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REPRODUCTION AND INHERITANCE IN ASCOMYCETES[1]

By Dr. B. O. DODGE
THE NEW YORK BOTANICAL GARDEN

IT is only within the last few years that students of the rusts and ascomycetes have obtained definite information about the processes actually involved in fertilization. Some of my associates have suggested that in view of this added knowledge it might be well to bring together in review on this program available information bearing on sex, maleness and femaleness as related particularly to heterothallism, incompatibilities and sterilities in the fungi. This did not seem to be advisable, however. Certainly by the time the naturalists hold their next regular quarter-century symposium on sex the mycologists will be in a better position to contribute positively to such a program. Although some of the topics presented for your consideration to-day may incidentally have a bearing on questions relating to sex in general, they are brought forward primarily to indicate some further profitable lines for thought as well as research in connection with the ascomycetes.

Muller believes that the essence of sexuality is Mendelian recombinations. While morphological differentiations are enhancing, they are secondary and dispensable. Allen has long studied sexuality and inheritance in the dioecious Bryophyta where differentiation of antheridial and archegonial haploid plants is clear cut and probably has a chromosomal basis. He would not be inclined to accept, as applying to his group, the theory of relative sexuality according to which all organisms, simple or complex, haploid as

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[1] Address of the retiring vice-president and chairman of the Section for the Botanical Sciences, American Association for the Advancement of Science, St. Louis, December 31, 1935.

well as diploid, plant or animal, are potentially bisexual or hermaphroditic. Allen voices a view held very generally when he says that sex and sexuality imply advantageous morphological and functional differentiations. Link would also insist that without differentiation into male and female there can be no sex and therefore no sexual reproduction. We shall understand the situation in the ascomycetes better if we first take a few illustrations from other groups of the fungi.

In *Sporodinia* the fusing gametangia are exactly alike and there is nothing whatever to suggest male and female structures. According to your definitions the formation of zygospores would not be a case of sexual reproduction. In *Zygorhynchus heterogamus* there is a noticeable difference in size and position of the fusing cells. This is sexual reproduction, the large cell being female, the smaller one male. In *Absidia spinosa* a similar difference is found; but when they grow the *Absidia* against the *Zygorhynchus* or test them out in some other comparable way, large cell reacts with large cell, small with small, that is, female reacts with female, male with male. These interspecific matings never result in viable zygospores, but the reactions are very definite. Either difference in size here is not a mark of maleness and femaleness or this distinction has relatively little significance.

There is something here we are apt to overlook. The nuclei that come together in the zygospore of hermaphroditic species are exactly alike genetically, while the nuclei in the +/– gametangia of heterothallic species such as *Phycomyces nitens* may be genetically very different. It would be difficult or perhaps impossible to prove this in the zygomycetes, but it has been proved many times in the ascomycetes. Biochemical studies by Satina and Blakeslee provide no evidence at least against Blakeslee's original conception that – races are male and + races are female. These authors are not ready to insist that +/– differences have a chromosomal basis even in dioecious species. The reactions in cross-matings can not be due to the fact that the nuclei involved are unlike or that they are alike genetically. There is some other change here correlated with age or maturity. Zygospore formation in the first case would assist evolution, but it would not do so very much in the second. Certainly we should not use the symbols +/– to designate genetic nuclear qualities in one case and merely phenotypic cell differentiations in the other. It is on this account that I have not employed these signs to designate races of opposite sex-reaction in ascomycetes like *Neurospora* where the reaction has a chromosomal basis.

Arthur seems to have favored de Bary's views on the nature of spermogonia of the rusts, yet he chose to apply the term pycnium to the spermogonium and refers to its spermatia as pycniospores. This no doubt greatly influenced the younger generation to come to believe that spermatia wherever found are merely microspores. Craigie's discovery that in the rusts they actually function as fertilizing elements changed this view almost over night. The pycnium must after all be male and its pycniospores are proved to be spermatia. Morphologists never have questioned this.

