws and make predictions) can come to terms with the fact that in Music Therapy the individual undergoes a fundamental transformation.
The second quality of the entities referred to in laws was that they possess idealized properties. A sample of liquid can be considered to be one hundred percent pure in predicting its reaction to heating, though in actuality there will always be traces of other elements present. And the earth is considered to be a perfect sphere moving through completely empty vacuum for purposes of calculating its orbit, though neither of these assumptions is strictly true. Again, the departures from the ideal are not considered to be significant enough to fundamentally alter the predictions implied by the laws. The criteria for determining whether or not we can make the assumption that the real sample is enough like the idealized one to warrant the use of the law is that the resultant difference is not great enough to cause an observable variation on the effects of outside forces being applied to the real sample.

Now the important question is whether or not we can make this assumption for people in general, especially as regards the properties affected in the Music Therapy process. It is true that no two individuals are exactly alike in a physiological sense. Even identical twins will have minute differences that render them un-identical in certain ways. Yet given this fact, it does seem possible that most people's physiology is enough alike that very broad, almost
universal, generalizations can be made about the effect of introducing various foods or substances into the body. In general, the ingestion of arsenic will cause a toxic reaction, while ingestion of a glass of a fine French Bordeaux wine will produce a favorable one.

This is true, unless the person doing the ingesting became sick after drinking a similar wine the previous week. In this case, the wine might produce a nauseous reaction similar to that produced by the toxic substance. Hence, we need to have some historical information to really predict the effect of various substances on the body. This is generally not the case in the physical sciences. Two atoms in identical quantum states are identical for all intents and purposes. The history of each should in no way effect how they will react when acted upon by other forces or particles.

It was earlier discussed how each of our musical histories is unique, and hence our reactions to music and perception of musical meaning are unique. This is why there is no absolute meaning, in either a designative or embodied sense, to the elements of music. In improvisation we create and re-create our musical language and its referents as we go. There does not appear to be an ideal relationship with music that each member our species only deviates from by a trivial amount. In fact, there are good arguments for
the position that the generalizations that can be made in this regard are culturally relative.

Our musical selves are then structured in a unique way, determined by the interaction between the nature of our experience while listening to and playing music, and all the other experiences we have lived through. There seems to be no basis for assuming a common, universal and ideal response to music that all individual responses only deviate from in minute and insignificant ways. Since the discovery of bona fide laws connecting musical phenomena and human reactions is dependent on there being this universal structure, it does not seem that the search for these types of laws will be fruitful.

This is not to deny that there are common and similar reactions to pieces of music, nor that different individuals will express similar feelings with similar musical manifestations. All music contains forms—or more accurately is expressed through certain forms—that reflect archetypal human modes of experience and expression. It is the presence of underlying, species general forms that allow music to function as an expressive bridge among individuals, and hence endow it with communicative significance. However the expressive content conveyed through these forms is not pre-determined but is evolved in each musical relationship, whether this is between a client
and a therapist, or a performer and a listener. The manner in which musical meaning arises is necessarily idiosyncratic and involves interactions with personal structures whose unique qualities are determined by historical factors. It does not appear that the assumption of a common, universal musical structure within individuals is warranted—thus mitigating the focus on establishing laws of musical interaction.

The next assumption regarding laws is that the entities they refer to consist of structurally homogenous classes—homogenous in relation to the forces whose effects define the class as such. This means, for example, that chemical laws can refer to table salt because every sample of table salt has an identical composition. Now each salt particle may be of a slightly different shape, but this is an incidental, irrelevant difference in terms of the criteria by which salt is defined. So this homogeneity must be defined in terms of certain parameters—here chemical composition—and is not absolute or given. Still, there is a sense, although not a clear one, in which the definition of salt as containing a specific portion of sodium and chlorine is not arbitrary. (Again, for the time being, we will assume that there is a meaningful way that organisms and species can be conceived of as classes—and not individuals—in order to even explore whether scientific laws can be
applied.)

In Music Therapy, the chosen parameter often consists of the clinical category of the client. Music Therapy treatment is normally variably conceived according to the clinical population being discussed: rationales for working with individuals with schizophrenia focus on the ability of Music Therapy to access the pre-verbal conflicts some think cause schizophrenia; rationales for work with autistic children can focus on the manner in which music fosters cognitive and sensory integration; work with the terminally ill draws upon the transpersonal quality of music; and rationales for working with the retarded emphasize the ability of music to effect more ordered functioning.

Similarly, Music Therapy treatment is also looked at in this unitary sense, especially as regards the appropriateness of treatment methods for various populations. Some would recommend in-depth analytic Music Therapy for neurotic clients, Music Therapy from a psycho-educational perspective for mentally retarded children, recreational Music Therapy for chronically hospitalized psychiatric patients, and rehabilitative Music Therapy for the physically disabled.

Yet, it is not obvious that these chosen parameters--disabling condition and method of treatment--are delineating essential and homogenous
properties. One must do more than just demonstrate that the disabling conditions are real and reflect naturally occurring categories. Whether or not categories like schizophrenia or autism actually represent unitary syndromes, or are functioning more like cluster concepts for a variety of conditions with different etiologies and prognoses, is still open to question. For the present purposes we can assume that they are unitary syndromes—acknowledging that this notion is problematic. What must be shown is that these categories represent characteristics relevant to the effects of Music Therapy process on the individual.

In Chapter V it was merely asserted that this was not the case. It was discussed how the Music Therapist treats not the condition itself, but the limitations placed on the individual by the disabling condition. Such considerations are supported by research outcomes that demonstrate that the remedial mechanisms of Music Therapy process are not correlated with the client's clinical category, whether such category is a traditionally pathological one, or is non-pathological.\(^{12}\)

The relevance for the current discussion is that disabling condition cannot provide the structurally homogenous class demanded by statements of law. It is not being argued that for an individual with schizophrenia, the illness will not affect the course
of treatment; just that the nature of this effect will not be generalizable to all clients with schizophrenia. Having schizophrenia, then, does not imply the presence of any single property that would lead us to assume a common response to, or use of, Music Therapy process.

Similarly, it is not clear that either level of intervention (supportive, re-educative or reconstructive Music Therapy)\textsuperscript{13} or theoretical perspective (analytic Music Therapy, behavioral Music Therapy, etc.) can provide the homogenous category from which law-like statements might be generated. In studies comparing outcomes for various schools of psychotherapy, it has been found that is not theoretical orientation, but personality traits of the therapist that are correlated with successful outcomes. Apparently theories of psychotherapy might vary, but either they have minimal effect on determining interventions, or more likely, the therapist's actions (behaviors) are not as important as the personal qualities with which the interventions are enacted.\textsuperscript{14} In either case, general statements regarding the efficacy of theoretical orientations are precluded.

My feeling is that we will find a similar situation in Music Therapy. Again, this needs to be empirically studied, but if current theory is most valuable as post-hoc explanation--as has been argued herein--then it would not be surprising if theoretical
orientation was not correlated with clinical outcome. This is because while theory is used to generate the clinical intervention, it is the quality of this intervention (degree of acceptance, warmth, caring, etc. shown by the therapist) that determines clinical outcome and is independent of theory. Focusing on these categories can direct attention away from the variables whose study holds more promise, such as the personal qualities of the therapist and the attitudes and motivational level of the client.

The next aspect of laws to be discussed here involves the assumption that the systems studied by science are closed. A system can be considered closed when all observable and measurable transactions are covered by principles of conservation; when an "output" from any part of the system is an "input" to the system as a whole; when all events have their effects only within the system as a whole and when events beyond the system have no effect on it; when, in a word, the "content" of the system is constant no matter how varied the form of the content may be.

Again, the study of our solar system provides a good example here. Planetary orbits are calculated by considering the gravitational effect of the planets and sun on each other. All of the other billions of stars that exist do exert gravitational force, but their effect is so negligible that we can assume that our solar system is "closed" to their influence.

This assumption of closure is necessary for the type of process laws currently under discussion. That
is, all the values of the relevant variables must be known in order to have predictive accuracy. If planets spontaneously and randomly emerged into existence, or if there was a parallel universe that was in principle undetectable, whose planets influenced the orbits of those in our solar system, then it would be impossible to predict the future course of planetary orbits. This is an example of a condition that would render the solar system an open one and would invalidate the application of the process law.

Biological entities (organisms, people, ecosystems, etc.) tend to be open systems because of their teleological character. This trait is derived from the fact that to account for the stability of these entities we must invoke concepts like goals, purposes and functions. Rather than appeal to the fact that human actions require teleological explanations—and are thus open systems resistant to explanation by laws—I would like to look specifically at our evolving conception of Music Therapy process and examine what elements bear on this question. This approach is being taken because it is possible that in our totality as organisms we are open systems, but, for the purposes of explanation, specific aspects of our functioning could be considered to be effectively closed. Therefore, the specifics of Music Therapy process, and not an a priori declaration, will be the ultimate arbiter of the
approach we take as researchers.

Hull notes that "teleological systems persist by changing."17 As examples, consider how 1) the continuity of artistic traditions depends upon constant development in style, 2) those species survive that can adapt to changing environments, and 3) humans develop and thrive by maintaining a fluid self-identity that is open to the change required by the varying demands of the successive stages of life. Certainly there is a sense in which the very purpose of Music Therapy is to effect change. It is the question of the nature of this change which bears on the present discussion. If the change to new states of being is incremental and to be accounted for completely by reference to previous states of the individual, then it is possible that the closed-system model could be adopted. If, however, the type of change we see is discontinuous with previous organismic states, then we would conclude that the nature of Music Therapy process is such that its results are inherently unpredictable and better characterized through open system descriptions. (It is worth noting that the debate around the character of scientific change has centered on a similar issue of discontinuity versus continuity.)

The crucial consideration here is that the essential characteristics of Music Therapy process are what was described previously as emergent properties,
not reducible to, nor completely contained in, their constituents. A variety of factors that were discussed in Chapter VI are relevant to this issue. Their consideration demonstrates that open-system mechanisms are better able to account for the phenomena observed in this process.

Primary among these characteristics is that the healing mechanisms emerge through a therapeutic relationship, the dynamics of which are obviously not present in the individuals comprising this relationship. The healing dynamics of a therapeutic relationship, by definition, cannot be reduced to the properties of individuals. The fact that rules can only be used in a post-hoc explanatory sense, and not as generative principles, for clinical process supports the notion that the essential aspects of this process are inherently unpredictable. And the manner in which the referents of the musical language are established in Music Therapy process--and are not predetermined by the musical elements--renders an open system description more accurate. In sum, the very purpose of introducing creative activity into a therapeutic process is to foster the emergence of novel and unpredictable methods of interaction and solutions to conflicts.

A truly closed system cannot allow for the presence of emergent properties. This is because while
forms are changed in this type of system, the essential content must remain stable. Emergent properties involve not merely a re-arranging of elements, but the appearance of novel properties, such as life consciousness and rationality, that are not limited by the constraints of their constituents. To allow for the existence of emergent properties in Music Therapy is to recognize the necessity of open-system descriptions and to provide another reason for considering process laws irrelevant in this domain.

The last characteristic of laws to be considered here is that their focus and domain must be universal and not refer to necessarily unique events. As Hull says, "scientific law cannot be limited necessarily to a particular time span or place in the universe." Developing the idea that species are necessarily unique events, Hull attributes this to the fact that they are spatially defined by a contiguous environment, and temporally defined by a continuous genetic connection. Thus, the argument is that no names of species can function in process laws because this would be akin to formulating a law of nature that only applied to the planet Mars, or Americans under the age of ten. Species are individuals in the same way that planets or people are. If this argument is valid there can be no genuine laws of psychology.
This argument is not conclusive, one objection being that "relative to one set of laws, a certain complex may be treated as an individual . . . relative to another set of laws, it may be treated as a class."\(^{19}\) There is no absolute sense in which one complex is said to be an individual (and hence an example of a necessarily unique "event") or a class; this judgment must be made in reference to a specific theory or plane of analysis.

Put in these terms, I do not see how this issue has any specific bearing unique to Music Therapy research. The issue really seems to be a meta-scientific one and involves the degree of necessity we want to attribute to empirical generalizations in sciences like biology and psychology. On the surface there does not seem to be any reason to preclude there being laws relating to living organisms, though it is acknowledged that it is harder to distinguish those qualities resulting from accidental correlations from those manifesting an underlying law when one looks at the biological world compared to the physical.

On the other hand, scientific laws are put forward not merely as descriptions but as guides to action.\(^{20}\) The problem of induction notwithstanding, the knowledge embedded in laws is not meant to merely account for past events but to provide reliable guidelines for future ones. The utility of an approach to generating
laws in Music Therapy is dependent upon the degree to which clinical process is facilitated by
1) applications of deterministic statements (rules), and 2) the ability to predict clinical outcomes. Since it will be subsequently argued that the degree to which clinical processes are circumscribed by factors (1) and (2) their clinical efficacy is inhibited, it is concluded that efforts to generate process laws will only lead further away from a clinically relevant research paradigm. In the following section I will discuss how focusing research efforts on the production of lawful statements is counter-productive in Music Therapy, a field where successful clinical efforts are defined by their ability to facilitate freedom, unpredictability and uniqueness.

Explanation

An essential component of the Received View was belief in the symmetry between prediction and explanation. Recall that an explanation which did not make a singular prediction was considered illegitimate. Now if prediction is not necessary for explanation then the need for process laws is lessened because it is the implications of such laws which allow for prediction. Conversely, if the formulation of process laws is not tenable for a specific domain, then the symmetry between explanation and prediction needs to be
abandoned—if we hope to build any type of explanation. In the previous section, a number of serious objections were raised to the possibility of formulating process laws applicable to Music Therapy. If these are valid it would seem that we would need to abandon a research focus oriented towards generating process laws.

The view of Music Therapy that one holds will influence the course of research pursued, as well as determine the types of explanations considered acceptable. If one believes the goal of Music Therapy is to control behavior, then theories and mechanisms that have predictive power will be emphasized. For only if one can predict the effect of applying certain techniques can one control the resultant behavior.

Certainly theories are often used to make predictions, but this does not necessitate that predictive power should be the identifying characteristic of a legitimate theory. Toulmin, for one, has articulated a view of theories where they are defined by their explanatory power, a property independent of the ability to make accurate predictions. Prediction is seen as "an application of science rather than the kernel of science itself."²¹

In the present study it is being argued that seeking for predictive power of behavior in Music Therapy treatment is counter-therapeutic in that it implies a lower degree of autonomous functioning on the
part of the client. Higher levels of human cognitive
and creative functioning—which are presumably the goal
of therapy—are reflected by their novelty and freedom
from dependence on irrational and externally imposed
constraints. The more choices the individual has, the
less predictable their actions become. To engineer
behavioral controls is thus to actually inhibit a
client's development.

In this section the elements of Music Therapy
process will be discussed in terms of their
relationship to traditional areas of concern in the
topic of explanation, especially those that bear on
this question of predictability. These areas include:
the symmetry thesis just described; the nature of
cause/effect relationships; the difference between
reasons and causes as explanatory statements; the
notion of what constitutes legitimate explanation, and
criteria for deciding where explanation stops.

