DR. WALLIN'S REPLY TO MY REVIEW OF HIS TAL HEALTH OF THE SCHOOL CHILD

BY F. KUHLMANN, Faribault, Minnesota.

In the September, 1914, number of this Journal, I reviewed the book here in question. In the following number Dr. Wallin replies with what he calls an "analysis" of my "attack" and an "exposure to the public scrutiny of what purport to be a scientific review." He regards my review as a whole or different parts of it as "obviously inspired by personal animus," "as literally 'shot full' of blunders and animadversions," with many of the statements as "not only and irrelevant, but utterly and inexcusably false and absurd"; as a "diatribe," as "juggling with the facts," as and "idiotic." The reviewer himself is described as "utterly careless of his facts or willing deliberately to distort in order to misrepresent or malign the writer," as a "past master at fighting men of straw, * * * or of ignoring or perversely clearly stated, or presenting his own assumptions as accepted facts," as "utterly incapable of writing a reliable, impartial review," and as calling to mind Pearson's statement that "It is a singular phase of modern science that it steals with a plagiaristic right hand while it stabs with a critical left."

A scientific periodical is not the place to answer entirely personal slanders and incriminations. I repeat them to show the temperament and state of mind that has produced the "reply," which seems to be directed more against the reviewer than against the review. Aside from this, it is a strange piece of literature, if, indeed, it is entitled to the courtesy of calling it literature. While, on the one hand, he seems to have searched the English language through for terms with which without the least sign of an effort or intention to distort facts, he on the other hand commits all the crimes against common decency that his imagination has been
There is not a paragraph in the reply that is not saturated with statements either absolutely untrue, or misconstructions of the review, or entirely beside the points in question. The occasion does not merit the attempt to answer Dr. Wallin. My reply is to show, in part, how much more severe the review might have been on the author of the book than it was, and still be entirely justified. He speaks of a duty to "expose" the review. Had I had any desire to see the author of "Mental Health" exposed, I could have wished for nothing better than excision of his reply.

The general charge is made that the review is "inspired by personal animus." The review is admittedly and intentionally severe; perhaps, also, it shows some animus. But that this animus is personal, and not aroused by the shortcomings of his work, I alone am in a position to say. There were no grounds for such personal feelings. Yet, Dr. Wallin finds such grounds for note his strange explanation that the animus is due to my discussions on the Binet scale, which are of his writing, as he says, were not yet in circulation! It would be interesting, further, to know under what circumstances Dr. Wallin regards an "animus" as legitimate. For evidently, it seems to him entirely proper and in place when he reply to an unfavorable review of his work.

My review states that "The Author's failure to consider previously published, and the copious advice, suggestions and plans given leave the impression of the unscholarly and amateurish." Of this he explains the absence of references as literature. This, is first, because "the chapters which lack bibliographies are, almost without exception, reprints of public addresses." If this statement were literally correct, it would not be sufficient excuse for not appending lists of references when the same material comes to be published in book form. Readers of a book are not an audience for a lecturer. But the statement is not correct. Twelve, only, of the sixteen chapters without lists of references to literature are indicated as addresses. These twelve have been altered on reprinting, in book form, leaving only five addresses unaltered. If his own state-
ments needed revision to adapt them to book form, why bibliographies not be added for the same purpose? Did author regard these revisions of more importance than his findings to those of others?

3. A list of references to Chapter VIII on "The Present Status of the Binet-Simon Graded Tests of Intelligence" was omitted because, he says, this was frankly only a summary of a larger work he had at the time not yet printed, and such summary he regards as entirely legitimate. My review makes no statement so absurd as to deny the legitimacy of such a summary in itself. It denies the right of the author to label such a summary the "Present Status" of the Binet-Simon tests.

4. Several hundred publications were not consulted for Chapter VIII, he says, because not that many existed at the time of the original address, October, 1911, and reference made to the "parallel findings of a few investigators whose publications were available at the time of writing." The first of these two statements is beside the point. The question is not what was available then, but what was available at the time of writing. The chapter as it stands does not indicate the "Present Status" of the tests in 1914. It should either have been revised or omitted. Its historic value is small.

The second statement is incorrect if he means by "available at the time of writing" all that had been published at the time of his writing. Five other publications only are referred to in the chapter. If this was all that was available to the writer at the time, he should not have attempted to say anything on the subject.