Craigie, Ruth Allen and Hanna again disturb our notions, settled for a time, by proving that the paraphyses which often extend well out of the spermogonium are trichogynes down which spermatia nuclei travel to accomplish fertilization. The spermogonium is then hermaphroditic, male and female. If these paraphyses are female sex organs, what are those trichogynous hyphae that grow out through stomatal openings to receive the male nuclei? Andrus says they are female organs. Furthermore, they now claim that the uredospores can be made to function as fertilizing elements, and finally that the spermatia after all are merely microspores because they can germinate to form mycelia. All of which leads me to ask: Does the way an organ functions count only when its functioning furnishes the evidence we are looking for to support some particular morphological concept?

Bornet and Thuret's work on the nature of trichogynes and spermatia of the red algae stimulated much corresponding research on the primordia of the fruit bodies of the lichens and other ascomycetes. Stahl's and Baur's figures of septate trichogynes with attached spermatia are familiar to every one. One recalls Thaxter's many beautiful illustrations of trichogynes and spermatia of species of the Laboulbeniales. For a number of years only the followers of Brefeld and Dangeard doubted that trichogynes of these ascomycetes function to receive the male nuclei from spermatia and antheridia, especially as Harper had proved the case cytologically for *Pyronema.*

We must have female and male chlamydospores in heterothallic *Ascobolus carbonarius.* The female, borne on shorter thicker stalks, germinates to produce a complicated ascogonial coil ending in a septate trichogyne which contacts the male, raised on a long thin stalk and serving as an antheridium. The idea that "male" nuclei could ever reach the ascogenous cells when they would have to break through twenty or more cross-walls has never been considered very seriously, to say the least. Atkinson held that all such septate outgrowths from ascogonia represent simply a progressive sterilization. No one, he said, has ever proved that such outgrowths could possibly function. It will be shown later that cross-walls need offer little hindrance to passage of nuclei.

In *Ascobolus magnificus* the ascogonium tapers gradually into a septate receptive end which seeks out the antheridium arising nearby. In this heterothallic species sex organs are not produced when races from single ascospores are grown alone. Yet as soon as two mycelia of opposite sex-reaction mix in a culture, anastomoses occur and antheridia and ascogonia appear, but always on different hyphal branches. It is in just such species that one should look for a close relation between the morphological differentiation of the reproductive structures and heterothallic nuclear sex-reactions.

As botanists continued to study ascomycetes culturally and no one demonstrated that spermatia actually functioned as male elements, it again became customary to refer to them as microspores or microconidia, especially as reports came in now and then of persons claiming to have germinated them. If not all spermatia germinate neither do the ascospores of all species germinate in cultures. Drayton, however, brings us back into line by demonstrating that the microconidia of *Sclerotinia Gladioli* do function in fertilization. Then we find the microspores of *Neurospora, Bombardia* and *Pleurage* doing the same thing. It is now generally accepted, and it would seem accepted again on the basis of *function,* that spermatia of all ascomycetes are male organs and ascogonia with their trichogynes are female.

The ascogonium of normal *Neurospora tetrasperma* does not differentiate a trichogyne. Both Colson and Schönefeldt have established this point cytologically. Each cell contains two sexually different kinds of nuclei from the beginning. It is only in its unisexual condition that trichogynes are formed, and then only after the incipient perithecium has reached some size. Furthermore, receptive hyphae often develop early from the base of the ascogonium to usurp the function of the morphological trichogynes, if there ever are such here. Trichogynes should develop whether they are needed or not if they are definite units.

There are still other features that should give occasion for thought. Spermatia of *Neurospora* are often borne on specialized compound spermatiophores, each of whose cells must be a spermogonium. The spermatia are pushed out through a very definite and characteristic cup-like aperture just as they are in *Pleurage anserina.* There is no question that they are highly differentiated organs morphologically and functionally, although in *Sclerotinia* spermatia may also bud out like a yeast directly from ascospores, conidia and hyphal cells. When spermatia are applied to unisexual incipient perithecia of *Neurospora, Bombardia* and *Pleurage* fertilization follows. Spermatia must be "male."

