The interrelation of genetic and non-genetic factors in development

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The work of Mendel has placed the study of development on a new and solid basis. From his work and that of his followers we havé at last learned to distinguish between the two kinds of factors in the development of the organisms, so that we may now study them separately, and find out what part each cathegory plays in ontogenetic development, and in how far each can have furnished the material for a phylogenetic development of the different types.

The object of this paper is to treat of the differences between the two sorts of factors in development, to point out their relation, and the possibilities in controlling this relation in some organisms for economic purposes. The lack of knowledge concerning the parts played by the genetic, transmitted factors on one hand, and the environment, the non-genetic factors on the other, in the development of the organisms, in pre-Mendelian times is very well illustrated by the opposition of the views of Oscar Hertwig and Weismann on the subject. Weismann, building forth on Darwin's conception of evolution as caused by selection on small differences, individual variation, has elaborated a vast complex of theories, concerning the constitution of the „germplasm", numerous living, protoplasmic particles of different sorts, each mysteriously calling forth one special organ or tissue. Parting from the altogether justified consideration that the effects of the environment on the individual are not transmitted, he has tried to ascribe the whole ontogenetic development to transmittable, genetic causes, neglecting the direct importance of the environment for this development. Oscar Hertwig on the other hand, realizing the enormous importance of all kinds of non-genetic factors for all the developmental processes, has gone to the other extreme, holding that the characters of the organisms are exclusively the result of the reactions of a non-specialized germ on the different conditions it encounters during growth. Not all the biomechanists have gone to this extreme, which will have to be looked upon as a reaction after Weismann's self-satisfied and research-stifling revival of the ancient evolution idea. Roux notably has from the beginning distinguished between the typischen Determinationsfaktoren and Realisations- und Alterationsfaktoren in ontogenetic development, he being the first to show by undisputable experiment that such a distinction was called for.

De Vries's modification of the Darwin-Weismann conceptions of inheritable determinants had still the drawback of assuming that the characters of an organism depend immediately from determining particles, and the fact that from the rediscovery of Mendel's work this conception of de Vries has been grafted upon Mendelism has undoubtedly done much to discredit it in the eyes of many biologists. It is only after it had been clearly shown by the work of Bateson, Miss Durham and Cuénot, that hereditary characters were not called forth each by a corresponding genetic factor, but that factors could, under circumstances, be present in the germ without their presence making itself felt, that it was possible clearly to distinguish between genetic, transmitted factors, and the characters to the differentiating of which they contribute.

I think the fact that these two things, the characters of the individuals, and the genetic factors transmitted through the gametes, have been mixed up by several authors, has given rise to much critisism of Mendelism by conservative thinkers, whose attitude would not have been hostile, had it only always been clear that we are studying the behaviour of one special group of factors in the development of the organisms, and that the generalizations, the laws of segregation and independance of these factors only indirectly concern the inheritance of characters.

Such terms as dominance, latency, etc. which have been invented to express the behaviour of the (abstract) qualities of organisms in hybrid families, must always rigorously been reserved for this use, and it must not be forgotten, that all the evidence points to it, that a gamete can only be in one of two possible conditions in respect to any genetic factor, it can either contain it or else lack it. This is the very limit of simplicity, and it requires no special study to see that there is no corresponding simplicity in the relation between a parent's final qualities and those of its offspring. William Ritter has emphasized the importance of studying things as they are in biology, and not as they might be, or ought to be on some or other theory. I fully agree with this view, but I think we need never give up trying to analyze complex processes like development into the different fundamental factors which give this whole, simply besause neither of these composing single things is by itself able to force its simplicity upon the final result.

In the study of development and evolution, Mendelism now takes a definite and extremely usefull place.

Systematicians can be said to study the differences and analogies between groups of organisms, each group homogeneous for at least a great number of genetic factors, the Biometricians are by statistical methods studying the average effects of variation of both genetic and non-genetic factors in their combined result on the variation of the individuals within these groups.

The biomechanists concern themselves with the interrelation and coöperation of genetic and non-genetic factors in the development of the organisms; experimental zoology and botany on one hand, and Mendelism on the other, may be conceived as two special divisions of the study of biomechanism, experimental zoology and botany being chiefly study of the non-genetic factors in the development, and of environment on function, Mendelism being germinal analysis, the study of the genetic factors in the development of the organisms.

