acting pump (Fig. 8); these are classed together, and a closer examination of the function and elements of each will immediately show the correctness of so doing. The two bars *c* and *c* in the ratchet train correspond with the two pump rods and buckets. The pump barrels *d d* correspond with the guide frames *d d* of the ratchet train, the valves *b* and *b* correspond with the pawls *b* and *b*, while the water in the two barrels is the exact equivalent of the ratchet *a a*. As the bar *c* descends the pawl *b* would pass over a certain number of teeth of the ratchet equal to the number in the length of stroke of *c*, if the bar *c* was disconnected with the lever but as it is, during the descent of *c*, through a certain distance the ratchet is lifted an equal distance by the other pawl *b*; thus we see that each pawl passes over twice as many teeth of the ratchet as correspond to the length of its stroke. This has an exact parallel in the double-acting pump, for there also each bucket in its down stroke moves through a length of water equal to double the length of its stroke. The following is the outline of Prof. Reuleaux' Classification of Constructive Elements:—

Rigid Elements—
- Joints (for forming links) such as rivets, keys, keyed joints.
- Elements in pairs or in links, such as shafts and axles, levers, cranks, &c.

Flectional Elements—
- Tension organs by themselves and used with chain-closure, such as belts, cords.
- Partners of pressure organs such as pistons and plungers, steam cylinders and pump barrels.
- Springs.

Trains—
- Click-gear.
- Brakes.
- Movable couplings and clutches.

In conclusion we must say that the cuts illustrating the book, are much superior to those generally to be found in theoretical books on machinery, but they do not, of course, equal the elaborate working drawings to be found in certain books on machine design. In Fig. 169, p. 218, the rope appears to have somewhat lost its way. The translator has done his work most admirably, and great must have been the ingenuity required to manufacture some of the names here presented for the first time to the English reader. In fact we could hardly imagine a book more difficult to translate, on account of the great number of specially-constructed words in it, nor do we remember having read one in which the duties of the translator have been more successfully carried out. The book appears at a particularly suitable time, now that the beautiful and extensive collection of kinematic models by Prof. Reuleaux, designed by him and constructed especially to illustrate his treatment of the theory of mechanism, is to be seen at the Loan Collection of Scientific Instruments at South Kensington.

PERIGENESIS v. PANGENESIS—HAECKEL'S NEW THEORY OF HEREDITY

UNDER the title "Perigenesis der Plastidule oder die Wellenzeugung der Lebenstheilchen," Prof. Haeckel has published quite recently a pamphlet containing an attempt to furnish a mechanical explanation of the elementary phenomena of reproduction which shall be more satisfactory than Mr. Darwin's ingenious and well-known theory of Pangenesis. I shall endeavour to show that Prof. Haeckel's theory is essentially that with which both English and German students of Mr. Herbert Spencer's works have long been familiar; and that it does not furnish a clearer explanation than does Mr. Darwin's Pangenesis, of the special facts of heredity which Mr. Darwin had in view.

Haeckel commences with a very concise statement of what is at present known as to the visible composition of "plastids," those corpuscles of life-stuff called "cells" by Schleiden and Schwann, "elementary organisms" by Brücke, "life-units" by Darwin. Most plastids possess a differentiated central kernel or nucleus, which again possesses one or more nucleoli. The substance of which the body of such a nucleated plastid or *true* cell is mainly composed is generally known by von Mohl's term, "protoplasma." Haeckel proposes to distinguish the substance of the nucleus by the name "coccoplasma." In the simplest form of plastid, the "cytod," which is devoid of nucleus, and is exhibited by those lowly organisms known as Monera, by the young Gregarina (Ed. van Beneden), by the hyphæ of some Fungi, and by the ripe egg of all organisms (if we may judge from the results of the most recent researches), coccoplasm and protoplasm are not differentiated, but exist as one substance, which Haeckel, following Ed. van Beneden, distinguishes as "plasson." Whether these distinctions have a real value or not, is of no moment for the question in hand. It is a widely-accepted doctrine—in fact, the fundamental generalisation on which Biology as a science rests—that the excessively complex chemical compound which forms the substance of plastids or life-units is the ultimate seat of those phenomena or manifestations of energy which distinguish living from lifeless things—to wit, growth by intus-susception, reproduction, adaptation, and continuity or hereditary transmission. Leaving Prof. Haeckel's pamphlet for a time, let us go back thirteen years.

