C.S. PEIRCE, MECHANICALISM, AND MUSIC

by

ARTHUR FRANKLIN STEWART, B.A., M.M., M.M.

A DISSERTATION

IN

FINE ARTS

Submitted to the Graduate Faculty
of Texas Tech University in
Partial Fulfillment of
the Requirements for
the Degree of

DOCTOR OF PHILOSOPHY

Approved

Accepted

Dean of the Graduate School

December, 1987

CONTENTS

ABSTRACT

C.S. Peirce, Mechanicalism, and Music is an interdisciplinary study which principally involves ideas drawn from music and philosophy. These ideas, as explored, propose useful interconnections between these areas of study. As the main point of philosophical reference, this examination uses aspects from the system of the eminent nineteenth-century American mathematician, scientist, and founder of philosophical pragmatism, Charles Sanders Peirce (1839-1914). Using the method of pragmatism as an overall guide, the present essay proposes the construction of a conceptually developed investigatory instrument to examine and test certain implications of the mechanical and anti-mechanical schools of thought found in the phenomenology of music, and to present from this examination a line of reasoning useful to the musical artist who seeks a rational choice between these schools.

Writings by the pianists/teachers Walter Gieseking (1895-1956) and Artur Schnabel (1882-1951) are among the sources employed to illustrate aspects of the mechanical and anti-mechanical positions in music. Extra-musical materials are used to form structural models of these two

musical positions, models by which the examinations and tests of these positions are conducted. These materials include items from the subjects of formal logic, mathematics, mathematical logic, and machine computability. Among the selected figures whose efforts illustrate these subjects are, respectively, Alan Marquand (1853-1924), David Hilbert (1862-1943), and Alan Turing (1912-1954). Published and unpublished materials by Peirce are used throughout.

ILLUSTRATIONS

PREFACE

A version of the scientific style of reference, otherwise known as the author/date system, is used in this essay. There are, however, two general exceptions to this usage. Thus, the reference figure *CP* abbreviates Hartshorne, Weiss, and Burks, 1935, 1938, 1958, *Collected Papers of Charles Sanders Peirce*, while *NEM* abbreviates Eisele 1976, *The New Elements of Mathematics by Charles S. Peirce*. In the scholarly apparatus will also be found entries using a bracketed, upper-case "P" followed by a number. These entries reference items of Peirce's authorship listed in Ketner with Stewart 1986, *A Comprehensive Bibliography of Charles Sanders Peirce*. Elsewhere will additionally be found cross-references to specific notes in this essay. The figure "I*n*1" refers, then, to the first note of chapter one. Remarks throughout in square brackets are my own.

The Philosophy Department of Harvard University has generously granted permission for me to cite from numerous unpublished Peirce manuscripts. These citations are referenced according to the dates and manuscript numbers given in Robin 1967, *Annotated Catalogue of the Papers of Charles S. Peirce*, and the pagination established by the Institute for Studies in Pragmaticism of Texas Tech University. The figure "Peirce 1907/MS 319: 07" references, then, the seventh sheet of his manuscript number 319, dated 1907. This Institute collection has also provided a fine work environment which includes the full complement of its 80,000-odd copied sheets of these manuscripts, voluminous quantities of secondary materials including the Max H. Fisch Papers and the Preston H. Tuttle Collection, and the opportunity to test the ideas of this essay against those of several visiting scholars.

CHAPTER I

AN OVERVIEW

> In mathematics the art of asking questions is more valuable than solving problems.
>
> Georg Cantor (1845-1918)
> -the title of his doctoral dissertation

This essay principally involves ideas drawn from the disciplines of music and philosophy, ideas which when adequately explored may provide useful interconnections between these areas of study. More specifically, I will examine two apparently conflicting schools of thought to be found in music performance and pedagogy from a philosophically oriented position. As the main point of philosophical reference, this examination will use aspects from the system of the eminent nineteenth-century American mathematician, scientist, and founder of philosophical pragmatism, Charles Sanders Peirce (1839-1914).

One may well encounter problems when utilizing the languages of two fields of study in an interdisciplinary undertaking such as this. Such problems are prominent in this project, a main concern of which is with the principal business of pragmatism: "finding out the meanings of hard words and hard concepts" by means of "the method of experiment of the physical sciences" (Peirce 1907/MS 319: 07). The philosophical and other extra-musical terminologies employed will usually be explained as they occur. In cases where such brevity will not suffice, more thorough explications will appear.

To sum up this project, then, I propose to construct, using the method of Peirce's pragmaticism[1] and examples drawn from logic and mathematics, an observational, investigatory instrument to examine and test some implications of the mechanical and anti-mechanical schools of thought found in the "phenomenology" of music (the study of the phenomena, or experiential, occurring events in the musical world), and to present from this examination a line of reasoning which would tend to compel a musical artist to at least reconsider the underpinnings of these schools and perhaps make a rational choice between them. I am particularly concerned here with looking into the formal properties of these mechanical and anti-mechanical outlooks. By "the musical world" I mean something quite old-fashioned and easily identified: the activities of Western classical music as taught, learned, performed, and here limited to the category of traditional piano literature. My examination of these issues will proceed according to the elements of which this essay itself is made: evidence drawn from various sources, devised explanatory hypotheses tested against such evidence, comparisons announced in the course of such testing, and the forthcoming conclusions which may or may not confirm a given hypothesis. This project, then, takes for its overall basis the general method of experimental science as approximately parallel to Peirce's pragmaticism. I shall make use of this important doctrine of American philosophy in developing an understanding of what role "experiment" has for the musical artist.

What are these two demonstrably incompatible schools of musical thought, and why is it appropriate to apply extra-musical procedures to them? I term these aspects of thought and conduct in the musical world as the "mechanical" (as in machine-like, exactingly formulaic, deterministic, predestinational) and "anti-mechanical" outlooks. We shall consider these outlooks in their extreme, paradigmatic aspects. Extra-musical ideas will be brought to bear upon them because within the discipline of music in and of _itself_ there does not appear to be a logically reasoned technique or sufficiently developed investigative apparatus for comparing or choosing between them. Roughly and summarily stated for now, the mechanical view holds that the outcome of a given activity can be completely, precisely, and _repeatably_ determined in advance, according to the elements with which the activity begins, and the finite procedure or definite or fixed method by which the activity is executed. The anti-mechanicalist, on the contrary, holds that the outcome of an activity cannot be so determined and insured. Put another way, the format of mechanicalism disdains experimental procedures in the phenomenology of music while the anti-mechanical outlook embraces them. The application of certain extra-musical considerations, such as structural models, provides for clearer understandings and comparisons of these outlooks in music, and demonstrates their *extreme* forms, at least, to be incompatible if not outright contradictory.

Such analogical analyses or, stated more strongly and preferably, analyses of structurally similar models, can be carried still further, allowing for a logically defensible choice between these viewpoints instead of expressions of mere personal preference. This is an issue of considerable practical importance. As the activities of the musical world, as enumerated above, are conducted, a position is being taken on this question of mechanicalism and anti-mechanicalism *whether one acknowledges the decision or not*. Personal tastes, or "likes and dislikes" alone, as bases for decisions between mechanical and anti-mechanical musical programs, seem to be the only decision tool with which classical musicians have thus far been equipped. Many persons active in the musical world I describe have struggled with this dilemma, arriving at a seemingly irresolvable doubt: how can we choose between mechanical and anti-mechanical modes of conduct only on the basis of what we ourselves or someone else finds personally congenial? This type of doubt can be at least more clearly understood and even, perhaps, resolved, I propose, by applying certain extra-musical considerations to it. We will find pragmaticism intimately involved in this process. The clear understanding and handling of this type of doubt has rather serious practical consequences indeed. Formats for learning music, performance of various works, stylistic practices, and musical interests of all varieties including, we should especially note, the sustaining of actual physical and psychic injuries, and the conduct of whole professional careers, are directly influenced by how this doubt is managed.

The pathology of the mechanical/anti-mechanical conflict is clear in both theory and practice, and its onset and symptoms, again, whether consciously acknowledged or not, are ubiquitous. A recent article in *The Piano Quarterly* by Claire Wachter and Dean Kramer[2] illustrates how belief in musical mechanicalism can work itself out in practical terms. They describe the thwarted mechanicalistic ambitions of so-called "piano animals," frequent inhabitants of the musical world:

> Their delusions about the world extend to their own careers, as well. Entering the music school or conservatory as teenagers, they assume that their first international competition prize will lure a NEW YORK MANAGER to their door [automatically and mechanically], contract in hand. Meanwhile, they bide their time, practicing incessantly and chalking up a few local competition wins. Sometimes, for amusement, they saunter into piano class to sneer at their classmates. On occasion they condescend to perform a piece to show the others how it's <u>supposed</u> to go. (Wachter and Kramer 1986: 50)

This adoption of the credo that there <u>must</u> be some completely, precisely, and repeatably determined recipe for the promotion of a career, if only one could find and decide in favor of the <u>right</u> recipe, is but one specific aspect of the more general structure of musical mechanicalism. This general structure is <u>itself</u> a recipe or a recipe <u>of</u> recipes, which can be paraphrased as something like: <u>without</u> <u>fail</u>, right materials subjected to right routines will yield right results. Drawing from the extract above, we can also observe in action the belief that the right competitor who exercises the right recipes, in this case the recipes <u>for</u> and <u>of</u> winning an international competition prize, will automatically attain a concert career. And should this general recipe not work out as planned, should the competition prize and automatic career not be forthcoming, the competitor who has endorsed and bought into the general recipe of mechanicalism to a sufficient degree can apparently resolve the doubt about that recipe that will surely arise only by adopting additional beliefs which can be stated as "I just didn't practice hard enough," or "I made the wrong choice of repertoire for the concerto round of the contest," or "I did not play well because of stage fright: I just stupidly got nervous and failed," and the like. However, even in such a high-stakes situation as just described, the integrity of the mechanical approach, both as general recipe and as specific subsidiary recipes, is _itself_ almost never called into question. Its postulated assurances are not doubted; the individual's ability to exercise and fully carry out the routine, however, _is_ typically viewed with suspicion. But such a competitor, as true believer in the mechanical philosophy, never doubts the alleged impeccability of the recipe-system itself.

An additional, contemporary illustration of this type of allegiance to the mechanical system is telling. It concerns the issue of stage fright and how modern pharmacology is seen as providing a panacea for this problem. Frank Wilson, M.D.,[3] recently commented in _The Piano Quarterly_ on the use of the drug Inderal for the control of such nervousness:

> Risk taking has always been part of performance; indeed, performers have traditionally regarded the opportunity to work with live audiences as a major career incentive. The uncertainties inherent in live performance have been welcome because they have contributed to that special aura of spontaneity that excites

> audiences. That was in the good old days. The facts of life in the performance career have changed in several ways recently; two such changes have had a profoundly negative impact on this idealistic model of live performance. First, the sheer number of hopeful professional musicians has grown so large that it is no longer realistic even for students entering leading conservatories to expect that they will be able to support themselves in performance careers. Second, the musical tastes of audiences (and to a certain extent, critics) seem to require virtuosic, or at least flawless, performance, especially by younger artists. If young artists are expected to play programs of extreme technical difficulty, especially when they are trying to break into the big leagues, it should not surprise us when they find performance impossible to approach with equanimity. It should surprise us less that they might welcome a pill with power to tame the psychic demons stalking them on stage. (Wilson 1986: 31)

However one may judge the idea of using pharmaceuticals to control one's stage nerves, the use of Inderal can be seen as yet another mechanicalistic belief put into practical action: another attempted means of bolstering or rationalizing the recipe-system. That is, it can be seen as another aspect of the general position that seeks mechanically insured means of controlling or insuring the production of musical right results.

The status of such recipes, or rationalized beliefs after such a recipe's failure, is described by Dorothy Taubman[4] in a recent interview in The Piano Quarterly. She observes that such recipes in the musical world can amount to "pet theories," there being about as many such potentially contradictory theories as there are persons willing to theorize. Without using the term "pragmaticism" itself, she clearly has both its definition as the experimental method of science (see In1, above), and its application as an observational, investigatory instrument, in mind:

> Many professions have developed a body of knowledge. They work within the framework of established paradigms--sets of generally accepted principles and strategies. By contrast ours is a hearsay tradition carried down the generations by word-of-mouth from teachers to students. We engage in very little research and have no experimental tradition. . . . In his book, Great Pianists, Harold Schonberg writes that there has always been disagreement amongst musicians (many of them composers) on how to play the instrument. Some have recommended balancing the hands towards the thumb, others towards the fifth finger. Some have said use a straight finger and some a rounded finger. Schonberg continues, "and glorious disagreement and confusion reigns to this day." This disagreement has resulted in some of our finest artists being no longer able to play. We cannot afford the luxury of every pianist and composer coming up with his own pet theories. . . . Adhering to tradition has been the downfall of many performers. . . . We are in the twentieth century, a time when man has made extraordinary progress in many fields. It has not happened in our profession. We should be using science to help come up with the answers, otherwise we will continue to produce crippled pianists. . . . I said earlier that tradition must be questioned, especially in our field where there is ample evidence that the old methods may not be working. But also if we seek to develop an experimental tradition then strategies . . .cannot be imposed on students. . . . The pianist must question any approach that does not seem to work. It is poor pedagogy and bad science to encourage students to follow blindly. (Wolff 1986: 25, 32)

Now whether or not Taubman is in fact only *feigning* an experimental posture, by way of eventually claiming to have experimentally "discovered" that her own pet theories are actually the really correct ones, I am not at all sure. Nevertheless, at least one important idea is expressed in her remarks. That is, the variety of questioning she admonishes implies a sought resolution of the doubt I have described, whether the desired result which is in conflict with this doubt be to insure completely a concert career via the international competition route, chemically to control stage fright, to announce immutable stylistic conclusions, to produce a repeatably determined means of teaching, learning, or performing, or to generate or adopt other such hoped-for certainties. It is arguable that the mere lip-service so often paid to anti-mechanicalism serves but to foster this doubt in pernicious ways. That is, this doubt remains unresolved when the cloaking of mechanicalist practice in experimental-sounding language persists.

Let me be clear in stating, generally, that I am not going to engage in admonitions on how "properly" to interpret a given passage of musical score, recommend specific procedures for "right" technical manipulations of the piano, or the like. Neither will the enumeration or exposition of examples bearing on any given issue, musical or otherwise, presume to be utterly exhaustive. The examples used have been carefully selected to illustrate issues rather than to exhaust them. The aim of this essay is to get at and elaborate the logically antecedent and, practically speaking, more far-reaching issue of clarifying these mechanical and anti-mechanical programs in music according to their formal properties, and to suggest a possible means for choosing between them in a logically and scientifically defensible manner.

In large outline, then, this piece will proceed by examining mechanical and anti-mechanical positions of the musical world and a number of extra-musical, structural models of the positions (especially models of the mechanical position), while investigating three general explanatory hypotheses that link and illustrate, in an interdisciplinary way, these musical and extra-musical elements.

In examining the two positions in music, I shall use as the main points of departure Leimer-Gieseking 1932 and Schnabel 1942. Other musical sources will be used to further illustrate the Gieseking versus Schnabel paradigms. The first general explanatory hypothesis is meanwhile taken up and considered as the following hypothetical question: "Is the one form of musical thought so structurally like the extra-musical forms that we can be warranted to term that form of musical thought 'mechanicalistic,' and furthermore analyze that aspect of the musical world in terms of extra-musical models?"

Again, clearer meanings of these terms in music will be developed by getting more precise ideas of what mechanicalism and anti-mechanicalism amount to in fields traditionally viewed as apart from or outside of music. Particularly important is the relation between the examples of musical mechanicalism to be given and Peirce's characterization of a "fixed method."

Observations on the nature of so-called "logical," "computing," or "reasoning" machines suggest themselves in regard to such considerations of fixed methods and extra-musical mechanicalism. That being the case, expositions on aspects of the work of Charles Peirce, the nineteenth-century figure Alan Marquand (a student and colleague of Peirce), several of Peirce and Marquand's predecessors, and our own century's Alan Turing will be given, using Ketner with Stewart 1984 and 1987 as bases. This section is designed to introduce, in an historical, detailed and extensive way, some important properties of extra-musical mechanicalism and its fixed methods that will illuminate implications of the first general hypothesis. Important in this section are various illustrations and examples of what is known as the logical relations of classes, and how (and to just what extent) problems in this area could be worked out according to what are termed "algorithms." Among these illustrations and examples are the formal characteristics of the categorical syllogism, how such characteristics can be expressed in either algebraic or graphical diagrammatic forms, how the workings of such forms came to be mechanicalized, and the nature and limits of such algorithmic, mechanical reasoning as exemplified in a logic machine proposed by Peirce and Marquand. The detailing in this section of the depth and actual "hands-on" knowledge Peirce had of logic machines and mechanical reasoning should render more meaningful and significant those remarks of his (given throughout this essay) on reasoning and the mechanical philosophy. Also important in this regard are considerations of such terms such as "algorithm," "finitistic," "deterministic," and "computability." The overall historical sweep of this section is also intended to convey something of the general atmosphere of extra-musical mechanicalism during the nineteenth and early-twentieth centuries, the aspirations and enthusiasms this outlook fostered, and something of the general limits the mechanical philosophy encountered. This section also provides the groundwork for the pending analysis of the apex of sophisticated extra-musical mechanicalism for the present essay, the attempted exhaustive formalism of the important nineteenth and early-twentieth century German mathematician, David Hilbert.

Using the meanings of mechanicalism and anti-mechanicalism derived to that point, we shall consider a second general hypothesis designed to test them against the concept of creativity. Not surprisingly, mechanicalism and creativity are found to be in opposition to one another, with anti-mechanicalism and creativity being mutually supporting ideas. The use of the term "creativity" will rely on the contributions in Dutton and Krausz 1981 that establish a minimum standard for creativity which agrees with our commonsensical usage: creativity involves not the finite reproduction or fixed replication of something, but rather the production of something which is in some sense novel or new.[5] The question here can be stated as, "What can be the relation of 'creativity' to mechanical and anti-mechanical viewpoints?" The extra-musical model of these viewpoints employed here is the supposed exhaustive mathematical formalism of Hilbert. To phrase the matter briefly, if thoroughgoing, exhaustive mechanicalism should ever have fulfilled its promise, it should have done so in Hilbert's formalism within mathematics. Investigation of this model proceeds, again, in an historical, detailed, and extensive manner to illustrate how Turing damaged that hoped-for formalism by demonstrating an inherent contradiction between it and creativity.

The third hypothesis asks: "What role has creativity in the activities of the musical world?" The investigation of this issue will be approached through the question: "What can be the subject matter of music?" The point here will be that classical music, <u>at least in part</u>, has as its subject matter the modeling or analogizing of emotions and emotional relations. Peirce provided an insight here that most musicians, I think, would endorse with hardly a second thought: " . . .the meaning of a piece of music is the play of feeling it produces" (Peirce 1907/MS 319: 09). Similar remarks can be found in Collingwood 1938, Langer 1957, and elsewhere. Thus the question becomes, "Does the musical artist's instantiations of emotions and emotional relations undergo change and produce something new, or do these items remain static and thus admit of fixed replication?" I take it as obvious that the emotional subject matter of music does indeed constantly undergo transformation. With these ideas accounted for, the main issue reveals itself as whether mechanicalism or anti-mechanicalism could best proceed to manage a subject matter which is not static, but changing. The choice, it seems clear, could be in favor of anti-mechanicalism, creativity, and the experimental approach. That is, if to be a musician implies being creative, then the musician is compelled to disdain mechanicalism and embrace anti-mechanicalism.

In sum, then:

1. Mechanicalism and anti-mechanicalism in the musical world can be more clearly analyzed and understood by comparatively applying models and meanings of these terms from outside music *per se*.

2. Creativity is a defining characteristic of the anti-mechanical view.

3. The musical artist, in dealing with a subject matter in flux, is required to adopt a creative approach and, by extension, the anti-mechanical viewpoint.

Additionally, the perhaps odd-seeming point, that anti-mechanical activities in the musical world can be seen as types of actual reasoning processes, will be defended. Peirce is rightly famous for a certain development in the history of logic called the "logic of relatives." Again, in the name of illustration, we do not need to undertake a complete explication of this subject. Nevertheless, certain characteristics of it, as structural models, can be brought to bear on the issue of the processes of musical reasoning and the case for music, at least in part, as an anti-mechanical phenomenon.

In the traditional forms of logic prior to the development of the logic of relatives, the standard forms of reasoning were considered to be "closed," as it were. That is, in these traditional forms, from a given number of premisses a <u>single</u> correct conclusion was expected. This section, when presented, will recall the earlier expositions on the categorical syllogism, Marquand and Turing. The logic of relatives, however, is something different. Here,

> . . .from any proposition whatever, without a second, an endless series of necessary consequences can be deduced; and it very frequently happens that a number of distinct lines of inference may be taken, none leading into another. . . . [Even] in minor points the doctrines of ordinary logic are so constantly modified or reversed that it is no exaggeration to say that deductive logic is completely metamorphosed by the study of relatives. (Peirce 1902)

Does this not sound quite like what the musician, truly understood, does with his single "proposition" of a musical score? The mechanicalist, of course, thinks that from a given musical score the <u>one</u> correct "conclusion" can be deduced by a repeatable routine of transformations. This is the outlook illustrated, for example, in those musical coaching sessions with master teachers wherein the student is informed as to "<u>the</u>" meaning of a musical work, "<u>the</u>" means of learning it, "<u>the</u>" correct manner of performing it, and so forth. But the anti-mechanicalist, the creative person or pragmaticist, sees the legitimacy of "an endless series of necessary consequences," and conducts affairs accordingly. In the course of using Peirce's logic of relatives as a structural model of musical activities, I shall make use of his distinction between "corollarial" and "theorematic" forms of deductive reasoning. Thus will a case be made that activities of music, properly understood, seem to follow Peirce's account of the processes of reasoning.

