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DR. WALLIN'S REPLY TO MY REVIEW OF H
TAL HEALTH OF THE SCHOOL CHIL

BY F. KUHLMANN, *Faribault, Minnesota.*

In the September, 1914, number of this Journal, the book here in question. In the following number lin replies with what he calls an "analysis" of my "a an "exposure to the public scrutiny of what purport scientific review." He regards my review as a wh ferent parts of it as "obviously inspired by persona "as literally 'shot full' of blunders and animadversion fications," with many of the statements as "not only and irrelevant, but utterly and inexcusably false and ible"; as a "diatribe," as "juggling with the facts," as and "idiotic." The reviewer himself is described as ei fully careless of his facts or willing deliberately to d in order to misrepresent or malign the writer," as a " at fighting men of straw, * * * or of ignoring or perv clearly stated, or presenting his own assumptions as accepted facts," as "utterly incapable of writing a reliable, impartial review," and as calling to mind son's statement that "It is a singular phase of mo that it steals with a plagiaristic right hand while i a critical left."

A scientific periodical is not the place to answ tirely personal slanders and incriminations. I rep show the temperament and state of mind that has p "reply," which seems to be directed more against than against the review. Aside from this, it is a s of literature, if, indeed, it is entitled to the courte it literature. While, on the one hand, he seems to h the English language through for terms with whic without the least sign of an effort or intention to facts, he on the other hand commits all the crimes ag and common decency that his imagination has been

There is not a paragraph in the reply that is not satur-
h statements either absolutely untrue, or misconstruc-
the review, or entirely beside the points in question.
sion does not merit the attempt to answer Dr. Wallin
My reply is to show, in part, how much more severe the
ight have been on the author of the book than it was,
be entirely justified. He speaks of a duty to "expose"
w. Had I had any desire to see the author of "Mental
exposed, I could have wished for nothing better than
cation of his reply.

The general charge is made that the review is "inspired
al animus." The review is admittedly and intentionally
perhaps, also, it shows some animus. But that this
personal, and not aroused by the shortcomings of his
lone am in a position to say. There were no grounds
such personal feelings. Yet, Dr. Wallin finds such
for note his strange explanation that the animus is due
lure to cite my discussions on the Binet scale, which
e **of his writing,** as he says, **were not yet in circulation!**
be interesting, further, to know under what circum-
Dr. Wallin regards an "animus" as legitimate. For evi-
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
ture previously published, and the copious advice, sug-
and plans given leave the impression of the unscholarly
eurish." Of this he explains the absence of references
ure. This, is first, because "the chapters which lack
phies are, almost without exception, reprints of public
." If this statement were literally correct, it would not
sufficient excuse for not appending lists of references
e same material comes to be published in book form.
of a book are not an audience for a lecturer. But the
t is not correct. Twelve, only, of the sixteen chapters
lists of references to literature are indicated as addresses.
these twelve have been altered on reprinting, in book
wing only five addresses unaltered. If his own state-

ments needed revision to adapt them to book form, wh bibliographies not be added for the same purpose? Di thor regard these revisions of more importance than his findings to those of others?

3. A lise of references to Chapter VIII on "The Status of the Binet-Simon Graded Tests of Intelligen omitted because, he says, this was frankly only a summ larger work he had at the time not yet printed, and such mary he regards as entirely legitimate. My review m statement so absurd as to deny the legitimacy of such a in itself. It denies the right of the author to label such mary the "Present Status" of the Binet-Simon tests.

4. Several hundred publications were not cons Chapter VIII, he says, because not that many existe time of the original address, October, 1911, and refer made to the "parallel findings of a few investigators wh lications were available at the time of writing." Th these two statements is beside the point. The questi what was available then, but what was available at th the publication of the book. The chapter as it stands indicate the "Present Status" of the tests in 1914. either have been revised or omitted. Its historic value The second statement is incorrect if he means by "av the time of writing" all that had been published at th his writing. Five other publications only are referred chapter. If this was all that was available to the wri time, he should not have attempted to say anything the subject.