In *Neurospora* the most characteristic feature is the production of vast quantities of monilioid conidia. If you apply conidia of one reaction type to incipient perithecia of the opposite type they will also effect fertilization. They are clearly differentiated morphologically and functionally from the female ascogonial coils. In sexual reproduction they are enhancing, they are advantageous, and they are even more efficient than the spermatia themselves. Therefore, according to your definitions they must be male sex organs, so that we have two kinds of spermatia, microspermatia and macrospermatia. But one says these monilioid conidia germinate to propagate the species asexually. So do the microspermatia germinate, 100 per cent. if you give them sufficient time and the right environment. The mycelia they produce do not differ at all from those derived from the monilioid conidia or from the original parent ascospores.

Proof that spermatia are altogether dispensable is easily obtained from a study of plate cultures of the facultatively heterothallic species of *Neurospora* and *Gelasinospora.* None of our races of *G. tetrasperma* is known to produce spermatia, but all of them do produce incipient perithecia. Therefore, all races must be female, that is if incipient perithecia are marks of femaleness. It is certainly very difficult to find ascogonia in some cases, however. Hüttig would probably call such primordia male if no ascogonia could be discovered. There are no male races and no hermaphroditic races, however, according to our definition. Fertilization must be accomplished some other way than by spermatization. In their reactions in sexual reproduction all these facultatively heterothallic races fall into two groups. If two races which are opposite in their sex reactions are grown from opposite sides of a plate culture, the fruit bodies tend to be distributed in a rather definite pattern on one side of the plate. Under proper cultural conditions the mycelium from one side does not invade the other side to any great extent. It has been proved for these species that, following hyphal fusions at the line of contact, nuclei from one mycelium migrate through cell after cell to the opposite side, so that a mycelium that originally contained only one kind of nuclei will become heterokaryotic, bisexual.

In heterothallic species of *Spirogyra* and *Zygnema* it is generally agreed now that where the contents of a cell from one filament pass out through the conjugating tube over into a cell of the opposite filament in which the zygospore is formed, there is a sexual differentiation, a difference in activity. One cell is male, the other is female. According to this criterion our race Gel. 1 must be male because its nuclei move out from Gel. 1 over into Gel. 11. Buller has shown that in the hymenomycetes nuclear migration is possible because of the pores left in the cross-walls as the result of the ring type of septum formation known to be characteristic of these fungi. Lehfeldt's idea that the cross-walls break down is probably incorrect.

Strain S_1 of *Neurospora tetrasperma* is male and S_9 is female if direction of moving nuclei is a basis for distinguishing the sexes. Certainly, most students will agree that the mycelia on which fruit bodies are formed must be female. But if you use a character for determining sex like that originally used by Blakeslee, then our tester race, S_1, which produces orange-colored conidia in much greater abundance than does tester race S_9, must be female instead of male. The point for thought here is that morphological differentiations of vegetative characters, differences in rates and directions of movements in nuclear migrations and the final location of fruit bodies are not true bases for distinguishing males and females, although according to your definitions they would be.

Neurospora Toroi is a species which comes from Puerto Rico. It manifests the same general type of perithecium distribution pattern, and its heterothallic races react strongly against corresponding races of *N. tetrasperma.* These two forms of *Neurospora* have about the same morphological characteristics so that it would be difficult to tell them apart. The best evidence that they are two distinct species is that when they are mated the F_1 asci are seldom formed. Similar sterilities are always found in other interspecific hybrids of this genus.