Notes

1. One characterization of this term, by Peirce, was the following: "The method of pragmatism is simply the experimental method, which (taking the word 'experiment' in its widest sense, so as to make it applicable to cases in which the fulfillment of the conditions has to be waited for instead of being artificially produced) is the invariable procedure of all successful science. Thomas Beddoes showed, as early as 1792, that it is the procedure even of mathematics." (Peirce 1907/MS 320: 29)

2. Wachter is listed, in the biographical matter concluding the article, as having been most recently an Instructor of Piano at the University of Oregon at Eugene. She holds degrees from Peabody Conservatory and the University of Texas at Austin, and has studied with Carlo Zecchi and John Perry. Kramer, a frequent contributor to The Piano Quarterly, is an Assistant Professor of Piano and Music History at the University of Oregon.

3. Wilson is the Chief of the Department of Neurology of the Kaiser Medical Center in Walnut Creek, CA, and a figure of prominence in the now-current music and medicine controversy.

Inderal is the trade name for the chemical compound propranolol; its function is to inhibit certain bodily responses to excitement of the sympathetic nervous system. At the time of Wilson's writing, neither the manufacturer of the compound nor the Federal Drug Administration had licensed its prescription specifically for the treatment of stage fright.

4. Taubman founded and directs the Dorothy Taubman School of Piano at Amherst College.

5. See therein, especially, Larry Briskman's "Creative Product and Creative Process in Science and Art." Briskman, of the University of Edinburgh's Department of Philosophy, also makes the point that considering the subject of creativity may now have become an actual necessity rather than a potentially entertaining curiosity.

CHAPTER II

WALTER GIESEKING AND ARTUR SCHNABEL: DIRECT EVIDENCE FROM THE MUSICAL WORLD

> Whence and how they come I know not; nor can I force them.
>
> Wolfgang Amadeus Mozart
> -on his best musical ideas

Walter Gieseking (1895-1956) was one of the truly influential pianists in the history of that profession. His biographers in The New Grove Dictionary of Music and Musicians (Sadie 1980) inform us of several items that contributed to his development and reputation. He seemed to have had no regular training as a musician until his sixteenth year, at which time he undertook studies with Karl Leimer at the Hanover Conservatory.

> Within a year he made his first public appearance, and at the age of 20 he played in Hanover a virtually complete cycle of Beethoven's sonatas in six recitals. (Sadie 1980/7: 364)

To perform even a fraction of the Beethoven piano sonatas at such an age seems incredible, much less to have delivered a virtually complete cycle of them. He is rightly remembered for his performances of works from the Impressionist school of composition, namely items by

Claude Debussy and Maurice Ravel. In this regard, his main legacy was his recordings of the complete works of these two composers, recordings "which set standards that have proved impossible to surpass" (Sadie 1980/7: 365). He seems also to have commanded equally strong respect as a teacher, attracting former students of such distinguished figures as Claudio Arrau, Alfred Cortot, Edwin Fischer, Wilhelm Kempff, Rosina Lhevinne, and Marguerite Long[1] to his Saarbruecken, Germany, master class from 1947 until his death. It is to his remarks in The Shortest Way to Pianistic Perfection of 1932[2] that we shall now turn.

Generally speaking, the larger view of the mechanical outlook includes three principal characteristics. It claims, both first and second, that with a duly circumscribed set of initial elements or conditions and an appropriately devised, finite method of manipulating these elements that, thirdly, the execution of this method will produce an outcome which is completely, precisely, necessarily, and therefore repeatably determined in advance. Put another way, with the right materials and method, the right end result will be insured. Taking the title of Gieseking's book as a point of departure, this general description of mechanicalism can be narrowed for an application to the musical universe. Hence, according to this view, if only we select the proper types of students, teachers, and performers, and rigorously apply Gieseking's method to their affairs, we shall produce examples of pianistic perfection. The title of his book may strike one as claiming an exhaustiveness that seems strange, but, nevertheless, an examination of the text reveals this choice of heading not to have been a slip of the pen.

What, then, can be such pianistic perfection, and what characterizes the methods and materials which produce it? Gieseking wrote quite clearly in at least the latter two regards. Concerning the sort of students or, more generally, beginning or initial elements suitable to his task, he stated:

> The following instructions are not intended for beginners, only for pianists who already have had experience as concert pianists or music teachers, or far-developed, serious-working dilettantes. (Gieseking 1932: 10)

This puts some rather clear restrictions upon who is qualified to take up the Gieseking method. Paradoxically, it seems, among those to benefit from this system would be, presumedly, "The young musician [who] almost never understands how difficult it is to play really correctly" (Gieseking 1932: 06). Of course any teacher hopes for the best students available and may even, insofar as this is possible, actually seek them out. However, Gieseking's qualifications for the student went, in the actual implementation of his method, well beyond the commonplace desire for agile learners. He used the forthcoming endorsements of his procedures by such carefully qualified students as proof of the legitimacy of his method. This is as if the President of the United States, having carefully selected and installed all the members of the Senate and House of Representatives, and having drafted, submitted, and seen unanimously approved certain pieces of legislation, should proclaim the unanimity of mind in the legislative branch as proof of his wisdom as embodied in the bills passed. The third chapter of _Pianistic Perfection_, entitled "Natural Interpretation," closes with this remark:

> I, myself, have found that there are not so many disputable interpretations, and that the opinions of musicians with a natural sense of feeling do not vary so greatly. I have nearly always succeeded in convincing my pupils of the way in which they should render the pieces they were studying. When they played them according to my instructions, they did so with so much warmth and intensity of feeling that I had the impression "they feel the same as I do." It was not their verbal agreement that convinced me but their musical interpretaion. With the aid of numerous examples, I have caused my pupils to learn the relative strength of a tone in relation to other tones and have found them agreeing with what I felt to be correct. I look upon this as a proof of the accuracy of my interpretation. (Gieseking 1932: 45)

It is important to note here that what is put forward to be learned is "_the_ way in which they _should_ render the pieces they are studying" (emphasis added) instead of _a_ way in which they _might_ so behave. Much the same sentiment was expressed by the influential piano pedagogue Tobias Matthay[3], an important figure from the generation just preceding Gieseking:

> A rational scheme of Education . . .would consist: in analysing the subject to be taught; analysing also the successful doings of successful artists; thence deducing the laws and rules that govern successful performance; and then _directly_ communicating such laws of procedure to the pupil, instead of leaving him to discover them for himself. . . . The rules . . .form (or should form) rules of procedure at the moment acceptable to the Majority. . . .
>
> Evidently, teaching, as applied to Science, Harmony, or Language, does not here signify, that the discovery of the implicated Laws shall be left to each individual learner. (Matthay 1903: 02)

Both Gieseking and Matthay, then, insist on a singularity of interpretive product and a uniformity of student behavior. One effect of these expectations would be, it seems, the eventual cessation of artistic inquiry: right musical results can be expected and supposedly generated. That is, one goal of the mechanical outlook is to reach what can be described as absolutely final interpretive conclusions. This belief in the singularity of interpretive product has a linchpin-like prominence for the mechanical view. According to Gieseking, " . . . there are not so many disputable interpretations," while on Matthay's account the governing rules for such conclusions are the rules of a majority of participants. The overall relation between such sought-after final conclusions and the nature of the proper type of student, a relation we shall encounter again and again in future analyses, is one of potential circularity and self-referentialism. In the system of Gieseking, for example, we observe that the proper type of student, the "right" kind of initial material with which to commence a mechanical process, is convinced <u>by the teacher</u> that only certain interpretive conclusions are correct. When thus duly convinced, the student's unanimity of mind with Gieseking concerning these provided conclusions is viewed as proof in favor of his interpretive point of view or, more accurately, his personal dogma.

What has this sort of student to do with analyses of mechanicalism in its larger view? The limitations on membership, in what we can call the Gieseking Consensus, to concert players, music teachers, or what he terms far-developed, serious-working dilettantes[4] clearly satisfy the requirement of the duly circumscribed set of initial elements or conditions by which I described the first or initial characteristic of a mechanicalistic point of view. Again, if we additionally consider these aspects of the Gieseking student apart from the larger view as such, we can also observe how this characterization of the student illustrates a byproduct of mechanical procedures, a closed or self-referential system. That is, a self-referential system is displayed if, as I suspect, the likelihood of a student's conformability to the Consensus is disingenuously figured in with the actually announced prerequisites for instruction. The closed, self-referential and potentially circular character of this situation is then easily summarized by a hypothetical teacher of the Gieseking or Matthay schools: "I shall endorse as students only those who, among other characteristics, seem highly likely to agree with the propositions I will put forward to them. Then, when they have enthusiastically admitted the truth of my prophecy and I have found them agreeing with what I felt to be correct, I shall take these enthusiasms and agreements as proof of the propositions I advanced."

This type of system is self-referential and circular in design, because no point of validation or invalidation exists for activity beyond the attributes of the system itself, which, in this case, are the personal criteria of the teacher. In this situation, apparently, any direct evidence from the musical materials themselves would have to take second place to the dogma of the teacher. The ordinarily unvoiced maxim of such a teacher includes having already settled upon the one, correct manner of playing a given piece and also the one, reliable type of student that is desired and will be tolerated. This type of teacher also counts these items as non-negotiable: any variant form of playing would plainly be described as wrong, and any other than the approved type of student behavior would also be plainly wrong. Additionally, this type of system or set of relations looks like a closed one, because the singularity of product and consistent endorsements of students do not admit of any new developments or the generation of new information. The use of "new" here denotes processes and other facts not already contained in the system itself when it was set in motion. That is, neither the natures of the product nor the student are susceptible to modifications suggested by unanticipated observations. In this system, the attributes of the indisputable interpretations and governing rules are certainly not going to change. Our hypothetical mechanicalistic teacher might actually say something like: "Why should I modify my interpretation when I know, based at least in part on the unanimity of opinion amongst my students, that <u>I've got it right</u>?" Again, by reading not too terribly deeply between the lines, we can discern that one of the primary credentials the student should have to be accepted for instruction is a propensity to join the Consensus.

The reader of the present essay may protest that the student in this system has indeed done something new, by having learned new musical works. My reply is that, according to the preconditions observed that elucidate the nature of the student as initial material for the mechanicalistic activities at hand, the student has not learned very much, if anything, about music _per se_. He or she, however, does seem to have absorbed quite a bit indeed about how to parrot an unchanging and preconceived, albeit highly intricate, mechanical apparatus that purports to produce pianistic perfection.

Even at this preliminary level of getting an idea as to the student's role in the larger scheme of things, we already find a fairly complete enumeration of the characteristics attributible to that larger mechanical scheme. First, we observe a well-defined set of initial elements or conditions including the correct interpretation sought and a certain credulity, if you will, on the part of the student. Secondly, we see announced an as of yet rather unclear method of manipulating these elements that involves overt convincing of students. Finally, we are presented with proof of the accuracy of the dictated interpretation, said proof produced from the singularity of interpretation and endorsements of the student that were built into the system from the very beginning. In being thus a closed system, this "proof" is repeatably determined in advance: it cannot vary.

It is precisely this sort of closed system using admonitory phrases like "_only_ in this manner" and "_must_ be this way" that discloses a mechanicalistic point of view. Of course, materials and methods of invariant constitutions seem absolutely appropriate if a truly perfect product is to be expected and its repeatable production is to be insured. For the larger view of mechanicalism, now, we must look into the nature of this method which produces this singular interpretation, and later try to discern just what this "pianistic perfection" is.

What are the characteristics of this procedure or method? Broadly viewed, as we know, it will take the beginning materials described and effect pianistic perfection in a manner necessarily and repeatably determined in advance. It functions, we might observe, as a type of insured recipe. It thereby prescribes a set of required activities and a routine for their execution. These activities include items as specific as which fingers to apply to certain piano keys. The specified routine is drawn from the stock of Gieseking's convictions and expressed through his system of training.

A number of first things or initial conditions are set out, the requirement being that these be undertaken prior to all others and managed according to what Gieseking described as "severe self-control." These include, for instance, the teaching and acquisition of self-auditing powers for the student. The teacher, thus:

> . . .educates the pupil at first to self-control; he shows the pupil how to hear himself. This critical self-hearing is, in my opinion, by far the most important factor in all of music study! Playing for hours without concentrating the thoughts and the ear on each note of the certain study in hand is wasted time! Only trained ears are capable of noticing the fine inexactitudes and unevennesses, the eliminating of which is necessary to a perfect technique. Also, through a continuous self-hearing, the sense for tone beauty and for finest tone shadings can be trained to such a degree that the student will be enabled to play the piano with an irreproachable technique and with a feeling for the sound-beautiful. Really accurately rhythmical playing can be achieved only through severe self-control. (Gieseking 1932: 05)

Thus, an irreproachable technique can be had only with such trained ears and severe self-control. Again, the use of "only" and the implied "must" alert us that a mechanical point of view is being expressed.

On the subject of learning an Etude or study exercise from the Instruction Book of Lebert and Stark, we are informed that "The first thing we have to do is to visualize the note-picture, so that the exercise can be written from memory" (Gieseking 1932: 14). The note-picture is the actual musical score. When elaborating this issue using the first two-part Invention of Bach, we are told that subsequent to this process of visualization the student "can concentrate upon how to learn touch by pressure of the keys" (Gieseking 1932: 26). Touch refers to how one actually manipulates the piano keys with the fingers. We are also told which ingredients and procedures are required or, alternately, forbidden for learning this physical aspect:

> It is necessary to play the invention very slowly, so as to be able to prepare the touching of each tone and to control every movement. . . . It is not advisable to play the whole invention straight through, when studying. In fact, this should be forbidden. Only small parts should be practiced at a time; and these should be repeated over and over again, so that irregularities and unevennesses may be immediately corrected. (Gieseking 1932: 26, emphasis added)

Returning to the Lebert and Stark study, we find these sorts of specific items and procedures laid out in even greater detail. Here we find that " . . .the fingers one (thumb) and five . . .must be kept motionless" and that they are "to be lifted about two inches above the keyboard" (Gieseking 1932: 19). A reader may protest that I have taken his descriptions of what incidentally happens in these matters and twisted them to appear like prescriptions of what actually must be done. This possibility is removed, especially in this very instance about motionless fingers and their distance from the keys, when it is announced that "these rules are to be taken literally, and they must be carried out to the letter" (Gieseking 1932: 19). Further evidence that these types of remarks are indeed prescriptive is found in the highly detailed practice routine established for study of the Lebert and Stark item during the first four days of work on it, and in the requirement that students "in the beginning of their studies should take a lesson daily and later on at least three times a week" (Gieseking 1932: 21). Also, Matthay's prescriptive method, his deduced laws and rules mentioned above, has, in his instructions about foot-position, what might strike us as an almost comically painful parallel to Gieseking's motionless and aptly heightened fingers:

> The position of the feet should be such, that the weight of the leg can rest upon the ground upon the pedal.
>
> The right foot should always be thus in contact with its pedal; the left foot, when not required for the *una corda* pedal, is best placed further back, with the sole of the foot only touching the ground, and with its toe almost as far back as the heel of the right foot--when the latter is engaged upon its pedal. (Matthay 1911: 305)

We indeed do find, then, that the methods of Gieseking and Matthay here highlighted count as integral a specified number of activities and routines denoting a mechanical outlook. I have not, by any means, provided an exhaustive exposition of the evidence in this matter.[5] However, we still are driven to the conclusion that this Gieseking method is indeed a mechanical one. The student must first perform visualization, may only subsequently look into the aspect of touch and must practice towards this end very slowly with study-playing of the whole work being forbidden, must account for certain physical dispositions in very precise ways, and so forth. Above all, "Rigid concentration *guarantees* control of the fingers and leads to success" (Gieseking 1932: 53, emphasis added). When this guaranteed or insured, repeatable, and finite recipe is secured we are apparently on the verge of pianistic perfection, or so it would seem. We may now inquire directly as to the nature of this third aspect, the right end product, of Gieseking's musical mechanicalism.

If we expect from Gieseking a completely detailed morphology of this pianistic perfection beyond the demand for right notes, we shall be disappointed. This lacuna in his exposition seems strange, for if we should apply the deterministic method described to the initial materials proscribed it only seems reasonable to expect a fairly extensive description of the product produced. Lacking this, what we do have are some pointers as to conditions under which this product would arise and, derivatively, a clear description of how the type of knowledge operative in these mechanicalistic affairs would function.

On the subject of Leimer's instruction, Gieseking announced:

> . . .I still am an unconditional partisan of this Leimer method, which I consider <u>the best</u> and most rational kind to bring pianistic possibilities to their highest state of perfection. (Gieseking 1932: Ø7, emphasis added)

This method, or course, includes the aspects of musical mechanicalism we have looked into thus far. Again, the specifics of this highest state of perfection remain unclear. The issue of how the composer's markings should relate to subsequent realizations of them is quickly dispatched:

> Only the most careful following of all his markings makes it possible to live in the thought and emotional world of a master and thereby to realize a perfect rendition of his works. (Gieseking 1932: Ø8)[6]

Once again, an ingredient or pointer for the process is at least somewhat delineated, while the subsequent ingredients of such a perfect rendition are left unclear. Unsurprisingly, of course, we are informed that,

> The manner of practicing here presented trains the fingers in a wonderful way, helps the pupil to gain command over them, and eventually leads to perfection in execution. (Gieseking 1932: 26)

This type of demand from Gieseking for right notes represents at least <u>one</u> specific characteristic of perfection, a right end product, in his system. The functioning of the actual <u>type of knowledge</u> at large in these matters, however, is an issue that can introduce us to what other musical figures of prominence have considered as such right end products, or conclusions of a mechanicalistic routine.

The variety of knowledge Gieseking described and relied upon is clearly a deterministic one:

> Inaccuracy and uncertainty in rhythm and

> dynamic feeling become _so firmly fixed in the brain_, that as has been already pointed out, they can be mended only with great loss of time, if ever. . . .
>
> When a part of a composition has been played for the first time, a picture of the same becomes _imprinted on the brain_. This picture varies in clearness according to the mental constitution of the pupil. In general, a very faint impression is left on the memory, similar to a photograph which is not clear or has been under-exposed. Through constant repetition the picture becomes more and more distinct and finally resembles a clear, sharp photograph.
>
> The mistakes made, when playing, again cause a picture to appear in the brain, which, however, being faulty, needs correction. This is very often a most difficult and wearisome business; and faults, especially in regard to rhythm, and acquired through incorrect practicing, can be _eradicated only by great effort_. For a pupil, therefore, who wishes to make quick progress, it is of the greatest importance to avoid mistakes, _from the very beginning_. (Gieseking 1932: 44, 47, emphases added)

These apparently invariant relations between subject matter and learning, where items become firmly fixed and imprinted on the brain, constitute a point of view on knowledge not unlike that of the seventeenth-century British empiricist John Locke (1632-1704). Locke held that the mind, when acquiring knowledge, functioned much like a type of blank tablet which passively receives information that, indeed, becomes "imprinted" on it. In his _Essay Concerning Human Understanding_, he characterized this view of the mind as a _tabula rasa_:

> Let us then suppose the mind to be, as we say, white paper, void of all characters, without any ideas; how comes it to be

> furnished? Whence comes it by that vast store, which the busy and boundless fancy of man has painted on it with an almost endless variety? (Calkins 1957: Bk. II, Ch. I; Sec. 2)

A century and a half after Locke's _Essay_, H.T. Riley gave the term _tabula rasa_ the following definition:

> _Tabula rasa_: "A smoothed" or "planed tablet." This expression is used by metaphysicians to indicate the state of the human mind before it has received any impressions. The ancients used tablets covered with wax, on which they wrote with an iron instrument called a _stylus_, one end of which was broad and flat, for obliterating what had been written by smoothing the wax. Hence the expression. (Riley 1856: 451)

Perhaps the clearest definition of this term, for present purposes, was contributed by C.S. Peirce to the _Century Dictionary_ in 1889:

> Tabula rasa: an erased table or tablet. That is, a wax tablet from which the writing has been erased; hence, a blank surface, or one without inscription or impression: in philosophy used by the Lockians to express their notion of the mind at birth, implying that the nature of the ideas which afterward arise are determined purely from the nature of the objects experienced, and depend in no degree upon the nature of the mind. (Peirce 1889: 6151)

By using Locke's characterization of the knowing mind as a blank tablet then, we can arrive at a clearer understanding of Gieseking's mechanical outlook. As a _tabula rasa_, the mind functions passively, receiving necessarily via a deterministic method the information that will come forth later as pianistic perfection. As such, the mind has this information imprinted on it.

Should any of this information prove faulty, should the wax or white paper of the mental tablet have received or had painted on it information that needs correction, the adjustments necessary can be made only by great effort, if ever. In Matthay's version:

> If correct habits are not at once formed, at least _thrice_ the time will have to be spent. There is the time wasted in fixing the wrong habit; then the time required to weaken that wrong habit to the point of effacement; then, at last, the time needed to form the correct mental-muscular connections.
>
> How requisite, therefore, that these laws--the Elements of Pianoforte playing, should be thoroughly understood! (Matthay 1911: 3_n_)

The Gieseking and Matthay accounts of the musical mind are quite congenial with the mechanical outlook. This outlook relies on the belief that the correct, deterministic programming of the _tabula rasa_ will necessarily yield the right results and will do so _repeatably_. This view suggests that the affairs of the musical world are securely managed by deterministically imprinting the right facts upon such a passively receptive memory device as a _tabula rasa_. It is thus the ultimate convenience to be able assuredly to retrieve items from such a stock of musical certainties that are "determined purely from the nature of the objects experienced, and depend in no degree upon the nature of the mind."