5. It is charged that my statement that he judged the Binet-Simon tests from the results of his examining 333 epileptics "without foundation," because, first, available confirmatory results are cited, and, second, because unpublished results of general experience with the tests confirm his conclusions, still has reference to Chapter VIII. The only available confirmatory results cited are those of Katherine Johnston Goddard (included in the five already mentioned, which together he regards as "more or less unsatisfactory for..."
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and refer only to one of his conclusions, namely, that for the ages of six to nine are “entirely too difficult.”

ing these confirmatory results does not prove that the not judged from the results of examining 333 epile-

page 201 we read: “What now do the results of the

made by various workers indicate with respect to the

ess of the Binet-Simon scale? The space at our dis-

akes it necessary to limit the discussion to a very brief

ation of a more extended monographic treatment.” (A

here refers the reader to the study of epileptics). On

we have: “When the results (referring to the results

epileptics) are critically examined it is found, as a

fact, that there is an amazing lack of uniformity be-

different tests of the same age. The extent of this

y may be expressed in quantitative terms by the average

ations between the percentages of successes for all

the same age. No mean variations have been completed

a colony of epileptics.” Again on the same page:

the differences between the easiest and the most dif-

its in the same ages, based on the performances of epi-

who classify in the given ages, amount to as much as

nt, in age six, 57 per cent. in age twelve ***. It is

ent that most of the age-norms contain tests varying

ously in difficulty.” On page 206 we are told that:

ongest indictment of the scale furnished by these curves

results) is supplied by the mean variations.” On

he tells us that the method of using the tests, as fol-

y others, is defective, as shown by his experience in

 colony of epileptics” and “certain types of insane pa-

For all these instances no other confirmatory results

Yet, Dr. Wallin is able to say that my statement that

d the tests from his results of examining 333 epileptics

foundation.”

The chapter in question makes no reference to unpub-

results of his own, or any of his general experience with

that confirm his conclusions drawn from results with

Moreover the chapter was an address given in De-
December, 1911. The tests were published in 1908, and his serious work with them, so far as appears from his publications, was done with the epileptics begun in the fall of 1910. I therefore somewhat puzzled to find his statement: "Moreover, it is not amiss to say that years of almost daily use of the scale for the purpose of practical diagnosis with a great variety of cases in university and public school clinics, entitles the user to the right to express a professional opinion." This experience seems to have been gained during the period from the fall of 1910 to December, 1911, eight months of which was given to the study of the epileptics.

7. To my statement that Dr. Wallin could not qualify as an expert according to his own definition, he replies: "I am not aware that I have ever posed as a paragon of skill." My review does not make this charge, directly or by implication, though one would have been entirely justified in charging that the book throughout implies that the author outclassed most if not all his colleagues as an expert in psychology. In the very paragraph in which the present statement is made he enumerates a long list of different lines in which he claims to have undergone training to fit himself for work in clinical psychology, which everyone who has followed the author's activities knows could not for the most part have been extensive or thorough enough to merit mentioning.

8. My review states that the schema for clinical study similar to the one Dr. Wallin presents "have never proved of great value in practical work," and he replies that I do not prove my statement and oppose my private opinion to the "well-nigh universal practice by the ablest examiners in the leading hospitals and medical schools of the country." I reply first that the sentence following the one he quotes from my review gives reason for my "private opinion." This notes that: "If the author had shown us definitely how the clinical data called for in his schema could be gathered and utilized in making mental diagnosis some contribution would have been made." It is one thing to outline pages of questions and topics to be inquired into concerning a child's past history, but to gather reliable
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there is quite a different matter. Even if we could obtain
such data, we have as yet no exact knowledge of the relation
to the grade of intelligence of a child at a given time, on
the one hand, and these supposed developmental factors in his
history, on the other hand. These clinical schema appear
very useful to the person who through long experience has
devoted a skill in gathering such data and in evaluating it. But
they are of little practical use because mental diag-

noses are dependent on this experience and skill of the
using them. They are not on the same basis with mental
test which requires, relatively, only a very small
amount of such skill. It is not the schema, which is only a pro-
gram for investigation, and not a method at all, but the skill
the essential thing. Second, there is nothing in this that
contradicts the practice of medical men. I am not aware that
the "ablest clinical examiners" lay more stress on these clinical
than they do on the experience and skill required in
use, nor that these medical men claim that they are especially
able to diagnose grade of intelligence because of their
training, alone and as such.

The review states that: "The epileptic has a special
kind which causes exceedingly irregular results in Binet-
testing. Apparently the author has not discovered this
regards it as irrelevant."

