"Intelligence" in Early American Psychology:
From Common Parlance to Psychological Concept

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Abstract

It is generally acknowledged that the history of psychology, conceived of in classical Victorian positivist terms as the history of psychology, bears the unmistakeable imprint of E.G. Boring. Only recently have some historians of psychology recognized that such a close identification of the discipline's history with one historian provides psychology with no more than a partial understanding of its history. As scientists, psychologists are attuned to a positivist account of their history, and have accepted this one uncritically. Consequently, Boring's classic A History of Experiemental Psychology has been perceived as normative, and his positivist philosophy, whether expressed in terms of the Zeitgeist or the Great Man theory of historical continuity, has pervaded most considerations of psychology's past. Arguments were advanced in this thesis to support the contention that historians of psychology must explore and justify alternate ways of presenting alternate historical accounts of their discipline. The purpose of the study was to develop such an alternate procedure that would permit historical reconstructions in the discipline of scientific psychology by formulating arguments from contextual data gleaned from documents written by professional psychologists.
The issue selected for exploratory reconstruction in this study was the comparative position of the word "intelligence" as a common parlance term in America in 1890, to that of a concept used by professional American psychologists by 1920. Laffal's Contextual Associates Analysis was employed

to determine the definition of variables relevant to an investigation of this area. The variables isolated were conceptual characteristics of the semantic field surrounding "intelligence" as it appeared in relevant professional documents. On the basis of these results, and of arguments contained in histories of psychology, hypotheses were advanced which, after their investigation, provided evidence of the procedure's usefulness. Three associated studies were also conducted for the purpose of exploring the usefulness of Laffal's instrument in historical research. It was found possible empirically to reject, and fail to reject, historical arguments both developed and explored, through access to a body of primary data.

Abstract

Histories of psychology have, up to the present, been largely narrative, and imbued with a positivist bias. Psychologists have paid very little attention to the application of their discipline's methodological rigor to historical considerations of psychology's past. The purpose of this study was to develop and demonstrate the usefulness of a procedure which would permit historical reconstructions in the discipline of scientific psychology by formulating arguments from data gleaned from documents written by professional psychologists. The comparative position of the word intelligence as a common parlance term in America in 1890, to that of a concept used by professional American psychologists by 1920, was selected for exploratory reconstruction. Laffal's Contextual Associates Analysis was employed to determine the definition of variables relevant to an investigation of this area. The variables isolated were conceptual characteristics of the semantic field surrounding intelligence as it appeared in relevant professional documents. On the basis of these results, and of arguments contained in histories of psychology, hypotheses were advanced which, after their investigation, provided evidence of the procedure's usefulness. It was found possible empirically to reject, and fail to reject, historical arguments both developed and explored, through access to a body of original sources.

Chapter 1

Reconstructing Accounts of Psychology's Past

The purpose of this study was to develop an alternative procedure for use in historical studies of psychology's past. It was projected that such a procedure would permit historical reconstructions in the discipline of scientific psychology by formulating arguments from contextual data gleaned from documents. The issue selected for exploratory reconstruction was the comparative position of the word intelligence as a common parlance term in America in 1890, to that of its use as a concept by professional American psychologists by 1920. The study was conceived and conducted in order to lay challenge to some important, and currently held assumptions in relation to the history of psychology:

a) that the history of the discipline is absolute—i.e., that the discipline began at some definable point and has moved continuously and progressively ever since;

b) that psychology's methodological rigor can make no significant contribution to historical research (see Young, 1966); and

c) that historiography—historical methodology—need only focus on laying challenge to psychology's master historian, E. G. Boring.

It is generally acknowledged that the history of psychology, conceived of in classical Victorian positivist terms as the history of

psychology, bears the unmistakeable imprint of E.G. Boring (Kelly, 1979). Only recently have some historians of psychology (O'Donnell, 1978, 1979; Weimer, 1974a; 1974b; Blumenthal, 1975; Danziger, 1979) recognized that such a close identification of the discipline's history with one historian provides psychology with no more than a unidimensional understanding of its history. As scientists, psychologists are attuned to a positivist account of their history, and have accepted this one uncritically. Consequently, Boring's classic, A History of Experimental Psychology (1929; rev. ed. 1950), has been perceived as normative, and his positivist philosophy, whether expressed in terms of the Zeitgeist or the Great Man theory of historical continuity, has pervaded most considerations of psychology's past. The image of the discipline's history has been accepted as one of continuous progress, with very little, and then only cautionary attention, paid to "dysfunctional" as opposed to "functional" aspects of its development (Kelly, 1976; Young, 1966; Stocking, 1965).

The most stringent and comprehensive attack on this particular image of psychology's past came from Young, a historian of science who, while acknowledging Boring's primacy in the field of the history of psychology, added

> Nothing said here should be construed as diminishing the sense of debt which every beginner in the history of psychology owes to him [Boring]: his contribution is nonpareil. But it must be stressed that the worst way to repay intellectual debts is to repeat the findings of one's mentors rather than extending, amending, and deepening them (Young, 1966, p. 10).

The most serious assaults by psychologists on our "manifest history" have appeared only during the last five years. They are critical of the discipline's dependence on Boring's history, but pay very little attention to the issue raised by Young in relation to historiography, that is, to the ways in which our histories are written. O'Donnell (1979) came closer to this matter than others when he advanced the argument that Boring's History was influenced by his professional concerns about the lesser status of experimental as opposed to applied psychologists in America during the nineteen twenties. When speaking of Boring's interpretation of psychology's past, he said "the historiography of psychology has followed Boring's lead" (O'Donnell, 1979). With the exception of two recent general texts—An Intellectual History of Psychology (Robinson, 1976), and The Persistent Problems of Psychology (MacCleod, 1975), this would appear to be the case. There is little evidence to be found in the periodical literature of the last fifteen years that the plea advanced by Stocking (1965) for less presentism in historical studies has been heeded. While some few psychologist-historians have demonstrated a willingness to eschew the perception of Boring's History as definitive (e.g., Blumenthal, 1975; Danziger, 1979; Kelly, 1979), none has paid significant attention to a central aspect of Boring's historiography—his positivism. Perhaps this can be attributed in part to the fact that, as Young (1966) pointed out, the history of psychology is written primarily by avocational, rather than professional, historians. As scientists, psychologists are not expected to explore in

depth such areas as philosophy of science, sociology of knowledge, historical research procedures, etc. By and large, they still continue to write stories rather than explore issues; in Butterfield's terms (1963, p. 16) they are interested in "the past for the sake of the present"—which explains the term "presentism." But as Young (1966) argued,"History, like science, is controversy, not story-telling."

Recognition of this point began to take effect in philosophy and history early in the twentieth century. The classical Victorian positivism had repudiated metaphysics, i.e., speculative considerations regarding the nature of reality, in favour of an adherance to observation and experience. All knowledge regarding matters of fact, it had been maintained, was based upon the "positive" data of experience. This philosophical bedrock had led inevitably to an expository, rather than a controversial, stance in history; it was in these terms that the histories written by late-Victorians were intelligible to their contemporary academic mainstream. This philosophy did not persist among historians, but perhaps because of their scientific operationism and their avocational interest in history, it has persisted amongst psychologist-historians. Ironically, one of the factors which influenced historians into moving away from historical positivism was the experimental evidence of individual differences in perception provided by early scientific psychology (e.g., see von Helmholtz, 1860; Wertheimer, 1912; Lotze, 1852; Titchener, 1910). Psychologist-historians, however, appear to have been blind to the knowledge generated by members of their

own discipline.

Becker (1968, p. 116) has argued that "of all the present-day psychologists, perhaps it was E.G. Boring who scorned philosophy most." I am more inclined to argue that Boring strove to reject the idea that metaphysics, rather than philosophy, had any useful part to play in a science of human behaviour, and that he was sympathetic to Mach's experiential positivism, and wholly committed to Bridgman's operational positivism. When Boring wrote the first edition of his History in 1929, psychology, as a discipline, was still trying very hard to demonstrate and justify its separation from the disciplines out of which it grew—philosophy, medicine, and theology. To say that Boring "scorned philosophy" is to make a statement similar to the equally inaccurate claim that Watson (1913) denied the existence of consciousness. Watson denied consciousness a place in the scientific discipline of psychology, and he did so because at that time psychology did not have the methodological tools for dealing with it empirically. Boring and Watson excluded metaphysics and consciousness respectively from psychology's domain because, in Boring's words (1950, p. 654), "those problems are not the psychologist's." As O'Donnell (1979) argued, with a great deal of convincing evidence, Boring was anxious, when he wrote his classic History in 1929, to redress what he perceived as an imbalance between applied and experimental psychologists in America at that time. Since, in Boring's words (1950, p. 656), operationism was then "a trend of the times," it can hardly be found surprising that his history both endorsed

and reflected this trend. What is being challenged in this study is the fact that present historians of psychology (with the exceptions noted above) have failed both to recognize this positivist bias in Boring's historiography, and to attempt to deal with it. As Young (1966) stated, psychologists owe their master historian more than uncritical devotion.

Blumenthal (1975), Weimer (1974a, 1974b), Mackenzie (1972, 1976), and Danziger (1979) explicitly acknowledged the desirability of multiple perceptions of an historical episode, or of a general historical account. But they illustrated their arguments by appealing to a comparison of different interpretations of the same historical episode, and pointing out that, in Weimer's (1974a) terms, "someone's account cannot be correct"—a regrettably positivist declaration. If one examines the only English language journal in its field—The Journal of the History of the Behavioural Sciences—one finds that apart from an excellent article by Stocking during the Journal's first year of publication (1965), only one other (Buss, 1977) has focussed on the historiography of histories of psychology.

By the 1930's, historians had moved into a post-positivist phase of perceptual relativity (Buss, 1977; Young, 1966). They were no longer convinced that any historical investigation dealt with, or could develop, a "true" story. In Hughes' words (1958, p. 16), they were "striving to comprehend the newly recognized disparity between external reality and the internal appreciation of that reality." The historical relativist excluded the possibility of any historical account being judged as

"right" or "wrong." An historical reconstruction, it is now argued, may be found convincing or unconvincing, and typically the focus of critical attention is directed to its components, and to the nature and persuasiveness of the arguments developed, rather than the perspective adopted by the historian. When this perspective, or bias, is critically evaluated, or when the existence of multiple perceptions of any historical situation is ignored (as in Weimer, 1974a), the logic of the evaluation leads inexorably to a verdict of "someone's wrong." I am suggesting that historians of psychology have still to free themselves from all the shackles of their historiographical mentor. It is not Boring's historical account of psychology's past that is "wrong," but historians' evaluations of his account on the basis of a right-wrong dichotomy. Such critics have not yet shaken off the pervasive positivism of psychology in general, and of the historians of psychology in particular (Robinson, 1976, pp. 402-411; Young, 1966; Wyatt, 1961).

I do, however, agree with the arguments advanced by Weimer, O'Donnell and others that historians of psychology must explore and justify alternate ways of presenting historical accounts of their discipline. The purpose of the present study has been to develop one such alternate procedure.

Alternate procedures in histories of psychology

As was indicated earlier, the systematic development of alternate procedures for reconstructing historical accounts of psychology's past

has been limited. Blumenthal (1975) has done some interesting and much-needed re-evaluations of Wundt, striving to rescue him from the uni-dimensional position of "the founder of experimental psychology" assigned him rather repeatedly by Boring. The procedure adopted by Blumenthal was to compare selected statements about Wundt made by different historians, one of whom was Boring. For the purpose of illustrating the key issue of multiple perceptions of an historical episode, such a procedure is impeccable. It is, however, only a first step in the attempt to re-examine the nature of Wundt's contribution to psychology; while Blumenthal was able to demonstrate that the historians presented significantly differing accounts, he concluded by evaluating Boring in particular, as Weimer did, on the basis of a right-wrong dichotomy. The logical corollary of relativist historicism, that no such positivist conclusion can be attempted, was ignored. Similar criticisms can be advanced with regard to articles by Mackenzie (1974, 1977), Weimer (1974a, 1974b), and Danziger (1979).

Three very interesting attempts to use quantitative methods in the history of psychology have been published by Cardno (1962a, 1962b, 1963). Young (1966) referred to them, rather patronizingly, as "odd, but curiously interesting." The fact that Young saw the application of empirical rigor to historical investigation as having only very limited usefulness no doubt accounts for his dismissiveness. But as Cardno (1963) pointed out, "The history of psychology abounds in succinct judgments . . . they are impressions, which though backed by more or

less evidence are arrived at by steps not always explicit." In each of his three studies, Cardno attempted to make explicit and systematic some of the grounds upon which these judgments are based. There is no evidence to indicate that other researchers have followed Cardno's lead. By and large, perhaps because they have paid more attention to their own history (or perhaps because they have never had a m ajor figure like Boring whose history they were willing to accept as definitive), it is sociologists who have produced some very interesting studies in the field (e.g., Ben-David & Collins, 1966; Merton, 1957, 1961). Psychologists have not been eager to apply their methodological principles--as Robinson (1976) says so acutely, their own metaphysic--to the impressionistic inferences which historians draw. And one area in which these inferences abound is in relation to the movement of common parlance words into positions where they become formal psychological concepts.

"Intelligence" in early American psychology

The word intelligence is not the only common parlance term adopted for professional use by psychologists. It could, in fact, be argued that the preponderance of such terms has been, and is, a significant handicap to the ease of unambiguous intra- and inter-disciplinary communication. The degree of attention paid in America to the nature and to the measurement of intelligence, and the variety of attempts made to reconcile its assumed nature with its measurement, was considerable. The acceptance of intelligence testing of the armed forces during World War I by

Congressional members testifies to the importance accorded this procedure by powerful bodies in the United States. It was felt that exploration of the use of the word intelligence in the early history of scientific psychology in America could provide some useful data and some interesting historical arguments in an area which is now coming under heavy attack in some American law courts (Opton, 1979).

Louch (1966, pp. 54-59) has nicely illustrated one of the factors contributing to the confusion that has frequently surrounded the use of the word intelligence. Like many words which have the joint function of being common parlance terms as well as psychological concepts, intelligence reflects both description of behaviour, and its appraisal. "A question about intelligence could not arise," Louch suggested, "unless some performances were prized; it thus becomes pointless to try to set aside our preferences in order to decide what intelligence really is." Early American psychologists did not set aside their preferences at all. The environmentalists, behaviourists, educators, and learning theorists strove to establish empirically the importance of external factors as determiners of intellectual capacity. The geneticists and eugenicists were often very seriously committed not only to verifying the importance of nature over nurture, but also to purifying the basic American population stock. Could this mean, then, that experimental as opposed to applied psychologists, for example, framed their questions involving intelligence differently? The unquestioned assumptions and firmly committed beliefs of an experimenter have been shown to influence

that person's empirically derived results (Rosenthal, 1966). An experimental psychologist may have been interested primarily in specific mental functions, and have worked only with subjects of average intelligence. An applied psychologist may have been motivated to separate the less intelligent from the majority for the purposes of obtaining their social and legal protection, or social education, or the protection of the majority—or all three. Such experimental and applied psychologists may therefore have delineated only partially overlapping experimental arenas, and have appraised the significance of their results from a somewhat less than mutually inclusive perspective.

Two of the major figures in America involved in this area were Goddard and Healy. Both worked primarily with the retarded, the delinquent, the orphaned, and other disadvantaged groups. Yet generally, Goddard could be considered a eugenicist, and Healy a social progressivist who challenged the current hereditary conceptions (Sarbit, 1980). Here two psychologists' firm convictions were in significant conflict. Goddard was, by and large, a geneticist who saw heredity as the primary causal influence in relation to intelligence. Healy did not accept the arguments advanced by eugenicists and others sympathetic to the "nature over nurture" resolution of this debate. Would Healy and Goddard, then, as two examples, have used the word intelligence somewhat differently, and surrounded its appearance in their documents with different concepts?

There is no evidence in the literature that these, and a host of

other similar questions, have been explored in a way that strives to make more systematic the procedures employed by traditional historians: a) selection of sources, b) gathering impressions derived from these sources, and c) drawing of impressionistic inferences (Carney, 1972). Nor has any significant attention been paid to the major point raised by Kuhn (1973, p. 3), that historians must respond to the need to understand the past in its own terms before comparing it with the present:

> Gradually, and often without entirely realizing they are doing so, historians of science have begun to ask new sorts of questions and to trace different, and often less than cumulative, developmental lines for the sciences. Rather than seeking the permanent contributions of an older science to our present vantage, they attempt to display the historical integrity of that science in its own time.

In one of the more recent books dealing with intelligence testing in America (Kamin, 1974), the anti-hereditarian bias is intrusive. No serious attempt was made to develop a critical exposition of why IQ tests were used at that time in the ways, or for the purposes, that Kamin suggested, and criticized so abrasively. It was also in an attempt to provide a means of considering some of these types of issues that this study was designed.

National and time variables

In considering some of the geographical boundaries for the present study, the entire continent of North America was not selected for a number of reasons. First, the nature and effect of Canada's ties to

England in relation to the use of intelligence in professional psychological documents could have been a factor influencing the ways in which the word was used. Also, the political, social and professional climates in America and Canada were not demonstrably the same. While this does not argue that therefore the uses of the word were different, it was felt that such a matter would require prior, and separate, exploration. The study thus restricted its exploration to the use of intelligence in professional psychological documents in America.

It was decided that attempts would be made to compare the use of intelligence between two decades. During the 1890's, the first efforts by American psychologists to apply laboratory measures to intelligence were published (e.g., Cattell, 1890; Bolton, 1892; Cattell & Farrand, 1896). Consequently, the initial introduction of the common parlance word intelligence to the vocabulary of professional psychologists in America can be explored during this decade—1890-1899.

The first decade of the twentieth century saw the formulation of dozens of mental ability tests in the United States (Young, 1923; Peterson, 1926, 96-116). Nevertheless, while these tests proliferated, their emphasis was more specifically directed to the measurement of special functions rather than general mental ability (Peterson, 1926, p. 114). It was not until Goddard published a translation of Binet's first scale in 1908, and of his 1908 scale in 1910, that psychologists in America were provided with a test of general intelligence. This test, and many others subsequently developed in the U.S., provided

American psychologists with empirical grounds upon which they could base their conceptualization of intelligence, and thus legitimatize the use of the term as a psychological concept. The first decade of the twentieth century was thus an important transition period in relation to the use of the word intelligence in psychology, and stands both as a useful bridge between, and a convenient separator of, the first time period selected for this study, and the second, 1910 to 1919.

