secondary bows formed by light from the real sun reflected from the water after leaving certain drops; primary and secondary formed by light from the sun reflected at the water, and, after leaving certain other drops, again reflected at the water. I have called the latter four distinct bows, because, although they looked like reflections of a solid set of four arcs, they were really formed by means of drops distinct from those which helped to make the first four bows. I append a sketch of what I saw.

PERCIVAL FROST.

15 Fitzwilliam Street, January 29.

[We have received other letters on the subject of Mr. Scouller's letter.]

Thought and Breathing.

I SEND you some extracts from the Sanskrit Yoga-sûtras which treat very fully of the prâ*n*âyâma, or the expulsion and retention of breath, as a means of steadying the mind.

A Yogi has first of all to assume certain postures which help him to fix his mind on certain objects. He cannot concentrate his mind while walking or running. He ought to assume a firm and pleasant position, one requiring little effort. To judge, however, from the description given of some of these postures, they would seem to us anything but pleasant.

When a Yogi has accustomed himself to his posture, he begins to regulate his breath—that is, he draws in the breath through one nostril, retains it for some time in the chest, and then emits it through the other nostril. The details of this process are given in the first chapter of the Yoga-sûtras, sûtra 37. Here the commentator states that the expulsion means the throwing out of the air from the lungs in a fixed quantity through a special effort. Retention is the restraint or stoppage of the motion of breath for a certain limited time. That stoppage is effected by two acts—by filling the lungs with external air, and by retaining therein the inhaled air. Thus the threefold prâ*n*âyâma, including the three acts of expiration, inspiration, and retention of breath, fixes the thinking principle to one point of concentration. All the functions of the organs being preceded by that of the breath —there being always a correlation between breath and mind in their respective functions—the breath, when overcome by stopping all the functions of the organs, effects the concentration of the thinking principle to one object.

Râjendralal Mitra, to whom we owe a very valuable edition of the text and translation of the Yoga-sûtras, adds the following remarks:—"All other Yogic and Tantric works regard the three acts of expiration, inspiration, and retention performed in specific order to constitute prâ*n*âyâma. The order, however, is not always the same. . . . The mode of reckoning the time to be devoted to each act is regulated in one of two ways: (1) by so many repetitions of the syllable om, or the mystic mantra (formula) of the performer, or the specific mystic syllables (vîja) of that mantra; (2) by turning the thumb and the index-finger of the left hand round the left knee a given number of times. The time devoted to inspiration is the shortest, and to retention the longest. A Vaish*n*ava in his ordinary daily prayer repeats the Vîja-mantra once while expiring, 7 times while inspiring, and 20 times while retaining. A Sâkta repeats the mantra 16 times while inspiring, 64 times while retaining, and 32 times while expiring. These periods are frequently modified."

The usual mode of performing the prâ*n*âyâma is, after assuming the posture prescribed, to place the ring-finger of the right hand on the left nostril, pressing it so as to close it, and to expire with the right, then to press the right nostril with the thumb, and to inspire through the left nostril, and then to close the two nostrils with the ring finger and the thumb, and to stop all breathing. The order is reversed in the next operation, and in the third act the first form is required. The Ha*th*adîpikâ says:—"By the motion of the breath, the thinking principle moves; when that motion is stopped, it becomes motionless, and the Yogi becomes firm as the trunk of a tree; therefore the wind should be stopped. As long as the breath remains in the body, so long it is called living. Death is the exit of that breath, therefore it should be stopped."

Some of the minor works on Yoga expatiate on the sanitary and therapeutic advantages of practising prâ*n*âyâma regularly at stated times. In America some spiritualistic doctors prescribe the same practice for curing diseases.

In India prâ*n*âyâma is only a means towards a higher object—namely, the abstraction of the organs from their natural functions. It is a preliminary to Yoga, which consists in *dhâranâ*, steadfastness, *dhyâna*, contemplation, and *samâdhi*, meditation, or almost a cataleptic trance. These three are supposed to impart powers or *siddhis* which seem to us incredible, but which nevertheless are attested by the ancient Yogis in a very *bonâ-fide* spirit, and deserve examination, if only as instances of human credulity. I say nothing of modern impostures.

Oxford, January 22. F. MAX MÜLLER.

IN connection with Prof. Leumann's recent researches into the relation between changes in respiration and changes in certain cerebral functions, it seems curious that the employment of deep and rapid respiration as an anæsthetic has received so little attention. Some dentists order their patients to respire as quickly and fully as they can for a period which varies, I believe, from four to six minutes, although as to the exact duration I am insufficiently informed. At the termination of this period the patient becomes giddy, and to a great extent loses consciousness, when a short operation can be painlessly performed. The patient, while unable to move his arms, opens his mouth at the order of the operator. I have heard of no casualties or evil effects from this mode of treatment. W. CLEMENT LEY.

Chiff-Chaff singing in September.

DURING more than forty years' observation of the singing of birds, I have invariably heard the chiff-chaff singing in September, although the song is much less frequently repeated than in the spring. In connection with this observation I may mention that both the male and female birds appear to be always mute for two or three days after their spring arrival in Northern Europe. W. CLEMENT LEY.

Lutterworth, January 31.

Foreign Substances attached to Crabs.

I HAVE read in recent numbers of NATURE some letters on sponges attached to crabs.

There are two crabs on the east coast of Australia—one of them allied to *Dromia vulgaris*—which cover themselves with sponges or with a composite Ascidian. I have in one case counted no less than seven species of sponges on one individual crab.

The Ascidian referred to is usually from ten to thirty times as large as the crab to the back of which it is attached.