This we must always bear in mind, that we are studying the genetic factors, their effect upon development, their nature, the way in which they are transmitted, the possibility of their being influenced, their relations toward one another, and that as yet in this study we have not laid our hand on any of these things, so that we must contend ourselves to notice the difference it makes to a developing germ whether any one of them is present or not. Thus, what we are actually regarding are differences between individuals, and we are trying to find out, by carefully planned breeding-experiments in how far these differences are due to the presence or absence of each genetic factor. We have abundant evidence that these genetic factors only act by influencing the development, often a very definite stage in it. Whenever this stage is not reached, for any reason, the factor can not have any influence, but that in such a case it is transmitted just the same can be seen from the fact that the individual's gametes contain it, so that, if the zygote, to which they contribute is such, or develops under such conditions, that the necessary developmental stage is reached, the factor once more asserts itself.

Among both cathegories of factors in the development, on one hand the material, selfmultiplying, more concrete things which are the genetic factors, on the other hand the non-genetic factors, conditions, interrelations, more abstract things, there are some which are absolutely indispensable. So are for example the presence of water, a constant atmospheric pressure, non-genetic factors which are indispensable. In the case of these non-genetic factors we can study each one of them by modifying it at will, or by creating factors antagonistic to the one we want to study.

In our study of the genetic factors, we meet an enormous limitation. We can not as yet take at will any given factor from a germ. We therefore simply must limit our study to those cases where a genetic factor has already been lost in at least one individual. And this is not all, the only way to be sure that the individual we observe has lost only one genetic factor, is to mate it with another which still has it, and observe the second generation from this mating. And this means, that in the case we are dealing with a genetic factor, which is indispensable for even a partial fertility of the individual to the development of which it normally would contribute, our analysis becomes very difficult and generally impossible.

There are only a few cases in which the analysis can still be made. For instance, if we find that one animal, if mated to some others produces nothing but normal offspring, but if mated to another gives one fourth the number of its offspring with a certain defect or infertile (goats), we can with some certainty say that here the difference between normal and abnormal, fertile and infertile is caused by presence or absence of one genetic factor.

This limitation of the field of study has given rise to the remarkable objection to Mendelism one hears rather often in France, namely that we are studying the inheritance of all kinds of unimportant unit-characters, whereas the true and most important unit in inheritance is the egg itself, reproducing the individual with all these qualities.

This of course is nothing but a play of words, and if it means anything at all, it means that the use of the word character in this connection is too little exact. If, instead of concerning us in the first place with the characters we give our attention to all the different factors, which ultimately in their cooperation will make the individual with all its characters as we know it, we see that we can do without a number of technical niceties which are not only superfluous but in many cases positively harmfull where they detract from the obvious simplicity of the whole thing. It is rather curious that the only thing which is not simple in Mendelism is its terminology.

The litterature of the subject fairly bristles with new names for all kinds of characters, and all sorts of hybrids and inheritances. I believe this is a serious error. There seems to be no reason why experiment should not enrich Biology by at least three new terms, as cumbersome as the phenomena they stand for are simple. Especially are these terms superfluous where the phenomena are perfectly well understood, and can easily be circumscribed in terms like: distribution of genetic factors over the gametes produced by a heterozygote, absence of one causing inactivity of others and action of non-genetic factors.

If, for instance, two individuals are crossed and the hybrid has a quality which neither parent-form had, we may call this combination of factors, and it is a secondary point what these factors have done in the parents. I thing it absolutely superfluous, not to call it by another name, to say that some „character“ of the hybrid was „latent“ in one of the parents.

If in a bakery one evening there will be all the factors for producing bread, such material things as flour and salt and yeast and such factors as the ovenheat and the skill of the baker and his assistants, but that one single factor, water, fails, there will be no bread produced in that bakery during that night. Would it help anyone to understand the situation, if, instead of simply stating that water was not to be had, we said that bread was there all the time, only in latent condition?

It is not even feasible to devide the cases of „latency“ into such where one factor would produce a character but is absent, such where two are necessary but where either is absent and so on, because any one quality of an organism is only reached by development under the influence of a great number of factors. And among these factors it can hardly be said that some are more important than others. In the case of our bakery, it is just as important that water or yeast are to be had as that the baker is sober or the oven heated.

It is very well possible to study the genetic and the non-genetic factors in heredity separately as such, but whenever we want to study the qualities of an organism, we must take both kinds into account.

