tion, wealth and originality of ideas, ideational types, etc.

(8) The combining and the elaborating (verarbeitenden) forms of thought ("combination," practical ingenuity, productive imagination, critical judgment, abstraction, etc.)

(9) Language mastery.

(10) Relation of the emotions and the will to intellectual traits (suggestibility, educability, interests, ethical and aesthetic sentiment, sense of veracity, justice, exactness, etc.)

(11) The functional relationships existing among individual traits which together constitute endowment.
ALYSIS OF DR. KUHLMANN'S ATTACK ON "THE MENTAL HEALTH OF THE SCHOOL CHILD"¹

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Kuhlmann's violent strictures on "The Mental Health of the Child" are so obviously inspired by personal animus so defamatory in character that I should give them no force, were it not for the fact that many of the statements are misleading and irrelevant, but utterly and inexcusably irresponsible. The perversion of the facts in some cases where the statements do not permit of misconstruction either that the reviewer is woefully careless of his facts or willing deliberately to distort facts in order to misrepresent the writer. I cannot allow material distortions to go unchallenged. The reviewer devotes a larger part of his review to an analysis which is literally "shot full" of blunders and animadversions and vilifications.² I consider his severest strictures and most glaring misapprehensions.

The reviewer charges that the book is amateurish because literary references are supplied in some chapters but not others, and he fails to see the reason for this discrimination. He particularly dwells on the lack of references in Chapter VIII on "The Present Status of the Binet-Simon Graded Intelligence," and complains that I fail to give any references except to "a study previously published and reprinted in this book by the author." The reasons for the lack of uniformity should be obvious to any fair-minded reader. First, bibliographies are, almost without exception, omitted from Psycho-Asthenics, September, 1914.

Dowing suggests the diatribe rather than the impartial scientific treatment that this "study" is reprinted in the book is false. Only the Experimental Studies (which is the citation in the text) is reprinted in The Mental Health.
ception, reprints of public addresses, and it is not customary to encumber public addresses with lengthy bibliographies; hence in these chapters I was following the best precedent in omitting long lists of references.

Second, Chapter VIII was prepared for the 1911 meeting of the American Psychological Association frankly as a summary of some of the conclusions arrived at in a larger work which I had not at that time brought into print. This explains why reference is made to the work in question, "more than half of the nine pages of this chapter is devoted to the "author's own" publication. I fail to see why it is eminently or why an author should be censured for summarizing some of his own conclusions from a larger experimental work and present them in public, even if it requires "more than half of nine pages" to do so. To deny such a prerogative is short of idiotic.

Third, the "several hundred publications that should have been consulted for this chapter" were not consulted for the excellent reason that "several hundred publications" dealing with the Binet scale were not in existence at the time the chapter was prepared (October, 1911). The date when the address was delivered is explicitly stated in the text. Evidently the reviewer does not regard dates as of any moment. An examination of the chapter, however, reveals the fact that reference was made to the parallel findings of a few investigators whose publications were available at the time of the writing. The source of the reviewer's animus is evidently the peevishness he feels at not finding his own discussions of the Binet scale —but his discussions were not in circulation when the chapter was written. Let it be said, however, that the appearance of later contributions in no wise alters the strictly experimental findings of my work in 1910 and 1911, although they may modify the inferences to be drawn from some of them.

2. The reviewer alleges that I have judged "the tests" from "examining 333 epileptics." This statement is without foundation. Available confirmatory results are given in Chapter VIII, where reference is likewise made to
Moreover, I have been engaged continuously since 1910 in mental examination of cases by means of the Binet and mental tests, only a very few of which are epileptics. I am precisely the same methods of giving the Binet tests cases as to the epileptics. Eventually I shall hope to material in print, and we shall then know definitely to what extent it is due to a variation in the difficulty of the study, is due to the peculiar organization of the epileptic cases for the purpose of practical diagnosis of a great variety of cases in university and public school titles the user to the right to express a professional and gives him an insight into the value of the tests for of diagnosis which it is impossible to get simply by analyzing the experimental results of the testing others—usually grade teachers. The reviewer evidently that the opinion concerning the value of diagnostic by a physician constantly engaged in diagnosing cases blue than conclusions drawn by a research worker from ed out by nurses. If that is his opinion I shall permit it without vilifying him for so doing.