Among the specimens brought by me from Australia, and now deposited in the National Collection of the British Museum, there are some of these crabs with sponges and Ascidians attached.

These might, perhaps, be interesting to your correspondents on the subject. R. V. LENDENFELD.

University, Innsbruck, January 25.

Foot-Pounds.

"A. S. E." will find moments, of resistance, of bending, or of turning, expressed in foot-pounds (often inch-pounds or foot-tons) in any treatise on civil, mechanical, or marine engineering, on architecture, land or naval, and, in fact, in every treatise on *real* mechanics he may consult. Why, then, should a different terminology be adopted in a Civil Service examination paper? In metric units, moments are given in kilogramme-metres or -centimetres; but in the C.G.S. system I do not suppose it is suggested to measure moments of dyne-centimetres in ergs.

February 3. A. G. GREENHILL.

IF "A. S. E." will push his researches further, he will find that in Government dockyards the stability moment on ships is calculated in foot-tons. V.

February 3.

PROF. WEISMANN'S THEORY OF HEREDITY.

IN NATURE of October 24, 1889 (p. 621), appeared a criticism by Prof. Vines of my essays on heredity and allied subjects. I should be glad to reply briefly to his objections, and the more so as I hope thus to be able to place the scientific problems at issue in a somewhat clearer light. With regard to the immortality which I attribute both to the unicellular organisms and to the germinal cells of the multicellular, if I understand Prof. Vines aright, he does not attack the proposition itself, but has simply overlooked the explanation in my book of the way in which mortal organisms arose out of immortal in process of phyletic development, a process which must have taken place if the Protozoa have developed in the course of the world's history into the higher Metazoa,—"the first difficulty is to understand how the mortal heteroplastides can have been evolved from the immortal monoplastides." My explanation was simply that which appears to be the true one for the origin of every higher differentiation—namely, the division of the cell-mass of the Protozoan, on the principle of the division of labour, into two dissimilar halves, differing in substance, and consequently also in function; from the one cell which performed all functions comes a group of several cells which distribute themselves over the work. In my opinion, the first such differentiation produced two sets of cells, the one the mortal cells of the body proper, the other the immortal germ-cells. Prof. Vines certainly believes in the principle of the division of labour, and in the part that it has played in the development of the organic world, as well as I; but it seems to him that this division of a unicellular being into somatic and germinal cells is impossible, and that my explanation of the process by dissimilar division is inadequate, because it strikes him as "absurd to say that an immortal substance can be converted into a mortal substance."

There certainly does seem to be a great difficulty in this idea, but in reality it arises simply from a confusion of two conceptions—immortality and eternity. That the Protozoa and the germ-cells of Metazoa are in a certain sense immortal seems to me an incontrovertible proposition. As soon as one has clearly realized that the division of a monoplastid is in no way connected with the death of one part, there can be no further question that we have to do with individuals of indefinite duration; but this in no way implies that they possess an eternal duration; on the contrary, we imagine that they have all had a beginning. The conception of eternity, however, extends into the past as well as the future; it is without beginning or end, and does not affect the present question; it is an entirely artificial conception, and has no real and comprehensible existence; to express it more accurately, eternity is merely the negation of the conception of transitoriness. Of the objects with which natural science deals, none are eternal except the smallest particles of matter and their forces, certainly not the thousandfold semblances and combinations under which matter and force meet us. As I have said years ago, the immortality of unicellular organisms, and of the germ-cells of the multicellular, is not absolute but potential; it is not that they *must* live for ever as did the gods of the ancient Greeks—Ares received a "mortal" wound, and roared for pain like to ten thousand bulls, but could not die; they can die—the greater number do in fact die—but a proportion lives on which is of one and the same substance with the others. Does not life, here as elsewhere, depend on metabolism—that is to say, a constant change of material? And what is it, then, which is immortal? Clearly not the substance, but only a definite form of activity. The protoplasm of the unicellular animals is of such chemical and molecular structure that the cycle of material which constitutes life returns even to the same point and can always begin anew, so long as the necessary external conditions are forthcoming. It is like the circulation of water, which evaporates, gathers into clouds, and falls as rain upon the earth, always to evaporate afresh. And as in the physical and chemical properties of water there is no inherent cause for the cessation of this cycle, so there is no clear reason in the physical condition of unicellular organisms why the cycle of life, *i.e.* of division, growth by assimilation, and repeated division, should ever end; and this characteristic it is which I have termed immortality. It is the only true immortality to be found in Nature—a pure biological conception, and one to be carefully distinguished from the eternity of dead, that is to say unorganized, matter.

If then this true immortality is but cyclical, and is conditioned by the physical constitution of the protoplasm, why is it inconceivable that this constitution should be, under certain circumstances and to a certain extent, so modified that the metabolic activity no longer exactly follows its own orbit, but after more or fewer revolutions comes to a standstill and results in death? All living matter is variable; why should not variations in the protoplasm have also occurred which, while they fulfilled certain functions of the individual economy better, caused a metabolism which did not exactly repeat itself, *i.e.* sooner or later came to a condition of rest? I admit that I feel such a descent from immortality into mortality far less remarkable than the permanent retention of immortality by the monoplastids and germ-cells. Small, indeed, must be the variations in the complicated qualities of living matter to bring in their train such a fall; and very sharply must the essentials of its constitution be retained, for metabolism to take place so smoothly without creating in itself an obstacle to its own continuance! Even if we cannot penetrate into the mysteries of this constitution, still we may say that a rigorous and unceasing natural selection is unremittingly active in maintaining it at such an exact standard as to preserve its immortality; and every lapse from this standard is punished by death.