To speak of the reactions of these races as sexual is not as inconsistent or as erroneous or as absurd as it is to use the terms male and female the way we do in these low plants, merely on the basis of very weak analogies. As others have said, we can not be consistent and we do not need to be where sex, as we call it, has probably arisen many different ways. To insist that these reactions are merely incompatibility relations is misleading because here they are nuclear. To say that this is what incompatibility should mean is begging the question because no plant breeder or horticulturist uses the term that way. We may illustrate the confusion that would arise from the use of this term by comparing certain interspecific reactions. We have three obligately heterothallic species of *Neurospora.* If we find that race A of *N. intermedia* from China produces fertile fruit bodies when grown with race B of *N. sitophila* from France, and this race B also produces fertile perithecia when mated with race C of *N. crassa* from Louisiana, and if we express these relations in terms of incompatibilities, we can not know in advance of actually proving the case by culture tests what will be the reaction between race A and race C. But if we say A and B are of opposite sex because they produce perithecia when mated, and race B is opposite to race C for the same reason, every one knows that races A and C are alike sexually so that no fertile perithecia need be expected when they are grown together.

It would be difficult to prove, but we should like to know whether in the dioecious red algae there is ever a case where the male plants of the species can be divided into two groups so that all the individuals in one group are cross-sterile or incompatible with the individuals of a corresponding group of female plants, while the plants of the second male group are compatible with these same females. In dioecious Bryophyta no such bipolar intersterilities have ever been reported. Furthermore, no case is known in *Sphaerocarpos,* Professor Allen informs me, where otherwise normal plants of a male clone are cross-sterile with normal plants of a female clone.

It is curious that certain groups of these fungi, otherwise so irregular and primitive, should have worked out so early the most perfect of all devices to prevent self-fertilization, that is, the differentiation of their nuclei genetically into two different kinds with respect to their reactions in sexual reproduction. One wonders how it happens to be that this very striking bipolar interaction should exist particularly in rusts and ascomycetes where differentiation of the sexes morphologically has not become stabilized and where, if I may be allowed to express it that way, all sorts of devices are being tried out to effect fertilization.

Morphologically speaking, I would be the last to deny that heterothallic races of *Neurospora,* for example, are potentially bisexual or hermaphroditic, with enhancing yet dispensable trichogynes and spermatia. Such races are, nevertheless, fundamentally and indispensably unisexual for purposes best adapted to evolution.

There are several other terms such as sporophyte, diploid, diploidization, hybrid and hybridization that are frequently used very loosely and inconsistently when applied to the fungi. If any one is able to work out a terminology that will more exactly express the situation in these fungi instead of adding to the confusion with glittering generalities we shall all be grateful. In the meantime, if we must continue to be inconsistent, let us be inconsistent where it will serve a useful purpose.

Hüttig very cleverly avoids the dilemma by boldly asserting, without much evidence, that all his self-fertile races of *Glomerella Lycopersici* are hermaphroditic and all self-sterile races are male, and goes on with his interesting breeding work just as though he were dealing with strictly heterothallic species. He has three linked factors, the first two of which, α and γ, are "realizators" and control sex-reactions. The third factor β is physiological and influences copulation. This author seems to be unaware of Edgerton's prior work on *Glomerella.* He thinks he has in this species evidence in support of the theory of relative sexuality. Relative, however, refers to relative degrees of fertility and not to relative morphological differentiations of the organs themselves. May we not nevertheless accept the theory of relative sexuality as well as the idea of incompatibility for what they are worth in these fungi and not let terminology interfere with productive research.

The first fertile hybrid ascomycetes developed artificially were obtained about ten years ago by crossing a four-spored species of *Neurospora* with an 8-spored species. They are the only interspecific hybrid ascomycetes that have been produced, yet either because the F_1 asci did not show six spores, the number intermediate between 4 and 8, or because races obtained by germinating the f_1 spores were not pure for either 4-sporedness or 8-sporedness, that work has been entirely ignored. Not so when a comparatively simple and easy cross between an albinistic non-conidial mutant race and the normal orange-colored race of the 8-spored species was reported. Of the eight f_1 spores from an ascus four gave rise to albinistic non-conidial mycelia and four to orange-colored, a perfect Mendelian segregation. This is what we wanted to see, if the same rules of inheritance govern the fungi and the higher plants and animals. The ignored interspecific hybrids are more interesting and important, however.