The author could not qualify as an expert according to definition." I am not aware that I have ever posed as a clinical skill. On the contrary, it was the realization importance of the practical consequences of mistaken men- and my own limitations in attempting to practi- cases for a public school system in 1909 that led me of the training needed by a psycho-clinical examiner, for the schools, and which caused me to pursue dur- of years a course of training embracing: First, study ins for feeble-minded, epileptic and insane cases; sec- special classes and psychological clinics in various institutions in a considerable number of cities in the and eastern parts of the country (it was while on station trips I became disgusted with the work of the
amateurs); third, special work in neurological, psychiatric, speech clinics in a number of the larger medical centers; the study of various medical specialties in medical school; consultation work in practical mental diagnosis in university school clinics, and supervision of public school cases; an study of the medical, psychological, educational, sociological genetics literature bearing on my problems. The reviewer's actual clinical (sic) experience is, I believe, limited to lab psychological research work in an institution for the minded and epileptic. It is from the realization of my own limitation that I have come to believe that I know the ments in the field of practical psycho-clinical diagnosis have been speaking only concerning diagnosis and not ing the mere administration of tests, as the reviewer mistakensly assumes. My vision is toward the future, past, nor even the present. I am interested in constructards of preparation for the most expert type of psycho examiner of the future, and not for the type of mediocris now too prevalent in most of the public schools and courts. That no one can today qualify on the standard is beside the mark. Could the physician trained one years ago, or even fifty or twenty-five years ago, qui skilled practitioner today?

4. I did not insist, as the reviewer alleges, "that a and technical training of the psychologist are necessa a reliable Binet-Simon tester," qua Binet tester, but th that such training is essential if he would also qualifi oughly trained for the difficult work of mental diagno

5. The reviewer implies that my schema for cli is worthless, because similar schemas "have never grea value in practical work." Unfortunately he give to prove the validity of his conclusion. It only reg own private opinion. Over against this dogmatic ve have the well-nigh universal practice by the ab examiners in the leading hospitals and medical sch country who are using analogous schemes of inves have not only used such schemes in conjunction with
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...a number of such institutions, but have for years made use of the general scheme of investigation which our reviewer singles out for condemnation. Such experience is not without value. It will be interesting to know what the reviewer conception is of a clinical examination in clinical psychology and medicine. I am not aware that he has made any contribution in this field which makes his private judgment of more than the settled practice of the institutions whose specialty is, namely, the medical schools.

“The epileptic has a special type of mind which causes exceedingly irregular results in Binet-Simon testing. Apparently the author has not discovered this fact, or regards it as irrelevant.” These statements may be taken as typical of the reviewer’s reckless regard for accuracy of statement and his desire deliberately to leave erroneous impressions in the reader. He permits the implication to be made that he is the discoverer of the fact that there are abnormal irregularities in the epileptic’s mind. Unless I mistake, my early publication was the first experimental study which showed this fact (See Experimental Studies of Mental Defectives, p. 53, 106ff). Need I remind any one who has carefully read the book that attention was explicitly directed to the irregularity of the mental development of the epileptic in the chapter which our reviewer, at no time over-cautious of the accuracy of his statements, criticises: “we are able to frame a picture of an interesting spectacle: a case of mental wreckage, the integrity of various mental functions has been impaired in various levels of mental development, and whereby lower psychic levels have been swept away while the higher levels remain intact. The mentality of epileptics makes the constellation that is extremely irregular” (p. 193; also p. 190). Our reviewer calls to mind Karl Pearson’s lament: “It is a singular phase of modern science that it steals with plagiaristic right hand while it stabs with a critical left.”

I did not regard this fact as “irrelevant” is demonstrably the fact, first, that I sought to corroborate my findings by comparison with the results then available based on...
testing other types of children (particularly normals),
and, by the fact that I have deliberately refrained from
the scale on the basis of the testing of mentally abnor-

7. How uncrupulous the reviewer may become in
with facts may be illustrated, again, from the follow-
ments: "He concludes that the typical epileptic catego-
of the condition of moronity * * * * while the typical
minded station is that of imbecility." "That the epi-
to an institution might be selected cases in any seri-
he does not think likely."