5. It is charged that my statement that he judged Simon tests from the results of his examining 333 ep "without foundation," because, first, available confirm sults are cited, and, second, because unpublished results general experience with the tests confirm his conclusio still has reference to Chapter VIII. The only avail firmatory results cited are those of Katherine John Goddard (included in the five already mentioned, which together he regards as "more or less unsatisfactory fo

") and refer only to one of his conclusions, namely, that s for the ages of six to nine are "entirely too difficult." ng these confirmatory results does not prove that the e not judged from the results of examining 333 epilep- n 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 lation of a more extended monographic treatment." (A e here refers the reader to the study of epileptics). On we have: "When the results (referring to the results e epileptics) are critically examined it is found, as a f fact, that there is an amazing lack of uniformity be- e different tests of the same age. The extent of this y may be expressed in quantitative terms by the average riations between the percentages of successes for all the same age. No mean variations have been completed or a colony of epileptics." Again on the same page: y the differences between the easiest and the most dif- ts 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 dent 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 e 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 a 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 out foundation."

The chapter in question makes no reference to unpub- sults of his own, or any of his general experience with that confirm his conclusions drawn from results with s. Moreover the chapter was an address given in December, 1911. The tests were published in 1908, and h serious work with them, so far as appears from his publi was done with the epileptics begun in the fall of 1910. therefore somewhat puzzled to find his statement: "Mo it is not amiss to say that **years of almost daily use of t for the purpose of practical diagnosis with a great va cases in university and public school clinics,** entitles the the right to express a professional opinion." This exp seems all to have been gained during the period from th 1910 to December, 1911, eight months of which was g the study of the epileptics.

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

8. My review states that the schema for clinical stu lar to the one Dr. Wallin presents "have never proven value in practical work," and he replies that I do not p statement and oppose my private opinion to the "well- versal practice by the ablest examiners in the leading and medical schools of the country." I reply first that tence following the one he quotes from my review gives son for my "private opinion." This notes that: "If th had * * * shown us definitely how the clinical data in his schema could be gathered and utilized in making diagnosis some contribution would have been made." thing to outline pages of questions and topics to be into concerning a child's past history, but to gather reli

e is quite a different matter. Even if we could obtain ta, we have as yet no exact knowledge of the relation n the grade of intelligence of a child at a given time, on e hand, and these supposed developmental factors in his story, on the other hand. These clinical schema appear ery useful to the person who through long experience has d a skill in gathering such data and in evaluating it. But thod they are of little practical use because mental diag- ith them is dependent on this experience and skill of the using them. They are not on the same basis with mental he use of which requires, relatively, only a very small of such skill. It is not the schema, which is only a pro- or investigation, and not a method at all, but the skill the essential thing. Second, there is nothing in this that cts the practice of medical men. I am not aware that est clinical examiners" lay more stress on these clinical than they do on the experience and skill required in e, nor that these medical men claim that they are espe- ble to diagnose grade of intelligence because of their training, alone and as such.

The review states that: "The epileptic has a special mind 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 epilep- his own discovery and accuses me of appropriating it . To prove the former he quotes from his book: "We to frame a picture of an interesting spectacle; a case tal wreckage, whereby the integrity of various mental s 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- plies nothing as to its original discovery. It also makes ence to the peculiarity he describes in his further quota- rove his own original discovery, which is a purely spec- leap of his own imagination, and for which his study o evidence. My statement refers to the fact that an

epileptic will so frequently fail in one or several lower a
and then pass in many much higher tests. To explain
this is what he tries to explain, by assuming that lower "
levels" have disappeared while higher ones have not is
gratuitous.

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

10. I am accused of "juggling with facts" when
his conclusion that "The typical epileptic category is th
condition of moronity, * * * while the typical feeble-mi
tion is that of imbecility" (p. 186). He now explains
conclusion had reference only to the inmates of the t
tutions in question, and that "generalizing the statemen
plying it to the whole group of institutional and no
tional epileptics and feeble-minded" was unwarranted.
is what he meant at the time it would have been well
said so in the beginning. As the text stands it gives
cation, by direct statement or by implication, that
eralization is not exactly what he intended. My statem
the epileptics sent to an institution might be selected
any serious degree he does not think likely," is brande
tively false," because, as he quotes to prove, he did
the **possibility** of their being selected cases. My re
tains nothing to deny that he considered possible sele
tors; it states his final opinion. Moreover, had he cont
quotation from his book another sentence he would
posed his own sham argument in his reply. For, afte
ing possible selective factors, we read: "We shall n