I believe that I have proved that organs no longer in use become rudimentary, and must finally disappear solely by "panmixie"; not through the direct action of disuse, but because natural selection no longer maintains their standard structure. What is true for an organ is true also for its function, since the latter is but the expression of the qualities of material parts, whether we can directly perceive their relations or not. If, then, as we saw, the immortality of monoplastids depends on the fact that the incessant metabolism of their bodies is ever returning exactly to its starting-point, and produces no such modifications as would gradually obstruct the repetition of the cycle, why should that quality of the living matter which causes immortality—nay, how *could* it be retained—when no longer necessary? It is obvious that it was no longer necessary in the somatic cells of the heteroplastids. From the instant that natural selection relaxed its watch on this quality of immortality began the process of panmixia which led to its abolition. Prof. Vines will ask, How can one conceive of this process? I answer, Quite easily. When once individuals arose among monoplastids, in the protoplasm of which occurred such variation in chemical and molecular constitution as to result in a gradual check on the metabolic cycle, it would happen that these individuals died; a permanent variety could not grow out of such variations. But if there arose among heteroplastids individuals with a similar differentiation of the somatic cells, the death of these cells would not be detrimental to the species, since its continuance is ensured by the immortal germ-cells. Upon the differentiation into germinal and somatic cells, natural selection was, speaking metaphorically, trained to bear on immortality of the germ-cells, but on quite other qualities in the somatic cells—on motility, irritability, capacity for assimilation, &c. We do not know whether the attainment of these qualities was accompanied by a constitutional alteration which caused the loss of immortality, but it is at least possible; and, if true, the somatic cells will have lost their immortality even more rapidly than through the unaided action of panmixia.

In the fourth essay of my book, I have cited the two Volvocinean genera Pandorina and Volvox as examples of the differentiation of homoplastids into the lowest heteroplastids; in Pandorina the cells are still all alike and all perform the same functions, in Volvox occur somatic and germinal cells, and in the latter case we should expect to find the commencement of natural death. Recent researches of Dr. Klein ("Morphologische und biologische Studien über die Gattung Volvox," *Jahrb. wiss. Botan.*, xx., 1889) show that this is actually the case; as soon as the germ-cells are ripe and emerge from the sphere, the ciliated somatic cells begin to shrivel up, and die in one or two days. This is the more interesting, as the somatic are also the nutritive cells; for, though the germ-cells also possess chlorophyll, the rapid growth of the latter (which attain an enormous size in Volvox) is only possible by the supply of nourishment from the somatic cells. The latter are so constituted that they assimilate, but cannot grow larger when once the sphere has reached its definite size; they transfer the nourishment which they derive from the decomposition of carbon dioxide, &c., to the germinal cells by means of fine pseudopodia; and themselves wither when once the germs are ripe. In this case adaptation to the nutrition of the germinal cells might well have accelerated the introduction of a natural death of the somatic cells, the capacity for considerable assimilation combined with a drain on their nutrition may have led after a certain time to stoppage of the process of assimilation and to death. To me, the idea that modification of the living matter may have been connected with loss of immortality does not appear more unlikely or more difficult than the generally received view of the gradual differentiation of the somatic cells in the course of phylogeny into their various species of digestive, secretive, motile, and nervous cells. An immortal unalterable living substance does not exist, but only immortal *forms of activity* of organized matter.

I maintain, therefore, in its entirety, my original statement, that monoplastids and the germ-cells of higher forms have no natural death. I do not know how this can to-day be better expressed than by saying that these living units possess a real and actual immortality as against the imaginary ideal immortality of the Greek gods. If death from internal causes does not exist for them, one may yet say with certainty that the fatal hour will one day strike for them all, not from internal causes, but because the external conditions for the constant renewal of vital activity will some day cease. The physicists prophesy that the circulation of water on the globe will end, not from any alteration in the qualities of water, but because external conditions will render this form of motion of aqueous particles impossible.

Prof. Vines then attacks my view of embryogeny. He finds it "not a little remarkable that Prof. Weismann should not have offered any suggestion as to the conception which he has formed of the mode in which the conversion of germ-plasm into somatoplasm can take place, considering that this assumption is the key to his whole position." He sees here the same difficulty as in the phyletic development, and says: "There is really no other criticism to be made on an unsupported assumption such as this, than to say that it involves a contradiction in terms." He means by this that the eternal cannot pass into the finite, as must be the case if the immortal germ-cell grow into the mortal soma. At the bottom of this objection lies the same confusion between immortality and eternity which has already been made clear. I do not wish to reproach Prof. Vines with this obscurity, as I felt the same objection myself for many years, and could not at once discover the reply to it; on the contrary, I am indebted to him for the opportunity to express myself on the point. Up to this time we have had no scientific conception of immortality; if this be accepted, the significance of immortality is not life without beginning or end, but life which, after its first commencement, can continue indefinitely with or without modification (specific changes in the germ-plasm or the monoplastids); it is a cyclical activity of organic material devoid of any intrinsic momentum which would lead to its cessation, just as the motion of the planets contains no intrinsic momentum which would lead to its cessation, although it has had a commencement and will some day, through the operation of extrinsic forces, have an end.

Prof. Vines says later: "I understand Prof. Weismann to imply that his theory of heredity is not—like, for instance, Darwin's theory of pangenesis—a provisional or purely formal solution of the question, but one which is applicable to every detail of embryogeny, as well as to the more general phenomena of heredity and variation." I have, as a matter of fact, designated Darwin's pangenesis as a "purely formal" solution of the question, but should like here to give a slight explanation of the expression, as I fear that not only Prof. Vines, but also many other readers of my essays, have misunderstood me. On the one hand, I am afraid that they see in my words a definite reproach against Darwin for his theory of pangenesis, of which I had not the remotest intention; and on the other, that they incline to charge me with too great an affection for my own theory.

