SCIENCE.—SUPPLEMENT.

FRIDAY, AUGUST 6, 1886.

THE PHYSICAL BASIS OF HEREDITY.

PROFESSOR HUXLEY, in his well-known essay, has described protoplasm as the material substratum of all vital phenomena, and established the term 'physical basis of life.' Recent investigations lead to the hypothesis that there is a special and visible substance, which is the material substratum of hereditary transmission from parent to offspring, and may be called, if we choose to imitate Huxley, the physical basis of heredity. The name of the substance is chromatine, in reference to the special affinity for coloring-matters, which is the most striking characteristic of the substance.

Chromatine, also called nucleine by some writers, is found in the nuclei of cells of all kinds. It is only recently that it has been clearly recognized, and a great deal of additional investigation must be accomplished before we can hope to know much about it. It was impossible to ascertain much concerning it hitherto, because the methods of preserving tissues for microscopical examination have become perfected only within the last few years, so far that the minute details of cell organization could be studied. Nor was it until the recent introduction of oil immersion objectives by Dr. Zeiss, that we had command of lenses sufficiently perfect for the investigation of chromatine. For those who wish to inform themselves more fully concerning the occurrence and peculiarities of chromatine, I refer to Carnoy's 'Biologie cellulaire,' which I venture to think the best general work yet published on the structure of cells.

For our present discussion a very brief statement will suffice. When cells, properly preserved, are stained with almost any of the dyes commonly used by histologists for the coloration of cell nuclei, the higher powers of the microscope reveal the fact that the nucleus contains three visibly different matters,—1°, the network of slightly colored threads; 2°, some dots or threads very deeply stained; 3°, the hyaline, or granular substance, in which the other parts are embedded. This basal substance, enchylema, is probably more or less nearly fluid during life, and is equivalent to the *kernsaft* of those German writers, who apply that term in its proper and restricted sense: unfortunately it is employed with a variety of meanings. The network resembles the protoplasm network of the body of the cells, and is probably the intra-nuclear extension of the protoplasm. The deeply dyed parts are the chromatine; and the presence thereof appears, so far as our present knowledge goes, the essential and distinctive characteristic of a nucleus.

During the division of cells, in the great majority of cases, very remarkable changes occur in the arrangement of the chromatine, leading to the development of those striking appearances known as karyokinetic figures, or, as Flemming would like to have them called, mitoses. It is difficult to refrain from styling the latter term new-fangled; for the systematic duplication of terms with which Professor Flemming has unnecessarily burdened science of late can only be condemned. It is curious to encounter such pedantry in so industrious and sensible a histologist, because to overvalue terminology is the mark of mental poverty. As the figures in question are described in the more recent text-books of anatomy and histology very fully, we need allude only to the conclusion that the nucleus appears to lead the process of division, and the chromatine to lead the division of the nucleus. Nussbaum (*Arch. f. mikros. anat.*, xxvi. 504) points out, however, that in some cases the protoplasm apparently leads, alterations in it preceding nuclear changes. He refers especially to observations on Infusoria by Everts (*Zeitschr. wiss. zool.*, xxiii. 601) and Jickeli (*Zool. anz.*, 1884, p. 491). But to interpret such observations, we must not forget that the nucleus and protoplasm are interdependent, neither being able to maintain its existence without the other, at least in any instance where they are normally united. The fact that the visible alteration of the protoplasm in a certain rare case comes before that of the nucleus shows that the protoplasm probably has an active *rôle* in cell-division; but since even then its arrangement depends on the position of the nucleus, the evidence of the superiority of nuclear control is, I think, not affected.