ly to settle this point until other institutions have under- milar studies on a large scale. But three general con- seem assured; first, that the great mass of epileptics ow the feeble-minded line; second that they do not ow this line to such an extent as the class of amented inded; and third, that the curve of distribution is mark- erent for the two classes" (p. 189). Could he draw these ons 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 Binet-Simon tests, most of which is foreign to anything my review. It is not necessary to reply in defense of sion. Unfortunately for Dr. Wallin's opinion, its very e reception during the three years since its publication harmonize well with his present belated utterances. I swer in continuation of exposing his methods. Perhaps, objects to my review as unscientific and not impar- are to take this as a sample of what he regards as a eview.

He says: "First, I have contended that the revision or ment of a scale of intelligence for normal children based on the testing of normal children * * *. Kuhl- s produced a revision for normal children which is based sting of feeble-minded children, at least so far as con- s own distinctive experimental contribution to the re- with the negligible exception of 'forty normal adults' e given only two higher-age tests, only one of which is test." His contention, merely, that no revision of tests al children can be legitimately made except on the basis s with normal children is not in itself convincing. My of using results with feeble-minded in making changes sts 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 tion" carries little weight.

b. He says further: "He eliminated eleven tests 1908 scale, added nine new ones and shifted six, but he to state that only a 'few' (sic) of these changes were his work on the feeble-minded." This is a most r statement, coming from one who accuses others of wil construing. On pages 4 and 5 of my revision we rea lowing: "2. Shifting of Tests to Other Age-Groups. In making these changes all available data were take count to place them accurately. (References to lite clude 38 articles). For the age groups III to XII, inclu three tests were shifted. * * * In accordance with th of others all the tests of Group XIII were shifted for several tests "the procedure was changed slightly to m more equally difficult with others in their group. This on the basis of my results with the feeble-minded al Elimination of Poor Tests. * * * In general, the tes are those most likely to be influenced by the variable training." (Reference to the literature cited here sho the eleven tests eliminated only one was dropped b results with the feeble-minded alone). "4. Reductio Tests for Each Age-Group and Addition of New Te Of these (the new tests added) IV 5 and X 1 only ar 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 dif thors. The norms given for the last two are based on of their use on forty normal adults and fifty feeble-mi mental age corresponding to the age group in which are found. The norm for IV 5 is based on the results a hundred feeble-minded with mental ages ranging f to five. This test has given exceptionally uniform res acteristic of this mental age."

c. The last statement quoted above from Dr. W tinues as follows: "Nor does he state in respect to th directions for giving the tests which he supplies and far as he has altered the procedure of others must be his work on the feeble-minded, or otherwise spun fro of consciousness.'" Binet and Simon in a great many 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, sonally examining over 1300 feeble-minded children, considering the literature on the tests bearing on this In the Psychological Clinic for December, 1911, Dr. ublished "A Practical Guide for the Administration of -Simon Scale for Measuring Intelligence," in which he adds and alters directions for giving the tests and in- responses, not found in the original, or based on any his own in examining normals. In this he specifically "The attempt has been made to outline the procedure have found most satisfactory" (p. 218). Does Dr. Wal- to my standardization of the tests because it is based xamination of 1300 feeble-minded instead of 333 epilep- ecause it is "spun from my inner web of consciousness" from his own?

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

Again, it is charged that my lower age tests "are ap- ed and used by a large number of uncritical Binet testers, * * * who have assumed, and with justice, that the of the placement of the tests has been demonstrated." I seem to be held responsible for the "uncritical Bine using the tests, for which there might be some justifi I had advocated that no special qualifications are requir use. But had Dr. Wallin cared to do so, he might h on pages 8, 9, and 10 of my revision my statement "Qualifications of the Examiner," in which occurs the ing: "The failure of the general public, of the school ties and medical profession in particular, to apprecia requirements (referring to qualifications of the examin present leading to an extensive misuse of the tests, wh necessarily tend to the result of depriving the tests of eral recognition of their merits and the public of the b their use." On my claims as to the accuracy of these t tests, he might have read as follows: "The institution feeble-minded are as a matter of fact constantly bei upon to pass on the mentality of children less than th old. They are doing so at present with inadequate In an effort to meet this need I have added tests for thr old children, six months, one year, and two years." T discussing the possible merits of these tests, I conclu believe, however, that on the whole these added tes about as well as the others in the scale because the to measure larger rather than smaller steps in ment of the child. Yet, under the circumstances, they must tentatively at present" (pp. 6 to 7). Finally, he res repeatedly does in his reply and elsewhere, to the cla sessing unpublished facts that prove his point, in thi his own experience with these tests has shown them accurate, and states that an organizer of baby clinics them impractical, without naming the person. Un cumstances I do not feel compelled to accept his wor lished fact. In the same manner, I might reply that ence of several years with these tests in examining fee in using them in baby clinics and elsewhere empha what he states his to be, and proves more than what d for them in my revision.