I believe there are two kinds of theory; one may term them the "real" and the "ideal"; practically they are rarely sharply to be discriminated; both often occur in one and the same theory, but should be conceived of separately. The "ideal" theories attempt to render conceivable the phenomena to be explained by an arbitrarily accepted principle, apart from the question whether the principle itself possesses any grain of truth or not; they seek only to show that there are hypotheses on which the phenomena in question become comprehensible. "Real" theories do not make hypotheses at pleasure, but strive to construct such as have some degree of probability; they desire to give not a formal, but, if possible, the right explanation. Sir William Thomson in endeavouring to make clear the dispersion of rays of light, never believed in the remotest degree that such molecules as he pictured really existed, but desired merely to show that there were hypotheses on which the phenomena of dispersion were comprehensible. Darwin's pangenesis was originally intended in this sense, and was by him termed a "provisional" hypothesis, although in later years he may have attributed to it the weight of a real theory. To me his "gemmules" are a pure invention, an invention in no way corresponding to the actual facts, but showing what hypotheses must be made in order to explain the phenomena of heredity. Are, however, such ideal theories worthless? Certainly not. They are often the first and essential step towards the understanding of complicated phenomena, and lay the foundation for the gradual erection of a real theory. It would perhaps never have occurred to me to deny the inheritance of acquired characters, had not Darwin's pangenesis shown me that the matter was only explicable on an hypothesis so difficult to conceive, as that of the giving off, circulation, and reassemblage of gemmules. I do not even now maintain that Darwin's pangenesis cannot possibly contain a kernel of truth; De Vries ("Intracellulare Pangenesis," Jena, 1889) has shown in a recent and most interesting memoir that the ideal impossible pangenesis may be transformed into a real and possible one by means of certain profound modifications; he accepts my view that acquired (somatogenic) modifications cannot be transmitted, and thereby puts on one side just that part of Darwin's theory which has always appeared to me to lie beyond the pale of reality—namely, the circulation, &c., of the gemmules. The future will show whether his view of modified gemmules or my hypothesis is the best explanation of the facts of heredity.

In any case, I am far from assuming that I have settled the whole question of heredity; I have undertaken researches on some of the more important parts of the problem, and have thus been compelled to formulate some fundamental principles for the explanation of the phenomena; but no one can be more convinced than I how far we are from a definite and complete explanation, not only of "every detail," but also of "the more general phenomena." My endeavour was to put forth a real, in place of the previous ideal, theory; and on this ground I took pains to make only such suppositions as might possibly correspond to actual facts. There certainly is a material carrier of heredity in the ovum; it certainly can be transported from nucleus to nucleus; it certainly can be modified in the process, or can remain the same; and even the supposition that it is able to stamp its own character on the cell contains nothing which seems to us impossible and non-existent; on the contrary, we are able now to state that it is so, even if we do not understand in what wise it happens. My hypothesis relative to the quiescent state of germ-plasma also rests on a basis of fact; we know that ancestral characteristics may be transmitted in a latent condition, and that the process of transmission is bound up with a substance, the idioplasma; there must therefore actually be an inactive stage of idioplasma.

If it could be shown that upon such principles an explanation of heredity is attainable, we should have made a distinct advance upon the ideal theory of pangenesis which is founded on unreal hypotheses. Possibly it is upon the path which I have opened up that we shall gradually attain a satisfactory solution of the numerous questions at issue; possibly further research will show that it is not the right path, and must be abandoned; no one, it appears to me, can foretell this. My reflections on heredity are not a conclusion, but a commencement—no complete theory of heredity which claims to provide a complete solution of all the problems at issue, but *researches* which, if fortunate, may sooner or later, by direct or circuitous paths, lead to a true appreciation of the question, to a "real" theory. In the preface to the English edition of my "Essays" I have stated this expressly.

I have also in that place distinctly insisted that the book was not written as a whole; that it consists rather of a series of researches, the one growing out of the other, and showing the development of my views as they shaped themselves during the course of nearly a decade's work. It is therefore unreasonable to extract ideas from an earlier essay and apply them against a later one. I have left them unaltered, and even "left certain errors of interpretation uncorrected," because, if altered, their internal connection could not have been understood.

I believe that the objections which Prof. Vines makes to my theory of the continuity of germ-plasma rest solely on an unintentional confusion of my ideas, as he compares the opinions expressed in the second essay with those of the later ones, with which they do not tally. I will endeavour to make this clear. In this second essay (1883) I contrasted the body (soma) with the germ-cells, and explained heredity by the hypothesis of a "Vererbungs-substanz" in the germ cells (in fact the germ-plasma), which is transmitted without breach of continuity from one generation to the next. I was not then aware that this lay only in the nucleus of the ovum, and could therefore contrast the entire substance of the ovum with the substance of the body-cells, and term the latter "somatoplasm." In Essay IV. (1885) I had arrived, like Strasburger and O. Hertwig, at the conviction that the nuclear substance, the chromatin of the nuclear loops, was the carrier of heredity, and that the body of the cell was nutritive but not formative. Like the investigators just named, I transferred the conception of idioplasma, which Nägeli had enunciated in essentially different terms, to the "Vererbungs-substanz" of the ovum-nucleus, and laid down that the nuclear chromatin was the idioplasma not only of the ovum but of every cell, that it was the dominant cell-element which impressed its specific character upon the originally indifferent cell-mass. From then onwards, I no longer designated the cells of the body simply as "somatoplasm," but distinguished, on the one hand, the idioplasm or "Anlagen-plasma" of the nucleus from the cell-body or "Cytoplasma," and, on the other, the idioplasm of the ovum-nucleus from that of the somatic cell-nucleus; I also for the future applied "germ-plasm" to the nuclear idioplasm of ovum and spermatozoon, and "somatic idioplasm" to that of the body-cells (*e.g.* p. 184). The embryogenesis rests, according to my idea, on alterations in the nuclear idioplasma of the ovum, or "germ-plasm"; on p. 186, *et seqq.*, is pictured the way in which the nuclear idioplasm is halved in the first cell-division, undergoing regular alterations of its substance in such a way that neither half contains all the hereditary tendencies, but the one daughter-nucleus has those of the ectoblast, the other those of the entoblast; the whole remaining embryogenesis rests on a continuation of this process of regular alterations of the idioplasma. Each fresh cell-division sorts out tendencies which were mixed in the nucleus of the mother-cell, until the complete mass of embryonic cells is formed, each with a nuclear idioplasm which stamps its specific histological character on the cell.

