tion, wealth and originality of ideas, ideational types,

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

(9) Language mastery.

(10) Relation of the emotions and the will to in (suggestibility, educability, interests, ethical and aesth ment, sense of veracity, justice, exactness, etc.)

(11) The functional relationships existing among vidual traits which together constitute endowment.[7]

[7] This article will be continued in the next number of the

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ALYSIS OF DR. KUHLMANN'S ATTACK ON "THE NTAL HEALTH OF THE SCHOOL CHILD"[1]

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lmann's violent strictures on "The Mental Health of the Child" are so obviously inspired by personal animus o defamatory in character that I should give them no fur- ce, were it not for the fact that many of the statements nly misleading and irrelevant, but utterly and inexcus- and irresponsible. The perversion of the facts in some ere the statements do not permit of misconstruction either that the reviewer is woefully careless of his facts e is willing deliberately to distort facts in order to mis- or malign the writer. I cannot allow material distor- misconstructions to go unchallenged. The reviewer de- larger part of his review to an analysis which is liter- full" of blunders and animadversions and vilifications.[2] onsider his severest strictures and most glaring misap- ns.

he reviewer charges that the book is amateurish be- part, literary references are supplied in some chapters others, and he fails to see the reason for this discrim- He particularly dwells on the lack of references in VIII on "The Present Status of the Binet-Simon Graded Intelligence," and complains that I fail to give any refer- is chapter except to "a study previously published and in this book by the author." The reasons for the lack ity should be obvious to any fair-minded reader. First, rs which lack bibliographies are, almost without ex-

[1] of Psycho-Asthenics, September, 1914.

[2] llowing suggests the diatribe rather than the impartial scientific author is lamentably ignorant of the theory and technique of a ike that of Binet-Simon."

[3] tement that this "study" is reprinted in the book is false. Only the Experimental Studies (which is the citation in the text) is re- Mental Health.

ception, reprints of public addresses, and it is not cus
encumber public addresses with lengthy bibliogra
these chapters I was following the best precedent in
long lists of references.

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

Third, the "several hundred publications that sh
been consulted for this chapter" were not consulted fo
excellent reason that "several hundred publications" de
the Binet scale were not in existence at the time the c
prepared (October, 1911). The date when the address
ered is explicitly stated in the text. Evidently the rev
not regard dates as of any moment. An examinat
chapter, however, reveals the fact that reference w
made to the parallel findings of a few investigators
lications were available at the time of the writing.
of the reviewer's animus is evidently the peevishness
feels at not finding his own discussions of the Binet
—but his discussions were not in circulation when
was written. Let it be said, however, that the app
later contributions in no wise alters the strictly ex
findings of my work in 1910 and 1911, although they
the inferences to to be drawn from some of them.

2. The reviewer alleges that I have judged "th
the tests" from "examining 333 epileptics." This
without foundation. Available confirmatory results
Chapter VIII, where reference is likewise made to

udies where the data of still other writers are also tab- Moreover, I have been engaged continuously since 1910 ental examination of cases by means of the Binet and ntal tests, only a very few of which are epileptics. I 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 nt the great variation in the Binet tests found in my dy, is due to the peculiar organization of the epileptic to what extent it is due to a variation in the difficulty ts. Moreover, it is not amiss to say that years of al- use of the scale for the purpose of **practical diagnosis** eat variety of cases in university and public school titles the user to the right to express a professional nd gives him an insight into the value of the tests for of diagnosis which it is impossible to get simply by and analyzing the experimental results of the testing thers—usually grade teachers. The reviewer evidently that the opinion concerning the value of diagnostic by a physician constantly engaged in diagnosing cases alue 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.