It has long been a question whether a good distinction was possible between these two kinds of factors, a question to which Mendelism has undoubtedly given a positive answer. We now know that genetic factors can only be either present or absent, non-genetic ones may each vary in intensity. As selection in a group, whose members exhibit a continuous variability for any quality has repeatedly been shown to shift the mean of the variation-curve in the desired direction, it is clear that either continuous variation can depend upon genotypic differences within the group, or else, that, if this variation should depend solely on a variation of intensity of the non-genetic factors influencing the quality, that these non-genetic factors must have transmittable influence.

By the experiments of Johannsen and Nilsson-Ehle, it has been conclusively shown, that continuous variation may depend on genotypic differences between the members of the population, and that in a group of organisms with identical genetic factors, the modifications by the non-genetic factors are not transmitted. Johannsen's well-known experiments with beans have shown that selection within a biotype has absolutely no effect. The author recently concluded a series of selection-experiments with dandelion, which fully corroborated Johannsen's work. The experiments of Nilsson-Ehle on the colour of wheat-grains, have conclusively shown that several different genetic factors can contribute to the development of the members of one biotype, which all of them tend to influence this development in the same direction. In the work with the colours of mice, and above all with cavies, evidence has been found for the same fact. (Miss Sollas). A population of which the members differ in respect to the possession or non-possession of one or more of these genetic factors presents a continuous variation between light and dark colour. But nevertheless by germinal analysis it can be shown that it are not the genetic factors themselves which vary in intensity, only the qualities produced by their cooperation with the other factors. This explanation of the effect of selection on a population, would seem to make it unnecessary to assume that ever the effects of the non-genetic factors would become hereditary, and the question naturally poses itself: Could, under circumstances selection or any other non-genetic influence change the constitution of the germ?

The experiments of Castle on the amount of black in the coat of hooded rats have been interpreted by their author to show that selection on continuous variation within a strain can shift the mean of the variation-curve. I am repeating these experiments, and as I have only bred some few hundreds I am not yet prepared to state how many genetic factors can constitute the difference between a dark rat and a light one. But I find, that selection has effect only in so far as one chooses between individuals differing in genetic constitution, but is without effect when the choice is made between individuals with the same genetic factors, but differing through the effect of non-genetic ones.

How should we have to picture a possible inheritance of modifications? Theoretically spoken, it is impossible to conceive of such an influence of a non-genetic factor on an organism, that one or more of the genetic factors going into its gametes are so changed as to produce in the offspring a change in the same direction as that produced in the parent by the non-genetic factor in question.

Thus, life in a warm environment affects the taillength of developing mice. The experiments of Przibram have shown that the tail gets longer if the animals are subjected to the changed environment from birth upwards, also, that the taillength is similarly affected by the same influences if the individuals are subjected to them from the moment of fertilization until the moment of birth. And if an individual grows in this medium before as well as after birth, the tail gets correspondingly longer. It might be conceived that sometimes this influence of the environment on the taillength would be accompanied by one on the gonads, so that e. g. a genetic factor, normally present, would get lost. We have reason to assume that sometimes such an effect of non-genetic factors is possible because of the experiments of Tower. But in such a case it is very improbable that the falling-out of a genetic factor should produce an effect on the taillength rather than on the colour or on the form of the animal's humerus or anything else.

It is very far from probable that the mice, whose taillength is modified by Przibram by subjecting them before or after birth to a different temperature, will produce offspring with longer tails than normal mice, on condition that these young will not themselves be either before or after birth subjected to the changed non-genetic factors for taillength. Hitherto the alleged cases of the inheritance of modifications are all based on a play of words, reckoning the new generation to begin at birth, instead of at the formation of the zygote, so that the effects of non-genetic factors on an unborn individual can be attributed to the modifications of its mother. A beautifull example of this is seen in the recent experiments of Kammerer. He found that a certain lizard, in a high temperature changed the white colour of its abdomen into red. He found that the young born from such a lizard had the colour of their mother. When this was red through high temperature, the young born were red, when the mother had lost its red colour after being brought back into a lower temperature, the young born were normal. He now found, that when he brought back the mother to a low temperature before the birth of the young, these young, when born sufficiently early after the change of temperature, were still red. He concludes from these facts that the effect of the temperature is transmitted. In a certain sense it is, for, as the young are inclosed in the body of the mother, they can only receive the additional heat through the intermediary of the tissues of the heated mother. Of course it is wordplay to call such a process by the name of heredity. One could as well say, that, as the offspring of heated lizards are born hot in a high temperature, this temperature was inherited. The fact that the young lizards, born after the mother has had time to cool off are born cold but still redbellied only shows that the red colour resulting from a hot environment persists longer than the body-temperature itself.