In contradiction to a statement of the review, he know of "several leading oral hygienists" who do ex- rge improvement in intellectual efficiency in the course l months following dental treatment. He is quick to take e of the form of my statement, "no one expects," in- taking its obvious meaning, "no one reasonably qualified expects." Elsewhere we have the repeated contention kes 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 e amount of mental improvement in question.

The review objected to his using tests without norms sing mental improvement after dental treatment, and s that: "We do not determine whether a set of tests 'intellectual efficiency' by consulting 'norms,' but by g the character of the tests which are employed. What measure can only be determined by a critical examina- he tests themselves." Dr. Wallin here lays claim to le powers, in face of the fact that the whole subject of sts in psychology is full of disputes and doubts as to elation and degrees the different mental functions are in given mental tests. If one can choose tests so easily sing changes in "intellectual efficiency" by merely "ex- their character," why did not Binet and Simon and others follow the same method in devising intelligence t 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. clusion 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- out dental treatment? Obviously none, which doubt-

less explains why Dr. Wallin attempts to hide the m
by trying to defend a relatively unimportant point.

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

15. The second part of his statement in reply fail
to meet the criticism. "Intellectual efficiency" is a co
different mental functions as well as what the Binet-Si
are designed to measure, and should for the same reaso
a larger number of tests "in order to afford a comp
survey of different mental functions."

16. My statement that he has regarded group tes
he used on the 27 children in question, as unreliable
he uses them is branded as an "inexcusable perversi
facts in the case," and to prove this he quotes other st
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 condem
self with his own words. We read on page 221, "Fi
tests require written responses. But the clinical ps
must reduce written responses to a merely nomin
partly because children differ in the rate of skill
without evincing a corresponding difference in in
partly because many abnormal children suffer from sp
defects of the hand, so that they cannot do thems
in the graphic tests." Can there be any question abou
ing here?

17. I misquote him, he says, in stating that h
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 logical tests." He omits the remainder of this sentence, reads, "provided they have been sufficiently trained." He mental tests as the chief factor in methods of making a is as to intelligence, and a large part of the book is es- a harangue against the reliability of the "amateur" is. On page 142 we have: "All that can be expected e Binet testing by persons who are not expert psycho-examiners is usually merely an independent confirma-the pedagogical rating assigned the child in the schools," is doubtful whether the Binet tests will afford an ama-ack-face his) in clinical psychology deeper insight into rations of the child's mind than the pedagogical tests the observant teacher." On page 132: "The proper of these cases, whether for the purpose of examina-commendation or prescription, can only be done by a ducational specialist." On page 113: "The more dif-ts * * * should invariably be made by the expert clin-chologist."

The review's statement that "These tests were given by or or by proxy, and he does not tell us anything further e proxy" brings the absurd reply that by implication I o his using a proxy, instead of to his not stating the tions of the proxy to conduct mental tests.

The statement that he does not tell us anything further e proxy is regarded as "groundless" because, he says, rther 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 erval between dental treatment and the giving of the eries of mental tests is very indefinite," to which Dr. eplies: "If the reader desires conclusive evidence that wer is utterly incapable of writing an accurate, reliable, review, let him consult page 277, where the precise very sitting is given." I, too, desire that the reader follow both my criticism and his reply, here as elsewhere. His reply here is one of many good illustrations out of how adept Dr. Wallin is in trying to hide his o by attributing them bodily to the reviewer, regardless shred of evidence.

a. First, in his reply here "sitting" presumably sittings for the mental tests, for he nowhere in the chap any dates for sittings for dental treatment. "Accuracy 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. formation alone is of little value, when equally precise not given for the dental treatment.