I really do not understand how Prof. Vines can find such remarkable difficulties in this idea. The appearance of the sexual cells generally occurs late in the embryogeny; in order, then, to preserve the continuity of germ-plasm from one generation to the next, I propound the hypothesis that in segmentation it is not *all* the germ-plasm (*i.e.* idioplasm of the first ontogenetic grade) which is transformed into the second grade, but that a minute portion remains unaltered in one of the daughter-cells, mingled with its nuclear idioplasm, but in an inactive state; and that it traverses in this manner a longer or shorter series of cells, till, reaching those cells on which it stamps the character of germinal cells, it at last assumes the active state. This hypothesis is not purely gratuitous, but is supported by observations, notably by the remarkable wanderings of the germinal cells of Hydroids from their original positions.

But let us neglect the probability of my hypothesis, and consider merely its logical accuracy. Prof. Vines says:—"The fate of the germ-plasm of the fertilized ovum is, according to Prof. Weismann, to be converted in part into the somatoplasm [!] of the embryo, and in part to be stored up in the germ-cells of the embryo. This being so, how are we to conceive that the germ-plasm of the ovum can impress upon the somatoplasm [!] of the developing embryo the hereditary character of which it (the germ-plasm) is the bearer? This function cannot be discharged by that portion of the germ-plasm of the ovum which has become converted into the somatoplasm [!] of the embryo *for the simple reason that it has ceased to be germ-plasm*, and must therefore have lost the properties characteristic of that substance. Neither can it be discharged by that portion of the germ-plasm of the ovum which is aggregated in the germ-cells of the embryo, for under these circumstances, it is withdrawn from all direct relation with the developing somatic cells. The question remains without an answer." I believe myself to have answered this above. I do not recognize the somatoplasm of Prof. Vines; my germ-plasm or idioplasm of the first ontogenetic grade is not modified into the somatoplasm of Prof. Vines, but into idioplasm of the second, third, fourth, hundredth, &c., grade, and every one impresses its character on the cell containing it.

Prof. Vines also attacks my view of the idioplasmatic nature of the *nuclear* substance (the chromatic grains); and maintains that it is as easy to speak of the continuity of the cell-body as of that of the nuclear substance, and that the one may transmit heritable qualities to progeny as well as the other. I quite understand that a botanist may easily be led to this view; and Prof. Vines is not the only one to hold it. Waldeyer (" Ueber Karyokinese und ihre Beziehung zu den Befruchtungs-vorgänge," *Arch. mikr. Anat.*, xxxii., 1888) has considered the observed facts insufficient to justify the regarding of the nuclear loops as idioplasm ; Whitman (" The Seat of Formative and Regenerative Energy," Boston, 1888) among zoologists has expressed himself against this view, and the same occurs in the recent book of Geddes and Thomson (" The Evolution of Sex," London, 1889). The facts which led me to the idea that the nuclear threads were the real carriers of heredity—were, in fact, the idioplasma—are enumerated in Essay IV. ; they were primarily the observations of E. van Beneden on the phenomena of fertilization in the ovum of *Ascaria megalocephala*, those of Strasburger on fertilization in the Phanerogams by a mere nucleus, and the researches of Nussbaum and Gruber on division in the Infusoria. One may further cite as of essential importance the facts of karyokinesis *per se*, and the circumstance that, only on the supposition that the nucleus contains the idioplasma can the extrusion of polar bodies from the animal ovum be rendered comprehensible. The latter process divides the nuclear substance of the ovum into two quantitatively equal halves, but the body of the ovum into two unequal halves, the size of which is different in every species. The essential part of the process must therefore be the division of the nuclear substance, not that of the cell-mass. These facts on reflection so completely convinced me that the nucleus alone acts as carrier of hereditary tendencies, that the theory of the physiological equality of the nuclei of the sexual elements which I had propounded ten years before (1873) struck me as a certainty ; and I then advanced the theory of fertilization which is contained on p. 246 of Essay IV. I believe that till recently Strasburger and I alone had expressed similar views of the essence of fertilization, at least so far as relates to the homodynamy of the sexual nuclei. That most distinguished observer, E. van Beneden, who has won such renown in the investigation of the process of fertilization, took his stand with regard to its theoretical significance on the platform of the older view, which regarded it as the union of two elements intrinsically and essentially the opposite of each other. He could not free himself from that dominant and deeply rooted idea, that the difference between the sexes is something fundamental, an essential principle of existence. The fertilized oosperm is in his eyes a hermaphrodite object, uniting in itself both male and female essences, an idea in which many other observers (cf. Kölliker, " Die Bedeutung der Zellenkerne für die Vorgänge der Vererbung," *Zeit. wiss. Zool.*, xlii., 1885) have followed him, and of which the logical sequence is that all the cells of the body are to be regarded as hermaphrodite !

