In this case the mass was as hard as the kidney as I felt it between finger and thumb before opening the abdomen, and I am confident that nothing but abdominal section could have saved the child's life, otherwise the swollen oedematous protrusion would never have passed the ileo-cæcal valve.

I have now put upon record all the cases of intussusception in children which I have been called upon to treat—three females and four males. All were infants under 18 months, except one a boy of 4 years and one of 12 years of age. One was treated by repeated injection, and inflation alone, with apparent reduction each time until gangrene and collapse came on, with speedy death. Six were treated by laparotomy after injection had failed completely. Of these, three recovered perfectly, one aged 4 years, one 5 months, the last 7 months.

It is interesting to note that out of the six operated on, three at least could not possibly have been reduced by inflation or injection. For in one the intussusception was high up in the small intestine, and in the other two several inches of ileum were prolapsed through the ileo-cæcal valve. In all the seven it may be said, then, that injection failed, and in three it must have failed. In spite of this, laparotomy saved three out of the six operated on. But when we come to examine the causes of death in the three which died after operation, it is clear that the result was due in each case to the operation not having been done soon enough. In one of them when the abdomen was opened the intussusciptiens was sloughing largely, in two places forming wide perforations. In another the gut was not actually sloughing, but was rotten from strangulation and incapable of being reduced. In the third a small slough was in process of formation at the time of operation, which gave way a day later with fatal perforation. All these cases might have been saved by a somewhat earlier laparotomy; they could not have been saved by injection.

In addition to the cases now related I have collected all those of children treated for acute intussusception in the surgical wards of University College Hospital from the year 1877 to the end of 1893. These, with my own 7 (1 in private), form a list of 25 (*vide* footnote), cases all under 13 years of age. Of these, 13 recovered, 12 died. The treatment adopted in 19 was either manipulation (1) or injection of air or water (7), followed in case of failure by laparotomy (11). In 5 cases laparotomy was done without any previous attempts at reduction; 2 of these were of the enteric variety; 4 died, 1 recovered.

Of the 19 cases where injection and manipulation had been tried, followed by laparotomy where necessary (11), 7 died and 12 recovered. Of these 12 recoveries, 7 followed manipulation or injection alone, 5 followed laparotomy where these had failed. Of the 8 fatal cases, 7 deaths followed laparotomy, 1 followed repeated injection. Taken in the gross, there were therefore 17 laparotomies with 11 deaths, and 8 injections or manipulations alone with 1 death.

From all this it would appear that the practice at University College Hospital for many years past has been to try injection of air or water in all cases not recognised at once as desperate, and, when these means have failed, to proceed to laparotomy at once. By these measures 13 out of 25 cases have been saved, and probably a larger number would have recovered if the laparotomies in several cases had been done earlier.

Of course the majority of the recoveries are credited to the injection method, inasmuch as laparotomy was only resorted to when the latter had failed. In other words, abdominal section was only done in bad cases; indeed, in several cases the condition was desperate. In the face of this fact it is encouraging to note that out of 17 cases treated by laparotomy, 6 recovered, although several were in a very bad condition at the time of the operation, in some the bowel being actually gangrenous. If we subtract these cases from the list the proportion of recoveries is fairly good considering the gravity of the condition.

The conclusions, then, which seem deducible from a study of these and many other records appear to me to be as follows:

1. That in all cases of intussusception in children injection of water or manipulation should be at once resorted to *if the patient is seen within a few hours of the onset of the strangulation.*

2. That if these means fail after a fair trial, not too much prolonged, laparotomy should be at once done as the safest treatment.

3. That there is a certain proportion of cases among all the varieties of intussusception which no amount of injection will relieve, or in which injection would be dangerous, and these can only be dealt with by opening the abdomen.

THE MILROY LECTURES ON DARWINISM AND RACE PROGRESS.

Delivered before the Royal College of Physicians.

By JOHN BERRY HAYCRAFT, M.D., D.Sc., F.R.S.E.,
Professor of Physiology, University College, Cardiff.

LECTURE I.—THE BIOLOGICAL POSITION.

BEFORE the simultaneous publication in 1858 by Darwin and Wallace of their law of natural selection, biologists believed in the Lamarckian view of heredity, a notable believer being our own Herbert Spencer. Lamarck would have accounted for the long neck of the giraffe by supposing that it was at first a short-necked quadruped, that it exercised its neck in browsing off the trees, that the neck elongated in consequence of this use, and that this effect, ever so slight, was transmitted, each generation of stretching accumulating until the present condition was established. In this Lamarckian view of heredity and racial change we have the two ideas; first, fresh characters may be acquired during an individual's lifetime, due to the action of environment; secondly, these are transmitted to the offspring, and produce in time marked racial change. Now the first of these ideas is certainly and admittedly true. The second idea—whether these changes are transmitted—is the rock upon which the Lamarckian boat founders.