Reasons and Causes in Explanation

Recall the difference between reasons and causes
as explanations. When a reason is invoked in an
explanatory function the notion of an agent acting with
purpose or intent is implied. A cause, on the other
hand, is a form of explanation in which requires no
such assumption of agency.
A musical event or action can be described and thus explained on both of these levels. To return again to the paradigm research question in Music Therapy, we can examine how these two types of explanation can deal with the question of why a particular clinician's music takes a given form in a therapy session, and why this form facilitates human contact and development. It is difficult to picture how a purely causal account of such a process might look, but it is worth making the attempt, as the way in which this effort is deficient will suggest what the attributes of an adequate explanation should be.

First we should note that no reference to the meaning of music could be made in such an account. Understanding the meaning of a symbol or event is a necessarily interpretive act, one which requires an agent doing the interpreting towards certain ends. Neither the therapist's nor the client's intent could have a role here either. These are the very elements that the causal account is attempting to eliminate or render unnecessary through a reduction to physical terms.

Understanding music means understanding the manner in which the different elements fit together to form greater wholes. Isolated tones become a melody and successive drum beats form a rhythmic pattern. Yet, since meaning is banished from our causal account
we cannot therefore talk about the dynamic qualities of any musical systems. It would seem that we are left only with the purely physical characteristics of tones such as intensity, attack and wavelength.

Hence, a purely causal explanation of a clinical improviser working at the piano would be limited to considering the following types of factors: the force with which the therapist pressed the piano keys; the velocity of the hammer before it hit the strings; the mass and tension of the piano strings and resultant sound waves formed; the resonating capacity of the particular piano; the overtones produced by the various consonant and dissonant intervals; etc. These observations regarding the purely physical relationships involved in producing tones are the only ones that can meet the determinative necessity required by strictly causal explanations.

What is lacking in this account, however, is any recognition of the factors that make the music characteristic of a therapeutic interaction; it is therefore transparent to the very phenomena which render it of interest to the researcher in the first place! Presumably, it is not the particular arrangement and movement of air molecules that define this interaction as therapeutic, but it is instead the goals and intentions of the participants which serves this function. To ask why clinical music takes a
certain form is to inquire into the beliefs, theories and inner processes of the agent producing this music. The purely physical description is enlightening if our question involves the physics of sound, but is irrelevant to explanations of therapeutic process.

Thus we can gain deterministic knowledge of a Music Therapy session, but the knowledge thereby gained will not bear on the properties that define the Music Therapy session as such. In discussing the type of explanation that Music Therapy research should strive for we must begin with a sense of the type of questions we would like to answer. And we must also determine the criteria that will serve to define legitimate answers to these questions. As we have seen above, merely possessing a research question does not itself suggest which type of answer is legitimate. We have to formulate our explanatory criteria based upon the purpose of asking the question. The type of explanations offered in the context of Music Therapy must then be of the type described as reasons, not causes. The purely physical level of description that causal statements are restricted to cannot account for questions of intent and motivation.

It is important to note that the purely physical/ causal explanation is considered unenlightening or illegitimate here not for purely metaphysical reasons—such as the denial of materialism
and reductionism as general beliefs—but instead flows from concerns regarding actual clinical processes and the factors that reasonable accounts of Music Therapy process must address.

Primary among these concerns is that the underlying mechanisms of creative clinical practice are not rule-based and thus their explication can not allow for prediction. We can not predict how a particular client will respond to clinical improvisation nor how a therapist's clinical intent will be manifested musically, because these are necessarily unique events, reflecting an interaction between the therapeutic relationship and the client's emotional needs at that moment in time and no other. Process laws that can give rise to causal explanations cannot deal with necessarily unique events.

Even if we can explain and describe the purely physical characteristics of the sound—those factors which could figure in a causal account—in a Music Therapy session, the significance of the sound for the therapeutic process will be context dependent. Eliminating the contribution of the context means that we are no longer dealing with music; considering the contribution of the unique context is to preclude purely causal explanations.

Music is a human creation that is defined by the intent with which it is created, and the purposes to
which it is put. Since it is the meaning-laden, intentional aspects of music which define it as such, eliminating these qualities from our investigation is to abandon hope of explaining the efficacy of Music Therapy. Not surprising, then, is the claim of advocates of a new research paradigm that traditional efforts cannot adequately account for the creative and musical component of this process. Any research program that is oriented towards uncovering purely causal mechanisms will not allow investigation of those factors which render sound, music.

It is clear that the events in a Music Therapy session requiring explanation are of the kind described by Robinson\textsuperscript{22} as social/historical ones, inexplicable through purely causal accounts, and whose explanation lacks the force of necessity required by such causal accounts. The appropriate type of explanation for Music Therapy will be of the kind similar to the disciplines of (human) history, evolution and geology, to name a few. All of these disciplines focus on formulating descriptive accounts of past events, accounts which are considered legitimate, though they lack the force of necessity and predictive power.

Robinson's account of the assassination of the mythical Smith was previously invoked to draw a distinction between the concepts of reasons and causes. To explain this assassination, a biologist will discuss
the effect of bullet wounds on human tissue, an historian will make reference to political forces and a psychoanalyst will draw upon aspects of the assassin's deprived childhood. Although only the biologist's account can be said to be a causal one, all three levels of description add something to the explanation of the event, with the historian's and analyst's contributions conceived of as reasons.

Robinson also makes the point that intentions and beliefs can not be causes in the physical sense because there must be an intervening level of description. The assassin might have missed with his rifle shot, Smith might have survived his wound or the gun might have misfired. Though all the historical and psychological aspects of the explanation may have remained intact, it still might have turned out that Smith was not assassinated. (This is why reasons as explanations do not have predictive power and hence, necessity.) The same cannot be said of the physical account of the biologist. A vital organ punctured by a gunshot wound will invariably lead to death.

While the intentions and beliefs of Music Therapists must play a role in the explanation of their clinical interventions, a complete account of Music Therapy process must go beyond such descriptions. The intentions and beliefs must be manifest in certain mechanisms external to the therapist. Hence the
previous claim that no set of clinical procedures can function as the vehicle of transformation can be seen in another light. To acknowledge that one's verbalizable clinical intent can not adequately explain the observed outcomes in Music Therapy is not to disclaim responsibility as a therapist. It is to open up another plane of analysis to reveal the true mechanisms of Music Therapy process. Intentional accounts, though vital, are not themselves sufficient.

Where Explanation Stops

It is important to formulate some criteria of what will constitute legitimate explanations based upon the unique and indigenous conditions of Music Therapy process. One component of this task is determining where explanation stops, i.e. how far we must go in our explanatory account to 1) be saying something of interest, and 2) yet not be required to explain everything. Scriven observes that "the request for an explanation presupposes that something is understood, and a complete answer is one that relates the object of inquiry to the realm of understanding in some comprehensible and appropriate way."23 What he is saying is that, of necessity, an explanation provides knowledge to a certain point, where it connects with what we already know to function as an explanation. Explanations are not deductively self-contained but
require an assumption of prior knowledge.

In physics, Newton's concepts of mass, force and momentum acquired such prestige that they became "ultimate causal powers." To explain an event meant to show how its occurrence could be traced back to one of Newton's laws. The important point here is where explanation "stopped" in classical mechanics was not determined by metaphysical necessity but by historical accident. However we conceive of explanation, it is a product of pragmatic considerations that evolve concomitant with our scientific knowledge.

Often in psychology, behaviors, thoughts and feelings—which may not themselves be rational—are explained by showing how they rest on more fundamental rational beliefs or circumstances. This is true of both psychoanalytic and behavioral explanations. A behavioral account of an individual's irrational fear of butterflies might involve showing how butterflies acquired a negative association through being paired with an unpleasant event such as being lost in the woods. The phobic reaction is reasonable now because it recalls the feeling that first arose in a legitimately fearful experience. The avoidance of butterflies is shown as reasonable, given certain beliefs which may or may not themselves have a rational basis.
Similarly, from a psychoanalytic viewpoint, the butterfly might symbolize some deeply repressed psycho-sexual feeling in the adult who, because of the unresolved infantile conflict, is still enslaved to fears of parental retaliation characteristic of this early stage of development. Showing that an adult is functioning with the fears characteristic of a child is to also show that the fear is rational, given certain other beliefs, without committing oneself to the ultimate rationality of these underlying beliefs. Typically paranoid behaviors are also given this sort of explanatory treatment. The action of avoiding even-numbered streets is reasonable if one believes that these streets are sabotaged with explosives. Again, the underlying belief is not rational, but the action is, given the underlying belief.

Explanations of human actions function by rendering these actions intelligible according to their "connection to real or imaginable goals in the future and to occurred beliefs or preparations in the past."25 This does not mean that the action becomes unqualifyingly rational through the explanation, just rational given the psychological states forming its context. Laudan describes this paradigm of explanation in the following way: "When a thinker does what it is rational to do, we inquire no further into the causes of his action; whereas, when he does what is in fact
irrational—even if he believes it to be rational—we require some further explanation.26 Here, we determine the rationality status of a belief or action according to the extent to which the individual's reasons conform to our standards of "good reasons."

I would like to propose a third category of actions which I refer to as "non-rational" actions. Rational actions are those that we believe will accomplish or help us to reach certain goals; irrational actions are those that inhibit this progress. In using the term "non-rational" I am deliberately choosing one that implies no specific position in terms of goal orientation, or reference to occurrent beliefs. Thus, non-rational actions may function to enhance or inhibit goal orientation, but they are not defined by reference to this quality; non-rational beliefs may result from other, more primary beliefs, or they may exist in a state of justificatory isolation.

In the three examples noted above, the irrational or unexplainable element is pushed back a level but is not eliminated entirely. The explanations invoked are satisfying not because they explain everything, i.e. they may not explain the distortions involved in the underlying beliefs, but because they move the unexplainable to a more fundamental level, thus
explaining the surface appearance. Note, however, that there is still an element of unexplained irrationality in all these examples, whether one considers the genesis of the paranoid belief or the mechanism of the association between the object of the phobia and the fearful feelings. It is as if the explanation of human actions requires a rational reconstruction of beliefs or behaviors which reaches to that which is unexplainable.

These considerations are relevant for the present study because we must decide whether or not it is scientific to end at an non-rational explanation for Music Therapy process; we must decide if this is an explanation at all. Of course what I am advocating is that if we want to know the real mechanisms underlying Music Therapy process--and not just create a rationale for treatment which meets certain pre-determined conventions--we must be open to the possibility (indeed probability) that creative clinical processes have a non-rational basis. It might be considered paradoxical that clinical goals are accomplished through a non-goal oriented process. Yet I am suggesting that the more fruitful research strategy for Music Therapy involves embracing this paradox, and seeing where it leads, as opposed to denying or circumventing it.

It may be that accomplished clinicians rely on intuitive, non-formalizable processes in their work,
and that the efficacy of treatment is due to these processes. If our view of explanation is such that non-rational processes can have no role—and the only legitimate theories are those which reconstruct a rational account of these processes—we will be limiting what we discover through the use of an unjustified convention. This attitude would seem to be more unscientific than allowing that the ultimate mechanisms of Music Therapy process may be not verbally formalizable, though they still may be modelled in some other form.

After all, it is not the case that only those accounts of human actions and beliefs that involve rational reconstructions can function as explanations. I might explain my own predilection for escapist entertainment on any given night by saying I had a rough day at work, the subway was stalled, I got depressed by the trash in the streets on the way home, etc. Here, I am proposing rational reasons for my desire to escape. Alternately, I might have said that I just feel like it, i.e. there is no particular reason for my choice of entertainment, it is just one of those transient impulses that does not seem to have a more fundamental cause. I would hold that this latter account—though having a circular component—is as explanatory as the former; attributing one's actions to the result of random factors with no firmer
deterministic basis is as valid as providing that basis. This judgement results from a personal recognition that my own actions and preferences involve both of these types of accounts. Sometimes, I have certain reasons for doing something, and other times I just feel like it. And I would argue that this latter account is not circular or contentless. It is making a specific claim--namely, that an account of my actions here that appealed to rational causes would be erroneous.

In fact, an explanation that appeals to contrived reasons is more accurately considered a rationalization. This implies that the ascribing of rational reasons for a given preference results more from an attempt to disingenuously make our personal behaviors and preferences appear to result from deterministic circumstances outside of our control. The rational reconstructive accounts move causation, and hence responsibility for action, outside of the individual. Escapist entertainment may be negatively valued in our culture, but "After all, with such a hard day I am caused to need it, (though I know it really is not good for me)." Pure reliance on this form of explanation reflects mechanistic beliefs which serve to relieve the individual of personal responsibility, a conceptual maneuver antithetical to the goals of therapy.
What needs to be separated is criteria for the justification of Music Therapy treatment from criteria for what will constitute legitimate mechanisms of Music Therapy process; the former must have some rational basis that is not required for the latter, and a rational justification for the application of Music Therapy need not presuppose a rational characterization of its mechanisms. Various methods of research can establish the efficacy of Music Therapy treatment, without specifying the mechanisms of such treatment. The use of folk remedies mirrors this situation. One need not be a biologist to know that application of the oils from certain plants will alleviate the itching caused by poison ivy. The use of the folk remedy is rational based upon past experience, though the mechanism of its healing may be either unknown or attributed to a metaphysical, non-rational or symbolic process.

The only possible rationale for Music Therapy treatment is that it works—a judgement necessarily independent of the type of theories proposed for the mechanisms by which it works. Explaining why it is a reasonable action to implement Music Therapy treatment will involve specifying the criteria for making the judgement that it works—criteria that must show the rationality of such a decision. This action must have rational components in order to be explained.
Music Therapy process, on the other hand, need not be rational in the same way. Indeed, it is not even clear what it means for a process to be rational. If what is implied is that the consequences of a theory of Music Therapy process must be consonant with other beliefs characterized as rational, or common-sensical, then the history of science provides evidence against maintaining such criteria. It seems that every scientific advance of knowledge involves some contradiction of common sense or rationality. One need only look at the paradigmatic scientific achievements continually referred to in this study such as those of evolutionary theory, heliocentric astronomy, and relativity theory to see how this occurs.

Alternately, to say that the dynamic mechanisms of Music Therapy process themselves must be rational is to misuse the term "rational." Beliefs are rational and actions are intelligible given rational beliefs. But the world itself is neither. It is not rational that the frequency of blue light is higher than that of red light, or that gravity is an attractive force rather than a repelling one; these are just facts of nature. While Music Therapy techniques are human artifacts, the underlying processes accessed by the techniques exist independently of our beliefs about them and are not constrained by our notions of rationality. Formulating a view of explanation of Music Therapy process that
requires the reduction of the therapist's actions to intelligible ones guided by rational beliefs can not be justified on the basis that explanation qua explanation is rational reconstruction. If it was, that would imply that a rationally reconstructed fiction is more explanatory than the true process which may not be amenable to rational modelling. This would put explanation at odds with truthful knowledge, a philosophical position that would be very difficult, if not impossible, to support.