Van Beneden was also influenced by the idea which sways the naturalists of so many countries, that fertilization is a process of rejuvenescence, in the sense that without it life cannot be prolonged to the end. Many still hold to this idea ; Maupas (" Recherches expér. sur la multiplication des infusoires ciliés," *Arch. zool. exp. gén.*, (2) vi. p. 165) very recently believed that he had found a proof of its correctness, and attempted to show that Infusoria, for a continuance of existence, must from time to time enter into conjugation, or die from internal causes if this conjugation be prevented. Even were his observations correct, they would still fall short of proving his conclusions ; they would prove nothing against the immortality of the Protozoa, or for a rejuvenescence in the sense here intended ; they would rather state the platitude that ovum and spermatozoon must die, if the condition of their continued existence, namely fusion, inevitable in most species of plants and animals, be prohibited ; but this is an accidental, not a natural, death. Richard Hertwig (" Ueber die Conjugation der Infusorien," München, 1889) has also briefly shown that the facts, on which Maupas bases his inference, are not universally true ; that Infusoria hindered from conjugation do not die, but increase by division, and may produce whole colonies of animals—nay, that they are generally thus rendered abnormally prolific.

I am distinctly opposed to the rejuvenescence theory, whether applied to unicellular or multicellular organisms ; my view is expressed in Essay IV., and may be summarized in this position—we should no longer speak of the conjugating nuclei of the sexual elements as male and female, but as *paternal* and *maternal*, there is no opposition of the one to the other, they are essentially alike, and differ only so far as one individual differs from another of the same species. Fertilization is no process of rejuvenescence, but merely a union of the hereditary tendencies of two individuals ; tendencies which are bound up with the matter of the nuclear loops ; the cell-body of the ovum and spermatozoon is indifferent in this connection, and plays merely the part of a nutritive matter which is modified and shaped by the dominant idioplasm of the nucleus in a definite way, as clay in the sculptor's hand. The different appearance and function of ovum and spermatozoon, and their mutual attraction, rest on secondary adaptations, qualified to ensure that they shall meet and that their idioplasmata shall come into contact, &c. ; and as with the cells, so the differentiation of *persons* into male and female is also secondary ; all the numerous differences of form and function which characterize sex in the higher animals, the so-called " secondary sexual characters," which reach even into the highest spiritual regions of mankind, are nothing but adaptations to ensure the union of the hereditary tendencies of two individuals.

These are briefly the views of fertilization which I have indicated since 1873, but have only published in a finished and definite shape since the discovery by van Beneden of the morphological processes in the fertilization of the ovum of Ascaris (Essay IV., 1885). I concluded then with these words :—" If it were possible to introduce the female pro-nucleus of an egg into another egg of the same species, immediately after the transformation of the latter into the female pro-nucleus, it is very probable that the two nuclei would conjugate just as if a fertilizing sperm-nucleus had penetrated [the ovum]. If this were so, the direct proof that egg-nucleus and sperm-nucleus are identical would be furnished. Unfortunately the practical difficulties are so great that it is hardly possible that the experiment can ever be made ; but such want of experimental proof is partially compensated by the fact, ascertained by Berthold, that in certain Algæ (Ectocarpus and Scytosiphon) there is not only a female, but also a male parthenogenesis ; for he shows that in these species the male germ-cells may sometimes develop into plants, which however are very weakly."

I have since attempted to fertilize one frog's egg with the nucleus of another ; the experiment was, as one would expect, not successful, owing to the enormous havoc caused by introducing a cannula into the egg ; but Boveri (" Ein geschlechtlich erzeugter Organismus ohne mütterliche Eigenschaften," *Ges. Morph. Physiol. München*, 16 Juli, 1889) was more fortunate, in finding an object which allowed of the converse experiment to mine ; following Hertwig's example, he removed the nucleus from an Echinoid ovum by agitation, and brought such denucleated ova to develop by introducing spermatozoa. From the spermatozoan nucleus was formed a regular segmentation-nucleus, the embryogenesis pursued its regular course, and there was formed a complete though small free-swimming larva, which lived for a week. From this experiment alone it follows that the views of Strasburger and myself on fertilization are correct, *viz.* that the sperm-nucleus can play the part of ovum-nucleus and *vice versâ*, and the older view, to which Prof. Vines (" Lectures on the Physiology of Plants," Cambridge, 1886, pp. 638-681) has also sworn allegiance, must be given up.

An interesting and important modification of Boveri's experiment confirmed both this experiment, and also, if it were necessary, the recognition of the nuclear substance as idioplasm, as maintained by O. Hertwig, Strasburger, and myself. If eggs of *Echinus microtuberculatus*, when artificially deprived of their nuclei, be fertilized with the spermatozoa of *Sphærechinus granulatus*, *larvæ are developed with the true characters of the second species*—that is to say, they have derived everything from the father, nothing from the mother; the nuclear substance alone it is which transmits heredity, and by it the cell-mass is dominated.