On the other hand, there are many observations which may be interpreted as proofs that the nuclei have a regulating power over the cells, especially as regards their division and organization. A few of these may be instanced. 1°. After a cell is formed, its nucleus enlarges first, and the cell body follows it in growth. 2°. Kölliker, in

his paper [1] on heredity (p. 29 ff.), discusses the relation of nuclei to growth very fully and ably. The great extent of his learning has enabled him to present the manifold aspects of the question more thoroughly than any other writer. His argumentation seems to me so satisfactory that it does not require the weight of his great authority to establish the conclusion that without nuclei there is no growth. Of this, the most faith-compelling evidence is offered by the important experiments of Nussbaum and Gruber,[2] who found that when unicellular animals are artificially divided, the fragments containing nuclei continue to grow, while pieces without nuclei die off. 3°. The large unicellular Thallophytes, such as Caulerpa and Codium, become multinuclear before they attain their adult size. Further illustrations are given by Kölliker (*l. c.*, pp. 19–20). 4°. Perhaps the most striking demonstration of the importance of the nucleus is afforded by the experimental alteration of the plane of division of the ovum. Pflüger [3] showed that the plane of the first division of the ovum is altered by tilting the ovum before the division begins, and keeping it in the same position during division; normally the plane passes through the white pole, but when the ovum is fastened in an oblique position, the plane is not in the axis of the ovum but in the line of gravity. Born [4] has continued these remarkable experiments, and discovered that the nucleus changes its position when the ovum is kept tilted, and that the site of the nucleus determines the plane of division of the ovum.

Still more pertinent to the theme of this article are the phenomena of the impregnation of the ovum.[5] In 1872 Bütschli [6] discovered that two nuclei are present in the fertilized ovum of Rhabditis dolichura, a nematod worm, and that the two nuclei unite, becoming the first nucleus of the embryo. Oscar Hertwig [7] proved three years later that the two nuclei, or as they are better called, pronuclei, are derived, one from the nucleus of the ovum, the other probably from the fertilizing male element, the spermatozoon. His observations, which were made at that time on echinoderms, led him to the theory that "impregnation depends upon the fusion of two sexually differentiated nuclei." Both Hertwig himself and many others, notably Fol Selenka, Flemming, Platner, and Strassburger, have confirmed this conclusion, so that there is a very strong presumption in favor of Hertwig's theory being a true law for all cases of fertilization. Strassburger was for some time [8] an opponent of the exclusive significance of the nuclei, holding the opinion that "there also occurs a copulation between the other equivalent parts of the spermatozoon and ovum," thus making the participation of cell protoplasm essential. But lately [9] he has acceded to Hertwig's opinion, and has expressed himself in a recent publication [10] with great distinctness in favor of the nuclei alone being essential to impregnation. Strassburger observed in some cryptogams the protoplasm of the male element to be so much reduced that hardly more than the nucleus remained, and found that in certain phanerogams only the nucleus of the pollen grain reaches the ovum.

The next point to be brought forward is that the spermatozoon, which forms one of the pronuclei, is in many animals developed exclusively from the nucleus. The formation of the spermatozoon has been much investigated, and yet very little thoroughly satisfactory work has been published in result. Although the great majority of the articles report more or less that is valuable, yet they also contain, too often, much that is crude, inaccurate, or even out and out false; so that it is a difficult task to unsnarl the truth from the mesh of error in which it is ravelled. Kölliker,[11] as long ago as 1841, advanced the hypothesis that the spermatozoa of all animals have the significance of nuclei. This is not quite correct, since the seminal corpuscles of nematods have the value of cells, as do probably also those of the higher crustacea, and possibly of other animals. It still remains true that in the majority of cases the spermatozoa are modified nuclei, and nuclei only. As regards the higher animals, the obser-

[1] 'Die bedeutung der zellenkerne für die vorgänge der vererbung,' in *Zeitschr. f. wiss. zool.*, xlii. pp. 1–46.

[2] *Science*, vol. vi. p. 4. See also Nussbaum's later paper in the *Archiv für mikroskop. anat.*, xxvi. p. 485. Nussbaum also cites Fr. Schmitz's experiments on the artificial division of plants. Schmitz's paper I have not seen: it was published in 1879, in the *Festschrift der naturforschenden gesellschaft zu Halle.*

[3] *Pflüger's Archiv für die gesammte physiol.*, xxxii. pp. 1–80.

