taste for grandiloquent oratory, but of that unaffected speech which is in earnest to force conviction, he was a consummate master. Fitted to shine in society, he usually avoided it, to the regret of his friends, as they felt that he would have been more widely beloved, had he been less of a recluse. Those who were invited to his home found him cordial in manner, fascinating in conversation, a brilliant talker, often speaking with humor, more willing to show his learning, his love of poetry, and his literary treasures at his own fireside than in any public theater.

Senator Sumner said:

During the whole period of the war, when appropriations were beyond precedent in the world's history, Mr. Fessenden's influence swayed the Senate, and what all our best generals were in the army he was in the financial field.

Hannibal Hamlin (Vice President, and presiding officer of the Senate, 1861-'5), said:

The duties and victories of civil life are as important as those of arms, and the statesman, who aids in wisely directing the councils of the Nation, should be held in as cherished remembrance as he who successfully commands our armies in the field. Such is the position the historian will assign Mr. Fessenden.

THE ORIGINATION OF ORGANIC FORMS.

By Dr. Charles A. White.

On November 5, 1907, the editor of *The Annals* wrote to his lifelong friend, the eminent scientist Dr. Charles A. White, once State Geologist of Iowa, as follows:

Dear Dr. White:

Your kind letter of recent date was forwarded to me at my home in Boone, where I had the pleasure of reading it some days ago. I was very glad to hear from you; glad that you are still able to write friendly and entertaining letters. I understand you to say that you have ceased writing for publication. I regret this because I have valued your contributions to *The Annals* very highly. I wish that you might still write an article upon the Mutation Theory. I believe that you are the leading exponent of that theory in this country and you understand the views of Professor de Vries probably more thoroughly than any other man in this country. I be-

lieve it would please your old Iowa friends, if we could publish an article from your pen, giving an analysis and outline of that new theory which seems destined to become one of very much discussion during the next decade. So believing, I hope you will reconsider your determination not to write any more for publication, and send me the article before the end of the year.

Faithfully yours,

CHARLES ALDRICH.

Dr. Charles A, White, Washington, D. C.

In reply Dr. White wrote as follows:

My Dear Mr. Aldrich:

The desire which you expressed to me in a recent letter to make *The Annals of Iowa* a record, not only of what the people of our State have done, but of the part they have taken with reference to current subjects of thought, is especially appropriate because such records are a part of the intellectual history of the commonwealth. There are, however, many reasons why I do not feel equal to the task which you have proposed to me of writing for *The Annals* a formal essay upon the history and present status of the various theories concerning the origination of organic forms which have been held since Iowa began its political existence, including special reference to the latest of them, the mutation theory of Professor Hugo de Vries. The facts which you refer to in a letter, that I have lived contemporaneously with every American naturalist who has published results of any systematic work, that I have had personal cognizance of all the theories referred to as they have successively prevailed and that I have often taken public part in their discussion, are of themselves sufficient to indicate that the time has come for me to lay aside my pen, at least as regards subjects which require exhaustive treatment to be of any value. Still, in view of our intimate friendship of many years I have written for you, in the style of our familiar conversations, the following discursive account of my personal experiences and opinions with reference to those theories and to the men who have accepted and advocated them respectively.

Although men have naturally speculated upon the manner of origin of organic forms ever since they began to study and classify them, only two principal theories pertaining to that

subject have prevailed, or, as it is generally expressed, only two theories of the origin of species, have been favorably received among cultivated people. These are the theory of special creation and that of evolution. The other theories of which I shall speak are subdivisions of the latter.

The theory of special creation, as the modern naturalist has held it, has required of him the belief that the specific, generic, family, and ordinal forms of animals and plants are the expression in earthly materials of categories of creative thought in the Divine Mind; and that the homologies of structure which are found in the species which constitute the respective classes and orders have, in each case, originated in accord with an archetypal plan which was also divinely conceived. It also required belief that every species was produced suddenly as a complete and permanent entity and, having had no antecedent existence, the generation of its kind and its heredity necessarily began after that act, and had no connection with its origin. The theory was purely speculative and quite illogical. It gave the inquirer not even a suggestion as to the method of execution of the creative act, or whether it is still occasionally performed in such a manner that it might be possible for some person to witness it.

