employ a large proportion of scientific experts are all on a gigantic scale is quite mistaken. Even those which are on a gigantic scale were small once; they have become large through applying science. Some small works in this country are highly scientific; some very large ones are exactly the opposite. The chief cause of manufacturing inertia is the mentality of British business men, which is essentially practical and distrustful of ideas. But the shock of war has undoubtedly disturbed them, and there is some prospect of a change. It is essential to success, as the committee admit. "We recognize that unless the generality of British firms can be induced to alter their present attitude we shall have failed profoundly in one of our appointed tasks." Research has hitherto offered no career for able and enterprising young men in this country. So they have not gone in for it, and when a manufacturer did want a man he had to go abroad for him. It was a vicious circle. But we believe that in the new prospect now opening up the committee are right in advocating the policy of increasing the supply of men. The demand will follow.—*London Times.*

SCIENTIFIC BOOKS

The Mechanism of Mendelian Heredity. By T. H. MORGAN, A. H. STURTEVANT, H. J. MULLER and C. B. BRIDGES. Henry Holt and Company, New York. 1915.

Students of genetics some six years ago learned with lively interest that Professor Morgan had discovered in the fly *Drosophila ampelophila* an example of inheritance parallel to that seen in the well-known descent of color-blindness in man. Substituting red eye and white eye in the fly for normal color vision and color-blindness respectively in man the phenomena were exactly similar. Hitherto no such case in an animal available for experiment had been known. We were aware of several instances, notably that of the moth, *Abraxas grossulariata,* the pigmentation of the silky fowl, and certain others in poultry, canaries and pigeons, in which analogous descents had been traced; but in all these the parts played by the sexes were reversed. From this evidence indeed it had been proved that in the moth and the birds the unfertilized *eggs* are differentiated into two classes, those destined to become females and those destined to become males. Obviously enough it would be inferred from the descent of color-blindness that in man the *sperm* was similarly thus differentiated into two such classes, destined to form females and males respectively, a phenomenon which Wilson and others had cytologically demonstrated in various insects. At this point the matter rested.

With the discovery of the peculiarities of *Drosophila* genetic research has passed into a new phase. The animal breeds rapidly, going through many generations in a year. It is inexpensive to breed, and the families consist of numbers which, relatively to those attainable in most subjects, are enormous. Since it first attracted Professor Morgan's attention it has been found to produce a long and intricate series of factorial varieties, or "mutations" as the authors prefer to call them, differing in the color of eyes and body, the sizes and shapes of the wings, and other respects, the number of these differences being now computed at more than a hundred. Professor Morgan and a band of enthusiastic colleagues set themselves with the utmost zeal to analyze the inter-relations of this mass of factors. Half a million flies have been bred, with the result that the data respecting the genetics of *Drosophila* in quantity now surpass those obtained from any other animal or plant. The advances made are on any estimate many and of quite exceptional significance. That much is certain. If we go further, and accept the whole scheme of interpretation without reserve we are provided with a complete theory of heredity, so far as proximate phenomena are concerned.

We may perhaps best approach the subject by reference to a class of facts with which all investigators are now familiar. Of the factorial differences detected in *Drosophila,* many of the more important were soon shown to be sex-limited, as we used to call it, the "limitation" being to males, just as in color-blindness and some other sex limited affections in man. From an analysis of the descents of these characters Morgan concludes that such limitation is in reality only a special case of that complete or partial association of factors in their parental combinations which was first recognized as coupling and repulsion. These phenomena may in fact be all one. They are examples of linkage between factors, the second factor involved in the case of sex-limitation being that for sex. The fundamental identity of these linkage-phenomena had naturally been suspected. Difficulty, however, lay in the peculiarity of sex-limitation, that in it the linkage has never been observed to be other than complete. The new theory, as will be seen, represents this distinction in a simple and readily conceivable way, so that we are at once attracted. It may be remarked that linkage is no mere incident of technical genetics. We can readily perceive that it must play a great part in the control of heredity. Close resemblances of offspring to parents and grandparents in features and other attributes are common even in families of mixed races like our own. Such resemblances must depend on the coexistence of multitudes of factors, and could scarcely ever be perceptible if the factors were really distributed at random among the germ-cells. The theory provides a mechanism by which their associations may be governed.