As long ago as July, 1863, Mr. Herbert Spencer, in his "Principles of Biology," pointed out at considerable length (vol. i., p. 181) that the assumption of definite forms, and the power of repair exhibited by organisms, is only to be brought into relation with other facts (that is to say, so far explained) by the assumption that *certain units* composing the living substance or protoplasm of cells possess "polarity" similar to, but not identical with, that of the units which build up crystals. Mr. Spencer is careful to explain that by the term "polarity" we mean simply to avoid a circuitous expression, namely, the still unexplained power which these units have of arranging themselves into a special form. He then points out that the units in question cannot be the molecules of the proximate chemical compounds which we obtain from protoplasm—such as albumen, or fibrin, or gelatin, or even protein. Further he shows that they cannot be the *cells* or morphological units, since such organisms as the Rhizopods are not built up of cells, and since, moreover, "the formation of a cell is to some extent a manifestation of the peculiar power" under consideration. "If then," he continues, "this organic polarity can be possessed neither by the chemical units, nor the morphological units, we must conceive it as possessed by certain intermediate units, which we may term *physiological*. There seems no alternative but to suppose that the chemical units combine into units immensely more complex than themselves, complex as they are; and that in each organism, the physiological units produced by this further compounding of highly compound atoms, have a more or less distinctive character. We must conclude that in each case, some slight difference of composition in these units, leading to some slight difference in their mutual play of forces, produces a difference in the form which the aggregate of them assumes."

Further on Mr. Spencer applies the hypothesis of physiological units to the explanation of the phenomena of heredity, introducing the subject by the following admirable remarks, which appear to me to assign in the most judicious manner, their true value to such hypotheses and to be as strictly applicable to later speculations as to his own. "A positive explanation of heredity is not to be expected in the present state of biology. We can look for nothing beyond a simplification of the problem, and a reduction of it to the same category with certain other problems which also admit of hypothetical solution only. If an hypothesis which certain other wide-spread phenomena have already thrust upon us, can be shown to render the phenomena of heredity more intelligible than they at present seem, we shall have reason to entertain it. The applicability of any method of interpretation to two different but allied classes of facts is evidence of its truth. The power which organisms display of reproducing lost parts, we saw to be inexplicable except on the assumption that the units of which any organism is built have an innate tendency to arrange themselves into the shape of that organism. We inferred that these units must be the possessors of special polarities, resulting from their special structures; and that by the mutual play of their polarities they are compelled to take the form of the species to which they belong. And the instance of the *Begonia phyllomaniaca* left us no escape from the admission that the ability thus to arrange themselves is latent in the units in every undifferentiated cell. . . . The assumption to which we seem driven by the *ensemble* of the evidence, is that sperm-cells and germ-cells are essentially nothing more than vehicles, in which are contained small groups of the physiological units in a fit state for obeying their proclivity towards the structural arrangement of the species they belong to. . . . If the likeness of offspring to parents is thus determined, it becomes manifest, *à priori*, that besides the transmission of generic and specific peculiarities, there will be a transmission of those individual peculiarities which, arising without assignable causes, are classed as 'spontaneous'. . . .

"That changes of structure caused by changes of action must also be transmitted, however obscurely, from one generation to another, appears to be a deduction from first principles—or if not a specific deduction, still, a general implication. . . . Bringing the question to its ultimate and simplest form, we may say that as on the one hand physiological units will, because of their special polarities, build themselves into an organism of a special structure, so on the other hand, if the structure of this organism is modified by modified function, it will impress some corresponding modification on the structures and polarities of its units. The units and the aggregate must act and re-act on each other. The forces exercised by each unit on the aggregate, and by the aggregate on each unit, must ever tend towards a balance. If nothing prevents, the units will mould the aggregate into a form in equilibrium with their pre-existing polarities. If contrariwise, the aggregate is made by incident actions to take a new form, its forces must tend to re-mould the units into harmony with this new form; and to say that the physiological units are in any degree so re-moulded as to bring their polar forces towards equilibrium with the forces of the modified aggregate, is to say that when separated in the shape of reproductive centres, these units will tend to build themselves up into an aggregate modified in the same direction." (P. 256.)