The first decade of the twentieth century is clearly of considerable interest as an important time in early American psychology. In relation to a comparative investigation of the transition in the use of intelligence from common parlance to psychological concept, one of its primary functions encompasses the work of Binet and his associates in the development of a test of general intelligence. Any pre- and post-Binet differences in conceptual understanding of intelligence in America were therefore of primary interest in this study.

Defining the variables

The primary documents sampled consisted of all the psychological works dealing with intelligence and written by Americans or American academicians, published for the first time during these two time periods. "American academicians" is a phrase chosen to include those whose academic careers took place largely in the United States (e.g., Titchener, Munsterberg). A sampling procedure (see Chapter 3) ensured the representation of all authors who published relevant books or

papers, irrespective of their frequency of publication. Laffal's Contextual Associates Analysis (1979), considered in the next chapter, provided a technique for defining variables that might be useful in any analysis. Determining these variables was the first stage of this study.

The major purpose of the study was to develop, and attempt to justify, an alternative procedure for use in considerations of psychology's past. It therefore focussed primarily on matters related to historiography of the history of psychology. The systematic procedure developed provided an interesting contrast to those used by traditional historians. In both cases the initial step involves source selection; in the present case the source selection was conducted systematically. Traditionally, historians then proceed to gathering impressions; in the present case data was collected empirically. The concluding step for the traditional historian is the drawing of impressionistic inferences; this study, by contrast, arrived at conclusions on the basis of the analysis of empirically derived results. It was felt that the development of such an alternative procedure could be found useful in historical research; psychologists, in fact, could benefit immensely from a careful application of their methodological rigor to questions related to the history of their discipline.

Chapter 2

Contextual Associates Analysis in Historical Research

This content analysis instrument enables inferences about a speaker's perceptions to be drawn from the speaker's language. Inferences are generally of two kinds: those relating to the significance of a word in the psychological framework of a speaker, and those about the psychological structure of an individual. To make these inferences, an assumed theoretical base accounting for the relationship between personality structure and language, and an empirical method for applying the theory to written and spoken texts, are necessary. Julius Laffal has spent over twelve years developing the method of contextual associates analysis, with good empirical results to support its reliability. He is the primary researcher in this field, and has applied his method to a variety of written and verbal communications (Laffal, 1961, 1966, 1968, 1969, 1976, 1979).

Laffal developed his instrument from what he felt to be a point of intersection, or common ground, between linguistic theories of semantics and associationist theories of language. Linguistic theories of semantics suggest that language is an instrument which cognitively organizes our world. Vocabulary is an organized, hierarchical structure, where individual words are conceptually related to other words (e.g., angry, hostile, annoyed, enraged, aggressive, etc.). These small conceptual groups are themselves an integral part of other groups by virtue of

conceptual relationships. Language is thus seen as a complex, finely structured cognitive "map" of the culture in which it is used.

Linguists are interested in the conceptual commonality of language as it reflects the cognitive structuring of a culture within and across time periods, social classes, etc. However, Olson (1970) has argued that these theories are related primarily to the structural components of language—grammatical, syntactic and semantic rules—and have excluded the strongly correlated area of references, i.e., of objects and events in the environment, thereby failing to consider the experiential domain. The cognitive map of the linguists is thus a structural rather than an experiential one, and focusses on the deep structure characteristics of language developed by Chomsky (1957, 1965, 1968).

Psychologists have also directed their interest to individual differences as revealed in language. Research into word association, and the importance of word association and word clustering in cognition, have established that the individual organizes words into groups on the basis of associative or conceptual commonality, where the integral part of such clustering is experiential in nature. The individual's cognitive word map, as proposed by psychologists studying language, intersects with that proposed by the linguists, at the point where both focus on the central importance of conceptual or cognitive structuring in language and thought (Deese, 1962; Mandler, 1962; Bousefield, Cohen & Whitmarsh, 1958). The commonality that has interested linguists is a structural one; that of interest to psychologists is experiential.

Laffal's purpose in developing his instrument was to explore the hidden, or latent meaning, in language, as in that expressed by symbolism. His interest has always been directed towards the language of schizophrenics, particularly in relation to "language as a verbalization where the words consistently carry a latent meaning for the subject different from their common, manifest meaning." (Laffal, 1966). However, because he was focussing on language which has a manifest as well as a hidden meaning, it was necessary for him to develop an instrument which could, amongst other things, separate idiosyncratic hidden features of a particular individual's language from that employed with its associated common meaning. Over the past twenty years he and his associates have also found it useful when applied to informational texts (e.g., Laffal, 1961; Hartsough & Laffal, 1970).

Laffal felt that the principle of the conceptual-logical dictionary devised by Wartburg and Hallig (see Laffal, 1973, p. 4) had some limitations when applied to the analysis of individual speakers with a view to making inferences about the speakers as individuals, rather than as members of a cultural group. Studies in the organization of verbal material in recall suggest that language operates as a determiner of experience for individuals, and similarly as a determiner of cognitions:

> Language does its work by evoking experiential associations which are suggested by pertinent words or, to put it a little differently, by rearranging the relative strengths and likelihoods of occurrence of groups of word-thing responses. The evoked hierarchy of associations, the meaning, reflects a fundamental behavioural and attitudinal shift in the listener in response to the stimulus (Laffal, 1965, pp. 41-2).

Thus it was felt that a conceptual-experiential dictionary, focussing on the experiential meaning of words, rather than Wartburg and Hallig's "logical universal" meaning, could tap more appropriately the cognitive organization of individual language.

Laffal devised his conceptual dictionary following a method similar to that used by Spurgeon (1958), who in a study of Shakespeare's imagery, classified her material in terms of themes and subject matter, and grouped images on the basis of similar and related meanings. This method complements that of the semantic theory linguists, who define a system beforehand, and group their material accordingly. An example of such preliminary grouping can be found in early editions of Roget's Thesaurus, where the author specified a number of general ideas under which he arranged his words: "The idea being given, to find the word, or words, by which the idea may be most fitly and aptly expressed." (Roget, 1852, p. xiii).

Spurgeon's and Laffal's methods are directed more towards moving with the language appearing in the texts, rather than assuming a commonality of meaning for all words, independent of the texts in which they appear. Laffal felt his approach was "an empirical, psychologically oriented" one, where words whose common meanings were experiential in nature, rather than logical, were grouped together progressively until a concept emerged. "The process," he says, "may be described as a sequence of steps in which increasingly general criteria for commonality of experience were used to group words." (Laffal, 1973, p. 6).

The contrast, then, is essentially that between a theoretical model (semantic theory linguists) and an experiential-empirical situation.

Laffal's dictionary was developed over a number of years. Its last published version appeared in 1973 in a book containing 118 categories and their descriptions, detailed scoring instructions, and extensive alphabetical listings of all the words in the dictionary, with their categories. There is also an alphabetized list of all dictionary words under their categories (Laffal, 1973). The dictionary is, according to Laffal (personal communication, 1979), undergoing constant revision (for examples of Laffal's categories, see Table 1). In Laffal's most recent publication, where a dictionary of 42,228 categorized word entries was used (1979, p. 323), the reliability of 23 profiles for a number of key words ranged from .77 to .98, with 22 of the reliability figures .84 or above (Laffal, 1979, p. 328). Its most recent application to an analysis of key words in historical documents provided evidence of its usefulness (Hyman & Shephard, 1980).

Laffal has defined words in a text or in verbalizations as tokens representing concepts; it is not the words, but rather the concepts they represent which are examined "for the ideational domain around the key item" (Laffal, 1966). The "key item" is the specified word, the target word, whose ideational domain, or semantic field, is the subject of interest in a study. In the case of the present study, the subject of interest was the way in which the word intelligence was used in documents written by early American psychologists. Intelligence was there-

Table 1

Category Labels and Descriptions From

Laffal's Conceptual Associates Analysis

AFAR - Words relating to the unusual, the unexpected and the distant. AFAR words are in contrast with the "familiar and usual" meaning in SIML. The kinds of ideas in AFAR are: distance and remoteness; abnormality and unnaturalness; unusualness and infrequency; chance and unpredictability.

AGRE - Words relating to cooperation, consent, approval and agreement. Words in AGRE have stronger effective overtones than those in SIML but less than those in FOND. The kinds of ideas in AGRE are: allowing and permitting; agreeing and accepting; praising and commending; cooperation and collaboration.

ANML - References to animals other than insects (which are scored BUG) are included here. For sea animals the category FLOW is also applied; for flying animals the second category is UP. Animal skins have FABR as a second score. If the reference is to the animal as food or to a food product derived from animals, the word is scored ANML FOOD. No distinction in categorization is made between wild and domestic animals. Human references are not included in ANML although general terms referring to the animal kingdom are (vertebrate, mammal).

Table 1 (cont'd)

BAD - Words with negative moral or ethical connotations. Contrasting words are in GOOD. The kinds of ideas in BAD are: evil and sin; profanity and blasphemy; baseness and debauchery; roguishness and scurrilousness; dishonesty and insincerity.

Table 1. Four examples of Laffal's categories selected from Laffal, 1973, p. 20.

fore the target word. By compiling a frequency count of concepts which appeared in close association with the target word in these sources, a semantic field could be constructed for different time periods. Frequency profiles could then be compared with each other (for examples of target words within their defined contexts, see Table 2).

When texts are being prepared for analysis, all the textual material is edited prior to card punching. Nouns are substituted for proper names and personal pronouns -- e.g., "the boy" or "the man," depending on the textual context, is substituted for "he" (or, given the appropriate context, the noun could be "horse," "dog," etc.). Where ambiguity is involved (for example, when the word "bear" appears in the text), it is assigned a number corresponding to the appropriate meaning found in Laffal's dictionary (e.g., 1 for "bear" = animal; 2 for "bear" = carry, etc.). Laffal described these and other editing procedures in detail (1973, pp. 10-15); all must be observed in any use of the instrument (for an example of text translation, without a target word, see Table 3).

Laffal's instrument in historical research

This instrument's ability to isolate the semantic field surrounding any given word appeared to make it particularly appropriate for the purposes of this study. It was felt that, in using this procedure, it should be possible to provide definitions of some variables to investigate the use of the word intelligence. By using these variables, historical arguments can be developed on the basis of original sources. The

Table 2

Example of Target Words Within Their Defined Contexts

Target word	Context
intelligence	and the minds taste for different studies. The author quotes the philosopher who would have the remarkable product of human industry an intelligence placed before the child, maintaining that nature will indicate innate aptitudes which the most attentive study would never be able to discover. An ingenious mechanism, for example attracts
environment	from the groups surroundings and treated as one thing. For ordinary common sense the world is mapped out into a plurality of the relatively independent units. Each of the units emerges from the unit environment like an island from the sea. The unit is detached from the units surroundings by the units separateness and unity of interest. The interest is ordinarily of a practical
emotion	association appropriate groups of imagery; only when the physical states fail of this are we entitled to say that there is no object and then we must add

Table 2 (cont'd)

that there is also no <u>emotion</u>. As in dealing with the causes of feeling, so we may now in like manner proceed to inquire whether in feelings, manifestations or effects there is any contrast corresponding to the opposing extremes of

Table 3

Example of Text Translation into Contextual Associates

We	hold	these	truths	to be	self-evident	that
WE	IDEA		TRUE		SOLE OPEN SHRP	

all	men	are	created	equal,	that	they
	WHOL	MALE	BGIN	SIML		

are	endowed	by	their	Creator	with	certain
	AID HAVE		WHOL MALE	BGIN HOLY	SHRP	

unalienable	(inalienable)	rights	that	among	these	(rights)
	EVER		TRUE MOTV	JOIN		TRUE MOTV

are	life,	liberty	and	the	pursuit	of
	LIVE	OPEN		GO MOTV		

happiness.	That	to	secure	these	rights,	governments
GLAD			AID		TRUE MOTV	

are	instituted	among	men,	deriving	their	just
LEAD LAW		BGIN	JOIN	MALE	BGIN	LEAD LAW

powers	from	the	consent	of	the	governed,
GOOD TRUE	POWR		AGRE		LEAD LAW	

that	whenever	any	form	of	government	becomes
	TIME		FORM	LEAD LAW		EVNT

destructive	of	these	ends,	it	is	the
DAMG		END MOTV				

right	of	the	people	to	alter	or
TRUE MOTV		GRUP		VARY		

to	abolish	it,	and	to	institute
	END	LEAD LAW			BGIN

Table 3 (cont'd)

new government,	laying	its	foundation	on such	principles	and	
NEW LEAD LAW	BGIN	LEAD LAW CRUX	FORM	SIML	CRUX IDEA		
organizing	its	powers in such	form,	as to	them shall	seem	
FORM	LEAD LAW	POWR	SIML FORM		GRUP	IDEA VIEW	
most likely to effect	their safety and happiness.		Prudence,	indeed,			
MUCH MUCH	END	GRUP	AID	GLAD	GOOD	EMPH	
will dictate that governments	long	established	should not be	changed for			
LEAD TALK	LEAD LAW	EVER	BGIN EVER	NO	VARY		
light and transient	causes; and accordingly	all	experience hath				
GLAD TRIV	VERY SEP	MOTV	SIML	WHOL	EVNT		
shown, that mankind	are more	disposed to suffer,	while	evils are			
VIEW	WHOL GRUP	MUCH	MOTV	PANG	TIME	BAD	
sufferable, than to	right	themselves by	abolishing the	forms to			
AGRE	GOOD TRUE	WHOL GRUP	END	FORM			
which they	are accustomed.	But	when a	long	train of	abuses	
WHOL GRUP	SIML EVER	OPPO	TIME	AFAR	FORW	DAMG	
and usurpations,	pursuing	invariably the	same	object	evinces		
DAMG HAVE	TO MOTV	EVER	SIML END	MOTV	OPEN VIEW		

Table 3 (cont'd)

a	design	to	reduce	them	under	absolute	despotism,
MOTV		LITL		WHOL GRUP	DOWN SUB	WHOL EMPH	AGGR LEAD
it is	their		right,	it is	their	duty,	to
	WHOL GRUP		TRUE MOTV		WHOL GRUP	MOTV WORK	
throw off	such	government,	and	to	provide new	guards	for their
GO SEP	SIML	LEAD LAW			AID NEW	BLOK AID	WHOL GRUP
future	security.	Such	has been	the	patient	sufferance	of these
FORW TIME	AID	SIML			EVER REST	PANG	
Colonies;	and	such is	now	the	necessity	which constrains	them
GRUP SUB		SIML	NEW		CRUX MOTV	BLOK	GRUP SUB
to alter	their	former	systems	of	government.	The history	of the
VARY	GRUP SUB	PAST	FORM		LEAD LAW	IDEA PAST	

Table 3. This example is presented in Laffal's A concept dictionary of English, 1973, p. 16.

instrument has, however, been used in only one study for historical research (Hyman & Shephard, 1980). It was found, then to be useful when attempts were made to correlate statements about historical change made firstly by historians, and secondly, by demonstrated changes in the use of selected target words in temporally related documents. However, the methodological questions to which the use of the instrument in quite a different domain (i.e., history) give rise, have not been explored.

One important element of language in historical research is its communicative function. For the listener, or the reader, language is typically informational; as such, it restructures perceptions and thereby influences thought. The speaker's language does not restructure his own thought; according to Olson (1970), speaking for that individual is in this sense -- i.e., the informational sense -- redundant. Such, however, is not the case for the receptor. It was argued earlier that a historical researcher examines sources relevant to the issue under consideration and draws impressionistic inferences. When an instrument which objectively quantifies the broad conceptual content of a text is used, the filtering processes which every mind uses to sort and organize impressions are considerably reduced. The instrument itself is a filter, but only one. Its categorizing is not affected, as any reader's is, by the material that preceded the text currently being analyzed.

But using the instrument in historical research introduces variables which could, by their presence, raise questions about the credibility of the data obtained. For many historical studies, <u>time</u> is an important

variable. Ideally, any such study which incorporates questions about change over time, about historical advance, about the stability of ideas over a considerable period, etc., would employ an instrument whose credibility in such a research setting had been established. In the present study, the instrument of choice focussed on the contextual associates of key words in written documents; that is, its operational field—language—changes over time. Does the instrument, one could ask, have the ability to discriminate between general language changes over time on the one hand and the stability of specified key word associates on the other? How sensitive is Laffal's instrument—is it able to isolate specific areas of difference within one period, such that a picture of both the macrocosmic stability and the microcosmic differences can be obtained? In an effort to deal with a number of such questions, thereby adding to the credibility of the instrument's use in historical research, three studies were conducted in association with this project (detailed reports of these studies can be found in Appendix 1, Appendix 2, and Appendix 3).

The purpose of the first of these preliminary studies (see Appendix 1) was to explore the stability of a concept over time. The earlier study (Hyman & Shephard, 1980) had demonstrated a significant temporal difference in conventional wisdom psychology in England between 1851-1867 and 1894-1914, in relation to the target word environment. It was also established that insight psychologists between 1851 and 1867 would use certain concepts associated with their innovative work in a detectably different way to their fellow conventional wisdom psychologists, but in the

same way as that of conventional wisdom psychologists in the later (1894-1914) period. Thus in terms of the general category profiles, some conceptual stability had been demonstrated. The first preliminary study conducted in the current research focussed on specific, nominated categories rather than the general category profile in relation to the use of the target word environment by insight psychologists in 1851-1867, in comparison to conventional wisdom psychologists from 1894-1914. No difference predictions were advanced in relation to eleven categories, and of the eleven z scores obtained, eight were not significant.

The second preliminary study (see Appendix 2) sought to replicate the appearance of significant differences in one time period when a conventional wisdom psychologist's use of the word emotion was compared with an insight psychologist's use of the same word. The results of this study did not support the historical arguments advanced. It was felt that this did not necessarily argue against the usefulness of Laffal's instrument in historical research, but did indicate some of the difficulties encountered in designing such studies.

The purpose of the third preliminary study (see Appendix 3) was to explore another aspect of conceptual stability within an area of demonstrable change. The use of the word ether by physicists in America prior to, and following Einstein's first relativity paper, which drastically altered the terms, but not the parameters, of the debate on the existence of ether, was explored. It was predicted that categories within which concepts related to these parameters were coded would not be used with

either greater or less frequency between the two time periods. The results obtained were consonant with this prediction.