I have interpreted the first polar body of the Metazoan ovum as a carrier of ovogenous plasm, which has to be removed from the ovum in order that the germ-plasm may attain the predominance. It is possible that this explanation is not correct; the most recent researches on the conjugation of Infusoria, as expressed in the splendid memoirs of Maupas and R. Hertwig, argue against my interpretation; but the idea which lay at the bottom of this explanation is justified. As it is the nuclear matter which gives to the cell-body its specific character, the ovum must, previous to fertilization, be dominated by a different idioplasm to the sperm-cell, since they are, up to this point, different in appearance and function. On the other hand, when they have united, they contain the same idioplasm—namely, germ-plasm; the consequence is that the first dominant idioplasm is different to that of a later period. This was the idea at the bottom of my explanation of the first polar body, and it is correct. One might perhaps imagine that the idioplasmata of ovum and spermatozoon were originally different, but that both possessed the power of alteration into germ-plasm; but it would be then incomprehensible why parthenogenetic ova should expel one polar body. Both facts, however, are explicable, if ovum and spermatozoon are dominated up to the period of maturation by different histogenetic idioplasmata with which a small quantity of germ-plasm is mingled, and if at a later period the former be removed and the germ-plasm come to rule in both cells. This process would be by no means abnormal and unparalleled, since entirely analogous divisions of the idioplasm into qualitatively dissimilar portions must occur hundreds of times in every embryogenesis. However, I am most willing to allow that the last word has not yet been said on this question, and would only maintain that my theory of heredity is not concerned thereby. It is not the interpretation of the first polar body, but that of the second, which is decisive; and one can none the less easily think of the latter as a halving of the number of ancestral germ-plasmata, even if it be proved that my explanation of the first polar body was erroneous. I would then express the first division merely as introductory to the second, as the necessary first step in the reduction of ancestral plasmata, the necessity for which we should thus perhaps learn to understand.

The regular modification of idioplasma during the ontogeny, which I have maintained and which so many have attacked (Kölliker[1] with special vehemence) will now stand out as justified. If the nucleus of a sperm-cell is capable of impressing on the denucleated mass of an ovum its own inherited tendencies, and of calling into being an organism with specific characteristics purely paternal, it will be found difficult to explain the ontogeny otherwise than as a regular modification of the idioplasm, continuous from one cell-division to another, which stamps on the body of each separate cell at each stage its peculiar character, not only with regard to shape but also to function, and especially with regard to the "rhythm" of cell-division.

[1] "Das Karyoplasma und die Vererbung: eine Kritik der Weismann'sche Theorie von der Continuität des Keimplasma's," *Zeit. wiss. Zool.*, xliv. p. 228, 1886.

A further objection is directed by Prof. Vines against my views on the origin of variation. In the fifth essay I have sought the significance of sexual reproduction in the fact that it alone could have called into existence that multiplicity of form of the higher animals and plants, and that constantly fluctuating union of individual variations, of which natural selection stood in need for the creation of new species. I am still of the opinion that the origin of sexual reproduction depends on the advantage which it affords to the operation of natural selection; nay, I am completely convinced that only through its introduction was the higher development of the organic world possible. Still, I am at present inclined to believe that Prof. Vines is correct in questioning whether sexual reproduction is the *only* factor which maintains Metazoa and Metaphyta in a state of variability. I could have pointed out in the English edition of my "Essays" that my views on this point had altered since their publication; my friend Prof. de Bary, too early lost to science, had already called my attention to those parthenogenetic Fungi which Prof. Vines justly cites against my views; but I desired, on grounds already mentioned, to undertake no alteration in the essays. Bessides, I was well aware when the essay was first committed to paper (1886) that my current view on the radical cause of variation was possibly incomplete; and so, in order to expose the truth of the view as far as possible to a general test, I drove its logical consequences home, and enunciated the statement that species reproducing parthenogenetically could not be modified into new species. I also began myself at that time experiments on the variation of parthenogenetic species which are still being continued, and on which on some future occasion I hope to be able to report.

Even if, however, from our present knowledge it is probable that sexual reproduction is not the sole radical cause of variability of the Metazoa, still no one will dispute that it is a most active means of heightening variations and of mingling them in favourable proportions. I believe that the important part which this method of reproduction has played in calling out the existing processes of selection, is hardly diminished, even if one grants that direct influences upon the idioplasm call forth a portion of individual variability. Prof. Vines even holds it probable "that the absence of sexuality in these plants [Fungi] may be just the reason why no higher forms have been evolved from them, for in this respect they present a striking contrast to the higher Algæ in which sexuality is well marked." But when Prof. Vines says, "there can be no doubt that sexual reproduction does very materially promote variation," he does not mean to say that this is a self-evident proposition; he is well aware that prominent investigators like Strasburger see in sexual reproduction the reverse action, that of maintaining the constancy of the specific character. But I gladly accept his agreement with my view, which confirms the main position of the fifth essay, which runs: Sexual reproduction has arisen by and for natural selection as the sole means by which individual variations can be united and combined in every possible proportion.