Thus, then, Mr. Herbert Spencer definitely assumes an order of molecules or units of protoplasm—lower in degree than the visible cell-units or plastids—to the "polar forces" of which and their modification by external agencies and interaction, he ascribes the ultimate responsibility in reproduction, heredity, and adaptation.

I am unable to say whether Mr. Darwin was acquainted with or had considered Mr. Herbert Spencer's hypothesis of physiological units, when in 1868 he published his own provisional hypothesis of Pangenesis. But an examination of the bearings of the two hypotheses shows that the former does not render the latter superfluous, nor is the one inconsistent with the other. Mr. Darwin wished to picture to himself and to enable others to picture to themselves a process which would account for (that is, hold together and explain) not merely the simpler facts of hereditary transmission, but those very curious though abundant cases in which a character is transmitted in a latent form and at last reappears after many generations, such cases being known as "atavism" or "reversion;" and again those cases of latent transmission in which characteristics special to the male are transmitted to the male offspring through the female parent without being manifest in her; and yet again the appearance at a particular period of life of characters inherited and remaining latent in the young organism. According to the hypothesis of pangenesis, "every unit or cell of the body throws off gemmules or undeveloped atoms, which are transmitted to the offspring of both sexes and are multiplied by self-division. They may remain undeveloped during the early years of life or during successive generations; their development into units or cells, like those from which they were derived, depending on their affinity for, and union with, other units or cells previously developed in the due order of growth."

In an essay ("Comparative Longevity," Macmillan, 1870, p. 32) published six years ago, I briefly suggested the possibility of combining Mr. Herbert Spencer's and Mr. Darwin's hypotheses thus: "The persistence of the same material gemmule and the vast increase in the number of gemmules, and consequently of material bulk,[1] make a *material* theory difficult. Modified force-centres, becoming further modified in each generation, such as Mr. Spencer's physiological units, might be made to fit in with Mr. Darwin's hypothesis in other respects." In fact in place of the theory of emission from the constituent cells of an organism of material gemmules which circulate through the system and affect every living cell, and accumulate in sperm-cells and germ-cells, we may substitute the theory of emission of force, the two theories standing to one another in the same relation as the emission and undulatory theories of light.

It may, however, be very fairly questioned whether our conceptions of the vibrations of complex molecules, or in other words their force-affections, are sufficiently advanced to render it desirable to substitute the vaguer though possibly truer undulatory theory of heredity for the more manageable molecular theory (Pangenesis). How are we to conceive of the propagation of such states of force-affection or vibration (as they are vaguely termed) through the organism from unit to unit? In what manner, again, are we to express the dormancy of the pangenetic gemmules in terms of molecular vibration? It is true that molecular physics furnishes us with some analogies in the matter of the propagation of particular states of force-affection from molecule to molecule, as, for example, in the various modes of decomposition exhibited by gun-cotton, in contact actions and the like; but it will require a very extended analysis of both the phenomena of heredity and of molecular phenomena similar to those just cited, to enable us to supersede the admittedly provisional hypothesis of Pangenesis by a hypothesis of vibrations. And it is necessary here to remark that in the fundamental conception of Pangenesis, namely, the detachment from the living cells of the organism of gemmules which then circulate in the organism, there is nothing contrary to analogy, but rather in accordance with it. It is quite certain that in some infective diseases the contagion is spread by specific material particles. This seems to be established, although it is far from settled as to whether these particles are parasitic organisms or portions of the diseased organism itself. Mr. Darwin's pangenetic gemmules may, even if not accumulated and transmitted from generation to generation, be called upon to explain the solidarity of the constituent cells of one organism; they may be assumed as agents of

[1] On this subject see Mr. Sorby's recent Presidential Address to the Royal Microscopical Society, in "Quarterly Journal of Microscopical Science," April, 1876.

a peculiar kind of infection,[1] by means of which the molecular condition or force-affection of one cell is communicated to others at a distance in the same organism. It is difficult without some such hypothesis of an active material exchange of living molecules between the various cells of the body, to conceive of the way in which "change is propagated throughout the parental system," or a modified part is to "impress some corresponding modification on the structures and polarities" of distant units, such, for example, as those contained in the mammalian ovum.