The explanations of the irrational phobic and paranoid behaviors offered above rest on more basic unexplained, irrational occurrences: there is no reason offered for the genesis of either the paranoid belief or for the initial association between the object of the phobia and the concomitant fear. Thus, we can not use the reduction of irrational (non-rational) behavior via a rational reconstruction in psychology in general as a rationale for using such criteria for explanation for Music Therapy as a special case of psychology. In sum, the two positions that I am arguing against are (1) that explanation of human actions is defined by a reduction to the rational components of such actions, and (2) that rational thought and action is determined and defined by adherence to rule-governed decision procedures.
An alternative criterion is to consider those explanations as fundamental which reach and touch our experience in a way that requires no further explanation. In terms of the characterization of explanation offered by Scriven, Music Therapy researchers must reach a consensus on the nature of our communal knowledge (which does not imply that what constitutes valid knowledge is defined by this consensus), in order to determine criteria for how far our explanations must go. Without the assumption of prior knowledge, and the details of what this knowledge consists of, nothing can be explained.

Prior experience can be our only guide in determining the nature of legitimate explanations. I am not satisfied with the Aristotelian account of dynamics because I have not experienced the strivings or desires of inanimate objects. I am not saying that it is impossible for rocks to manifest desires, just that their actions do not conform to those that normally indicate, to me, the presence of desires. On the other hand, I have experienced these traits in fellow humans and I am prepared to accept references to desires or goals in explanation of human actions. Similarly, explaining the efficacy of a therapeutic intervention in the Music Therapy milieu in terms of 1) the melodic interval employed, 2) environmental contingencies, or 3) psychoanalytic rationales, does
not explain to me because I have not experienced the healing effect of intervals as such, reinforcers or sublimative tension releases. Yet, providing a mechanism connecting some aspect of the musical experience to facilitating access to an altered state of consciousness, ritual space or creative process, would be explanatory to me because it relates to aspects of my experience, which, for the present, need no further explanation.

What is claimed in Chapter V is that the essential elements of the process of generating clinical music—or creative interventions within any Music Therapy technique for that matter—are not themselves amenable to rational reconstructions. But this does not mean that they cannot be explained by being modelled or described in some other fashion. It does mean the mechanisms of explanation must conform to the aspects of the experience which define it as such. Therefore, the only criteria for adequate explanation must involve appeals to personal experience.

Utilizing appeals to personal experience in assessing the adequacy of explanatory accounts in Music Therapy fits conceptually with some of the other crucial positions being advocated for in this study. The need for studying the experience of both client and therapist becomes paramount, if this experience is to function as an arbiter in evaluating theories and
explanations. It is also essential that the role of clinician and researcher be combined. Only clinician/researchers with experience in the modality being researched can judge the adequacy of the explanations offered to account for the mechanisms of this modality. The process of building indigenous theory in Music Therapy will necessarily involve the forming of a community consensus among clinician/researchers as to both the nature of this experience and what will function as an adequate explanatory account.

A possible criticism of this view is that it appears to subsume formal aspects of criteria for legitimate explanations to the unpredictable vagaries of individual, subjective experience. This notion goes against the traditional view that theories and explanations gain power from their formal structure, a characteristic independent of the arbitrary nature of individual judgments.

Yet it was noted previously that all scientific domains require specialized training before one can be a competent evaluator of theories and explanations—a training which involves the internalization of tacit knowledge which is not itself fully formalizable. There is a very real sense in which the nature of a scientist's prior experience performs an evaluative function. Further, as the analysis of the Received
View demonstrated, there does not exist any consensus on formal or logical structures common to all scientific theories or branches of science. Explanations are considered appropriate that solve problems of interest in a manner that is intelligible within the specialized concepts and experience of a particular scientific domain; there is no universal form of explanatory structure because these different domains give rise to variant experiences and hence, worldviews.

Theory-Building in Music Therapy

Realism vs. Fictionalism

Realist accounts of theories presuppose that the entities and processes thus represented have a bona fide existence apart from our consideration of them. The knowledge provided by theories, though inferred, is considered to be as valid as the knowledge provided by our senses. Fictionalist views hold that theoretical entities are merely calculating devices whose value is purely a heuristic one. They help us to draw patterns from observed phenomena—and thus make predictions about the future—but have no ontological status or independent existence. Their existence is purely conceptual and they function as convenient "fictions."

Appeals to fictionalism (alternately called instrumentalism or conventionalism) usually result from
extra-scientific concerns. Because a helio-centric astronomy conflicted with Church dogma which placed man at the center of the universe, Copernicus' system was considered by some to be a calculating device to predict planetary positions, not a real model of the solar system. In modern times, strict adherence to empiricism—the doctrine that all knowledge is provided by experience—has led philosophers to argue for fictionalist accounts of theories. Since theoretical entities are inferred and not experienced sensorily, the empiricist cannot admit that they represent true knowledge without violating empiricist strictures.

Persistent crises or problems in science not yielding to conventional methods of problem solving can also lead to the advocacy of fictionalism. An example here is the manner in which the tendency of electrons to exhibit properties characteristic of both particle and wave phenomena have led some to argue that efforts to determine which conception is correct is misguided. In this view, electrons are "really" neither; the particle or wave model exists only as heuristic, with each model having its realm of application.

Traditionally, Music Therapy theorists have not taken specific positions on this issue, but it was argued previously that fictionalist views were supported indirectly in the literature through the emphasis on research as a providing a rationale for
treatment. One can formulate a variety of accounts of Music Therapy process in both psychodynamic and behavioral terms in creating such rationales. But there is no way of judging the independent truth value of theories implied because each "theory" of Music Therapy process is complete within the system it is proposed. One cannot compare gestalt, psychoanalytic and behavioral Music Therapy theories against each other because their fundamental assumptions are different and their concepts do not overlap. As these various accounts proliferate, they assume a status perilously close to a rationalization, rather than a rationale.

This situation is acceptable only if one believes that the function of theories is merely to provide a conceptual matrix to organize our experience and not to model real processes. Researchers can only test the usefulness of such theories, for to test the theory itself is meaningless in this sense. There is no objective reality or process against which the theory must be measured. If traditional standards are maintained, the evolution of theory-building in Music Therapy will be inextricably tied to the relative popularity of available psychological constructs.

A problem here is that there is no way in which we can then characterize the history of Music Therapy as representing any type of advancement of knowledge. If
adequate rationales for our work is all that we seek then those theories will be accepted and advocated which best meet the current need for which the rationale is formulated.

And this is not just to say that only those extrinsic rationales are counter-productive which arise from the need to explain Music Therapy work to other professionals such as medical doctors or social workers. The internal demands of the profession of Music Therapy also play a role here. In formulating criteria for training competencies and curricula, professional journals and conference symposia, we implicitly approve a fictionalist program by accepting the legitimacy of contributions consisting of the application of external systems to Music Therapy process--applications whose legitimacy is questionable.

Research efforts have not provided us with more knowledge regarding the inherent mechanisms of Music Therapy process than when the profession was first established, a state of affairs that can be directly attributed to the implicit employment of fictionalism as an account of theories. The very purpose of this study is to advocate for an indigenous approach which embodies a realist view of theories. Only indigenous theory can reflect a bona fide realism.

In addition to the personal preference of this author for a realist view, considerations derived from
the portrait of clinical practice presented herein also support realism. Thus, the advocacy for realism as appropriate for Music Therapy has a philosophic basis—that this is the only way all sciences, and hence Music Therapy will progress—and a pragmatic basis—indigenous views of the specifics of Music Therapy process suggest that such an approach will be more fruitful.

The factor that is most relevant here is the claim that to understand Music Therapy process necessarily involves understanding the generating principles of the interventions so described. Here, the therapy is contained in the creative process and is not mediated by a more basic psychological process. However, I would first like to consider the alternatives and show how they are consonant with a fictionalist view. Then, I will show how the indigenous perspective reflected in the ascribing primacy to the creative process necessitates the adoption of a realist position.

A plausible argument can be made for a fictionalist position in Music Therapy if one begins from any of the following assumptions: the efficacy of therapy is to be sought in the effect of the specific musical elements upon the client; understanding the psychodynamic value of music, through mechanisms such as sublimation and repression, will adequately account for Music Therapy process; the musical product can be
considered the result of behavioral reinforcement. In all of these cases the actual process of producing music recedes in importance and becomes a secondary concern, if it is considered at all.

The theories generated from any of these perspectives will employ the constructs of these particular systems and facilitate communication among analytic, psychodynamic or behavioral Music Therapists. It does not matter if the theories model actual creative processes—or if the constructs used are conducive to describing creative activity—because these creative processes are not being considered relevant to clinical outcome; they merely provide the field in which the more basic psychological or behavioral process is manifest. The value of theories in these non-indigenous systems is determined by how well they facilitate communication among researchers using the theory, not by the degree to which actual creative processes are represented. The continuing proliferation of personality and psychotherapy models can only lead an objective observer to the feeling that they are just convenient calculating devices, and the fictionalist position becomes self-justifying.

Conversely, let us assume that the clinical value of Music Therapy lies in the activation of creative processes. To explain the efficacy of Music Therapy we would then need to account for the contribution of
these processes. Thus, it would be important for the theories we devise and test to accurately represent the actual processes as they exist in the clinical milieu. Our goal here is to learn more about the actual generation of clinical music, not to merely re-interpret musical phenomena through an alien formal system. The theories used here would find their value through the degree to which they reflected a majority of practitioners' experiences which are the phenomena we are trying to explain.

In order for a scientific theory to add to knowledge it must be capable of being wrong. In discussing realism, Manicas & Secord note that "one must be a realist ontologically to be a fallibilist epistemologically." What this means is that one must hold that theories refer to an external reality in order to say that some are true and some are false. Astrology does not add to human knowledge in a cumulative sense because it is not falsifiable—any outcome can be interpreted in light of the astrological prediction. To assume a fictionalist position is then to deny that theories are ever right or wrong, and is thus to deny any progression to scientific knowledge. Certainly this can become a self-fulfilling prophecy, as has been argued is the case in Music Therapy.
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**Natural Classes and Domains**

Contemporary philosophy of science holds that there is no objective "given" that theories attempt to explain—observations are not theory-neutral. While a theory determines the relevant data for a particular field, and thus the variables which one can expect to be connected, it does so with the assumption that the data so defined reflect an underlying natural order or class. Since the nature of a theory defines the data of interest, we can see that the assertion "that a body of information constitutes a domain is itself a hypothesis, and may ultimately be rejected." This means that any particular formulation on the nature of a research discipline is itself open to question. In Chapter IV we saw how the various theoretical approaches alternately construe the domain of Music Therapy by what they consider the given (data), and how they attempt to account for it.

The most influential position in Music Therapy research in this regard, has been the behavioral one. Attempts are made at establishing correlations between observable behaviors and formal aspects of music, presumably because these represent the only mode of description of Music Therapy treatment which can meet methodological criteria. It is important to note that the behaviorist does not postulate a fundamental, underlying relationship connecting these two elements.
They are considered part of a unified domain more from a process of elimination, i.e. no other descriptions are appropriately public, objective, etc., than from a positive assertion that there is a reason to believe that correlations thus established reflect an underlying unity.

Yet without a fundamental working hypothesis on why these items should be connected there is no reason to believe that they comprise a scientifically meaningful domain! Since there is no absolute "given" that all theories in a field strive to explain, the assertion that musical behavior should be the data of interest to Music Therapy research is dependent upon a theoretical rationale, i.e., construing the discipline in this manner reflects a particular theory whose validation has value in answering important questions. Yet, what has been argued in this study is that construing the data in Music Therapy as "behavior" reflects a meta-scientific, methodological concern, and there is no indigenous research problem which justifies this particular formulation.

In addition to lacking an indigenous rationale, there are strong conceptual reasons for believing that limiting data to musical behaviors necessarily can not illuminate the processes relevant to creative Music Therapy techniques. (Recall the arguments in Chapter V regarding the necessity of studying the meaning of
clinical music in any justification for Music Therapy treatment.) Thus, it is concluded, that the behavioral formulation, as applied to Music Therapy, fails to delimit the discipline in a way that holds promise for the illumination of its fundamental processes. The lack of theoretical development and continuing lack of research applications support this conclusion.

But we still need to articulate those elements which can figure in a promising formulation of Music Therapy research. This means articulating a level of description which reflects a natural order or class, and thus holds promise for yielding to research efforts. In this study I have postulated that the therapeutic efficacy of Music Therapy results from the action of creative, transformative forces (for which I claim neither exclusivity nor originality) that therapists help clients access through their intuitive and creative clinical interventions. Though this may appear un-scientific at first glance, I would ask the reader to consider more traditional scientific hypotheses such as those postulating the existence of the force of gravity, innate instincts, biological evolution, holographic phenomena and self-actualizing drives, and to at least consider that there is nothing in my hypothesis rendering it inherently less scientific than the aforementioned ones. In any case, the challenge remains to formulate this hypothesis in a
way that it can lead to more specific, testable theories. (Note that the concept of a creative force is only one, imperfect way of providing a fundamental description of what I have in mind. Other possible descriptions are "field or ritual space" [Kenny], "transformative agent" [Hesser] and "altered state of consciousness" [Bonny].)  

This task involves an empirical effort beyond the scope of this study. What I would like to do here is not pursue the specifics involved in such a task, but instead defend its general intelligibility by suggesting how empirical efforts could be construed in such a framework. If this task can be accomplished, the way is open, conceptually at least, to develop a clinical model of Music Therapy that defines a natural order and hence holds potential for theoretical development. 

If creative Music Therapy does involve accessing the type of transformative force described above, and if therapists are recognized who are more effective than their peers in achieving the goals of therapy, than it follows that these advanced therapists can better facilitate access to, and work with, this force. Certainly the first step in any indigenous research program would be to discover what accomplished, creative Music Therapists are actually doing while engaged in their work. This would involve descriptions
of the type of information therapists are processing, how their clinical intentions are being formulated and how they are experiencing their client's expression. Each Music Therapist must have an implicit theory that is guiding their work—even if this theory does not meet conventional scientific criteria. An indigenous research effort must focus on describing these various theories and discovering if there exist common or universal elements.

Of course the nature of the client's experience is just as vital here. Since it is the transformation of, or contact with, the client that is the purpose of Music Therapy, their experiential reports (when not limited by communicative barriers) must be the ultimate arbiter of the nature and efficacy of clinical techniques. These reports can help to determine how the therapist's intentions are being manifest through the music and to the client, and which aspects of the therapist's intentions are essential to this process, and which are peripheral.

Thus the third element in this research model is the comparison of the therapist's experience to that of the client. This step is necessary to distinguish the aspects of the therapist's experience functioning more as a personal heuristic or facilitator, from those which are correlated with the client's experience and are thus connected to the therapeutic process in a more
essential way.

For example, therapist's might use the concept of energy fields in describing their experience of the client and the manifestation of changes in the client. A client on the other hand, may or may not feel a personal change that can be described in this way. If client reports are positively correlated with therapist reports in this regard, that would be evidence that the transfer of energy is a vital part of Music Therapy process. If such correlations are not evident, it would suggest that the concept of energy used in this explanatory fashion was due more to factors idiosyncratic to the therapist, than to essential components of the therapeutic process. This is not to say that the uncorrelated descriptions therapists use to report their experience is only a heuristic, for it may be the heuristic which allows access to the transformational capacity; it is to say that the lack of correlations would suggest that there is a more fundamental relationship correlating the therapist's intent to further their client's growth, with the factors that are enabling this to occur.