Each of the studies was designed in an attempt to deal with questions to which the use of Laffal's instrument in historical research give rise. In all cases the hypotheses proposed were based on historical arguments advanced in texts and articles.

Target word for investigation

The first use of the word intelligence in English is recorded in the Oxford English Dictionary as occurring in the middle of the fifteenth century. As with most common parlance terms, it has a number of synonyms which appear in the many editions of Roget's Thesaurus. However, as Tuddenham (1968) pointed out, as a formal psychological concept the word has a short history and was not referenced separately in Baldwin's Dictionary of Psychology and Philosophy, published in 1901. Instead, it appeared as a synonym of the word "intellect" (Baldwin, 1901, p. 558). It was concluded, therefore, that temporally relevant synonyms of the target word for this study, as presented in psychological dictionaries or textbooks dealing with the subject, should be included.

Primary sources

Characteristics. Psychology is fortunate in having a copious supply of documents. These primary sources, selected on the basis of the national, time and content restrictions specified previously, contain linguistic

forms such as definitions, explanations and discussions. The language used for elaboration and communication could be very rich in its implied linguistic content. Language has the properties of a record, a transmitter, a reflector and a shaper of information, perceptions and ideas. The systematic analysis of documents, it was felt, could supply basic components in historical arguments relatively uncontaminated by preconceived ideas as to the nature and form of these arguments.

The focus of this study on the use of the word intelligence in early American psychology made an exploration of linguistic characteristics contained in professional documents particularly apposite. If one depicts intelligence as a label (cf. Laffal, 1966), and examines linguistic contexts within which it is embedded, its "critical features" (Taylor, 1976, p. 285) can be assessed. By stating that the word intelligence changed from the position of a common parlance term to that of a psychological concept over a specified period of time, the possibility that these "critical features" of the label changed over time was advanced. By determining the definition of variables (i.e., the "critical features"), it became possible to determine empirically whether or not this speculation was justified, as well as others that have been advanced in relation to the term. As Taylor (1976, p. 285) has pointed out, "the use of a label such as 'obssessive [sic]-compulsive neurotic' may give us the illusion that a complex phenomenon has been explained." When using the label intelligence, in what ways, and to what extent, was the complex phenomenon that the label represented being explained by early American

psychologists?

Significance of the variables

It was proposed that a number of ways of investigating the significance of the variables be undertaken in this study. Propositions including any variables determined by the application of Contextual Associates Analysis to the samples of the use of intelligence in the documents were formulated and explored. For example, it was felt that answers to the following questions could be advanced, and subsequently examined empirically:

a) Is there a significant difference between the contextual associates of the word intelligence and the contextual associates of its synonyms during either time period?

b) Is the time factor a significant variable?

c) Is there more than one semantic field surrounding the use of the word intelligence and/or its synonyms during either time period?

d) What are the semantic field characteristics of intelligence and/or its synonyms as recorded in the documents?

The answers obtained to these questions, it was felt, could permit the empirical investigation of a major historical argument advanced in some histories of mental testing. In one of the first published (Young, 1923), it was stated that "divergent standpoints of the applied psychologists and the experimental" had arisen in the United States by the

beginning of the 1920's. O'Donnell (1979) has supported this argument, suggesting that during the nineteen twenties there were controversies between "pure" and "applied" psychologists. It was felt that these apparent differences could be reflected in the contexts within which the word intelligence and/or its synonyms was embedded in professional documents. Consequently, it was hypothesized that differences reflecting this divergence between 1910 and 1919 would be detectable.

It was proposed that this study would provide historians of psychology with a useful methodology for constructing historical arguments, and thus with an alternate way of understanding psychology's past.

Chapter 3

Methodological Procedures and Results: Stage 1

This study was carried out in two distinct stages. The first stage involved the selection and preliminary analysis of a body of professional documents in order to develop the semantic field(s) of the word intelligence and its temporally relevant synonyms. It was projected that when some answers to the general question, "What is contained in these documents?" were provided, it would be possible to formulate hypotheses derived from these results. The second stage of the study focussed on the examination of some of these formal hypotheses. The details of the first stage are presented in this chapter.

Primary Sources

All historical considerations of American psychology during the two periods that were selected for this study--1890-1899 and 1910-1919--contain bibliographies, some very extensive (Young, 1923; Roback, 1952; Peterson, 1926; Pintner, 1923). While a considerable body of original sources could be drawn from these lists, the construction of a bibliography, unless specified otherwise, involves making selective decisions. The reference lists provided by Young, Peterson and Pintner, who wrote the earliest histories of mental testing and thus developed some of the earliest bibliographies of works dealing with intelligence,

are not identical. The bibliographies developed by Kohs (1914, 1917) on literature on the Binet-Simon scale appear exhaustive (there are about 750 entries), but they deal only with the years up to 1917 and do not claim to be complete.

In an attempt to construct an historical argument, it is preferable for the structural materials to be based wholly on primary sources, without the intrusion of later, or contemporaneous but selective, decisions, i.e., of those who developed a bibliography from hindsight, or for their own specified purposes at the time—see Kohs, 1914, 1917; Young, 1923, etc. Consequently, this study attempted to examine all published psychological works dealing with intelligence and its related synonyms and written by Americans or American academicians during the two time periods noted above.

Period 1: 1890-1899. The Psychological Index, first published in America in 1894, is a series of volumes recording all psychological publications annually, irrespective of country. For the years 1894-1899, this source was used to develop the required list of primary sources. Each volume is arranged into sections with headings that underwent modification and expansion as the discipline developed. To reduce the list development procedure to manageable proportions, only the sources listed under headings which relate to the issue under investigation were scanned.

The volume headings are the same for 1894 to 1897; some changes were introduced for 1898 and 1899. Every major heading is followed by a General sub-heading, and because any general text could contain references

to intelligence, all these sub-sections were scanned for sources. The rest of the heading selection procedure was formulated on the basis of the subjects indicated in the headings and sub-headings. For the years 1894-1899, see Appendix 4 for all Contents lists and the sections and sub-sections that were scanned.

Prior to 1894, when the Psychological Index was inaugurated, no single bibliographical source relevant to the subject under investigation has been discovered. While the American Journal of Psychology was founded in 1887 and the Pedagogical Seminary in 1884, these journals did not purport to present all potentially relevant material. A number of additional publications to which American psychologists submitted their articles prior to 1894 were isolated, and the Contents pages of the following journals were scanned for relevant publications. The selection procedure was the same as that outlined above for the Psychological Index:

1. Mind
2. Science
3. Popular Science Monthly
4. American Journal of Education
5. Philosophical Review
6. American Journal of Psychology
7. Pedagogical Seminary

All the above journals contain extensive lists of new publications, making possible the selection of books as well as articles.

Period 2: 1910-1919. The list of possible sources for the second

time period was developed by using the Psychological Index, and following the selection procedures specified for the first period. For copies of the Contents lists and their selected headings and sub-headings, see Appendix 5.

Scanning Procedure. List selection was made, as indicated above, on the basis of source titles. Any title which appeared in English, did not represent a translated work, and was indicative of including some attention to mental activities, was recorded. The conceptual profile listed below was developed to formulate both as precisely and inclusively as possible the grounds for recording any title as a member of the required list. Where titles contained any of the terms grouped under A and B, they were recorded. Those containing the terms listed under C, D, E, F and G were recorded if the title also indicated that the focus of the work related to cognitive processes and functions, or mental characteristics. All titles dealing with mental testing (H) were recorded.

Cognitive Profile for Source Selection.

A. cognitive processes, functions

   a) intelligence, intellection

   b) (mental) ability, trait, skill, capability, aptitude, qualities, attributes, dimensions

   c) performance

   d) judgment

   e) talent

   f) reason(ing)

g) thought, think(ing)

h) memory, recollection

i) knowledge, knowing

B. mental characteristics

a) (mental) faculties

b) (mental) factors

c) (mental) status: i. bright, genius

ii. dull, deficient, deficiency, imbecile, idiot, moron, subnormal

iii. exceptional

d) mental disease

C. mind

D. learning, education, pedagogy, development

E. individual differences

F. children

G. test(ing)

H. mental test(s)(ing)

Clearly, the selection procedure involved scanning thousands of sources. While many historically prominent names were encountered, raising no difficulty with nationality placement, many more were far more obscure and largely unknown. During the initial selection process, the only names whose works were excluded were those of known non-American nationality, e.g., Wundt (German), Galton, Spearman, Pearson (English), Binet, Simon (French). The nationality of all other authors was verified

later, so that the final list consisted only of works written by Americans, or by those whose academic careers took place largely in the United States. Two figures who illustrated this point were Titchener and Munsterberg. Titchener, an Englishman, came to Cornell University in the States in 1892 after obtaining his Ph.D. under Wundt in Germany. He remained at Cornell until his death in 1927 (Boring, 1950, p. 412). Munsterberg, a German, spent 1892 to 1895 at Harvard University, returned to Germany for two years, then worked again at Harvard until his death in 1916 (Boring, 1950, p. 428). Their names feature prominently in all historical considerations of early American psychology. For these reasons, the relevant works of both men were included. In his recent bibliographical source, Eminent Contributors to Psychology, Watson recorded both figures under the nationality of their career environment as well as their country of birth (Watson, 1974, pp. 314-9).

A list of approximately 3,000 sources was developed. This list constituted the body of primary sources on intelligence and its synonyms published during the two time periods. At this point, no attempt had been made to survey any of the sources in order to confirm their relevance. As specified above, their inclusion was directed by the terms contained in their titles.

Research Design

Target Words. As indicated earlier, the target words investigated in this study were intelligence and its temporally relevant synonyms. For Period 1, these synonyms were drawn from Baldwin's Dictionary of

Psychology and Philosophy (1901, p. 558), which has recorded the following terms as being synonymous with intelligence:

intellect

faculty of knowing

capacity of knowing

mental faculty(ies)

mental capacity

intellectual capacity

intellectual faculty

No dictionary of psychology published in America in the early 1920's has been discovered. The one closest in time of publication to the end of Period 2 appears to be H.C. Warren's Dictionary of Psychology (1934). Therefore, the synonyms for intelligence for Period 2 were drawn from Pintner (1923). This is not the only book dealing with intelligence testing published in the early 1920's; it does, however, contain a section (pp. 45-51) which summarizes the definitions of intelligence in current use, and provides a considerable number of synonyms:

mental trait	capacity
higher complex > mental processes	general capacity
	adaptability
mental efficiency	faculty
ability to learn	learning power
general ability	

The word intelligence was considered as one target word for each

period, and its temporally relevant synonyms were investigated as the second target word. This procedure was adopted in order to control for the possibility that the synonyms, which theoretically can be substituted for the word intelligence in any of the primary sources, were nevertheless embedded in different linguistic contexts. During Period 1, intelligence, as Tuddenham pointed out (1968, p. 469), was barely recognized as a psychological concept. During Period 2, Spearman's "g" factor theory of intelligence "came to constitute the conceptual basis for Binet's test approach" (Tuddenham, 1968, 504), thus keeping such synonymous terms as "mental faculties" legitimately operative within the discipline of scientific psychology. It is nevertheless conceivable that the use of this and other synonyms was embedded within a different linguistic context.

Time Strata. Both of the decades selected for this study were significant for the nature and extent of their focus on matters related to intelligence. Peterson (1926, 72-95) and Young (1923) specified the last decade of the nineteenth century as the period during which the first efforts to establish mental tests were made. While the influx of dozens of mental tests came at the beginning of the twentieth century (Peterson, 1926, 117-214), the first steps were being taken during the 1890's to place discussions of intelligence on an empirical footing.

The second decade of the twentieth century has been noted as the heyday of mental testing, and the debates inaugurated by Boring (1923) and by Walter Lippmann's articles did not begin until the 1920's (Young,

1923). Yet this decade, like the 1890's, appears to have produced both excited support for mental testing and the beginnings of trenchant criticism. Both periods seem to have produced important arguments, debates and advances in the area, and in considerations of intelligence by professional psychologists.

For example, group intelligence tests were developed for use with the Armed Forces during World War I. The formulation of these, the Alpha and Beta Intelligence tests, occurred towards the end of the second decade selected for this study. It was felt that this, and possibly other advances or changes in focus, procedure, etc., could have contributed to detectable changes in the use of the word intelligence in some psychological documents. Consequently, both of the time periods to be explored in this study were stratified into two five-year sections (see Table 4 for a summary of the research design).

Sampling Procedure

The list of approximately 3,000 primary sources was arranged alphabetically according to authorship for each five-year section. Where any one author was found to have published a number of works during any of the five-year sections, the years of publication for each work were indexed randomly. The first work of each author was then drawn from this index for each of the resulting four lists of primary sources. These samples were surveyed for verification of relevance to this study. Where any source was found not to contain the target words, it was discarded. If the author of the discarded source had published more than

Table 4

Summary of Research Design

Time Periods	Target Words	Codes
1890-1894	intelligence	IN9094
	intellect; faculty of knowing; capacity of knowing; mental faculty(ies); mental capacity; intellectual capacity; intellectual faculty	SY9094
1895-1899	intelligence	IN9599
	intellect; faculty of knowing; capacity of knowing; mental faculty(ies); mental capacity; intellectual capacity; intellectual faculty	SY9599
1910-1914	intelligence	IN1014
	mental trait; higher/complex mental processes; mental efficiency; ability to learn; general ability; capacity; general capacity; adaptability; faculty; learning power	SY1014
1916-1919	intelligence	IN1519
	mental trait; higher/complex mental processes;	

Table 4 (cont'd)

mental efficiency; ability to learn; general ability; capacity; general capacity; adaptability; faculty; learning power	SY1519

Table 4. Codes provided in this table are used in all subsequent tables and figures for ease of communication: IN = intelligence; SY = synonyms; 9094 = time period 1890-1894, etc.

one potentially relevant work in the same time segment, the second indexed source was surveyed.

This sampling procedure was adopted in order to obtain as broad and inclusive a sample as possible for all uses of the word intelligence and its synonyms. All four samples included both books and articles, with articles outnumbering books considerably. A list of the four samples of primary sources used can be found in Appendix 6.

Content Unit Selection. One example of the use of any target word was isolated for empirical investigation in each source sampled. Efforts were made to ensure that the place of appearance in any source—beginning, middle, or end—was distributed randomly. A total of 12 content units was selected for each text sample in Period 1 (IN9094, SY9094, IN9599, SY9599) and 21 for each in Period 2 (IN014, SY1014, IN1519, SY1519). The number of content units was directed largely by the number of sources available for Period 1. As the membership records of American Psychological Association show (Fernberger, 1932), the Association had 31 members in 1892 and approximately 450 by 1920. As indicated earlier, five lines of IBM cardpunched text represents one content unit. With an average of 12 words per line, the body of text analyzed for Period 1 was 240 lines, or 2880 words. For Period 2, 420 lines, or a total of 5040 words, were analyzed.

Since Laffal's instrument is computerized, all of the coding of the textual material (content units) into conceptual categories was conducted

by Laffal's computer program. The data consequently made available for analysis were figures articulating numerically the strength of categories found in close association with the target words for each of the four time segments.

Consideration of the Data Collected

As was discussed in Chapter 2, the text samples were categorizable into 118 different categories in Laffal's instrument; therefore, the data were recorded in an 8 x 118 table (see Table 5). The figure in each cell represented the potency of any one category in the sample text representative of the use of either target word in any one of the four time segments. The fact that they were arrived at by adding the number of times words were coded into any one category was interpreted as an indicator of the potency of that category. The more often words and phrases in the content units were coded into a category, the more potent that category became as a close associate of the target word in that content unit. Thus the figure 12 for the category LARG during 1890-1894 when intelligence was the target word (IN9094), illustrated the potency of that conceptual category when a sample of documents about intelligence during that time segment was analyzed. In order to obtain a general assessment of the arrangement of the relative potency of all 118 categories in each of the eight samples, the eight time and target word columns (n=118) were cross-correlated using Pearson's product-moment correlation. All of the correlation coefficients in the correlation matrix (see Table 6) were significant beyond the .01 level.

Table 5

Primary Data for Application of Pearson product - moment correlation

Category	IN9094	SY9094	IN9599	SY9599	SY1519	IN1519	SY1014	IN1014
1.	0.	2.	1.	5.	5.	6.	2.	7.
2.	0.	3.	4.	3.	2.	4.	6.	2.
3.	2.	5.	2.	2.	8.	2.	4.	4.
4.	10.	9.	15.	10.	22.	13.	23.	19.
5.	1.	2.	6.	5.	0.	0.	1.	6.
6.	0.	0.	0.	0.	0.	0.	0.	0.
7.	2.	1.	2.	1.	2.	5.	5.	1.
8.	0.	0.	0.	3.	1.	1.	0.	1.
9.	5.	7.	2.	1.	8.	5.	9.	12.
10.	3.	2.	3.	0.	4.	9.	0.	7.
11.	2.	2.	3.	9.	2.	9.	12.	7.
12.	4.	8.	3.	4.	3.	1.	0.	4.
13.	0.	3.	3.	0.	0.	0.	0.	0.
14.	1.	1.	1.	0.	0.	0.	1.	0.
15.	0.	0.	0.	0.	1.	0.	0.	3.
16.	0.	0.	0.	0.	0.	0.	0.	0.
17.	0.	0.	2.	1.	2.	5.	0.	1.
18.	0.	0.	4.	0.	1.	0.	1.	0.
19.	0.	0.	2.	0.	0.	2.	0.	0.

Table 5 (cont'd)

20.	7.	15.	5.	3.	8.	12.	18.	14.
21.	3.	1.	1.	4.	6.	2.	4.	0.
22.	1.	0.	1.	0.	0.	0.	0.	1.
23.	1.	0.	0.	0.	0.	0.	0.	0.
24.	2.	2.	4.	2.	3.	10.	0.	4.
25.	0.	2.	1.	0.	9.	10.	1.	5.
26.	8.	5.	6.	14.	19.	14.	14.	31.
27.	4.	7.	11.	10.	15.	12.	15.	9.
28.	5.	4.	6.	0.	13.	6.	5.	5.
29.	2.	1.	0.	0.	1.	0.	2.	3.
30.	8.	3.	12.	9.	27.	20.	11.	14.
31.	3.	18.	9.	6.	9.	4.	10.	16.
32.	0.	0.	0.	0.	0.	0.	0.	0.
33.	1.	0.	1.	0.	0.	1.	1.	2.
34.	2.	5.	2.	0.	3.	7.	1.	7.
35.	3.	1.	0.	0.	0.	0.	0.	0.
36.	2.	1.	0.	6.	4.	2.	2.	2.
37.	3.	0.	0.	0.	1.	0.	2.	0.
38.	0.	0.	5.	7.	1.	8.	1.	2.
39.	17.	14.	9.	8.	10.	29.	29.	21.
40.	7.	9.	5.	9.	8.	10.	18.	15.
41.	0.	3.	0.	0.	2.	0.	0.	1.