In the human ovary no egg-cells are produced after the age of two and a half years. Each of the many hundred eggs there contained reposes quietly in its follicle, whilst the growth and development of other organs is proceeding. Then a renewed period of activity for the ovary commences, but the majority of the originally-formed egg-cells retain their vitality and form-individuality for more than forty years. How, we may ask, during that time are they subjected to the influence of new polar forces acquired by the other units of the body? We know that they are so impressed, or have such influences propagated to them. Is it by "action at a distance," or by the contact action of circulating infective gemmules?

Such being the state of speculation, in England at any rate, with regard to the mechanical explanation of heredity, we return to Prof. Haeckel's recently enunciated theory of the Perigenesis of plastidules.

It is clear, to begin with, that Prof. Haeckel has either never studied or has forgotten Mr. Herbert Spencer's writings. His attempt to substitute something better for Mr. Darwin's provisional hypothesis of Pangenesis, as he tells us, has its origin, to a great extent, in the admirable popular lecture of Prof. Ewald Hering of Prague, "Über das Gedächtniss als eine allgemeine Function der organisirten Materie" [On Memory as a General Function of Organised Matter], published in 1870, and to some extent, including terminology, is based on an essay by Elsberg, of New York, published in the *Proceedings* of the American Association, Hartford, 1874. With the latter of these publications I am only acquainted through Prof. Haeckel's citations, but with the former at first hand. Prof. Hering gives a brief outline in the lecture in question, of the fundamental doctrine of physiological psychology, which had been previously worked out to its consequences on an extensive scale, by Mr. Herbert Spencer. Prof. Hering has the merit of introducing some striking phraseology into his treatment of the subject, which serves to emphasise the leading idea. He points out that since all transmission of "qualities" from cell to cell in the growth and repair of one and the same organ, or from parent to offspring, is a transmission of vibrations or affections of material particles, whether these qualities manifest themselves as form, or as a facility for entering upon a given series of vibrations, we may speak of all such phenomena as "memory," whether it be the conscious memory exhibited by the nerve-cells of the brain or the unconscious memory we call habit, or the inherited memory we call instinct; or whether again it be the reproduction of parental form and minute structure. All equally may be called "the memory of living matter." From the earliest existence of protoplasm to the present day, the memory of living matter is continuous. Though individuals die, the universal memory of living matter is still carried on.

Prof. Hering, in short, helps us to a comprehensive conception of the nature of heredity and adaptation by giving us the term "memory," conscious or unconscious, for the continuity of Mr. Herbert Spencer's polar forces or polarities of physiological units.

[1] It is a striking exemplification of the unity of biological science that we should have to look to the pathologist for the next step in this region of speculation, and that fermentations, phosphorescence, fevers, and heredity, should be simultaneously studied from a common point of view with psychology.

Elsberg appears (though this is only an inference on my part) to be acquainted with Mr. Herbert Spencer's hypothesis of physiological units. Adopting Haeckel's useful term "plastid" for a corpuscle of protoplasm (cell or cytod), he designates the physiological units "plastidules," a name which Haeckel has accepted, and which may very possibly be found permanently useful. But Elsberg does not appear to have helped on the discussion of the subject to a great extent, since he proceeds no further than is implied in adopting Mr. Darwin's theory of Pangenesis, whilst substituting the "plastidules" for Mr. Darwin's "gemmules." It appears to me that Elsberg, in his combination of the Spencerian and Darwinian hypotheses, has omitted the sound element in the latter, and retained the more questionable. He should have conjoined Mr. Herbert Spencer's conception of "plastidules" possessing special polarities or force affections which they are capable of propagating as *changes* of state (*i.e.*, force-waves) to associated plastidules, and so to offspring with Mr. Darwin's conception of a universal and continuous emission of such changes from all the cells of an organism, and the frequent occurrence of a persistently latent condition of those changes—a condition which Hering's happy use of the term "memory" enables us to illustrate by the analogous (or we should rather say identical) "latent" or "dormant condition" of mental impressions.