Now, besides Gieseking's demand for right notes, what other examples of pianistic perfection, or right end products, can be cited that are supposedly assured by the workings of the mechanical philosophy onto such supposedly mental blank tablets? As but a single example, consider Dean Elder's interview (in a recent volume of _Clavier_) with Fanny Waterman, Chair of the Leeds International Pianoforte Competition. Waterman has some rather definite ideas indeed as to what certain correct end products she expects:

> The right Mozart sound must not be too big. . . . When Beethoven writes _pianissimo_, it is nearly always synonymous with a _mysterioso_ sound. . . . With more advanced pupils I may play a C-major chord and tell them to produce the chord in the style of Beethoven, Mozart, or Bartók. It's amazing that by bringing out different notes you can make a chord sound Bartókian or noble, like Beethoven. A chord isn't just a block of four, five, or six notes. If you want to bring out one note . . .you play it a microsecond before the other notes. You pedal and the whole thing blends into one sound. _It's a trick of the trade_. (Elder 1986: 8, 9, 10, emphasis added)

As represented in this extract, not only does Waterman apparently expect the pupil assuredly to retrieve mentally imprinted items of musical style as correct end products of a mechanical routine, she also details the first two components of the mechanical routine that precede the production of such products. The right end products of this routine are, then, the production of C-major chords in _the_ style of Beethoven, _the_ style of

Mozart, or the style of Bartók. These products are manufactured repeatably, assuredly, and finitely, using a recipe or right and definite method that includes the emphasizing of certain chord tones and the precise use of the damper pedal. And for right initial materials she requires pupils that are suitably advanced. Here, then, we are confronted with the philosophy of musical mechanicalism expressed in a clear, detailed, and concise form.

How then are the three principal characteristics of musical mechanicalism viewed from an anti-mechanical position? To answer this question we shall turn to Artur Schnabel's *Music and the Line of Most Resistance* of 1942.

Artur Schnabel (1882-1951) was one of the legendary pianists and teachers of the modern age. His own tuition as a pianist was begun at seven years of age, in Vienna, under the well-known teacher Theodor Leschetizky. William Glock, as author of the Schnabel article in *The New Grove Dictionary*, recounts two revealing remarks made by these men. Of Leschetizky, Schnabel recounted that he had essentially no "method" as such. His instruction was " . . .like a current which sought to release all latent vitality in the student." Leschetizky pronounced to Schnabel that "You will never be a pianist; you are a musician." In keeping with this judgement, Schnabel made his main reputation as a performer not on the so-called pyrotechnical literature of Liszt and Chopin. Rather, he is justly remembered for his renderings of Schubert and Beethoven, music which, according to him, "was better than it could be performed" (Sadie 1908/16: 681). He played all the thirty-two sonatas of Beethoven, as a group, first in Berlin in 1927 and subsequently again in Berlin (1932) and in London (1934). In accordance with this interest in Beethoven he also made a complete recording of these sonatas, along with the Diabelli Variations and the five concertos, and prepared an edition of the sonatas appearing in 1935 under the auspices of Simon and Schuster. As teacher he worked at the State Academy in Berlin, the University of Michigan, and privately at his summer residence on Lake Como, Italy. Numbered among his students were such luminaries as Sir Clifford Curzon, Claude Frank, and Leon Fleisher.[7] He also composed, but seems to have been extraordinarily reluctant to bring his works before the public. Of his three books, Music and the Line of Most Resistance is the second. Let us now look into this exposition of an anti-mechanical view.

When Schnabel spoke of the three traits I have examined in connection with a mechanical outlook, he tells us not what the right materials, methods and products are, but rather what they are not. A discussion of the "right" type of student, then, never arises:

> Traces of originality, of differences in talents and types of talents, can be noticed in the early stages. . . . Even if only a minimum of freedom is granted, one student will scarcely ever solve the problems at hand in exactly the same manner as any other student. This proves that music, apart from a few elementary and arbitrary stipulations, is not as fixed as the multiplication table. Nor has the classification of right or wrong any validity for music, which is beyond being measured and judged by quantity or by "moral" standards. (Schnabel 1942: 16)

Here in brief form, then, we see already how an anti-mechanical point of view differs from the mechanical version. Thus, the search for just the right type of student is never undertaken, as differences in talents and types of talents belies the effort. This being acknowledged, attempts at thoroughgoing exactitude of instructional method are bound to meet with frustration. The few elementary and arbitrary stipulations that are appropriate cannot constitute a mechanicalistic method, truly understood. Likewise, the determination of the musical product as pianistic perfection is seen as an exercise in logical absurdity: if we recognize the variability in initial materials and thence enjoin procedures appropriate to these variances, the mechanical demand and recipe for a singularity of product is rendered intellectually inoperative.

Once again, the circularity of the recipe approach, the closed nature of mechanicalistic systems, is observed. The internal pervasiveness of this determinism is easily explicated. If one endorses a singularity of interpretive product, a pianistic perfection, then having just the right types of students and the right method will insure the desired result. On the other hand, if one can enumerate the components of just such a method, one is quickly driven to the observation that only a carefully selected type of student will best carry it out. Likewise, if one is initially convinced in favor of an exclusive class of student, one will soon find that only a certain type of method will do, and so forth. No matter where within the specifics of this musical mechanicalism one takes up the recipe-based point of view, generally considered, the other details of the system seem strictly and <u>necessarily</u> entailed.

How does an anti-mechanical posture such as that of Schnabel compare to this sort of self-referentialism? His view of the nature of musical product was stated succinctly. It is " . . .that other shore which, to be sure, can only be sighted but never reached" (Schnabel 1942: 14). The idea of an ongoing process rather than a closed system is clearly implied. More generally speaking,

> Art is not convenience, is not just <u>one</u> feature of some structure; it is an independent organism and each single representation of it is independent as well. It is its intrinsic nature to be released by the noblest aspiration

> of man and addressed to the noblest aspiration of man, to be released by man's profoundest demands on himself, by his conscious desire for contact with invisible reality and unequivocal truth--in a region that is above the egotistical, the utilitarian and the mechanical. (Schnabel 1942: 59-60)

This citation makes glaringly clear that Schnabel was keenly aware of the conflict between musical <u>result</u> and the <u>products</u> of the egotistical, the utilitarian and the mechanical. Indeed, he thought the teacher should be alert to attempts at a mechanically standardized product, and guard against them:

> The teacher must insist upon the observance of the natural sequence of the elements of performance. He must impress upon the student the axiom that the tonal idea can be efficacious on one single occasion only. Therefore, it has to emerge afresh on each new occasion, and <u>this function can never become automatic</u> (Schnabel 1942: 22, emphasis added).

Musical result, then, as I distinguish it from product, is not <u>the</u> end feature or conclusion of some mechanical structure or other, but is instead <u>a</u> consequence among many possible. Thus, "it has to emerge afresh on each new occasion." This does not imply that an anti-mechanical approach amounts to a license for solipsism or pure self-indulgence. Student, teacher, and performer alike must account for rather a different set of demands:

> The prospective performer has to labor mentally to comprehend the finished piece of music which he undertakes to transform into sounding reality, to comprehend it not merely as an arrangement of tones, but as a formed and organized expression. His teacher must direct him methodically through this multitude of

> requirements, must guide his instinct, release his emotion and intelligence, without restricting his urge for freedom. And, ultimately, he has to be careful to mould the student's ambition, concentration and exertion into a permanent, increasing, inspiring and conscious joy. (Schnabel 1942: 18)

The mechanicalist will pounce on the latter observations: "Ah, you see? You've merely described another, different set of rules and called it 'anti-mechanical.' But rules they are nevertheless!" Indeed, an anti-mechanical approach does deal with rules, as it must. The experimental approach cannot dispense with rules, of a sort, without risking a result something akin to "Well, yes, just go ahead and do whatever you like, whatever seems self-satisfying." The point here is that the rules of an experimental approach are of a different type. To paraphrase Peirce's earlier remark on consequences to be drawn in the logic of relatives, a number of distinct rules or guidelines may be undertaken, none leading necessarily and deterministically into another. Alternatively put, regardless of what point at which one enjoins an anti-mechanical musical system, it does not seem that one can always predict and mechanically insure the entailment of other points.

The functions of observation and experiment are anathema to the mechanical approach in general form; observation and experiment are at odds against a system whose conditions and procedures are foreordained and fixed. A pragmaticistic, anti-mechanical approach, however, counts observation and experiment among its constituents. Schnabel was clearly aware of this distinction between scientific method and mechanicalism. He gives, with insightful irony, an account of his observations of a certain musical phenomenon that reveals how opposed "experiment" and "mechanical" are:

> Experiments have been made in orchestral performances without the guidance of a conductor. I heard concerts given by such a group in Moscow in 1925. It would, of course, have been more interesting and instructive to be present at their rehearsals. The men, all expert players, were seated in a circle facing each other as if at a round table, but without the table. The first passages sounded most impressive as a technical feat, astonishing as a triumph of drill. But soon the performance became more and more stiff, mechanical, lifeless, fixed to one straight flat track, excluding spontaneity and ease. And by the way, there was one among them who regulated the "traffic"--a secret conductor--and there was a system of signalling "nuances" as well. It may be that the details of the performance had been decided by vote. It was still necessary for the players to warn each other at dangerous points. To me the whole adventure seemed pointless, inartistic and artificial. (Schnabel 1942: Ø5-Ø6)

This painfully comic report will remind the reader of the likewise disingenuous means used by Gieseking in his selection of students: lip-service is paid to experiment while mechanical strategems do, in point of fact, carry the day. It is this idea that a mechanical format will provide for the safe or guaranteed dispatch of the musical affairs at hand that, probably, makes this approach so seemingly attractive. Schnabel drew out this belief when examining the issue of virtuosity:

> Yet there is something in musical performances which invites performers to exhibit dexterity. Brilliant effect is possible simply with speed or noise, or speed and noise. But since this kind of effect is fairly sure to be produced, while the effect produced by solutions of artistically nobler tasks is not so sure (unless the task is <u>well solved</u>), the preference of the safer line becomes quite understandable. (Schnabel 1942: 75, emphasis added)

That musical tasks call for being "well solved" is a notion that is quite congenial to an experimental method, a pragmaticistic one: a number of solutions and means of arriving at them are expected and welcomed. Further evidence that the Schnabel version of anti-mechanicalism does indeed require observational powers and experimental procedures that are divorced from the mechanical approach of Gieseking, but congenial to Peirce's pragmaticism, are easily found in <u>Music and the Line of Most Resistance</u>. For now I shall cite but one additional piece of such evidence:

> What I can say about music is exclusively a result of sheer musical experience in both meanings--experience through continual experiences. . . . I welcome the opportunity to relate, without method or dogmatism, some of my ideas on music and on conditions which surround music. (Schnabel 1942: 32)

This opposition of a dogmatic method against an ongoing collection and review of experiences or evidence makes for a brief and rather complete description of the mechanical/anti-mechanical conflict. In his "Our Senses as Reasoning Machines" of 1900, Peirce formed a parallel distinction between forms of reasoning requiring learning and those of a more mechanical type.

> What, then, is the use of designating some formations of opinion as rational, while others (perhaps leading to the same results) are stigmatized as blind followings of the rule of thumb or of authority, or as mere guesses? When we reason we set out from an assumed representation of a state of things. This we call our premise; and working upon this, we produce another representation which professes to refer to the same state of things; and this we call our conclusion. But so we do when we go irreflectively by a rule of thumb, as when we apply a rule of arithmetic the reason of which we have never been taught. The irrationality here consists in our following a fixed method, of the correctness of which the other method affords no assurance; so that if it does not happen to be right in its application to the case at hand, we go hopelessly astray. In genuine reasoning, we are not wedded to our method. We deliberately approve it, but we stand ever ready and disposed to reexamine it and to improve upon it, and to criticize our criticism of it, without cessation. (Peirce 1900/MS 831: 09-11)

Now let us look into where such a "fixed method" and its antagonist would be found outside the musical world.

Notes

1. Biographical information on these and other musicians may be found in the appropriate volumes of Sadie 1980, and in Slonimsky 1984.

2. This work was first brought out in 1931 by B. Schott's söhne in Mainz as Modernes Klavierspiel nach Leimer-Gieseking. The English translation The Shortest Way to Pianistic Perfection of 1932, published by Presser in Philadelphia, retains Karl Leimer as the first author with Walter Gieseking as the second. A second work by these authors, Rhythmics, Dynamics, Pedal and Other Problems of Piano Playing was reprinted with Pianistic Perfection under the title Piano Technique in 1972 by Dover Publications in New York. In this appearance, unlike the previous ones, Gieseking is listed as the primary writer. That Gieseking was, or at the very least considered himself as, the principal author is established in the opening sentence: "The present treatise explains the method of my piano playing, that is, what I consider to be the foundation of my pianistic technique." Hereafter, when citing from Pianistic Perfection, I will be using the pagination of the Dover reprint and Gieseking as the primary author.

3. Details of Matthay's career are given at Sadie 1980/11: 832. Perhaps his most visible student was Dame Myra Hess.

4. What can this affectedly contradictory phrase mean?

5. From Gieseking alone, for now, additional instances are easily cited. On the subject of the first three-part Invention by Bach: "It is left to the teacher to show the pupil how to master the above problems in the best and most correct manner" (Gieseking 1932: 31). In general, "The pupil must be trained to feel what is correct" (Gieseking 1932: 44). Some quite specific illustrations figure into this as well. His recipe for the execution of trills carried the following guarantee: "Whoever follows the method here advised may expect certain success" (Gieseking 1932: 57, emphasis added). He also designated the fingers with which to begin the C major scale. We might well object, here, that any pianist of significant experience will acknowledge the C-major scale to be the one admitting of the widest variance in fingering possibilities.

6. I must leave any really detailed inquiry into the important and somewhat intricate issues of composers and intentionalism versus composers and notational vagaries for later consideration. By way of summary, though, I will add here that the belief that a composer's intentions are definitively expressed through a notational apparatus and that, hence, it is logically possible for these intentions to be definitively known looks very much like a mechanicalistic position. That is, the information disclosed via a musical score is considered exhaustive; functioning in fact as another closed system governed by recipes. An example of how important extra-musical information can be for the realization of a musical score is given in Christoph Wolff's piece appearing in the July 1971 installment of The Musical Quarterly (pp. 379-408). Wolff's essay is considered in the final chapter below.

7. The present author was, at various times from 1977 to 1982, under Fleisher's tutelage.

CHAPTER III

ALAN MARQUAND AND ALAN TURING: LOGICAL MACHINES, ALGORITHMS, AND FIXED METHODS

> And then we have this belief that everything can be reduced to quantities, to systems and to formulas.
>
> Russell Sherman
> -from an interview in
> Piano Quarterly, 1985

One of the more fascinating developments during the late eighteenth and nineteenth centuries was that of so-called logical machines. Their development paralleled the general curiosity as to just how much of the natural world, including the operations of the thinking mind, could be subdued, explained, and thus controlled by man. Work in the area addressed the debate concerning to what degree the workings of the mind could be managed by such logical machines. One important issue in this debate was to investigate to what extent problems in the logical relations of classes could be mechanically worked out according to finite sets of instructions or algorithms[1].

Broadly considered, two types of these machines were devised: those dealing with mathematical calculation and others concerned with logical operations. The initial figure in these developments was Charles, third Earl of Stanhope (1753-1816), who devised examples of both types of machines. About 1777, he constructed calculating machines whose function was to figure results in mathematical addition, subtraction, multiplication, and division. He subsequently produced his "Stanhope Demonstrator, an Instrument for performing Logical Operations."[2] William Stanley Jevons (1835-1882), although unaware of Stanhope's Demonstrator, constructed another machine for such logical operations in 1869.[3] His so-called Logical Piano, which did indeed resemble a small, upright piano complete with keys, was exhibited to the Royal Society the following year.[4]

The general optimism and enthusiasm about logical machines was encouraged by the development and successful employment of sophisticated calculating devices. Anticipated advantages of such devices were increases in speed and accuracy when manipulating the data of such pursuits as astronomy, demographic and actuarial calculations, and geodesy. At that time these types of computations were still being carried out by hand. Perhaps the most important figure in the development of such calculating engines was Charles Babbage (1792-1871), who, among other activities, sought mechanical means by which to rid current mathematical tables of their errors.[5] Internal clues suggest that Peirce probably authored the obituary of Babbage appearing in The Nation for 9 November 1871. This obituary illustrates something of Babbage's personality as well as details of his "engines" and the enthusiasms, both intellectual and fiscal, that they engendered.

> The death of Mr. Charles Babbage, the inventor of calculating machines, is announced. He was born December 26, 1792. The analytical power of his mind was early manifested. In 1815, when he was only twenty-two years old, appeared his remarkable "Essay towards the Calculus of Functions," a very general and profound sort of algebra, of which he was the chief author. About 1822, he made his first model of a calculating machine. It was a "difference engine," that is, the first few numbers of a table being supplied to it, it would go on and calculate the others successively according to the same law. This, at least, is as correct as so short and easy a statement can be. In the following year, at the request of the Royal Society, the Government made a grant of 1,500£ to enable Mr. Babbage to proceed with the construction of his machine. In 1829, the Government largely increased this sum, and in 1830 assumed the property of the machine, and declared their intention of defraying the cost of completing it. This Mr. Brunel estimated at 12,000£ at a time (February, 1831) when from 8,000 to 9,000£ must have been spent. It was in 1830 that Babbage published his "Enquiry into the Causes, of the Decay of Science in England," a savage attack on the management of the Royal Society; on Mr. Pond, the Astronomer-Royal; on Captain Sabine, and other influential scientific men. But it was after the publication of this book that Government agreed to furnish the engine. In 1833, a portion of the engine, sufficient to illustrate the working of the whole, was put together. It was a wonderful piece of workmanship, of a precision then unknown, and still unrivalled. To make it, it had been necessary not only to contrive new tools, but to lay a scientific foundation of the principles of tools, and to educate the

mechanics who were to use them. Not a penny of the money paid by the Government ever went into Mr. Babbage's pocket, but, on the contrary, he had always advanced the money to pay the workmen until the Treasury warrants were issued, so that he was usually in advance 500 to 1,000£. In 1833, Mr. Babbage declined to continue this system, and, in consequence, the engineer discontinued the construction of the engine, dismissed the workmen, and took away all the tools. During the suspension of the work caused by this circumstance, the great misfortune of his life befell Mr. Babbage. He discovered the possibility of a new analytical engine, to which the difference engine was nothing; for it would do all the arithmetical work that that would do, but infinitely more; it would perform the most complicated algebraical processes, elimination, extraction of roots, integration, and it would find out for itself what operations it was necessary to perform; and the principle of this machine was such as immensely to simplify the means of attaining the object of a difference machine. One would suppose that, finding himself so unlucky as to have thought of such a thing, Babbage would at least have had the sense to keep it strictly to himself. Instead of that, he wrote immediately and communicated it to the Government! Before that, all was going smoothly; after that, they never would advance another penny. But it must be admitted that Mr. Babbage himself does not seem to have been very ardent to go on with the old machine after the new one was invented. Another difference engine has since been made by a Swede, named Scheutz. This machine is now at the Albany Observatory, and a duplicate of it is used in the office of the Registrar-General in London. Recently, an important new plan for such an engine has been invented in this country; and careful estimates show that it could be constructed for at most $5,000. But the analytical engine is, beyond question, the most stupendous work of human invention. It is so complicated that no man's mind could trace the manner of its working through drawings and description, and its author had to invent a new notation to keep account of it. This mechanical notation has been found very serviceable for simpler cases. (Peirce 1871)[6]

Likewise new levels of mechanized efficiency were sought for human reasoning. The object was to investigate to just what extent certain forms of logical operations could be executed by machines. Stanhope's Demonstrator and Jevons's Logical Piano were the first devices mechanically to perform such operations. Alan Marquand further developed the device of Jevons and thereby produced the most advanced logical machine of the type in the late nineteenth century. Peirce's definition in the Century Dictionary for "logical machine" places these subjects in a proper context:

> Logical machine, a machine which, being fed with premises, produces the necessary conclusions from them. The earliest instrument of this kind was the demonstrator of Charles, third Earl Stanhope: the most perfect is that of Professor Allan Marquand, which gives all inferences turning upon the logical relations of classes. The value of logical machines seems to lie in their showing how far reasoning is a mechanical process, and how far it calls for acts of observation. Calculating-machines are specialized logical machines. (Peirce 1889: 3560)

To what extent could reasoning be accomplished by mechanical means? An answer to this question and an exquisite illustration of extra-musical mechanicalism lies in an examination of Marquand's "most perfect" of such logical machines and its grist, the logical relations of classes.

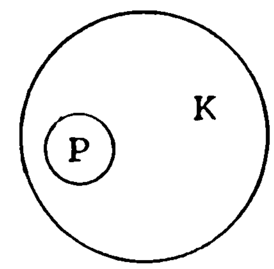
Using citations from the Century Dictionary definitions of "class" and "relation," we can arrive at an understanding of the logical relations of classes. For "class" is given the following definition:

> A number of objects distinguished by common characters from all others, and regarded as a collective unit or group; a collection capable of a general definition; a kind. (Peirce 1889: 1029)

For the present illustration, let us count pianists, keyboard players, musicians, and artists as examples of different classes. The class or set of pianists, then, would be the class or set including and limited to all pianists. For the term "relation" is given:

> A character of a plurality of things; a fact concerning two or more things, especially and more properly when it is regarded as a predicate of one of the things connecting it with the others; the condition of being such and such with regard to something else. (Peirce 1889: 5057)

Thus could we assert the relation of the classes "pianists" and "keyboardists" as "All pianists are keyboardists" and take it as the first premiss of a four-term syllogistic deduction. Taking the first letters of these class names as abbreviations for them, we can state this first premiss more briefly, in propositional form. Thus, where "P" abbreviates "pianists" and "K" abbreviates "keyboardists," the proposition "All P is K" abbreviates this first premiss. For greater clarity, we can also render this premiss in a diagrammatic form of the type introduced by the noted eighteenth-century mathematician, Leonhard Euler (Illus. 3-1):

Proposition	Diagram
All P is K	

Illus. 3-1. First Euler Diagram.

By means of the diagram given above, we immediately grasp that each and every thing which is a pianist, that is, the class of pianists (represented by the encircled P), is related to each and every thing which is a keyboardist, or, the class of keyboardists (represented by the encircled K), by the relation expressed through "is." The relation expressed through "is," in this case, can be defined as "is included in." The Euler diagram representing this relation illustrates it by means of the geometric relation "is inside of."