What are the facts in the indictment? The
drawn by the writer as to the comparative intelligence
ed the groups of epileptics and feeble
who were actually studied. The reviewer has taken t
of generalizing the statement and applying it to the who
of institutional and non-institutional epileptics and
minded.4 The writer assumes no responsibility for the
unwarranted inferential leap. Moreover, had he read
with not only more regard for its spirit but for statem
t of a strictly unequivocal character he would have avo
the second statement quoted above, which is positive.
On page 189 of the text we read: "The institu-
Skillman may not be representative. Our curve in
valid on the assumption that the epileptics tested are
should not like to think that any worker in this f
orant that he does not know that the same state
institutional cases of the feeble-minded). Accord
theory of the probability surface we are justified in
them as typical if the selection represents a chance
But it is possible that two selective processes have o
way to distort both extremes of the curve, etc. * * *
not be able definitely to settle this point until oth
have undertaken similar studies on a large scale." Per

4 He has apparently taken a similar unwarranted liberty in
experimental findings in the dental experiment, for which I her
claim all responsibility.
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ents as the above justify my critic in accusing me of "dog-
maticism," and of overlooking the selective influences which de-
termined the distribution of cases in institutions! The justifica-
tion of the reviewer's tawdry aspersion ("Alas for the profession
is should come from the 'expert clinical psychologist' ")
safely left with the fair-minded reader.

My critic takes me to task for my criticism of certain re-
views of the Binet scale: "Superficial work like this is mislead-
tends to arouse contempt for the slipshod standards of
work obtaining in this field of scientific psychology."
are the fact in support of this indictment? I shall here
only those charges which, by implication, my critic ap-
pies to his revision of the Binet scale. I did him the courtesy
in the original presentment not to single him out for special
m, but he has thrown down the gauntlet (in a peculiarly
manner) and I am forced to meet the issue. First, I
intended that the revision or establishment of a scale of
nce for normal children must be based on the testing of
children. Therefore, I have refrained from revising the
basis of my own results with abnormal cases. Kuhl-
produced a revision for normal children which is based
testing of feeble-minded children, at least so far as con-
ns own distinctive experimental contribution to the revis-
the negligible exception of "forty normal adults" who
ven only two higher age tests, only one of which is a
test. The reviewer contends that had I said "a few
ce mine) of the changes made were based on the per-
feeble-minded the statement would have been cor-
In his "Revision," however, he emphasizes that "the
revision * * * is largely along other lines" (blackface
than the revisions of others. If so, what is the scientific
his changes largely along other lines if not his own ex-
tal work on the feeble-minded, for it is not apparent that
ime he had done any Binet work on normal children
normal adults" excepted in the case of only two tests).
inated 11 tests from the 1908 scale, added 9 new ones
ed six, but he neglected to state specifically that only
a "few" (sic) of these changes were based on his work on the feeble-minded, nor did he so state in respects tailo
d direc
tions for giving the tests which he supplies, so far as he has altered the procedure of others, mere
do not so state in respect to the detailed directions for giving the tests which he supplies, and only to a slight extent on his own work at the time entirely confined to the feeble-minded, so far as the Binet scale is concerned.

Second, I have charged that in some "instances age-norms have been * * * supplied although not a single child tested in those ages." This statement applies absolutely to the following ages in my critic's scale: "age three months," "age six months," "age one year," and test 1 in age 2. The reviewer's explicit admission, "were devised on the basis of these observations (observations by writers on children a careful searching through the literature on them, supplemented by a few chance observations of by own normal infants * * * *." "The norms for them are necessarily a small number of cases in a number of instances." find a confession that norms have been embodied in a scale on the basis of definite experimental tests, but on the basis of recorded observations in literature and the author's chance (sic) observations, and this is the type of science critic attempts to defend, although he attempts to impugn my scientific competency because I have stated certain experimental findings which were based, not on a "few chance observations" but on carefully controlled experiments on 27 children receiving mouth hygienic treatment. It may be left to the reader to determine who is guilty of "dogmatism." Can it be that my critic has developed such a degree of hypermetropic motes in my work that he has become profoundly myopic in his own work? Any one knows that the ob
cs in the genetic literature are usually based on the study of a
work is "superficial and "tends to arouse contempt,"