[4] *Breslauer arztlich. zeitschr.*, 22 März. 1884. I have not seen the original. There is an abstract in Hofmann und Schwalbe's *Jahresbericht* for 1884, p. 444.

[5] For a synopsis of recent investigations, the reader is referred to the article 'Impregnation' by the author in Wood's 'Handbook.'

[6] 'Beiträge zur kenntniss der freilebenden nematoden,' in *Nova acta*, xxxvi. 1773.

[7] 'Beiträge zur kenntniss der bildung, befruchtung und theilung des thierischen eies,' in *Morphol. jahrbuch*, i.

[8] *Ueber befruchtung und zelltheilung*, 1878, pp. 75–77.

[9] *Ueber den bau und das wachsthum der zellhäute*, 1882, pp. 250–252.

[10] *Neue untersuchungen über den befruchtungs-vorgang bei den phanerogamen als grundlage für eine theorie der zeugung*, Jena, 1884 (see p. 77).

[11] *Beiträge zur kenntniss der geschlechtsverhältnisse und der samenflüssigkeit wirbelloser thiere, nebst einem versuch*, etc., Berlin, 1841.

vations of Flemming [12] and of several other recent authors seem to me conclusive.[13] The footnote communicates more fully the further significant fact, that the male element is developed chiefly from the chromatine of the nucleus. The facts stated prove that a body consisting mainly of chromatine from the nucleus of a sperm cell can impregnate an ovum.

Oskar Hertwig was the first [14] to point out the bearing of this induction upon the problem of heredity. It is obvious, since qualities may be inherited from the father, that the nucleus alone can furnish the means of transmission from parent to offspring. And, since it can accomplish this on the paternal side, it is probable that it can do as much on the mother's side, an assumption against which no evidence has been brought forward: hence the hypothesis that *the nucleus is the organ of hereditary transmission.* Further, since the chromatine is the characteristic of the nucleus, and since spermatozoa in some cases consist almost exclusively of chromatine, it is probable that *chromatine is the essential factor in the function of heredity.* The leading defenders of this double hypothesis are Hertwig, Strassburger, and Kölliker, all biologists as able as they are distinguished. Careful study of their writings must, I think, lead a candid mind to accept their argumentation; though of course one does not forget that hypotheses are not demonstrations.

Hertwig's paper [14] is to be recommended as the best single essay, the one to be read by those who desire to grasp the essential points of the discussion of heredity, and yet have not the leisure to go through all that has been published. Hertwig writes admirably: his matter is well arranged, his language direct, and his thinking clear and forcible. In brief, his papers have many of the qualities which we expect in a model of scientific writing. Some of his later ones exhibit less careful preparation.

Johannes Frenzel [15] has published what may be characterized as a lengthy, and on the whole half-hearted, criticism of the hypothesis of Hertwig. The objections he brings forward are in large part those which necessarily occur of themselves to every competent judge of the problem. An older investigator would have perceived this, and accordingly dealt with the discussion with much greater brevity. Frenzel's first objection is, that it is not certain that the nuclei of the male elements are not still accompanied by some protoplasm when they fuse with the ovum. Unfortunately our author has overlooked that the best investigations show the mammalian spermatozoon to be derived solely from the nucleus. Frenzel's second objection is that there are cells without nuclei. Careless and incomplete observations have frequently led to the assertion that there are such cells, but the error has been again and again refuted. On pp. 97–98 Frenzel cites Bobretzky and Korotneff as authorities, but these authors have not made sure of the absence of the nuclei. On the contrary, their investigations on the insect eggs, in which cells without nuclei are supposed to occur, are so obviously insufficient that it is astonishing to find stress laid upon them. For my own part, I feel little hesitation in asserting that except, perhaps, among the very lowest organisms, *there are no cells without nuclei.* As regards the lowest organisms, there is uncertainty. Nothing to be called a nucleus is known in bacteria, for instance. We cannot, indeed, state at present that the continuance of life is impossible without a nucleus. On the other hand, our knowledge of the minute fungi and supposed monera is so imperfect, that it would be foolish to accept the dogma that these organisms have no nuclei. It is conceivable that in the lowest forms of life the material basis of heredity is a diffused substance, which in the progress of evolution has gathered together to result in the genesis of nuclei. Therefore, whether the lowest bionts are nucleate or not, they do not offer, so far as at present known, any valid objection to Hertwig's theory that the nucleus is the organ of heredity. There is nothing else in Frenzel's article requiring notice in this brief review. It will not, I think, repay those not engaged in the special study of the subject to familiarize themselves with the essay in question, for I am able to commend it only with reserve.