"All pianists are keyboardists" is our first premiss. Let us suppose that the investigation of our syllogistic deduction is to conclude what the relation is between pianists and artists. We then add two additional premisses to the first, these two being "All keyboardists are musicians" and "All musicians are artists." By employing the propositional and diagrammatic conventions used above, we can then represent the complete categorical syllogism with its conclusion. Using the three premisses in their original propositional formulations, we would have the following.

All pianists are keyboardists

All keyboardists are musicians

<u>All musicians are artists</u>

Therefore: All pianists are artists

Following below (Illus. 3-2) is another representation of this argument, using suitably abbreviated propositional forms and their diagrammatic equivalences.

An important aspect of this type of argument is <u>not</u> the actual everyday contents of these classes, whose relations as stated we might find alternatively flattering or naive, but rather the manner in which these classes are brought into relations with one another to produce a valid <u>form</u> of argument. The following argument, for example, asserts relations of classes whose actual everyday contents render the conclusion rather silly. Nevertheless, its <u>form</u>, a duplicate of the one given above, is completely valid.

All academics are perjurers

All perjurers are heroes

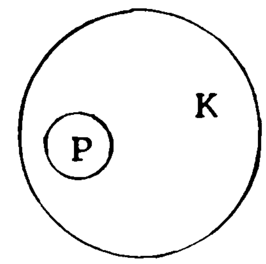
<u>All heroes are idiots</u>

Therefore: All academics are idiots

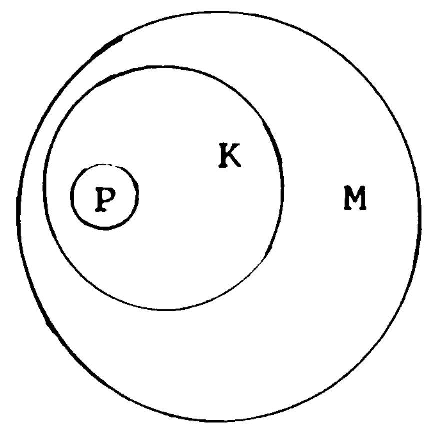
This kind of argument in the logical relations of classes is of the type that can be treated completely and correctly, and by algorithmic means, using the kind of

Propositions | Diagrams

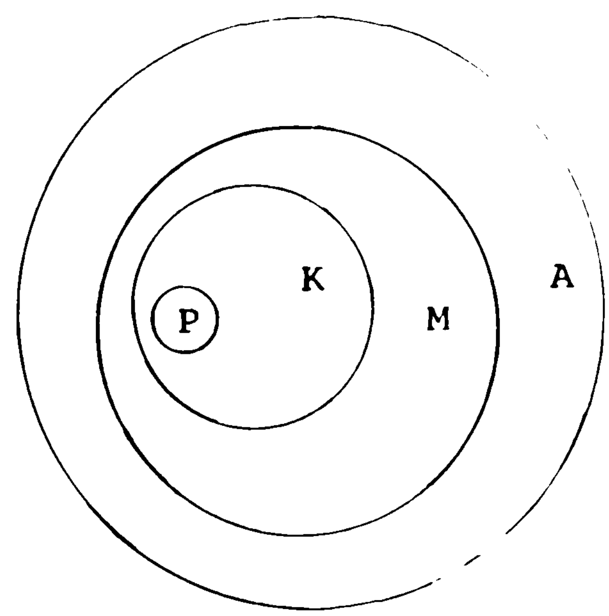
All P is K



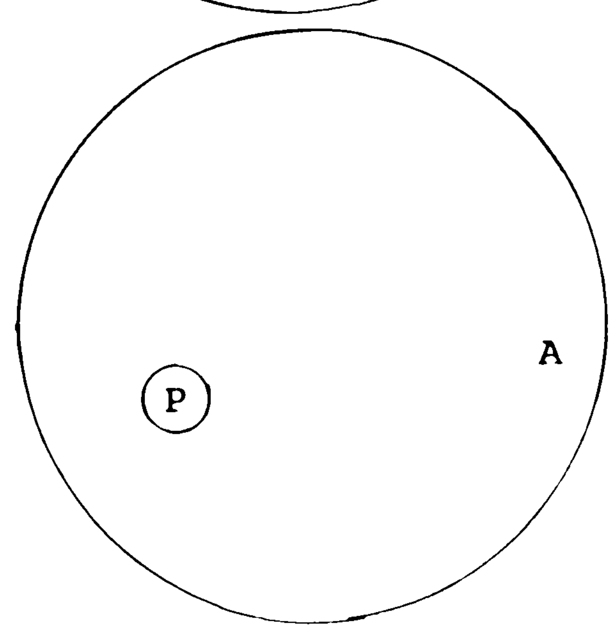
All K is M



All M is A



Therefore: all P is A



Illus. 3-2. Second Euler Diagram.

logical machine proposed by Peirce and his student and colleague, Alan Marquand.

Alan Marquand (1853-1924) was the son of the art collector and philanthropist Henry G. Marquand, and perhaps the most prominent of Peirce's students during his tenure at the Johns Hopkins University.[7] Peirce was a part-time lecturer in logic at Hopkins from 1879 through 1884, and concurrently maintained a full-time position as Assistant at the U.S. Coast and Geodetic Survey. He held the Coast Survey position from 1859 until his resignation in 1891. While at Hopkins he was active as well in the areas of mathematics,[8] psychology, and philosophy. However, it is Peirce's activities in logic, especially with Marquand, that concern us here.

Marquand was a Fellow in Philosophy and Ethics at Hopkins until the completion of his doctorate in 1880. His thesis on the logic of Philodemus was written under Peirce's supervision, and was the only doctoral dissertation ever completed under Peirce's direction.[9] <u>Studies in Logic by Members of the Johns Hopkins University</u>, which appeared in 1883 under Peirce's editorship, included the introduction to Marquand's dissertation, entitled "The Logic of the Epicureans," as the first essay. In 1881 Marquand joined the faculty at Princeton as Tutor in Latin and lecturer in Logic. In 1883, probably as a result of a dispute with President

McCosh, he changed his position at Princeton by becoming Professor of Art and Archaeology, a position he held until his death.

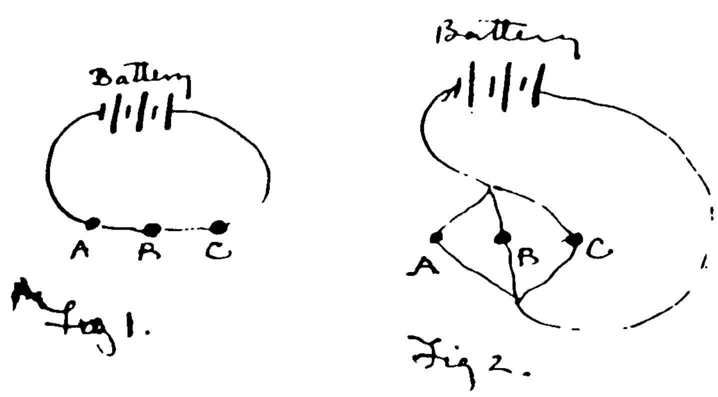
While still a student at Hopkins, Marquand had been directed, by Peirce, to the task of developing an improved version of Jevons's Logical Piano. In addition to authoring the first number in Studies in Logic, Marquand had also provided the second essay of that 1883 edition, "A Machine for Producing Syllogistic Variations," and the following "Note on an Eight-Term Logical Machine." From these contributions it is clear that Marquand had already devised three types of aids for logical computation. The first such device consisted of logical diagrams, the second was a mechanically crank-operated realization of these diagrams, and the third made up the improved Jevons machine, complete with operation by depression of keys.[10] Peirce, of course, was completely familiar with these developments and certainly had the upgraded Jevons device in mind when writing the logical machine entry for the Century Dictionary in 1889. But what, exactly, was Peirce referring to when there noting Marquand's "most perfect" machine?

James Mark Baldwin reported the important third development by Marquand, the improved Logical Piano, in his Dictionary under the entry "Logical Machine." He also mentioned a crucial fourth development, one that, by involving electricity, departed radically from its predecessors:

> In 1882 Marquand constructed from an ordinary hotel annunciator another machine in which all the combinations are visible at the outset, and the inconsistent combinations are concealed from view as the premises are impressed upon the keys. He also had designs made by means of which the same operations could be accomplished by means of electro-magnets. (Baldwin 1902/II: 29-30)

Did Peirce have, in addition to his thorough acquaintance with the improved Logical Piano, knowledge of such designs involving electromagnets in mind when reporting in the _Century Dictionary_ on Marquand's "most perfect" logical machine?

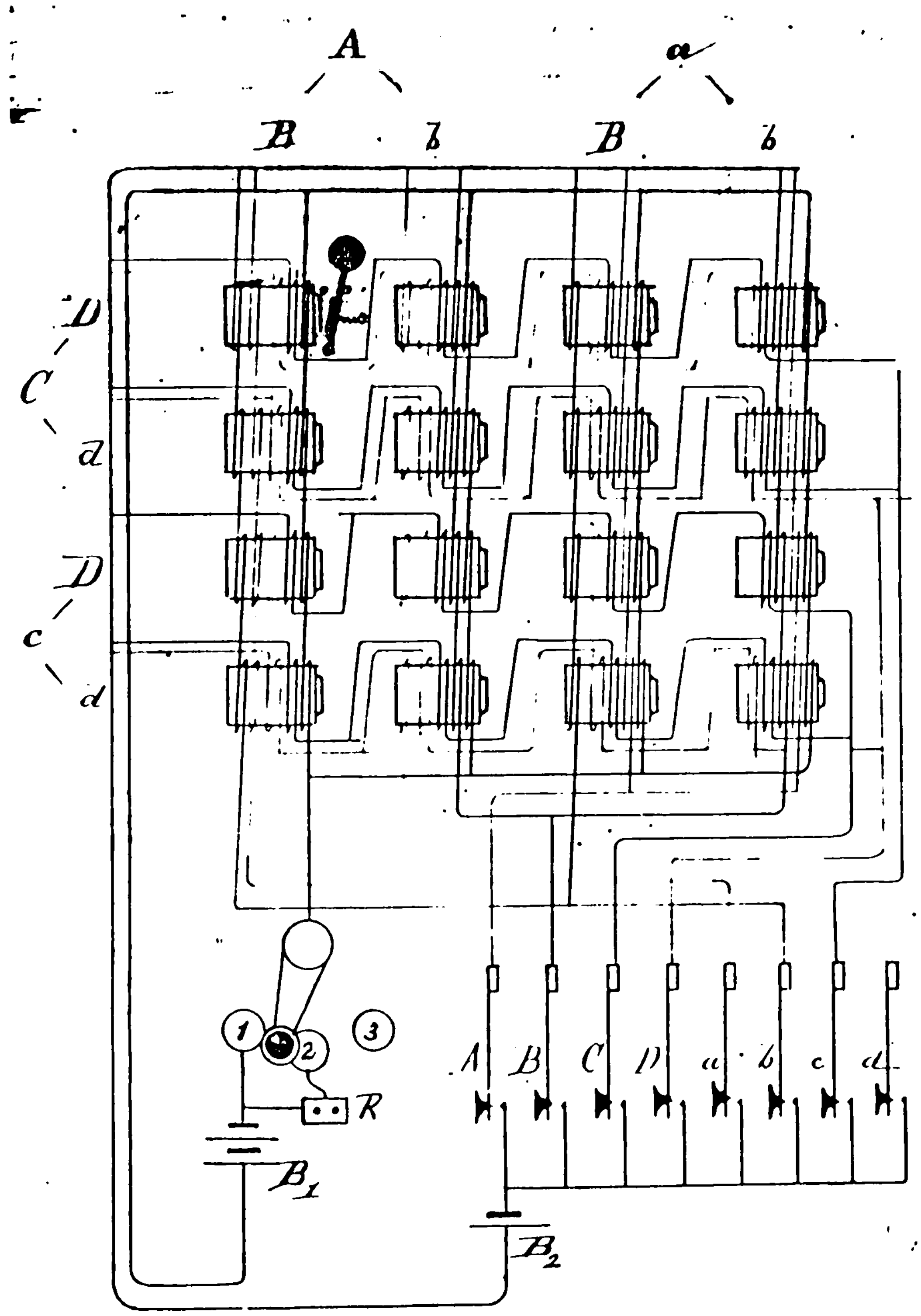
Marquand published on the subject of his hotel annunciator advancement of the Logical Piano in the _Proceedings of the American Academy of Arts and Sciences for 1885-1886_, naming the essay "A New Logical Machine."[11] He had read this paper before the AAAS on 11 November 1885, and apparently was disappointed with its reception. That this was the case is plainly stated in a letter from Peirce to Marquand (headed 30 Dec. 1886, 36 W. 15th St., New York): "You spoke, when I saw you, as if disappointed with the reception your machine had met with."[12] It is in this same letter where, in the spirit of improving the hotel annunciator device, Peirce suggested the introduction of a new component in the design of logical machines. This component quickly lead to a design, as reported by Baldwin, that involved electromagnets; this component was electricity:

> I think you ought to return to the problem, especially as it is by no means hopeless to expect to make a machine for really very difficult mathematical problems. But you would have to proceed step by step. I think electricity would be the best thing to rely on.
>
> 
>
>
> Let A, B, C be three keys or other points where the circuit may be open or closed. As in Fig. 1, there is a circuit only if all are closed; in Fig. 2 there is a circuit if any one is closed. This is like multiplication & addition in logic. (Peirce 1886, using his hand-drawn figures)

These diagrams of series and parallel electrical circuits effect logical multiplication and addition, the conjunction ("A and B") and disjunction ("A or B") of propositions.

This remarkable letter lay in the Allan Marquand Papers at Princeton University Library, apparently unnoticed, until its discovery about 1970 by Professor Preston Tuttle.[13] In addition to this unique letter there is another, even more fascinating piece of evidence on the subject of Peirce, Marquand, and logical machines in the Marquand Papers at Princeton. This is a design of the type using electromagnets that, according to Baldwin, Marquand "had made." This electromagnetic design was first noticed at Princeton by Professor Alonzo Church, about 1950.[14] Historical evidence surrounding this diagram makes for an exceptionally strong case that it was drawn up about 1887 and that its author was Charles Peirce.[15] Following on the next page below is a replication of this diagram (Illus. 3-3).[16]

This diagram represents a proposed electro-mechanical machine that performs, by purely algorithmic means, deductions in the logical relation of classes involving up to four terms (classes). It exhibits four basic types of components: electromagnets, circuit keys for the introduction of premisses, an operations switch, and (a power supply) two batteries. Consistent with the instructions for operation found on the verso of the original drawing and with Baldwin's remarks, we would begin logical operations with this machine with "all the [logical] combinations [being] visible at the outset" (Baldwin 1902/II: 29). These logical combinations, four letters each in length, would be matched to the electro-magnets according to the grid of exhaustive possibilities implicit in the lettering on the diagram, where uppercase and lowercase letters represent the truth and falsity of each of the four terms (see following Illus. 3-3).



Illus. 3-3. Diagram for an Electromagnetic Logical Machine, probably by C. Peirce, ca. 1887. (The Allan Marquand Papers, Princeton University Library.)

ABCD	AbCD	aBCD	abCD
ABCd	AbCd	aBCd	abCd
ABcD	AbcD	aBcD	abcD
ABcd	Abcd	aBcd	abcd

Premisses are introduced by means of the circuit keys which are lettered on the diagram to handle these four terms and their negations. As premisses are thus entered, the inconsistent logical combinations that result are, by actions of the electromagnets, removed from view (see III*n*16). Let us suppose, now, we wish to work a four-term categorical syllogism of the type initiated earlier with the premiss "All pianists are keyboardists" (see p. 56, above). Here, however, we will use the four terms A, B, C, and D instead of the complete nouns. Our three premisses are, then:

All A is B

All B is C

<u>All C is D</u>

Our aim is to discern what logical relation may obtain between classes represented by the terms A and D. These premisses are entered in discrete steps with each term or variable handled in turn. After all three premisses have been entered and the attendant inconsistent logical combinations concealed, the result of our mechanicalized working of a four-term categorical syllogism is the valid conclusion "All A is D," given as:

ABCD	----	----	----
----	----	----	----
----	----	----	----
----	----	----	----

We see that by observing the conventions for entry of premisses and thus setting into action the matrix of the proposed machine, this result is completely, precisely, and <u>repeatably</u> determined by finite, algorithmic means. The elements with which this deductive activity began were, of course, our premisses for a four-term categorical syllogism. This is, then, an example from the collection or set of argument forms which the Peirce-Marquand machine <u>can</u> execute. Especially important is the procedure or method embodied in the actual physical arrangement of the machine with its entry keys, electromagnets and such, by which this activity is carried out. This method mechanically, deterministically, and repeatably proceeds from appropriate beginning materials to conclusions by means of a recipe, or finite set of instructions. Such a method, as we know (see II<u>n</u>1), is termed an algorithm. The disjunctive syllogism "A or B, not A, therefore B" however, is an example of an argument that the proposed machine <u>cannot</u> execute: it is of a form from <u>outside</u> the set or class of possibly workable argument forms. Now if the list of premisses to be introduced had been infinite instead of <u>finite</u> in length, and if the machine itself had been modified to accept such a list of infinite length, the machine, in theory at least, would continue computing <u>forever</u>. That is, it would never stop: it would never reach an absolutely final conclusion.

We should observe how structurally similar this state of affairs is to the mechanicalistic determinations of Gieseking, Matthay, and Waterman. With Gieseking and Matthay, generally speaking, the premiss of the right kind of student subjected to the right method or algorithm will produce right conclusions or results. And of course with Waterman, more specifically, <u>the</u> styles of Mozart, Beethoven, and Bartók she names are, as right results, assuredly produced if the properly advanced type of student learns and executes the correct algorithms or, as she phrases them, tricks of the trade (see p. 38, above).

We can construct an actual two-premiss <u>hypothetical</u> syllogism that illustrates this facet of Waterman's outlook:

Premiss: If students are advanced, they will learn my tricks of the trade.

Premiss: If my tricks of the trade are thus learned, <u>the</u> styles of Beethoven, Mozart, or Bartók will be produced.

Thus: Advanced students will produce such styles.

The musical mechanicalist's activities, then, are prescribed and limited, as classes, in a manner strikingly like the limitations on logical classes appropriate to the proposed Peirce-Marquand logical machine. For the musical mechanicalist, only certain kinds of beginning materials or classes of materials will work within the system. Likewise, only certain kinds of right conclusions or classes of conclusions are to be allowed as possible within the system. And the means of moving from such premiss-classes to conclusion-classes is the right algorithm or group of algorithms

> [that enable] a person by following a routine of rules, to solve any problem of a given kind . . .to perfection, . . [and] should pass from premiss to conclusion in the smallest number of steps possible. (Peirce 1906/MS 498: 09)

A more comprehensive understanding of this crucial idea of an algorithm, and the one to be used in the pending examination of Hilbert's position, can be obtained by considering the works of the mathematical logician and cryptanalyst Alan Turing.

Alan Mathison Turing (1912-1954) was an authentic scientific genius and a genuinely tragic figure.[17] He was educated in Dorset, England, at Sherborne School, and subsequently distinguished himself in mathematical logic at King's College, Cambridge. Study under Einstein at Princeton's Institute for Advanced Studies anticipated an offer to be the personal assistant of a principal in the history of American computing, John von Neumann.[18] Declining this, Turing returned to England to take up an assistant's position at the Foreign Office's Government Code and Cipher School under that country's foremost cryptanalyst, Alfred Dilwyn Knox. He thus joined the recondite profession of deciphering secret or cipherable codes. In 1951, with the sponsorship of Max Newman and Bertrand Russell, he became a Fellow of the Royal Society. Subsequent to this, he was persecuted by the Government as an alleged homosexual. He was then given the choice of being incarcerated or "treated scientifically" with the hormone estrogen, and chose the latter alternative. It seems that the authorities had concluded this approach of "organotherapy," as it was termed, to be preferable to its predecessor, lobotomy. Shortly thereafter, apparently at his own hand, he died of cyanide poisoning.

Before and during the Second World War, the members of the Government Code and Cipher School staff found themselves installed approximately forty miles north of London, just outside the town of Bletchley. It was here that Turing undertook the task which vindicated his own theories on computing and especially computing machines: breaking for the Allies Germany's supposedly uncrackable secret-code enciphering and deciphering device, the Enigma machine.

The problems encountered when trying to decode the Enigma machine were astonishing. First of all, merely by virtue of being a machine, it represented a progressive development in speed and efficiency beyond traditional hand encryption and decryption of cipherable codes. Clearly, should one possess information whose transmission must be accomplished in secret-code form, a profound advantage is enjoyed by the use of machines instead of hand calculations. Moreover, the Germans had developed the technology of their machine in such a way that its internal state, or finite list of operational rules, or algorithm, could be changed at any time. That is, the finite list of procedures and rules that defined the actual configuration of the machine's physical integuments at any given moment, and therefore the code in use at any given moment, could itself be changed at will. Thus, any message could be encoded into any of a seemingly limitless number of forms by the same machine. So, assuming one had deciphered an individual encoded message produced by a machine operating with a particular internal state, no guarantee obtained that the very next coded message to be received would not be the result of some new modification of an Enigma's algorithm. Worse still, Enigma machines had been multiplied throughout the Wehrmacht. Thus, the British cryptanalysts had not only to keep account of sequential modifications in any

<u>particular</u> Enigma but also to maintain vigilance over simultaneous changes between, perhaps, <u>thousands</u> of Enigmas. Brown commented on these aspects of the situation for the codebreakers at Bletchley Park:

> Until the arrival of the machine cipher system, enciphering was done slowly and carefully by human hand. Now Enigma, as Knox and Turing discovered, could produce an almost infinite number of different cipher alphabets merely by changing the keying procedure. It was, or so it seemed, the ultimate secret writing machine. Hitler evidently trusted Enigma completely. Long, persistent and secret inquiry established that at [chief signals officer] Fellgiebel's recommendation Enigma had been adopted for use throughout the three armed services of the Wehrmacht; it was being, or already had been, introduced from the highest down to the regimental level of command. . . . U-boats and even small ships liable to capture were equipped with the machine, for the possession of an Enigma by an enemy was not sufficient to enable him to read encoded traffic. Only knowledge of the keying system and procedure would permit that. (Brown 1975: 20-21)

The apparently impossible task of Turing and his compatriots was, then, to devise some means of keeping account of these seemingly endless and purposely elusive permutations of Enigma's algorithmic encoding and decoding procedures. At any given time, a given Enigma would encode and decode infomation according to a particular algorithm. The ease of instantiating new algorithms, combined with the plethora of Enigma machines, made the number of such algorithms theoretically limitless. Turing's eventually successful strategy for dealing with these circumstances relied on two conceptions announced in his article "On Computable Numbers, with an Application to the Entscheidungsproblem" of 1937, the Turing machine and the Universal Turing Machine.