I found this theory to be at least tacitly held by all the naturalists with whom I came in contact when, as a boy, seventy years ago, I first began to study nature and to question every naturalist with whom I could get a hearing. Not one of them ever rebuffed me, for the true naturalists always loves an inquisitive boy. That theory prevailed until I had myself become the author of several paleontological papers, all based upon Iowa fossils and all bearing tacit reference to the then prevailing theory. Indeed, I did not abandon that method of thought until about the year 1866.

The people generally, especially those who held any definite religious belief, adhered firmly to the theory of special creation until the mental battle was finally won by the naturalists. For the devout naturalist the belief in special creations was an agreeable one, for he felt that he was dealing with living forms just as they came from the hand of the Creator and if,

in the study of fossil remains, or when exploring those regions of the earth in which primeval faunas and floras still prevail, he discovered new species of animals or plants, he felt that he was specially favored in being the first to look upon forms that had been the result of divine conception and special creative acts. He who held such views, however, usually had little disposition to philosophize, and the subject was to him purely one of sentiment and faith. He did not pretend to know how the creative act was performed; it was enough for him to know that his God performed it.

My early associates were peculiarly earnest men, and thought seriously and honestly upon the subject of the origin of species, although they seldom referred to it in either writing or speech. That their mental attitude, however, was unsettled upon that subject was shown by the readiness with which they accepted the theory of evolution when the great revolution in biological thought, which I am about to mention, swept over not only our own country but the whole world. It is to me a grateful remembrance that such men were my earliest mentors and that many of them remained my personal friends as long as they lived. It was those men who laid the foundations of biological science in America and as the years went on, their numbers and the effectiveness of their work increased. Among the leaders of this group of pioneer naturalists may be mentioned Agassiz, Dana, Gray, Hall, Newberry, Torrey and many of their contemporaries whose names are familiar to every naturalist, even of the present day. All these men were gifted with clear insight into the mysteries of nature and all were self-taught as naturalists for, up to the time I now refer to, about the middle of the last century, the curriculum of no American college provided for adequate instruction in even the elements of any branch of biological science. Chairs for several of the branches were soon afterward established in the principal colleges, and those self-taught naturalists became the first professors who occupied them.

In 1846 Professor Louis Agassiz began to lecture on zoology at Harvard University and his success soon made him popularly the most prominent scientific personality in the world.

Many of his contemporary naturalists were quite as able as he, but no person was ever more successful in awakening public, as well as special, interest in biology, and no person ever had greater success than he in drawing young men to its study. I mention him particularly in this connection because, when the great revolution in biological thought occurred he, almost alone of the naturalists in our country, attempted to stem the tide; but it was all to no purpose. Darwin's book, *The Origin of Species by means of Natural Selection*, like a firebrand, set the thinking world ablaze. The first edition was published in 1859 and other editions followed, all reaching our country when events were culminating in our civil war. Even the mad rush of battle could not prevent men from following the progress of the far-reaching revolution in scientific thought of which that book was the chief exciting cause. When the war was ended thousands of returning soldiers and other young men thronged the schools to recover lost opportunities for education. The students of biology, almost without exception, became earnest advocates of the Darwinian theory, and many of them went farther in its advocacy than the honest and cautious author himself had ventured. Even the special students of Agassiz accepted the theory of the origin of species as propounded by Darwin. A few of the older and more conservative naturalists of our country accepted that theory tentatively or, to use their own words, "as a working hypothesis;" but even this faint opposition soon ceased. Until his death in 1873, Professor Agassiz continued to teach the views which he had always inculcated and, although he made effective use in his teaching of the weak points in Darwin's theory, little heed was given to the unpopular side of that much discussed subject. As time passed, however, naturalists began to give more attention to those weak points, a large part of which Darwin himself had frankly and carefully discussed, especially in the later editions of his work. I need to make no extended mention of the hostile attitude that many people assumed, especially the leaders of the church, toward evolution, and only remark that in the earlier years of the controversy it was exceedingly bitter and that as knowl-

edge of the real character of the theory increased such opposition decreased until it is now rarely met with.

The term "evolution theory," as it is often broadly used, is applicable to so many subjects, not only biological but astronomical and physical; and even to the human and social sciences, that the term "thesis" would logically be more appropriate for that general use. I shall of course use the term evolution theory in this connection only with reference to its biological signification. In this sense that theory requires the belief that every now existing animal and plant, not excepting man, has been genetically produced along collateral and diverging chronological lines, beginning far back in geological time with minute single-celled organisms, such as those to which the names monad and infusoria are generally applied. That theory has hitherto not included inquiry as to the origination of those first forms of life for, as a rule naturalists considerately decline to concern themselves with predeterminate causes.