From the beginning it was tempting to interpret the processes witnessed in the maturation of the germ-cells as the visible means by which factors are segregated. Cytologists have shown that when the chromosomes are formed anew from the rested nucleus their number and on the whole their forms are constant for the species. They may thus be regarded as having a permanence or individuality. Further, they consist of pairs, one of each pair doubtless representing the material contributed by each parent, the two contributions having retained their identity through all the divisions and changes which have happened since the original fertilization.

If, therefore, the number of genetic factors were never greater than the gametic or haploid number of chromosomes, we should obviously conclude that each chromosome carried one factor, and the ordinary distribution of factors would be produced by a random allocation of one chromosome from each pair to the set comprised in each gamete. But we know that the number of genetic factors in various types of life greatly exceeds the gametic number of chromosomes and consequently this simple account was discarded as insufficient. At this point we meet the first of the far-reaching suggestions which Morgan offers, namely that all the factors are linked together in groups, and that the number of the independent groups is that of the haploid chromosomes. This number in *Drosophila* is four, and it is claimed that, on genetic analysis, the various factors of *Drosophila* can be proved to be so interrelated as to constitute four linked groups and no more. Before wholly accepting a proposition of such magnitude we naturally entertain a provisional reserve, but it may be at once admitted that all the evidence available is capable of this construction. Among the animals and plants already studied are many in which the factors, apparently subject to no linkage, in number far exceed that of the haploid chromosomes, but Morgan is able to reply with force that the possibility of linkage in these cases has not been exhaustively investigated. Tests of the heterozygotes by breeding with double recessives on a considerable scale provide the only really sufficient method of detecting linkages. Such work (especially in plants) is commonly very laborious and has rarely been carried out. Thus, though the presumption would *a priori* seem to be rather against the view that linkage will be found so abundantly operating even in the familiar examples, the speculation is quite legitimate. That it is extraordinarily promising as offering at least a chance of positive progress must be obvious to all.

But if the factors enter the offspring in linked groups—the chromosomes of each pair representing severally the parental combinations—the formation of new combinations inside any one group must mean that there has been an interchange or "crossing-over" between the two homologous chromosomes. We know that such new combinations can be formed. Gametes bearing them are produced in all cases in which the coupling or the repulsion—to use the older terms—is not complete. To account for the crossing-over of factors from one chromosome to its mate Morgan appeals to certain phenomena of twisting and interlacing of chromosomes in synapsis, first made prominent by Janssens, who observed them in Amphibia. It is suggested that in the course of this process of twisting the chromosomes may anastomose and again break, exchanging parts of their substance. For those unversed in practical cytology it is not quite easy to judge how far this hypothesis is in accord with observed fact. That twisting takes place in many types, especially Amphibia, is clear; but neither the figures reproduced from Janssens nor the originals from which they are taken—still less the very fragmentary observations of both Stevens and Metz from *Drosophila*—provide more than a slender support for this most critical step in the argument. It is to be hoped that the authors will before long tell us exactly upon what evidence they are here relying.

The formation, then, inside a linked group, of factorial combinations other than those which entered from the parents, is ascribed to crossing-over from one chromosome to its fellow or mate. At an early stage in the work, the curious and very significant fact was observed that in the male no such crossing over took place in regard to the various factors which had been proved to be *sex-linked*. The cytological interpretation of this discovery was ready to hand. In many forms, especially insects, the sperms have been proved to be of two kinds, those possessing an X chromosome, destined to form females, and those without this chromosome, destined to form males. If therefore the X chromosome carries the sex-linked factors—a supposition inevitable inasmuch as these factors are all destined to go into the daughters—and if there is no real mate to the X chromosome, evidently there can be no interchange or crossing-over here. Therefore in the case of sex-limited characters linkage is complete.

On tracing the growth of the theory or group of theories which have been built up on the *Drosophila* evidence the consideration just propounded stands out as the original foundation-stone. It was so introduced in the chief inaugural paper of the series. This "sex chromosome in the male has no mate," Morgan tells us, and consequently no interchange with it takes place.[1]

On reference, however, to the work of Miss Stevens (1908) whose paper is given as authority for the mateless condition of the X chromosome in *Drosophila ampelophila,* we read that she found extreme difficulty in studying the cytology of this creature, but ultimately satisfied herself that there is an unequal pair. The more recent cytological work of Metz relates entirely to the female, but in a note on the male he remarks