The natural classes of phenomena in Music Therapy--those entities referred to in theories--must arise on a level of description which is both basic and suggests an even more fundamental relationship. Throughout this study it has been argued that
behavioral accounts capture mere epi-phenomena which are not of intrinsic interest, and that this data cannot figure in bona fide theory. It is not behaviors that successful Music Therapy theory will account for, but the experience of client and therapist, and the process that gives rise to this experience.

**Compositional vs. Evolutionary Theories**

Earlier, an interesting and unexpected similarity was noted between behavioral and psychodynamic explanations in Music Therapy. Both types of theories were seen in terms of enacting a reductive, structuralist approach towards explanation. Though providing radically different types of answers to the question of the mechanisms of Music Therapy process, both of these approaches share a common view of the research problem as what Shapere refers to as a compositional one: "A compositional problem is one which calls for an answer in terms of constituent parts of the individuals making up the domain and the laws governing the behavior of those parts." Thus the inadequacy of each of these approaches for Music Therapy is due to a common fault, which goes beyond their non-indigenous character.

Shapere contrasts these compositional problems with evolutionary ones that "call for answers in terms of the development of the individuals making up the
domain; paradigm examples are the Darwinian theory of biological evolution, theories of stellar evolution, and theories of the evolution of the chemical elements.\(^{31}\) I would like to argue that indigenous considerations regarding 1) the transformative nature of Music Therapy process, 2) the status of the salient properties of this process as emergent ones, not explicable by the relationships among its constituents and, 3) the very characterization of Music Therapy treatment as a spontaneously evolving process, not the application of a static, formalizable procedure, all strongly support the conception of the fundamental research problem in Music Therapy as an evolutionary one for which compositional theories are inappropriate.

Recognizing the concept of transformation as a legitimate form of scientific explanation is a relatively recent development, which "was not applied . . . before the second half of the nineteenth century."\(^{32}\) Previous to that time it was felt that the heavens were immutable (reports of "new" stellar objects were denied), chemical elements maintained their permanence, and the identity of an organic species was fixed. It was only with the gradual success of Darwinian theory that evolutionary reasoning was accepted in science. The study of processes and transformation represents an advance in forms of reasoning over conceptualizations involving static,
permanent qualities.

Transformation is a difficult concept for mechanistic science built upon traditional concepts of objectivity to deal with. Qualitative differences emerging from transformative processes tend to be obscured in such explanatory systems. Consider a typical experimental research design that might be employed in Music Therapy. The effects of Music Therapy are being compared to drug therapy in enhancing the social interactions of hospitalized psychiatric patients. We will assume a standard experimental design with control groups, random assignments and the even distribution of all relevant variables.

A fundamental assumption of this type of research is that, at the conclusion of the study, Jones who received Music Therapy, is the same individual as before the study, except for the effects of his treatment. Who Jones is has not changed, in this view, but now Jones initiates five social contacts in the day room as opposed to three.

Yet the reality is that who Jones is has undergone some sort of fundamental change. If Jones has achieved a slightly higher degree of freedom from his paranoid delusions and fears, and now possesses an enhanced potential for a satisfying life, I would argue that the change in Jones is not merely a quantitative one, that can be fully captured by the observed increase in
social contacts. Jones' growth might in fact be manifested by a reduced total of social contacts, but an increase in the depth and hence the quality of these contacts for Jones. Once one's capacity to respond to the world has been altered, and this capacity can not be fully captured by an operational definition, then the change must be described as a qualitative one.

The fundamental premise of compositional theories is violated when true transformation occurs. One cannot assume the presence of an unchanging, stable background or context through time; when one's personality is transformed, the stable background against which quantitative change must be measured, itself changes. A chemical element that has an electron and proton added is not just "an element with two added particles" but a new and fundamentally different element. A new species is not just its ancestor with a different skeletal structure or arrangement of chromosomes, but a discreetly different biological entity. Recognizing that Music Therapy process effects qualitative changes requires that the theories we invoke to explain this process be up to the task of accounting for these types of changes.

Taking into account the full implications of the fact that Music Therapy occurs in music suggests why transformational conceptualizations--and hence evolutionary theories--are more appropriate in this
domain. Burrows observes that "a piece of music is no enduring spatial entity going through transformations of shape or location," and that "music . . . goes as far as may be toward isolating the quality of change without surrendering the quality of integrity."\(^{33}\) Music is the constant transformation we experience in the ever fleeting, present moment. There is no permanent, unchanging thing which is the piece of music apart from our experience of its continual development. Its identity is revealed in the process of experiencing it.

Taking the view that "an organism is an entity whose stability depends on change,"\(^{34}\) suggests that people are more like what we normally conceive of as processes than things. The maintenance of our individual identities, and our ability to flourish in life (thus our stability), depends upon the degree to which we can yield to and embrace the processes of change and transformation. This was made clear in the previous discussion regarding the life and health enhancing nature of creative activity.

As living beings, we share with music this quality of identity through transformation. Burrows characterizes music as "the clearest artistic acknowledgement we have of the radical interdependence of stability and change which is life."\(^{35}\) Any attempt to understand and research Music Therapy process must
come to terms with this recognition--and thereby explain why and how Music Therapy works based upon the essential, defining qualities of music.

An Epistemology for Music Therapy: Concluding Remarks

Musical interaction is a unique way of gaining information about ourselves, other people and our physical, social, and psychological environment. Through Music Therapy techniques like clinical improvisation, a therapist can engage in a musical interaction with a client, and help to create music which facilitates a fundamental transformation in the inner being of the client. In some way, the inner world of the client becomes manifest to the therapist; clinical results render this judgement undeniable. Rather than forcing the knowledge gained in this manner into categories derived from purely verbal modes of thought, I am advocating that we allow the musically obtained knowledge to suggest its own epistemology. It is essential, therefore, that the construction of Music Therapy research models proceed from a cognizance of what type of knowledge the model must account for. In order for research methods to illuminate the true mechanisms of Music Therapy process, they must--first and foremost--be adequate to our experience as clinicians.
In arguing for a return to clinical experience as the "archimedean point" around which the elements of a research paradigm must be built, I am arguing for a position which may seem antithetical to the direction science has traditionally proceeded. Particle physics, biological evolution and theories of geological development all seem to point to the fact that both "the way things are" and, how they got to be this way, are quite removed from our everyday, common-sense experience. Mountains are not produced in dramatic, catastrophic upheavals, but are constantly in the act of being created even as we observe them. And the chair I am now sitting on is not solid at all, but consists primarily of empty space with a few points where an energy field is locally condensed. The progression of natural science reveals that the world is fundamentally different from our conscious experience of it.

This should not be a surprising conclusion because scientific knowledge results from an interaction between highly specialized techniques and novel concepts and worldviews suggested by the knowledge thereby gained. Metaphysical and conceptual speculations are not superfluous to scientific activity but are integral components of it.\textsuperscript{36} For example, through an effort to make sense of the empirical knowledge gained about the structure of matter,
physicists have always struggled to define and enlarge epistemological and ontological conceptions. Changes in epistemological notions resulting from research efforts (involving both the laboratory and the armchair) then fold back on these efforts to suggest new methods of research. There is no cookbook style recipe for how to do science because notions of what we consider legitimate science evolve through an interaction among empirical discoveries, conceptual reorganizations of existent data, and novel conceptions of what it is possible to know.

What I hope is clear now is that the activity of science involves continual reappraisals of epistemological notions in light of novel research discoveries. And yet this is exactly what I am proposing we do in Music Therapy. In requiring our epistemology, and hence our research methodology, to be adequate to our experience as clinicians, I am placing Music Therapy research in the mainstream of scientific activity—not in opposition to it. In maintaining that our theories and methods be adequate to our experience as clinicians, I am only saying that our conceptual apparatus must be adequate to our data—a standard to which all scientists aspire.

The crucial link here is to realize that the clinic is our laboratory and the experience of clinicians and clients in Music Therapy process is a
highly specialized one. Clinical research is not something that must be performed apart from treatment but is contained in our activity as clinicians. Embedded in our clinical techniques are highly refined methods of gaining information about the needs of clients and the manner in which our musical interventions can meet these needs. We need not create experimental constructs alien to clinical techniques in order to abstract and model this knowledge; what we do need to do is find ways to render our implicit knowledge more public, thus putting it in the service of enhanced clinical treatment and training.

In implementing such a research program it is essential to accept the judgement that scientific rationality is not identified with "experimental control and quantitative precision."37 No philosophy of science that has attempted to model the rationality of science according to such criteria has been successful. That the method(s) of acquiring knowledge through Music Therapy techniques has neither of these qualities is no criticism of the rationality of such knowledge.

Indigenous research needs and contemporary philosophy of science suggest a more pragmatic view of scientific rationality and progress where "rationality consists in making the most progressive theory choices, not that progress consists in accepting successively
the most rational theories. The traditional dependency of progress upon rationality is inverted; what is rational is what leads to progress, not the reverse. Progress, in this view, is defined by enhanced problem-solving ability. In Music Therapy, those theories and methodological guidelines will be most progressive—and hence most rational—that can address and solve those problems deemed important by the community of researchers. The adequacy of any theory or research program is not to be judged upon its formal structure, or ability to rationally reconstruct the elements of clinical process.

We may find that in pursuing the consequences of clinical experience elaborated by such methodological concerns we are inexorably drawn to an epistemology which is in opposition to more common-sense notions. In fact, we should possibly expect successful theory to produce such tensions because integral to scientific activity is the enlargement and transformation of epistemological categories. Our challenge as Music Therapy researchers is to maintain the open-mindedness, intellectual rigor, and commitment to the nature and meaning of our clinical work, necessary for truly progressive and clinically relevant research, regardless of where it leads.

2. Ibid., 83.

3. Ibid., 84.


6. Ibid., 149.


9. Ibid., 44.


13. This scheme for differentiating among the various levels of psychotherapeutic intervention was first devised by Wohlberg, first applied to Music Therapy by Barbara Hesser (personal communication, January 21, 1991) and first published as an application to Music Therapy by Barbara Wheeler, "Levels of Therapy: The classification of music therapy goals," *Music Therapy* 6(2), 1987, 39-49.


17. Hull 120.

18. Hull 47.


22. Robinson, Philosophy.


24. Hanson 91.

25. Robinson 55.


28. Shapere 527.


31. Ibid., 534.

32. Ibid., 554.


34. Ibid., 242.

35. Ibid., 246-247.

36. Laudan.
37. Laudan 191.
38. Laudan 6.
CHAPTER VIII

METHODOLOGICAL RECOMMENDATIONS

The final step in this study is to provide a conceptual framework for studying aspects of Music Therapy process that lie outside the domain of traditional research. Those aspects of Music Therapy process--identified primarily in Chapter V--not amenable to investigation by traditional research efforts will be of primary interest here. By considering these phenomena to be in the legitimate domain of research in Music Therapy, the explanatory focus of the field is being redefined. A qualitative change of this type requires a concomitant change in research methodology. Since the limitations of traditional methods stem from their philosophical bases, it becomes apparent that new methods will require a new philosophical justification.

This effort necessitates changes in standards of observation and explanation, rather than the abandonment of such standards. The goal here is to preserve the structure and salient elements of scientific practice, though components of this practice may change in non-trivial ways as necessitated by the nature of creative clinical practice.
As has been repeated often in this study, there is no single view of science that enjoys a significant degree of widespread acceptance; indeed there are good reasons to suppose none are possible and that the nature of scientific activity cannot be formalized or presented through any content-free description. Suppe holds that the attempt to do so is a fault common to both the Received View and the Worldview analyses, positions which are, in other respects, diametrically opposed.¹

There is no scientific method, per se, but this does not mean that philosophers of science have packed up their bags and sent out resumés! Philosophical efforts to model science continue through investigating: scientific forms of reasoning;² persistent themes that influence theory-building;³ the effect of shared exemplars;⁴ and the role of problem-solving ability in theory acceptance⁵. There is still a role for philosophy of science, though that role is changing in the direction of becoming less prescriptive and more descriptive.

In this chapter, I would like to go against this trend and adopt a prescriptive pose. In doing so, it is imperative to realize that I am not stepping outside the role of a researcher, but am engaged in the conceptual component of scientific research. The methodological suggestions herein proposed flow from
the inadequacies of the conceptual apparatus of the traditional paradigm in accurately describing and explaining clinical process.

The activity of science involves coming to terms with new and unfamiliar information. As such, it is quite similar to basic processes that all humans undergo as we develop and learn about the world. Cognitive psychologists (strange that such a term is not considered a redundancy!) such as Jean Piaget and Ulric Neisser describe two alternate strategies we can engage in when confronted with new experiences or information: We can subsume the new instance under existing cognitive structures in a process called assimilation; conversely, we can alter these structures and develop new concepts and presuppositions in a process described as accommodation. Assimilation involves a quantitative gain in knowledge as we add to the applications of our existing concepts and structures; accommodation involves a qualitative gain as our intellectual identity undergoes a fundamental transformation. This latter type of modification "is entered upon in the hope of achieving thereby closer contact with reality".6

Music Therapists—by opting for existent conceptual systems such as behaviorism and psychoanalysis—have traditionally leaned more heavily on assimilative methods and theories in research. This
overly conservative strategy has limited the degree to which our theories and models accurately represent clinical process. It follows, then, that any new and successful research program must consist of principles, strategies and structures of an accommodative nature. They must allow for the creation of novel concepts, not merely for the assimilation of information to existent ones.

What this implies is that the process of research, no less than that of Music Therapy itself, requires an openness to personal transformation. This is a consequence of understanding research activity as the throwing into relief of a specific aspect of clinical practice. Since the practice of Music Therapy involves transformation, and research is a component of this process, it follows that engagement in research will subject the participant to the same transformative capacities present in clinical practice. In stressing accommodative research methods which facilitate qualitative changes, we are building a philosophy of science whose precepts are in accordance with the essence of that which we want to study.

Although appearing unconventional, this realization is consistent with the denial of other traditional dichotomies stressed throughout this study: namely, that scientific research is a specialized form of knowing that is, nevertheless, not distinguished
from humanistic inquiry by a higher claim to truth and certainty; Music Therapy research is contained in treatment and is not a fundamentally different activity; the tacit knowledge gained in musical interaction is valid, regardless of our ability to rationally model its process of acquisition via systems of rules.

In this chapter, the areas of experimental language, theory-building, models, and research designs and methods will be looked at with a focus on considerations that will enhance the accommodative aspects of these tools. In this way, the resultant, implicit conceptual basis will be epistemologically supportive of investigations of the transformative processes held to be the defining characteristic of Music Therapy process.