The theory of organic evolution, as it is generally accepted, maintains that those simple original forms of life contained potentially the germs of all possible future forms, and that all the animals and plants which now exist, and all that ever have existed, have resulted from the genetic unfolding of those germs. That is, all those forms were derived from pre-existing forms by the ordinary process of natural generation. No person now rationally questions the fact of evolution of organic forms; the prevailing differences of opinion all have reference to the manner in which evolution has been accomplished. I therefore now lay aside all references to the old belief in special creation as having no scientific basis. With the next following paragraph I also discontinue references to the great theory of organic evolution and to the greater thesis of universal evolution because they are unquestionable from a scientific view and need no verbal elucidation.

The theory of organic evolution was proposed by de Maillet as early as 1758, a hundred years before Darwin's great work appeared. Before the appearance of that work also, Darwin's grandfather, Erasmus Darwin, Goethe, the poet-philosopher,

Lamarck, Geoffroy St. Hilaire and more than twenty other authors published their advocacy of that theory. It may be said in passing that, because men are so little inclined to leave the beaten paths of thought until marshalled on less trodden paths by a master mind, the authority of those able men, with the truth on their side, produced but little impression upon the then prevailing theory of special creation. It may also be truthfully said that even among the naturalists of those days little interest was taken in the views which those writers enunciated before Darwin's great work was published. Even the main features of Darwin's theory had been incidentally recognized by a few other men, but he formulated his theory so fully and admirably that its acceptance was assured. Therefore it hardly need be mentioned that Charles Darwin did not originate, and never claimed to have originated, the theory of evolution. The object of his famous work was to show how, according to his conclusions, evolution of organic forms has been accomplished. He required a comprehensive volume in which to express his views, but I must try to give you a summary of them in a few sentences.

The leading proposition of Darwin's theory may be stated as follows:—Variation is a constant and natural condition with all animals and plants, no two individuals, even of the same parentage, ever being exactly alike. The terms fluctuating, common and gradual variation are often applied to this kind of instability of smaller organic details. These variations are known, at least in a general way, to every one, and they are especially familiar to those who have practical knowledge of the waters and of field and wood-craft. One thus knows every tree, and every leaf of it, at sight; and yet he never saw any tree or any leaf exactly like another. Animals are similarly variable. Much as the birds of a flock resemble one another, and much as that resemblance enables us to distinguish them from other kinds of birds, no two of them are ever alike in all details of bodily structure, plumage and habits. Darwin assumed that species are produced by the accumulation of these, and correlated kinds of variation through long periods of time, by a process so slow that a human life is far too short

in which to witness any material change. The slowness assumed by Darwin's theory is such, indeed, that the process of specific variation by natural selection could hardly be detected, even by a long succession of generations of men trained to biometrical observation.

With that central idea of the origin of species by the accumulation of ordinary natural variation, Darwin proceeded to show what phenomena have prevailed which he believed were sufficient to modify, accelerate, or retard the process of evolution. These phenomena include the various conditions of environment under which the animals or plants exist, chief among which, as the title of his book denotes, is that form of vital competition that is usually designated as natural selection, and which Darwin thus defines in a single sentence: "This preservation of favorable variations and the rejection of unfavorable variations I call natural selection." The terms "survival of the fittest" and "the struggle for existence" are also often used with reference to the same subject. Darwin justly shows that animals and plants multiply themselves so rapidly that, if they met with no adverse conditions they would soon cover the earth with their progeny; but because of the prevalence of various adverse conditions and of the difference in vitality and adaptability of individuals there is a constant struggle in which the more vigorous survive and the weaker perish. The claim, however, that this struggle is a factor in the origination of species is vigorously denied, especially by Professor de Vries.