> so far as my observations go, they indicate an unequal XY pair in the male, without any additional piece attached to either. Neither my observations nor those of Miss Stevens are conclusive, however, owing to the difficulty of observing the chromosomes in these stages. The question is important for the bearing it has upon the breeding experiments with this fly, and we are doubly unfortunate in being thus far unable to settle it.[2]

In 1913, Sturtevant in introducing the first formal development of the theory of linear arrangement, presently to be considered, repeats that there is no crossing over among the sex-linked group of factors in the male, "since the male has only one sex-chromosome."[3] When we come to the book of 1915 the same authors have an entirely different conception of the cytological phenomena. There are two sex chromosomes in the male, and though as a matter of convention, one of them is represented as different from the other in shape, the reader is very properly told that the distinction has not yet been observed.[4]

1 *J. Exp. Zool.*, 1911, XI., p. 383.

2 *J. Exp. Zool.*, 1914, XVII., p. 49, *note.*

3 Sturtevant, *J. Exp. Zool.*, 1913, p. 44.

4 In the recent paper of Bridges (*Genetics,* I., 1916) the distinction in shape is stated to be a reality.

Without insisting too much on the point, we can not avoid noticing that this complex web of theory is so exceedingly elastic as to be capable of being fitted to a framework of cytological fact, the converse of that for which it was designed. Still, as some animals are found to have no second heterochromosome the suggestion that such a body, when present, may be inoperative might be offered in extenuation.

Presently we meet, however, a fact which is much more difficult to harmonize with the theory, though constituting one of the most novel and remarkable of the discoveries made in the *Drosophila* work. Not only do the sex-linked factors show no crossing over in the male, but experimental breeding shows that in the male there is no crossing over even of the factors composing the other groups. *Crossing over, in fact, in Drosophila, turns out to be exclusively a phenomenon of the germ cells of females.* This is a genetic discovery of the first magnitude, whatever its ultimate significance, but the cytological interpretation of crossing over must now bear a very considerable strain: for, on the one hand, though the absence of crossing over in the sex-linked characters had fitted well with the belief that the sex-chromosome in the male was unpaired, this chromosome is now admitted to be paired; and on the other hand the characters ascribed to the chromosomes known to be paired turn out to be equally unable to cross over in the male. It is with some surprise that we find neither in the book nor in the material previously published any coherent discussion of the difficulties thus created. If further cytological work shows that the chromosomes of the female twist and anastomose, but that those of the male do not, the chromosomal theories of heredity will receive a very remarkable support. Meanwhile on this part of the subject there is little more to be said.

Recombination then within the limits of a linked group is regarded as a consequence of crossing over, or the interchange of parts between one chromosome and its mate or homologue. This conception, whether well- or ill-founded, has led on to a further and very remarkable speculation. If the factors are carried by the material of the chromosomes, what more likely than that they, or rather the particles severally bearing them, should be arranged in a row, like a string of beads, along the length of the chromosome? The proportion of cross-over gametes might thus give an indication of the actual relative positions of the factors along the chromosome. On this inspiration, the intertwining of two strings of beads providing always the mechanical analogy, the numbers in the experimental families have been carefully studied. The percentage of crossovers is taken to indicate the position of the factors. Where there is no linkage, this percentage is, of course, 50, all combinations occurring in equal numbers. But if two factors AB show 50 per cent. crossing over and both A and B can severally be proved to be coupled to a third factor C, then all three may in reality be members of one linked group, and the fact that in the case of one pair there is 50 per cent. of crossing over may be a consequence of the relative positions of these factors in a linear series. The amount of crossing over can thus be interpreted as an indication of the relative positions of each factor in such a series. Upon this follows the great thesis of the book: that this series is in fact a row of points along each of the four chromosomes, and that the redistributions or recombination of characters can be correctly represented by strings of beads which twist together in pairs, breaking and joining each other at nodes. Whether this conception is sound or not, we accept it as a gallant attempt to move on. No other of equal promise has been offered and we must observe its development with cordiality and respect.

Confronted with a theory of so much novelty and importance, the reader's first desire is to examine the details of the evidence from which it has been deduced. A serious charge lies against the book inasmuch as the material for such an examination is not contained in it. We are provided with a sketch—a vigorous and impressionist sketch—of the facts as the authors see them, but we want a much nearer view. Pending this, judgment must be suspended. We are told that the breeding numbers prove the factors to be in four linked groups. We would like to take each one separately and follow the proof regarding its linkages. As yet there is no means of doing this. Of the evidence the book avowedly gives illustrative specimens merely, and even the long array of *Drosophila* papers leaves great gaps unfilled. Take the first or sex-linked series. The book tells us that more than 40 factors have been located in it and arranged in order. Respecting the great majority we have no details at all and as to most of the remainder very few. There are, however, six that we can examine in the light of the data summarized by Sturtevant in *Zeits. f. Vererbungsl.*, 1914, the last considerable body of evidence to hand.