As to the development of each individual of a strain cooperate about the same set of non-genetic factors, and as the reaction of the development on these factors is always essentially the same, it was easy to believe that in the course of time all these different things became part of the inherited set. But this is obviously an unnecessary assumption. If, to take an example, in an organism there exist organs, which are so constituted that, under the influence of a grouping of certain transmitted non-miscable substances, under the influence of gravitation, it reacts by taking a certain position in respect to the vertical, I for one do not see the necessity, even granted the possibility, of this effect of gravitation becoming hereditary. The earth is always under all these organisms to attract them.

If we find that two species of the genus Mus, one living in our parts, the other in the tropics, differ amongst other things in taillength, it might be said by a Neo-Lamarckist that here the effect of temperature at least had become hereditary. But after the experiments of Przibram we know that it is unnecessary to make this assumption, the constantly higher temperature in the tropics causing all the individuals of the species to have long tails. One could only compare the genetic factors in the taillength of these animals, by growing them for a generation in identical temperature.

The colours of butterflies are due to a number of factors, among others a certain temperature during the pupal stage. Only if parent and offspring be subjected to the same temperature are they identically coloured. Loeb has suggested that the differently coloured patches might have a different temperature-coefficient of development. In such a case, the effect of temperature does not become fixed, it is not fixable, and it would not be of any use for the organism if it were. Undoubtedly, such examples are very numerous. For instance in some birds, pigmentformation requires a certain minimum temperature, below which their feathers are produced pigmentless. Probably it can be said that the temperature-coefficient for the growth of their feathers is smaller than that for the formation of pigment. Such birds, like the ptarmigan, the razor-bill, the guillemot will be lighter-coloured in winter than in summer. In the reverse case, when the temperature coefficient of feathergrowth is greater than that for pigment-formation, the birds must be lighter in summer. Such is obviously the case with the snow-bunting, which is whiter in summer than in winter. In mammals we have some examples of the first sort like the stoat and the weasel, and some of the second sort, like some bears and the chamois. Probably there are a great number of animals in which the difference between the temperature-coefficients of feather (hair) growth and that of pigment-formation would be considerable enough to cause such a seasonal dimorphism, if only they lived in a country where the temperature at the time of their springmoult differed sufficiently from that at the time of the autumnmoult. Further, it stands to reason that animals which only moult once a year can never show seasonal dimorphism from this cause. Such may be the case of the gyrfalcon, which is constantly light in northern countries, and coloured at other places.

In all organisms that have been studied in respect to both genetic and non-genetic factors in their development the interrelation of these two groups has been very obvious. In making the best possible economic use of an organism, it always pays to study this interrelation, and to find in how far it will be possible or profitable to change some or more of these factors.

There are always two ways of making the cultivation of an animal or plant pay better than it does, either one can change the set of genetic factors to suit the existing non-genetic ones, or else, one can change these latter to suit an existing biotype.

The study of the suitable non-genetic factors in the production of animals and plants is the subject of constant study at the agricultural and horticultural experimentstations. The manipulation of the genetic factors has until recently consisted entirely in a more or less conscious selection. The research-work with genetic factors of the last ten years has given us a basis upon which the experimental breeder can build forth, to improve the genetic constitution of the cultivated animals and plants. We can make the animal or plant to suit the conditions and methods of cultivation only in as far as we have the required genetic factors to combine or, if the elimination of some factor or factors is required, if we can find at least one individual devoid of them. In very many cases, as in agricultural plants where methods of cultivation can not vary very much, or where as in the case of climate, it is impossible to change one important non-genetic factor, we simple have to produce a biotype which will develop in a satisfactory way even with such non-genetic factors as we have. In other cases, it will be found impossible to change the genetic constitution of an organism. For instance when only one biotype of any form is imported to a quite different country, where there are no other types to hand for crossing purposes. Such is the case with a great number of animals and plants from milder climates, which can be profitably cultivated even with us, but only on condition that they are given the necessary shelter, such as the tomato and poultry.