Towards an Experimental Language

The prevailing conceptual systems that have been applied to Music Therapy all, in one or another way, maintain a view towards language that renders them inappropriate for application to Music Therapy process. Psychoanalytic authors (with the notable exception of Jung) equate nonverbal language with preverbal modes of functioning. Thus, the use of art forms, in an expressive sense, is necessarily reduced to deficit functions such as regression and sublimation. Here,
the verbal expression is considered to be the healthiest and most direct, and the musical one only holds a "disguised" version of the verbal content—a disguise necessary to outwit defense mechanisms or psychological and social taboos. There is no transcendental, expressive, poetic—or otherwise non-denotative—truth the psychoanalytic account must be faithful to; the fundamental meaning of the music is explicated solely in terms of the dynamics of the relevant personality structures.

The logical positivist basis of behavioral thought places a similar primacy on the role of verbal language in knowledge. Science is seen not in terms of a human activity, but as the "linguistic product of such activity".⁷ All knowledge can be represented linguistically here, and all sentences of language represent "logical claims, empirical claims, and nonsensical utterances".⁸ The realms of logic and science exhaust the forms of meaningful discourse. There is no room in such a philosophy for notions of symbolic or expressive truth; whatever is not logical or empirical is meaningless.

The relevant deficiency of both of these systems is that they assimilate musically acquired knowledge to pre-existing epistemological categories, thus filtering out that aspect of the communication that requires a symbolic, non-denotative, temporal form to begin with.
No language system operating under such assumptions can capture the aspects of a musical expression that either 1) require musical form or 2) are better expressed and apprehended through music.

Some authors, such as Susanne Langer,⁹ admit that "music articulates forms which language cannot set forth" and that the "classifications which language makes . . . preclude many relations." Yet, in Langer's case at least, it is concluded that it is "because music has [a different pattern] that it lends itself to the revelation of non-scientific (my emphasis) concepts."¹⁰

Here, then, is where the current study is departing from traditional writings on music and its representation in verbal forms. I would ask that if these forms are knowable, by what criteria are they deemed non-scientific? I question the claim that the knowledge of forms and relationships conveyed by music is beyond the pale of scientific knowledge just because it is not subject to discursive representation in verbal language.

This observation taken alone is not enough to disqualify what I am calling musically-encoded knowledge from systematic investigation. In physics, for example, there exist mathematical models (of light, spatial dimensions beyond three, alternative geometries, etc.) that resist non-formal (iconic)
modelling. Yet these mathematical equations—representing the structure of matter and space—are not considered non-scientific because they are neither literally picturable in an iconic fashion, nor representable on paper or in space. In other words, though the numbers are not directly translatable into an entity that can be conceived of non-mathematically, i.e., through verbal language, physicists still employ mathematics.

Music, then, serves the Music Therapist as mathematics serves the physicist. That is to provide access to knowledge unrepresentable solely through verbal, discursive forms. In fact, though Langer does want to consign knowledge obtained through music to a non-scientific status, she nonetheless feels that "the limits of language are not the last limits of experience and things inaccessible to language may have their own forms of conception . . . their own symbolic devices." And it is precisely because the "things accessed" through music have a non-denotative, non-discursive nature that musical expression and interaction can be such a valuable form of therapy; it is in the very difference between music and verbal language that one finds the clinical efficacy of music. It is not that verbal language cannot capture all of the experience of music that is the problem—rather it is that verbal
language, traditionally employed, cannot capture the very elements of music that render it of interest to the music Therapist.

As was observed previously, creating music with another individual is a way of acquiring information about that individual, much as in a verbal interaction. The difference between the two is that while the information we gain through a verbal interview needs no translation to be conveyed to others—which it may undergo a process of distillation—the knowledge gained through musical interactions undergoes a transformation in representative form when we talk or write about it. It is therefore essential when talking about musical phenomena (and by this I mean not just music itself but also those contents of consciousness encoded musically) to do so in a way that preserves the qualities of the experience that render it of interest to the Music Therapy researcher.

Burrows suggests that one motive for representing music is to fulfill the need "to establish control over ... things ... by converting them into alternate forms."\(^{13}\) Truth in representation is not necessarily sought because "it is ... their very distance from the original that gives those representations motivated by a need for psychologic control their utility."\(^{14}\) In utilizing a language devoid of expressional, aesthetic and temporal components Music
Therapists have fallen into this trap of desiring to control the musical component of Music Therapy through their verbalizations. This desire to control the "messy" aspects of music has led to an impoverished language, lacking the ability to represent the unique and defining aspects of clinical experience—those most in need of illumination.

Our verbal renditions of Music Therapy process must be motivated not by a deficit need to control, but by a spirit of discovery and emergence. Kenny suggests that to accurately communicate the nature of musical healing requires "a new language, one which expands, just as music itself." A language or conceptual system adequate to the task of conveying the nature of Music Therapy process must then be an expansive, accommodative one, capable of generating new cognitive structures and thereby capturing the organic nature of this art.

A language with strict, pre-determined, denotive values will not be able to function as a tool in an accommodative process. To expand our categories of knowing requires a more flexible language system that can allow for the emergence of naturally-occurring phenomena. And this is where we see one of the primary deficiencies of utilizing "imported" conceptual systems in a descriptive function.
Any musical interaction can withstand as many verbal, post-hoc interpretations as there are interpretive systems. Each system has its own elementary components—ego, anima, response—that serve as its units of experience and description. Further, since these units were not created with musical phenomena in mind, their connections to such will necessarily be arbitrary and idiosyncratic. They can then never be used to form the basis of a language of experience among the community of clinical improvisation practitioners. Since such units or classes of experience may not be relevant for Music Therapy, the resultant taxonomy has less likelihood of delineating naturally occurring categories and processes. Only indigenous theory can lead to a natural classification system, as opposed to the artificial categorization resulting from imposed theory.

The representations we use to discuss Music Therapy must be reliable; their referents must be agreed upon by practitioners, those who share the experience the language refers to. This necessitates that research be guided and undertaken by experienced clinicians. The only way to build a research base with a strong foundation is to ground the constructs and language in the experience to be illuminated. Once this effort has made gains it will be possible to
effect the translation to a more objective language, one whose referents are not dependent upon the shared experience.

An implication of Wittgenstein's analysis of language is that all language ends in acts of pointing. If two people cannot "point to" the same object, process or experience, they cannot establish a mutually understood language. In the first stage of a new research paradigm for Music Therapy it is essential that practitioners find ways to reach agreement on the fundamental nature of clinical process, whether or not the language they do this through is intelligible to the uninitiated. To accomplish this, it will be necessary for the language of research to be more isomorphic with music itself, sharing in its expressive qualities.

Once the need for this expansive, expressive language is recognized an abundance of sources of information become available to the researcher: these include iconic images and symbols, subjective feelings, reports of body sensations, and other accounts of clinical process from therapists, clients and third party researchers/observers. By allowing subjective reports in the chosen language of the reporter, we ensure that the effect of the researcher's preconceptions are lessened, and that the true determinants of clinical process are allowed to emerge.
What is obvious is that to research Music Therapy process the translation from musical to verbal language must occur at some point. Behavioral research attempts to employ purely quantitative aspects of music, thus maintaining the precision of language, but sacrificing the representation of the real data of interest: the meaning and expressive value of clinical music. Those reports in the language of imported psychodynamic theories involve a translation that occurs outside the pale of the research effort. The first order description of the data is in the language of the imported system, thus obscuring the fact that there is any translation at all. What I am proposing is that we embrace the fact that this translation is necessary by making it an integral component of the research effort. In using expressive and poetic language in clinical reports we allow our initial verbal translation to continue to carry the expressive value of the music. The subsequent conceptualization will occur as part of the public aspects of the research. In this way we can allow the expressive value of the music to speak in the research effort—-not be obscured or denied---and to guide us in building descriptive and explanatory constructs. Thus, our primary verbal reports are more veridical representations of the phenomena and our theoretical constructs are more likely to articulate essential aspects of clinical process.
Some may be tempted to criticize this view, holding that utilizing a language intelligible only to a limited few and without publicly agreed upon referents is anti-scientific. Yet earlier a case was made for considering the experience of clinicians to be the ultimate arbiter of theories in Music Therapy. If we accept this argument, then the fact that our research language is best (or only) understood by such arbiters is not a criticism, but may actually be a necessity. If establishing a consensus among researcher/clinicians as to the nature of Music Therapy process is our research goal, we should be prepared to use language in the most expeditious way to facilitate this goal. What is scientific, in this view, is adapting our methodology to solve our research goal, rather than holding to an a priori methodological tenet (regarding the nature of an appropriately scientific language) regardless of the tractability of our problem.

The nature of the data we describe through the specialized scientific language is determined by the theory we hold and all theories exist as part of a larger research tradition. One of the functions of a research tradition is to sanction certain working assumptions (of a methodological and epistemological nature) so that individual researchers can pursue the practical and conceptual problems in their areas of
interest. Observers external to the particular research tradition may criticize the theories so constructed, but "the scientist knows that his primary audience--fellow researchers within the same tradition--will not find his working assumptions problematic." In insisting that the language, theories, and explanations used in Music Therapy be adequate to the experience of clinicians, I am merely establishing that the primary audience of initial research efforts is fellow Music Therapists. To establish a primary audience that is limited in this way may in fact facilitate scientific progress because we can more readily assume the presence of a common body of background knowledge against which our theories will be evaluated. Rather than being anti-scientific, this strategy is eminently scientific.

Understanding the nature of theories and their functions in research traditions is crucial to making any recommendations in their formation for Music Therapy. It is to this question that we now turn.

Theories, Problem-Solving and Pragmatism

Theories and Progress

Anyone who studies philosophy of science must come away humbled when confronting the literature on the structure of theories. The last thirty years has seen the complete abandonment of the logical positivist view
of theory and explanation with no single account able to replace it. It is a startling fact that philosophers cannot seem to articulate the precise nature and function of theories in science.

Rather than focus on formal structure, or degree of consonance with an external reality, i.e., truth, Laudan has pursued a more pragmatic approach towards characterizing theories. I would like to explore his ideas further before drawing the parallels to the role of theories suggested by indigenous concerns in Music Therapy. Laudan advocates that in evaluating theories

it is more important to ask whether they constitute adequate solutions to significant problems than it is to ask whether they are "true," "corroborated," "well-confirmed" or otherwise justifiable within the framework of contemporary epistemology.¹⁸

Laudan refuses to see the dispute between realism and fictionalism regarding the ontological status of theoretical entities as providing the terms of debate for characterizing the nature of theories. Any one theory may turn out to model actual processes and entities, or it may turn out to be merely a calculating device; Laudan's claim is that practicing scientists use other criteria in determining theory-acceptance.

Declining to see the debate over identifying the status of theories in these terms, and yet wanting to preserve a notion of scientific progress, it is Laudan's task to provide a description of theories which preserves the possibility of rational criteria
for choosing between them. It is his characterization of the essence of science as embodying problem-solving which Laudan proposes as an alternative to the traditional emphasis on science as acquiring greater truth.

Where Laudan's approach is novel is in recognizing that scientists endow certain facts and problems with a greater significance than others. In studying theory acceptance, the typical philosopher has tended to "ask how many facts confirm it, not how important those facts are. He will ask how many problems the theory solves, not about the significance of those problems." 19 This approach ignores the fact that scientists assign alternate values to different problems in the domain, and criteria for an adequate theory will depend upon the importance of the problem. The adequacy of any given theory is then dependent upon its ability to solve significant problems, and only secondarily consists of its amenability to verification by objective criteria.

Though the methodological beliefs of scientists play an important role in their evaluations of theories, sometimes in the conflict between theory and methodology it is the methodology itself which is altered. This occurred in the physics of the second half of the eighteenth century where certain Newtonians "took it on themselves to hammer out a new methodology
for science which would provide a license for theorizing about unseen entities."\(^{20}\) The important point here is that a truly compelling theoretical advance carries its own justification. Our notion of what constitutes valid explanation and confirmation changes to accommodate such formulations. Theory, then, is not dependent or derived from method, but interacts with method in a mutually transforming relationship.

Another important function of a research tradition (Laudan's term for the social unit of scientific organization) is to supply the researcher with "an ontology which specifies ... the types of entities which exist in the domain"\(^{21}\) and the allowable modes of interaction of these entities. Theories address the "empirical problems in the domain by 'reducing' them to the ontology of the research tradition."\(^{22}\) In this way, behaviorism specifies that all that exists for purposes of psychology is organismic behavior and the external environment. The only sanctioned mode of interaction is through various conditioning principles. Similarly, mechanistic physics held that all that existed was particles in empty space and the only allowable mode of interaction was for one particle to strike another. All the specific theories of behavioral psychology and mechanistic physics assumed their respective ontologies and functioned by putting
empirical problems into the allowable terms.

The important points of Laudan being considered here can be summarized as follows: at times the nature of theory determines methodology, not the reverse; the most important characteristic of a good theory is its ability to adequately solve important problems; theories function as bridges, connecting the empirical world with our metaphysical assumptions of what the world itself consists of. In all of this, we can see the implementation of highly pragmatic concerns. After exploring the implications of these ideas for Music Therapy research, I will discuss some general concerns regarding this epistemological pragmatism.

The idea that method sometimes follows theory is in accordance with the basic goal of this study: the formation of an indigenous research paradigm. Changing one's methodological assumptions and standards is legitimate when existing structures cannot accommodate either data or important research problems. Deriving theory from clinical experience, and method from theory, is essential in establishing an indigenous paradigm. Thus it would seem that the present effort is not an essentially radical one, but is part of normal scientific activity. The fact that Music Therapists are beginning to explore new methods, new models (see below) and new formulations of the nature of their domain is permissible (and even inevitable) in
this evolutionary view.

The difficult point to realize—and one whose validity must be assumed—is that the criticisms of novel methods and theories according to the standards of a prior paradigm are inappropriate and irrelevant to a new and emerging paradigm. Methodological standards that determined a good behavioral Music Therapist are not applicable to researchers working under new assumptions. This is not to say that there are no standards—just that there are new ones.

For example, behavioral Music Therapists allowed the bounds of their data to be drawn by methodological concerns of observability and quantifiability. The data one could work with was determined by these epistemological limitations. In the current view, it is adequacy to the research question that should be the valutative criterion. Here, the nature of our data is determined by what we need to know in order to answer important research questions. Behavioral research in Music Therapy reverses these priorities, allowing the nature of research questions to be determined by more fundamental beliefs about what constitutes legitimate data.

This brings us to the second point about theories, regarding their ability to address important problems. One of the biggest deficiencies of traditional Music Therapy research has been the failure to articulate
1) criteria for deciding upon important problems, and
2) the nature of these problems themselves. Needless
to say, I am in sympathy with the view expressed by
Graham earlier that most research in Music Therapy has
been conducted because it can be done, not because it
needs to be done. We can only remedy this situation
when criteria for (1) and (2) are developed and agreed
upon.

One criterion for importance is that the results
of research be directly applicable to either treatment
or training. In Chapter I we looked at the state of
affairs in Music Therapy today regarding the
inapplicability of most research. Though researchers
working in the behavioral paradigm tend to see the
problem in terms of working out the mechanics of this
application, the problem is deeper than this. Since
the fundamental bases of traditional research are
inimicable to those of clinical treatment, it was
argued that the very focus of research must change in
order to be applicable. It is not that behavioral
research is too narrow, quantitative or overly
technical to be applied--rather it is too behavioral.