Table 5 (cont'd)

42.	1.	0.	0.	0.	1.	0.	1.	2.
43.	1.	1.	2.	0.	1.	2.	1.	3.
44.	11.	3.	10.	10.	12.	9.	18.	18.
45.	3.	3.	8.	5.	9.	6.	12.	7.
46.	5.	3.	14.	8.	28.	18.	21.	28.
47.	5.	8.	5.	4.	8.	7.	5.	8.
48.	1.	1.	2.	2.	3.	0.	0.	2.
49.	0.	0.	0.	0.	0.	3.	0.	0.
50.	1.	2.	0.	0.	2.	0.	1.	2.
51.	1.	0.	0.	1.	0.	0.	0.	1.
52.	1.	1.	1.	1.	6.	10.	4.	7.
53.	0.	0.	0.	0.	0.	0.	0.	0.
54.	50.	51.	36.	30.	53.	83.	96.	66.
55.	5.	7.	1.	3.	2.	5.	4.	5.
56.	0.	0.	0.	0.	0.	0.	0.	1.
57.	7.	6.	2.	1.	14.	4.	18.	14.
58.	2.	1.	3.	0.	1.	0.	10.	3.
59.	1.	3.	6.	4.	14.	5.	8.	7.
60.	12.	8.	7.	8.	11.	7.	12.	18.
61.	0.	1.	0.	0.	1.	4.	2.	1.
62.	7.	9.	6.	7.	13.	12.	7.	24.
63.	2.	4.	3.	2.	5.	2.	1.	4.

Table 5 (cont'd)

64.	2.	2.	5.	0.	0.	2.	1.	8.
65.	11.	4.	3.	7.	12.	7.	30.	9.
66.	0.	2.	0.	0.	3.	0.	2.	2.
67.	2.	1.	0.	1.	4.	3.	2.	5.
68.	1.	0.	1.	1.	2.	0.	1.	2.
69.	1.	2.	2.	1.	2.	1.	3.	0.
70.	16.	17.	16.	18.	20.	19.	21.	38.
71.	4.	6.	0.	0.	11.	15.	8.	11.
72.	1.	3.	1.	0.	3.	4.	1.	2.
73.	6.	6.	10.	11.	13.	14.	12.	15.
74.	0.	2.	1.	0.	1.	0.	0.	0.
75.	0.	4.	0.	0.	0.	0.	1.	1.
76.	2.	6.	6.	4.	12.	7.	12.	8.
77.	2.	0.	0.	2.	7.	2.	0.	3.
78.	8.	2.	6.	13.	13.	13.	20.	17.
79.	0.	0.	0.	0.	0.	0.	0.	0.
80.	14.	10.	7.	14.	38.	22.	17.	19.
81.	6.	5.	5.	1.	9.	20.	11.	8.
82.	12.	9.	5.	13.	23.	18.	21.	16.
83.	1.	1.	2.	2.	3.	1.	2.	6.
84.	2.	0.	1.	4.	1.	5.	1.	2.
85.	5.	3.	5.	3.	2.	4.	5.	7.

Table 5 (cont'd)

86.	2.	1.	3.	4.	3.	10.	1.	5.
87.	4.	5.	3.	7.	3.	5.	2.	6.
88.	0.	2.	0.	1.	0.	0.	2.	2.
89.	8.	7.	14.	12.	29.	16.	14.	16.
90.	4.	0.	3.	0.	2.	0.	2.	2.
91.	1.	4.	1.	2.	1.	7.	5.	4.
92.	2.	0.	0.	0.	1.	1.	6.	5.
93.	4.	4.	2.	3.	2.	0.	4.	3.
94.	0.	0.	1.	3.	0.	0.	0.	2.
95.	6.	5.	8.	4.	10.	14.	10.	11.
96.	4.	0.	1.	0.	5.	5.	1.	3.
97.	10.	10.	11.	11.	13.	15.	21.	16.
98.	1.	0.	0.	0.	0.	0.	0.	0.
99.	3.	6.	7.	13.	15.	10.	10.	13.
100.	7.	12.	1.	1.	4.	1.	6.	8.
101.	2.	6.	10.	7.	13.	10.	8.	8.
102.	0.	0.	1.	1.	1.	1.	0.	0.
103.	9.	8.	8.	16.	17.	11.	15.	15.
104.	12.	5.	7.	8.	20.	37.	28.	19.
105.	1.	1.	0.	0.	2.	0.	1.	2.
106.	10.	7.	15.	5.	27.	31.	18.	15.
107.	5.	2.	3.	3.	7.	6.	5.	7.

Table 5 (cont'd)

108.	2.	0.	0.	1.	0.	0.	0.	0.
109.	10.	10.	8.	15.	16.	17.	19.	15.
110.	0.	0.	0.	0.	0.	1.	1.	2.
111.	0.	0.	0.	2.	0.	0.	0.	1.
112.	7.	9.	8.	13.	6.	9.	13.	13.
113.	0.	1.	0.	0.	0.	0.	0.	0.
114.	1.	1.	4.	9.	17.	12.	3.	3.
115.	10.	12.	11.	11.	10.	14.	15.	15.
116.	2.	2.	2.	3.	5.	12.	5.	6.
117.	13.	6.	9.	4.	15.	4.	18.	8.
118.	12.	8.	1.	13.	16.	26.	29.	25.

Table 6

Pearson Product-Moment Correlations Between

All Values of Time and Target Word Variables

	IN9094	SY9094	IN9599	SY9599	IN1014	SY1014	IN1519	SY1519
IN9094	1.0000							
SY9094	.8776	1.0000						
IN9599	.8115	.7881	1.0000					
SY9599	.7801	.7137	.7966	1.0000				
IN1014	.8851	.8379	.8377	.8429	1.0000			
SY1014	.9392	.8526	.8201	.7896	.8833	1.0000		
IN1519	.8720	.7889	.8060	.7558	.8554	.8894	1.0000	
SY1519	.8006	.6997	.8299	.7996	.8407	.8086	.8353	1.0000

Table 6. Correlation matrix with n of 118 (categories). All values of r were significant beyond the .01 level.

For reliability of category distribution, a split-half (or odd-and-even) analysis was conducted, correlating the textual analysis of every other word in the content unit samples beginning with the first word, with that of every other word beginning with the second. This procedure was adopted in order to account for the possibility of sampling error (see Table 7).

The uniformly high positive correlation coefficients in Table 6 suggested that the 118 categories had approximately the same relative position in the distribution of potency figures across all values of the time and target word variables. The results, therefore, indicated that early American psychologists shared largely the same general conceptual understanding of intelligence when writing on the subject. Whether the word intelligence of any of its synonyms was used as the target word, the obtained correlations were significant beyond the .01 level. This was also the case when the synonyms for Period 1 were compared with those of Period 2. While these synonyms were not the same for each period (see p. 41 above), the results suggested, since all were synonymous with intelligence, that their use did not alter the generally similar understanding of the subject as revealed in the documents sampled. Distributions of potency figures associated with all values of the time variable, including the two most widely separated (1890-1894/1915-1919), were also significantly correlated.

In the documents sampled, the data indicated that early American psychologists writing about intelligence frequently associated the use

Table 7

Reliability of Text Samples for Each Value of the Time and Target Word Variables

Time and target word	Reliability
IN9094	.8613
SY9094	.7732
IN9599	.8562
SY9599	.8243
IN1014	.9014
SY1014	.9115
IN1519	.9158
SY1519	.8074

Table 7. Odd-and-even analysis for category distribution reliability (n=118). All values of r were significant beyond the .01 level.

of that word closely with such concepts as thinking, memory, knowledge, rational processes, understanding, and awareness. Each of these words and phrases is coded under the category IDEA, which was the most frequently coded category in all of the eight text samples. They also used many words and phrases coded into the category FORM, which contains "references to structures and configurations both of a concrete and an abstract nature" (Laffal, 1973, p. 23). Such ideas as those related to shapes, forms, plans, policies, theories, etc. are coded into this category. Again, the category MOTV appeared frequently; ideas related to "motivational and feeling states, predilections and responsibilities: (Laffal, 1973 , p. 26) were often used in close association with intelligence and its synonyms. The results in Table 6 suggested that, to approximately the same degree, the relative potency of the 118 categories was the same during the periods investigated. Overall, then, the conceptual understanding of intelligence among American psychologists was the same in 1919 as it was in 1890.

The fact that intelligence was used in common parlance in 1890, but had become, in addition, a psychological concept by 1919—and indeed by 1910—was discussed earlier (see Chapter 1). The relative stability of the general use of this word over a particularly eventful 30-year period is intriguing. Is this a characteristic, one feels impelled to ask, of any more of psychology's multitude of common parlance words? Do we still use such words as "personality," "behaviour," "learning," and "consciousness," for example, in ways whose general stability has

remained constant over the last one hundred years? And how do present psychologists use intelligence in their documents?

It would be very short-sighted to suggest that none of the research that has been conducted in any of these areas has had a significant impact on the ways in which these words are generally used by present-day psychologists. A more fruitful avenue to explore would be a comparison of the ways in which intelligence, personality, etc. have been used in documents written on the one hand, for popular consumption, and on the other, for professional communication. By and large, when any group is having a conversation in which the words "intelligence," "behaviour," and "personality" are used, there is a general agreement about what those words mean. A psychologist in the group may have a more precise and a more sophisticated understanding of those words, but they are still used in common parlance. An examination of the characteristics of the semantic fields surrounding these words as they are used in common parlance, as well as in professional documents, could illustrate certain differences between these groups. If professional psychologists do use these words with more conceptual precision, this could be demonstrated in a comparative study, using Laffal's instrument. It could be predicted that, while the two groups' general category profiles would be significantly correlated, the psychological texts made use of fewer categories than the popular texts. If the predictions were confirmed by the results obtained, it would be possible, and very interesting, to examine the direction of any comparative conceptual restraint in psychological

documents.

The assessment of the relative potency of the categories in the distribution of frequencies permitted responses to questions posed earlier (see p. 33) to be advanced. By and large, the parameters of the semantic field surrounding intelligence in the documents sampled were delineated by the results. As noted above, the contextual associates of intelligence and of its synonyms were approximately the same; we appear, therefore, to be dealing with just one semantic field. One of the four questions advanced earlier related to the characteristics of the semantic field surrounding intelligence in the documents sampled. In order to explore this question, some of the rows of category frequencies across the time and target word variables were examined.

This examination suggested that some row differences could be isolated. The fourteen highest frequency categories were ranked on the basis of the highest low frequency proportional scores across the eight profiles (for an illustration of this ranking procedure, see Table 8). The highest frequency categories were selected for this analysis because the scores reflected the number of times words in the sample texts were coded into the different categories. The highest frequency scores thus represented concepts which appeared most often in close association with the target words. It was felt that an examination of the categories most frequently associated with the target words could provide some interesting information. The absence, or occasional apperance only, of some of the 118 categories is, of course, another interesting feature

Table 8

Illustration of Category Rankings

Category Rank	Proportional Frequencies in Profile							
	IN9094	SY9094	IN9599	SY9599	IN1014	SY1014	IN1519	SY1519
1 IDEA	10.3	10.5	7.6	6.1	7.4	10.9	10.0	5.9
2 MOTV	3.3	3.5	3.4	3.7	4.2	2.4	2.3	2.3
3 VARY	2.1	2.1	1.8	3.2	3.3	2.2	2.1	1.9

Table 8. Three highest categories ranked for proportional frequency. IDEA was ranked #1 because the lowest proportion figure was 6.1, the highest of any low proportion. MOTV was #2 (next of the lowest proportional frequencies of 2.3); VARY ranked #3 with its lowest proportional high frequency of 1.8.

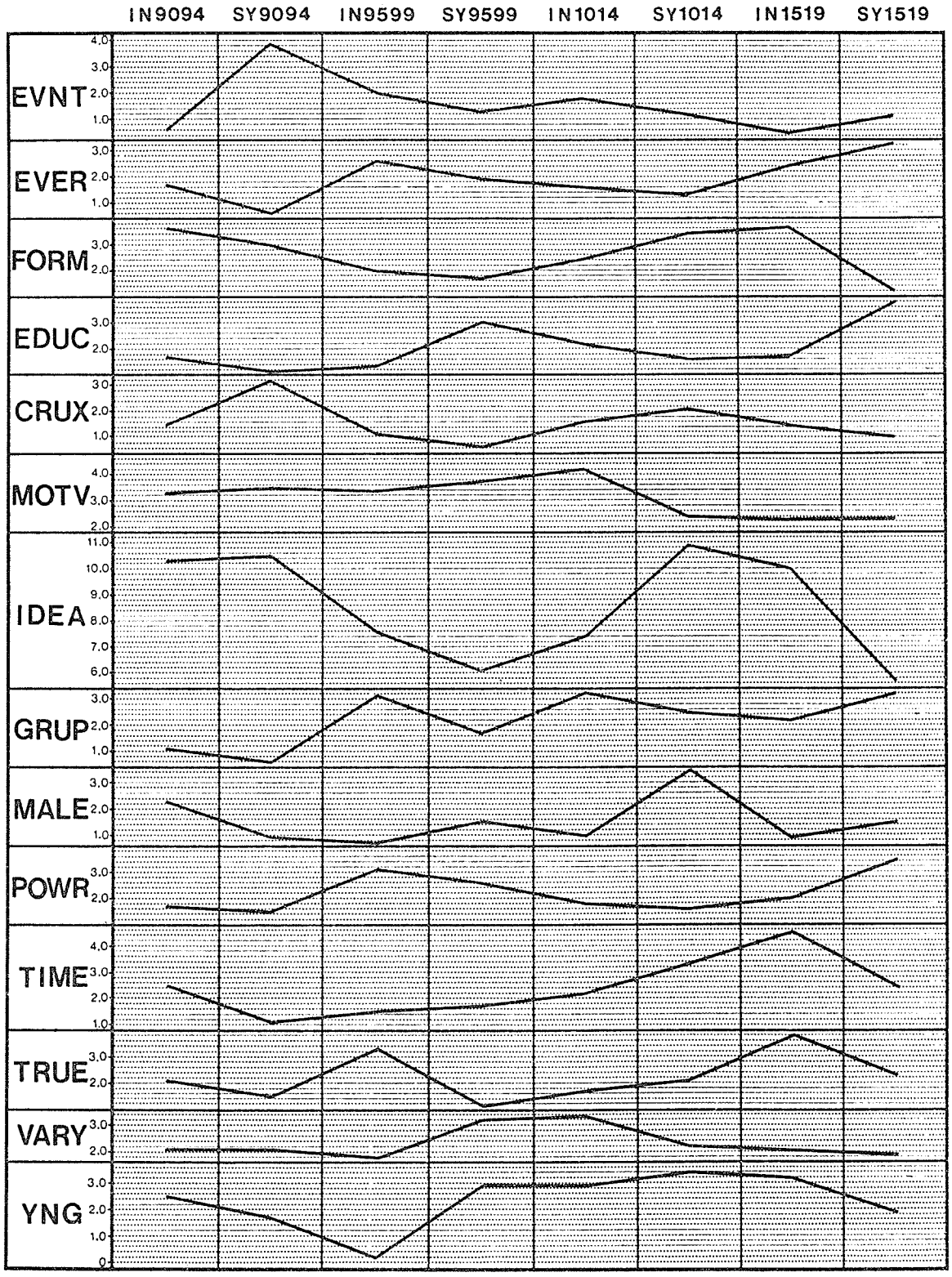
of the conceptual understanding of intelligence in these documents. However, it was decided to focus this discussion on what appeared often; in other words, what appeared to have been rather than what appeared not to have been. The top fourteen (rather than 10, 20, etc.) were selected because they appeared to form a definable block in terms of the frequency scores; the lowest high frequency per row declined to figures well below 1 per cent after the fourteenth highest frequency category.

Proportional scores rather than frequencies were used in this analysis because of the differences in text sample sizes betweeen Period 1 and Period 2. These proportional scores were used to calculate a total of 392 differences in proportions, expressed as z scores. The proportion scores for all comparisons for the fourteen categories were graphed (see Figure 1) to provide an illustration of the direction of frequency changes that occurred. 118 of the z scores calculated were significant at, or beyond, the .05 level.

Some of these significant differences in proportions have been isolated for discussion. In relation to the category EDUC, for example, which appeared intuitively to be highly relevant to the conceptual focus of this study, significant z scores were obtained when each frequency score for EDUC was compared with that for synonyms in 1915-1919. The category EDUC incorporates "references to education, learning, and teaching. Included are such ideas as schools and students; scholarship and erudition; teaching and training" (Laffal, 1973, p. 22). The pattern of the z scores in Figure 1 shows the direction these frequency scores

Proportion Scores for Highest Frequency Categories

Across All Values of the Time & Target Word Variables



took over time. There appeared to be some decrease during the 1890's, with a rise in the second half of the decade when synonyms were the target words. A similar pattern appeared during the second decade of the 20th Century, with a very marked increase, again where synonyms were the target words, during 1915-1919. These results give rise to a host of as yet unanswered questions. Why do we appear to have an increase in concepts coded into the category EDUC in 1895-1899, and again in 1915-1919? These increases only occurred in the documents sampled when synonyms were the target words. There did not appear to be any evidence overall that systematic differences between the use of intelligence and the use of synonyms as the target words were present in the documents.