With reference also to the problem of the inheritance of acquired (somatogenic) characters, Prof. Vines is again my opponent; he holds that such inheritance is possible. I have denied it, because it did not appear to me self-evident—as was formerly universally assumed—but rather utterly unproven; and because I think that completely unfounded assumptions of such far-reaching consequence should not be made, when requiring a large number of improbable hypotheses for their explication. I have tested all the available evidence for such inheritance as accurately as I could, and have found that none has the value of proof. There is no inheritance of mutilations, and this constitutes up to now the only basis of fact for the supposition of the inheritance of somatogenic variations. If, in the last essay, I have not denied every possibility of such a transmission, Prof. Vines should interpret that in my favour, not to my discredit; it is not the business of an investigator to set forth a proposition, which on the existing evidence he is compelled to believe, as an infallible dogma. Prof. Vines finds my "statements of opinion so fluctuating that it is difficult to determine what [my] position exactly is," but he could have easily discovered my meaning, if, instead of promiscuously contrasting the eight essays and the eight years of their production, he had merely brought the last of them to the bar of judgment. This essay is especially concerned with "the supposed transmission of mutilations," and at its conclusion my verdict on the state of the problem of the inheritance of acquired characters is thus summarised:—"The true decision as to the Lamarckian principle [lies in] the explanation of the observed phenomena of transformation. . . . If, as I believe, these phenomena can be explained without the Lamarckian principle, we have no right to assume a form of transmission of which we cannot prove the existence. Only if it could be shown that we cannot now or ever dispense with the principle, should we be justified in accepting it." The distinguished botanist De Vries has proved that certain constituents of the cell-body, *e.g.* the chromatophores of Algæ, pass directly from the maternal ovum to the daughter-organism, while the male germ-cell generally contains no chromatophores. Here it appears possible that a transmission of somatogenic variation has occurred; in these lower plants, the separation between somatic and reproductive cells is slight, and the body of the ovum does not require a complete chemical and physical alteration to become the body of the somatic cell of the daughter. But how does this affect the question whether, for instance, a pianoforte player can transmit to his progeny that strength of his finger-muscles which he has acquired by practice? How does this result of practice arrive at the germ-cells? In that lies the real problem which those have to solve who maintain that somatogenic characters are transmissible.

It is proved by the observations of Boveri, quoted above, that among animals the body of the ovum contributes nothing to inheritance. If the transmission of acquired characters should take place, it would have to be by means of the nuclear matter of the germ-cells—in fact, by the germ-plasm, and that not in its patent, but in its latent condition.

To renounce the principle of Lamarck is certainly not the way to facilitate the explanation of the phenomena; but we require, not a mere formal explanation of the origin of species of the most comfortable nature, but the real and rightful explanation. We must attempt, therefore, to elucidate the phenomena without the aid of this principle, and I believe myself to have made a beginning in this direction. A short time ago I tried this in one of those cases where one would least expect to be able to dispense with the principle of modification by use—namely, in the question of artistic endowment.[1] I proposed to myself the question whether the musical sense of mankind could be conceived of as arising without a heightening of the original acoustic faculty by use. But even here I came to the conclusion that, not only do we not need this principle, but that use has actually taken no part in the development of the musical sense.

A. WEISMANN.

THE LIFE AND WORK OF G. A. HIRN.

THE three men who worked at the experimental determination of the mechanical equivalent of heat and at practical Thermodynamics have passed away within a few months of each other—Clausius, Joule, and now Hirn.

[1] "Gedanken über Musik bei Thieren und bei Menschen," *Deutsche Rundschau*, October 1889.

They were much of the same age, and began their experiments while young at almost the same time; and the practical agreement of the conclusions drawn from their experimental results is our best guarantee of confidence in the modern theory of Thermodynamics which is built upon these results.

Gustave Adolphe Hirn was born at Logelbach, in Alsace, on August 21, 1815, and died on January 14 of this year, a victim to the prevailing epidemic of influenza; but for this, we might have expected still further developments of his scientific theories, as he continued at work on his favourite subjects to the last.

Self-taught, so far as his scientific education was concerned, he found himself, with his elder brother Ferdinand, a manager of the works of Haussman, Jordan, and Co., an establishment for the fabrication of *indiennes*, established in 1772. Finding the machinery antiquated and worn out, Hirn, in setting to work to make the best of it, was really better placed for theorizing and experimentalizing than if he had charge of modern works in first-rate order. The different parts of the works being at a distance from each other, his brother Ferdinand brought out his system of cable transmission of power; and it was Gustave who pointed out theoretically the advantage of a thin light cable run at a high speed.

Hirn also turned his attention to the important economic question of the lubrication of machinery, and upset the previous prejudice against the use of mineral oil for this purpose. He also demonstrated experimentally that, while the old laws of friction enunciated by Morin were sufficiently accurate for the contact of one dry metal against another, these laws are powerfully modified when the surfaces are well lubricated, as with machinery. Now the friction varies as the square root of the pressure, and as the surface and the velocity; so that the theory falls in with that of the viscous flow of liquids. These laws have received confirmation of recent years by the experiments carried out under the auspices of the Institution of Mechanical Engineers.

But it is chiefly for his experiments on a large scale on the steam-engines under his charge that Hirn is best known, and from his varied methods of determining the mechanical equivalent of heat by the friction of metals on metal or water, and finally from observation of the amount of heat consumed by the steam-engine, when every source of gain or loss is carefully followed up.

With this object he investigated experimentally the separate effects of conduction, of jacketing, of initial condensation in the cylinder, and of its prevention by superheating.

If we watch the performance of a modern marine triple-expansion engine, we notice that the high-pressure cylinder appears choked with water from initial condensation, while the intermediate and low-pressure cylinders work comparatively dry. It was considered in the early days of compound engines that this initial condensation was a source of great loss, and superheating was introduced to minimize it. But the superheated steam ruined the packings, and dried up the lubricant, so that the superheater was found practically to do more harm than good. A characteristic story is told of John Elder, the pioneer of compounding in modern marine engines, too long to insert here, which bears on this point.

Nowadays this initial condensation is looked upon as inevitable, and as not really so uneconomical as the books make out, when attendant advantages are considered; but to the theorist such as Hirn this condensation was something to be avoided at any cost, and he worked hard to make its prevention feasible.

Hirn was a man of varied reading, taste, and pursuits, and he worked into his treatises on his favourite subject of Thermodynamics a good deal of speculative metaphysics, which make his books rather curious reading sometimes to modern tastes, and we must go back to the