c. Third, page 277 to which he refers the reader cise dates has not a word about these dates.

d. Fourth, to repeat my original criticism, his seve ments as to dates for dental treatment, etc., and me make it absolutely impossible to determine just what he had between treatment on the one hand and menta the other. On page 276 we are told that the dental was given "during the first few months of the exp year," which was from May, 1910, to May, 1911. On we learn that mental tests of series 1 and 2 were giv dental treatment; that "the last four tests (3 to 6) 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, 3 and 4 were given only one or two months after the of the treatment for more than half the pupils." On incidentally, and in another connection, gives the of the six sittings for the six series of mental tests. the chapter says about dates. It seems to represent D idea of "accuracy" of statement. To verify the jus my criticism, let the reader try to figure out from th what the time intervals were between dental trea mental tests.

21. Dr. Wallin claims that he has mentioned turbing factors that might invalidate conclusions

dental treatment study. The review does not deny this; izes him for drawing the conclusions he did under the ances. It is not enough to point out faults in one's periments; one should show some evidence that these ave been considered in the final conclusions drawn.

He replies that he did not base his conclusions "wholly e results of the psychological tests." "Some of the ing evidence consisted of clinical studies made by duly d dentists and physicians. The reviewer evidently does n know of the existence of such data." I reply that the would *not* suspect the "existence of such data" from any nt in his chapter, for no mention of such is made. Again, s based his conclusions partly on such data it is in con- n to his frequently and emphatically expressed opinion tists and physicians are not qualified to judge the ques- olved.

Further, he says: "I do not know that my critic has de any contributions to the science of oral hygiene which m a special insight into the physical and mental effects th sanitation and thorough mastication." The compli- ay be returned. His own study on the subject is hardly e, either in quantity or quality, to justify the claim to ecial insight." But the justification of my criticizing him subject without "special insight" depends on the nature oints criticized. Are they peculiar to this particular sub- are they points involving matters common to most any ogical experiment?

Again, he has shown no scruples in disregarding his vice here. In a recent number of the Psychological Clinic s to have the "special insight" required for criticizing y the methods and results of field workers but also in ting hereditary data. Here he notes that: "Many ary charts are based on the sheerest guess work, on data d by persons quite lacking in scientific discrimination te unskilled in the art of hereditary, psychological, or diagnosis. It is one thing to send out field workers, us- achers, nurses, and social workers who are novices in the methodology of scientific research, to interview relatives, friends, enemies, clergymen, physicians, and ficers with regard to the mental condition of the con or ancestral relatives of the cases under investigation; on the basis of the field-workers' reports, have some who has probably never seen or examind a single rela struct awe-inspiring heredity charts, definitely and nutely labeled and evaluated. But it is quite a differe to assume that because certain symbols have been sta a piece of cloth, the correctness of the markings or the of the hearsay or snapshot estimations and diagnoses has been conclusively established." (See The Psychologic 1914, p. 3). This he labels the "prevailing methods of hereditary data." But this is not all. He not only re remarkable insight into the qualifications and methods jority of our present field workers, and knows that the tirely incompetent to gather the data, and that they do diagnoses as to mentality of individuals, causes, etc of merely gathering the facts from which such diagno be made; he has also the technical knowledge requi terpret such data and call to account the highest Ame thorities whose specialty is the study of heredity by th ods. "I shall in no way concern myself," he says, "w *ing out the confusing, blundering, slipshod, inaccurate* tific ways in which many—fortunately not all—of the hereditary charts have been worked up and interpre yet, what reader knows of any field work or study that Dr. Wallin has done to entitle him to the cl "special insight?" My review of the "Mental Hea that the book gave the impression of the amateurish ent quotation is more; it is sophomoric, which appl well to much of his book.

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NEWS AND NOTES

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

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

will be recalled that Dr. Seguin emigrated from Paris, France, to ntry in 1848. In 1851 he went to Cleveland, Ohio, and there began ice of medicine. Not being satisfied with the work nor his success 1854 he decided to return to New York City. On his way there, for son, he stopped off at Albany, and took a seat in the station to wait in to New York. On the same day Dr. Wilbur happened to be in on business, and also went into the station to await a train for and incidentally took a seat by the side of Dr. Seguin. They natur- into conversation with one another and were soon surprised to