Perhaps, and for reasons that are as yet undetermined, there was a relative upsurge during these two five-year periods in relating discussions of mental ability, i.e., of synonyms, etc., to the education process. It has been argued (Peterson, 1926) that interest in mental testing in the United States declined during the last few years of the nineteenth century and the first few of the twentieth century. This possible decline would not have been detected in this study, since emphasis has not been placed on frequency of reference to the concept intelligence, but rather, on how psychologists wrote about it. Sources considering the history of the child-study movement in America (e.g., Senn, 1975; Sears, 1975), have suggested that, while psychologists paid less attention to mental testing during this time, educators did not—it is conceivable that the first increase in concepts coded into EDUC

could be attributed partly to such a possibility. It would be interesting to compare the frequency of the use of the word intelligence between psychologists and educators—did the latter group use synonyms more frequently? (It will be remembered that such journals as Pedagogical Seminary and American Journal of Education were researched for articles relative to this study—whether the authors were psychologists, educators, or educational psychologists was not explored.)

It was more difficult to arrive at an explanation for the increase in potency for EDUC in the SY1519 cell. It could be that the extensive work with Army recruits during this time period was an operative factor; it certainly appears to be worth further investigation. It has been suggested (Peterson, 1926; Young, 1923) that the results of the Alpha tests gave rise to extensive discussion of the education system, among other things.

Another interesting pattern of differences appeared in relation to the category MOTV, defined by Laffal (1973, p. 26) as: "References to motivational and feeling states, predilections and responsibilities. The underlying ideas relate to: wishing and wanting; preferring and choosing; risks and attempts; obligations and committments; basic motivations such as hunger and thirst; concern and attention; attitudes and sensitivities." Significant *z* scores were found for every comparison with the proportional frequency score for IN1014 (intelligence, 1910-1914). What characteristics of the documents on intelligence that were written during this five-year period led to such a marked difference in

that particular conceptual area? In relation to the other high frequency categories, the IN1014 sample did not feature either more, or less, prominently in the occurrence of significant differences in proportions (for the list of significant z scores, see Table 9). One possibility is that some of the effects of Freudian theories were starting to be reflected in discussions of intelligence betweeen 1910 and 1914.

It was reported earlier that the category IDEA was the most potent in relation to the use of both target words during both time periods. Yet an investigation of Table 9 reveals that this relative potency was highest during 1890-1894, and for the two cells SY1014 and IN1519, with a significant decrease for SY1519. To some extent significant differences in proportion in relation to other categories seem to compensate for the periodic decreases found in relation to IDEA; for example (see Table 9), the categories GRUP, POWR, and perhaps EDUC. Words like "ability," for example, are coded into POWR; "class" is coded GRUP EDUC. It is possible that some of the ideas advanced in relation to the category EDUC are reflected in some of the differences detected in the potency of IDEA.

Clearly, all the groups of significant differences within categories give rise to a large number of interesting research possibilities. The results make it possible, and legitimate, to advance a number of hypotheses related to characteristics of the texts, or of any number of other factors, which may have accounted for some of these differences. It would be interesting to focus on authorship, or author groupings, as a variable; could any of the differences in relation to the category

Table 9

Significant z Scores for Proportional Differences

Within 14 Highest Ranked Categories

Category	Comparison	z Score	Category	Comparison	z Score
TRUE	IN9599/IN1519	2.40	IDEA	IN9599/SY1014	5.50
	IN9599/SY9599	2.27		IN9094/SY9599	2.34
	IN1014/IN1519	2.40		IN9094/SY1014	3.02
	SY9599/SY1014	2.74		IN9094/SY1014	4.03
	SY1014/IN1519	2.20		SY9094/SY9599	2.44
MOTV	SY9599/IN1014	2.72		SY9094/IN1519	2.92
	IN9094/IN1014	3.06		SY9094/SY1014	3.93
	SY9094/IN1014	2.89		IN9599/IN1519	4.51
	IN9599/IN1014	3.06		SY1519/IN1519	2.69
	SY1014/IN1014	2.52		SY1519/SY1014	3.70
	IN1519/IN1014	2.56		SY9599/SY1519	2.61
	SY1519/IN1014	2.41		SY9599/IN1519	5.22
				SY9599/SY1014	6.19
FORM	SY1014/SY1519	3.08		SY1014/IN1014	2.48
	IN1519/SY1519	3.19			
			GRUP	IN9094/IN9599	2.14
EVNT	IN9094/SY9094	3.33		IN9094/IN1014	4.06
	SY9094/SY9599	2.49		IN9094/SY1014	3.17
	SY9599/IN1519	3.04		IN9094/IN1519	4.75
	SY9599/SY1519	2.73		IN9094/SY1519	3.83

Table 9 (cont'd)

Category	Comparison	z Score	Category	Comparison	z Score
GRUP	SY9094/IN9599	2.70	TIME	SY9599/SY1519	2.29
(cont'd)	SY9094/IN1014	4.55	(cont'd)	IN1014/IN1519	2.45
	SY9094/SY1014	3.72		IN1519/SY1519	2.29
	SY9094/IN1519	3.30			
	SY9094/SY1519	4.33	CRUX	SY9094/IN9599	2.27
	IN9599/IN1014	2.19		IN9094/IN1519	2.22
	SY9599/IN1014	3.38		SY9094/SY9599	2.87
	SY9599/SY1014	2.44		IN9599/IN1014	2.08
	SY9599/IN1519	1.98		IN9599/SY1014	2.74
	SY9599/SY1519	3.13		SY9599/IN1014	2.69
				SY9599/SY1014	3.30
TIME	IN9094/SY1014	2.57		SY9599/IN1519	2.34
	IN9094/IN1519	3.64		SY1014/SY1519	1.98
	SY9094/IN1014	2.89			
	SY9094/SY1014	4.06	EDUC	SY1519/IN9094	3.74
	SY9094/IN1519	5.04		SY1519/SY9094	4.40
	SY9094/SY1519	3.03		SY1519/IN9599	4.17
	IN9599/IN1014	2.38		SY1519/SY9599	2.58
	IN9599/SY1014	3.60		SY1519/SY1014	2.57
	IN9599/IN1519	4.62		SY1519/IN1519	2.57
	IN9599/SY1519	2.53		IN1519/SY9094	2.08
	SY9599/IN1014	2.14		SY1014/SY9094	2.08
	SY9599/SY1014	3.38		IN9094/IN1014	2.15
	SY9599/IN1519	4.41	POWR	SY1519/IN9094	3.51

Table 9 (cont'd)

Category	Comparison	z Score	Category	Comparison	z Score
POWR	SY1519/SY9094	3.72	EVER	SY9094/IN9599	2.35
(cont'd)	SY1519/IN9599	2.33		SY9094/IN1014	2.69
	SY1519/SY9599	2.38		SY9094/SY1014	2.59
	SY1519/SY1014	2.32		SY9094/IN1519	3.59
MALE	SY1014/IN9094	3.02		SY9094/SY1519	4.45
	SY1014/SY9094	4.53		IN1519/IN9094	2.29
	SY1014/IN9599	4.78		IN1519/SY9094	3.59
	SY1014/SY9599	3.84		IN1519/SY9599	2.07
	SY1014/IN1014	3.41		SY1519/IN9094	3.26
	SY1014/IN1519	3.84		SY1519/SY9094	4.45
	SY1014/SY1519	2.82		SY1519/IN9599	2.44
YNG	SY9094/SY1014	3.51		SY1519/SY9599	3.05
	SY9599/SY1014	2.51		SY1519/IN1014	2.06
	SY9599/IN1014	1.97		SY1519/SY1014	2.63
	IN9094/SY1014	2.70			
	IN9599/SY9094	2.35			
	IN9599/IN9094	3.09			
	IN9599/SY9599	3.25			
	IN9599/IN1014	4.77			
	IN9599/SY1014	5.19			
	IN9599/IN1519	4.88			
	IN9599/SY1519	3.68			

EDUC, for example, be accounted for by comparing applied with experimental psychologists? One major problem which has, up to now, argued convincingly against defining such groups a priori has been the problem of arriving at satisfactory definitions. Basing these on specific and empirically derived text characteristics, however, would appear to be both persuasive, and equally important, replicable.

Historians of psychology have frequently displayed interest in the master-pupil relationship in early American psychology (see, for example, Boring & Boring, 1948). Efforts have been made to chart such a pattern of continuity as an explanatory tool. While a considerable amount of the necessary archival material is available, these efforts, nevertheless, have not been very convincing. How persuasively can one argue that Goddard's understanding of intelligence in the 1920's, for example, can be largely accounted for by the fact that he was a student of Cattel's some fifteen years earlier? Alternatively, it has been suggested by more than one historian of psychology (Sarbit, 1980; Reisman, 1966), that Goddard and Healy dealt with intelligence from two widely differing positions. Goddard was, by and large, a eugenicist, and Healy placed much more emphasis on the effects of an individual's social, educational and medical environment. The results of Stage 1 of this study suggest that it would be possible, by examining some of the characteristics of the semantic field surrounding the use of intelligence by these men, to explore this issue empirically. More generally, these results provide the researcher with a sense of direction when attempting to conduct

research in the history of psychology. They suggest that some aspects of such research can be conducted empirically; both a sense of direction and a useful empirical procedure are features of research in the area which, outside the narrative histories that plague our discipline, have been sorely lacking.

The category profile comparisons provided evidence that the relative potency of all categories were significantly correlated over time, irrespective of whether intelligence or its temporally relevant synonyms were the target words. The category proportions differences demonstrated by the significant z scores indicated that the categories in Laffal's instrument are potentially useful variables. These results suggested that the exploratory methodology developed for this study was able to isolate some significant conceptual differences in documents written by early American psychologists when considering the concept intelligence.

Chapter 4

Use of the Methodology in Hypothesis Testing: Stage 2

The general purpose of the study up to this point has been, when considering documents by early American psychologists containing discussions of intelligence, to devise an exploratory methodology which could provide some empirically derived answers to the question "what, not necessarily explicit, is contained in these documents?" The results of Stage 1 indicated that the general conceptual understanding of intelligence among early American psychologists was largely the same during the 1890's. Investigation of individual categories in Laffal's instrument for differences in proportion indicated that many of these were significant, suggesting that a number of semantic field characteristics (i.e., categories) were different across values of the time and target word variables. On the basis of these results, Stage 2 of the study was designed in an attempt to demonstrate the use of this methodology in hypothesis testing.

Historical introduction

In relation to efforts by psychologists both to explain the nature of intelligence, and to define it, "there has been," according to Tuddenham (1968), "a more or less continuous Donnybrook of rival views in which any number of theorists could and did join in." No one has

seriously challenged Wechsler's statement (1958) that

> Some psychologists have come to doubt whether these laborious analyses have contributed anything fundamental to our understanding of intelligence while others have come to the equally disturbing conclusion that the term intelligence, as now employed, is so ambiguous that it ought to be discarded altogether. Psychology now seems to find itself in the paradoxical position of devising and advocating tests for measuring intelligence and then disclaiming responsibility for them by asserting that "nobody knows what the word really means."

The most useful point to focus on appears to be not "what the word really means," but, rather, "when psychologist X used the word intelligence, how was it used?"; "within what linguistic context did psychologist X embed the word when writing about it?" The evidence of the present study up to this point has suggested that the broad parameters of this linguistic context were generally the same over a period of time in America when the word intelligence moved from its position as a common parlance term to that of a psychological concept. Within those parameters, however, there were many differences. Isolating some of those differences in relation to, say, psychologist X, could put a researcher in the position of being able to draw some inferences about what psychologist X meant when using the word intelligence. Up to now, the focus of historians has tended to be analogous to a (non-empirical) investigation of "between groups differences"; comparatively little attention has been paid to differences "within groups." With this point and the results of Stage 1 in mind, it was decided that Stage 2 would investigate some specific characteristics of the semantic field surrounding intelligence as it appeared in documents written by two American psychologists considered in history of psychology

texts to be representative of different and often contradictory philosophies and ideas about the subject. The two psychologists selected were H.H. Goddard and William Healy.

Neither Goddard nor Healy was an academic psychologist. From 1906 to 1918 Goddard was the Director of Psychological Research at the Vineland Training School in New Jersey, which provided service to mental retardates. Healy was a psychiatrist who, in 1909, became the Director of the Juvenile Psychopathic Institute in Chicago which concerned itself primarily with delinquent children. Goddard was the first American to translate Binet's 1908 measuring scale for intelligence, and according to many sources, became its ardent advocate (Tuddenham, 1968; Fink, 1938, pp. 219-239; Reisman, 1966, p. 105; Sarbit, 1980). The nature of his advocacy—and this point does not necessarily relate to his enthusiasm—was that he considered obtaining a certain score on this one intelligence test to be, in effect, the defining characteristic of feeblemindedness (Sarbit, 1980). Goddard was not alone in this position; the American Association for the Study of the Feeble-Minded recommended, in 1910, that diagnostic classification of retardates be based on Binet mental ages. It did suggest that original diagnoses be established on the basis of additional criteria, but Reisman (1966, p. 121) has claimed that "In practice, however, the idea of social incompetence tended to be neglected and mental age became the major criterion for making the diagnosis."

Given that intelligence, at this time, was generally seen as a function of heredity, with only lip service paid to environmental factors,

the unidimensional approach to such diagnosis and classification is not surprising. Goddard undoubtedly contributed strong support to this position with his successful study on The Kallikak Family (1912). In the conclusion of this book he stated (by his own admission, dogmatically):

> Feeblemindedness is hereditary and transmitted as surely as any other character. We cannot successfully cope with these conditions until we recognize feeble-mindedness and its hereditary nature, . . . (p. 117)

In an earlier paper (1911), Goddard devoted two paragraphs to an "environment list" of causes of feeble-mindedness. After specifying that many of these were not causes, but only "popularly thought" to be so, he concluded:

> But all these causes combined are small compared to the one cause—heredity. The vast majority of feeble-minded persons are so because parent or grandparent was feeble-minded and there is true inheritance.

The points that have been advanced here in relation to Goddard's somewhat less than complex attitudes towards mental testing, and his understanding of intelligence as a hereditary factor, are well-documented in numerous historical sources. In none of his publications that dealt either with theoretical or practical considerations of mental testing did he mention (or use) any test other than Binet's (e.g., Goddard, 1911, 1911-12, 1912, 1912-13a, 1912-13b).

The position of William Healy with regard to these areas has been less extensively researched, but is not difficult to detect in his own writings. By and large Healy has been passed over by historians of psychology; in Boring (1950)—undoubtedly the most thoroughly documented of our narrative histories—Healy is mentioned twice, each time in a

passing sentence (p. 569, p. 733). Reisman (1966) considered him to some extent but the most detailed accounts of his work, his philosophy, and the nature of his influence appear in texts dealing with delinquency and the history of criminology.

All of these sources make clear statements pointing to a major influence that Healy had on attitudes towards intelligence. In relation to a book Healy published in 1915, Reisman (1966, p. 130) stated that "it was eminently successful in influencing legal authorities and others to discard organic and hereditary concepts of etiology in favor of environmental and psychological explanations." (See also Fink, 1938, pp. 239-251; Hawes, 1971, pp. 250-262.) In a letter Healy wrote, in 1909, to Julia Lathrop, who was President of the Institute he directed, he stated that, for the projected study of juveniles in Chicago's detention homes, "The examination would have to involve all possible facts about heredity, environment, antenatal and postnatal history, etc." (quoted in Hawes, 1971, p. 250). Some three years later, when a second study was being suggested, he expanded the same point (Healy, 1912): "The study that is necessary is that of the whole human individual which includes all things that are likely to have influence upon the formation of character and conduct." The significance of Healy's position, when one is interested in comparing it with Goddard's, is twofold.

First, Goddard stated emphatically and repeatedly that the cause of delinquency was feeble-mindedness. In one article (Goddard, 1912-13), he maintained that

> We have the fact before us that 80% of the children in the Juvenile Courts [of Manhattan and the Bronx] are feeble-minded. . . . They come before the court because they have impulses which they, because of their defective mind, are unable to control.

In direct contrast to such a claim, Healy (1914) stated that

> It is most dangerous to proceed to rendering diagnosis or prognosis without knowledge of the individual's background in heredity, developmental history and social environment.

Besides arguing consistently for the significance of environmental factors in relation to causes of delinquency, Healy was also able to point clearly to the logical fallacy embraced by Goddard's position. He was not surprised, he said, to find that a higher proportion of mental defectives was found among delinquents; "obviously the brighter ones are handled under probation, are found positions, and succeed better on the outside because they have more foresight and learn better by experience" (Healy, 1914). Unlike Goddard, Healy noted that the hereditary intelligence proposed by the eugenicists and tested by the Binet scales was not necessarily the same thing as the intelligence measured by these scales.

The second point to which Healy's statement in the 1912 article gives rise when his position is being compared with that of Goddard, bears on their respective attitudes towards Binet's mental test. Goddard, as we have seen, was an enthusiastic advocate of Binet's scales as being able to provide the definitive answer he sought. Healy both advocated, and used, a wide variety of mental tests (Healy, 1914; see also 1913-14):

> In the present state of our knowledge concerning methods, discretion is needed in the selection of tests. Those

> primarily adapted to one group may not be valid for another social or age group.

With regard to Binet's test in particular, he argued against its exclusive use as the method of mental evaluation on the grounds that it, like many others being formulated, was (at that time) a language test. As such, he felt it to be unfair unless its use was combined with others to evaluate perception, mental imagery, self-control, ability to learn by experience, etc. Neither was he satisfied that tests established for children were equally valid for adults.

These may not have been the only areas in which Healy and Goddard were at odds, but do appear to have been the major ones. In the most detailed comparative study of both available to date (Sarbit, 1980), Healy's inductive, idiographic research procedures were contrasted with Goddard's interest in normative data and theories and his deductive approach. The unidimensional position of Goddard with regard to both testing instruments and the causality of delinquency was compared with Healy's insistence on multiple causation and his use of many diagnostic instruments, including the case history. Any of these points of difference between Healy and Goddard could be reflected in the ways in which each of these pioneer figures in psychology used the word intelligence in their documents. Some of the variables derived from Stage 1 of this study were used for the purpose of examining some of the semantic field characteristics surrounding the word intelligence when it was used by Goddard, and by Healy. On the basis of historical arguments found in the sources discussed above, it was possible to

formulate hypotheses using certain of Laffal's conceptual categories as variables. It was proposed that such a study could both demonstrate the use of the methodology being developed and provide empirical support for some theoretical arguments advanced by historians with regard to differences between Goddard and Healy.