The law of natural selection brought forward by Darwin and Wallace may be stated as follows. No two offspring of the same parent are quite the same; they vary often widely. Under the conditions in which they live, some of these offspring will, from the very fact that they differ from each other, have an advantage over the rest, and in the struggle for existence will run the better chance of carrying on the race and transmitting their inborn peculiarity.

Keeping to the case of the giraffe, the history of its development would be as follows: It was at one time advantageous, from the point of view of getting food, for the ancestral giraffe to browse upon trees, therefore those born with long necks and legs would have an advantage over the others, and would in greater numbers survive those with short legs and necks. These would have offspring the average of whose necks would be as long as their parents', but who would again vary, some having longer, others shorter necks. The ones with longer necks would again have an advantage, and produce most offspring, and so on.

Now, there are three ideas in this law of natural selection; first, that there are inborn variations among offspring, even of the same family; secondly, that these variations occurring in surrounding conditions, on the whole uniform and common to all of them, will start some with an advantage and others with a relative disadvantage, and that those with an advantage will, more of them, tend to produce offspring; thirdly, that the variation, inborn in this case and not acquired, will probably be transmitted.

Variations will be of advantage or disadvantage to their possessors, and in animals and plants, where there are no social props to the weak, the variations may, and do, determine survival. To give an idea of the keen competition of existence which we find among the lower animals, we have only to enumerate the number of the progeny produced by each pair, which is often prodigious. Knowing as we do that the number of individuals in a species remains virtually the same in a given district for long periods of time together, and that the room of the parents is just filled by a younger pair, all the excess of their progeny over and above this one pair must have succumbed in the great struggle.

The third idea in the law of natural selection—namely, that inborn variations are transmitted, is also a fact that is

universally admitted, not only among biologists of the present day, but also by those who trust only to their every-day experience. The law of natural selection is therefore fact, every bit of it, and no mere theoretical improvised fancy. The only question is, How far may this law be applied? Is there also a concomitant transmission of acquired inborn characters (Lamarck's view)? Darwin thought there was; he held that certain racial distinctions were due to the action of the environment of the parents, and the transmission of the change thus produced upon their offspring. In *The Variation of Animals and Plants under Domestication*, 1875, vol. ii, chap. xxvii, he enumerates some of these. They may be divided roughly into two classes—first, instinct and habit;

Fig. 1.—Diagram to illustrate (A) the transmission of acquired characters, (B) modification of type by natural selection. In A an individual of rounded proportions at the top of the diagram has two children. Environment is represented by a board with holes through which they must pass. In so doing they become thinner, transmit the thinness to their children, and so on. In B a man of rounded proportions has two sons, who vary, one being fat, the other thin. The fat son cannot get through the hole in the board, but the thin one does, has children who again vary, the thin ones having an advantage.

second, results of use and disuse. Darwin believed that the tamed habits of dogs and horses, the tameness of the rabbit and other domestic animals, were due to the direct and transmitted effects of man's contact. He held that the large size of the leg and small size of the wing of the domestic, as compared with the wild, duck, are gradually acquired and transmitted by use and disuse. That he might picture to himself by means of material particles the views that he held of heredity, Darwin supposed that during their lifetime every cell of the parent disengages small living particles (gemmules), which find their way to, and are stored up in, the generative cells, ready to develop in the next generation into cells similar to those from which they came. This theory was actually framed to support those cases where Darwin supposed that acquired characters were transmitted.¹

In this theory of pangenesis the gemmules in the body or somatic cells will be subject to such influences as affect the cells, and will naturally transmit any change to which they have been subjected to the offspring that they eventually build up. Darwin's view may be graphically represented by Fig. 2. Since Darwin's time most biologists have come to

Fig. 2.—Diagram to illustrate (A) Darwin's theory of pangenesis, (B) the continuity of the germ plasma; (A) an ovum above full of gemmules (only three of which are represented) develops into an individual made up of cells, three of which are shown. These cells give off gemmules which collect and form the substance of the ovum of the next generation. One can see how the gemmules formed by the body cells will be influenced by any change in these. (B) An ovum gives rise, on the one hand, to body cells, and, on the other hand, to the substance of the ovum of the next generation. A change in the somatic cells does not influence the ovum.