further charged that norms established as the above
(from no definite tests, or only from a few tests) are
used and used by a large number of uncritical Binet
are neither psychologists nor scientists, and thereby
judged or stigmatized on the basis of unproved as-
What are the supporting facts? First, I have seen
is lowest age tests given in baby clinics by "uncrit-
i c a l  B i n e t  testers" who have assumed, and with justice, that
accuracy of the placement of the tests has been demonstrated,
ly have been embodied in a scale of tests having the
proved reliability. Second, my own use of these
baby clinics and elsewhere, has failed to show that they
value for grading the intelligence of young infants that
ify one in placing them in an age scale. Third, one
organizers of the baby clinic who has used the tests
wise with infants tells me that she has discarded the tests
cause they are not workable. My charge of "unproved
s is based on first-hand observations and tests, not
m."

my critic has very much to say about my dental tests.
he demonstrates admirably that he is a past-master
men of straw, and windmills a la Don Quixote, or of
ver perverting facts clearly stated, or of presenting his
ions as universally accepted facts.
one would expect a very large improvement in the
nseveral months following dental treatment." Does
ver make this statement as a fact or as his opinion
alleged fact? Several leading oral hygienists who are
me make precisely this expectation. Possibly these
o ones, in the reviewer's estimation.
do not know whether they (the tests) measure intel-
liency at all, for no norms at all are given." My re-
we do not determine whether a set of tests measure
efficiency" by consulting "norms," but by examining
eter of the tests which are employed. What the tests
measure can only be determined by a critical examination of the tests themselves. That question has absolutely nothing to do with the subject of norms.

"Only five different tests were used," while I claim the Binet-Simon scale the number of tests for each age group should be increased from five to ten in order to make tests reliable." "These tests (my dental tests) were group tests. The author has warned us before that the results of group tests are not reliable." The reviewer demonstrates conclusively either that he has not read the text with ordinary care or that he has no conception of what I mean by clinical examinations and the requirements which I propose for them in contradistinction to mere mental tests. He appears to labor under the delusion that the tests I gave the dental squad were clinical tests, and that I so regarded them, and that they should apply to them the standards which I apply to examinations. But I have nowhere claimed that the dental tests were conducted as clinical tests. On the contrary, I carried them out as group tests, under the usual rigid conditions applying to any kind of group testing in educational and mental psychology.

The reviewer accuses me of maintaining that the results of group tests are not reliable." This statement is an unmerited perversion of the facts in the case. What I did say was "Norms of mental functioning established by experimental or educational psychologists by group tests on squads may have little practical value as clinical tests" (p. 220). This statement was not made dogmatically, as the reviewer fain have the reader believe, appears very clearly from the following statement: "At any rate, some one should make a comparative study to determine whether there is any difference between norms established by group tests and norms for the same tests established clinically" (p. 220).5 "It is quite practicable . . .

5 In contrast with the guarded character of the above statement, following recent pronouncement of our professedly temperate critic furnishing a single shred of supporting fact, but suggesting that actual experience" had "positively and emphatically" proved the development of Intelligence "comes practically to a stop at the age of fifteen. Experimental data now in the hands of the writer will show, "positively and emphatically," that this statement is "dogmatism" ineffable.
The educational psychologist may be forced to give lengthy tests because usually by one sitting he attempts to measure only a limited number of traits. But the psycho-clinicist, in order to get a comprehensive picture of his case, may test a very considerable number of functions” (p. 221). The difference between the reviewer’s conception of a clinical examination (if indeed he has one) and my own is that he thinks it sufficient merely to give a large number of tests—“twenty to thirty,” “with several age-groups,” while my plea is that we must “survey a maximal number of fundamental functions”—not the same but different functions. The more of these we have at a given age-level the better it is to give a large number of tests in various age-levels (wide-range testing), many of which may test precisely the same functions. I do not advocate increasing the number of tests to 10 for each age, “in order to make those tests reliable,” but in order to afford a comprehensive survey of different functions for an accurate clinical picture.