This is, in fact, the position which Prof. Haeckel takes up—though independently of what Mr. Spencer has written on the subject, excepting so far as the influence of the latter is to be traced in Elsberg's essay. For Haeckel, living matter, protoplasm, or plasson consists of definite molecules—the plastidules—which cannot be divided into smaller plastidules, but can only be split into lower chemical compounds. What Mr. Spencer calls polarities or polar forces Haeckel speaks of as "undulatory movements"—a symbol which has the advantages and disadvantages of analogy, but which, like "polarity," is *only* a symbol, and covers our incapability of conceiving more definitely the character of the phenomenon it designates. The undulatory movement of the plastidules is the key to the mechanical explanation of all the essential phenomena of life. The plastidules are liable to have their undulations affected by every external force, and once modified the movement does not return to its pristine condition. By assimilation they continually increase to a certain point in size, and then divide, and thus perpetuate in the undulatory movement of successive generations the impressions or resultants due to the action of external agencies on individual plastidules. This is Memory. All plastidules possess memory—and Memory, which we see in its ultimate analysis is identical with reproduction, is the distinguishing feature of the plastidule; is that which it alone of all molecules possesses in addition to the ordinary properties of the physicist's molecule; is in fact that which distinguishes it as vital. To the sensitiveness of the movement of plastidules is due Variability—to their unconscious Memory the power of Hereditary Transmission. As we know them to-day, they may "have learnt little and forgotten nothing" in one organism, "have learnt much and forgotten much" in another, but in all, their Memory, if sometimes fragmentary, yet reaches back to the dawn of life on the earth.

E. RAY LANKESTER

Addendum.—It will interest many readers to know that Prof. Haeckel takes an opportunity in this pamphlet of referring to Bathybius. He does not allude to the report from the *Challenger*, to the effect that Bathybius is a gelatinous precipitate of sulphate of lime, but speaks of it as of old. He draws attention to the recent observations of an excellent naturalist, Dr. Bessels, who, I find, in the *Jenaische Zeitschrift*, 1875, vol. ix. p. 277, writes as follows:—"During the last American expedition to the North Pole, I found, at a depth of ninety-two fathoms in

Smith's Sound, large masses of free, undifferentiated, homogeneous protoplasm which contained no trace of the well-known coccoliths. On account of its truly Spartan simplicity, I called this organism, *which I was able to observe in the living state*, 'Protobathybius.' It will be figured and described in the Report of the expedition. I will merely state here that these masses consisted of pure protoplasm, with only accidental admixture of calcareous particles, such as formed the sea-bottom. They formed exceedingly viscid, net-like structures, which exhibited beautiful amœboid movements, took in carmine-particles as well as other foreign bodies, and showed active granule-streaming."

This is certainly a very deliberate and definite statement on the part of Dr. Bessels, who is a well-known and accomplished observer. It will be interesting to see how these observations can be reconciled with the view taken by Sir C. Wyville Thomson and Mr. Murray.

DINNER TO THE "CHALLENGER" STAFF

ON Friday last, Sir C. Wyville Thomson and other members of the *Challenger* staff were entertained at dinner in the Douglas Hotel, Edinburgh, by a large and distinguished company. Besides the civilian chief himself, the other members of the staff present were Mr. J. Y. Buchanan, Mr. J. Murray, Lieut. Balfour, Dr. Crosbie, and Paymaster Richards. The Lord Provost occupied the chair, the croupiers, as the vice-chairmen are called in Scotland, being Professors Huxley and Turner. The speeches were unusually happy and spirited, but we have space to give only a few quotations from that of Prof. Huxley in proposing the health of the scientific staff of the *Challenger*, and their director, Sir C. W. Thomson. After referring to previous Government expeditions for ocean exploration, Prof. Huxley pointed out that the peculiarity of the *Challenger* Expedition was that in her case the cruise became secondary and the scientific object primary; that she was, in fact, fitted up and instructed with the view of obtaining certain scientific data which were requisite for the further progress of natural knowledge. In her case the duty of geographical exploration was reduced to *nil*, and the duty of scientific investigation had become paramount.