The view that treatment should in some way depend
or be derived from research was earlier shown to be an
inadequate one for Music Therapy. Since treatment
methods have developed faster than research techniques,
it would be reasonable to reverse this dependency and
derive research efforts from existent clinical paradigms. Researchers must recognize that clinicians possess a body of implicit knowledge, gained through the use of these clinical techniques, and focus research efforts on illuminating the nature of this knowledge. Such an effort would involve a multitude of tasks, such as the following: determining the type and number of processes engaged in by creative clinicians; discovering the relevance of theory adhered to in determining clinical interventions; evaluating different methods of training students in creative and improvisational techniques; determining if various creative techniques have core processes in common; and, discovering how various therapists obtain, conceptualize and work with the information they receive while engaged in clinical work. This is just a partial list and merely illustrates the scope and depth of the research paradigm being advocated here.

Since theories serve to connect empirical phenomena with the basic entities of which the world is constructed, it is incumbent that Music Therapists articulate—at least for purposes of research in this domain—their ontological context. This means providing answers for questions like: What does the world in which Music Therapy process occurs consist of? How can we best characterize a human activity that takes place within in art form and is oriented towards
transformation? What worldview best models the activity of Music Therapists and their clients?

It may seem presumptuous to include such questions as a part of scientific research. Yet any scientific paradigm works within such defined categories, though being of such a fundamental nature they often function on an implicit level, or, conversely, are defined by negation. In eighteenth century physics the connection between the physical theories and metaphysical ones was overt and intentional. This connection was not so explicit as regards twentieth century psychology and the advent of behaviorism. Here, the ontological commitment denying existence to mind and consciousness was implied by behaviorist methodology, though it remained for the development of Skinner's neo-behaviorism for the implicit connection to be pursued to its logical consequent.

It is no exaggeration--nor, I hope, by this point is it controversial--to say that scientific activity takes place within a larger world view whose validity must be assumed. It is only within such a context that the process and products of science become intelligible. It is clear that though the validity of the assumptions comprising such a world view is not demonstrable, the worldview still has empirical import, primarily observed in the manner in which it guides research efforts. Laudan\textsuperscript{23} demonstrates how we can
rationally chose between competing research traditions, though their epistemological bases contain no mutual overlap sufficient to generate mutually testable predictions.

To say that Music Therapists must articulate a worldview for the intelligibility of theoretical proposals is not to say that we must make extra-scientific commitments of a tenuous nature. It is to say that theories function in a larger context and validation of theories supports the chosen context. Examples of these larger contexts that have begun to be applied to Music Therapy process include field theory, systems theory and holism. The important consideration in formulating and applying these contexts is to do so in a way that preserves the integrity of the indigenous research effort.

One way, of course, is to utilize contexts that are derived from experience in Music Therapy process. All modern scientific themes of this sort have a source, whether it is ancient Greek philosophy, mystical insight or prior results from a laboratory. Since Music Therapy process represents a unique form of gaining knowledge it can function as a fertile ground for the development of such metaphysical contexts.

It also possible to apply constructs from the larger cultural milieu in a manner that honors indigenous concerns. The most important consideration
here is that the system so employed be evaluated not on
its own merits, but by how well it can model the
essential aspects of clinical process. This effort, of
necessity, must be a trial-and-error approach, with
theories and associated models being derived from these
larger systems in a manner by which their efficacy for
representing clinical process can be evaluated.

Retroductive Reasoning

In this effort, an important tool can be what
Hanson describes as "retroductive" reasoning.
Traditional accounts of science, such as Baconian and
positivistic ones, emphasize the importance of
inductive and deductive reasoning respectively. In
studying the logic of discovery, Hanson argues that
only retroductive arguments can lead to the novel ideas
and concepts characteristic of scientific progress. He
explicates the form of these arguments in the following
way:

1. Some surprising phenomenon P is observed.
2. P would be explicable as a matter of course if
   H were true.
3. Hence there is reason to think that H is
   true.

The inductivist holds that H emerges from repeated
instances of P. Logical positivist accounts "make P
emerge from some unaccounted-for creation of H as a
'higher-level hypothesis.'

Neither of these proposals embody a satisfactory explanation for the emergence of novel theories. Retroductive logic does provide such a mechanism because one is drawing an inference regarding what must be the case if an experience is to be intelligible. Thus, we "reason back" to the cause from the effect rather than deduce the effect from the cause.

Physicists use this form of reasoning when certain particle collisions seem to violate the law of conservation of energy. The existence of heretofore unobserved particles is postulated to account for the seeming energy loss. The presence of those particles are postulated whose existence would explain the observed results by keeping the results consonant with known laws, e.g., the conservation of energy. The outer planets of our solar system were also discovered in such a fashion. The orbits of known planets were seen to deviate from the patterns predicted by gravitational laws and thus the existence of unobserved planets was postulated to account for the seeming deviations. Notice, though, that the novel hypothesis H is neither deductively nor inductively implied by the data.

In Music Therapy we first must decide what our "surprising phenomenon" is. I have argued here the ability of clinical improvisation to facilitate
emotional development, provide solutions to emotional conflicts, and allow for contact with otherwise isolated individuals all represent good starting points. As Music Therapists it is possible that through daily contact we take the ability of music to serve these purposes for granted. Yet, we really have no good a priori reason for expecting such results from our clinical efforts.

Let us just consider one of these elements for now: that is the ability of participation in clinical improvisation to resolve emotional conflicts. Taking this as a given, we can then ask what might explain such an ability. One answer could be that the client's inner life becomes identified or merges with the musical expression. The various stages of conflict, tension and release seen in the music are the components of the client's inner life becoming manifest. As the music reaches an organic resolution, so does the psychological conflict. We also note that the therapist's contribution facilitates the processes of identification and resolution. To make this fact intelligible, we then must assume some mechanism by which the client's music conveys the nature of his inner conflict and suggests musical interventions to the therapist which will move the conflict into the musical realm, thus facilitating the working through of the conflict.
Of course all of these are speculations and I do not intend to support them further here. They are offered to demonstrate how the retroductive form of reasoning can be utilized to generate working hypotheses in Music Therapy. The hypotheses generated thusly do not have the certainty that either deductive or inductive reasoning provides, yet they have the advantage of being adequate to the nature of that which we want to explain. Additionally, assuming Hanson's argument is a cogent one, this form of reasoning is typical of discovery in science. Given that researching these types of processes is just beginning in Music Therapy, it would seem that the use of such methods is warranted by the incipient status of such investigations.

**Pragmatism in Music Therapy**

Since all of these proposals are resting on acceptance of some form of pragmatism, I would like to explore some issues associated with this doctrine that are relevant to this study. The pragmatist holds that "humans develop languages and concepts to help them cope with their experience and solve their problems."\(^{27}\) Science possesses no special claim to truth in this pragmatic perspective, but is one method of coming to terms with existence, on a par with other significant human endeavors. Additionally, the value of knowledge
is not determined by its conformity to nature, but instead finds its significance "in its guidance of action; knowledge is for the sake of doing."\textsuperscript{23}

In Music Therapy it is essential that we employ a version of this pragmatic philosophy in formulating a new research paradigm. This involves allowing the explanatory power of our theories to take precedence over other methodological commitments, focusing our research efforts in a way that will directly enhance clinical practice, and formulating criteria for explanation that is appropriate to clinical methods. While we will still maintain methodological standards, these considerations necessitate the abandonment of methodological dogma to allow these standards to evolve.

The reader will be correct if noting a seeming contradiction between the emphasis on adopting a pragmatic approach on one hand, and previous conclusions that have criticized traditional research advocates for employing just such a strategy. Early Music Therapists adopted first a standard medical model and then a behavioral model out of pragmatic concerns, and in this way invoked the very non-indigenous factors that have been criticized throughout this study. A fair question would pertain to the criteria by which the present author is now advocating for the very approach shown to be inadequate. Further, pragmatic
conceptions of knowledge that emphasize its usefulness are opposed to realist conceptions of knowledge that emphasize the degree to which our representations conform to an external reality or "truth." This also seems to contradict the realist position regarding the status of theories that was argued for previously.

While these contradictions cannot be completely resolved, two important considerations will mitigate their importance. Firstly, there is a sense in which all knowledge acquisition has a pragmatic aspect. Even those sub-atomic physicists working on the most esoteric problems seemingly removed from any application have pragmatic concerns. The results of any given experiment are not sought "for their own sake" but to validate a specific theory, justify the millions of dollars spent in the research program, or establish the professional credentials of the researcher. So the relevant consideration is not whether a particular paradigm will invoke pragmatic concerns, but which--or possibly, whose--pragmatic concerns should be invoked.

It was argued previously that the particular concerns invoked in traditional Music Therapy research did not relate to the essential questions of understanding clinical process, but to acquiring professional recognition and acceptance. While this choice can be justified as a rational one--given the
past status of Music Therapy in the health profession--it cannot be characterized as a legitimately scientific one. I propose that we can legitimately appeal to pragmatic concerns when the particular methodological component so described is oriented towards solving an important research problem. One can recognize the epistemological purity of a behaviorism which disallowed introspective reports, and still choose to abandon such purity when an important research problem--such as exploring the source of clinicians' creative interventions, the nature of musical meaning in the clinical milieu, etc.--can not be addressed under such constraints. In fact, the same consideration goes for any conceptual or philosophic position which the current study has articulated as being supportive of Music Therapy research.

By averring that Music Therapy research exists to serve clinical practice I am committing to a position that sanctions pragmatic research choices oriented towards illuminating the mechanisms of clinical process. A variation of the second objection noted above might be that it is possible that fictionalist accounts of clinical process--as I have characterized those resulting from the imposition of psychoanalytic or other personality models on musical process--could serve well enough in discussing Music Therapy process. One might pragmatically justify the assertion that
Music Therapy theory should provide a language to represent clinical process and that there is no necessity to discover the language. After all, to a pragmatist that knowledge is true which "works", not which necessarily conforms to an external reality. Here, my epistemological realism is still running up against my methodological pragmatism.

This is a necessary consequent of the absence of an absolute, formalizable scientific method, adherence to which could ensure the progression of knowledge. Since there is no single method, and methodological thinking constantly evolves, one can make a plausible argument that a pragmatic openness to evolving a method suited to the unique content of Music Therapy is actually a characteristically scientific approach. That is, genuine science is characterized by the primacy placed upon finding answers to important problems. That the nature of the answer sometimes changes our thinking about what legitimate "answers" consist of was seen in the above example of eighteenth century physics. To be a scientist means adopting a necessarily pragmatic approach regarding method.

Yet this does not necessitate that the knowledge thereby gained is any less true; it only would do so if truth was dependent on method. Seeing that adherence to method cannot provide the orientation towards truth in Music Therapy I have proposed an alternative, which
is that the theories we test be adequate to the nature of the participants' subjective experience while engaged in clinical practice. The intuitive, subjective, seemingly ineffable, core of clinical process is the truth of Music Therapy. This needs to be the beacon by which we orient our research methods. Approaching the essence of this truth is the most fruitful criteria we have to establish and maintain the progressive nature of Music Therapy research.

Though I would agree that it is inarguable that scientific activity consists of more than just various researchers attempting to seek communal validation of their private experience of the world, I nevertheless feel that the way in which it is different is not immediately apparent. All of the traditional arguments to support the public objectivity of science have been shown to be faulty in one or another ways. Since truth is not to be gained through an unvarying method, the appeal to experience would appear to be a reasonable alternative.

The Nature and Function of Models

What is needed is a sphere of discourse in which various Music Therapy researchers can present the nature of their clinical experience in a form which allow comparisons among researchers. The most productive form to do this in is through models of
clinical process. Models can capture some of the those qualities of music resistant to verbal and formal articulation. The model can pick up where words leave off.

The term "model" has been used very loosely--and I believe, incorrectly--in Music Therapy, and it is apparent that there is no agreement on how models should function. What follows is a brief introduction to the concept of a scientific model, with implications for the functions models can serve in the view of research being advocated herein.

Models provide a community of researchers "with preferred analogies or, when deeply held, with an ontology." The difference between these two extremes is an important one for this study. As a preferred analogy, a model is merely a calculating device; it is a convenient way of conceptualizing a phenomenon for certain limited, specific purposes. The model is not considered to represent the actual form or structure of the phenomenon under study. Here, the model has a status similar to that of theories in the fictionalist view.

As an ontological commitment a model is an actual representation of real phenomenon. The status of the structures articulated by the model here is analogous to that of theories in the realist view. Thus, in the "analogy" view, physicists can talk about light as both
a wave and particle phenomenon without contradicting themselves--each conception has its realm of application. In the "ontology" view, light really is one or the other (a particle or a wave, or perhaps some as yet unconceived of third type of entity) and the current inability to decide here represents a serious stumbling block for scientific progress.

In an indigenous research effort for Music Therapy it is essential that we utilize ontological criteria in determining the usefulness of our models of clinical process. In fact, the use of such criteria distinguish indigenous efforts from the alternatives. Analytic and behavioral models of these processes can adequately represent Music Therapy phenomena in the language and constructs of their respective systems. They may or may not organize and express phenomena in a manner relevant for Music Therapists (with the emphasis on 'Music'). These imported models must, first and foremost, be adequate to their imported conceptual context, not to the actual musical phenomena. The primary criterion for their evaluation is how well they account for the musical phenomenon within the constraints of their respective systems.

In contrast, what defines an indigenous effort is the degree to which the models employed therein represent the actual phenomena, independent of pre-existing theoretical constraints. Unless we endow our
models with ontological status—and evaluate them according to this criteria—we have undercut the rationale for the indigenous research paradigm. The rationale being that a paradigm so defined will enhance our knowledge of Music Therapy process by employing research methods that will bring us closer to the phenomena so described.

In addition to the two functions of models noted above, scientists avail themselves of two general types of models. One of these general types has been alternately described as "mathematical,"\textsuperscript{30} "formal"\textsuperscript{31} and "analytic."\textsuperscript{32} A model of this type functions primarily as a "semantic interpretation of a theory."\textsuperscript{33} The second type of model, and the one that is of interest here, is an \textit{iconic} model. This type of model is a "real or imagined thing, or process, which behaves similarly to some other thing or process, or in some other way than in its behaviour is similar to it."\textsuperscript{34} Model airplanes represent actual ones, mannequins can represent humans in training certain practices of first-aid, the physical constituents of the universe can be treated as billiard balls and atomic nuclei can be considered miniature solar systems—all of these are examples of iconic models.

Iconic models are representations of theories that go beyond the observable. As such, their use allows us to extend our knowledge of the world: "Our lack of
knowledge of the real mechanisms at work in nature is supplemented by our imagining something analogous to mechanisms we know, which could perhaps exist in nature and be responsible for the phenomena we observed.\textsuperscript{35} Reasoning by analogy is an important part of science.