Every reader of Darwin's book will observe that while he has presented an astonishing array of facts which he applies in support of his theory, it is in important respects a speculative one. Even its main proposition cannot be demonstrated because it required a length of time which has no known limit or ratio. For example, the oldest known fossiliferous strata in the geological series, which are much older than the oldest of the Lower Silurian, contain remains of invertebrates belonging to no less than five of the six sub-kingdoms which compose the animal kingdom; namely, the Mollusca, Annulosa, Annuloida, Coelenterata, and Protozoa; remains of the sub-

kingdom Vertebrata only being absent. Those remains show that a large proportion of the species which then existed were as highly organized as are any of their kind which exist to-day. That is, the progeny of those early forms have come down to our time in lines which are nearly parallel; or they have so little evolutional divergence that they indicate only slight progressive differentiation for the successive generations. Therefore, if we bound a chronological column representing those early forms and their descendants by an imaginary straight line upon each side of it, and extend those lines back into the abyss of time until they meet at a converging point which shall be assumed to represent the time of introduction of the first life upon the earth, we shall have an evolutional parallax which will carry that point back to a time inconceivably remote. That immeasurable antiquity of the origin of life upon the earth is really required by the Darwinian theory and belief in the accuracy of that assumption is accepted by those who have adopted that theory without qualification. The possibility that the earth has existed so long in a habitable condition for animals and plants is positively denied by able physicists and astronomers, and it is no less difficult for a layman to believe. It is this requirement for illimitable time and the production of systematic species by the accumulation of common variation, to which the strongest objection is made by those who oppose the Darwinian theory.

Among his numerous writings Darwin proposed the theory of pangenesis in support of his views of heredity which, in its chief features, is strangely like a theory that was enunciated by Democritus in his Atomic System four hundred years before Christ. Darwin assumed that gemmules, or infinitely minute granules, derived from all parts of the body, circulate through the body and finally gather in the germ cells. These gemmules, having the power of reproducing the cells from which they were derived, endow the germ cell, in bud or ovule, with the power to produce a complete individual. The cells and their contents, the nuclei and protoplasts, are readily seen under the microscope, but the pangenetic gemmules are beyond the reach of vision and their existence is therefore theoretical.

The foregoing paragraph, with which I close direct reference to Darwin's labors is of special importance with relation to the labors of Professor de Vries, which culminated in his mutation theory. Professor de Vries is a botanist and his experiments and demonstrations have hitherto been confined to plants, but he logically believes that his conclusions will be found applicable to animals also. In his investigations he carried the pangenetic idea beyond the limits which were assigned to it by Darwin and in 1889 he published his views on that subject in a small volume entitled *Intracellulare Pangenesis,* in which work he deals with molecular conditions within the cells. He hypothetically assumes that every heritable attribute is attached to a material vehicle within the living protoplasmic substance of the cell. These vehicles, together with their respectively associated attributes, he calls pangenes and claims that they enter into the structure of all living protoplasm. He says, "Each heritable attribute, be the species ever so numerous through which it has descended, has its own special kind of pangenes. Many such kinds of pangenes are associated together in every organism and they increase in number with the increase of organic differentiation." A part of the pangenes are functionally grouped within the cell, especially the germ cell, and more particularly within the nucleus. Their progeny are transported to and from the various parts of the organism along the protoplasmic streamlets that radiate from, and connect together, the protoplasts, or cell contents, of all the living parts of the organism.

It was while formulating this theory of intracellular pangenesis that Professor de Vries conceived the idea of his mutation theory, which is now before the world. The following translation from among the formal statements made by the distinguished author in his great work presents his idea of specific mutation concisely:

> The attributes of organisms are built up of fixed and sharply defined units [the pangenes]. These units combine in groups, and in the kindred of species the same units and groups are reproduced. The origination of a new unit signifies a mutation. Every addition of a unit to a group constitutes a step, originates a new group and separates the new form sharply and fully, as an individual species, from the one out of which it has been produced. The new species

is at once such, and originates from the former species without apparent preparation and without gradation. Each attribute of course arises from the one previously present, not by their common variation but by one sudden change. Provisionally, one may compare these changes, but only in the simplest manner, with chemical substitution.

Since I began to speak of pangenesis we have been beyond the aid of either vision or palpation and upon the border-land of the knowable. Let us return to the field where Professor de Vries demonstrated his theories by practical experimentation. The ten years following the publication of his *Intracellulare Pangenesis* were devoted to the collection and collation of facts in support of his already conceived theory of mutation. He reviewed the floras of many regions and gathered a large number of plants from their natural habitats into the University garden at Amsterdam for experimentation. The story of his labors is of absorbing interest, but I need now only say that, with all the advantages of cultivation and protection which he gave to his selected plants few, if any, of them showed any more indication of mutability than did those which he had long studied in the field. Finally, however, a few miles from Amsterdam, he found specimens of the American evening primrose, *Oenothera Lamarckiana,* which had become acclimated and very abundant in Holland, both wild and cultivated, to be in an active state of mutation. Among the abundant typical specimens of that species he found two specific forms which were new to him and which he believed had been then and there spontaneously derived from *Oe. Lamarckiana.* Transferring these new forms, together with many plants of the common form, to his experimental gardens he obtained by artificial breeding a repetition of the new forms which he had discovered in the wild state, and also several other new species, all being direct progeny of *Oe. Lamarckiana.* Moreover, some of the new species became themselves mutable and gave origin to other new species, until the new ones numbered not less than half a dozen.