The factors concerned, called Y, W, V, M, R, Br, are represented as arranged along the chromosome in such a way that two, Y and W, are at the zero end, two more, V and M, near together at 33.5 and 36.5, and the remaining two, R and Br, also near together at 53.3 and 57.7. The numbers indicate that the members of each set of two are closely linked, for with fair consistency the breeding ratios are those characteristic of close "coupling," namely, nAB: 1Ab: IaB: $n$ab, and of "repulsion" in the form IAB: nAb: naB: 1ab, the value of n being much greater than 1. The relations of Y and W to V and M are also of this kind, the coupling being of course less close. But taking Y with R, W with R, V with R, or M with R, we meet numbers of a very different order, and it is not clear by what system they have been interpreted. For instance, we find the following extraordinary series given,

for *Y* with *R*				
as *repulsion*	342	58	466	19
as *coupling*	235	50	194	56
for *W* with *R*				
as *repulsion*	567	143	697	91
as *coupling*	294	61	175	108
for *V* with *R*				
as *repulsion*	147	147	520	36
for *M* with *R*				
as *repulsion*	430	795	1,716	189
as *coupling*	4,189	93	850	1,033

The numbers in which the new combinations come are then added in each case and set out as percentages of the totals, these percentages being taken as indications of the linear distances between the loci in which the factors are presumed to be. To those accustomed to series of this class, these numbers are so aberrant as scarcely to suggest *prima facie* that they represent Mendelian series at all, and it seems at least improbable that they can be used to calculate percentages comparable with those obtained from the various comparatively normal series by which for instance Y and M, V and Bz,[5] Y and W, or W and M are interrelated. Throughout the experiments indications of differential viability recur, largely masking the true proportions of the classes, but as has been remarked by the authors in reference to certain special cases, the incidence of this differentiation is so irregular that allowance can not be made for it in any consistent fashion. Meanwhile the data look so intractable that a doubt has sometimes arisen whether the account here given may not be a consequence of some radical misunderstanding of the author's meaning.

One is tempted further to ask whether all parts of the several proofs are really independent of each other. In the present state of the evidence only the authors themselves can positively answer this question. They declare that all the factors are proved to be disposed in four separate systems of linkage, but the argument that they are thus arranged contemplates a very great variety of possibilities not obviously included in this scheme. For example, the fact that two pairs of gens or factors give 50 per cent. of cross-overs might in the authors' view be a consequence of the location of the two pairs in distinct chromosomes. It may equally be a consequence of the two being in the same chromosome but at the terminal and central positions respectively. It may also

[5] The numbers given for V and Br by Sturtevant are misprinted, 260 standing for 2,660. Thus emended they are fairly normal. The worst examples all involve *R*, and it might be suspected that this was a source of special difficulty, but analogous numerical abnormalities occur also in the ''second chromosome'' series, nor can any hypothesis of differential viability be readily applied to such figures as those quoted above.

be produced by double or triple crossing over, and in other ways also. Moreover, granting that the factors seem to be related to each other in four systems of linkages, it must next be proved that there is no linkage between members of distinct systems. The evidence of such independence is admittedly meager, and indeed as to the behavior of the factors comprised in the third system we have been told very little at all.

The machinery for dealing with unconformable cases is extraordinarily complete. Besides differential viability we hear of some twelve lethal factors by whose operation certain classes may be extinguished; changes in output with age; a special phenomenon spoken of as "interference" inside single chromosomes; some interaction between chromosomes; even of a factor modifying the normal amount of crossing over, and lastly of an altogether distinct kind of crossing over in the four-strand stage. Can the action of all these processes be severally traced? Can their consequences be distinguished from each other, and especially from those of multiple crossing over? There remain, of course, also the various slips to which all experimental work is liable, such as in this case errors from the overlapping of generations—several times alluded to as a real danger—and others similar which no doubt have been obviated more or less with the improvement in technique. Apart from obscurities of this more superficial kind, is it clear that the series of alternative hypotheses is capable of ultimate analysis? As has been already said, the authors may be able to make such an analysis, but they have not yet offered it to the reader in irrefragable form. Meanwhile the suspicion is unavoidable that, given a conviction that the factors *must* be arranged in rows along four chromosomes, the various interpretations provide rather a method, or perhaps we should say alternative methods, by which the facts can be reconciled with the hypothesis, than a proof that this hypothesis is correct.