In building iconic models there are two levels of description on which the analogous relation can hold. A model whose source and subject is identical is a homeomorph; one whose source and subject differ is a paramorph. A mannequin is a homeomorphic model because it is a model of a person and is also modelled on a person. The molecular model of gasses is paramorphic because while being a model of a gas, the molecules themselves are not modelled on gaseous substances but upon solid material particles.\textsuperscript{36} Paramorphic models tend to be reductive in nature because they explain processes in one domain based upon the properties of a more fundamental domain.

I would like to argue that Music Therapists have traditionally created models of a paramorphic nature, thus unwittingly implementing a form of reductionism. This has occurred because theorists have chosen flawed analogies that refer to processes that do not capture the essential elements of Music Therapy process. Bruscia\textsuperscript{37} for example, describes Music Therapy models as incorporating techniques, strategies, goals, theoretical foundations, philosophies of therapy and
assessment procedures in a comprehensive and consistent approach to therapy. Yet, these are models of clinical practice which I would like to distinguish from models of clinical process. They explain and provide rationales for the therapist's actions in the clinical milieu, yet propose no indigenously based mechanisms to represent Music Therapy process.

We have seen earlier that the components articulated by Bruscia--limited to formalized procedures, guidelines, rules and imported rationales--are peripheral to the actual locus of effect of Music Therapy process. These social and professional obligations and activities do not represent the actual vehicles of transformation--it is the as yet unrepresented processes in this latter area that new clinical models must endeavor to represent.

Behavioral "models" of Music Therapy are not true models because they are restricted from going beyond the data to suggest the workings of unobserved processes. Psychodynamic models do attempt to represent the inner workings of the individual, yet the analogy these types of models depend on--that of the human society--is inadequate for Music Therapy process. Just as a society consists of individual members working either cooperatively, in isolation or at cross-purposes, psychodynamic theories postulate the existence of semi-autonomous entities residing internal
to the individual, and whose relations are of a kind with those of societies. As was argued earlier this structural, reductive approach does not do justice to musical phenomena.

Both behavioral and psychodynamic strategies involve creating models of music but neither are modelled on music. Neither approach can do justice to the creative, intuitive and musical components of Music Therapy process because their explanatory assumptions require modelling the effect of music according to alien principles. The fact that these models are basically unmusical models is why I consider them to be paramorphic and also explains why their clinical relevance is limited.

Truly representative models of Music Therapy process must share the essential elements of the process: in this study emphasis has been placed on the creative, poetic, expressive and transformative aspects of clinical process. Though a complete model would have to represent all of these aspects (and possibly more), the acquisition of such a global model, if it is even possible, is not imminent. The prime value of the present discussion is not to insist that valid models represent all of these elements simultaneously, but to provide a taxonomy of the various levels of description relevant to Music Therapy process. Individual clinician/researchers may choose to focus on one or
more of these elements, thus adopting a more fruitful research strategy than tackling the enormity of clinical process within one model. Though one goal of physics is to devise a unified field theory accounting for all of the forces of the physical world, physicists apportion their labors and work on single aspects of individual forces. As individual Music Therapists we should focus on those aspects containing the highest degree of personal meaning, thereby creating a complete portrait of clinical process through the sum of our communal efforts.

One characteristic that will be indispensable to productive models is that they be accommodative in nature. By this I mean that they must be open and flexible models, capable of adaptation to novel conceptualizations and empirical discoveries. Evolutionary models that can represent process rather than structure will be better able to accomplish this. If I am correct in asserting that homeomorphic models will better represent Music Therapy process, it is essential that we build into our models all the attributes of the process which define it as such.

Music has important dimensions that become manifest in both space and time and models in Music Therapy can be built from either one of these perspectives. As a process, it is obvious that music and Music Therapy exists through time. The essence of
music as a model of time\textsuperscript{38} is inextricably connected to
certain of the therapeutic potentials of Music Therapy.
These include the ability of music to represent growth
and development, to recall memories and trauma, as well
as to establish a sense of "sacred time" and a
concomitant return to one's primeval origins.\textsuperscript{39}

Similarly, music exists in space, permeating the
area in which it is audible with sound waves. Musical
interaction can create a sense of "sacred space" and
allow for the suspension of normal taboos and create
new therapeutic possibilities. Music can connect two
(or more) individuals through the empty space between
them and alleviate feelings of loneliness and isolation
through the contact it affords. Hence, it is not an
exaggeration to observe that Music Therapy process can
facilitate the transcendence of the normal boundaries
of space and time.

Though it is acknowledged that Music Therapy
process exists in both of these realms, one cannot
escape the inference that, as process, its
manifestations in time are probably more primary than
those in space. Indeed, there is a sense in which
progressive scientific thinking has evolved from static
and geometric themes, to active, chronological ones.
As was noted in the discussion of evolutionary and
compositional theories this has occurred in the fields
of geology, biological development and stellar
evolution to name a few. Even Galileo progressed from an (incorrect) distanced-defined notion of acceleration to one which defined acceleration in terms of time.\textsuperscript{40} In modern physics this trend has continued to where the classical notion of particles is changing to one where "such evanescent entities are far more appropriately called 'events' than 'particles' or 'corpuscles.'"\textsuperscript{41} And mass "is no longer seen as measuring a fundamental substance but rather as a form of energy, that is, as measuring activity or process."\textsuperscript{42} In all of these examples we can see the shift from an emphasis on structure to one on process as representing an increasingly progressive explanatory perspective.

Music Therapy iconic models that are spatially based must simultaneously allow for a process component. This is necessitated by the requirement that the structures articulated by such models be capable of representing development and transformation. Since traditional spatial models that delineate structures, such as maps, tend to be static in nature, we must look towards other types of representations. Two that have been suggested as being suitable for Music Therapy process include systems and fields.\textsuperscript{43}

Systems thinking is distinguished from earlier models by the emphasis placed on the primacy of process. In the mechanistic view, process is what occurs when structures interact through prescribed
forces and mechanisms. This relationship is reversed in a systems perspective and "every structure is seen as the manifestation of an underlying process." Though focusing on spatial structure, this approach is recommended for Music Therapy because structure is evaluated in terms of, and determined by, process, rather than reverse.

The concept of a field also breaks away from the restrictions on spatial properties characteristic of mechanistic science. A field does not exist in space, as a particle resides in a void, but is more properly considered a property of space. The field is defined not by its structure, but through its effects on entities in the domain subject to its influence. Fields were postulated in science to explain magnetic, gravitational and other phenomena that seemed to result from action at a distance, a scientifically proscribed model of interaction. Insofar as music can establish a realm of interaction between separate, autonomous individuals in a therapy session it is functioning as field. It is this characteristic which recommends models based upon field thinking to Music Therapy.

In suggesting guidelines for model construction the view of science assumed here is one that does not acknowledge either the presence of shared rules or a formalized structure in unifying a scientific domain. Music Therapy is also a specialized mode of gaining
knowledge whose unity is also not reducible to these very elements. Consequently, attempts to explicate Music Therapy process in terms of universal structures, procedures, guidelines or rules for practice will be mistaken. The elements of veridical models must be drawn elsewhere. Hopefully, we can create flexible and expansive models that are closer to the creative process itself than is allowed by non-indigenous concepts, and through our models complement the strict denotative meanings of a traditionally "scientific" vocabulary.

Experimental Research: A Critique

The advocates of experimental methods in Music Therapy research have traditionally appealed to the "shared rules" view of science that has been criticized thus far. Once we abandon this view of science we must evaluate experimental research based upon indigenous concerns rather than uncritically accept its application based solely upon tradition.

Certain designs and methods will lend themselves more to the type of research that is needed in Music Therapy, just as others are built on premises that restrict their current applications. As with theoretical orientation, some may argue for a diversity of approaches rather than relying on specific methods or designs exclusively. Nevertheless, all methods and
designs are not created equal—physicists tend not to use questionnaires and anthropologists generally engage in field research. The nature of each of their respective domains renders certain methods more appropriate. Once we commit ourselves to a view of the natural domain of Music Therapy process certain conclusions regarding methodology should be drawn, and I do not believe that avoiding such recommendations in the name of methodological pluralism has any scientific merit.

Experimental methods are often seen as representing the ideal in science. Certainly Music Therapy theorists have traditionally advocated for enhanced quantitativness and control in research—hallmarks of experimental research—and have equated empirical knowledge with experimental knowledge. Yet there are severe problems with applying experimental methods in Music Therapy research—problems that are not merely practical but that penetrate to the core assumptions of experimental research.

The primary goal of experimental research is to establish bona fide cause-effect relationships through isolating variables. Two of the primary assumptions here are that 1) results are due to the manipulations of the researcher and, 2) third variables over which the researcher has no control are evenly distributed among experimental groups, and are not affected by the
experimental manipulations. Thus, in testing a particular educational method one would ensure that various experimental groups have similar mean I.Q. ratings, so that the results of the experiment could be attributed to the variable of interest, the particular teaching strategy, and not a third, uncontrolled for variable such as intelligence.

In applying experimental research to Music Therapy we must examine whether these assumptions are warranted. I would like to do this in the context of discussing a recent study in the literature entitled "The effects of music therapy on the self-esteem of emotionally-disturbed adolescents."

Since this study actually overcame many of the limitations and practical problems inherent in the application of quantitative, experimental research applied to Music Therapy, the subsequent analysis is not intended to criticize the specifics of this study, but instead uses its procedure to highlight the deficiencies of experimental methods in general, particularly regarding assumptions (1) and (2) stated above.

Briefly, in this study a planned course of either music therapy or verbal therapy was administered to two experimental groups with the intent of comparing the change in pre-test and post-test administrations of a self-esteem inventory. Results showed no significant difference either between groups or from pre-test to
post-test scores.

The first problem here involves the assumption that the label "emotionally-disturbed adolescent" accurately defines a class of individuals that share a property that is relevant for their reaction to Music Therapy. I have previously argued that this assumption is not warranted because such a clinical category does not delineate a singular syndrome, and that Music Therapy treatment is not directly focused on the causes or manifestations of such a syndrome, but instead involves the inner person coping with the difficulties caused by or exacerbated by the clinical condition.

In order to make such a study of legitimate scientific interest there must be some factor which leads us to believe that emotionally-disturbed adolescents are enough alike to warrant treating them as an homogenous class for purposes of their reactions to Music Therapy. That a study investigating the effect of Music Therapy on red-headed individuals would be of limited scientific interest is obvious. Though it is not as obvious, I am arguing that treating "emotionally disturbed adolescents"--or autistic children, or "the blind," or "schizophrenics" for that matter--as a scientifically relevant class is of no greater value. We will find just as great a personality distribution--and just as broad a use of Music Therapy--in these latter classes as in the "red-
headed" class. Because of the psychiatric terminology used in taxonomic structures of pathology, however, we talk as if "autism" or "schizophrenia" articulated categories with a similar epistemological status as "measles" or "malaria" but this is in fact, not the case. Classification is a necessary component of science and I am not arguing against this activity. What I am saying is that factors that we build our classification schemes upon must be relevant to, and correlated with, some natural orders.

Though this study obtained no significant difference between music and verbal therapy in enhancing self-esteem, I would like to assume, for the moment, that there was a significant difference in favor of the Music Therapy group. Even with this positive outcome we would still not be warranted in attributing this to the efficacy of Music Therapy. In this study the researcher conducted both therapy groups. Any significant difference could just as well be attributed to the fact that the researcher was a better Music Therapist than verbal therapist. And we cannot get around this problem by having equally competent therapists from both modalities because then any obtained difference could be attributed to the difference in therapist and not treatment method.

The problem here is that Music Therapy does not have a generic structure like aspirin or psychoactive
medication. If the therapeutic relationship—formed through music—is a crucial component of Music Therapy process, and if each human relationship is necessarily unique, one cannot apply and test "Music Therapy" apart from the interpersonal context in which the process emerges. Mainly, because there is no impersonal entity called "Music Therapy process" that exists apart from each of its unique manifestations. Thus, there are serious problems with outcome studies of Music Therapy via experimental methods.

Next I would like to look at the assumption that random assignment can ensure that the effects of extrinsic, third variables are cancelled out. In this study, the administration of a pre-test ensured that there was no pre-existing difference in levels of self-esteem between the two experimental groups that would confound the results. Yet this is not the only variable—or even the most important one—to control. The most important determinant of the efficacy of Music Therapy is the subject/client's motivation.66 Highly motivated clients will make better use of the possibilities of therapy which would, presumably, be reflected in the scores of the personality inventory. To have the bare minimum of experimental control necessary to draw meaningful conclusions, subjects within and between each experimental group would have to possess equal predispositions towards the two
modalities being tested. Balancing for motivation is the only way to truly test the therapeutic intervention.

Assuming that such measures and tests can be devised and accurately employed we are still left with a big problem. That is that the results of the study so constructed would only be applicable to clients who possessed the pre-requisite equivalent levels of motivation! In other words, in balancing the motivation factor in our experimental study we are limiting the implications of the study to individuals with equivalent amounts of motivation towards each modality, presumably a quite narrow group. It would not even make sense to apply the conclusions of this study to individuals with unequal levels of motivation. What would be the rationale in studying the effect of Music Therapy on individuals predisposed towards verbal therapy, or vice versa?

The experimenter in this study observed that while "none of the subjects in the music therapy group was disappointed or complained about his or her treatment . . . all but one of the subjects in the verbal therapy group were disappointed about their placement."47 Since these feelings "affected the subjects' motivation within their respective groups"48 it was suggested that a follow-up study employ "matched treatment groups from various locations, so that the groups' motivation will
not be affected by the difference in the treatment modality."49

What is remarkable here is that the ability of Music Therapy to increase client motivation is not looked at as a salient aspect or an important advantage of Music Therapy treatment, but as a confounding variable to be controlled! But this is a typical state of affairs in experimental designs that demand control and equalization of all factors relevant to treatment outcome. Since individual differences must be minimized to allow for the establishment of cause/effect relationships the effect of these differences cannot be studied.

Yet it might be that these individual differences are the salient features in determining the efficacy of the particular therapy modality. It was stated earlier that all of our musical histories are unique and hence our reactions to and use of music has a necessarily individual character. In testing "Music Therapy" as a mode of treatment, one is assuming that one is administering the same treatment to each individual in a study. When medical treatments are considered we can make this assumption because most of us are alike enough physiologically (barring allergic reactions) so that the chemical structure of the medicine is responded to in a similar manner by different bodies.
With music, however, we cannot make this assumption. In addition to the relationship factors noted above, how music is perceived is crucial to the effect of Music Therapy, and this perception is guided by each of our personal histories which is necessarily unique. None of us have lived the same lives. The salient aspect of the music is not determined by the physical characteristics of the tone but by the interaction between the physical tone, its expressive quality and our necessarily unique perception of this music. All members of a Music Therapy group, though exposed to the same physical stimulus, are not therefore exposed to the same treatment.

Even if we can create groups balanced on relevant variables, we will still not have equivalent groups because each individual's history is different. Two individuals may achieve identical MMPI profiles, or identical self-esteem scores, but this does not mean that they are the same persons, or even that they are similar enough for studying the effect of Music Therapy. It is the way in which they are different, their individual personality that will determine their use of Music Therapy process, not the way in which they are similar.