The last few years have not only brought us

[12] *Archiv für mikrosk. anat.*, xviii. p. 249.

[13] The following authorities covering the period of the last eighteen months have dealt with the development of the spermatozoon in mammalia: BROWN, *Quart. journ. micros. sc.*, xxv. 343; WIEDERSPERG, *Arch. f. mikrosk. anat.*, xxv. 113; PLATNER, *Ibid.*, xxv. 564; BIONDI, *Ibid.*, xxv. 594; PLATNER, *Ibid.*, xxvi. 343; LA VALLETTE ST. GEORGE, *Ibid.*, xxvi. and xxv. 581. Others might be cited. I have given a synopsis of these researches in the *Boston medical and surgical journal*, cxiv. 460. Nussbaum, even in his latest paper, adheres to his belief that the spermatozoa are always cellular, and not exclusively nuclear. Unfortunately he does not state upon what grounds the results of so many investigators are to be set aside. The authors cited show that the chromatine gathers together within the nucleus, and that it forms the head of the spermatozoon, while a large part of the nucleus breaks down: hence the spermatozoon arises chiefly from the chromatine of the nucleus of the cell (spermatoblast).

[14] 'Das problem der befruchtung und der isotropie des eies, eine theorie der vererbung,' in *Jena zeitschr. naturwissensch.*, xviii.

[15] 'Das idioplasma und die kernsubstanz,' in *Archiv für mikros. anat.*, xxvii. 1886, pp. 73–128. Frenzel's position is best shown by a paragraph on p. 89 which summarizes his view.

fresh insight into the morphological basis, but also into the physiological function of heredity.

A few words are necessary about pangenesis. The hypothesis, as originally advanced by Darwin, was the suggestion of a masterly mind, and as a succinct and comprehensive expression of the facts of heredity, commands admiration. But the real worth and real significance of the hypothesis have not been grasped by those who have tried to better it: its value was not in explaining, but in expressing, heredity in hypothetical terms, which were at once suggestive and comprehensible. Haeckel, whose judgment has too often to be deplored, accepted pangenesis in the mistaken way, and made an attempt to improve upon it as an explanation, in a pamphlet[16] which no competent critic any longer assigns serious value to. Indeed, were some one to assert that the alliterative euphony of its title, 'Die perigenesis der plastidule,' was its cleverest part, a physiologist might feel unable to prove the assertion erroneous. According to Darwin's hypothesis, every part of the body throws off particles, or gemmules, and some of these from each portion of the body enter the sexual elements, each of which, therefore, contains contributions from every part of the parent. The gemmules, by their multiplication in the embryo, reproduce their own kind, and so rebuild on the former pattern. Haeckel's perigenesis is, when separated from his rhetoric, the substitution of rhythmical vibrations for the different kinds of gemmules. It need hardly be said that not a tittle of evidence for this notion is shown, and that, as elaborated by its author, it violates the elementary laws alike of biology and physics. In these respects it recalls the delightful theory of Dr. Cohen,[17] who, having noticed a certain resemblance of the ovum to a ganglion cell of the spinal cord, and of the spermatozoon to the unipolar cells of the sympathetic ganglia, gravely concludes, "The influence of the spermatozoon, the male hereditary influence, extends above all to the cerebro-spinal system, while the action of the ovulum, Goethe's 'ewig weibliches,' shows itself above all upon the organs subordinate to the sympathetic nervous system" (pp. 30–31). In physics, also, Cohen even surpasses Haeckel: he attributes (p. 19) the entrance of the spermatozoon into the ovum to reaction between the positive electricity of the one and the negative of the other.