There were, clearly, a number of apparent differences between Goddard and Healy, any of which could be detected in their use of intelligence in written documents. It was decided that Stage 2 would focus on just one of these areas of difference: that related to method, to measurement, and by association, to precision. These three words are each coded into different conceptual categories: method is coded FORM; measurement is coded MSMT, and precision is coded SHRP. Laffal's definitions of each of these categories are presented below:

> FORM: References to structures and configurations both of a concrete and abstract nature. The kinds of ideas in FORM are: shapes and forms; plans and policies; doctrine and theory; construction and configuration; maps and charts; rituals and ceremonies (Laffal, 1973, p. 23)
>
> MSMT: References to measurement and instruments of measurement (Laffal, 1973, p. 26)
>
> SHRP: References to sharp edges, points and precision. Included are such ideas as: knives and cutting; accuracy and clarity; concentrating and focussing (Laffal, 1973, p. 28).

It was predicted that all three of these categories would be significantly more potent in association with Healy's considerations of intelligence than they would be with Goddard's.

Method

One of the eight time periods investigated in Stage 1 was selected for this second stage. While there were no overall time differences detected in Stage 1 when entire profiles were compared, the differences in proportion established with *z* scores revealed many when attention was focussed on individual categories. In order to eliminate the possibility of time intruding as a confounding variable, just one period--1910-1914--was selected. A number of relevant documents published by each of the two authors in this time period were isolated. In both cases the dates of publication were fairly evenly distributed over the five-year period. Six of Goddard's documents were selected randomly from the list of primary sources compiled for Stage 1 of the study. Healy's primary focus was delinquency rather than feeblemindedness, and he made comparatively few references to intelligence or its synonyms in his publications. Of eleven articles written by Healy between 1910 and 1914, eight were found to contain uses of the target word and were used as the sample of texts representing Healy. Three of Goddard's documents contained two uses of the target word, one contained three, and one more was found in the sixth document. In two of Healy's articles two content units were isolated, with one each in the remaining six.

Both intelligence and its synonyms were selected as the target word. The results obtained in Stage 1 indicated that both synonyms of intelligence, and intelligence itself, were embedded within generally

similar linguistic contexts. However, to prevent the possibility of any bias intruding, the number of times either intelligence or its synonyms was selected was the same for each author. Ten examples of the use of the target word were isolated for analysis in each set of documents, six of intelligence and four of its synonyms.

The instrument used for textual analysis was the slightly less sensitive 1973 edition of Laffal's Contextual Associates. The difference between this and the 1979 version used both in Stage 1 and in the three studies in Appendices 1, 2 and 3, does not relate to the number or the definitions of the categories. In Laffal's 1973 edition, the number of words in his dictionary (i.e., words with assigned categories) was 23,500. In the 1979 edition, 42,228 categorised word entries were used (Laffal, 1979, p. 323). The 1973 version has been found useful in a number of studies, including one which focussed on documents written during the nineteenth century (Hyman & Shephard, 1980). The procedure which directs its use is identical to that for the 1979 version.

Three hypotheses were advanced:

1. That the potency of the category FORM in Healy's texts would be significantly stronger than in Goddard's.
2. That the potency of the category MSMT in Healy's texts would be significantly stronger than in Goddard's.

3. That the potency of the category SHRP in Healy's texts would be significantly stronger than in Goddard's.

In all three cases the data were analysed by testing for differences in proportion using a *z* score.

Results and Discussion

Of the three *z* scores calculated, two were significant beyond the .01 level (see Table 10). The category FORM was significantly more potent in Healy's documents in close association with intelligence and its synonyms, as was the category SHRP. There was no significant difference between the two psychologists with regard to the potency of the conceptual category MSMT.

Table 10

Differences in Proportion for 3 Categories in Goddard and Healy Texts

	Potency Figures		
Category	Healy	Goddard	*z* Score
FORM	8	0	2.86
SHRP	10	1	2.74
MSMT	3	6	1.01

FORM. As the figures in Table 10 indicated, at no time in the texts sampled did Goddard use words which would have been coded into this category. Healy, however, frequently used words such as "method," "model," "pattern," and "logical." For example, in one of the content units coded (from Healy, 1914), he was cautioning against indiscreet and uncritical selection of tests of intelligence: "In the present state of our knowledge concerning methods . . . ." [italics added]. When he used the words "model" and "pattern" he was examining critically the proposed efficacy of certain tests--none of which, incidentally, were taken from Binet's scales. These empirically derived results tend to confirm the historical arguments advanced by Sarbit (1980), Reisman (1966), Fink (1938), and Hawes (1971) that Healy was unwilling to regard diagnostic procedures unidimensionally. In a symposium conducted by Seashore in 1916 he reaffirmed his position:

> In young individuals and among the lower feebleminded we grade by Binet, plus school work and a few other simple tests. In all other instances we feel the urgent necessity of using a wide range of tests for special abilities, which, we find, begin to vary so greatly at 10 years or so of age. These often have much more significance for social implications than any age-level test.

Healy's position stands in marked contrast to Goddard's. For the same symposium, Goddard sent a letter to Seashore, in which his rather unqualified acceptance of the one test (Binet's) was made very clear:

> We cannot get away and never will get away from the mental levels as established by Binet. You will perhaps ask, as others have, why I hold so solidly to Binet. . . . I have found from nearly ten years living with the feeble-minded that Binet was correct in his theories of the feeble-minded

> and of their psychology, to a much greater extent than is given to most mortals (Seashore, 1916).

It must be remembered that Goddard worked with the feeble-minded, and Healy with delinquents. The requirements of their professional commitments with regard to both diagnosis and treatment were not the same, a position which could be advanced quite persuasively as some explanation for their different ideas about mental testing. The purpose of this study has not been, however, to either defend, justify, or perhaps even explain, these differences. The intent, rather, has been to demonstrate the usefulness of the methodology developed, and provide evidence that it can be used both to develop and explore empirically historical arguments based on a body of primary data.

SHRP. The significantly stronger potency of this category in the analysis of Healy's texts provided additional support for the argument that Healy paid more attention than Goddard to methodological issues. The words coded into this category (e.g., "perception," "points," "graphic scoring") were used, on investigation of the content units, in relation to assessments of testing procedures (e.g., Healy, 1914, 1911). Both the categories FORM and SHRP clearly incorporate concepts which one would expect to find closely associated with a critical focus on method and test design in a consideration of intelligence.

MSMT. The hypothesis that a significant difference between Goddard and Healy would be found with regard to the potency of this category was not supported. The logic which led to the formulation of this hypothesis related, once again, to Healy's apparent emphasis on a

wide range of diagnostic procedures, compared to Goddard's use of just one. The results suggested that both psychologists used words which would be coded into this category to approximately the same degree; and that MSMT did not discriminate significantly between the semantic field characteristics within which either writer embedded intelligence or its synonyms. Referral to the definition of this category (see p. 80) indicates that references to all measurements would be coded into MSMT irrespective of whether just one, or many types, were being discussed or referred to. This position had, in fact, directed against formulating a hypothesis predicting that the category TRUE would be significantly more potent in Healy's text. The word "test" is coded into TRUE; it had been felt that since this category did not discriminate between types of tests, an hypothesis could not be derived logically for its differential potency.

This argument could, perhaps, be considered to account for the failure of the hypothesis regarding MSMT to be confirmed by the data. However, this, it is felt, must be considered very tentatively. Inability to reject the null hypothesis is not unusual in most experimental research and can often be accounted for very persuasively by, for example, considering elements of conflicting theories and exploring prior research in the relevant areas. However, neither of those options was available for this study. At present, the most logical argument does appear to relate to the nature of the category MSMT, but that could well be the case partly because research in this whole

area is still new and comparative studies are almost nonexistent.

In an earlier study (Hyman & Shephard, 1980), it was hypothesized that concepts associated with the target word personality would be significantly different when the texts compared were drawn from two widely separated time periods which historians saw as representing different historical epochs. The data did not permit rejection of the null hypothesis of no difference, and it was suggested that this could have related to the nature of the texts sampled. The texts were popular novels, and evidence was provided that pointed to the popularity of the novels sampled for the first period in both time periods. It was suggested that the same hypothesis be explored in an analysis of texts sampled from novels of more ephemeral popularity in the first time period.

Alternatively, reference to Appendix 2 in this paper points to another study where the data did not permit rejection of the null hypothesis. In this case it was argued that it could be attributed, at least in part, to the doubtful stability of historical arguments in the current history of psychology texts. Speaking analogously, this could be expressed as being akin to the "lack of a criterion variable" in this research. The paucity of studies in the area, it is felt, argues strongly against facile attempts to account for failures to reject the null hypothesis.

The results obtained in Stage 2 of this study provided some evidence that demonstrated the usefulness of the methodology developed. The two hypotheses supported by the data suggested that it could be used successfully to provide empirical support for historical arguments. The failure to confirm the hypothesis relating to MSMT has, however, pointed to the care which needs to be taken in the formulation of hypotheses derived from historical arguments. The results of both Stage 1 and Stage 2 have indicated that Laffal's instrument is quite a sensitive one. Its use assists in the development of the semantic field surrounding a nominated target word, and in the isolation of its conceptual characteristics. A second study of Goddard's and Healy's documents, using either "test," or "measurement," or both, as target words, would provide the details of their semantic fields and its characteristics; these may overlap considerably with those derived for intelligence in this study.

Chapter 5

Evaluation of the Procedure

Historiography in history of psychology

The major purpose of this study was to make a significant contribution to historiography in historical considerations of psychology's past. It was argued in Chapter 1 that historians of psychology have paid very little attention to this subject--i.e., to the ways in which their histories are written--with a consequent stagnation of historical procedure. With the exception of Robinson (1976) and MacLeod (1972), psychologist-historians have continued to write the positivist narratives of which Young (1966) was so critical; "the study of the history of psychology," he maintained, "has suffered mightily from those who have taken it literally. . . . a scholarly tradition which is based primarily on textbooks has severe inherent limitations."

Psychologists would not hesitate to agree with such statements; without the enormous body of empirical research they have been so careful to encourage and record, the discipline would not have been able to develop so extensively. Yet, unlike physicists, biologists, and astronomers, for example, few psychologists have been able to move away from taking for granted didactic, expository histories of their discipline. More, and with less excuse, the same criticism can be advanced in relation to most psychologist-historians. What they have generally failed to do is regard the history of psychology as "a research dis-

cipline whose standards must be as high as those practised in the laboratory" (Young, 1966). Consequently, psychologists still have an image of psychology as a continuously progressing discipline; that whatever research was conducted in a certain field during, say, the 1960's, must by definition be an advance on that conducted in the same area forty years earlier. Such may very well be the case, but not ipso facto. Methodological sophistication appears to be increasing all the time, and new research instruments are being developed constantly. But, perhaps in the process, psychologists have tended to lose sight of the fact that they are still grappling with what MacLeod (1972) has called "the persistent problems of psychology." Robinson (1976) addressed this issue when he wrote of methodology as the metaphysic of psychology. By placing emphasis on the methodology of a study, he argued, to the exclusion of an equally rigorous consideration of the issue being investigated, psychologists have sometimes allowed themselves to be enraptured by their procedure rather than challenged by their questions. And, in order to respond to that challenge, a more sophisticated understanding of the fundamental problems on which these questions are based, and some of the ways in which earlier attempts were made to deal with them, would appear to be necessary.

For example, intelligence: social histories of turn-of-the-century America (e.g., Wiebe, 1967), generally agree that a desire to establish social order motivated much of social, political, educational, and economic life at that time. The professionals (including, of course,

psychologists) played a significant part in responding to expressions of this anxiety. Social progressivists, for example, were eager to design a humane, somewhat paternal social system to the carefully ordered benefit of all citizens. These humanitarian concerns are evident in Healy's work; the desire for order is clear in Goddard's. These, and doubtless many other issues (Sarbit, 1980) appear to have influenced the ways in which Healy and Goddard (as just two examples) designed their studies, interpreted their results, and presented their findings. Yet the matter now is being dealt with from a sharply conflicting position (see Opton, 1979). Early in the twentieth century, a major purpose in the development and application of intelligence tests was to separate the less from the averagely intelligent. The process that was then and, until very recently, praised as useful and necessary is now being criticized as discriminatory. It has not been the purpose of this study to enter into this debate; it cannot be denied, however, that its existence may be contributing significantly to the ways in which questions about intelligence are now being framed and empirically explored. The fundamental problem with which questions about intelligence have always been attempting to deal is that which seeks to develop an understanding of the basis of cognition. It can be argued that such attempts have always, and, in some ways, been influenced by concurrently existing social pressures. Unless historians of psychology begin to develop some procedures which will allow them to examine such matters, their discipline will continue to ignore, with potentially damaging results, some of the ways in which their work has been shaped.

By focussing on ways in which people expressed and communicated ideas, it is possible to explore historical episodes from both an internal (within the discipline) and an external (the social, political, economic, etc. context of a discipline) perspective. A consideration of aspects of the language contained in the professional documents of psychologists permits one to examine in some detail what psychologists were saying. A procedure that permitted such an examination was developed in this study, and attempts were made to demonstrate its usefulness. The results obtained in the studies designed for this purpose were felt to lay successful challenge to Young's argument that psychology's methodological rigor can make no significant contribution to historical research (while Young felt that history of psychology needed to become a scholarly, demanding research discipline, he tended to derogate the application of empirical procedures to historical research).

Alternative historical procedures

Procedures traditionally employed by historians have not been accessible to empirical verification. They have been detailed, careful, and well-constructed, but until recently have not involved the application of systematic and replicable procedures to a body of data. However, the use of principles of research design and quantitative methodologies by historians has been given impetus by the development of a number of content analysis instruments and by evidence of their usefulness (Carney, 1972; Holsti, 1969; Gerbner et al., 1969; Berelson &

Janowitz, 1967; de Sola Pool, 1959). In particular, the use of quantitative methodologies when considering historical questions (Mosteller & Wallace, 1978) and word meanings (Cliff, 1978; Kruskal, 1978) encourages the development of procedures suitable to the conduct of historical research.

The procedure developed in this study did not differ in principle from those traditionally used by historians. The differences lay in the means used to pursue the method of a) selection of sources, b) gathering of impressions, and c) drawing of inferences, that historians—indeed all researchers—employ.

Selection of sources. Like most other disciplines, psychology (at least in America) has kept a very careful record of all its publications. It was therefore possible to develop a list of potentially relevant sources for this study (see Chapter 3) which amounted, in effect, to the entire "population." Similarly, it was possible to define objectively the grounds for the selection of the sources (see Appendices 4 and 5). Subjective decision-making was therefore excluded almost entirely. By employing a sampling procedure, it was possible to collect data from defined groups of these sources, such that this first stage of the procedure was replicable.

Gathering impressions/data. Laffal's Contextual Associates Analysis appears to be the most sophisticated and the most frequently employed of all the content analysis instruments currently in use. Instead of gathering information by employing filtering processes to sort and organize impressions, the use of Laffal's instrument in this

study made it possible to collect data empirically. The general question "What, not necessary explicit, was contained in these documents?" could therefore be answered, i.e., this sample, drawn in this way from a population defined according to these restrictions, contained these data in relation to the use of the word intelligence.

Drawing inferences/conclusions derived from the data. By collecting data empirically, the use of the precedure developed in this study made it possible to conduct a number of statistical analyses upon which conclusions were based. Thus each section of the procedure is replicable, a quality to which traditional historical procedures cannot lay claim.

It is possible that some of Young's scepticism about the usefulness (in principle) of empirical procedures in historical reserach could relate to the restrictions necessary to the effective design and conduct of empirical reserach. At present, historians of psychology do not have an extensive body of scholarly research upon which to base their texts, particularly of empirical research in areas of interest to psychologist-historians. Since Young does feel that such studies are desperately needed, his implied objection ot the "smallness" of empirically-based historical investigations is questionable. The necessary precision and apparent restrictiveness involved may conflict with the broader sweep obtained when traditional historical procedures are used, but this precision, it is felt, permits the development of more precise, and defensible, historical arguments.

The development of historical arguments

The results obtained in Stage 1 of this study permitted a number of statements to be made. Despite the fact that between 1900 and 1910 the enormously influential work of Binet appeared, American psychologists embedded the word intelligence in their documents in semantic fields which were largely the same both during the 1890's, and from 1910 to 1919. Some of the characteristics of the semantic fields did change significantly during both these time periods, but by and large, the parameters of the fields did not. In other words, there was no significant general pre- and post-Binet effect when the use of the word intelligence was investigated. It would be interesting, and possibly revealing, to compare the linguistic context of Binet's use of this word in his translations with that of American psychologists. Could any of the semantic field characteristics that did appear to be significantly different after 1910 when they were compared with those between 1890 and 1899 have duplicated these characteristics in Binet's texts? Yet Binet was translated by Americans; it would not be unreasonable to assume that any translated work could reflect, at least in part, some of the translator's linguistic features. But in terms of linguistic content which we have noted as being the focus of Laffal's instrument, there may well have been detectable differences.

The fact that intelligence has always been used in common parlance gives rise to another interesting question. During the periods explored in this study, how was the word used in popular sources--novels,

magazine articles, etc., not written by psychologists? In another study which compared the use of some words used both in common parlance and as psychological concepts (Hyman & Shephard, 1980), the results obtained indicated that both groups used the words in approximately the same way. However, an examination of the raw data did suggest that there may have been some differences in the critical features of the semantic fields. These differences appeared to relate to the number of linguistic categories in Laffal's instrument that each group used; there was some suggestion that popular sources used the target words under investigation more loosely, and that their appearance in professional documents was surrounded by a smaller range of categorized concepts. This impression appears intuitively to be logical; it would not be surprising to find that when writing for professional communication, psychologists used these words more precisely. An exploration of this question in relation to the word intelligence could prove interesting.

The results obtained in relation to the semantic field characteristics provided variables that were found useful in Stage 2 of this study. If one were interested in comparing theories dealing with intelligence that were operative in early American psychology, for example, the use of appropriate contextual categories to explore hypothesized similarities and differences could be effective. It will be remembered that Tuddenham (1968) suggested that Spearman's g factor theory was incorporated into considerations of Binet's intelligence tests in early American psychology. Was this incorporation linguistically consistent between

Binet, Spearman, and American psychologists? Binet strove constantly to develop a testing procedure that would permit him to explore intelligence as a global, rather than a fragmented, concept. Yet Spearman, and almost immediately, American psychologists, were eager to retain their understanding of intelligence as being composed of different mental abilities. It would appear that the procedure developed in this study could deal very successfully with attempts to map the details of this presumed conflict, and the ways in which psychologists may have tried to resolve it.