Dr. Wallin claims that this peculiarity about the enilep-
his own discovery and accuses me of appropriating it.
To prove the former he quotes from his book: "We
frame a picture of an interesting spectacle; a case
wreckage, whereby the integrity of various mental
has been impaired in various levels of mental
ment; and whereby the lower psychic levels have been
way while the higher levels remain intact." My state-
nothing as to its original discovery. It also makes
ence to the peculiarity he describes in his further quota-
prove his own original discovery, which is a purely spe-
ep of his own imagination, and for which his study
no evidence. My statement refers to the fact that an
epileptic will so frequently fail in one or several lower age tests and then pass in many much higher tests. To explain this is what he tries to explain, by assuming that lower "psychic levels" have disappeared while higher ones have not is gratuitous.

b. His attempt to corroborate his findings with the results of others, and his refraining from revising the tests he points out as proof that he did not regard epileptic peculiarity as irrelevant in discussing the accuracy of tests. We have already noted how extensive his efforts have been to corroborate his results with those of others. A refraining from revising the tests because he does not regard his results with the epileptics as reliable for this purpose does not explain why they should still be quite reliable for the purpose of drawing a large number of conclusions as to inaccuracies and defects.

10. I am accused of "juggling with facts" when I quote his conclusion that "The typical epileptic category is that of moronity, **while the typical feeble-mindedness is that of imbecility" (p. 186). He now explains that his conclusion had reference only to the inmates of the institutions in question, and that "generalizing the statement applying it to the whole group of institutional and non-institutional epileptics and feeble-minded" was unwarranted. I have already noted that this generalization is not exactly what he intended. My statement "that the epileptics sent to an institution might be selected to any serious degree he does not think likely," is branded as positively false," because, as he quotes to prove, he did admit the possibility of their being selected cases. My statement contains nothing to deny that he considered possible selective factors; it states his final opinion. Moreover, had he continued the quotation from his book another sentence he would have posed his own sham argument in his reply. For, after mentioning possible selective factors, we read: "We shall not
ly to settle this point until other institutions have under-

similar studies on a large scale. But three general con-

clude assured; first, that the great mass of epileptics

aw the feeble-minded line; second that they do not

ow this line to such an extent as the class of amented

ined; and third, that the curve of distribution is mark-

ent for the two classes” (p. 189). Could he draw these

ions if he did not think that it was not likely that epilep-

to an institution might be selected cases in any serious

Dr. Wallin has much to say in criticism of my revision

inet-Simon tests, most of which is foreign to anything

review. It is not necessary to reply in defense of

sion. Unfortunately for Dr. Wallin’s opinion, its very

ception during the three years since its publication

ize well with his present belated utterances. I

ower in continuation of exposing his methods. Perhaps,

ects to my review as unscientific and not impar-

are to take this as a sample of what he regards as a

He says: “First, I have contended that the revision or

tment of a scale of intelligence for normal children

ased on the testing of normal children * * *. Kuhl-

roduced a revision for normal children which is based

esting of feeble-minded children, at least so far as con-

own distinctive experimental contribution to the re-

ith the negligible exception of ‘forty normal adults’
’re given only two higher-age tests, only one of which is

test.” His contention, merely, that no revision of tests

tal children can be legitimately made except on the basis

with normal children is not in itself convincing. My

of using results with feeble-minded in making changes

tests is clearly and fully stated in my revision and other

that the revision refers to. Had he pointed out defects

method, not taken into account by myself, his criticism

point might have been worthy of attention. His present

ion” carries little weight.
b. He says further: "He eliminated eleven tests from the 1908 scale, added nine new ones and shifted six, but he failed to state that only a 'few' (sic) of these changes were based on his work on the feeble-minded." This is a most remarkable statement, coming from one who accuses others of willfully misinterpreting. On pages 4 and 5 of my revision we read the following: "2. Shifting of Tests to Other Age-Groups. In making these changes all available data were taken into account to place them accurately. (References to literature include 38 articles). For the age groups III to XII, inclusive, three tests were shifted. * * * In accordance with the findings of others all the tests of Group XIII were shifted forward several tests "the procedure was changed slightly to make them more equally difficult with others in their group. This was on the basis of my results with the feeble-minded alone." Elimination of Poor Tests. * * * In general, the tests are those most likely to be influenced by the variable training." (Reference to the literature cited here shows the eleven tests eliminated only one was dropped because of results with the feeble-minded alone). "4. Reduction of Five Tests for Each Age-Group and Addition of New Tests. Of these (the new tests added) IV 5 and X 1 only are new. V 5, X3, XII 5, and XV 4 are taken from the 1911 revision. XV 5 is borrowed from Goddard. X 5 are modifications of tests that have been used by different authors. The norms given for the last two are based on ninety of their use on forty normal adults and fifty feeble-minded of mental age corresponding to the age group in which they are found. The norm for IV 5 is based on the results of a hundred feeble-minded with mental ages ranging from three to five. This test has given exceptionally uniform results characteristic of this mental age."