Brooks's[18] modification of the theory of pangenesis well deserves consideration, although the subsequent progress of biology does not lead me to think it felicitous; but we can now recognize it as a step towards Nussbaum's valuable theory, and also towards Weismann's conception that sexual reproduction has for its object the maintenance of variability. Brooks's theory is advocated in his book on 'Heredity' (Baltimore, 1879): he states it succinctly[18] as follows:—

"This paper proposes a modification of Darwin's hypothesis of the same name (pangenesis), removing most of its difficulties, but retaining all that is valuable. According to the hypothesis in its modified form, characteristics which are constitutional and already hereditary are transmitted by the female organism by means of the ovum; while new variations are transmitted by gemmules, which are thrown off by the varying physiological units of the body, gathered up by the testicle, and transmitted to the next generation by impregnation."

If this theory was tenable, there should be — to mention a single objection — little variation in individuals produced by parthenogenesis; and they ought always to be females, whereas they are sometimes males. There remains, not a new theory of pangenesis, but the valuable suggestion that the maternal influence causes less variability than the paternal. I am, however, strongly disinclined to anticipate the confirmation of this suggestion, especially because the males are not more variable than the females, as we should expect. I have some extensive statistics, which show that in mammals, at least, there are no essential differences between the sexes in variability. Even if Brooks's thesis should be established, it would prove only that the inheritance from the mother is stronger than from the father, and there would lack reasons for his abstruse hypothesis.

The first important step towards the substitution of a new theory, *vice* pangenesis, was taken by Dr. Moritz Nussbaum, whose memoirs[19] on the differentiation of sex deserve great attention. Every one who feels interest in the general problems of biology, and is able to follow a technical paper, will find Nussbaum's memoirs profitable reading.

[16] The pamphlet was published at Berlin in 1876. For some, considering its character, very gentle criticisms, see Ray Lankester in *Nature*, July 13, 1876, xiv. 235–238. Elsberg has also written on the subject in the Proc. Amer. assoc. adv. sc., xxv. 178, and cites there earlier writings of his own. The perusal of his article has not enabled me to recognize any thing novel except the substitution of the term 'plastidule,' for 'gemmule' used by Darwin, and speculations as to composition of plastidules, as if he was groping after the conception of the micella of Nägeli, with which he was apparently unacquainted.

[17] *Das gesetz der befruchtung und vererbung*, etc., Nördlingen, 1875.

[18] Proc. Amer. assoc. sc., Buffalo, 1876, p. 177, abstract of a paper read before the section of natural history.

[19] 'Zur differenzirung des geschlechtes im thierreich.,' in *Arch. für mikrosk. anat.*, xviii. (1880) pp. 1–113; and 'Ueber die veränderung der geschlechtsprodukte bis zur eifurchung, in *Ibid.*, xxiii. 155.

Professor Weismann [20] has adopted Nussbaum's conception, and defended it with considerable energy, adding also several important modifications. Nussbaum pointed out that there is noteworthy evidence in the development of various animals, tending to show that the germinal cells from which the sexual products are derived are separated off from the other cells of the embryo very early, and undergo little alteration. Hence he concluded that some of the original germ substance is directly abstracted from the ovum, and preserved without essential alteration to become, by giving rise to the sexual elements, the germ substance of another generation. Weismann insists upon the corollary, that the whole nature of the animal or plant depends upon its germinal substance, and that the reason why the offspring is like the parent is that in each biont some of the germinal matter is preserved unchanged. He calls this view the theory of the continuity of germ plasma. He follows Nussbaum also in emphasizing the fact that this theory is inconsistent with the theory of pangenesis and with the theory of the transmission of parental characteristics which are acquired through external causes. On these two points Weismann's second and third papers mentioned in the footnote [20] are the most important. I fully coincide with him as regards pangenesis, but am less inclined to do the same as regards acquired characteristics. It is upon the latter theme that Virchow has opposed him. I am compelled to say, however, that the distinguished pathologist has failed to understand Weismann's position correctly, and that most of his criticisms I cannot deem valid.