After showing the great importance of a knowledge of the nature of the sea-bottom, Prof. Huxley went on—

"Thirty years ago it would have been absolute madness for anyone—I was going to say—to have hoped to obtain any knowledge of the nature of the sea-bottom or of the things which lived there at depths of 5,000, 6,000, 15,000, or 20,000 feet. But then here comes one of those admirable examples of the way in which the theoretical life of this world and the practical life interlock with one another, and interact with one another. Theoretical science, abstract investigation, carried on without reference to any practical aim whatever, that sort of abstract investigation which recent Acts of Parliament have endeavoured to throw a slur upon in this country, though I am happy to say that that has been removed in the House in which it originated—that kind of abstract investigation without immediate practical result, gave us the electric telegraph. When the electric telegraph was got, practical men desired to use it as a means of connecting remotely removed countries. For that purpose it was necessary to lay submarine telegraphs. For that purpose it was necessary to improve our means of sounding; and so out of the electric telegraph came those means of sounding at great depths of the sea, which have enabled us, for the first time, to bring up from the bottom, from a depth of two or three, or it may be four miles of sea-water, the actual things which are to be found at that enormous depth. That took place twenty years ago. In 1858, my friend Commander Dayman was engaged in the survey of the Atlantic for the purposes of the cable; and the Americans, who joined in the like service, had invented means by which specimens could be brought up from that depth. So that, if I may so say, ten years ago it was in the air to apply those new methods supplied by practical life to scientific purposes, to apply the methods of sounding, the methods of dredging, and the methods of ascertaining temperature which had been devised for the purposes of the telegraph engineer, to further investigation of the contents and nature of the sea. But it is all very well for ideas to be in the air. It needs clear brains to get them out of the air, and in this case there were two very clear brains at work on the subject—one of them the brain of our distinguished guest of to-night, Sir C. Wyville Thomson—and the other the brain of my friend Dr. Carpenter, who is well known to the scientific world."

Prof. Huxley then referred briefly to the history of recent deep-sea exploration and to the influences brought to bear on the Admiralty to send out the *Challenger*. He spoke of the object of the expedition and of the important results which have been achieved. "It was a very considerable task," he said, "it was a task which would have been absolutely chimerical thirty years ago, but it was a task which had been rendered possible, and which has been actually performed in the most satisfactory manner. The *Challenger* has brought home, I am informed, the records of such operations performed at between 300 and 400 stations—that is to say, at 300 or 400 points along that 70,000 miles, we know exactly the depth of the sea, the gradations of temperature, the distribution of superficial life, and the nature of what constitutes the sea-bottom; and such a foundation as that for all future thought upon the physical geography of the sea up to this moment not only had not existed, but had not even been dreamed of. I won't detain you by speaking of the great results of the expedition, for one very good reason, that I don't know them. They are in the breast of my friend at the opposite end of the table. But he has been good enough to favour us at the Royal Society from time to time with reports of what he has been about, and some of the discoveries which have been made by the *Challenger* are undoubtedly such as to make us all form new ideas of the operation of natural causes in the sea. Take, for example, the very remarkable fact that at great depths the temperature of the sea always sinks down pretty much to that of freezing fresh water. That is a very strange fact in itself, a fact which certainly could not have been anticipated *à priori*. Take, again, the marvellous discovery that over large areas of the sea the bottom is covered with a kind of chalk, a substance made up entirely of the shells of minute creatures—a sort of geological shoddy made of the cast-off clothes of those animals. The fact had been known for a long time, and we were greatly puzzled to know how those things got to be there. But the researches of the *Challenger* have proved beyond question, as far as I can see, that the remains in question are the shells of organisms which live at the surface and not at the bottom, and that this deposit, which is of the same nature as the ancient chalk, differing in some minor respects but essentially the same, is absolutely formed by a rain of skeletons. These creatures all live within 100 fathoms of the surface, and being subject to the fate of all living things, they sooner or later die, and when they die their skeletons are rained down in one continual shower, falling through a mile or couple of miles of sea-water. How long they take about it imagination fails one in supposing, but at last they get to the bottom, and there, piled up, they form a great stratum of a substance which, if upheaved, would be exactly like chalk. Here we have a possible mode of construction of the rocks which compose the earth of which we had previously no conception. But this is by no means the most wonderful thing. When they got to depths of 3,000 and 4,000 fathoms, and to 4,400 fathoms, or about five miles, which was the greatest depth at which the *Challenger* fished anything from the