The new species thus produced were clearly distinct in essential attributes from the parent species and from all other known species of the genus *Oenothera.* Moreover, by sub-

sequent breeding Professor de Vries found those attributes to be as heritable and invariable as are those of any other species. As a rule, however, the differences between these newly originated species are not so great as are those between the ordinarily recognized species of systematic classification.

These initiative forms Professor de Vries designates as experimental species. They enter at once into the struggle for existence with all associated plants, even with the parent form and its other mutated progeny. In this struggle multitudes of new species doubtless perish and leave no sign that they have ever existed, for the struggle itself in all such cases is conspicuous evidence of nature's extravagant wastefulness. Such perishing, together with additional mutations, makes gaps between the surviving experimental species, which are broader than the original initiative gaps. The competitive struggle also naturally tends to bring the victorious experimental species to the prominent condition of those which are commonly recognized in systematic biological work.

The evening primroses bear an abundance of seed which, in reproduction, are generally as true to the parent species as are the seeds of any other plant. In no case did Professor de Vries find all the seeds of any plant, or all the seeds of a single pod, in the mutative condition. On the contrary, he found only a very small percentage of the seeds of any one of those plants to be mutable. Of these, some might occupy separate pods, or all of them might occupy a single pod; and each one of the mutative seeds might give origin to a plant of a different species. All the mutated species might be new, or a part of them might be repetitions of former mutations. We are to understand that in every such case there was a different disturbance and rearrangement of the pangenes in the germ cell of the mutating seeds, although they were produced by the same plant in the same pod; and those variously mutating seeds might have grown closely adjacent to each other and to normal seeds. It is also important that the specific status of the parent plant was in no way affected by the fact that it had given mutative origin to a part of its progeny.

Up to the time of the publication of his great work, *Die Mutationstheorie* in 1901, Professor de Vries had discovered no other species in the mutative condition besides *Oe. Lamarckiana* and its progeny, but there is reason to believe that others have since been discovered. This fact leads to the statement of a specially important feature of the mutation theory as enunciated by Professor de Vries. According to the published views of that author, his theory involves the immutability, as well as the mutability, of species. That is, aside from the ever-present common variation of organic forms, which has no phylogenetic connection with mutation, immutability is the rule, mutability the exception. He concludes that, with rare exceptions, all species pass the greater part of their existence as such in the immutable state and that their apparent stability is real when in that state.

While Professor de Vries was preparing his *Mutationstheorie* for publication I was experimenting with tomato plants in my garden. For two separate seasons I had obtained very remarkable results from my usual sowing of seeds of the Acme tomato, which is a typical and well-known variety of *Lycopersicum esculentum*. Every plant of both of those crops was a typical *L. solanopsis* or "potato-leaved tomato," and every plant bore one and the same new variety of fruit for both seasons. That is, there was in these cases not only a specific plant mutation but a varietal fruit change. I obtained at this time advance numbers of Professor de Vries' great work and became convinced that I also had induced a case of mutation, but as it differed by its remarkable comprehensiveness from the cases described by Professor de Vries I called it aggregate mutation, and have often so designated it in my publications.

Within the past year Dr. O. F. Cook of the U. S. Department of Agriculture, who is making special studies of the species and varieties of cotton plants, has discovered several cases of aggregate mutation among them. These cases are quite parallel with my cases of tomato mutation and the existence of that kind of mutation among plants may therefore

be accepted as established. The determinate* cause of the disturbance and rearrangement of the pangenes in Professor de Vries cases acted for each mutation upon only a single seed, chosen from among hundreds of associated normal seeds. Accepting the theory of intracellular pangenesis as applicable to the cases of aggregate mutation we must assume that the determinate cause has acted upon every seed, of every fruit, of every plant of the whole crop.