he author could not qualify as an expert according to finition." I am not aware that I have ever posed as a clinical skill. On the contrary, it was the realization rtance of the practical consequences of mistaken men- es and my own limitations in attempting to practi- ose 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 ns for feeble-minded, epileptic and insane cases; sec- to special classes and psychological clinics in various stitutions in a considerable number of cities in the and eastern parts of the country (it was while on tion trips I became disgusted with the work of the amateurs); third, special work in neurological, psychia[...] speech clinics in a number of the larger medical centers[...] the study of various medical specialties in medical scho[...] consultation work in practical mental diagnosis in unive[...] school clinics, and supervision of public school cases; a[...] study of the medical, psychological, educational, sociolo[...] genetics literature bearing on my problems. The revie[...] tual clinical (sic) experience is, I believe, limited to la[...] psychological research work in an institution for th[...] minded and epileptic. It is from the realization of [...] limitation that I have come to believe that I know the [...] ments in the field of practical psycho-clinical diagnosi[...] have been speaking only concerning diagnosis and not [...] ing the mere administration of tests, as the reviewer [...] mistakenly assumes. My vision is toward the future[...] past, nor even the present. I am interested in construct[...] ards of preparation for the most expert type of psych[...] examiner of the future, and not for the type of medioc[...] is now too prevalent in most of the public schools an[...] courts. That no one can today qualify on the standa[...] is beside the mark. Could the physician trained on[...] years ago, or even fifty or twenty-five years ago, qu[...] skilled practitioner today?

4. I did not insist, as the reviewer alleges, "that [...] and technical training of the psychologist are necessa[...] a reliable Binet-Simon tester," qua Binet tester, but [...] that such training is essential if he would also quali[...] 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 [...] great value in practical work." Unfortunately he giv[...] to prove the validity of his conclusion. It only rep[...] own private opinion. Over against this dogmatic [...] we have the well-nigh universal practice by the abl[...] 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 [...]

a number of such institutions, but have for years made use of the general scheme of investigation which our singles out for condemnation. Such experience is not value. It will be interesting to know what the reviewception is of a clinical examination in clinical psychology medicine. I am not aware that he has made any contri- this field which makes his private judgment of more an the settled practice of the institutions whose specialty sis, namely, the medical schools.

"The epileptic has a special type of mind which causes gly irregular results in Binet-Simon testing. Appar- author has not discovered this fact, or regards it as t." These statements may be taken as typical of the 's reckless regard for accuracy of statement and his ap- sire deliberately to leave erroneous impressions in the the reader. He permits the implication to be made that discoverer of the fact that there are abnormal irregular- e epileptic's mind. Unless I mistake, my early publica- the epileptic was the first experimental study which is fact (See **Experimental Studies of Mental Defectives,** f, 53, 106ff). Need I remind any one who has care- the book that attention was explicitly directed to larity of the mental development of the epileptic in the ter which our reviewer, at no time over-cautious of the of his statements, criticises: "we are able to frame of an interesting spectacle: a case of **mental wreckage,** the integrity of various mental functions has been im- various levels of mental development, and whereby ower psychic levels have been swept away while the els remain intact. The mentality of epileptics makes ellation that is extremely irregular" (p. 193; also p. r reviewer calls to mind Karl Pearson's lament: "It lar phase of modern science that it steals with a pla- ght hand while it stabs with a critical left."

I did not regard this fact as "irrelevant" is demon- the fact, first, that I sought to corroborate my find- omparison with the results then available based on testing other types of children (particularly normals); ond, by the fact that I have deliberately refrained from the scale on the basis of the testing of mentally abnor viduals.

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

What are the facts in the indictment? The co drawn by the writer as to the comparative intelligence tion of the epileptic and feeble-minded was simply an statement affecting the groups of epileptics and feebl who were actually studied. The reviewer has taken t of generalizing the statement and applying it to the who of institutional and and non-institutional epileptics an 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 of a strictly unequivocal character he would have avoid the second statement quoted above, which is positi On page 189 of the text we read: "The institutiona 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 fiel norant that he does not know that the same statem to 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 di But it is possible that two selective processes have op way to distort both extremes of the curve, etc. * * * not be able definitely to settle this point until other have undertaken similar studies on a large scale." Po

4 He has apparently taken a similar unwarranted liberty in experimental findings in the dental experiment, for which I he claim all responsibility.