According to the theory of Nussbaum and Weismann, the cells in the embryo separate into two kinds, — 1°, the germ cells, which are converted into the sexual elements; and, 2°, the somatic cells, which constitute the body of the organism. The germ cells descend directly from the impregnated ovum, and undergo little alteration, so that they have (in suspension) the power to produce a whole organism. It is difficult to agree to this remarkable speculation: on the contrary, we must side with Kölliker (*l. c.*, pp. 44–46), who says that a sharp division between germ cells and somatic cells cannot be maintained. The feeling that there is a flaw in Weismann's argumentation cannot be escaped. While we recognize the ability, the great ability, of his essays, and cannot read them without our minds appropriating much from them, we remain sensible of the mysticism which zigzags across his pages, now and then blurring his expressions, and making his thought indefinite. After reading his article on the 'Continuity of the germ plasma,' there lingers an uncomfortable sense of mental haze. I have already indicated elsewhere [21] a more comprehensive theory, which is irreconcilable, so far as I can perceive, with the continuity theory. My views I hope to defend on another and more appropriate occasion. Nor is a discussion of Nussbaum's theory essential in this article. We turn, therefore, to the next point demanding attention.

Nägeli, the celebrated botanist, published in 1884 a large work containing a series of views reached at the culmination of a remarkable career of scientific research. The volume [22] has been less studied than one would wish: it comprises over eight hundred pages, and is decidedly abstruse. Nägeli is led to the theory that there are in every living cell two substances, — one, which he calls *idioplasma*, in distinction to the other, which he names the nutritive plasma. It is the idioplasma alone which carries on the function of hereditary transmission. We have here the definite conception, that the character of a special constituent of living matter regulates the organization of it. In other words, Nägeli assumes the formative force to reside in a specific material substratum, which reproduces and perpetuates itself, occurs throughout the organism, and supplies fragments of itself to the genital products. The argument in support of this theory is very able, and one can but join in the praise which Kölliker and others have bestowed upon it so cordially. The theory itself supplies us for the first time with a tangible notion from which to work ahead. A clew is given as to the physiological process of heredity.

Putting together all that has been said, the conclusion is evident. Nägeli's hypothetical idioplasma is probably identical with the nuclear chromatine of morphologists.

[20] Weismann's first paper was read before the University of Freiburg as a Prorectorats Rede, and was published in pamphlet form at Jena in 1883, with the title 'Ueber die vererbung.' A second paper was read before the German Naturforscherversammlung in 1885, and appeared in the Tageblatt of that association: it was subsequently amplified and republished with the title, 'Die continuität des keimplasmas,' etc. (Jena, 1885). A third paper, 'Ueber die bedeutung der geschlechtlichen fortpflanzung für die selectionstheorie,' was likewise addressed to the Naturforscherversammlung, and published at Jena (1886). A notice of this last by Kollmann was given in the *Biolog. centralbl.*, v. pp. 673 and 705. At the same meeting of the Naturforscher, Virchow also delivered an address (since published in *Virchow's archiv*, ciii. pp. 1, 205, 413, and in shorter form in the *Biolog. centralbl.*, vi. pp. 97, 129, and 161), in which he attacked Weismann's thesis. To Kollmann and Virchow, Weismann has replied (*Biolog. centralbl.*, vi. p. 33).

[21] MINOT, — 'Organization and death,' in Proc. Amer. assoc. adv. sc., Ann Arbor, 1885.

[22] Mechanisch-physiologische theorie der abstammungslehre, Munich, 1884 (available abstracts are given in the *Biolog. centralbl.*, iv. pp. 488 and 517).