The author has insisted that in order that the results of any test may be reliable the tests must be given by a trained psychologist.” The reviewer again misquotes me. What I said was: “psycho-educational amateurs may be competent to administer formal psychological tests” but not, therefore, deceive ourselves with the thought thereby training competent psycho-educational diagnosticians. My critic is prone to put into my mouth any words that meet his fancy.

By implication he objects to some of these tests being given (through force of circumstances) by proxy. This comes with grace from one who has drawn important deductions from his own Binet revision on the basis of tests made by grade teachers. He avers that the writer “does not tell us anything further about the proxy.” That this accusation is groundless the reviewer will discover if he will consult the unabridged original, to which he was referred in the chapter in which he contrasts clinical study with formal psychological tests.

At times he contrasts clinical study with mental tests or the Binet tests as though clinical tests, mental tests and the Binet tests were mutually exclusive. At other times his discussion seems to indicate that the clinical examination is with history taking.
question, but which, evidently, he has not seen, although he
not hesitate to pass damaging judgment on the whole.
Such are his conceptions of the scientific reviewer's func-
tions.

"The statement as to the time interval between dental treat-
ment and the giving of the several series of mental tests
indefinite." If the reader desires conclusive evidence that
reviewer is utterly incapable of writing an accurate, reli-
partial review, let him consult page 277, where the pre-
of every sitting is given.

"Only twenty-seven pupils were tested, but the au-
us that in order to establish reliable norms for the Bin-
tests not less than a hundred cases for each sex for each age
must be tested." A cursory reading of the book by a
minded judge will show that I did not set myself the
establishing "reliable" sex or age "norms" in the dental
ment. On the contrary, I proposed merely to measure the
pupils' improvement by means of a comparison of their ex-
cessive scores.

The reviewer has not made any discovery, as he
think, when he says that there were other factors than
tal treatment which influenced the results, or when he
control squad should also have been used. The writer
and again has called attention to both of these facts, and
made allowance for them in the conclusions drawn (e. g.,
Mental Health, pp. 280, 288). Had the reviewer been
by motives to play fair with the author he would have
stated thus much. The only other construction is either
has merely skimmed a book which he is attempting to
ally review, or else he has deliberately set himself the task
crediting the credibility and competency of the writer.

My critic alleges sarcastically—and with an un-
consciousness of his own superior knowledge—that "in
an experiment, made under such conditions," the writer
"drawn conclusions, the results of which are of far
importance to the state and the nation." My reply
fold: First, I did not base the conclusions wholly up-
the psychological tests, and I pointed this out so clearly one except those who set out on a voyage of destruction fail to see it (p. 289). Some of the supporting evidence of clinical studies made by duly qualified dentists and physicians. The reviewer evidently does not even know of the existence of such data. Second, I do not know that my critic has made any contributions to the science of oral hygiene which in a special insight into the physical and mental effects of sanitation and thorough mastication. He opposes his opinion, unsupported even by a pretense of scientific investigation of the problem, to the opinions of a considerable body of men and women who have been investigating the problem. Does the reader prefer to follow Kuhlmann’s theories, or the conclusions of those who have investigated the problem at first hand (dentists, physicians, teachers, psychologists, nurses)?

"The Mental Health of the School Child" makes no claim to perfection. It is subject to all the defects appertaining to a publication of scattered addresses. It distinctly disclaimed being a "systematic treatment of one central theme" (see Preface). It has a right to be judged by what it aims to accomplish, not by what it does not pretend to do. Whether it is guilty of the above crimes alleged by my critic can be safely left to the impartial judgment of those who are enabled to read it without preconception and who are not "telescoping" to find fault, or to invent faults not found.

I may close this peculiarly odious task of exposing to public view what purports to be a scientific review by paraphrasing a statement from the Preface, which our reviewer ironically quoted at the conclusion of his review: "Superficial reviewing is misleading and should arouse the righteous contempt to love accuracy and fairness and hate perversion and aspersion."