c. The last statement quoted above from Dr. W...
Binet and Simon in a great many instances fail to give specific directions on how to proceed in test and how to interpret responses obtained. Every must supply these for himself. I have supplied them, personally examining over 1300 feeble-minded children, considering the literature on the tests bearing on this matter. In the Psychological Clinic for December, 1911, Dr. published "A Practical Guide for the Administration of Binet-Simon Scale for Measuring Intelligence," in which he adds and alters directions for giving the tests and interpreting responses, not found in the original, or based on any of his own in examining normals. In this he specifically states: "The attempt has been made to outline the procedure which I have found most satisfactory" (p. 218). Does Dr. Wallin object to my standardization of the tests because it is based on examination of 1300 feeble-minded instead of 333 epileptics because it is "spun from my inner web of consciousness" of his own?

He objects to my tests for the ages of three months, six months, and one year, because the norms for them are based on observations in literature," and because "not a single child has been tested in these ages." This is a good illustration of apparently deep-rooted aversion to accepting other people's observations, manifested throughout his book. My revision cites the literature in which these observations are given. They are the observations of Preyer, Moore, Shinn, Major, G. V. N. Dearborn, and many others these authors cite. Dr. Wallin does not deny the reliability of these observations, or that my tests follow these observations accurately. It is simply because I have not myself added my own verification, and because, as I have said "the norms for them are necessarily based on a small number of cases in a number of instances." But evidently I have not said "small number," since he regards twenty-seven as a quite adequate number in his own study on the effects of mouth hygiene treatment.

Again, it is charged that my lower age tests are applied and used by a large number of uncritical Binet test-
ers, * * * who have assumed, and with justice, that the accuracy of the placement of the tests has been demonstrated." I seem to be held responsible for the "uncritical Binet using the tests, for which there might be some justice." I had advocated that no special qualifications are required for their use. But had Dr. Wallin cared to do so, he might have read on pages 8, 9, and 10 of my revision my statement: "Qualifications of the Examiner," in which occurs the following: "The failure of the general public, of the school authorities and medical profession in particular, to appreciate the requirements (referring to qualifications of the examiner) present leading to an extensive misuse of the tests, which necessarily tend to the result of depriving the tests of general recognition of their merits and the public of the benefits of their use." On my claims as to the accuracy of these lower age tests, he might have read as follows: "The institutions for the feeble-minded are as a matter of fact constantly being called upon to pass on the mentality of children less than three years old. They are doing so at present with inadequate methods. In an effort to meet this need I have added tests for three years old children, six months, one year, and two years." Then, after discussing the possible merits of these tests, I conclude: "We believe, however, that on the whole these added tests will do about as well as the others in the scale because they attempt to measure larger rather than smaller steps in mental progress of the child. Yet, under the circumstances, they must be tentatively at present" (pp. 6 to 7). Finally, he repeatedly does in his reply and elsewhere, to the charge of possessing unpublished facts that prove his point, in this his own experience with these tests has shown them accurate, and states that an organizer of baby clinics has found them impractical, without naming the person. Under the circumstances I do not feel compelled to accept his word for established fact. In the same manner, I might reply that my experience of several years with these tests in examining feeble-minded children, in using them in baby clinics and elsewhere emphatically...
what he states his to be, and proves more than what for them in my revision.

In contradiction to a statement of the review, he know of “several leading oral hygienists” who do ex- change improvement in intellectual efficiency in the course months following dental treatment. He is quick to take re of the form of my statement, “no one expects,” intaking its obvious meaning, “no one reasonably qualified expe” Elsewhere we have the repeated contention takes the qualifications of an expert clinical psychologist a reliable mental diagnosis. Here, when he needs their the “oral hygienists” merely, seem to be qualified to amount of mental improvement in question.