nts as the above justify my critic in accusing me of "dog-" and of overlooking the selective influences which de- the distribution of cases in institutions! The justifica- the reviewer's tawdry aspersion ("Alas for the profession s 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- of the Binet scale: "Superficial work like this is mislead- tends to arouse contempt for the slipshod standards of c work obtaining in this field of scientific psychology." re the fact in support of this indictment? I shall here only those charges which, by implication, my critic ap- his revision of the Binet scale. I did him the courtesy original presentment not to single him out for special , but he has thrown down the gauntlet (in a peculiarly s manner) and I am forced to meet the issue. First, I ntended 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 the basis of my own results with abnormal cases. Kuhl- s produced a revision for normal children which is based esting of feeble-minded children, at least so far as con- s **own distinctive experimental contribution** to the revis- h the negligible exception of "forty normal adults" who en **only two** higher age tests, only one of which is a st. The reviewer contends that had I said "a **few** ce mine) of the changes made were based on the per- es of feeble-minded the statement would have been cor- In his "Revision," however, he emphasizes that "the revision * * * * is **largely** along **other lines**" (blackface an the revisions of others. If so, what is the scientific his changes **largely along other lines** if not his own ex- al work on the feeble-minded, for it is not apparent that me 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 on the feeble-minded, nor did he so state in respect tailed directions for giving the tests which he supplies so far as he has altered the procedure of others, mu on his work on the feeble-minded, or otherwise merely the "inner web of consciousness." In other words, specifically state that his revision is largely based on of others, and only to a slight extent on his own wo at the time entirely confined to the feeble-minded, so Binet scale is concerned.

Second, I have charged that in some "instances have been * * * supplied although not a single chil tested in those ages." This statement applies ab the following ages in my critic's scale: "age three mo six months," "age one year," and test 1 in age 2. Th on the reviewer's explicit admission, "were devised on of these observations (observations by writers on ch ter a careful searching through the literature on them plemented by a few chance observations of by own 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 on the basis of definite experimental tests, but on th recorded observations in literature and the author's chance (sic) observations, and this is the type of scien critic attempts to defend, although he attempts to im scientific competency because I have stated certa mental findings which were based, not on a "few chanc tions" but on carefully controlled experiments on 27 c ceiving mouth hygienic treatment. It may be left to to determine who is guilty of "dogmatism." Can it b critic has developed such a degree of hypermetropic motes in my work that he has become profoundly m beams in his own work? Any one knows that the ob in the genetic literature are usually based on the study cases. I shall let the reader pass on the validity of 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 ted and used by a large number of uncritical Binet o 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 's lowest age tests given in baby clinics by "uncrit- testers" who have assumed, and with justice, that y of the placement of the tests has been demonstrated, ey have been embodied in a scale of tests having the of proved reliability. Second, my own use of these by clinics and elsewhere, has failed to show that they alue for grading the intelligence of young infants that ify one in placing them in an age scale. Third, one e organizers of the baby clinic who has used the tests 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."

y critic has very much to say about my dental tests. n, he demonstrates admirably that he is a past-master men of straw, and windmills a la Don Quixote, or of r perverting facts clearly stated, or of presenting his mptions as universally accepted facts.

ne would expect a very large improvement in the several months following dental treatment." Does er 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 no ones, in the reviewer's estimation.

do not know whether they (the tests) measure intel-ciency at all, for no norms at all are given." My re- we do not determine whether a set of tests measure al efficiency" by consulting "norms," but by examining ter of the tests which are employed. What the tests

measure can only be determined by a critical examinati
tests themselves. That question has absolutely nothi
with the subject of norms.

"Only five different tests were used," while I claim
the Binet-Simon scale the number of tests for each
should be increased from five to ten in order to ma
tests reliable." "These tests (my dental tests) we
tests. The author has warned us before that the
group tests are not reliable." The reviewer demonstr
clusively either that he has not read the text with ordi
or that he has *no conception of what I mean by clinica
ations and the requirements which I propose for them
tradistinction to mere mental tests.* He appears to la
the delusion that the tests I gave the dental squad
ical tests, and that I so regarded them, and that th
should apply to them the standards which I apply to
examination. But I have nowhere claimed that the d
were conducted as clinical tests. On the contrary, I d
carried them out as group tests, under the usual rigid
applying to any kind of group testing in educational
mental psychology.

