efficiently elsewhere. The advent of systems of artificial humidification have, however, neutralised to some extent the natural advantages of the climates of those localities, and the work to which reference has already been made indicates that a complete knowledge of the mechanism of these actions is now within measurable distance.

The development of textile research would appear to deserve not only the attention which the British Association has properly given to it, but also the best which can be afforded by our academic and industrial bodies. Research is not, and should not be, the responsibility of one particular body. The research associations can do a great deal. The industries and the large academic bodies have also

an equally important duty to perform. The co-operation and sympathetic efforts of all these bodies should, however, ensure that textile research shall, under the favourable conditions which now exist, enter upon one of the most important stages in its development. Whitehead, in the preface to his "Principles of Natural Knowledge," points out that his inquiry "raises more difficulties than those which it professes to settle. This is inevitable in any philosophical work. . . . All that any one can hope to do is to settle the right sort of difficulties and to raise the right sort of ulterior questions, and thus accomplish one short step further into the unfathomable mystery." So it is with our textile researches.

The Germplasm and its Architecture.¹

By Dr. F. A. E. CREW.

THE neo-Mendelian theory is like its parents a particulate theory, implying that the germplasm of organisms is not an indivisible whole but an organisation of units or factors which can be dissociated and recombined in various ways, just in the same manner as on the atomic theory a chemical compound is not a thing indivisible but an organisation of units which can be dissociated and recombined in other ways. But it differs from the theories that have gone before in that it is not purest speculation, incapable of confirmatory test; it is not merely an *ad hoc* explanation. It is not a pre-formatist doctrine, for no one ever seriously held that a factor was in any way a miniature replica or even was representative of a single character. The term unit character itself has been dropped, for it was misleading in view of the manifest fact that characters can never be units. The attitude of the geneticist in regard to the architecture of the germplasm is similar to that of the chemist in regard to the atomic constitution of matter. It is held that the hereditary constitution of an individual is not only composed of units in particular proportions and arrangement, but that its effects are determined by these proportions and by this arrangement just as much as by the particular nature of the units themselves. A character is the resultant of a large number of agencies of the internal environment of the zygote interacting with a large number of agencies of the external environment.

Weismann, in constructing his edifice of theory, with its purely hypothetical determinants, biophors, and ids, at least did one thing that must make his name immortal; he attached his speculations to something tangible by locating his units in the then newly investigated chromosomes. In this guess he enshrined the truth, for *Drosophila melanogaster*, in the hands of Morgan, Bridges, Sturtevant, Muller, and their colleagues, has provided definite and final proof that the chromosomes are indeed the germplasm, that upon each chromosome is borne a certain definite association of the hereditary factors or genes, and that each gene has its own particular place upon a particular chromosome. It has been

possible in the case of *Drosophila* to construct a map of the chromosomes showing the relative position of all the genes so far discovered, and this map, like a railway time-table, giving the sequence of the places and the distance between them, in the hands of the breeder of this animal endows him with the powers of the synthetic chemist in the manipulation of his own material.

As yet, there exists but little exact knowledge of the chemical nature of chromatin: it is known, however, that it is intimately related to the activities of the cell as a whole; that it has a definite architecture and disposition within the nucleus; that during cell division it becomes condensed and homogeneous and then displays most clearly its organisation into units, the chromosomes. It is established that the number, size, form, and behaviour of the chromosomes are constant in a species and are characteristic of that species. It is known that even when the chromatin is thus condensed in the form of chromosomes, it still retains its organic contact with the non-chromatic part of the cell of which it is but a part, though an essential part: it is established that no development is possible at all in the absence of at least one haploid set of chromosomes. It has been amply demonstrated that the only identifiable cell organ which can satisfy the demands made upon the germplasm by the results of countless experiments are the chromosomes, and that in the behaviour of these are realised the precise conditions of hereditary transmission as recorded by observation.

The exact parallelism between the distribution of the chromosomes and the distribution of the hereditary characters affords an explanation of the ratios obtained in experiments involving freely assorting characters, if it is assumed that within the chromosomes are resident the genes, for the gene within the chromosome must necessarily go whither the chromosome passes. It is established that the chromosomes in the immature gamete and in the body cells generally are present in pairs, and that of each pair one member has been received from each parental organism. It has been demonstrated that in the ripe gamete only one member of each pair of

¹ Continued from p. 698.

homologous chromosomes is present, and moreover, that the sorting of the chromosomes during the maturation of the gamete is at random, the distribution of one member of one pair being in no way influenced by that of either member of any other pair. It is not difficult, therefore, to explain the results of an experiment involving more than one pair of factors and giving free assortment and recombination, for all that must be postulated is that the different factor pairs are resident in different chromosome pairs.