Ever since the discovery of systems of linkage it has not been in dispute that several factors, perhaps all, are arranged in some ordinal system or systems. We are dealing with phenomena of *linkage*. The hypothesis of reduplication was offered as one way in which the processes could be logically represented, at least in plants. It is admittedly a very crude conjecture, but it has the merit of being noncommittal and applicable to units of various magnitudes. So much may be remarked in parenthesis; but the critical point now is whether in the various forms of life the number of independent factors, or systems of factors, is or is not greater than the haploid number of the chromosomes. The determination of this question all students of genetics will now await with keen interest.

In all the various parts of the subject explored, whether the main theory prove ultimately to be truth or fallacy, there can be no doubt as to the extraordinary value of the *Drosophila* work as a whole. Of the discovery that may perhaps come hereafter to be regarded as the most illuminating of all—the phenomenon of "non-disjunction"—we have still to speak. The exploration of this group of facts has been made by Bridges, who, since the brief note contained in the book, has published in *Genetics* a detailed account of his experiments. With this publication it must be admitted we are lifted on to something like solid ground. Hitherto amidst all that cytology has contributed, in one respect only has it been found possible to connect quite positively cytological appearances with somatic characters. That in certain forms of life sex is connected with the X chromosome is the one unambiguous fact.

To this Bridges now adds evidence of a new and very remarkable kind. In crosses between females with recessive eye color and normal "wild" males, the daughters normally resemble the father and the sons the mother. As exceptions, "matroklinous" daughters are produced, that is to say in this case with eyes of the recessive color. It was argued *a priori* that such a result might be reached if the *two* X chromosomes of the female were by some chance together passed into an ovum and that ovum were fertilized by a Y-bearing sperm. Such a zygote would be female by virtue of the two X chromosomes. But for this it would have been male, for it is fertilized by the sperm normally destined to males. Since also all the dominant sex-linked factors possessed by normal males are borne by the sperm normally destined to daughters, the sperm that the exceptional daughter receives is recessive, and therefore these daughters are matroklinous. It follows as a corollary from this argument that fertilization might take place between ova bearing no X at all and a sperm bearing X, and it is said that such a class has been actually recognized as consisting of sterile males. Once the matroklinous daughter has appeared, by breeding from her, a complex variety of consequences may be expected, all deducible from the *a priori* analysis. In the breeding experiments, apart from certain numerical aberrations still unexplained, these have now all been realized experimentally.

Cytologically also the expected appearances have been found—in the sense that egg cells of the "exceptional" females have been seen to contain three instead of two of the chromosomes which the authors now agree are the heterochromosomes. Moreover, from an XXY female it should be possible to breed an XYY male and the two in combination may lead to forms with XXYY, and figures are given showing that these also have been produced and cytologically demonstrated. No one can doubt that this is a very fine achievement. Though still sceptical as to the adequacy of the theory of cross-overs and especially of the soundness of the arguments by which the factors are assigned to serial positions in the chromosomes, it is difficult to see how we can deny that the sex-linked characters have some very special relation to the sex-chromosomes.

In our present ignorance of the nature of life we cannot distinguish cause and effect in these phenomena and it is not possible to attach any satisfactory meaning to the expression that the sex-linked factors are "carried" by a chromosome, but if any one wishes to describe the association of the phenomena in that way there is nothing to forbid him. The properties of living things are in some way attached to a material basis, perhaps in some special degree to nuclear chromatin; and yet it is inconceivable that particles of chromatin or of any other substance, however complex, can possess those powers which must be assigned to our factors or gens. The supposition that particles of chromatin, indistinguishable from each other and indeed almost homogeneous under any known test, can by their material nature confer all the properties of life surpasses the range of even the most convinced materialism. Hence it may well be imagined that even if cytologists decide that in synapsis there is no anastomosis and no transference of material, the effective transference of the gens may occur. The transference may be one of "charges." Perhaps even we might profitably consider whether the chromosomes may not be thrown up, and the gens grouped along their lines by the interplay of the same forces.