The review objected to his using tests without norms using mental improvement after dental treatment, and as that: “We do not determine whether a set of tests ‘intellectual efficiency’ by consulting ‘norms,’ but by the character of the tests which are employed. What measure can only be determined by a critical examina- the tests themselves.” Dr. Wallin here lays claim to the powers, in face of the fact that the whole subject of tests in psychology is full of disputes and doubts as to relation and degrees the different mental functions are in given mental tests. If one can choose tests so easily using changes in “intellectual efficiency” by merely “ex- their character,” why did not Binet and Simon and others follow the same method in devising intelligence at this point was not the main criticism made by the Granted that the tests he used do measure “intellectual this does not do away with the need for norms. Sup- in his “A-test,” for example, the 27 children showed ent. improvement six months after dental treatment. lusion can we draw as to the effect of dental treat- en we have no results on what amount of improvement e been made by normal children, or by these 27 chil- but dental treatment? Obviously none, which doubt-
less explains why Dr. Wallin attempts to hide the main issue by trying to defend a relatively unimportant point.

14. The review pointed out that he used only five tests to determine improvement in "intellectual efficiency," while another place stated that the Binet-Simon tests should be increased to ten for each age group to make them more reliable. He replies that he did not advocate this increase to make them more reliable "as individual tests," but in order to afford a comprehensive survey of different functions for an accurate clinical picture." Let it be granted that the reliability of an individual test in itself and alone is not affected by additional tests; also, that the review does not accuse anyone of such absurdity.

15. The second part of his statement in reply fails to meet the criticism. "Intellectual efficiency" is a complex of different mental functions as well as what the Binet-Simon tests are designed to measure, and should for the same reason, require a larger number of tests "in order to afford a comprehensive survey of different mental functions."

16. My statement that he has regarded group tests, which he used on the 27 children in question, as unreliable while he uses them is branded as an "inexcusable perversion of facts in the case," and to prove this he quotes other statements at great length which he also made about group tests. If he had merely continued one of his own quotations the next three sentences he would again have condemned himself with his own words. We read on page 221, "First, group tests require written responses. But the clinical psychologist must reduce written responses to a merely nominal amount, partly because children differ in the rate of skill in handwriting without evincing a corresponding difference in intelligence; partly because many abnormal children suffer from special motor defects of the hand, so that they cannot do themselves justice in the graphic tests." Can there be any question about my thinking here?

17. I misquote him, he says, in stating that he holds for the results of any test to be reliable it must be
psychologist, because he stated explicitly that "psycho-
amateurs * * * may be competent to administer formal-
tests." He omits the remainder of this sentence, reads, "provided they have been sufficiently trained." He
considers tests as the chief factor in methods of making a
diagnosis as to intelligence, and a large part of the book is es-
nentially a harangue against the reliability of the "amateur"
thesis. On page 142 we have: "All that can be expected
Binet testing by persons who are not expert psycho-
examiners is usually merely an independent confirma-
tion of the pedagogical rating assigned the child in the schools,"
It is doubtful whether the Binet tests will afford an ama-
black-face his) in clinical psychology deeper insight into
ations of the child's mind than the pedagogical tests
observant teacher." On page 132: "The proper
ng of these cases, whether for the purpose of examina-
recommendation or prescription, can only be done by a
education specialist." On page 113: "The more dif-
should invariably be made by the expert clin-
ologist."
The review's statement that "These tests were given by
or by proxy, and he does not tell us anything further
 proxy" brings the absurd reply that by implication I
his using a proxy, instead of to his not stating the
ions of the proxy to conduct mental tests.
The statement that he does not tell us anything further
proxy is regarded as "groundless" because, he says, oth-
statements were made in the original publication.
son given admits all that the review stated or implied.
The review charges that "The statement as to the
terval between dental treatment and the giving of the
series of mental tests is very indefinite," to which Dr.
plies: "If the reader desires conclusive evidence that
er is utterly incapable of writing an accurate, reliable,
review, let him consult page 277, where the precise
every sitting is given." I, too, desire that the reader
follow both my criticism and his reply, here as else-
where. His reply here is one of many good illustrations out of how adept Dr. Wallin is in trying to hide his or by attributing them bodily to the reviewer, regardless shred of evidence.

a. First, in his reply here “sitting” presumably refers to sittings for the mental tests, for he nowhere in the chapter has any dates for sittings for dental treatment. "Accuracy" might have called for his saying so.

b. Second, the giving of precise dates for the sittings for the mental tests the review does not deny. This information alone is of little value, when equally precise dates are not given for the dental treatment.