A Turing machine of the sort discussed in "On Computable Numbers" is a machine which can calculate such "computable numbers":[19]

> The "computable" numbers may be described briefly as the real numbers whose expressions as a decimal are calculable by finite means. Although the subject of this paper is ostensibly the computable _numbers_, it is almost equally easy to define and investigate computable functions of an integral variable or a real or computable variable, computable predicates, and so forth. The fundamental problems involved are, however, the same in each case, and I have chosen the computable numbers for explicit treatment as involving the least cumbrous technique. I hope shortly to give an account of the relations of the computable numbers, functions, and so forth to one another. This will include a development of the theory of functions of a real variable expressed in terms of computable numbers. According to my definition, a number is computable if its decimal can be written down by a machine. (Turing in Davis 1965: 116)

An example of a computable number, then, would be the fraction 1/7. The mechanical, deterministic means of calculating its expression as a decimal according to a finite procedure, its algorithm, would in a general sense be something like "Divide 1 by 7." This finite _means_ of calculation results here in the infinite numerical _series_ Ø.142857142857142857l . . ._n_. Thus, "computable" for Turing meant "calculable according to an algorithm." A

Turing machine, then, operates according to an algorithm or, in his language, the "configuration of the machine." Turing used the abbreviation "_m_-configuration" to express this notion, a notion that conspicuously parallels what I have described as the "method" embodied in the Peirce-Marquand machine and as the "configuration" of an individual Enigma machine at a specified time:

> We have said that the computable numbers are those whose decimals are calculable by finite means. . . . We can compare a man in the process of computing a real number to a machine which is only capable of a finite number of conditions q_1, q_2, . . q_R which will be called "_m_-configurations." (Turing in Davis 1965: 117)

Having isolated the _m_-configuration as the distinguishing characteristic of a given Turing machine, a crucial cryptanalytic question suggested itself in theory, at least: could a Turing machine be devised whose _m_-configuration would imitate the _m_-configuration of an Enigma machine? Put another way, could a Turing Machine imitate the algorithm of an Enigma machine? If so, the two machines would, in terms of observable consequences, be _indistinguishable_: decryption of Enigma encryptions could be performed. It was known that the _m_-configuration of any Turing or Enigma machine could be modified at any time. Turing then applied to these circumstances the idea of his Universal Turing Machine, a machine whose _m_-configuration could imitate the _m_-configuration of _any_ Turing machine. This, then, was the theoretical answer to the "ultimate secret writing machine" (Brown 1975: 20). These relations, where the internal state or algorithm of an individual Enigma machine is imitated by the m-configuration or algorithm on an individual Turing machine, and whereby the m-configuration of the Universal Turing Machine can imitate the theoretically limitless number of individual Turing machines, assumed physical form in an actually constructed Universal Turing Machine:

> The task of penetrating Enigma mechanically presented Knox, Turing and the other experts at the GC&CS with a towering challenge. . . . The Foreign Office obtained an appropriation for the machine, specifications were soon ready, and they were with the engineers during the last quarter of 1938. The contract went to the British Tabulating Machine Company at Letchworth, not far from Bletchley, and BTM assigned the task of building "The Bomb"--as the Turing engine came to be called--to its chief engineer, Harold Keen, and a team of twelve men. In complete secrecy--remnants of that secrecy were still being encountered in 1974--the machine took shape. . . . It was a copper-colored cabinet some 8 feet tall and perhaps 8 feet wide at its base, shaped like an old-fashioned keyhole. And inside the cabinet was a piece of engineering which defied description. As Keen said, it was not a computer, and "There was no other machine like it. It was unique, built especially for this purpose. Neither was it a complex tabulating machine, which was sometimes used in cryptanalysis. What it did was to match the electrical circuits of Enigma. Its secret was in the internal wiring of (Enigma's) rotors, which 'The Bomb' sought to imitate." (Brown 1975: 22)[20]

Just as the Peirce-Marquand device had a class of syllogistic problems it could algorithmically execute, so

Turing's Bomb had its class of Enigma-based code problems within which it too could operate mechanically, deterministically, and with a finite set of instructions. Turing's ruminations on computability and algorithmic executions would not only win a war, literally speaking, but had also provided the background for a definition of the limits of such computability. But a question had already been put forward for which an algorithmic solution was to prove an impossibility. This question had been raised some decades previously in a realm far larger than those of syllogistic arguments or secret writing codes--the foundations of mathematics itself. It is there we shall find an antagonist to the "fixed method" decried by Peirce in his "Our Senses as Reasoning Machines."

Notes

1. At Brody 1967: 57, under "Logical Terms, Glossary of" is found: "algorithm. A mechanical procedure for carrying out, in a finite number of steps, a computation that leads from certain types of data to certain types of results."

2. See the Stanhope number in Stephen and Lee 1917. A detailed account of the Demonstrator is given at Harley 1879.

3. See the Jevons numbers in Stephen and Lee 1917, and in Gillispie 1970.

4. Jevons' own account of his appliance was given in 1870 at "On the Mechanical Performance of Logical Inference." Philosophical Transactions of the Royal Society, 160: 497-518. His The Principles of Science, A Treatise on Logic and Scientific Method of 1887 (London: Macmillan) has as its frontispiece an engraving of the Logical Piano.

5. See the Babbage numbers in Stephen and Lee 1917, and in Gillispie 1970.

6. Subsequent to Babbage's death, his son, H.P. Babbage, was responsible for the construction of, apparently, at least a portion of the analytical engine. As of 1970 this speciman was housed in the Science Museum, London, where also resides that portion of the difference engine Peirce mentioned as assembled. Investigations into Babbage's work continue; the Charles Babbage Institute operates at the University of Minnesota. For further information on the fate of the Scheutz engine and the notorious circumstances that befell its leading proponent at the Dudley Observatory, Benjamin Apthorp Gould, see Ketner with Stewart 1987, Dudley Observatory Scientific Council 1858, Dudley Observatory Trustees 1858, and Dudley Observatory 1866.

7. For further information on Marquand's life and career, see Ketner with Stewart 1984.

8. See Eisele 1979 and especially Fisch 1986, "Peirce at the Johns Hopkins University."

9. See Fisch 1986, "Peirce's General Theory of Signs."

1Ø. See Peirce, ed. 1883, 12-15. Photographs of earlier and later versions of Marquand's improved Jevons Piano and of the only known surviving Marquand machine are given at Ketner with Stewart 1984, 19Ø-194.

11. Reproduced as Appendix A, Ketner with Stewart 1984.

12. The Allan Marquand Papers, Princeton University Library.

13. After efforts by Max Fisch and Arthur Burks, this letter was first published at Eames, Charles and Ray 1973: 33.

The Preston H. Tuttle Collection of the Institute for Studies in Pragmaticism houses those photographic negatives of Amy Fay and her relatives used at Stewart 1985b. Persons interested in the inimitable nineteenth-century pianist/composer Franz Liszt will recall Amy Fay's Music Study in Germany as an important source on the subject of Liszt's manner of teaching. Less well known is the fact that the sister of Amy (known to have served as editor for Music Study), Harriet Melusina Fay, nicknamed "Zina," was the first wife of Charles Peirce. Charles and Zina did in fact travel to Europe during Amy's studies there, but the question as to whether or not Peirce and Liszt may have met as a result of those circumstances remains unanswered. See also IVn11, below.

14. See Mays 1953: 281-282.

15. A full account of this case is given at Ketner with Stewart 1984, with a replication of the original diagram at p. 2ØØ. A fully operational model of this proposed electromagnetic machine awaits construction.

16. A more detailed account of the operation of this proposed machine is given at Ketner with Stewart 1984.

17. See Brown 1975 and Hodges 1983.

18. While definitive evidence may never come to hand, these circumstances make it quite likely, I think, that Turing did indeed inspect the surviving Marquand machine in Fine Hall at Princeton (see IIIn1Ø, above). He was certainly aware of at least some historical aspects of computing, having made mention in a penetrating way of Babbage's Analytical Engine (Turing 195Ø: 45Ø).

19. Originally appearing in the Proceedings of the London Mathematical Society (1936-37) series 2, 42:

230-265. I cite from the reprinted version in Davis 1965. A more detailed account of the operations of Turing machines is given in Hopcroft 1984.

20. A photograph of a naval Enigma machine is supplied at Hodges 1983: plate 4.

CHAPTER IV

DAVID HILBERT AND CHARLES S. PEIRCE: FORMALISM AND THE LIMITS OF ROUTINE

> The wish for perfect security is one of those snares we are always falling into, and it is just as untenable in the realm of knowledge as in everything else.
>
> Bertrand Russell
> -Facts and Propositions, 1918

> Philosophy today is a dead field.
>
> James K. Feibleman
> -from his Presidential Address to the Southwestern Philosophical Society, 14 November 1981

David Hilbert (1862-1943), the leading German mathematician of his generation, expressed through what he termed "my proof theory" the conviction that all questions in mathematics could be answered by algorithmic means.[1] The Hilbert program of formalism, as it came to be known, counted among the items in its charter the belief that the truth or falsehood of any given mathematical statement could be obtained by some mechanical implementation of fixed rules. The idea of such a definite method employing fixed rules explicitly required each group or listing of such rules or steps to be of finite length. Each and every mathematical issue was seen as decidable according to some such definite method. This question of decidability, or the Entscheidungsproblem, in its German formulation, was a major component of the Hilbert program.

This program of formalism sought to delineate all of mathematics in terms of strictly formal properties, those properties that can make it resemble a type of game. The familiar game for two players known as tick-tack-toe, for instance, relies on such formal properties. In this game, a successful array for winning is produced whenever any three squares falling in a straight line are filled with the same symbol. The straight line can, of course, be vertical, horizontal, or diagonal. From the formalist's viewpoint of this game, the important characteristic is not the consideration of any quantitative values that the symbols used by the two players may seem to have, but rather that the formal requirement for winning, by filling three squares in a straight line with the same symbol, be satisfied regardless of what symbols are used. The only stricture on the choice of symbols is that we be able to recognize the symbol used by each player, and that the symbols of the two opponents be distinguishable. The values of the chosen symbols in and of themselves are irrelevant: the patterns we make with them are what determine the outcome of the game.

Addressing this issue of formalism for geometry, Hilbert asserted as early as 1891: "It must be possible to replace in all geometric statements the words <u>point, line, plane</u> by <u>table, chair, beer-mug</u>" (Reid 1970: 264). This being the case, the powers of empirical observation involved in using points, lines, and planes in visually observable geometric constructions are critically diminished in value, if not rendered entirely superfluous. What matters, instead, is the formal consistency and integrity of the system itself. This, like the remarks above on tick-tack-toe, should recall the earlier exposition on the categorical syllogism (pp. 55-59, above) and the Peirce-Marquand logical machine (pp. 61-67, above), where it was illustrated that the formal integrity of the syllogism is in no way affected by any content of the terms with which the argument is populated. So indeed, for Hilbert's formalism, the statement "table is to chair as chair is to beer-mug" was <u>formally</u> equivalent to "point is to line as line is to plane."

This type of anticipated formal equivalence relies explicitly on the principle of <u>axiomatization</u>. The primacy of axioms as components of formalized systems of geometry had been in mathematical currency since antiquity:

> Ever since Euclid, axiomatizing a theory has meant presenting it by singling out certain propositions and deducing further ones from

> them; if the presentation is complete, it should be the case that _all_ statements which could be asserted in the theory are thus deducible. Axiomatization has also come to mean a similar reduction of vocabulary, in that certain notions should be taken as primitive and all further notions which are introduced in the development of the theory should be defined in terms of the primitive ones. In essence this is the conception of an axiomatized theory which prevails today. (Parsons 1967)

Thus, a completely axiomatized system should provide for an investigator to deduce each and every true statement within it. In Euclid's system of geometry, axioms such as "All right angles are equal to one another" and "Things which are equal to the same thing are also equal to each other"[2] are used to derive theorems such as "In right triangles the square of the length of the hypotenuse is equal to the sum of the squared lengths of the remaining two sides" (Barker 1967 and Heath 1956/I: 155-6). While it seems possible and reasonable to assume that Euclid attempted an exhaustive axiomatization of geometry, thereby seeking to prove deductively _every_ truth in the subject, such formalistic perfection proved elusive.[3]

Hilbert, on the other hand, entertained no doubts that his own efforts towards an exhaustive formalization of mathematics would not only prove successful, but that such efforts towards a thoroughgoing axiomatization of the subject could be accomplished with actual _ease_: "Hilbert . . .thought of his programme as one of _tidying_ up loose ends" (Hodges 1983: 93, emphasis added). He had stated the essence of the axiomatic position in 1891 with his speculations on tables, chairs, beer-mugs, and points, lines and planes. In 1899 he went beyond this, constructing an axiomatization of Euclidean geometry that did not rely on references to concrete, visually observable examples in the physical world. With this important development Hilbert had accomplished a major step towards separating the abstract, formalized aspect of mathematics from its empirical, experientially derived origins and applications. He found himself succeeding in the construction of his formula game. Reid offers some details of this game and recalls some appropriate remarks by Hilbert:

> The formula game . . .enabled mathematicians to express the entire thought-content of the science of mathematics in a uniform manner and develop it in such a way that, at the same time, the interconnections between the individual propositions and the facts became clear. It had, besides its mathematical value, an important general philosophical significance.
>
> "For this formula game is carried out according to certain definite rules, in which the technique of our thinking is expressed. These rules form a closed system that can be discovered and definitively stated. The fundamental idea of my proof theory is none other than to describe the activity of our understanding, to make a protocol of the rules according to which our thinking actually proceeds . . .If any totality of observations and phenomena deserves to be made the object of a serious and thorough investigation, it is this one--since, after all, it is a part of the task of science to liberate us from

> arbitrariness, sentiment and habit and to protect us from the subjectivism that [has] already made itself felt." (Reid 1970: 185-186).

A closed system of mathematics which operates according to a protocol of rules that can be definitively stated is a system which, in its formalized employment of axioms and their treatment by algorithmic means, is more than simply an analogue of the systems of musical mechanicalism we have seen most prominently displayed by Gieseking and Matthay. My claim is that a much stronger and more exact similarity holds between these mathematical and musical systems, namely, that Hilbert's theory of mathematical formalism *structurally models* what I am now prepared to term the formalism of musical mechanicalism. A portion of Peirce's *Century Dictionary* definition of "analogy" will help clarify this sense of similarity which I take to be less strict than modeling:

> *Analogy* strictly denotes only a partial similarity, as in some special circumstances of effects predicable of two or more things in other respects essentially different: thus, when we say that learning *enlightens* the mind, we recognize an *analogy* between learning and light, the former being to the mind what the latter is to the eye, enabling it to discover things before hidden. We say that there is an analogy *between* things. (Peirce 1889: 195)

The formalisms of Hilbert, Gieseking, and others, then, share stronger similarities than can be denoted by analogy: both structures aim at complete formalization of their subject matters within closed, finite systems. Now while the subject matters *per se* of these systems obviously differ in important aspects, it is these systems *themselves* that *formally* model each other. Both systems employ specified initial materials as axioms, treat these postulates according to unswerving algorithmic means, and produce or derive conclusions which are then completely, precisely, necessarily, and therefore repeatably determined. I should add that this kind of formalized exhaustiveness and insured predictability is at least what each system *aims* to accomplish. Whether or not this goal is attained is of paramount importance. We acknowledge already that within the phenomenology of music *itself* no known rational technique allows for a scientifically defensible choice between mechanical and anti-mechanical modes of conduct. Now we also know that mathematical and musical formalisms model one another to such an extent that Hilbert's system can be used as an analytical, observational instrument to investigate and more clearly understand musical mechanicalism itself. Using such an observational tool, then, if we can detect a technique and concomitant choice or choices for or against formalism embedded in the mathematical system envisaged by Hilbert, the structurally modeled similarity of these musical and mathematical systems should allow for at least an incisive understanding of, and, perhaps, a defensible choice or choices for or against mechanicalism and formalism in music. I use the designation "defensible choice" quite deliberately: what this essay's overall application of the pragmaticistic method to the musical world can produce are experimentally developed and observed, provisionally endorsable consequences that call for continued investigation, rather than hard and fast conclusions that bring enquiry to an end. Stated another way, we can surely employ Hilbert's system as an investigative apparatus that will allow a better understanding of musical mechanicalism. We can actually get a clear-headed grasp of what this musical mechanicalism is about, how it functions as a system, and what it can and cannot provide, rather than simply reacting to it according to our personal tastes or an orthodox dogma.

A hypothetical objection to this, provided by the mechanicalist viewpoint, would run something like: "Why bother with all this apparatus for continued inquiry? After all, we know the recipe that will produce the answers, the absolutely final conclusions, to these questions. This so-called 'inquiry' is but sophistical jibbering." The adoption of the axioms that inquiry shall indeed terminate with inviolable conclusions and that such results are desirable and routinely producible is a distinguishing characteristic of the mechanical viewpoint. But the pathologies or functional manifestations in music that arise from the mechanical/anti-mechanical conflict do not seem to be so easily dismissed. That is, the doubt engendered by the mechanical/anti-mechanical musical pathology I discuss does not seem to find resolution by simply adopting and employing such formalistic, mechanical axioms.

If mechanicalism was ever to fulfill its aim of total, mechanical exhaustiveness and thereby provide a resolution of doubt, it would do so, it seems, by way of Hilbert. After all, what could be more easily formalized in an exhaustive way than a system devoid of quantifiable values, a system made up of neutral, formal relations? We must ask, then, what was the fate of Hilbert's formalism, which is the apex, for the present essay, of sophisticated extra-musical mechanicalism.

The manuscript for Hilbert's address to the Second International Congress of Mathematicians held in Paris in 1900 contained a list of twenty-three questions whose investigation he believed would help define the future course of mathematics.[4] Here Hilbert's intended axiomatization of the whole of mathematics attained a new sophistication by way of specificity. The last of these questions dealt with devising algorithmic means for, as Hopcroft put it, "establishing the truth or falsity of any statement in a language of formal logic called the predicate calculus" (1984: 86). Hilbert believed that the answer to this question must necessarily be an affirmative one, for not to be able to employ a strictly algorithmic, deductive apparatus in the establishment of all statements in that calculus would jeopardize the integrity of his exhaustive formalization. In 1904 his public declarations on the fundamental issues or foundations of mathematics ceased, not to be resumed until his address to the Swiss Mathematical Society in Zürich during 1917. Here he announced four problems for the foundations issue. The last of these problems was on the decidability of a mathematical question by a finite procedure. As Reid suggests, the lecture might as well have been named "In praise of the axiomatic method" (1970: 151). One might say, in view of the demand for a finite, mechanical procedure, it should have been titled "In praise of the *algorithmic* method." Another eleven years were to elapse before Hilbert stated his aim for the algorithmic solvability of mathematical questions in the form which is of interest to us here.

He chose a most conspicuous venue in which to state this program, namely the 1928 International Congress of Mathematicians held in Bologna. This was the first set of international meetings to which the Germans had been invited since World War I:

At that 1928 congress, Hilbert made his

> questions quite precise. First, was mathematics complete, in the technical sense that every statement (such as "every integer is the sum of four squares") could either be proved, or disproved. Second, was mathematics consistent, in the sense that the statement "2 + 2 = 5" could never be arrived at by a sequence of valid steps of proof. And thirdly, was mathematics decidable? By this he meant, did there exist a definite method which could, in principle, be applied to any assertion, and which was guaranteed to produce a correct decision as to whether that assertion was true. (Hodges 1983: 91)

Hilbert again believed that the answers to these questions must be affirmative ones. However, the Czech mathematician Kurt Gödel was to demonstrate, in 1931, that arithmetic, for instance, could not meet the demand for completeness.[5] It is, however, how the question of decidability, or the Entscheidungsproblem, was answered by Turing that is of greater importance for the present analysis.

Alan Turing, after having turned in his dissertion at Cambridge, went on to enroll in an advanced course in the foundations of mathematics under M.H.A. Newman. Newman had heard Hilbert's 1928 address, and in the conduct of his own foundations course brought Turing up-to-date with how the completeness and consistency demands of Hilbert had been handled by Gödel. The question about decidability of mathematical systems in general and arithmetic in particular, however, still remained unanswered. Turing was to demonstrate how this question required a negative answer.

Newman had recast this third question, modifying the thrust of it from decidability as to truth or falsity to decidability as to provability. The issue of provability for Newman can be understood as something like "demonstratability." We can enunciate this issue as the question, "Is there a purely mechanical means for deciding the provability of any proposition X of a mathematical system." For the Hilbert program, as Hodges comments, it was required that the truth of assertion X "be shown by working <u>within</u> the axiomatic system" (1983: 92). Could the axiomatic system show the truth of <u>every</u> true mathematical assertion which was contained in it? If so, the system would be complete, and in one sense, then, the <u>Entscheidungsproblem</u> would be settled in the affirmative. If not, then the system would be incomplete and the <u>Entscheidungsproblem</u>, in this sense, would be settled in the negative. Of course, Gödel had already disturbed the alleged impeccability of the Hilbert system by showing arithmetic, as a system, to be incomplete:

> This was an amazing new turn in the enquiry, for Hilbert had thought of his programme as one of tidying up loose ends. It was upsetting for those who wanted to find in mathematics something that was absolutely perfect and unassailable; and it meant that new questions came into view. . . . The <u>third</u> of Hilbert's questions still remained open. (Hodges 1983: 93)

This third of Hilbert's questions deals with a second and further sense of the <u>Entscheidungsproblem</u>, namely:

> Was there a definite method, or as Newman put it, a _mechanical process_ which could be applied to a mathematical statement, and which would come up with the answer as to whether it was provable? (Hodges 1983: 93)

For this further decision problem, it is _not_ required that a decision process be effected _within_ the system: what is crucial is merely that the decision process be a mechanical or algorithmic one. Whether a proposition is provable or not within the system may be decided by a process that is not itself contained within the system. Moreover, what is being decided upon is the provability of statements and not, at least directly, their truth.