Though as must frankly be admitted the *Drosophila* work is on the whole favorable, and in certain respects strongly favorable, to the view that all segregation is effected at the reduction division, it may be well to remind the workers in this field of the phenomena which are inconsistent with that conception. There are, of course, the old difficulties that if the chromosomes play this prerogative part we should expect some broad consistency between their differentiation and that of the forms of life, and we should not anticipate that they would be capable of great irregularities of number and behavior. But apart from these there remain the perfectly authenticated instances not merely of somatic differentiation in regard to Mendelian characters, but the whole range of bud-sports and chimæras of various kinds, and lastly the indubitable evidence that the male and female sides of the same plant may have distinct genetic properties. Such facts, to be sure, are no indication as to the powers of chromosomes, but they are a strong indication that the reduction process is not the only moment at which segregation may be effected. Presumably the advocates of chromosomal views would admit that these are exceptions, but still they are exceptions of a most significant kind. Conceivably we may be led to the conclusion that there is some radical distinction between plants and animals in these respects.

Many matters of importance are treated in the book, especially the vexed question of the nature of "mutations," to which no justice can be done here. All that can be now attempted is an outline of the essential discoveries. To some it may seem that the disposition of this article is towards undue scepticism. To doubt the theory of cross-overs, for instance, at this date is almost in effect to "draw an indictment against a nation," which we know on high authority is an impossible task. Let it then be explicitly said that not even the most sceptical of readers can go through the *Drosophila* work unmoved by a sense of admiration for the zeal and penetration with which it has been conducted, and for the great extension of genetic knowledge to which it has led—greater far than has been made in any one line of work since Mendel's own experiments.

W. BATESON

PROCEEDINGS OF THE NATIONAL ACADEMY OF SCIENCES

THE ninth number of Volume 2 of the *Proceedings of the National Academy of Sciences* contains the following articles:

The Mechanism of Diffusion of Electrolytes through Animal Membranes: Jacques Loeb, Rockefeller Institute for Medical Research, New York. For the diffusion of certain electrolytes through animal membranes there is required besides the osmotic pressure a second effect called the "salt effect" upon the membrane. This consists probably in an ionization of the protein molecules of the membrane.

The Rotation and Radial Velocity of the Spiral Nebula N.G.C. 4594: Francis G. Pease, Mount Wilson Observatory, Carnegie Institution of Washington. The radial velocity is + 1,180 km., in good agreement with the value found by Slipher. The linear velocity of rotation at a point 2 minutes of arc from the nucleus is over 330 km.

A Simple Method for Determining the Colors of the Stars: Frederick H. Seares, Mount Wilson Solar Observatory, Carnegie Institution of Washington. The method suggested consists in determining the ratio of exposure-times which is necessary to produce photographic and photovisual or more briefly, blue and yellow, images of the same size.

Studies of Magnitudes in Star Clusters, III. The Colors of the Brighter Stars in Four Globular Systems: Harlow Shapley, Mount Wilson Solar Observatory, Carnegie Institution of Washington. It is concluded that in all the clusters examined and probably in all globular clusters the volumes of the bright red stars are very great in comparison with the stars that are fainter and relatively blue.

The Effect of an Electric Field on the Lines of Lithium and Calcium: Janet T. Howell, Mount Wilson Solar Observatory, Carnegie Institution of Washington. Lithium and calcium were examined both for longitudinal and transverse effects.

A Proof of White's Porism: A. B. Coble.

A Contribution to the Petrography of the Philippine Islands: J. P. Iddings and E. W. Morley, Brinklow, Maryland and West Hartford, Conn. Six detailed analyses are given of rocks from Luzon, P. I.

Salt Antagonism in Gelatine: W. O. Fenn, Laboratory of Plant Physiology, Harvard University. The experiments on gelatine support the hypothesis that anions antagonize cations in their effects upon organisms. The hypothesis here developed resembles that of Clowes except that it requires that NaCl should antagonize any electrolyte which has either a strong anion or a strong cation. The point of maximum antagonism is an isoelectric point at which the amount of alcohol needed for precipitation is at a minimum, and the aggregation or amount of precipitation is at a maximum.

Similarity in the Behavior of Protoplasm and Gelatine: W. O. Fenn, Laboratory of Plant Physiology, Harvard University. A close analogy to Osterhout's experiments on the electrical resistance of *Laminaria* is found in gelatine (plus NaOH), if we assume that the effect of time in the *Laminaria* experi-