In Stage 2 of the study, some of the variables derived from Stage 1 were used in order to subject an aspect of a historical argument dealing with Healy and Goddard to empirical investigation. The results obtained permitted statements to be made which generally supported this argument, thus establishing the usefulness of the procedure for such research purposes.

It will be remembered that the arguments of psychologist-historians have, up to now, been developed on the basis of traditional historical research procedures, which conclude by drawing subjectively impressionistic inferences. The results obtained in the study reported in Appendix 2 did not wholly support the historical arguments on which it was based; perhaps the impressionistic basis of these arguments could make it difficult, at times, to derive empirical support. It is not felt, however, that this necessarily reduces the usefulness of the procedure developed in this study; it would appear that psychologist-historians may benefit from the

application of their discipline's methodological rigor to questions related to the history of their discipline.

The research conducted in this study is still in an exploratory stage. It appears to have progressed satisfactorily beyong the initial study (Hyman and Shephard, 1980) which focussed on the use of one aspect of Laffal's instrument—general category profiles. As was reported earlier (see p. 31), analysis of the data collected in that study provided results which, in relation to three of the four hypotheses formulated, were not inconsistent with the historical arguments on which they were based.

One of the advances in the use of Laffal's instrument in the present study was the systematic focus on within-category differences across the time and target word variables. This particular emphasis permitted the reduction of very broad, general sets of data into much more specific and narrowly defined conceptual groups. Such a reduction in the complexity of historical data at this exploratory stage is very useful, and provides the initial step towards an inductive approach to the exploration of histories of psychology—another quality which, in this area, has been sorely lacking.

In relation to the general category profiles in this study, no time differences were found, despite the 20 year difference between 1894 and 1915. This, it was felt, did not argue convincingly against the use of Laffal's instrument in the procedure developed. When the general profiles associated with the target word personality, for example, were

compared between 1851-1807 and 1894-1914 in the earlier study (Hyman and Shephard, 1980), the predicted time difference was obtained. These, and other associated results, established the ability of Laffal's instrument to isolate temporal changes.

The present study selected for its exploratory demonstration an area in the history of psychology which has never been investigated empirically, and about which only the most tentative expectations could be advanced. While the 30 years between 1890 and 1920 saw enormous strides in the area of intelligence testing, there is no convincing evidence that these advances related significantly to the ways in which the target word intelligence was used. Tuddenham (1968) has, in fact, suggested that despite all the new work, ideas, test development, etc., there may well have been considerable implicit resistance in America to relinquishing an established understanding and use of the word. In effect, he appeared to argue, American psychologists were attempting less to acquire a greater understanding of the nature of intelligence, than to obtain the means of "measuring" that quality about which they had already firmly pre-conceived ideas.

Summary

The purpose of this study was to develop and demonstrate the usefulness of an alternate research procedure that would permit historical reconstructions in the discipline of scientific psychology from data not necessarily explicit gleaned from documents written by professional

psychologists. Stage 1 of the study was successful in isolating variables relevant to an investigation of the use of intelligence in early American psychology. In Stage 2, some of these variables were used in an exploration of their significance for a historical argument. It was concluded that the alternate procedure developed was useful, permitting the formulation, and testing, of hypotheses based on historical arguments.

Appendix 1

Comparison between insight and conventional wisdom

psychology in England over two time periods

This paper reports one of a series of studies designed to explore the usefulness of Laffal's Contextual Associates Analysis in historical research. Laffal has conducted a number of studies over the last twenty years (e.g., Laffal, 1960, 1961, 1963, 1976), but with the exception of two (Hartsough & Laffal, 1970; Laffal, 1960), all have focussed on changes in a patient's language during psychotherapy, or on general characteristics of language. None has called for the use of such variables frequently necessary to a historical study as time boundaries, geographical or national restrictions, social, professional or economic group membership, etc. When such variables are introduced, the desirability of exploring questions to which a different research design, and a different exploratory purpose give rise, becomes evident. Quite often, these questions are similar to ones which Laffal has dealt with successfully in a non-historical context.

Some of these questions were investigated in an earlier historical study (Hyman & Shephard, 1980). In an attempt to develop a methodology which would prove useful as an operational definition of Zeitgeist, Laffal's instrument was applied to data collected from popular and professional scientific sources written in England during one of two time periods--1851-1867 and 1894-1914. These time periods have been

seen by many historians as being both definable and historically important (e.g., Altick, 1973; Hynes, 1968, 1972). The data focussed on the contextual associates of four target words: personality, behaviour, environment, and heredity. The purpose of the study was to demonstrate that where predictions made on the basis of traditional mythologies (i.e., those of historians) were supported by the data obtained, the methodology delineating this operational definition could be considered useful.

One of the professional scientific bodies whose sources were explored was psychology, which was divided into two sub-groups--Insight and Conventional Wisdom. On the basis of arguments advanced by historians, principally in this case, by Boring (1950, p. 744, 1955, 1963), it had been predicted that within any one time period the category potency profiles associated with any one target word would be significantly different when Insight psychology was compared with Conventional Wisdom psychology. A second prediction compared the Conventional Wisdom psychology of each period, with the expectation of finding a significant difference between the two. In each case these predictions were confirmed by statistical analysis of the data.

However, in a historical study, where comparisons between time periods are frequently of interest, expectations of no difference may arise from the logic of the methodology. While there is no model that would permit "proof" of the null hypothesis, this condition of no difference could very well be compatible with the historical arguments advanced. An illustration of such a "no difference" expectation was

presented in the 1980 study, where a comparison was made between Insight psychology during Time 1 and Conventional Wisdom psychology during Time 2, in relation to the use of the word "environment." In accord with Boring's arguments (1950, pp. 744-745), no difference in potency profiles between authors representative of the two groups was predicted. The results obtained were not inconsistent with this expectation. They also provided evidence that the general language changes occurring over time did not obscure the conceptual similarities found in the two sources written fifty years apart.

The purpose of the present study was to explore another question related to the same body of data. While the 1980 study focussed only on the contextual associates (general) profiles of the target words, another feature of Laffal's studies, the potency of individual categories, has not been explored in historical research. In effect, while the general outline of the semantic field associated with a target word has been found to be a useful historical tool, the semantic features of these fields have not been examined. Laffal (1961) has shown, in a non-historical study where he was hypothesizing change, that his instrument can isolate both general profile shifts and specific changes in category potency. A logical corollary would be that where a prediction of no change in general profile is supported by the data, then similarly, a prediction of no change in specific category potency could be made. This corollary was explored in the present study.

Method

Considerations of the concept environment in history of psychology texts have been directed primarily towards some of its components, e.g., education, child-rearing, social class, and associated or comparative ideas, e.g., evolution and heredity. Many history of psychology texts have not indexed the word (Boring, 1950; Wolman, 1968; Robinson, 1976), and none has addressed directly the subject of interest in this study -- what appears to have been the nature of the conceptual understanding of the term in English psychology during the latter half of the nineteenth century?

Rather casual inclusion of the word in history of psychology texts appears in relation to two general areas: a) discussions of the impact on psychology of the Darwinian revolution, and, later, the nature/nurture debate, and b) the development of comparative psychology. Examination of these discussions led to the isolation of a number of concepts apparently associated with the term environment; the history of psychology texts were augmented by a number of sociological sources (Nisbet, 1969; Bock, 1963; Bristol, 1915).

Associated Concepts	Contextual Categories
development	(LARG) VARY
progress	FORW GO
growth	LARG VARY
change	VARY
adaptation	SIML VARY

Associated Concepts	Contextual Categories
adjustment	SIML VARY
functional behaviour	WORK EVNT
functional habits	WORK EVERY
correspondence between internal and external relations	SIML JOIN IN OUT SIML JOIN

No clearly formulated arguments about variations in the conceptual understanding of the word environment in nineteenth century English psychology were uncovered. However, there were reasonably direct comments in the texts referenced above that suggested fairly close association of these words and phrases with the concept environment. When they were coded into Laffal's categories, a total of eleven categories appeared (for definitions, plus examples, see Table 1).

It was therefore predicted that there would be no difference in category potency for these eleven conceptual categories between the insight source published in 1855 and the conventional wisdom source published in 1899. It was similarly expected that the general profiles of each would be significantly correlated, indicating that all the 118 categories would have approximately the same relative potency in the distribution of frequencies (i.e., of potency figures).

Sources. Works which hindsight shows as having had a major impact upon psychology in England within twenty years subsequent to the period during which they were published were seen as being representative of

Table 1, Appendix 1

Definitions of Categories With Examples

EVER References to constancy, habituation and duration. Included are such ideas as: everlasting and continuous; rigidity and hardness; custom and habit; the usual and the conventional; consistency and stability; preservation and permanence.
Examples: constant, keep, regular

EVNT References, usually of a somewhat abstract and non-specific nature, to events, behavior and circumstance. Included are such ideas as: mien and bearing; happening and occurring; state and situation; being and doing.
Examples: act, behaviour, situation

FORW References to front, forward and sequences. Words of future time are included with the added scoring TIME, but future tense of verbs is not categorized as such. The kinds of ideas included are: forward and toward; preceding and before; series and lists.
Examples: advance, succeed, toward

GO References to travel and movement. Movement is implicit in many words, but GO is scored only when it is prominent in the meaning. Words relating to roads and directions are

under PATH. The kinds of ideas included in GO are: traveling and going; gyrating and dancing; sending and throwing.
Examples: move, send, take

IN References to being physically inside, to entering, and to being directed inward. The preposition in is categorized IN only when there is clear allusion to being physically in.
Examples: absorb, implant, internal

JOIN References to coming together and uniting. Included are such ideas as: allegiance and ally; meeting and joining; touch and contact.
Examples: between, combine, interplay

LARG References to largeness, increase in size and distance, and weightiness. Included are such ideas as: bigness and thickness; excessive and overdone; blossom and grow; add and extend.
Examples: bulk, extent, scope

OUT References to being outside, to discharging and to emanation. Included are such ideas as: emitting and expelling; external and outward; extraction and extirpation; erasing and eroding.
Examples: abstract, issue, without

SIML References to similarity, equality and suitability. Included are such ideas as: approximation and agreement; applicability and appropriateness; repeating and retracing; sign and symbol;

consistency and evenness.

Examples: according, equal, symmetry

VARY References to alteration of form or position, deviation, instability and transience. Included are such ideas as: modification and evolution; plasticity and flexibility; oscillating and vibrating; avoiding and replacing; bending and circling.

Examples: bias, exchange, various

WORK References to work and to activities requiring considerable expenditure of energy. Included are such ideas as: construction and manufacture; hand tools and jobs; effort and labor.

Examples: busy, effort, task

Table 1. Definitions and examples taken from Laffal, 1973, pp. 22-30, pp. 291-296.

insight into issues which came to be of concern to scientific psychology. If these works made no impression when they were first published, but did so up to twenty years later, they can be seen as forerunners in the discipline. These criteria established the definition of *insight* sources.

Both Boring (1950) and Wolman (1968) have suggested that Herbert Spencer had a major influence on the development of psychology in England during the last half of the nineteenth century. However, as Spencer recorded in his *Autobiography* (1904), the sales record of his *Principles of Psychology* (1855) was minimal, its reception in scientific circles was non-existent, and the nature of its reviews was hostile. It was not until the second edition was published in 1870 that his evolutionary principles were accepted. Clearly, *Principles of Psychology* made no significant impact on psychology at its time of publication, but did so fifteen years later. Spencer also appeared as one of the top fifty-three figures in an article by Annin et al. (1968) listing important psychologists from 1600 to 1967. *Principles of Psychology* was therefore selected as the *insight* source for the period 1851-1867.

Works which were considered representative of *conventional wisdom* in psychology were those written by people holding both prominent and respected positions in their discipline, and whose works were cited as standard texts at leading British universities during the time period. Elitist profiles ("elitist" in the Lasswellian sense of referring to an elite group of prominent and important figures) were constructed for

English psychologists who were a focus of attention in Boring (1950), Hearnshaw (1964), and Wolman (1968), and were functioning as elite figures during the period 1894-1914. Boring specified Stout and Ward as the leading psychologists in Britain during this period, and well beyond it. Stout in particular he saw as the effective communicator, of whose Manual of Psychology (1899) he stated that "without a successful competitor it determined for many years the pattern of British systematic psychology" (Boring, 1950, p. 464). Stout's Manual of Psychology was selected as the conventional wisdom source.

Content units. Each of the two selected sources was divided into six sections, and the first appearance of environment in each section was isolated. In accord with Laffal's most recent definition of a target word context (1979), two IBM cardpunched lines of text immediately preceding the line containing the target word, and two immediately following it, were recorded as the context within which each sample of the target word was embedded. With an average of 12 words per line, the body of text analyzed for each source was 30 lines, or 360 words.

The data were analyzed by testing for differences in proportion, using z scores, for the potency figures acquired by each of the eleven nominated categories in relation to the coded texts of Spencer and Stout. A Pearson's product-moment correlation coefficient was calculated when the two general profiles were compared.

Results and discussion

A Pearson's r of .569 (significant at the .01 level, n=118) was obtained when the two general profiles were correlated. This confirmed the prediction that each of the 118 categories would have approximately the same relative position in the distribution of potency figures for each author. The results obtained in the earlier study (Hyman & Shephard, 1980), where the 1973 version of Laffal's instrument had been used, were thus verified. Both Spencer and Stout appear to have shared a similar conceptual understanding of the word environment.

Of the eleven z scores calculated in order to investigate for the possibility of differences in proportion in the potency of the nominated categories, eight z scores were not significant. These results were not entirely inconsistent with the expectation that for both Spencer and Stout these eleven conceptual categories would have similar potency in association with their use of the word environment (see Table 2).

Of the three categories which were significantly more potent in one author's text when compared to the other's (FORW, EVNT and EVER, see Table 2), an investigation of the raw data revealed the direction of these differences. None of the words in Spencer's text were coded into FORW; the category proportion figure for Stout was 1.47. Stout did not use any words which could be coded into EVER; the category proportion figure for Spencer was 2.21. (The phrase category proportion figure refers to the figure representing the percentage of words coded into one category in a text. This total invariably differs somewhat between

Table 2, Appendix 1

Differences in Proportion for 11 Categories in

Textual Analysis for Environment in Spencer & Stout

Categories	z scores
LARG	.45
VARY	0
FORW	2.02*
GO	.51
SIML	.46
EVNT	2.19*
EVER	2.26*
WORK	1.01
JOIN	1.02
IN	1.35
OUT	1.01

Table 2. z scores denoted * indicate significance at the .05 level.

authors, since not every word or phrase used is coded; for example, the phrase "in this case" is not coded. Essentially, differences in the total number of words coded in a text relate to the frequency of any one author's use of non-content phrases. There is some evidence to indicate that writing styles in use prior to 1900 or so were more discursive, particularly when one compares disciplinary texts written before the establishment of a formal discipline with those that were written after.)

Both Spencer and Stout used words which were coded into the category EVNT; the proportion figures were 6.61 for Spencer, and 2.21 for Stout. This category was the most potent for Spencer in the entire distribution; in other words, when using the word environment, Spencer associated it closely with references to aspects of behaviour. On the other hand, Stout surrounded his references to environment with words coded into the category HAVE, which relates to ideas of owning, belonging, maintaining and keeping (Laffal, 1973, p. 24). It is conceivable that the significant proportion difference between Spencer and Stout in relation to the category EVNT may be a reflection of Spencer's interest in, and extensive consideration of, sociology and social behaviour. There appears to be less indication in Stout's texts that he shared this interest to the same extent.

Conclusion

Despite the fifty years difference between dates of publication of

the two texts sampled, the results suggested that, by and large, both Stout and Spencer shared a similar conceptual understanding of the word environment. By using this instrument, it was possible to confirm empirically the theoretical argument advanced earlier, that insightful ideas proposed by a psychologist in one period would be consonant with those present in mainstream psychology in a succeeding historical epoch. In addition, specific characterists of the linguistic context surrounding the use of a target word were isolated, and compared for their degrees of relative potency in the texts of two authors sampled. Of the eleven categories (i.e., semantic field characteristics) which were expected to figure with proportionally similar potency in each author's texts, the results obtained were not inconsistent with this expectation in relation to eight of these categories.

Tracing the development, movement, stability, and modification of ideas involves dealing with a particularly intangible area of investigation. Yet, this is one which is both crucial to acquiring an understanding of psychology's past and only rarely considered to be accessible to empirical procedures. Evidence was obtained in this study to suggest that Laffal's analysis of contextual associates can be found useful in historical research.

Appendix 2

Differences and Similarities Between Category Profiles in One Period: "Emotion" in England 1894-1914

This paper reports the second of three studies designed to explore the use of Laffal's Contextual Associates Analysis in historical research. As with the first study reported (see Appendix 1), this one also expanded on one of the questions explored empirically in an earlier study (Hyman & Shephard, 1980). It had been hypothesized that the conceptual frequency (i.e., category potency) profiles associated with the target word environment relative to a source representative of insight psychology, and to one representing conventional wisdom psychology during the same time period, would be different. The hypothesis was supported by the data obtained, and it was concluded that the evidence permitted statements which were consonant with those based on the traditional methodologies as the historical analysis of social historians, and historians of psychology. The present study replicated the comparison of insight with conventional wisdom during one time period, with some changes in the design details.

As was reported in Appendix 1, the 1980 study dealt only with general frequency profiles. Again, the focus of the present study was on specific category profiles to allow features of the semantic field surrounding a target word to be explored. In a similar study dealing with predicted category potency differences within one time period (Hartsough & Laffal, 1970), one broadly defined group (scientists) was

broken down into two sub-groups: verbal imagists and visual imagists. Predictions were advanced in relation to groups of verbal-imagery as opposed to visual-imagery categories in the writings of representative members of these groups, and Laffal's instrument was found to be successful in discriminating between them.

A second change in the research design of the present study was made in order to control for the possibility of differences between two sources occurring because of individual differences alone. When he has been comparing specific category potencies, Laffal has consistently included this feature (Laffal, 1960, 1961, 1979).