The facts of linkage can be interpreted by postulating that the factors for linked characters are resident in one and the same chromosome, and that for this reason they must remain together in inheritance so long as the chromosome concerned retains its integrity. The facts of crossing-over can be explained by postulating that the cause of this is an interchange of material between the members of a pair of homologous chromosomes, these chromosomes being dissimilar in their genetic constitution. It is frankly admitted that in this matter of crossing-over genetics has outrun cytology, just as at one time cytology had outpaced genetics in regard to the mechanism of sex-determination, but the fact of crossing-over must not be confused with the validity or otherwise of the suggestion put forward to explain it. It is the case that during the maturation division in gametogenesis the segregation of homologous chromosomes is complicated by the fact that the members of each pair, prior to their separation, become most intimately intertwined. This apparently single, but really double, chromosome then splits longitudinally. This conjugation of homologous chromosomes provides the opportunity for crossing-over. It cannot be shown to have occurred, but if during this conjugation the two chromosomes stick, fracture, and rejoin before separation, then interchange of chromatin will have occurred. It is not without significance that conjugation occurs when the chromosomes are drawn out to their greatest attenuation, so that the homologous chromomeres derived from the two parents achieve the maximum degree of association in a linear series.

It is seen that the equivalent chromosome contributions of the two parents and their random assortment in maturation and chance recombination in fertilisation, together with the possibility of an inner reorganisation of each chromosome through its most intimate association with another of identical structure but different content, provide an infinite range of new combinations of character which can be tested out by environmental agencies. The chromosome mechanism can supply the variations upon which the forces of selection can operate. It becomes apparent also that this conjugation of chromosomes in synapsis excludes the possibility of fruitful crosses between species widely different in chromosome constitution.

If the conjugation of homologous chromosomes is accepted as evidence in support of the conception of crossing-over, and if the genes are strung like beads upon a string, each particular gene having its own particular locus upon a particular chromosome,

then it follows that the percentage of crossing-over between any two loci can be regarded as an indication of the distance between them. If the members of a pair of homologous chromosomes during their conjugation are as likely to fracture and reunite at one point as at any other point along their length, it follows that the farther apart any two genes lie in the chromosome, the greater is the chance of crossing-over occurring; and, conversely, the nearer together the genes lie, the smaller is the chance of crossing-over occurring. If this is so, then it is possible to construct a map of the chromosomes, showing the relative positions of the different genes resident in each. If *A*, *B*, and *C* form a linear series, and if *B* lies between *A* and *C*, then the crossing-over percentage occurring between *A* and *C* should equal the sum of the crossing-over between *A* and *B*, *B* and *C*.

The conception of the localisation of the genes in linear alinement is due to the peculiar differences between the crossing-over between genes of the same character linkage group. The relation of three or more points to each other is a relation of linear order and cannot be represented in space in any other manner than by a series of points arranged in a line. Linearity is the expression of a system in which there is a fixed succession of elements. The distance between any two elements is constant but is variable throughout the series. In the case of the more simply organised forms of life it is possible to conceive the germplasm having the form of a congeries of chromatin units, the members of which can exist separately within the nucleus and become associated by chance. In the more complex forms, however, the precision exhibited in the inheritance of a most complicated characterisation demands a very precise and more complex hereditary mechanism, such as is supplied by the chromosomes in their constitution and behaviour.

The Columbia school has practically completed the essentials of the static theory of heredity concerning the existence and distribution of genetic units initiated by Mendel. The germplasm has been identified and its architecture defined. The chromatin material of the nucleus of the cell is the germplasm and the hereditary units are the genes. A gene is a particular state of organisation of the chromatin at a particular point along the length of a particular chromosome. It is a particular area or locus of the chromosome in a particular state. One particular condition of this chromatin can be replaced by others and with each change another gene appears.

The Columbia school has, however, done more: it has initiated a dynamic theory concerning the relation of these genetic units to development. Bridges, out of his magnificent work on balanced intersexuality in *Drosophila*, was able to lay the foundations of the fundamental theory of genic balance, according to which the effect of a gene depends not only upon environmental conditions but also and particularly upon the other genes with which it is associated in the hereditary constitution of the individual. Bridges has proven up to the hilt that the sex-chromosomes exert their effects

not *per se* but only when in a certain relation with other chromosomes. Further, it has been shown that the subtraction or addition of whole chromosomes or of whole sets of chromosomes, as in haploid or polyploid individuals, is associated with constant and definite variations in characterisation. This observation, first made by Gates, has been extended, for example, by Bridges to *Drosophila*, by Wettstein to mosses, and by Blakeslee to *Datura*.