It has long been impossible to grow citrous trees anywhere but in a subtropical climate. By crossbreeding oranges and lemons with one of the hardy citruses, Dr. Swingle has of late years succeeded in producing hardy trees which can be profitably grown even in countries where winter brings a moderate amount of frost. This is an excellent demonstration of the relative cost of the two possible ways of bringing about the necessary balance between genetic and non-genetic factors, for even if the crossings and extensive sowings and judging have cost a good deal of money, if once the hardy type is produced all further expenditure ceases, whereas the only other way to grow oranges in countries where it freezes, ist to grow them under glass, a practice which would cost so much every year that it could not be thought of. This relative cost of the different factors is an important thing in agriculture. For instance, the average length of growing season required is about equal for maize and for tomatoes. Still, tomatoes can be profitably grown where maize is not, simply because the value of the crop is so high that it pays to grow the young tomatoplants under glass. This can be done equally well with maize, so that it is possible to get it to bear well in any climate, only, in relation to the value of the crop, this would be too expensive. As tomatoes are somewhat of a luxury and expensive, it has hitherto not been judged worth while to try to breed types which could do without artificial heat, or perhaps even the thing has to be stated the other way round, the price of the fruits being high because of the added cost of planting out.

Nearly in every case it will be found that it pays well to find or make a biotype which is as well adapted to the cheapest methods of cultivation as possible. And it will be seen that not those organisms find the most extensive cultivation which command the highest price per unit of area, but that the most extensively cultivated plants and animals are those which as the result of hybridization or spontaneously, exhibit the greatest genetic variability, in other words, amongst which there can, for a great number of genetic factors be found individuals or types having them and others without them.

Such is the case with maize, with the horse, wheat, dogs peas and swine, sheep and poultry. Let us examine wheat, wich is next to the dog perhaps the best example. Among the fourteenhundred pure types of wheat in the collection of the seedfirm Vilmorin-Andrieux, there are only a few more than a dozen which can be profitably grown around Paris. All the others have combinations of factors which make, that under the conditions under which wheat is grown here, they can not compete with the first dozen. But this does not mean that these few varieties of wheat are therefore the best generally. They are simply the most profitable here. The individual adaptation of all the other types and a little study as to some special conditions they may require, suffice to make it possible that they are all grown at Verrières. It would be possible among such a collection to find wheats, which there are only kept for curiositys sake, but which would be excellent in some other part of the world, with a longer or shorter season, with a greater or lesser rainfall, with an abnormally wet or very severe winter, or with an exceptionally hot summer. A combination of genetic factors which in France or in Sweden gives undesirable characters, may in Thibet or in New South Wales prove to be just the thing required. Thus may long glumes be looked upon as undesirable in countries where they have no use whatever, and only serve to heighten the chance to catch rust or smutspores, whereas in countries with an excessively hot summer like Thibet or Oklahoma long glumes may protect the young grains from the withering effect of hot winds. In countries with a rainfall limited to one season like California, it will be necessary to choose amongst rapidly-stocking summerwheats, which would in Western Europe be unable to compete whith slow-growing winterwheats.

Sometimes there are very special conditions under which it would seem impossible to grow wheat, and it is something astonishing to see how some varieties can under them give a paying crop. Thus there exists a variety, „hâtif de la Saone“ which can be grown on land standing under water for weeks at the time. This is again a good instance of the relative cost of the manipulating of the genetic constitution, and the manipulating of the environment to suit a given type. With this wheat, or with some other type into which its peculiarity is brought by cross-breeding, wheatcultivation becomes possible on many a rich bottomland, now given over to pastures or to maize, where, to grow the ordinary types of wheat, it would be necessary to keep the land from flooding by expensive works.

One of the reasons why the importation of wheatvarieties from other countries nearly always means disappointment, lies, I think, in the fact that always „good“ varieties are tried. And if a variety has the name of being excellent anywhere, this necessarily means that it is by its genetic constitution especially well adapted to local conditions and uses and local tastes. Thus lately therehave in England and in Holland been tried several varieties of excellent Australian wheat. Without exception these wheats have proven valueless, because of the high susceptibility to rust, among other things. Similarly, several strains of wheat produced by the Svalof experiment station in Sweden have been tested in Holland.

It seems that in Sweden a better price is paid for coloured than for white grain. The varieties tested in Holland had all coloured grain.