"Emotion" in early English psychology

During the latter half of the nineteenth century, the discipline of psychology in England was less aggressively concerned than that in America with divorcing itself from the confines of philosophy. This is perhaps illustrated by the fact that the primary psychological texts read by English students until the 1890's were two books written by Bain in the 1850's: The Senses and the Intellect (1855), and The Emotions and the Will (1859). Bain was deeply interested in a scientific study of mental life, and this was expressed primarily by his extensive neurological examinations of the sense receptors. He was not an experimentalist, and his consideration of such "mentalistic" phenomena as the emotions, while influenced in later editions by Darwin (Klein, 1970, p. 793), still held to its strongly associationist perspective. Bain's

discussions of emotion were descriptive rather than analytic, and attempts to deal with the causes and consequences of feeling states were couched either in organic or philosophical terms; by and large they were not behavioural. Bain's position as a conventional wisdom psychologist in England was taken over by Stout at the turn of the century. Together with another eminent figure, Ward, he occupied the mainstream of early twentieth century psychology in Britain. Both men rebelled against associationism, and both reformulated important ideas in psychology--particularly in relation to cognition, development, and consciousness. Both, while systematizers, were essentially philosophical in their approach, and neither was particularly interested in new experimental developments (Hearnshaw, 1964, pp. 139-143).

Of primary interest in this study was the evidence that neither figure advanced a dynamic psychology with a considered focus on motivation. Titchener (1929, p. 244) called Stout an act psychologist, and Boring (1950, p. 693) said the same of Ward, but the philosophical direction of both, and their tepid interest in a dynamic, motivational psychology led to little modification of the organic, physiological understanding of the emotions promoted by Bain.

Method

The two figures chosen as representative of conventional wisdom in English psychology during the period 1894-1914 were selected on the basis of elitist profiles developed in the 1980 study. A significant

difference between a conventional wisdom source and an insight source written and published during one period had been established in this earlier study, and the logic of the methodology did not demand a control conventional wisdom source to eliminate the possibility of the difference being attributable to individual, rather than group, differences. It was felt that the present study needed to control for this possibility, and consequently both Stout and Ward were chosen as conventional wisdom representatives. Stout's Manual of Psychology (1899), and Ward's article Psychology in the 1911 edition of the Encyclopaedia Brittannica were selected as sources.

In a test of opposing interpretations of features of Schreber's Autobiography, Laffal (1960) found that his instrument was able to isolate both individual differences and similarities in the interpretive texts. The results suggested that where sources appear to be philosophically and/or interpretively different, evidence of such differences is likely to occur in some of the category potency figures. Similarly, where these differences would not be expected, results of no difference can be predicted.

No one would be likely to challenge Boring's statement that "the principal source of dynamic psychology is, of course, Freud" (Boring, 1950, p. 693). There were others who preceded him--notably Spinoza, Leibnitz, Herbart and Brentano. Their works, however, did not lead to the development of a strong area of motivational psychology during the early twentieth century. From a background in psychopathology rather

than psychology, Freud, by dealing with psychological conflicts, drew the topic of motivation into the mainstream of psychology, and thus enlarged the conceptual understanding of emotion. A figure who Hearnshaw (1964, p. 164) felt "sowed the first psychoanalytic seeds in this country [England] in the 1890's" was Havelock Ellis. His part was, of course, overshadowed by that of Freud, but Hearnshaw saw the two men as complementary figures--not in the sense of equal stature, but in similarity of perspective. If Ellis did, as Hearnshaw suggested, presage Freud, then evidence of his inclusion of motivation as a significant aspect of emotion could become evident in an investigation of the use of this word in any of Ellis' books (which had great difficulty being published in England). Ellis was selected as an insight figure for this study, and the source used was the 4th edition of Man and Woman (1904). It was predicted that some of the category potency figures associated with the use of the word emotion in this source would be different from those obtained from both Stout and Ward.

Since the Ward source was an article rather than a text, the sampling procedure attempted to account for the possible effect of volume differences by isolating relevant chapters in the two textual sources used. Six context samples were taken from Chapter XIII of Ellis, and six from Book III, Ch. IV of Stout. Both these chapters dealt primarily with emotion, and the six samples taken from Ward's article occurred largely in the section where he, too, was examining the topic of emotions. Laffal's (1979) most recent definition of a target

word context was adopted. Three 30-line, 360-word texts were analyzed.

It was predicted that there would be

a) no significant differences in the potency of the categories SOMA LIVE (organic), SOMA IDEA (physiological), and MOTV (motivation) in the category profiles obtained from Ward and Stout;

b) significant differences in the category potency figures for these same categories between Ellis and Stout, and between Ellis and Ward.

Laffal's definitions for these categories (1973) are presented below:

LIVE References to living and abiding. Included are such ideas as: existence and life; residing and inhabiting; organism and animate (p. 25)

MOTV References to motivational and feeling states, predilections and responsibilities. The underlying ideas relate to: wishing and wanting; preferring and choosing; risks and attempts; obligations and commitments; basic motivations such as hunger and thirst; concern and attention; attitudes and sensitivities (p. 26)

SOMA References to bodily and vegetative processes and internal body parts. Included are such ideas as: odor and smell; ingesting and excreting; anatomical and physiological (p. 29)

IDEA References to rational processes, thinking and knowledge.

> Included are: understanding and intelligence; memory and awareness; analyzing and interpreting; symbolic representations such as maps and diagrams; doctrines and ideas (p. 25).

These four categories were selected because it was felt that they could capture the concepts associated with a physiological, organic perspective on considerations of emotion on the one hand (SOMA LIVE, SOME IDEA), and the inclusion of motivation and feeling states on the other (MOTV). Historical arguments advanced in a number of sources suggested that such differences between Stout and Ward (organic) and Ellis (dynamic) should be detectable in an analysis of their texts dealing with emotion.

The data were analyzed by testing for differences in proportion, using z scores, for the potency figures acquired by each of the four categories in relation to the coded texts of Bain, Ward, and Ellis.

Results and discussion

Of the twelve z scores calculated, eight were significant at or beyond the .05 level. Five of these eight related to predicted differences between Ellis and Stout, or Ellis and Ward. The remaining three pointed to differences between the two conventional wisdom psychologists Stout and Ward, which were not consistent with expectations based on historical arguments (see Table 1 for results).

Table 1, Appendix 2

z Scores for Differences in Proportion for MOTV, IDEA, SOMA & LIVE in Stout, Ellis & Ward Comparisons

	Stout	Ellis	Category
Ellis	1.25		MOTV
Ward	2.85*	1.64	
Ellis	4.12*		IDEA
Ward	.54	2.73*	
Ellis	2.91*		SOMA
Ward	4.69*	2.04*	
Ellis	3.54*		LIVE
Ward	2.73*	1.42	

Table 1. *z* scores significant at or beyond .05 level marked *.

It was predicted that significant differences in relation to the potency of the four categories would be found in all comparisons of Stout and Ward with Ellis (total of 8). Five of these eight predictions were met by the results. Of the four comparisons of category potency conducted between Stout and Ward, three were found to indicate significant differences in proportion, results which did not support the predictions advanced. Examination of the category proportional frequencies (i.e., potency figures) for each author provided some indication of the direction of the differences between them (see Table 2); the frequency scores were transformed into percentages in order to take account of the slight differences between each author of the number of coded words found in each text. Non-content words (e.g., "in the case of") are not coded by the instrument.

The category MOTV was significantly more potent in Ward's text than in Stout's (z=2.85); there was no significant difference when Ellis' use of words coded into MOTV was compared with Stout's or with Ward's. The historical arguments advanced by Hearnshaw (1964) in particular do not appear to be supported by these results. The category IDEA was significantly less potent in the analysis of Ellis' text than in either Ward's or Stout's, and the lack of significant difference in proportion for this category when Ward was compared with Stout was not inconsistent with the arguments. Yet for the other three Ward/Stout comparisons, significant z scores, indicating differences in proportion, were obtained. From Stout to Ellis to Ward there appeared to be a steady decline in the

Table 2, Appendix 2

Percentage Scores for Categories MOTV, IDEA, SOMA & LIVE for Stout, Ellis & Ward

Categories	Stout	Ellis	Ward
MOTV	3.23	5.57	9.09
IDEA	11.80	2.90	10.50
SOMA	10.79	4.46	1.45
LIVE	4.30	0	0.72

Table 2. Numbers of coded words (n) per author were: Stout, 278; Ellis, 269; Ward, 275.

use of words and phrases coded into the category SOMA (see Table 2), and of the three authors, only Stout appeared to make use of ideas which could be coded into LIVE (see Table 2). The results provided some evidence to indicate that Ellis and Ward shared more of the same understanding of emotion than did Stout and Ward.

In order to explore this evidence more closely, two sets of cross-correlations, using Pearson's Product-Moment correlation co-efficient, were conducted. The first set (see Table 3) compared the relative potency distribution of all 118 categories. The logic of the research design led to the expectation that the overall potency distribution of the 118 categories would be significantly correlated between Ward and Stout, and not significantly correlated between Stout and Ellis, or between Ellis and Ward. It was expected, then, that the general conceptual understanding of emotion would be similar in the texts written by Stout and Ward, but that Ellis' use of the word emotion would not, by and large, be similar to either Stout's or Ward's.

Table 3

	Stout	Ellis	Ward
Stout			
Ellis	.59		
Ward	.70	.65	

The results of this first set of cross-correlations indicated that the overall conceptual understanding of emotion as demonstrated by Ward,

Stout and Ellis was similar. Each of the three correlations were significant beyond the .01 level (n=118).

The second set of cross-correlations focussed on the thirteen most potent categories for the three authors; that is, the categories most frequently and therefore most closely associated with the use of emotion by all three. Once again, the logic of the research design suggested that the relative potency of these categories would be significantly correlated only between Stout and Ward (see Table 4).

Table 4

	Stout	Ellis	Ward
Stout			
Ellis	.19		
Ward	.26	.61	

Table 4. With n=13, r=.61 significant at .05 level. The thirteen categories were SOMA, LIVE, IDEA, MOTV, EVNT, GO, MUCH, OPPO, PANG, SHRP, SIML, UP, VIEW.

The results obtained in this set of cross-correlations were not consistent with the expectations. The only significant r was that derived from the correlation of strong potency categories between Ward and Ellis. These results are, however, not inconsistent with the suggestion obtained from the z scores that Ellis and Ward may have shared a more

similar understanding of emotion than Ward and Stout. They do not support the historical arguments found in Hearnshaw (1964), Boring (1950) and Klein (1970). It was felt that this study may have provided evidence that casts some doubt on the credibility of these arguments.

In a comparative investigation of historical arguments about some aspects of Wundt (Blumenthal, 1975), conflicting ideas and interpretations in a number of historical texts were isolated. Blumenthal's study was theoretical rather than empirical, but he was able to illustrate convincingly that no more than other historians can historians of psychology lay claim to providing their readers with a "true" picture. In a critical review of Watson's The Great Psychologists, Young (1966) stated that

> I do not believe that such a book [i.e., a standard general treatment] can be successful in the present state of our knowledge. . . . It will be some time before the research has been conducted which will make it feasible to attempt a survey of the history of psychology, unless one is prepared to continue indefinitely to accept works based on a wholly inadequate corpus of scholarship.

Probably Young's most damning comment in relation to general history of psychology texts (up to 1966) can be found in his casual labelling of these books as "secondary sources."

The point is that whether or not such stringent criticism is justified, there do appear to be an enormous number of subjects within the history of psychology which would repay intensive study, and very few which have been conducted. The results of this study have suggested that attempts to establish the usefulness of an instrument in historical

research can be handicapped by weaknesses in the "theory" (i.e., historical arguments) upon which predictions relating to the use of the instrument are based.

One problem encountered when this study was being designed was the selection of an insight psychologist in England early in the twentieth century. In the earlier study which included a comparison between an insight and a conventional wisdom psychologist (Hyman & Shephard, 1980), the selection of the insight figure (Spencer) was less ambiguous. An insight figure in British psychology at the turn of the century, however, is not clearly evident; Freud, for example, was German, and MacDougall is difficult to classify in terms of nationality. It is possible that some aspects of the research design were poorly conceived, yet they were not inconsistent with arguments found in history of psychology texts.

These historical arguments were found in texts by Boring (1950), and Hearnshaw (1964). Those advanced by Reisman (1966), however, challenged the conclusions drawn by Boring and Hearnshaw. If the design of this study had been based on Reisman's arguments, would the results obtained have offered them empirical support, and thus laid more serious challenge to those of Boring and Hearnshaw? If one accepts Young's biting "secondary sources" indictment of history of psychology texts, the question becomes more purposeful and sharply focussed. It does point to an area in the discipline which highlights a debatable weakness in currently used histories of psychology.

Appendix 3

Conceptual Stability Surrounded By Change: "Ether" in America 1885-1914

A third study was conducted in an attempt to explore another question to which historical research gives rise. Where a concept is the focus of extensive debate within one discipline for a considerable period of time, can some of the broad parameters of that debate be isolated by Laffal's instrument, even when new information is speculatively advanced to enrich the ongoing discussions?

Such a situation can be found in physics from about 1885 to 1930 (Swenson, 1970) in relation to the concept ether (or aether). The subject was given a certain national character and coherence in America when an important study was published in the American Journal of Science, around which a good deal of the debate was centered (Michelson & Morley, 1887). Two questions about ether were advanced late in the nineteenth century, before which time ether was generally assumed to exist as a subtle (i.e., visually undetectable) material substance occupying apparently empty space. Many physicists remained convinced of its existence, even after the famous Michelson and Morley experiment which obtained negative results, pointing to the non-existence of ether. In attempts to rebutt the 1887 experiment, attempts which were entered into with enthusiasm by Michelson and Morley, the experimental focus was directed more towards efforts to establish the nature of this medium. So the two basic questions were (a) does the ether exist? and (b) of

what is it composed? Numerous answers to the second question were advanced (see Schaffner, 1972; Tonnelat, 1965), and Einstein's special theory of relativity, published in 1905, contributed significant new ideas to the debate. However, those who refused to accept the theoretical advances which argued against the existence of ether as a material substance continued to conduct research into the nature of its composition well beyond 1920 (Swenson, 1970). In his impressive paper considering the ether debate, Swenson argued convincingly that the traditional assumption that the advent of relativity replaced the theory of ether in physics is an example of received history. Further, the pedagogically convenient belief that the Michelson and Morley experiment of 1887 was the crucial test for the existence of ether is naive history, he maintained, since only when it was replicated in experiments during the 1920's did the experiment become regarded as crucial (Swenson, 1970, p. 69).

If Swenson's argument is supported by evidence found in documents, it should be possible to advance predictions about some of the characteristics of the semantic fields within which the word ether was embedded in those documents. Whether the ether was luminiferous, electromagnetic, or dielectric, as Swenson suggested it became from 1885 to 1905, it was nevertheless seen by some physicists as a material substance (MTRL). The relativity theorists who rejected the existence of ether spoke of a vacuum (VAPR LACK) occupying empty space. It was proposed that the application of Laffal's instrument to a sample of relevant documents

could provide some evidence in support of Swenson's argument.

Method

Two time periods -- 1885-1889 and 1910-1914 -- were selected for this study, and works by American physicists were used as the relevant sources. Six papers dealing with either an experimental or a theoretical consideration of the ether debate were isolated for each time period, and the following predictions were advanced:

1. words which will be coded into the categories MTRL and VAPR LACK will be associated with the appearance of the target word ether in the sources for both periods;
2. no difference in the relative potency of these categories will be found when the texts of the two periods are compared.

The definitions supplied by Laffal (1973) for these categories are:

LACK Words referring to lack or absence. Included are such ideas as: void and emptiness; loss and want; expenditure and waste (p. 25)

MTRL References to basic materials other than fabrics, and to substances out of which other products are made. Such materials as woods, metals, chemicals, oils and plastics are included in MTRL (p. 26)

VAPR References to vapors, gases and mist. Included are such ideas as: cloud and fog; aroma and odor; breath and respiration (p. 29).

Six examples of the use of the target word ether were isolated for each time period. Once again Laffal's most recent definition of a target word (1979) was adopted. Thus, for each time period, the body of text analyzed was 30 lines, or 360 words. The data were analyzed by testing for differences in proportion of the potency of the three categories when comparing those appearing in the 1885-1889 texts with those in the texts published between 1910 and 1914.

The prediction of no difference advanced introduces again the problematic nature of a no difference argument. When statistics based on a probability model are employed, it is legitimate to make an absolute statement when results are obtained which permit rejection of the null hypothesis within specified limits of probability. It is not possible, however, to advance a similarly definitive statement in support of the null hypothesis when results obtained fail to provide grounds for its rejection. While such a statement can be made, the probability model does not permit the assumption that failure to reject the null hypothesis is the equivalent to "proof" of the existence of no difference. This appears to be one of the problems inherent to historical research, where no difference predictions can be expected, and at times be logically imperative, when attempts are being made to support a historical argument which suggests grounds for making such a prediction.

This, of course, was the case in the present study. It was felt that, if the results obtained did not permit the rejection of the null hypothesis of no difference, it could be suggested that such results

were not inconsistent with the historical arguments advanced.

Results and discussion

The three categories, LACK, MTRL and VAPR, were utilized in the coding procedure of the words in the texts analyzed, thus indicating that the use of the word ether by the physicists sampled was associated with these conceptual categories. None of the three z scores calculated for differences in proportion was significant (see Table 1).

Table 1: Appendix 3

Potency Figures and z scores obtained for 3 Categories Associated With ether By American Physicists

	Potency Figures		
Categories	1885-1890	1910-1914	z scores
LACK	2	1	.58
MTRL	5	4	.34
VAPR	8	11	.70

The results obtained were not inconsistent with the arguments advanced by Swenson (1970). While the use of ether by the writers sampled was associated with each of the three categories selected, the analysis of the data for this study did not permit rejection of the null hypothesis.

The methodological importance of this study appeared to be twofold.

First, it demonstrated the usefulness of Laffal's instrument when stability over time is an issue of concern. The two periods selected were separated by twenty years, yet the results did not permit any statement to be made about differences in relation to the potency of the three categories under examination. Ironically, the debate around the existence or non-existence of ether was based, in the first analysis, on the Michelson Morley experiment which failed to detect the differences predicted. Neither they, nor many other physicists, were willing to accept the failure to reject the null hypothesis that their results demanded.

The second methodological issue of interest in this study related to the problem of no difference predictions in historical research. It was felt that, where results obtained in a study following one procedure could be shown to be not inconsistent with arguments developed on the basis of another, such results would be acceptable. In particular, the usefulness of the instrument used to obtain the data—i.e., Laffal's—would be reinforced.

APPENDIX 4

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1894 - 1897

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