The reviewer accuses me of maintaining that th
of group tests are not reliable." This statement is a
able perversion of the facts in the case. What I did sa
"Norms of mental functioning established by expe
educational psychologists by group tests on squads
may have little practical value as clinical tests" (p.
this statement was not made dogmatically, as the revi
fain have the reader believe, appears very clearly from
ing statement: "At any rate, some one should make
tive study to determine whether there is any differen
norms established by group tests and norms for the
established clinically" (p. 220).[5] "It is quite practi

5 In contrast with the guarded character of the above state
following recent pronouncement of our professedly temperate criti
furnishing a single shred of supporting fact, but suggesting that
actual experience" had "positively and emphatically" proved th
velopment of intelligence "comes practically to a stop at the age
perimental data now in the hands of the writer will show, "po
phatically," that this statement is "dogmatism" ineffable.

al psychologist to give lengthy tests because usually ny one sitting he attempts to measure only a limited of traits. But the psycho-clinicist, in order to get a nsive picture of his case, may test a very considerable f functions" (p. 221). The difference between the re- conception of a clinical examination (if indeed he has d my own is that he thinks it sufficient merely to give umber of tests—"twenty to thirty," "with several age- —while my plea is that we must "survey a maximal num- ndamental functions"—not the same but different func- nd the more of these we have at a given age-level the nd not merely give a large number of tests in various (wide-range testing), many of which may test pre- same functions. I do not advocate increasing the s to 10 for each age, "in order to make those tests re- individual tests, but in order to afford a comprehensive different functions for an accurate clinical picture. author has * * * insisted that in order that the any test may be reliable the tests must be given by a ychologist." The reviewer again misquotes me. What was: "psycho-educational amateurs * * * may be to administer formal psychological tests" * * * ust not, therefore, deceive ourselves with the thought e thereby training competent psycho-educational diag- ." My critic is prone to put into my mouth any words his fancy.

plication he objects to some of these tests being given force of circumstances) by proxy. This comes with race from one who has drawn important deductions ct to his own Binet revision on the basis of tests made —grade teachers. He avers that the writer "does not thing further about the proxy." That this accusation ess the reiewer will discover if he will consult the un- original, to which he was referred in the chapter in

he contrasts clinical study with mental tests or the Binet tests ical tests, mental tests and the Binet tests were mutually exclu- times his discussion seems to indicate that the clinical examination with history taking.

question, but which, evidently, he has not seen, althoug
not hesitate to pass damaging judgment on the whole
Such are his conceptions of the scientific reviewer's h
tions.

"The statement as to the time interval between de
ment and the giving of the several series of mental tes
indefinite." If the reader desires conclusive evidence
reiewer is utterly incapable of writing an accurate, reli
partial review, let him consult page 277, where the pr
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
must be tested." A cursory reading of the book by
minded judge will show that I did not set myself th
establishing "reliable" sex or age "norms" in the dent
ment. On the contrary, I proposed merely to measur
pils' improvement by means of a comparison of their
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
a control squad should also have been used. The wri
and again has called attention to both of these facts,
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 hav
stated thus much. The only other construction is eith
has merely skimmed a book which he is attempting to
ally review, or else he has deliberately set himself the ta
crediting the credibility and competency of the writer.

My critic alleges sarcastically—and with an un
consciousness of his own superior knowledge—that "f
an experiment, made under such conditions," the w
"drawn conclusions, the results of which 'are of fa
importance to the state and the nation.'" My repl
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 destruc- fail to see it (p. 289). Some of the supporting evidence of clinical studies made by duly qualified dentists and s. The reviewer evidently does not even know of the of such data. Second, I do not know that my critic has e any contributions to the science of oral hygiene which n a special insight into the physical and mental effects sanitation and thorough mastication. He opposes his opinion, unsupported even by a pretense of scientific in- on of the problem, to the opinions of a considerable body and women who have been investigating the problem . Does the reader prefer to follow Kuhlmann's theor- ictures, or the conclusions of those who have investi- e problem at first hand (dentists, physicians, teachers, gists, nurses)?

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

y close this peculiarly odious task of exposing to pub- ny what purports to be a scientific review by paraphras- tement from the Preface, which our reviewer ironically the conclusion of his review: "Superficial reviewing is misleading and should arouse the righteous contempt o love accuracy and fairness and hate perversion and aspersion."