But there, white grain commands a better price, so that, to begin with, the very colour which was an advantage in Sweden proved a fault in Holland.

The experiments with Swedish wheats were rather failures, probably also, because they could not compete in Holland with varieties which would not even survive the Swedish climate.

It has been often proclaimed that every country should produce its own variety of wheat or other agricultural plants, but this is obviously not true. The necessary combination of genetic factors can be made anywhere, but the choice between the types should be made on the spot, testing each form under the most economic conditions in comparison whit the best others. The fact that those wheats at Svalof are excellent in Sweden stands in no relation to their production in Svalof. Probably the first valuable wheats there were not made in Sweden at all, but imported amongst others from France, England or Germany. But they are good, because of the fact that they have been chosen in Sweden by Swedish experts from amongst the mixture in which they found themselves. In latter years, Dr. Nilsson-Ehle produces for the station of Svalov new combinations of genetic factors, by crossing-experiments with a definite aim, which obviously is a much more certain way than the happy-go-lucky selections from mixtures of Svalof's earlier days.

But it is well to remember that Dr. Nilsson-Ehle could do his excellent work as well in Madeira or in California as in Svalof, and with quite the same results for his countri, if only he sent his seeds there to be tested by his agricultural experts. For any country, wishing to produce varieties more suitable to different conditions in its diverse parts, it is quite possible, as well as most economical, to have one single easily accessable station at which an experimental breeder can make a great collection of diverse varieties, and produce new ones for the agricultural experts at the different local experiment-stations to try and compare.

I would especially emphasize the need of having a great collection of types, imported from other countries, quite independently from their greater or lesser importance in their own country. I am quite certain that in trying only „good" imported varieties, one considerably limits one's chances to find a suitable one for one's own, probably somewhat different conditions. It is obvious that, whenever a choice is made between a great number of different biotypes, a great number of them are rejected, which would be found excellent if only the exact locality for which they would fit could be ascertained.

The requirements of a variety of wheat are extraordinarily different for different localities. In one place the straw has nearly the same value as the grain, and a wheat is required, which will respond to a dose of artificial fertilizer by a heavy yield of good straw, at another place the straw has no value whatever, and the same wheat might be far from profitable in favorable years. In some localities even the wheat with the weakest straw does not lodge and there a variety may be excellent which at other places would be hopelessly ruined by heavy rains.

In arid and semi-arid regions everything depends upon the price of the water available for irrigation. Whereever water can be had in sufficient quantity, and is not too expensive, it will be found, that a rather longlived wheat, capable of producing a good yield when irrigated will be required. Where water is expensive and only one soaking can be given, a rapid-stooling, shorter-lived wheat will probably be best and will profit most. In semi-arid, dry farming regions, everything depends upon the drought-resistance of the variety. In choosing a suitable wheat for such and for irrigated regions the utmost care will have to be exercised to get one that exactly fits the conditions, for here every dollar spent in making or finding the very best type, will be many times repaid, each season, by greater yield or lesser waterdues. In extending the wheatbelt northwards, it is again not one ideal variety which is required. In some places the winter is extremely rigorous but not too long, at other places the summerseason is very short of duration. In the first spot, a summer-wheat will probably be most profitable, whereas in such places where the growingseason is too short, a winterwheat will be required.

The different wheats found wild by Aaronson in Palestine might prove usefull in dry countries, if only as carriers of usefull genetic factors, or the reverse, serving to get rid of some factors absent from them.

The work of producing suitable varieties of wheat and other plants for different regions of one country is not the work for one man. It is impossible to ask the experimental breeder to do the work of testing the different types he has produced or collected. This is the work of the agriculturist, the wheat-expert or the farmer. One man could do all the scientific work for one country, if only the practical men will cooperate, by stating their needs and testing his types in comparison. In this a connection between a single central experiment-station under an experimental biologist and the different agricultural experimentstations will be most usefull. It has been sufficiently demonstrated at Svalof, that there are no short cuts to the recognition of a useful variety as such, that the only way to see whether a given type has practical value is to grow it under field-conditions in comparison with established types.

It has been beleived here and there that one could recognize the valuable types by a botanical study, important qualities being correlated to anatomical differences, and I believe much valuable time has been formerly lost at Svalof by this kind of work. With the exception of Blaringhem in his work with barley, I think all experimenters with agricultural plants have given up the correlation-idea. I do not think it will have to be the work of the experimental biologist to pass judgement upon the merits of a given type, his work is the study of the genetic factors, leaving the practical men to judge his combinations.