Since there is no consensus on which therapeutic elements must be present to define a specific treatment as a valid form of Music Therapy we have no criteria
for determining the validity or generalizability of any given treatment study. As an example, in the study currently under discussion the researcher planned a sequence of Music Therapy activities on a weekly basis so that the verbal and music groups would have similar experiences. Now it may be that for the true potential of Music Therapy to be accessed it is essential that the therapist suggest activities and musical structures spontaneously, according to the in-the-moment needs of the group members. If this is so, then the study under question is not examining the effects of Music Therapy proper, but is instead testing some form of "musical therapy" that shares certain incidental properties with Music Therapy.

The point here is that we cannot perform any kind of outcome research with Music Therapy until we have discovered, explored and defined its essential characteristics. Even medical research articulates the etiological, structural and transmissional factors relevant to a certain disease before advancing to testing and comparing medications. We cannot "apply" Music Therapy until we have some idea as to the working mechanisms of its underlying process(es), thereby gleaning those factors essential to considering a given method or technique a bona fide form of Music Therapy.

The fact is that nature, or the real world, is not like the laboratory in that "physical conditions are
rarely held constant whilst certain factors are allowed to vary for the benefit of the well-placed observer."^50 The strict view of causality characteristic of experimental research is only possible because of the design of the experiment. A view of causality derived from experimental research is not necessarily relevant for the world existing apart from such research.

My suggestion for Music Therapy is that rather than ignore or try to control the factors that intrude on experimental research but are actually the salient elements of practice—such as the effect of the therapeutic relationship, client motivation, the variably expressive nature of the music, etc.—we make these components the objects of study and abandon the requirements of controlled experimental research. What is the rationale for "controlling for" the effect of these factors when they are the very elements of Music Therapy process that endow it with therapeutic potential? We can adopt the trappings of medical research but this does not guarantee that we will acquire similarly useful knowledge. In fact, this strategy can actually be considered damaging as it diverts resources away from studies with more potential use for the field.
Elements of Clinically Relevant Research

It is possible that some may construe the preceding remarks as a criticism of quantitative research, though this would be mistaken. Even though I do believe that the precepts of qualitative research are more consonant with the exigencies demanded by the clinical practice of Music Therapy and incipient efforts in this new paradigm emphasize qualitative methods, it is more important how one thinks of the relevant data—phenomenal experience, feelings, images, behaviors, formal aspects of music—than how one treats the data.

One can treat behavior qualitatively as well as quantify aspects of empirical experience, and my criticism of behavioral research should be not considered a criticism of quantitative methods in general. In fact, the reliability of measures of phenomenological variables is comparable to those of behavioral variables. Rogers' use of such variables has led him to conclude that "well-selected phenomenological variables may even be more likely than behavioral variables to exhibit . . . [predictive] potency."51 The manner in which data is treated should be determined by the purposes of each particular study; the manner in which it is conceptualized should be determined by the inherent structure of the phenomenon.

The type of research methods we do use should be
similarly determined by our methods of clinical practice. For example, in the study discussed above if it was the case that the therapist normally did not plan a rigidly adhered to sequence of therapy activities weeks in advance, there is no reason to design a research study with such a strategy. In an effort to make the two treatment forms as equivalent as possible, this researcher removed from the Music Therapy groups the very quality that defines it as therapy: that is the ability to offer spontaneously conceived experiences based upon the clients' present needs.

In fact, it would not be hard to imagine that being given the opportunity to select their own activities--and thus establish a sense of mastery and control over their social environment--will directly enhance the self-esteem of the clients so treated. The demands of experimental research may have actually led the researcher to treatment decisions that worked counter to the treatment goals she wished to assess.

The process oriented aspect of establishing a therapeutic relationship is possibly the element of clinical practice that is most difficult to transfer to research settings. It was previously argued that the nature of this relationship is the context from which the meaning of the events in a Music Therapy session are derived. If true, then we must be wary about
applying research results exploring the mechanisms of creative clinical practice that do not account for the extended, inter-personal matrix evolving between client and therapist, to long-term work that emphasizes the importance of the therapeutic relationship. Such a relationship is built through time as mutual trust, knowledge and awareness develops. Examining, say, one clinical improvisation session created for the purpose of a research study will not allow for the study of those clinically salient factors that only emerge over an extended period of time.

Yet this is not to say that a study so designed is not worth conducting. Certainly every therapeutic relationship has a first meeting, which some might say is of crucial importance to the subsequent course of therapy. Additionally, the factors operative in short-term therapy might be similar to those in a long-term relationship. If one wishes to investigate the types of processes engaged in by clinicians in generating their clinical music, it is possible that the short-term milieu might offer a less complex field of study. Since the information provided by personal experience with the client is so impoverished here the therapist must be relatively more dependent upon the intuitive and general processes common to work with all clients. Since there is less concrete information for the therapist to appeal to one might argue that the
creative process itself will then be less obscured. An important component of empirical study of creative clinical practice should be to examine whether or not the processes in long- and short-term therapy are similar, and how the time-limited nature of brief therapy affects creative processes. What is of greatest importance here is not to assume that studies not accounting for the effect of the therapeutic relationship are unqualifiedly applicable to actual clinical practice.

Similarly, it has been argued here that each clinical encounter and relationship involves the formulation of a novel and idiosyncratic language. Research into the nature of this aspect of Music Therapy process must itself consist of a creative and flexible use of language. New research methods must then have as a goal the very search for a language in which to report findings and not assume a prior language. In this way, the creative aspect of Music Therapy process involving the establishment of a mutually-understood language between therapist and client will be modelled in the research design and can be addressed by the research effort.

I do not mean that we necessarily have to invent new words to describe Music Therapy process, though this type of invention is typical of scientific research and should not be resisted if it indeed
embodies the best solution to the problem of describing Music Therapy process within indigenous guidelines. What is equally possible is that we adapt existing terms, possibly modifying their meaning, to more accurately represent our clinical experience. The formal language of music theory (including terms like dissonance, pulse and rhythm) has some uses applied to clinical music, though it is being argued here that we need to supplement such a vocabulary to describe the unique characteristics and properties of music created through clinical Music Therapy techniques. Dance therapists have taken a lead here through the establishment of movement notation systems employing specialized categories like "effort-shape" and "tension flow" to describe bodily tensions and energy patterns with therapeutic import.\textsuperscript{52}

Productive directions for future Music Therapy research would be to 1) discover the vocabulary clients and therapists use to report their clinical experiences when freed of prior constraints upon the form of such reporting, and 2) establish new terms and categories based upon such efforts and discover whether an empirically-derived vocabulary can be used with an adequate degree of inter-subject reliability. We also need to determine empirically whether the loss in specificity implied in using a more poetic/expressive language in clinical reporting is outweighed by the
ability to more accurately represent the data of interest. In fact, it may be that a specifically denotative account of a Music Therapy session actually represents a greater distortion of clinical processes than does a poetic account attempting to convey meanings in a more connotative manner. If the experience of the participants is a vague, amorphous, ineffable one then language which can convey the sense of this experience more accurate represents its nature than does language that distorts through its precision.

It may, however, be the case that the efforts described in (2) will actually lead to a relatively precise language able to convey meaning via strictly denotative criteria. Possibly we are led to vague and general descriptions of clinical process because we lack the precise vocabulary that can accurately convey the nature of the experience, not because the experience is inherently structured in this manner. In either case, what links these two efforts is the realization that we must maintain a flexible approach to descriptions of clinical process and make the vehicle of such descriptions the actual focus of our research efforts. It is not unreasonable to expect that Music Therapy process--emerging, as it does, from combining a highly specialized training with a unique intent to use the creation of music as a vehicle for human development--leads to unique human experiences
demanding novel representations when converted to
discoursive forms of representation such as verbal
language.

I would like to conclude this section by re-
emphasizing that I am not arguing against the value of
basic research, nor am I attempting to narrowly define
applied research as the only worthwhile contribution to
the profession of Music Therapy. What I am saying is
that research divorced from meaningful ends—whether
applied or basic—is not really research at all. All
bona fide research is research in respect to a
meaningful and important question with implications
that go beyond the domain of any one experiment. It is
not disparaging to say that the meaning of a particular
study lies outside the study itself—it is to identify
the source of the study's significance. Until Music
Therapy researchers begin to formulate their research
problems in respect to prominent clinical methods, and
abandon those methodological strictures preventing such
formulations, they will find their work denigrated—or
worse, ignored—for its lack of applicability.

Since Music Therapy treatment itself contains
research it is sensible—indeed necessary—that
research be guided by accomplished clinicians. Only
such clinicians can have the intuitive grasp of the
essential elements of Music Therapy process upon which
the light of systematic research should be directed.
Notes


2. Dudley Shapere, Scientific Theories and Their Domains, In Frederick Suppe, (Ed.), *The Structure of Scientific Theories*.


4. Thomas Kuhn, Second Thoughts on Paradigms, In Frederick Suppe, (Ed.), *The Structure of Scientific Theories*.


8. Ibid., 28.

9. Whom I am citing as someone generally seen as sympathetic to elevated conceptions of music as a bonafide representative form. My purpose here is to show that even someone with strong sympathies in this area does not go far enough in admitting the epistemological implications of musical interaction and expression.


11. Even though mathematics is a discursive form, and one can literally translate equations into verbal language merely by saying or writing them, my point is that one cannot literally describe in words the entity represented by the mathematical equation.


17. Laudan 93.


20. Laudan 60.

21. Laudan 79.

22. Laudan 79.

23. Laudan, *Progress*.


25. Ibid., 86.

26. Ibid., 86.


34. Harré 174.

35. Harré 175.
36. Harré.


40. Hanson, *Patterns*.


43. See Carolyn Kenny *Music and The Field* for extended discussions of applications to Music Therapy.

44. Capra 147.


47. Haines 88.

48. Haines 89.

49. Haines 89.

50. Hanson 68.


CHAPTER IX
CLOSING REMARKS

When the idea for this study was first conceived I envisioned providing a neat and thorough conceptual alternative to research done under positivistic constraints. I was not sure what such a paradigm would look like, yet I had an intuition that its foundation lay in recent Kuhnian and post-Kuhnian philosophy of science. Eventually I realized that much of what this philosophy (and meta-theoretical analyses such as Mackenzie's) had to say led me to the viewpoint that advocating for any content-free methodology was a mistake. To do what I originally envisioned would be to repeat the mistakes of logical positivist philosophy and behavioral methodology.

That is why there is no clear method emerging from this study. All research tools must find their value in how well they work, not based upon their conceptual roots. What I do hope the reader does take from this study is more important than a specific recipe for conducting research; it is an attitude towards this research that I feel has been too often lacking in Music Therapy. I see the essential elements of this attitude as follows:
1. An awareness that research exists to further practice and thus enhance our clients' lives as well as our own.

2. A respect for the creative aspects of science and a sense of the spirit of discovery at the core of scientific activity.

3. An awareness of science as an inherently human activity whose purpose is to meet human needs.

In terms of (1), too often researchers have subscribed to a "trickle down" theory of the benefits of pure research. That is, all researchers have to be concerned with is doing research that meets methodological criteria and eventually the benefits will "trickle down" to clinical work. The attitude here is that "Science" (with a capital 'S'!) operates under rigid methodological constraints and clinicians should adapt their practice to reap the benefits of the researcher's "tough-mindedness."

In contrast, the view I have been promoting is that the application of research must be built into the design from the very beginning. Since application is the primary justification for undertaking the research to begin with, we should adapt our research methods to serve those clinical techniques that have demonstrated their value, rather than reverse this hierarchy. I have tried to support this claim (which after all is a value judgment on my part) with evidence that the
methodological constraints that researchers traditionally appeal to can no better guarantee the discovery of "truth" than can the contrasting strategies necessitated by clinically-relevant research. Thus, the researcher's claim to be in possession of a superior epistemological foundation is seen as baseless.

Regarding (2), I believe that research activity should be exciting, stimulating and even enjoyable. As Maslow has said: "Science at its highest level is ultimately the organization of, the systematic pursuit of, and the enjoyment of wonder, awe and mystery."¹ Too often I find no sense of these qualities in the Music Therapy research literature. Studies done merely to meet academic obligations or to gain professional prestige are apparent by their implementation of tired and irrelevant designs that no other researchers wish to replicate or build upon. It is essential that Music Therapy theorists capture and build upon the sense of excitement that is both a part of true discovery and a component of Music Therapy treatment. Perhaps the following remarks of Edward Tolman—an eminent early behaviorist of all things!—written shortly before his death can be inspiring here:

Since all the sciences, and especially psychology, are still immersed in such tremendous realms of the uncertain and the unknown, the best that any individual scientist, especially any psychologist, can do seems to be to follow his own gleam and bent, however inadequate they may be. In fact, I suppose that actually this is what we all do. In
the end, the only sure criterion is to have fun. And I have had fun.²

I feel compelled to state (3) because science is often portrayed as a dry, mechanical activity that functions according to strict rules of logic, and whose products represent the only valid type of knowledge. Needless to say I disagree with all of these claims. In reality, science--like art and spiritual discipline--is a human creation that represents one way for human beings to come to terms with their existence. Rational inquiry has no generic, formalizable structure, naive views of science notwithstanding.

Since science is a human artifact we are free to adjust it according to the needs of a specific discipline at different points in its development. That methodological constraints in science are the result of conventions, and not a priori necessity, seems inarguable; this does not mean, though, that such conventions are therefore arbitrary--they should be guided by the indigenous needs of the particular discipline and the communal wisdom of the discipline's members. Research in Music Therapy will be what we make of it; its form will be dictated by the intellectual judgments of those participating in its construction, not by alien procedural rules.

Here I would also like to address the often polemical tone that this study has assumed. I regret that some readers may have found this alienating. My
purpose was not merely to champion a specific viewpoint but to create a portrait of creative clinical practice, and draw, in as consistent a manner as possible, the subsequent implications for research. Of course, actual empirical research will involve compromises with the absolute recommendations contained herein. Yet I feel that it was important to set out the research ideal in this study, and allow the necessary compromises to result from practical concerns.

I have found that the personal values that guide my work as a Music Therapist have played an important role in my arguing for a clinically-relevant research paradigm that respects and honors the humanity of both client and therapist. Science is a value laden activity—the only question relates to whose values will be implemented. To deny the importance of values and hide behind claims of neutrality or pure objectivity is to deny the purpose of engaging in research, and hence, the researcher's own humanity. It is also to obscure evidence which is relevant to the rational evaluation of a given method or theoretical approach.

One of the reasons for the creation of an alternative Music Therapy association in the United States in 1970 involved the timing of the clinical internship. Those who were dissatisfied with the then current arrangement felt the need to establish a
training program which contained the internship as an integral component, thus fostering the development of a clinically-informed education for Music Therapists. It is equally important that the research component of Music Therapy maintain this connection to clinical practice. If this study can play a small part in this process of integration it will have achieved my purpose. If not, I hope that it is not the purpose that has been judged to be misguided, but merely the means by which I have chosen to pursue it, and that subsequent efforts can be built upon the successes and failures of this one.
Notes


BIBLIOGRAPHY