¹ *Life and Letters*. First edition, vol. iii. p. 44.

doubt whether there is any evidence that acquired characters are transmitted at all, and incline rather to view racial change as altogether due to inborn variations, some of which variations have an advantage over others. Darwin himself, as Huxley pointed out,² laid less weight on the influence of acquired habits in his later than in his earlier writings. It has been known for many years that every cell in the body, including the sperm cells or ova, are descended from a fertilised ovum. Of these cells of the body all obviously die except these sperm cells and ova which give rise to the next generation and so on. We have, therefore, a continuing chain of actual organic matter linking every living form with those that are most ancestral and remote, and from these chains all the so-called living individuals that have ever existed have, as it were, been thrown off. Many have emphasised this point—Owen, Haeckel, and others; but perhaps Francis Galton must be given much of the credit of clearly stating it, though similar views have more recently been popularised among biologists by Weismann. The "continuity of the germ plasma," the title by which this view is generally known, expresses the fact that germ substance continues in an unbroken line from generation to generation; a man is similar to his parents because he develops out of a similar plasma. The continuity of germ plasma (stirp of Galton), like Darwin's selection, is a fact, not a theory.

Now, while Darwin's theory of pangenesis compelled one to believe that the effects of disuse and use, the action of acquired disease and mutilation must be transmitted by the gemmules contained in the parts so affected, the fact of the continuity of the germ plasma leaves the question of their transmission an open one still. We can imagine that the generative cells within our body are affected by what goes on in our other cells, or that they lie there unaffected, the lymph and blood being in fact the only channels of communication between the two. The continuity of the germ plasma does not compel us to accept either view.

The fact of the continuity of germ plasma was shortly and precisely stated by Galton in 1875, and by Weismann in his *Studies in the Theory of Descent* published in English in 1882, but since that time Weismann has speculated at great length upon what I may term the actual process of heredity. His essays, many of them, deal with facts capable of test and verification, and are of a value that it is impossible to overestimate.

In 1883, in his *Essay upon Life and Death*,³ Weismann looked upon the germ plasma simply as the substance of the germinal cells, but since that time Weismann has left facts behind, and has elaborated the details of this germ plasma from his own fancy. To him it consists of ultimate living particles, to which he assigns various and specific purposes, and groups them at will, group within group, like nests of Chinese boxes. As new discoveries of facts are made by the histologist or embryologist these fancies have to be altered and extended, and his particles have to possess such and such fresh qualities. We cannot test his view, it does not assist us, for it merely refers the qualities we observe in men and animals to the properties of groups of chemical molecules where these qualities are equally without possibility of explanation, and still confront us; and, lastly, we have no reason for assuming that living matter is made up of living molecules, but rather the reverse.

Now, we may speak of a molecule of a gas or of incandescent iron, but we must hesitate before speaking of a molecule of an ordinary solid at ordinary temperature, and one would certainly, if an exact thinker, never dream of speaking of a molecule of wood or flesh or bone. The idea of molecule is a conception of an ultimate bit of a substance towards which experimental breaking may in actual experience partially reduce it. When, therefore, we translate this idea to the realms of biology where bodies are not made up of similar parts, it is obvious that we do this without warrant in face of our biological experience, and that we are thoughtlessly accepting a physical theory of gases and incandescent solids without appreciating the foundation upon which this theory rests. The fact of the matter is, we are in utter ignorance as to the ultimate constitution of any living matter; but, so far as

² *Life and Letters*, ii, p. 14.

³ *Essays upon Heredity and Kindred Biological Problems*, Eng. Trans., 1889.

microscopic investigation goes, we learn that it has unlike, not like, parts.

How much can we explain of racial change by selection, and how much must we leave over to be explained by the transmission of acquired characters? Here we are dealing with definite issues which can be settled by observation and direct experiment, and which have been so far settled already as to enable us to state such a conclusion as will safely guide us in further inquiry.

Francis Galton,⁴ as long ago as 1876, is most clear and concise as to this question. He says: "The conclusion to be drawn from the foregoing arguments is that we might almost reserve our belief that the structural cells can react on the sexual elements at all, and we may be confident that at the most they do so in a very faint degree; in other words, that acquired modifications are barely, if at all, inherited in the correct sense of the word." Thirteen years later,⁵ during which period he had been a devoted investigator of the facts of heredity and an indefatigable collector of fresh facts, he expressed himself in precisely the same terms.

In 1882 August Weismann questioned whether there is as yet any proof that acquired characters are transmitted. He writes: "The theoretical conception of variation as a reaction of the organism to external influences has also not yet been experimentally shown to be correct. Our experiments are still too coarse as compared with the fine distinctions which separate one individual from another, and the difficulty of obtaining clear results is greatly increased by the circumstance that a portion of the individual deviation always depends upon heredity, so that it is frequently not only difficult but absolutely impossible to separate those which are inherited from those which are acquired." Since that time Weismann, in a series of important essays, indicating a profound knowledge not only of comparative morphology, but of the habits and modes of life of a vast number of animals and plants, has shown, first, that the cases which Darwin and others have assumed to be those of transmitted acquired characters, may reasonably be explained—most of them—by selection; and, secondly, that characters experimentally produced are, as a fact, not transmitted. The gradual increase, generation after generation, in the size of a useful limb, or the perfection of a valuable organ of sense, may readily be explained by selection alone. The fact that the limb or organ is of use to the race in its struggle will determine the survival of those born with the possibility of growing those serviceable parts, and these in their turn will produce others as favourably, and more favourably, structured than they are, and so on. In this case we may assume the survival of a useful variation, and are not compelled necessarily to assume that the used organ transmitted the effect of its usage on succeeding generations.