To illustrate how beautifully some of these ascomycetes are adapted to demonstrate Mendelian segregation one needs only to culture *Bombardia lunata,* according to Zickler. Here the characters *viridis* and *rubiginosa* are expressed not only in connection with the mycelia but also by the ascospores while they are yet within the ascus, four of each kind, variously arranged accordingly as segregation for these factors occurs in the first or in the second division. Culturing from the ascospores it was proved that these factors are linked with the sex-reaction factors.

Sectoring which shows variation in the type of mycelial growth and sporulation has usually been interpreted to represent a mutation if the variant remains constant through vegetative propagation. If such mutants could be carried through sexual reproduction, however, we must expect an occasional reversion to normal. This could be proved by analyzing the eight spores from individual asci. In some species there would be a characteristic arrangement of the spores carrying the factors concerned.

Zickler has recently proved that mutants arising as sectors in plate cultures of *Bombardia* are genetic by analyzing progeny obtained by crossing the variants with the normal wild type. In some cases there was the tendency to revert to normal, which was easily seen because in the races *viridis, rubiginosa* and *lactea* the ascospores themselves reflect by their color their genetic constitution. He observed a 2 : 6 arrangement of spores, an arrangement which Lindegren and Wülker have also interpreted to represent reversions in cases of mutant strains of *Neurospora* which they have studied.

Not all sectoring need denote mutation. Albino spermatial strains of *Pleurage anserina,* which had first arisen as sectors from very dark-colored non-spermatial races, have themselves sectored to show differences in the abundance of spermatia. Breeding tests indicate that the phenomenon here may be in part merely phenotypic, at least the subsectoring of the albino was not due to a mutation.

If one crosses an albino non-conidial race of *Neurospora sitophila* with a conidial race of *N. crassa* and then back-crosses two or three times he will obtain asci in which all eight spores mature. One usually finds a clear-cut segregation, four non-conidial and four conidial. Sometimes, however, two of the albinos develop a few conidia (something that does not occur if you make the cross between conidial and non-conidial races of the same species). In other cases four of the albinos will show a few conidia. Geneticists would no doubt interpret the phenomenon as a partial reversion, or a reversion of one of the factors concerned in conidium production.

We have developed as the result of x-ray treatment races the asci of which frequently abort without spore formation. In one case the abortion is apparently due to an incompletely dominant lethal gene. The nuclear fusions occur as usual, but after three or four successive nuclear divisions the ascus contents degenerate. The ascus wall then takes over the differentiations characteristic of the ascospores themselves, and persist as striated olivaceous-brown sacs. All asci in every perithecium are heterozygous for this lethal. Some asci succeed in cutting out spores, but spores carrying only lethal nuclei die after germination. Asci homozygous for the lethal can not be obtained. It is interesting to see that when a lethal nucleus is associated with a normal one in the same cytoplasm, it divides continuously and is carried along until it fuses in the ascus with a normal nucleus.

The type of ascus abortion just described is genetic, but a similar though non-heritable type can be induced both by chemical treatment and as the result of certain abnormal cultural conditions.

We have another type of ascus abortion which occurs when asci are homozygous for a recessive lethal gene. Here no ascospores are ever cut out and the asci finally disintegrate. Asci heterozygous for the lethal form spores normally, but each spore will be provided with one lethal and one normal nucleus. Mention was previously made of relative amounts and coloration of masses of conidia produced by unisexual races of *Neurospora tetrasperma.* The presence of this recessive lethal l, in race S_1 for example, so inhibits the factor 0 that the S_1 race looks like the S_9 race, although the sex-reactions are not at all affected by the lethal for ascus abortion.

In the 8-spored species of *Neurospora* the nuclear spindles during reduction division in the ascus are so oriented that segregation of any pair of factors in the first division is manifested by the arrangement of the spores in the ascus, 4 and 4, for these characters. If the segregation occurs in the second division this is manifested by the alternation of the spores 2 and 2, or 2, 4, 2. Should two or more pairs of factors be involved and one pair segregate in the first division and another pair in the second division, then we shall have four kinds of spores formed in an ascus.