Turing exquisitely demolished the hope that Hilbert's third question could be answered in the affirmative. In doing so, he relied on the notion of Turing machines as an interpretation of mechanical or algorithmic procedures. He employed this notion to prove false the claim that _each and every_ statement of a given mathematical system could be _mechanically_ identified as provable or not, that is, identified by an algorithmic process to be provable or not.

Each Turing machine, as we know, computes a "computable number" according to its "machine configuration" or algorithm. In his "On Computable Numbers" of 1937 Turing had defined a computable number as a number the expression of which as a decimal is "calculable by finite means." For Newman's version of

Hilbert's decidability question to be answered in the affirmative, we should be able to determine, by purely mechanical, finite means, for each and every mathematical assertion of any given system, whether the assertion is provable or not. This, however, can be shown to be impossible.

In order to appreciate why this is impossible, we need to bear in mind an important fact that Gödel had established in his classic work of 1931, namely, that mathematical propositions in a mathematical language of the sort Hilbert wished to employ can be correlated one-to-one with numbers. The exact technique for establishing this correlation, called "Gödel numbering," is not a subject that requires detailed discussion in the present context. What is important is that Gödel numbering not only enables one to express propositions of a given mathematical system in terms of numbers, it also enables one to express assertions about a given mathematical system in terms of numbers. In this way, then, we can correlate the set of provable propositions of a mathematical system with a unique set of numbers, the Gödel numbers of the provable propositions of the system. It follows that the set of provable propositions of the mathematical system is ascertainable by an algorithmic procedure, that is, ascertainable by a machine, if and only if its correlated set of Gödel numbers is decidable in its entirety by an algorithmic or machine procedure.

Turing showed that there are numbers that are not machine computable, even when we understand by "machine" a machine in the broadest possible sense: a Universal Turing Machine. Such uncomputable numbers, moreover, include the number whose nth figure is 1 if n is the Gödel number of a provable proposition of arithmetic, and Ø if n is not the Gödel number of a provable proposition of arithmetic. Thus, Turing showed that there is at least one mathematical system the provable propositions of which are not machine-determinable, and that one such system is formal arithmetic. And the production of but one such counter-example is sufficient to disprove Hilbert's original hypothesis about the provability of all such systems.

In showing his result, Turing employed a version of a famous argument created about fifty years earlier by the German mathematician Georg Cantor (1845-1918). It is known as the Cantor "diagonal argument" for showing that the rational numbers (in effect, the simple common fractions) cannot be correlated one-to-one with the real numbers in their entirety.[6] Cantor showed that if we have an alleged list of all possible real numbers, and thus an alleged correlation of the real numbers with the counting numbers or positive integers, we can use this list to construct a real number which is <u>not</u> included in the list. This is a contradiction. Turing, in effect, argued analogously about the set of Gödel numbers corresponding to the provable propositions of arithmetic. If we allege that the set of these numbers is machine decidable, then we can arrange them in a list. This list may then be used to construct a Gödel number of a provable proposition of arithmetic, which number, however, is <u>not</u> in the list. This is a contradiction, and shows that the provable propositions of arithmetic are not in their <u>entirety</u> machine decidable. It is worth considering Turing's argument in a little more detail, by looking at a version of it that is quite similar to Cantor's actual diagonal argument.

Let us propose an array of Turing machines, each machine "matched" to the production of an individual number whose expression as a decimal is calculable by finite means. On the definition of the Universal Turing Machine, then, <u>each</u> <u>and</u> <u>every</u> individual number so calculable, each and every computable number, must be computable by the Universal Machine. Turing's question was, by analogy, can we find a number that is <u>not</u> computable according to this definition of computability? That is, if we can find a number whose expression as a decimal is, on this definition of computable numbers, <u>uncomputable</u> we would then--to continue Turing's analogy--have a negative answer to Hilbert's decidability question: not *every* mathematical assertion in a given system could be determined to be provable or not by a purely mechanical process. Let us now seek out such an uncomputable number.

Our task, now, shall be to illustrate by analogy Turing's technique of *reductio ad absurdum*, in this case the exposure of a contradiction between a proposition assumed to be true and at least one consequence of this proposition (Peirce 1901-5/II: 434). Assume we can mechanically compute every real number between 0 and 1 into a decimal expression. This assumption then enables us to construct at least one additional such decimal expression not among those computed. We begin a list of such mechanically computed decimal expressions and continue adding to it. We can also assign a numeric counting series to our list of decimal expressions, thereby demonstrating that, although we can continue adding new decimal expressions *ad infinitum*, the individual members of our list are nevertheless *countable*: we have an infinite but *countable* list of such decimal expressions. We also know, based on the definition of a Turing machine, that the computation of each such decimal expression is accomplished by purely algorithmic, mechanical means. Hence, even though the series of digits representing each individual decimal is, like the list of decimals itself, of infinite length, we can, at any specified time, note to which decimal place and to which decimal expression computation has proceeded:

1	.5000000000000000000 . . .
2	.3333333333333333333 . . .
3	.2500000000000000000 . . .
4	.6666666666666666666 . . .
5	.2000000000000000000 . . .
6	.1666666666666666666 . . .
7	.4000000000000000000 . . .
8	.7500000000000000000 . . .
9	.1428571428571428571 . . .
10	.6000000000000000000 . . .
11	.1250000000000000000 . . .
12	.2857142857142857142 . . .
13	.8000000000000000000 . . .
14	.1111111111111111111 . . .
15	.4285714285714285714 . . .
16	.1000000000000000000 . . .
.	. . . .
.	. . . .

(Hodges 1983: 101)

Now consider the underlined diagonal string of digits, which reads 5306060020040180 . . . Now change this diagonal number by adding 1 to each digit, with the exception that 9 converts to 0: 6417171131151291 . . . The construction of this infinite decimal, used some fifty years previous to Turing's demonstration by Cantor to display the existence of irrational numbers, is a method for identifying an uncomputable number. This number cannot be a computable number, because it differs from the first computed decimal in its first decimal place, from the fifth computed decimal in its fifth decimal place, and will, in like fashion, always differ from a given computable number. Hence, this mutated diagonal number, this number which we have created by observation of and experimentation upon a mathematical diagram, cannot <u>ever</u> be included in the list of mechanically computable numbers, no matter to what length the list is extended. On Hilbert's assumption, this diagonal number, as a computable number on Turing's definition, had to be included in the list, and yet was later shown by Turing himself <u>not</u> to be in the list: contradiction. This diagonal number, by virtue of being new, or not in "the list," is accounted for by a creative act of observation and learning rather than by a non-creative mechanical operation. To this number, then, would correspond an example of a demonstratable mathematical assertion which cannot be demonstrated by mechanical means. With this demonstration, the Hilbert decidability question is settled in the negative, and his program of mechanicalism collapses. We can of course still <u>decide</u>, pragmaticistically and creatively, and we can thus slip the snare of the mechanicalist's mistaken idea that all decisions can be made as <u>mechanical</u> decisions.

The application of Turing's argument to musical formalism is striking. We know that the components of the mechanical procedure, whether mathematical or musical, form closed and self-referential systems.

Axioms, algorithms, and conclusions that are completely, precisely, necessarily, and repeatably determined are the defining characteristics of these formalisms. The musical mechanicalist believes that his endeavors can be exhaustively axiomatized just as Hilbert believed that all of mathematics could be brought under the strictures of formalism. Again, these mathematical and musical formalisms are more than just analogies of one another, they are structurally equal. And if they are, in precisely a structural way, so alike, is there anything corresponding to the Turing diagonal number to be found in the characteristics of a given mechanical schema in music?

I would like to put to your consideration, now, two examples by way of attempting an affirmative answer to this question. The first example deals with an issue of pianistic style which has been, at various times, hotly debated: the performance of works by J.S. Bach on the piano. The observation that this issue seems, nowadays, rather passé makes it quite suitable for use in this first example. It is not an issue that many contemporary investigators seem to have much personal concern about; thus my aim is that this comparative lack of concern will allow a focus rather on the formal characteristics of the mechanicalistic structure of which it is an illustration. The object in this first illustration, then, is to compare the issue of playing Bach at the piano, as an ingredient of a mechanicalistic recipe, against evidence which was discovered and is now known by observation to be true, evidence external to the recipe or list itself. The second illustration will subsequently take this structural similarity to the Turing demonstration further, into some rather more up-to-date issues that concern pianists.

In matters of musical style, it is observably the case that the mechanicalist viewpoint treats a given style as a group of axioms to be algorithmically dispatched towards an end which is predetermined. Musicians, as we know, are often confronted with pronouncements about "<u>the</u> style of so-and-so." We have seen in this essay, for example, pronouncements about "<u>the</u> Beethoven style," "<u>the</u> Mozart style," "<u>the</u> Bartók style," and so forth (see, again, p. 67, above, and Elder 1986 at p. 38, above). Think for a moment about "<u>the</u> Bach style," in what I take to be an anachronistic formulation, and its article of faith forbidding the performance of Bach at the piano. We can begin the actual construction of a type of Turing diagram which lists axioms of "<u>the</u> Bach style," so considered:

1. Do not perform Bach on the piano.
2. Bach must always be rendered in absolutely strict tempos: no rubato.

3. Ornamentation in Bach must always conform to the realizations given in the Schirmer edition.
4. Do not ever interpolate unwritten ornaments in Bach.
5. Adhere strictly and without variation to marks of dynamics provided by your teacher.
6. Do not ever interpolate bass notes doubled at the lower octave in Bach.

. . . .

. . . .

Now, is there any item about performing Bach that we _know_ by observation to be legitimate but which cannot be mechanically generated within "_the_ Bach style," so named and so listed? Is there some aspect of knowledge about the performance of Bach which can function as a structural model of the diagonal number demonstration? That is, is there anything relating to this axiomatized, mechanical schema in music which, like the Turing project in mathematics, demonstrates that the mechanical formula is at odds with a "findable" or "demonstratable" fact or decidable proposition which, _on the terms of mechanicalism itself_, we cannot account for? Is there some aspect of playing Bach which can only be accounted for by using creative acts of observation and learning rather than turning the mechanicalist's crank? Is there a musical Entscheidungsproblem in this example?

Indeed there is such an item, which was thoroughly discussed in Christoph Wolff's July 1971 piece appearing in the Musical Quarterly. For there the newly discovered fact that in the last years of his life Bach was known not only to perform at the piano, but "happily," no less. And he also served as a sales agent, plus what we would call "engineering consultant," to the piano manufacturing concern of Gottfried Silbermann.[7] The first axiom of "the Bach style," so considered, is thus placed in jeopardy. A hypothetical objector might rejoin: "Well now, this bit of trivia about old Sebastian Bach and Silbermann's pianos is off the point of Turing's efforts. The author of the list of Bach-axioms just didn't know any better." But this so-called bit of trivia about Bach and pianos is exactly on the point, formally considered. In one sense, the thoroughgoing mechanicalist cannot allow that his or her system or method would fail to know any better: the system is supposed to contain and produce all the right answers and beliefs without need of emendation. But in another sense his or her method cannot make such emendations--this system cannot allow for coming to "know and better." Why? Because the musical mechanicalist is confronted with the same problem as the mathematical formalist: on the basis of a purely mechanical, finite system, any proposition that is truly foreign to the _system_ itself cannot be either endorsed or rejected _solely_ on the basis of the propositions which exhaust the system _itself_. As with Newman's reformulation of Hilbert's decidability question, if the method of musical mechanicalism cannot even account for a foreign but known "yes," then the method is incapable of asserting that a foreign but introduced proposition should be greeted with a "no," a "yes," or, in Peirce's language, even a "May-be."

It might well seem, at this point, that the heart of the preceding example amounted only to the fact that Christoph Wolff documented an historical detail whose acceptance into modern practice was already a foregone conclusion. So consider now a second example, one wherein the facts do not fall into a contradictory pattern as neat as the first example.

Matthay remarked in the prefatory matter to his _The Visible and Invisible in Pianoforte Technique_ of 1932 on the reception that had developed, in the previous decades, to the remarks in his _Act of Touch_ of 1903: "The basic principles of my teachings are generally accepted, and indeed have become _axiomatic_ as pianistic knowledge" (Matthay 1932: vii, emphasis added). Here, then, is a recipe, list, or "Enumeration of Touches":

1. There are eight distinct Varieties of Finger-staccato.

2. There are Six distinct Varieties of Hand-staccato (Wrist-staccato) touches.
3. There are Four Varieties of Arm-staccato Touch.
4. There are Ten distinct Varieties of Finger-tenuto (or Legato) touches.
5. There are Eight Hand-tenuto touches.
6. There are Six varieties of Arm-tenuto.

(Matthay 1911: 242-46)

Now the point is not that we seek out someone who will claim to use nine or ten varieties of finger-staccato, announce a variance of opinion therewith, pronounce a contradiction, and then abandon the subject. That would only maintain a condition of doubt. Matthay limits the number to eight, while a hypothetical claimant may expand it beyond eight. How does one choose: "What shall I do? _Are_ there but eight such varieties? Do _I_ have all eight? Do I have _enough_? Do I have _too many_?"

The point is that should someone _actually_ make such a claim, the mechanicalist recipe of Matthay for touch-forms cannot, _as a system in and of itself_, make any more of a warranted "yes or no" response in this case than other recipes could for the Bach versus piano issue or Hilbert's formalism. To _actually_ decide whether or not the claimant's charge about varieties of finger-staccato was true, we would have to do that which characterizes pragmaticism most succinctly: creative experimentation and objective inquiry. Musical mechanicalism itself is impotent in the face of such a musical _Entscheidungsproblem_.

The mechanical position cannot allow for taking in "new" or unformalized information into its system. That is, the mechanical formula is "frozen" within its own boundaries: no provision holds within it for a creative act of learning, that is, learning from experience. One such act is, namely, in the first example, taking into account and modifying one's knowledge and beliefs in accordance with the heretofore unacknowledged evidence about Bach and the piano. The mechanical approach does not contain the equipment necessary to be able to modify itself in accordance with new, experimentally derivable and observable data. An axiomatized construct in music can no more account for such new evidence, whether generated by a student, a teacher, a performer, or, in this case, an historical musicologist, than can the axiomatized mathematics of David Hilbert account for Turing's analysis. Thus, the mechanical position in music does not number among its ingredients the potential for self-correction or, to foreshadow the pending discussion of Peirce, "self-criticism."

Put another way, is not the musical mechanicalist's credo of thoroughgoing, exhaustive formalization a rather impolite fiction: a rationalizing disguise for egoism or dogmatism? Can we not make a pragmaticistically or scientifically defensible choice in favor of an anti-mechanical approach, our warrant for that being that an anti-mechanical position accounts for the development and observation of new evidence more completely than a mechanical schema? I think so. Because its apparatus *can* account for the development and observation of new evidence, because it provides for the execution of just those creative steps that *allow* for the development and observation of new evidence, the anti-mechanical position is the superior explanatory hypothesis for the activities of the musical world. It also seems clear that the anti-mechanical position can account for mechanical sorts of behaviour without jeopardizing its own self-correcting, evolutionary nature, while the mechanical posture cannot allow for a like consideration towards the open-ended, anti-mechanical viewpoint. These are among the types of issues which future work on the mechanical and anti-mechanical outlooks will continue to examine. Before engaging in a discussion of Charles Peirce and how his system can further illustrate and guide such examinations, I shall offer an additional speculation as to a possible direction in which examinations of the mechanical position could proceed. This involves the venerable debate over the so-called emotional subject matter of music: is music expressive of emotion?

Musicians who function within what I have called the musical world usually answer this question in positive fashions. Much excitement has been expended by persons of genuine philosophic ability in this matter. I do not intend, however, to become embroiled in those technical facets of aesthetics whose goal, it seems, is to put quite a fine point on precisely what the character of a given "emotion," in its expression, might be. I am inclined, as at least a provisional hypothesis, to agree with the inhabitants of the musical world and, as we shall see, with Charles Peirce, when it is said that music is expressive of emotions: some sorts of emotions expressed in some sorts of ways. Now the mechanicalist would be compelled, by force of his or her own system, to operate within an axiomatized collection of emotions which would be presented to himself or other listeners by algorithmic means. Setting aside for the moment the important question as to whether or not emotional import admits of algorithmic treatment, could this axiomatized collection of emotions ever be truly exhaustive? That is, can the mechanicalist indeed compile an exhaustive list of emotions (to be dealt with, or to be expressed by a performer or composer, or to be experienced by an audience member) by algorithmic-musical means? If one is inclined towards the finitist, mechanicalist view one is compelled to answer "yes." After all, a mechanicalist could not very well allow for the expression of an emotion not "in the list." I think the answer to the question is likely to be "no." At the very least, the opinions of Peirce and musicians in the matter should be investigated. Such an investigation would pursue the following hypothesis: if one could apply the model of Turing's method to a mechnicalist enumeration of emotions "attachable," or "matchable," as it were, to some musical experience or other, it looks like we could reasonably expect to detect "findable" or "demonstratable" emotions that would not be included "in the list." The mechanicalist could rejoin that once discovered, such a freshly observed emotional phenomenon could be subsumed into "the list." To this we should inquire: how? The mechanical view, again, has not within it the devices which could provide for the assimilation of such new evidence, or the exercise of a creative step, any more than did Babbage's engines, the proposed Peirce-Marquand machine, a Turing machine, or Hilbert's formalized mathematics.

It is time, now, to turn to the issue of how an anti-mechanical position could be seen to function in regard to these issues of experimentally observed data and creativity. To do this, let us examine certain aspects of the system of Charles Sanders Peirce.

Peirce, now recognized as perhaps America's leading native intellect, lived from 1839 to 1914. In 1859 he received his undergraduate degree from Harvard, where his illustrious father Benjamin was a faculty member in mathematics. He subsequently received a chemistry degree, _summa cum laude_, from the Lawrence Scientific School at Harvard. Concurrent with his appointment as lecturer in logic at the new Johns Hopkins University from 1879 to 1884, he was recognized by that University's head of mathematics, J. J. Sylvester, as one of the active faculty researchers of the department. In the employ of the United States Coast and Geodetic Survey from 1859 until his resignation in 1891, he performed definitive pendulum experiments towards determining relative gravity at a variety of stations around the world. Such work was directed towards a more informed understanding of the dimensions and shape of the globe. He became the first American invited to participate in an international scientific association, reporting his findings from experiments _on_ pendulums themselves to the International Geodetic Association in Paris during 1875. In 1877 he presented his cartographic masterpiece, the Quincuncial Projection of the globe, which preserved straight-line distances in a manner strikingly superior to other maps such as the Mercator Projection. Presently, there is a National Oceanographic and Atmospheric Administration research ship named after Peirce, registered as No. S-328.

Peirce contributed well over 300 reviews, mostly unsigned, to the influential literary instrument, _The_

Nation, on topics as disparate as wine tasting and nuclear physics,[8] authored over 7,000 definitions given in the monumental Century Dictionary,[9] and penned better than 80,000 sheets of his own manuscripts.[10] The collection of his lifetime publications occupies more than 12,000 microfiche exposures. He was twice married. His first wife was Harriet Melusina Fay, the sister of Amy Fay. "Zina," as Harriet was known, was pivotal in the appearance of Amy's book Music Study in Germany, a document of no small importance for research into the nineteenth-century musical figure Franz Liszt.[11] Whatever role Charles may have had in the formation of that document remains unknown. Peirce's second wife, Juliette Froissy, survived him. She, assisted by Professor Victor Lenzen, later of the Department of Physics at the University of California at Berkeley, was instrumental in the removal of the Peirce manuscripts to Houghton Library at Harvard, after Charles' death. Very little about Juliette's origin and background is known with certainty.

After 1891, Peirce lived almost constantly in penurious circumstances, and, both as intellect and personality, carried the reputation for being rather "plain spoken." Nevertheless, he had contact in one way or another with most of the major minds of his day, including such important figures as John Dewey, Oliver

Wendell Holmes, Jr., and William James. These descriptive comments, though far from exhaustive, will suggest to the reader unfamiliar with Peirce the astonishing breadth and diversity of his activities.

Peirce is also numbered among the principals of the classic period of American philosophy. Max H. Fisch, in his "Some General Characteristics of American Philosophy" of 1960, outlined the principal characteristics of this movement. Even a selected enumeration from his list shows how the present study can be seen as an instance of American philosophy, and how these characteristics can illustrate the anti-mechanical viewpoint:

> The classic period of American philosophy begins immediately after the Civil War and continues through the first half of the twentieth century . . .Central [to it] has been the problem of the nature of science, its functions, its relations to technology and to other aspects of culture, and its limitations. The leading characteristics of this movement are:
>
> 1. It has substituted philosophy of science for the traditional discipline of epistemology or theory of knowledge.
>
> 2. It has rejected the static two-term subject-object analysis of knowledge and substituted various analyses involving three terms or more, understood dynamically rather than statically.
>
> 3. It has conceived science not as consisting of propositions of sets or systems of propositions which scientists have ascertained to be true, but as consisting of the doings of scientists; not as something scientists know, but as something they know how to do; not as conclusions or results, but as method; not as knowledge but as know-how.

4. It has conceived of science as consisting primarily in what researchers or investigators do, but secondarily in what technicians do; that is, as including the whole range of the so-called applications of science. . . . The applications are a necessary part of the experimental or testing phase of science, . .because the technicians in the course of their applications are continually turning up fresh problems for the investigators.