It is an obvious fact that the extent of our knowledge of the genetics of a particular form is determined by the frequency of mutation in that form. So much is known of *Drosophila*, because, amongst other things, observed mutation in this form is relatively common. The gene is stable and so far has resisted most attempts to modify it experimentally. It will be recognised that if the usual genetical methods of investigation are to be used, then in the case of an animal or plant of economic importance in which mutation is rare, there can be no rapid increase in our knowledge of the genetics of this form until it has become possible to provoke mutation at will. It has been reported repeatedly that germinal changes, presumably mutational, had been induced by X-rays, radium, alcohol, lead, antibodies, and so forth, but the interpretation of the data thus secured has been highly disputatious. It is known that X-rays can and do affect the distributive mechanism of the chromosomes and the phenomenon of linkage, and it is now reported by Muller that radiations of short wave-length can produce gene mutations in a high proportion of germ cells so treated. Mutations thus produced are stable in their inheritance and behave in a manner typical of Mendelian chromosomal mutant genes found in organisms generally. Such treatment produces an increase of about 15,000 per cent. in the mutation rate of the stock. Many of the mutations invoked are those which have previously appeared in *Drosophila*; many, on the other hand, are mutations in loci, in which mutation apparently has never been observed before.

Through the use of appropriate doses of X-rays

also it has been found to be possible to provoke a high proportion of rearrangements in the linear order of the genes associated with fragmentations and translocation of portions of a chromosome. Here, lying now in our hands, is the very tool wherewith to explore the hereditary constitution of animals and plants of interest and of economic importance. No longer must we wait for mutations to occur: no longer need we squabble amongst ourselves concerning the rôle of external agencies in the provocation of mutation, for it is demonstrated that it is possible in our chosen experimental stocks to manufacture series of artificial strains for use in the study of genetic and phænogentic phenomena. It may soon be possible to produce to order sufficient mutations to furnish adequate chromosome maps, and thereby to give to the breeder of domesticated stock the powers of control that he must have to be ultimately successful. There is no reason to assume that X-rays alone will do this thing: doubtless there are plentiful agencies, chemical and physical, which appropriately exhibited will do the same.

Advances in genetics in the more immediate future will be in the borderland of formal genetics and of developmental physiology. They will result from the deliberate and artificial induction of mutation and from the study of the gene in action. In this latter field the trail has been blazed by Goldschmidt and by Haecker. *Drosophila* has served its purpose magnificently, but the time now approaches when other animals will usurp the centre of the genetic stage; experimentation will become more laborious, since each gene will have to be examined for the kind and rate of change which it produces. Geneticists, hearing of the vein discovered by Mendel, rushed headlong to this new Klondyke, avid for the gold of truth. They are now returning, some empty-handed, but many with nuggets of great worth. It will be well for biological science if those who resisted the call and stayed dutifully behind deny themselves the simple amusement of taunting the wanderer until at least they have assured themselves that nomadism of this kind must necessarily be profitless.

Rationalisation in Industry.

FOR many reasons the introduction of scientific research into the old-established industries of Great Britain as an aid to production is a lengthy and difficult process. Much progress, however, has been made in recent years, notably with the aid of co-operative research associations, but much more remains to be done. The degree of interest in research shown by industries is a matter of concern to every scientific man, for an increasing appreciation of the potential industrial value of research spells more practical interest in the work of universities and similar institutions where pioneering investigations are carried out, and increased outlet for scientific employment.

How confidence can best be fostered is a matter of concern to many, and various ways have been tested or considered. Research is slow to fructify,

and industrial research may be especially so when the objective is restricted and definite. While broad researches are maturing the interest of the manufacturer may wane unless he have the stimulus of some accessory assistance. The successful solution by scientific research of a simple technical problem may be as convincing to a manufacturer as a far-reaching investigation of, say, the properties of his raw materials, necessary though that investigation may be. The simple problems cannot be ignored. Similarly, the demonstration of the utility of existing information is a valuable aid. There is much scope in industry for the increased utilisation of current information, scientific or otherwise, and valuable propaganda work can be accomplished by the organised collection and distribution of such information. In