Bridges has suggested that where in any case four kinds of spores or gametes are formed from a mother-cell following reduction division this could be due to crossing-over. Lindegren analyzed over 100 asci obtained in crosses involving two pairs of linked factors, + / − for sex-reaction and P/p for mycelial characters. He obtained about 22½ per cent. recombinations. He also found that about 13 per cent. of the asci showed segregation for the + / −, and 33 per cent. for the P/p factors in the second division. If a second division segregation indicates a simple cross-over, Lindegren argues, second division percentages could be used to compute chromosome map distances. Map distance units obtained through actual crossing-over percentages (indicated by second division percentages according to Lindegren) are always twice as large as standard units computed on the basis of recombination percentages. Since the distances between + / − and P/p computed these two ways independently agree (22.5 : 23) Lindegren believes he has proved his assumption which was that the spindle fiber attachment points (S. F. P.) always reduce in the first division just as they do in *Drosophila.* Post-reduction, meaning separation of non-sister chromatids in the second division, does not occur in *Neurospora.* Such logic is correct only if second division segregations can not be explained in any other way.

Zickler, analyzing over 8,000 asci of *Bombardia lunata,* finds that crossing-over does not account fully for the number of second division segregations of his linked factors. One must assume, he says, that any two of the four chromatids may move back together during the first division, a pair to each pole, so that two thirds of the time there could be reduction of the S. F. A. in the second division.

Wülker takes a middle ground view to explain his second division segregations. His results (180 asci analyzed) prove, he thinks, that there must be a side by side working of both crossing-over and post-reduction. It is clear that further work along lines first suggested by Lindegren is necessary to settle this question, which it seems is a very important one from a genetic standpoint. It will be difficult to settle it cytologically in case of *Neurospora.*

In *Peziza subumbrina,* which Matsuura and Gondô studied, there is regularly found at reduction a pair of heterochromosomes, one a long rod, the other a small spherical body, suggesting that the species may be heterothallic and that this pair of chromosomes may be the mechanism governing the sex-reactions. It is interesting that the authors sometimes find this pair of chromosomes splitting equationally in the first division, non-sister chromatids, one long and the other short, going back to each pole. They believe that the other chromosome pairs may also split equationally in the first and reduce in the second division, but this can not be determined cytologically because of their small size.

Whether or not Lindegren's views that the S. F. A. always reduce in the first division will apply to these ascomycetes, he has certainly given us an excellent set of models for recording and interpreting our culture results genetically.

Glomerella Lycopersici, it would seem, must be a type intermediate between the strictly heterothallic and the homothallic species. Hüttig isolated several kinds of haplonts which vary in their degree of fertility ranging from strongly self-fertile hermaphroditic clones to those that are wholly "male" and completely self-sterile. The results upon which he bases his conclusions were obtained by analyzing ascospores collected at random from cultures in which his self-fertile and his so-called self-sterile or "male" races were mixed. Any one who knows these ever-mutating Glomerellas realizes that such results can have little genetic significance. The mechanism by which fertilization occurs must be unique, because, as is well known, races heretofore self-sterile may become self-fertile when grown against another race, merely because of the altered nutritional conditions. If, however, Hüttig will analyze the eight spores from individual asci and obtain comparable results he will have proved *Glomerella* to be one of the most interesting ascomycetes yet studied genetically.

Mycologists who undertake the study of ascomycetes or other fungi genetically will always be handicapped by their lack of that training which is necessary for adequate analysis of any extensive breeding work. Our proper function is to remain mycologists. We must discover for the geneticist species which are well adapted not only morphologically but culturally for their type of work. We should develop the culture technique and work out the morphology and the details relating to propagation and reproduction, and prove by our preliminary hybridization work that there is need for a further genetic study of the species. On the other hand, geneticists without mycological training undertaking to study the ascomycetes should realize that the ways of these fungi are devious and beset with pitfalls. Nevertheless, it is encouraging to know that fundamentally in their reproduction and inheritance the fungi follow exactly the same laws that govern these activities in the higher plants and animals.