It is my conviction that the hypothesis of pangenesis, both in its original form and in all its subsequent modifications, has been definitely set aside. In its place we have the theory that the nature of the germ, i.e., of the impregnated ovum of each species, is the same over and over, not because there is in each case a similar collocation of gemmules or plastidules, but because the chromatine perpetuates itself, so that the same kind of chromatine is found in the one generation as in the generations preceding it and following it. *The child is like the parents, because its organization is regulated by not merely similar, but by some of the same, chromatine as that of the parents.* Perhaps, instead of chromatine we ought to say, in order to avoid an unjustifiable explicitness, nuclear substance.

When it is recalled that heredity is one of the fundamental phenomena of life, and that hitherto we have seen no hopeful way leading to its comprehension, we can understand the delight with which biologists welcome the new theory and its rich promises. CHARLES SEDGWICK MINOT.

ROSMINI'S PSYCHOLOGY.

THIS is the sixth volume of the translation which Rosmini's English disciples have undertaken to make of his principal writings, — a labor of devotion surely, not only by reason of the mere pains involved, but in view of the probable thanklessness of the English-reading public for whose sake they are all taken. When one thinks of the mere quantity of labor which Rosmini accomplished in his not long life, one cannot refuse to him the title of being one of the very small number of intellectual giants of the world. He is of the race of the Aristotles, the St. Thomases, the Leibnitzes, the Kants, and the Hegels. The mere cogitative energy of him, too, is fully equal to theirs. Every page he writes is filled with thinking as hard, subtle, and original as theirs; and his style is as clear and flowing as theirs is usually the reverse. His learning is prodigious too. In short, he is a miracle of intellectual force, compared with whom a mere reviewer's mind is as a midge against an elephant. But Rosmini is a *dead* giant, and the reviewer can have it his own way with him, because *he* is alive, and writes for readers taught by all their Lockian and Protestant education to treat the kind of thing that Rosmini represents — thoroughgoing, concatenated, and systematic ontologizing and theologizing by the conceptions of principle and term, substance and essence and act — as 'scholastic jargon,' and so to close their ears. Scholastic jargon, too, it seems to this reviewer; only he has a bad conscience about saying it so shortly, and therewith turning Rosmini over to the disdain of many of our native philistines who at bottom are spiritually unfit to loosen his shoe. The last word has not yet been said about scholasticism. We are all scholastics without knowing it, so sure as we talk of things and acts and essence and force. But we don't elaborate our scholasticism, because Locke taught us that to do so led to no practical use. The only practical gain which accrues to a scholastic from his elaboration of what we all believe, is what Rosmini calls "the experience in himself of a kind of jubilation and felicity, which is so peculiar as to be unlike any other feeling and to bear testimony to its infinite source." This is the rapture of all intellectual order and harmony; but our race would willingly part with it, if only thereby it could buy a new way of peeling potatoes, or of teaching children how to read. We renounce one thing, scholasticism another. It is not that the distinctions made by Rosmini and other scholastics are false. On the contrary, they seem for the most part true. They are one way of seeing and naming the facts of life. But they are sterile: we can deduce from them no immediate practical receipts. To peel potatoes, we must look at other aspects of the world than substantiality and accidentality and the distinction between immanent and transient acts. Many are the aspects of every bit of reality, and all are equally true. But each carries us a different way. By a succession of accidents modern critics and men of science have stumbled on the aspects which lead to the ways of foreseeing and handling particular material events. Together, these aspects form the armament of the scientific and positivistic view of life, a hodge-podge of which we moderns are very proud, but of which, great as the practical fruits are, the speculative dignity leaves much to be desired. Maybe some disciple of Rosmini may show a path down from his categories to the practical details of life. It were sad that such strenuous and in many ways such exquisite thinking as his should be among the mere superfluities of human history. W. J.

Psychology. By ANTONIO ROSMINI SERBATI. Vol. ii. London, *Kegan Paul, Trench & Co.*, 1885. 8°.

CLERKE'S HISTORY OF ASTRONOMY.

THIS is in some respects a remarkable book, and takes its place at once in importance beside Grant's 'History of physical astronomy,' which it in a measure supplements. No clearer indication

A popular history of astronomy during the nineteenth century. By AGNES M. CLERKE. Edinburgh, *Black*, 1885. 8°.