The work of manipulating the genetic factors of the agricultural plants of all the different parts of one country can easily be centralized, and done for a sum of money, insignificant in comparison to the benefit the population in general would ultimately derive from it, and undoubtedly most governments will realize this importance and follow the example given by the government of the United States. But for obvious reasons it is impossible to do the necessary work with animals at one station. The only way to have this work done is to leave it to the practical breeder, under supervision and advise of the experimental biologist. It is not to be desired that the advise of this man will take the form of showing the ideal towards which must be bred, for, more often than not, in the case of animals, the difficulty is not to produce a not yet existing combination of genetic factors, but rather to produce a homogeneous type, a strain homozygous for all the desired genetic factors.

The practical men know very well what they want, and it remains for the experimental biologist to show them how they can attain the goal they have set themselves. Often also, it will be necessary for him, to devise experiments to find out, in how far the desired effect depends upon a certain combination of genetic factors, and in how far upon non-genetic factors, which might be easy of control. Such experiments with animals can be centralized in a certain way, by having the experiment-records kept at the central experiment-station. In work with animals everything has yet to be done from the bottom upwards. The existing system of breeding animals, and above all of registering them has resulted in a great many cases in a population in which a minority have the desired genetic constitution, the rest, though nearly all bred from individuals, coming up to this standard, being below it. In the first place it will in such cases have to be ascertained in how far the desired result depends upon genetic factors. It will be found that in most cases, such qualities as appear difficult to fix are such which need for their formation the cooperation of a genetic factor, for which most of the individuals are heterozygous. It will have to be taught the practical breeders that their only chance of making a breed pure for any character, is to have a study made of the genetic factors, necessary for this character, and by breeding from homozygotes only. It is after some study comparatively easy to find the necessary individuals which can be used for testing the stock. It will have to become understood by breeders, that in judging an animal for stock-purposes, they must not in the first place ask for the qualities of its parents, but rather inquire after the quality of all of its children from animals of different quality. As in the studbooks the inferior individuals are for different reasons not inscribed, they give an utterly false idea of the history of the variety, and they have hardly any value, even for the study of less important genetic factors. But as in animals it is practically never required to produce a new biotype, a general understanding of Mendelism, and the use of testmatings can be of enormous benefit. It will have to be the aim of biologists to devise systems of breeding, which can be followed by the practical men „by rule of thumb“ as the present systems are now, if they will produce a lasting effect on the amelioration of the breeds.

Whereas the work of manipulating the genetic factors of plants can be done for the whole country at one central station, the practical men getting their pure seeds directly from the station or indirectly through the seed-growers, and a more general knowledge of the principles is relatively superfluous to them, in the breeding of animals, nearly everybody using them must necessarily breed them. If therefore, in the case of plants, it will suffice for any country, if one or two men, well versed in this work, will apply themselves to it, it is of the utmost importance that practical breeders of animals should be taught to understand how to choose what they want.

In man the relation of genetic and non-genetic factors is perhaps still less understood than in animals and plants. And here, as everywhere when positive knowledge is scarce, personal beliefs on the subject are all the more imposing. One man beliefs that to ameliorate mankind and social conditions, it will be necessary to prohibit by legislation some special types of marriages. This idea goes with the other, that conditions outside the germ have only an unsignificant influence on the making of a man's character. Thus have we lately heard a curious tale of an experiment, in which a large number of boys and girls from criminal or otherwisely undesirable parents were herded together on an island inhabited by a pious, but somewhat slow population of fisherman's families. The anonymous author, who relates the story seems to confound schoolchildren with germs, and from the fact that these hordes of illguarded children remained as they were in their native slums, he concludes that the experiment shows that even an ideal environment can not affect a morally defective born child. Other people there are, who want to know nothing about inheritance, and who believe that the non-genetic factors count for so much that a permanent amelioration of the environment is all that in the future will be found necessary. Personally I am not much more inclined one way or the other. The subject is essentially one of study rather than of opinion. I fear that the greatest danger which threatens a sane and sober study of eugenics will be that enterprising politicians will accentuate the difference between the two opinions as to the course to follow, and will ruin the prospect of the necessary public endowment of the study by taking either the „breeding-principle" or the „amelioration-of-environment-principle" as planks in their political platforms.