A GREAT PUPIL AND A GREAT DISCOVERY—BOTH SUPPORTED BY A GREAT TEACHER

By Dr. HARRY N. HOLMES

OBERLIN COLLEGE

ON this February 23d, it will be exactly half a century since an Oberlin College student, young Charles Martin Hall, gave aluminum to the world as an industrial metal.

This brilliant discovery of the present industrial process of making aluminum was no accident but the result of long-continued effort and intelligent planning—in cooperation with one of the best-trained teachers of chemistry in America, Frank Fanning Jewett.

Hall at 22 succeeded where many of the greatest scientists of his century failed. It is true that Oersted, the Dane, famed for his fundamental research in electricity, was the first to isolate aluminum (1825) and that Wöhler, the German, dominant authority in chemistry, improved slightly upon Oersted's method (1827), wrongly receiving world credit for the discovery; but these pioneers saw only a little black powder instead of shining, massive aluminum. The scientific world was thrilled, but industry was not greatly benefited.

The following equation represents the method by which this metal was isolated:

$$AlCl_3 + 3K \rightarrow 3KCl + Al.$$

The eminent French chemist, Deville, complaining that aluminum was still slightly more expensive than silver, lowered the cost in 1854 by the simple substitution of the cheaper metal sodium for potassium in attack on a mixture of aluminum chloride and sodium chloride.

$$AlCl_3 + 3Na \rightarrow 3NaCl + Al.$$

Within two years, by 1856, the price of the metal dropped from $90.00 per pound to $27.00.

Sir Humphry Davy made earlier attempts than Wöhler's to reduce the oxide and failed, as did Silliman. Berzelius, the eminent Swedish chemist, almost succeeded in anticipating the success of Wöhler when he heated cryolite, the double fluoride of aluminum and sodium, with potassium. Unfortunately, he used an excess of potassium and got an alloy of aluminum with potassium. Had he used an excess of cryolite, Berzelius would now be given credit for presenting aluminum to science. Deville actually gave them all a start on the right track in another method that failed. He electrolyzed melted cryolite, a double fluoride of aluminum and sodium, but the results were unsatisfactory. Bunsen is said to have done the same thing at the same time. Half a century later these unsuccessful experiments were dragged into court in an effort to deny Charles M. Hall the fruits of his own great discovery.

Deville worked hard to cheapen the cost of the necessary sodium for his reduction process and actually founded a small industry. Heating sodium carbonate (and some calcium carbonate) with carbon he secured metallic sodium in commercial quantities.

Then came Castner, who, with admirable clearness, saw that cheaper sodium meant cheaper aluminum. With equally admirable directness he proceeded to devise a cheaper process of making sodium and at once cut the cost of aluminum to the encouraging figure of $6.00 (later, $4.00) a pound. Then, just at the moment of reasonable success, poor Castner was flattened by the news of young Hall's great discovery!

Now, in 1880, enters upon the scene a quiet, studious lad, Charles Martin Hall, son of a minister in the village of Oberlin, Ohio. Dreaming of his "schemes" to make great discoveries for humanity, actually making a real invention at 17 and finding it already patented, reading in a dog-eared old chemistry of the properties of aluminum, he was ready to enter Oberlin College in 1880.

It was most fortunate for Hall and for Oberlin College that in 1880 Frank Fanning Jewett accepted the chair of chemistry and mineralogy, bringing to the work a training equal to the best of that time. A Yale graduate, he had gone to Germany, where, in Göttingen University, he was one of the small group of American students who at that time specialized in chemical work under highly trained German teachers. Among his close friends at Göttingen were Provost Edgar F. Smith, of the University of Pennsylvania,