5. It has thus conceived the organization of science not as that of a body or system of doctrine, but as that of a community, or, if I may so put it, a nest of communities: first, the relatively small community of investigators; second, the larger community of those skilled technicians who keep their practice abreast of the latest work of the community of investigators; and lastly, the still larger community of those who enjoy the fruits of investigation and application, and willingly support the investigators and technicians. The locus or residence of science, on this view, is not in the mind or consciousness of the individual scientist or student of science, but in the community at large as a going concern, so far as it is so organized as to maintain continual investigation, continual dissemination of the results of investigation, and continual modification of all other social functions by the function of investigation.

6. In this solution of the problem of the nature of science, it has found at least a partial solution of the problem of the nature of human community. If science is the best authenticated knowledge we have, and if science, which had traditionally been supposed to be an individual creation or possession, turns out to be an organization of social functions, it should be possible, from this premise, to work out a general theory of community and of the relations between the individual and the community.

7. Furthermore, it has found here, if not a solution of the more concrete and practical problems of society, at least a method for their solution. The method consists simply in giving primacy to the function of inquiry or investigation, and in diffusing the spirit and

> the results of inquiry through all the institutions of society, beginning with the educational institutions.
>
> 8. It has asserted that the way to begin with the educational institutions is to reduce the emphasis on lectures, textbooks, and drill, and to increase the emphasis on laboratory work, field work, library work, and group discussion. . . .
>
> The most striking development of the last decade or two has been the increasing attention to the philosophy of art. Here again, art is conceived as what artists do, and what others do with what artist have done or made, rather than as objects immediately apprehended as having certain distinctively aesthetic characters such as that of beauty. . . . That is, our philosophers are thinking about art in the same way as that in which they have thought about science. (Fisch 1986: 111-113)

In the spirit of American philosophy and pragmaticism, then, the present study will remain open-ended. The methodology of this examinational process, or, if you will, the means of proceeding, was described rather succinctly by Peirce in his 1892 essay for the Monist, "The Doctrine of Necessity Examined":

> The conclusions of science make no pretense to being more than probable, and considering that a probable inference can at most only suppose something to be most frequently, or otherwise approximately, true, but never that anything is precisely true without exception throughout the universe. (CP 6.39)

Recognizing the basic characteristics of pragmatism and scientific method as kindred notions, the procedures of anti-mechanicalism in music can be further illustrated in terms of Fisch's tenets of classic American philosophy. (1) The anti-mechanical viewpoint substitutes the method of pragmatism for the traditional mechanical approach. (2) It has rejected the finitist procedures of mechanicalism that allow for only the two characterizations of a musical endeavor as "correct" and "perfect" execution or "incorrect" and "imperfect" execution. (3) It conceives of the activities of music not as consisting of mechanical routines that reach finitist conclusions which can only be counted as being "true" or "false." (4) It conceives of the activities of music, properly considered, as not merely the issuance of mechanical edicts and the rigid following of them: such "rules" must submit to such contrary results as their applications may divulge. (5) It has thus conceived the organization of the musical world not as a finally codified body of doctrine, but as an investigatory process. (6) In being a process involving communities of investigators, it rejects the egoism of the mechanicalist's dogma. (7) Furthermore, it has found here, if not the supposedly unassailable veracities of the mechanical viewpoint, rather a means for proceeding that places primacy on the function of inquiry and investigation. (8) It asserts that the way to conduct musical activities involves a reduction in the emphasis on method books, cookbook procedures for the acquisition of technical skills, drill, and the like, in favor of pragmaticistic procedures.

Thus, our musicians are thinking about and doing their art in the same way that the pragmatists have thought about and done science, philosophy, mathematics, and other fields of inquiry.

A more specific definition of mechanicalism as a philosophy was given by Peirce, again citing from his "Doctrine of Necessity Examined." The specific subject under scrutiny here is the behaviour of particles in motion:

> The proposition in question is that the state of things existing at any time, together with certain immutable laws, completely determine the state of things at every other time. . . . Whoever holds that every act of the will as well as every idea of the mind is under the rigid governance of a necessity coordinated with that of the physical world will logically be carried to the proposition that minds are part of the physical world in such a sense that the laws of mechanics determine anything that happens according to immutable attractions and repulsions. In that case, that instantaneous state of things, from which every other state of things is calculable, consists in the positions and velocities of all the particles at any instant. This, the usual and most logical form of necessitarianism, is called the mechanical philosophy. (<u>CP</u> 6.37-.38)

We can now be even more specific in our description of mechanicalism, by introducing an additional term from mathematics. This term is the word "calculus." For an understanding of this term we shall turn to an excerpt from Peirce's 1906 address to the National Academy of Science. The subject under consideration was logic:

> The great misconception of the majority of non-logicians, and I fear a good many logicians with them, is that the great purpose of a logical algebra or other system affiliated to logical algebra is to serve as a calculus, that is a contrivance for deducing conclusions from premisses by means of a routine of transformations. . . . A calculus, in the sense of the definition I just gave, a system of signs, enabling a person by following a routine of rules, to solve any problem of a given kind in order to fulfill its purpose to perfection, should pass from premiss to conclusion in the smallest number of steps possible. (Peirce 1906/MS 498: 07-09)

We can see from these two quotations, the first addressing the term "mechanical philosophy" and the second defining the term "calculus," that these two terms have a great affinity for one another. The mechanical outlook thus assumes the employment of a calculus, the following of a routine of rules that will allegedly solve the problem to perfection.

Now we can formulate--using observations from Gieseking, and others, and the two citations from Peirce on the mechanical philosophy and the calculus offered earlier--a more pointed understanding of musical mechanicalism and the musical calculus. Thus: 1) the proposition in question is that the musical state of things at any time, be it learning, teaching, or performing, together with a prescriptive method, can completely determine the musical state of things at every other time. This, the usual and most logical form of musical necessitarianism, I call the philosophy of musical mechanicalism. Thus, 2) the great misconception of the majority of non-musicians, and I fear a good many teachers, students, and performers of music, is that the great purpose of methods of learning and performing music or of other systems affiliated to them is to serve as a musical calculus, that is, a contrivance for producing the learning and performing of music by means of a non-varying routine of transformations. A musical calculus, thus understood, allegedly enables the subject by following a prescriptive method, to solve every musical problem of a given kind to perfection.

In opposition to this, we can observe Schnabel as an experimentalist. In line with a pragmaticistic, experimental philosophy, Schnabel considers both the teaching studio and the concert platform to be experimental laboratories. To wit, the facts are displayed, the experiment is announced and conducted, and the results are observed. These results then form a set of facts to be employed in further experimentation.

Thus, new discoveries and observations are involved in what is an ongoing process instead of a pre-set formality. As an ongoing process, the business of the teacher and performer involves passage towards objectives that may well never come fully to hand. In Schnabel's language, then, we continue our experiments

"from seemingly simple and modest aspirations

> by way of increasing (even frightening) complications . . .toward that other shore which, to be sure, can only be sighted but never reached" (Schnabel 1942: 14).

This sort of anti-mechanicalistic approach thus has as its methodology not a musical calculus, but instead what I would like to term a musical diagram or model. What is meant by this new term, musical diagram? The anti-mechanicalist proceeds, as we have seen, by displaying the facts at hand, performing experimentation on them, and subsequently observing the results. Now one of Peirce's more fecund insights concerns an important characteristic of our processes of thinking, that of thinking according to mentally constructed patterns, or diagrams. We can, for our purposes, understand such mental diagrams as the arrangements or locations in our thinking processes where we "display" the musical facts to be experimented upon. For the musical world as I have delimited it, the facts to be so diagrammatically formed could include, obviously, the very arrangements of sounds with which the pianist deals. Peirce certainly did not limit the formation of such mentally constructed diagrams to the purely visual. Peirce commented on this, in an 1892 essay entitled "Critic of Arguments," and also explained that the formation of and experimentation upon such a diagram, or body of facts, is termed an algebra:

> Such a diagram has got to be either auditory [11] or visual, the parts being

> separated in the one case by time, in the other in space. But in order completely to exhibit the analogue of the conditions of the argument under examination, it will be necessary to use signs or symbols repeated in different places and in different juxtapositions, these signs being subject to certain "rules," that is, certain general relations associated with them by the mind. Such a method of forming a diagram is called algebra. All speech [for example] is but such an algebra. (*CP* 3.418)

Thus, whereas the mechanicalist philosophy relies on the employment of a mechanical calculus of one sort or another, the anti-mechanicalist system uses the experimental instrument of diagrams or models, one variety of which is termed "algebra." To my knowledge, music is not usually considered by musicians as involved with reasoning. However, I am now prepared to state that the workings of such musical diagrams can be considered as types of *musical reasoning*.

Viewed from the standpoint of Peirce's system, both the paradigmatic Gieseking and Schnabel programs, for instance, are examples of reasoning. They are, however, forms of reasoning at odds with one another. Having already characterized them as employing, respectively, deterministic algorithms as opposed to inductive, scientific, pragmaticistic procedures, we can further characterize their conflicting natures as examples of two kinds of reasoning patterns Peirce distinguished: corollarial versus theorematic. These forms of reasoning were considered by Peirce to be forms of deduction, deductive reasoning being seen as the most appropriate form to mathematical activities. In a letter of 28 December 1909 to William James, Peirce announced:

> I first found, and subsequently proved, that every Deduction involves the observation of a Diagram (whether Optical, Tactical, or Acoustic[11]) and having drawn the diagram (for I myself always work with Optical Diagrams) one finds the conclusion to be represented in it. Of course a diagram is required to comprehend any assertion. My two genera of Deductions are 1st those in which any Diagram of a state of things in which the premisses are true represents the conclusion to be true [Babbage, Peirce-Marquand or Turing machines; Hilbert's or Gieseking's formalism] and such reasoning I call Corollarial because all the corollaries that different editors have added to Euclid's Elements are of this nature. 2nd Kind. To the Diagram of the truth of the Premisses something else has to be added, which is usually a mere May-be and then the concluusion appears [Schnabel's pragmaticism]. I call this Theorematic reasoning. (NEM III/2: 869-870)

The reasoning processes of the musical mechanicalist are then indeed structurally like the algorithmic exercises of a Babbage engine, the Peirce-Marquand machine, a Turing machine, or a Hilbert formalism: corollarial. The reasoning process of the anti-mechanicalist in music, however, can now be seen as the type involving the creative manipulation of new data or new hypotheses; the type allowing that "something else has to be added, which is usually [of the type of] a mere May-be": theorematic.

Additional citations from Peirce can further illustrate the differences between mechanical and anti-mechanical positions. Almost a quarter-century before his letter to James announcing his distinction of corollarial from theorematic reasoning, Peirce outlined one of the disadvantages of mechanicalism:

> It is my fate to be supposed an extreme partisan of formal logic [mechanicalistic logic], and so I began. But the study of the logic of relations has converted me from that error. Formal logic centers its whole attention on the least important part of reasoning, a part so mechanical that it may be performed by a machine, and fancies that that is all there is in the mental process. For my part, I hold that reasoning is the observation of relations, mainly by means of diagrams and the like. It is a living process. This is the point of view from which I am conducting my instruction in the art of reasoning. I find out and correct all the pupil's bad habits in thinking: I teach him that reasoning is not done by the unaided brain, but needs the cooperation of the eyes and hands. Reasoning, as I make him see, is a kind of experimentation, in which, instead of relying on the intelligible laws of outward nature to bring out the result, we depend upon the equally hidden laws of inward association. I initiate him into the art of this experimentation. I familiarize him with the use of all kinds of diagrams and devices for aiding the imagination (Peirce 1887b)[12]

We can rephrase these remarks by way of placing musical mechanicalism in a new perspective. Musical mechanicalism centers its whole attention on the least important part of musical reasoning, a part so mechanical that it may be performed by a machine, and fancies that that is all there is in the musical process of reasoning. An example of such a form of musical reasoning, that is, the corollarial form, would be the matching of a given mark on the musical score to the appropriate key on the piano: an execution so mechanical that it may be performed by a machine. The anti-mechanicalist, on the other hand, holds that musical reasoning is rather the observation of musical relations, for instance the relations of the musical sounds themselves, and that these relations are observed and experimented on by means of diagrams or models. This, the theorematic form of musical reasoning, is a living process and not a mechanical formalization. This is the standpoint from which the musician conducts his or her investigations into the art of music. Bad habits are detected and worked on by a seemingly indirect means. That is, musical reasoning of this form is not done by a mechanical execution of the unaided brain, but needs the cooperation of our sensory faculties. Hence, theorematic musical reasoning, as the musician comes to acknowledge, is a kind of experimentation in which, instead of relying on the intelligible laws of algorithmic execution, he or she depends instead on the equally hidden principles of his or her own faculties of inward association and diagrammatic observation. By continuing an earlier citation from Peirce's "Our Senses as Reasoning Machines," we gain a further understanding of this idea of that form of musical reasoning which is theorematic rather than corollarial, pragmaticistic rather than mechanical:

> In genuine reasoning, we are not wedded to our method. We deliberately approve it, but we stand ever ready and disposed to reexamine it and to improve upon it, and to criticize our criticism of it, without cessation. Thus the utility of the word "reasoning" lies in its helping us to discriminate between the self-critical and uncritical formations of representations. If a machine works according to a fixed principle involved in the plan of it, it may be a useful aid in reasoning; but unless it is so contrived that, were there any defect in it, it would improve itself in that respect, then, although it could correctly work out every possible conclusion from premisses, the machine itself would afford no assurance that its conclusion would be correct. Such assurance could only come from our critical examination of it. Consequently, it would not be, strictly speaking, a reasoning-machine.
>
> Self-criticism can never be perfectly thorough for the last act of criticism is always itself open to criticism. But as long as we remain disposed to self-criticism and to further inquiry, we have in this disposition an assurance that if the truth of any question can ever be got at, we shall eventually get at it. (Peirce 1900/MS 831: 11-12)

In his "On Quantity, with special reference to Collectional and Mathematical Infinity" of 1896, Peirce gave an outline of the processes of diagrammatic or modeling thought. This outline can also describe theorematic musical reasoning, diagrammatic thought of a musical variety:

> Modern exact logic shows that every operation of deductive reasoning consists of four steps as follows:
>
> 1st, a diagram, or visual image, [or auditory image] whether composed of lines, like a geometrical figure, or an array of signs, like an algebraical formula, [or an arrangement of sounds, like a musical performance] or of a mixed nature, like a graph, is constructed, so as to embody in iconic form, the state of things asserted in the premise.

> 2nd, Upon scrutiny of this diagram, the mind is led to suspect that the sort of information [expression] sought may be discovered, by modifying the diagram in a certain way. This experiment is tried.
>
> 3rd. The results of the experiment [rehearsal; performance] are carefully observed. This is genuine experiential observation, even though the diagram exists [perhaps only for a moment] in the imagination, for after it has once been created, though the reasoner [musician] has power to change it, he has no power to make the creation already past and done different from what it is. It is, therefore, just as real an object as if it were drawn on paper. Included in this observation is the analysis of what is seen [heard] and the representation of it in general language. What is so observed is a new relation between the parts of the diagram not mentioned in the precept by which it was constructed.
>
> 4th, By repeating the experiment, or by the similarity of the experiment to many others which have often been repeated without varying the result, the reasoner infers inductively, with a degree of probability practically amounting to certainty, that every diagram constructed according to the same precept would present the same [general] relation of parts which has been observed in the diagram experimented upon. (Peirce 1896/MS 15: 19-20)

This kind of musical reasoning, involving creative and ongoing experimentation upon the musical diagram or model, differs sharply from the limiting protocols of the mechanical form of reasoning, the form which represents the inferior explanatory hypothesis:

> Every reasoning machine, that is to say, every machine has two inherent impotencies. In the first place, it is destitute of all originality, of all initiative [creativity]. It cannot find its own problems; it cannot feed itself. It cannot direct itself between different possible procedures. For example, the simplest proposition of projective geometry, about the ten straight lines in a

> plane, is proved by von Staudt from a few premisses and by reasoning of extreme simplicity, but so complicated is the mode compounding these premisses and forms of inference, that there is no less than 70 or 80 steps in the demonstration. [See Gieseking or Matthay on the production of pianistic perfection.] How could we make a machine [musician] that would automatically thread its way through such a labyrinth as that? And even if we did succeed in doing so, it would still remain true that the machine would be utterly devoid of original initiative, and _would only do the special kind of thing it had been calculated to do_. This, however, is no defect in a machine [as such]; we do not want it to do its own business, but ours. The difficulty with the balloon, for instance, is that it has too much initiative, that it is not mechanical enough. We no more want an original machine, than a house-builder would want an original journeyman, or an American board of college trustees would hire an original professor. If, however, we will not surrender to the machine, the whole business of initiative is still thrown upon the mind, and this is the principal labor.
>
> In the second place, the capacity of a machine has absolute limitations; _it has been contrived to do a certain thing, and it can do nothing else_. (Peirce 1887a: 168-9, emphases added)

By way of speculation and announcing an avenue for future investigation, I shall here put forward the idea that the anti-mechanicalist or pragmaticist in music, in experimenting and reasoning upon what I have termed musical diagrams or models, need not limit the effect of these reasonings only to the exhibitions of various combinations of musical sounds themselves. Why could not the notion of music as expressive of emotion be included when working with musical diagrams in such theorematic, self-critical ways? Could not these musical diagrams or arrangements of tones be seen to mediate between the performer or composer and the auditor in matters of the expression of emotional relations? That is, I think the idea that the anti-mechanical inhabitants of the musical world, as I have defined it, perform theorematic reasonings on diagrams which seek to model, iconize, or signify emotional relations, is an explanatory hypothesis of great value. Just how might such theorematic reasoning in music be accomplished?

Peirce was certainly in agreement with musicians who claim music to be expressive of emotion when he observed that " . . .the meaning of a piece of music is the play of feeling it produces" (Peirce 1907/ MS 319: 09). Just as we have found his accounts of reasoning helpful in clarifying the general phenomenology of music, so too he apparently looked upon music as a clear illustration of the processes of reasoning-- reasoning requiring, as we know, the use of diagrams. This observation on the role of music as a model for Peircean reasoning must be left as but an hypothesis for now. My speculation that his version of experimental deduction, theorematic reasoning, forms a model for anti-mechanicalism in music, however, can bear further examination. Certain remarks in one of his "Pragmatism" manuscripts of 1907 can help clarify the mediating role of the musical diagram in the expression of emotion:

> A concerted piece of music brings . . .a succession of musical emotions, formed out of a succession of sounds, as the interpretant of corresponding musical ideas on the part of the composer [or performer] . . .[Such] a piece of concerted music is evidently a sign [or musical diagram]. For it mediates between the series of musical feelings and emotions of the composer [or performer] and those of the auditor. It conveys; namely it conveys feelings. (Peirce 1907/MS 318: 299, 201)

Theorematic reasoning by the performer would then appear to be something like the following. The performer, whether a composer per se or what I call a "re-composer," deals with the expression of some sorts of emotions which shall be expressed, musically, in some sorts of ways. As such, the active participation in this kind of musical experience by the performer yields experiments on the musical diagram (whether the diagram be a sonic pattern existing only in the mind, recorded on paper, or one embodied in matter vibrating at some audible frequency) that accord with the same tenets of diagrammatic thought Peirce outlined for what he termed "modern exact logic" in his "On Quantity" of 1896, above. The performer, therefore, theorematically modifies the musical diagram, the "concerted piece of music," and thus can suggest to the auditor's "own faculties of inward association" modifications in his or her own "succession of musical emotions," accordingly. Consider the following illustration. An auditor finds him or herself in the following situation: the auditor goes to the concert hall, prepared to hear yet again an old standard of the repertoire, and perhaps what he or she is about to hear will be an example of "<u>the</u> style of so-and-so" such as I mentioned earlier. The auditor thinks he knows beforehand, then, how, as it were, "it's supposed to go," both tonally and emotionally speaking. He finds the performer not, however, to be of the mechanistic persuasion that such repetitions of how "it's supposed to go" require. This performer is not taking <u>the</u> style of so-and-so, finitistically defined, as an axiom in the formalized sense of a Babbage engine, the Peirce-Marquand machine, a Turing machine, or Hilbert's mathematics. The auditor experiences inward <u>surprise</u>. This performer, supposedly using a well-worn piece of music as his or her venue for Peircean diagrammatic thought, is found to be conducting an experiment whose tonal and emotional ideas, to cite from Schnabel, "can be efficacious on one single occasion only . . . , [have] to emerge afresh on each new occasion, and . . .can never become automatic." By engaging in a pragmaticistic inquiry guided in part by self-criticism, this performer has experimentally developed and, on my account, <u>reasoned out</u> new musical and emotional evidence which no mere turning of the mechanicalist's crank could produce.

A final citation from Schnabel's <u>Music and the Line of Most Resistance</u>, continued from earlier, concerns the dichotomy he observed many pianists to make between instrumental technique and other aspects of the musical universe. If we allow ourselves to read between the lines even only a bit, however, we can see the opposition of mechanicalism and anti-mechanicalism clearly displayed yet again. Here, Schnabel decries the isolated acquisition of instrumental technique. He has in mind an algorithmic system like that of Gieseking's, one which, in Peirce's language, "[can] only do the special kind of thing it [has] been calculated to do, . .and it can do nothing else":

> Yet there is something in musical performances, which invites performers to exhibit dexterity. Brilliant effect is possible simply with speed or noise, or speed and noise. But since this kind of effect is fairly sure to be produced, while the effect produced by solutions of artistically nobler tasks is not so sure (unless the task is well solved [11]), the preference of the safer line becomes quite understandable. This brings up that very strange confusion . . .about the different degrees of difficulty of execution. Only that is really difficult, which cannot be learned, or at least which cannot be learned by incessant practising. There is no rivalry between perseverance (the capacity to sit, if it is a question of the piano) and genius (which is located elsewhere). A genius could scarcely be expected to spend hours and hours every day just in order to train his fingers and muscles. Also, that would be quite futile and superfluous, except for the satisfaction of athletic ambitions. No sportsman, by the way, trains after the fashion of a musical technician. The marathon runner does not start his daily training by first walking slowly for an hour, no tennis champion begins with high and soft shots. Musical performers have been intimidated by an error of instruction first

> committed in the nineteenth century, and which is still being committed now. To isolate the acquisition of technique from the subject it will later have to serve is nonsense. (Schnabel 1942: 75-76)

I bring you, then, to a question about the mechanical philosophy of music which is, I think telling: have musical performers been intimidated by the mechanical philosophy to such an extent that their goal has become the cultivation of a "safer line" not unlike that of David Hilbert's formalism: the alleged mere tidying of "loose ends?" Does not the mechanical philosophy ignore, neglect, or suppress both the element of pragmaticism and the very pathology of doubt in the phenomenology of music I have investigated in this essay?