c. Third, page 277 to which he refers the readers for precise dates has not a word about these dates.

d. Fourth, to repeat my original criticism, his several statements as to dates for dental treatment, etc., and mental tests make it absolutely impossible to determine just what he had between treatment on the one hand and mental tests on the other. On page 276 we are told that the dental treatment was given "during the first few months of the experimental year," which was from May, 1910, to May, 1911. On we learn that mental tests of series 1 and 2 were given during the course of treatment, or after its close; that two tests were given from three to five months after treatment had been completed for all the pupils, while 3 and 4 were given only one or two months after the end of the treatment for more than half the pupils.” On page 277 incidentally, and in another connection, gives the dates of the six sittings for the six series of mental tests. The chapter says about dates. It seems to represent Dr. Wallin’s idea of “accuracy” of statement. To verify my criticism, let the reader try to figure out from the text what the time intervals were between dental treatment and mental tests.

21. Dr. Wallin claims that he has mentioned disturbing factors that might invalidate conclusions to.
He replies that he did not base his conclusions "wholly on the results of the psychological tests." "Some of the supporting evidence consisted of clinical studies made by duly qualified dentists and physicians. The reviewer evidently does not even know of the existence of such data." I reply that the would not suspect the "existence of such data" from any cut in his chapter, for no mention of such is made. Again, if he has based his conclusions partly on such data it is in con-tradiction to his frequently and emphatically expressed opinion that dentists and physicians are not qualified to judge the qus-

Further, he says: "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." The compli-

mation may be returned. His own study on the subject is hardly, either in quantity or quality, to justify the claim to special insight." But the justification of my criticizing him on this subject without "special insight" depends on the nature points criticized. Are they peculiar to this particular sub-

are they points involving matters common to most any logical experiment?

Again, he has shown no scruples in disregarding his nice here. In a recent number of the Psychological Clinic is to have the "special insight" required for criticizing the methods and results of field workers but also in hereditary data. Here he notes that: "Many hereditary charts are based on the sheerest guess work, on data gathered by persons quite lacking in scientific discrimination: he unskilled in the art of hereditary, psychological, or diagnosis. It is one thing to send out field workers, us-

Iachers, nurses, and social workers who are novices in
the methodology of scientific research, to interview relatives, friends, enemies, clergymen, physicians, and officers with regard to the mental condition of the contemporary or ancestral relatives of the cases under investigation; on the basis of the field-workers' reports, have some who has probably never seen or examined a single relative, reconstruct awe-inspiring heredity charts, definitely and minutely labeled and evaluated. But it is quite a different matter to assume that because certain symbols have been stamped on a piece of cloth, the correctness of the markings or the correctness of the hearsay or snapshot estimations and diagnoses has been conclusively established." (See The Psychological Clinic, 1914, p. 3). This he labels the "prevailing methods of gathering hereditary data." But this is not all. He not only reveals a remarkable insight into the qualifications and methods of the majority of our present field workers, and knows that they are entirely incompetent to gather the data, and that they do not diagnose as to mentality of individuals, causes, etc., of merely gathering the facts from which such diagnoses might be made; he has also the technical knowledge required to interpret such data and call to account the highest American authorities whose specialty is the study of heredity by these methods. "I shall in no way concern myself," he says, "in pointing out the confusing, blundering, slipshod, inaccurate, scientific ways in which many—fortunately not all—of the hereditary charts have been worked up and interpreted, yet, what reader knows of any field work or study that Dr. Wallin has done to entitle him to the claim of "special insight?" My review of the "Mental Health" noted that the book gave the impression of the amateurishness; this quotation is more; it is sophomoric, which applies well to much of his book.
NEWS AND NOTES

The following interesting note is communicated by Dr. James C. Carso, of Syracuse, New York.

A year or two before the death of the second Mrs. Wilbur, she related the following interesting account of the accidental first meeting of Dr. Seguin and Dr. Wilbur, in the railway station at Albany, N. Y.

It will be recalled that Dr. Seguin emigrated from Paris, France, to this country in 1848. In 1851 he went to Cleveland, Ohio, and there began the practice of medicine. Not being satisfied with the work nor his success he decided to return to New York City. On his way there, for some reason, he stopped off at Albany, and took a seat in the station to wait for the train to New York. On the same day Dr. Wilbur happened to be in Albany on business, and also went into the station to await a train for Syracuse and incidentally took a seat by the side of Dr. Seguin. They naturally fell into conversation with one another and were soon surprised to...