There are at least seven great groups of parasitic plants, three of which groups are well-known in our country as dodders, mistletoes and broom-rapes respectively; and another group is equally common and includes the louse-warts, painted-cups, and many other equally well-known plants. They all bear flowers and fruits such as characterize the phenogams, flowering plants, and all the groups are, by their parasite characters, clearly distinct from one another and from all other plants. One cannot doubt that in the great evolution of the vegetable kingdom they all become phenogams before they become parasites. That is, they originated by degradation from higher forms, and were not progressively developed from lower forms. Because the parasitic characters of each group are so distinctly defined and because no trace of the ancestral lineage of any of those characters has been discovered, I assume that each group of parasitic characters has originated by an abnormal aggregate mutation somewhat similar to, but wholly distinct from, the cases of aggregate mutation already mentioned.

There is a prevalent popular belief that species, especially of plants, originate as fertile hybrids from the union of pre-existing species. It need not be doubted that some, perhaps many, such cases occur, even in the wild state, but that fact does not account for the origin of those parent species. Moreover, Mendel long ago showed that in cases of fertile hybrids the progeny tended to revert to, and become lost in, the pre-potent one of the two parent species. It is therefore plain

*The terms "determinate" and "predeterminate" are necessarily used somewhat loosely because we have so little definite knowledge of those causes. I use the latter term as the more comprehensive one.

that hybridity is not a fruitful source of new species in a systematic sense.

The foregoing remarks on aggregate mutation of tomato and cotton plants, the probable origination by abnormal aggregate mutation of the flowering parasites, and hybridity, are introduced to show that the scope of origination of organic forms has been much more comprehensive than is merely the origination of species, even in its most comprehensive scope. Besides this, the origination of the heritable varieties, races, breeds, etc., is doubtless similar in character to that of species. The ordinary definition of a species is that it is a sub-division of a genus and composed of individuals which have characteristics of structure, form, color and habits in common, and which reproduce their kind without material variation by successive generations. A species however is not a definite quantity. Some are conspicuous by the comprehensiveness of their attributes or characters and their great difference from other species of the same genus, while other recognized species differ so little from one another that many naturalists regard them as only heritable varieties. Indeed, much of the present disagreement among naturalists as to the manner of the origin of species is connected with the differences of opinion which they hold as to what constitutes a species. The foregoing discussions are necessarily brief, but they are sufficient to show that while there have been remarkable changes of opinion concerning the manner of origin of organic forms the question is still an open one among naturalists.

You ask me to give you my personal estimate of the mutation theory. The remarks which I have already made indicate this but I may add that my disposition toward it is favorable. I cannot believe that the facts which Professor de Vries has published will ever be disproved, and his theory accords with well-known biological conditions concerning which the Darwinian theory is deficient. I also do not think that any better explanation of the molecular movements within the protoplasmic contents of cells can be made than is that which is offered by his theory of Intracellulare Pangenesis. Still, I confess to grievous disappointment that so few plants have been found in

the mutative condition. To meet the developmental requirements of the vegetable kingdom there should be somewhere a large number of species of plants in the mutative state, but hitherto they have not been discovered. Botanists should give the subject no rest until this vital question is settled.

Faithfully yours,

March 1, 1908. CHARLES A. WHITE.

REPORT ON OPERATIONS OF IOWA TROOPS IN MISSOURI IN JUNE, 1861.

BY COL. SAMUEL R. CURTIS.

In the course of its work in November, 1907, the commission having in charge the preparation of a complete roster of Iowa soldiers, sailors and marines discovered a most interesting military report. It is in the original manuscript form such as was prepared customarily at the front, and is subscribed in his own hand by Samuel R. Curtis, at the time Colonel of the Second Iowa Infantry. It is published below verbatim. No Iowa document appears to have referred to it, while opinions appended support the belief that it was never before published. E. R. H.

Society of the Army of the Tennessee,
Office of Recording Secretary,
Cincinnati, O., Dec. 12, 1907.

My Dear Mr. Aldrich:

The only reference that I find to Curtis's 2d Iowa on the Hannibal and St. Joe road is Vol. 3 of War Records of the Rebellion at page 388, where Lyon says: "Col. Curtis is, I suppose, on the Hannibal and St. Joe road; vigorous measures should be shown the disorderly in that region. * * *" I am quite sure that the report in question has not been published.

Yours sincerely,

CORNELIUS CADLE, Rec. Sec.

War Department, The Adjutant General's
Office, Washington, D. C., December 7, 1907.

Hon. Charles Aldrich,
Des Moines, Iowa.

Sir,—I have the honor to advise you that nothing has been found of record in the War Department to show the receipt of such a re-