Many cases, indeed, exist of which no explanation can be given from the selective point of view, such, for instance, as the effect of climate upon the wool of sheep. In these cases, however, the information we possess is often of a slight nature, so that judgment can hardly be given at present, and all that we can say is that the more closely these cases are studied the greater is the number which suffer a ready explanation as having been brought into existence through selection. This being so, the direct evidence we possess that experimentally induced changes, such as the effects of mutilation, and compressions of various parts of the body, are in no cases transmitted, while we can vary an animal or plant, almost at will, by selective agencies, points strongly to selection as being the prime producer of racial change, while the direct action of the environment, and the transmission of its effects, are of little, if any, account. Mutilations practised upon male infants by Jews, and other Semitic races for thousands of years, have still always to be repeated, for the lost parts are reproduced as at the earliest periods of the race's history. Many breeds of dogs and sheep have been for many generations systematically docked, and yet the young are born with as long tails as those of other breeds. Chinese women have compressed their feet from times long back in their history, yet Chinese infants are not born with small feet. More curious still, as affecting an organ of paramount importance—the brain—a certain tribe of Indians artificially

flatten their heads in early life, entirely changing not only the shape of the skull, but also that of the brain itself, and yet their children are born with rounded heads.

We are bound, then, to admit that the thousands of germinal cells lying in the body of the parent are, like other cells of the body, of slightly different constitution; they vary among themselves in size and constitution. They live and are kept alive in the organ where they lie; and the rest of the parent organisms may be mutilated, compressed, suffer change in parts from use or disuse, from climatic or other conditions, and the germ cells rest where they are unchanged. If they develop, the resulting individuals will be what the germs had it in their own nature to be, uninfluenced by changes induced in the body of the parent.

But the ovum may be affected, as all admit it to be, by changes in the blood. The environment is not therefore without action upon the germinal cells, inasmuch as the parent may be thrown by adverse conditions into unhealthy states of body, under which circumstances the tissues, including the germ cells, will all alike suffer from want of nutrition.

We may sum the whole subject up by saying that a constitutional, but not a purely local, disturbance may affect a germ cell. One man may use his arm, another his leg, and these members will hypertrophy in consequence. The voice may be trained, or skill in music may be acquired, or reasoning obtained during the lifetime of an individual without affecting the constitution of the body; while, on the other hand, starvation or bad food will impoverish the blood and the whole system, and good food and healthy exercise will enrich the blood and endow it with energy; whilst acquired zymotic disease will produce general as well as local symptoms. In the former cases the germ cells will be unaffected; in the latter they may suffer change, in common with other tissues, depending for their vitality as they all do upon the general well-being of the body. Both Galton and Weismann clearly draw the line that I have indicated; and when they state that the environmental changes produced in an individual are not transmitted, they positively exclude those cases where the environment may act directly through the blood upon the germinal cells.

Whereas the tendency is strongly in the direction of showing that variation cannot be acquired by the germ as the result of environmental changes of a local kind in the parent, and that all such apparent cases are explainable by selection, the evidence that variations can be acquired from constitutionally acquired parental changes is quite conclusive. Both among plants and animals the change from a better to a poorer food will in the individual lifetime diminish the size of the individual, affect the richness and quality of the blood and sap, and so modify the size of the germinal cells which depend for their growth and sustenance upon the blood or sap.

We cannot state that in no case does a local change produced by the environment, acting directly upon an organism, induce corresponding change in the germ cells within that organism. We can, however, affirm that while the evidence is here not sufficiently complete to convince a majority of naturalists, everyone is agreed that the environment by selection can do almost anything in the way of producing racial change.

Environment may therefore act upon a race in two different ways. In the first case it may give a preference to certain innate qualities of certain members of the race, and so determine the production of the race from these. We have also undeniable evidence that the environment can vary the size and general well-being of a race through the parental blood, and in the same way we shall see that poisons or micro-organisms introduced into the blood may cause qualitative changes in the germ cells which will appear in the individuals which develop from them as disease, or as predisposition to disease.

(To be continued.)

ITALIAN DERMATOLOGICAL SOCIETY.—At a meeting of the leading dermatologists of Italy held in Rome on December 29th, 1893, the statutes of the new Dermatological Society were finally approved. Professor De Amicis was elected President and Professor Barduzzi Secretary.

⁴ *Journal of the Anthropological Institute*, vol. v, pp. 344-47.

⁵ *Natural Inheritance*, 1889, p. 14.