Following on the second page below are offered some direct comparisons of the mechanical and anti-mechanicalist points of view (Illus. 4-1). The mechanicalist, as we have seen, uses a musical calculus in a prescriptive, deterministic, recipe-oriented manner. His or her procedure seeks an insured, uniform, exactly repeatable product. As I suggested earlier, this belief in an insured, uniform product seems rather like an unfortunate fiction. The idea of a single, absolutely correct manner of conducting the affairs of the musical world seems to harbor within it a glaring self-contradiction. It also looks like the mechanicalist abhors any sort of so-called "error" in the learning or performance of music, and, in complete line with this, attempts to indemnify against any sort of risk whatsoever in the activities at hand. The mechanicalist relies on what we can term <u>antecedent</u> knowledge. That is, the mechanicalist relies on a belief that all the issues in a musical activity, whether they be learning or performance, are decidable and have been decided in advance. He has supreme confidence that what I have termed the musical <u>Entscheidungsproblem</u>, in the largest sense, requires an affirmative answer. This confidence in the state of affairs of the musical/mechanical universe seems to inevitably lead to an abrupt egoism, including pleadings from consensus and authority, and can lead him to trade off the developmental posture of anti-mechanicalism for static exhibitions of ego. The anti-mechanicalist, however, employs a musical diagram or model in a descriptive, non-deterministic, experimental manner. Instead of placing primacy on insurance and uniformity, he seeks rather to interpret and illuminate variety. The anti-mechanicalist additionally appreciates the value of error in learning and performance, and in agreement with this embraces the productivity of risk. In terms of the experimental, pragmaticistic outlook of Charles Peirce, the anti-mechanicalist sees his or her knowledge of any current affairs as provisional and <u>consequential</u> rather than antecedent and predetermined.

Mechanicalism	Anti-Mechanicalism
prescriptive	descriptive
deterministic	non-deterministic
algorithms	experiment
insured conclusion sought	consequences interpreted
abhors "error" in learning/ performance	appreciates value of error in learning/ performance
attempts insurance against risk	treats "risk" as of generative value
exhibitor (ego)	transmitter (non-ego)
antecedent knowledge	consequential knowledge
"Betty Crocker" method; blueprints	Peirce's pragmaticism
formalization; closed systems	variety; open-ended systems that account for new developments
egoism	realism

Illus. 4-1. Comparisons of Mechanical and Anti-Mechanical Points of View.

Further affinities of the musical anti-mechanicalist for the philosophical realism of Charles Peirce, like many other items raised in this essay, remains a topic for future investigation.

I conclude, with your indulgence, by paraphrasing the citations of Russell and Feibleman with which this section began. The wish for perfect security is one of those snares that the musical mechanicalist is always falling into, and likewise it is a wish the mechanicalist claims can always be granted. Nevertheless, it is just as untenable in the realm of musical phenomenology as in mathematical formalism or reasoning itself, on Peirce's account. By ignoring or denying the value of experimental method, or the pragmaticism of Charles Peirce, music today could surely become a dead field.

Notes

1. See Reid 1970 and Hodges 1983. Reid's book, arguably one of the better expositions on Hilbert in English, nevertheless suffers from a pronounced lacuna of references. No bibliography is supplied, only seventeen notes are given for the entire work, and the index is but a rudimentary compilation of names. This situation thwarts thorough documentation of the many telling remarks by Hilbert which Reid supplies. I therefore enjoin the caveat given by Hodges at his note 2.9, "Here and elsewhere I have drawn upon C. Reid, Hilbert . . .for quotations."

2. In Euclid's Elements these two statements are distinguished, technically, as a "postulate" and an "axiom," respectively. Modern usage, generally, does not preserve this distinction (see Barker 1967: 285).

3. At Parsons 1967: 190 is given an explanation as to why such an exhaustive formalism remained beyond Euclid's grasp.

4. At the urging of his colleagues Oskar Minkowski and Adolf Hurwitz, Hilbert shortened his remarks by actually stating only ten of the twenty-three questions he had prepared: nos. 1, 2, 6, 7, 8, 13, 16, 19, 21, and 22. The text of Hilbert's address, translated with his permission, was preserved by Dr. Mary Winston Newson in the Bulletin of the American Mathematical Society, vol. 8 of 1902. Apparently Minkowski was of the opinion that the formation of this list of "foundations" questions for mathematics would ensure Hilbert's position in the history of mathematics: he was right.

5. See Gödel's "Uber formal unentscheidbare Sätze der Principia Mathematica und verwandter Systeme I," Monatshefte für Mathematik und Physik, 38 (1931): 173-198, preserved as "On Formally Undecidable Propositions of Principia Mathematica and Related Systems I," in Davis 1965.

6. More complete definitions of the important terms for this example are given under the appropriate entries in Baker 1961. A rational number is a number which can be expressed in the form p/q where p and q are integers and q is not zero (1/2, 1/3, 1/4, and so forth). An irrational number is a number which cannot be so expressed as an integer or the quotient of integers (pi, the square root of 2, and the like). A real number may be rational of irrational.

7. See Wolff 1971, especially p. 401*n*43, and p. 403*n*45, *n*46 for further details of the situation.

8. Preserved in Ketner and Cook, 1975-79.

9. The titles of these definitions are compiled at Ketner with Stewart 1986: entry P 00373. The definitions themselves are preserved on the microfiche accompanying P 00373: fiche 47-88 and supplemental fiche 8-9.

10. A complete set of these manuscripts, in reproduced form, is held at the Institute for Studies in Pragmaticism.

11. A new contribution on the subject of the Fay sisters is McCarthy 1986. This book skirts again the fact that Charles sued Zina for divorce on the grounds of desertion (see there p. xi).

Sister McCarthy is presently preparing a biography of Amy Fay, extensively using materials from the Preston H. Tuttle Collection and Max H. Fisch Papers of the Institute for Studies in Pragmaticism. See also III*n*13, above.

12. The original of Peirce's letter of 29 March 1887 to Mr. J.M. Hantz, Northwestern University, Watertown, Wisconsin, has been lost. However, Max Fisch, Senior Editor at the Peirce Edition Project in Indianapolis, preserved it in typed-transcript form.

REFERENCES

Apel, Willi 1961. _The Notation of Polyphonic Music, 900-1600._ 5th ed., revised. Cambridge: Mediaeval Academy of America.

Baldwin, J.M., ed. 1901-5. _Dictionary of Philosophy and Psychology._ 3 vols. in 4. New York: Macmillan.

Barker, Stephen F. 1967. Geometry. In Edwards 1967/3: 285-290.

Beddoes, Thomas 1792. _Observations on the Nature of Demonstrative Evidence; with an Explanation of Certain Difficulties occurring in the Elements of Geometry._ London: J. Johnson.

Brody, Boruch 1967. Logical Terms, Glossary of. In Edwards 1967/5: 57-77.

Brower, Harriette 1926. _Modern Masters of the Keyboard._ New York: Frederick Stokes. Reprinted 1969. Freeport, N.Y.: Books for Libraries Press.

Brown, Anthony Cave 1975. _Bodyguard of Lies._ New York: Harper & Row.

Budd, Malcolm 1985. _Music and the Emotions: the Philosophical Theories._ London: Routledge & Kegan Paul.

Burke, James 1985. _The Day the Universe Changed._ Boston: Little, Brown and Company.

Burrowes, J.F. 1877. _Burrowes' Piano-Forte Primer; Containing the Rudiments of Music._ Philadelphia: Lee and Walker.

Calkins, Mary Whiton, ed. 1957. _Locke's Essay Concerning Human Understanding._ La Salle, IL: Open Court.

Clark, Ronald W. 1984. _The Survival of Charles Darwin: A Biography of a Man and an Idea._ New York: Avon Books.

Cohen, H.F. 1984. Quantifying Music: The Science of Music at the First Stage of the Scientific Revolution, 1580-1650. Dordrecht, Holland: D. Reidel.

Collingwood, Robin G. 1938. The Principles of Art. Oxford: Clarendon Press.

Copi, Irving M. 1982. Introduction to Logic. 6th ed. New York: Macmillan.

Corrigan, John 1986. Remembering Walter Gieseking. Clavier 25/9: 12-16.

D'Abreu, Gerald 1964. Playing the Piano with Confidence. New York: St. Martin's Press.

Davis, Martin 1958. Computability and Unsolvability. New York: McGraw-Hill.

Davis, Martin, ed. 1965. The Undecidable: Basic Papers on Undecidable Propositions, Unsolvable Problems and Computable Functions. Hewlett, NY: Raven Press.

Davis, Philip J., and Reuben Hersh 1981. The Mathematical Experience. Boston: Houghton Mifflin.

Dudley Observatory Scientific Council 1858. Defence of Dr. Gould by the Scientific Council of the Dudley Observatory. Albany: Dudley Observatory.

Dudley Observatory Trustees 1858. The Dudley Observatory and the Scientific Council. Statement of the Trustees. Albany: Dudley Observatory.

Dudley Observatory 1866. Annals of the Dudley Observatory. Albany: Dudley Observatory.

Dutton, Denis, and Michael Krausz, eds. 1981. The Concept of Creativity in Science and Art. The Hague: Martinus Nijhoff.

Eames, Charles and Ray 1973. A Computer Perspective. Cambridge: Harvard University Press.

Edwards, Paul, ed. 1967. The Encyclopedia of Philosophy. New York: Macmillan.

Eisele, Carolyn, ed. 1976. The New Elements of Mathematics by Charles S. Peirce. 4 vols. in 5. The Hague: Mouton.

Eisele, Carolyn 1979. Studies in the Scientific and Mathematical Philosophy of Charles S. Peirce. R.M. Martin, ed. The Hague: Mouton.

Eisele, Carolyn, ed. 1985. Historical Perspectives on the Logic of Science: A History of Science. Berlin: Mouton.

Elder, Dean 1982. Pianists at Play. Evanston: The Instrumentalist Co.

------ 1986. Fanny Waterman: Leeding Lady. Clavier 25/7: Ø6-11.

Feibleman, James K. 1981. The Third Sophistic. Presidential Address to the Southwestern Philosophical Society, 14 November, at San Marcos, TX. Typescript held at Texas Tech University: Institute for Studies in Pragmaticism.

Fisch, Max Harold n.d. The Max H. Fisch Papers. Held at Texas Tech University: Institute for Studies in Pragmaticism.

Fisch, Max Harold 1986. Peirce, Semeiotic, and Pragmatism. K.L. Ketner and C.J.W. Kloesel, eds. Bloomington: Indiana University Press.

Fisch, Max H., Edward C. Moore, and Christian J.W. Kloesel, eds. 1982-. Writings of Charles S. Peirce: A Chronological Edition. Bloomington: Indiana University Press.

Gavoty, Bernard, and Roger Hauert 1955. Walter Gieseking. Geneva: Rene Kister.

Gillispie, Charles Coulston, ed. 197Ø. Dictionary of Scientific Biography. New York: Charles Scribner's Sons.

Harley, Rev. Robert 1879. The Stanhope Demonstrator, an Instrument for Performing Logical Operations. Mind 4: 192-21Ø.

Harris, James 1771. Hermes. 3rd ed. London: I. Novrse and P. Vaillan.

Hartshorne, Charles, Paul Weiss, and Arthur Burks, eds. 1935, 1958. Collected Papers of Charles Sanders Peirce. 8 vols. in 4. Cambridge: Harvard University Press.

Heath, Sir Thomas L., trans. 1956. *The Thirteen Books of Euclid's Elements*. 3 vols. New York: Dover.

Hilbert, David 1900. Mathematical Problems. Mary Winston Newson, trans. *Bulletin of the American Mathematical Society* 2nd ser. 8: 437-479.

Hodges, Andrew 1983. *Alan Turing: The Enigma*. New York: Simon and Schuster.

Hopcroft, John E. 1984. Turing Machines. *Scientific American* 250/5: 86-98.

Jevons, William Stanley 1870. On the Mechanical Performance of Logical Inference. *Philosophical Transactions of the Royal Society* 160: 495-518.

Ketner, Kenneth Laine, and James Edward Cook, eds. 1975-79. *Charles Sanders Peirce: Contributions to the Nation*. 3 vols. plus forthcoming Index vol. Lubbock: Texas Tech Press.

Ketner, Kenneth Laine, with Arthur Franklin Stewart 1984. The Early History of Computer Design: Charles Sanders Peirce and Marquand's Logical Machines. *The Princeton University Library Chronicle* 45: 188-224.

------ 1986. *A Comprehensive Bibliography of the Published Works of Charles Sanders Peirce*. Revised edition with expanded microfiche collection of Peirce's lifetime publications. Bowling Green, Ohio: Philosophy Documentation Center.

------ 1987. Peirce and Turing: Comparisons and Conjectures. *Semiotica*, in press.

Langer, Susanne K. 1957. *Philosophy in a New Key*. Cambridge: Harvard University Press.

Lavin, Marilyn A. 1983. *The Eye of the Tiger: The Founding and Development of the Department of Art and Archaeology, 1883-1923*. Princeton: The Department of Art and Archaeology and the Art Museum.

Leimer, Karl, and Walter Gieseking 1932. *The Shortest Way to Pianistic Perfection*. Philadelphia: Theodore Presser. Reprinted 1972 as Gieseking, Walter, and Karl Leimer. *Piano Technique, including the two complete books The Shortest Way to Pianistic Perfection and Rhythmics, Dynamics, Pedal and Other Problems of Piano Playing*. New York: Dover

Lhevinne, Josef 1924. Basic Principles in Pianoforte Playing. Philadelphia: Theodore Presser.

Marcus, Adele 1979. Great Pianists Speak. Neptune, N.J.: Paganiniana Publications.

Marquand, Alan 1886. A New Logical Machine. Proceedings of the American Academy of Arts and Sciences 21: 303-307.

Matthay, Tobias 1903. The Act of Touch in All its Diversity: an Analysis of Pianoforte Tone-production. London: Longmans, Green, and Co.

------ 1913. Musical Interpretation: Its Laws and Principles, and their Application in Teaching and Performing. Boston: Boston Music Co.

------ 1932. The Visible and Invisible in Pianoforte Technique. London: Oxford University Press.

Mays, Wolfe 1953. The First Circuit for an Electrical Logic Machine. Science 118: 281-82.

McCarthy, Sister Margaret W., ed. 1986. More Letters of Amy Fay: the American Years, 1879-1916. Detroit: Information Coordinators.

Parsons, Charles 1967. Mathematics, Foundations of. In Edwards 1967/5: 188-213.

Peirce, Charles Sanders 1854-1914. Unpublished manuscripts. Texas Tech University: Institute for Studies in Pragmaticism.

------ 1871. Notes (on the death of Mr. Charles Babbage). The Nation 15: 307. [P 66a]

------ ed. 1883. Studies in Logic. By Members of the Johns Hopkins University. Boston: Little, Brown & Co.

------ 1886. Letter to Allan Marquand of 30 December 1886. The Allan Marquand Papers: Princeton University Library.

------ 1887a. Logical Machines. The American Journal of Psychology 1: 165-170. [P 344]

------ 1887b. Letter to Mr. J.M. Hantz, in the transcription by Max H. Fisch. Indianapolis: the Peirce Edition Project.

------ 1889. Definitions in _The Century Dictionary and Cyclopedia_. Whitney, W.D., ed. New York: The Century Company. [P 373]

------ 1890. Ribot's Psychology of Attention. _The Nation_ 50: 492-493. Preserved at Ketner and Cooke 1975-79/1: 83.

------ 1892. The Doctrine of Necessity Examined. _The Monist_ 2: 321-337. [P 474]

------ 1893. The Algebra of Relatives. MS. 418.

------ 1893. Reply to the Necessitarians _The Monist_ 3: 526-570. [P 525]

------ 1896. On Quantity, with special reference to Collectional and Mathematical Infinity. MS. 15.

------ 1898. Reasoning and the Logic of Things. MSS. 437, 441, 439, 442, 444-445, 443, 446, 951, and 948. An edition by Ketner is anticipated. [P 652]

------ 1900. Our Senses as Reasoning Machines. MS. 831.

------ 1901-5. Definitions in _Dictionary of Philosophy and Psychology_. Baldwin, J.M.. ed. New York: Macmillan.

------ 1902. Relatives, logic of. In Baldwin, J.M., ed. 1901-5. [P 908]

------ 1906. On Existential Graphs as an Instrument of Logical Research. MS. 498.

------ 1907. Pragmatism. MSS. 318, 319, 320, 321.

Reid, Constance 1970. _Hilbert_. Heidelberg: Springer-Verlag.

Rhodes, Richard 1986. _The Making of the Atomic Bomb_. New York: Simon & Schuster.

Riley, H.T. 1856. _Dictionary of Latin Quotations_. London: Henry G. Bohn.

Robin, Richard S. 1967. _Annotated Catalogue of the Papers of Charles S. Peirce_. Amherst: University of Massachusetts Press.

Robinson, E. 1955. Thomas Beddoes, M.D. and the Reform of Science Teaching in Oxford. Annals of Science 11: 137-141.

Rosenberg, M.J. 1983. The Cybernetics of Art. New York: Gordon and Breach.

Russell, Lord Bertrand 1901-1950. Logic and Knowledge. Robert C. Marsh, ed. New York: G.P. Putnam's Sons.

Sadie, Stanley, ed. 1980. The New Grove Dictionary of Music and Musicians. 20 vols. London: Macmillan.

Saerchinger, Cesar 1957. Artur Schnabel: A Biography. New York: Dodd, Mead & Co.

Schnabel, Artur 1934. Reflections on Music. New York: Simon and Schuster. A translation (Cesar Saerchinger) of Schnabel's 1933 address at the University of Manchester.

------ 1942. Music and the Line of Most Resistance. Princeton: Princeton University Press. Reprinted 1969. New York: Da Capo Press.

------ 1963. My Life and Music. New York: St. Martin's Press.

Shannon, Claude E. 1938. A Symbolic Analysis of Relay and Switching Circuits. Transactions of the American Institute of Electrical Engineers 57: 713ff.

Siek, Stephen 1986. Matthay, The Psychologist. The Piano Quarterly 134: 19-23.

Silverman, Robert J. 1985a. Russell Sherman: Interviewed. The Piano Quarterly 129: 14-24.

------ 1985b. Earl Wild: Pianist-Teacher. The Piano Quarterly 131: 45-48.

------ 1986. Jorge Bolet Speaks Out. The Piano Quarterly 133: 15-21.

Slonimsky, Nicolas 1984. Baker's Biographical Dictionary of Music and Musicians. 7th ed., revised. New York: Schirmer Books.

Stephen, Sir Leslie, and Sir Sidney Lee, eds. 1917. The Dictionary of National Biography. London: Oxford University Press.

Stewart, Arthur Franklin 1978. Bechstein and von Sauer: A Legacy that Lives. Journal of the American Liszt Society, Inc. 4: 53-55.

------1985a. C.S. Peirce and Music: A Neglected Affinity. An hour address to the 1985 Festival of the American Liszt Society, Inc., 2 November 1985, at the University of Missouri, Columbia.

------ 1985b. The Fay Exhibit: Evidence from the Preston Tuttle Collection at Texas Tech. Given to the Office of the President, the American Liszt Society, Inc. Presently in the Liszt Archive at the Library of Congress.

------ 1985c. La Notte and Les Morts: Progressive Aspects of Franz Liszt's Style. Journal of the American Liszt Society, Inc. 18: 67-1Ø6.

------ 1987a. Pragmatism and Music: A Brief Look. An address to the 1987 meetings of the New Mexico and West Texas Philosophical Society, 11 April 1987, at Santa Fe, NM.

------ 1987b. Pragmatism and Music: A Brief Look. Southwest Philosophical Studies, abstract in press.

Stock, John Edmonds 1811. Memoirs of the Life of Thomas Beddoes, M.D., with an Analytical Account of His Writings. London: J. Murray.

Trakhtenbrot, B.A. 1963. Algorithms and Automatic Computing Machines. Boston: D.C. Heath.

Turing, A.M. 1937. On Computable Numbers, with an Application to the Entscheidungsproblem. Proceedings of the London Mathematical Society 42: 23Ø-265. Reprinted at Davis 1965.

------ 195Ø. Computing Machinery and Intelligence. Mind 59: 433-46Ø.

------ 1954. Solvable and Unsolvable Problems. Science News 31: 7-23.

Tuttle, Preston H. n.d. Information Pertaining to Proposed Legislation, C.S. Peirce National Memorial. From the Preston H. Tuttle Collection, held at Texas Tech University: Institute for Studies in Pragmaticism.

------. The Preston H. Tuttle Collection. Held at Texas Tech University: Institute for Studies in Pragmaticism.

Wachter, Claire, and Dean Kramer 1986. The Piano Animal. The Piano Quarterly 134: 50-52.

Whitney, W.D., ed. 1899. The Century Dictionary and Cyclopedia. 10 vols. New York: The Century Company.

Wilson, Frank R., M.D. 1986. Music and Medicine 1986: II. Inderal for Stage Fright? The Piano Quarterly 134: 30-35.

Wolff, Christoph 1971. New Research On Bach's Musical Offering. The Musical Quarterly 57/3: 379-408.

Wolff, Konrad 1972. The Teaching of Artur Schnabel. London: Faber and Faber.

------ 1986. Dorothy Taubman: The Pianist's Medicine Woman. The Piano Quarterly 133: 25-32.