refers generally to the discussion of the observations of several of the minor planets with the view to correcting the mass of Jupiter, and to the observations of elongations of the fourth satellite by the present Astronomer Royal at Cambridge, which last assigned for the denominator of the fraction 1046·77. He then remarks upon the circumstance of Bouvard having deduced from his comparison of the theory of Saturn with seventy-four years' observations a mass so nearly identical with that of the *Mécanique Céleste;* Bouvard left no details of his work behind him; it is only known that he adopted at the outset the value of Jupiter's mass admitted at the time, that of Laplace, and M. Leverrier explains that on the method of procedure adopted, Bouvard could not do otherwise than reproduce at the termination of his calculations the value he had assumed at starting. This is illustrated by the result of Leverrier's solution of his own equations of condition, founded upon the much longer period of 120 years, which proved wholly insufficient for the correction of Jupiter's mass. He remarks, with respect to Bouvard's work, that any value of the mass taken arbitrarily within certain limits will allow of a tolerable representation of the observations of Saturn, on the condition that this same arbitrary value is introduced throughout in the functions representing the mean longitude, mean motion, excentricity and longitude of perihelion; the elements obtained by Bouvard are therefore found represented by these functions of his arbitrary quantity, and he reverts to the mass assumed at the commencement of his work.

In conclusion, M. Leverrier insists that the use of the elongations of the fourth satellite for the determination of the mass of the Jovian system, has at present an incontestable superiority over the employment of the theory of Saturn, on account of the too short period over which the observations as yet extend, but in the lapse of time this superiority of the former method will diminish and the use of the perturbations will become the more advantageous. It is really, he adds, the same question as that which presents itself with regard to the solar parallax, which is determinable on two methods: the one, geometrical, the method by transits of Venus; the other, mechanical, depending for instance on the large inequalities in the motion of Mars. The method by transits, so important in 1760, but limited in its means of application, must eventually give way to the method of perturbations, the accuracy of which will increase unlimitedly with the course of time.

The first evaluation of the mass of Jupiter is that of Newton in the Cambridge edition of the "Principia" (1713), inferred from Halley's observation of an emersion of Jupiter and his satellite from the moon's limb, giving for the denominator of the fraction (whereby it is usual to express the mass) 1033. In the later editions of the "Principia" the mean distance of the fourth satellite resulting from Pound's observations, to which allusion is made above, was substituted in the calculation of the mass, which was found to be 1067. (It may here be mentioned that from later observations by Pound with a micrometer on a telescope of 123 feet focus, on the mean distance of the third satellite, Bessel found for the mass 1066). The next attempt in this direction appears to have been made by Triesnecker, Director of the Observatory at Vienna. In 1794 and 1795, making use of a Dollond object-glass micrometer, he obtained a series of measures of distances of all four satellites, the notice of which appears in the Vienna Ephemeris for 1797. Bessel deduced from them, by a mean of the four values, 1055·68. Then follow Bouvard's investigations already mentioned. It is understood that Gauss was the first to bring the perturbations of the minor planets to bear upon the determination of the mass of Jupiter, and that from the perturbations of Pallas he perceived the necessity of an increase to the mass adopted by Laplace. The circumstance, so far as we know, rests upon the authority of Nicolai, who, following in the same steps, discussed observations of Juno at fifteen oppositions, between the year 1804 and 1823, and (in the *Berliner Astronomisches Jahrbuch* for 1826) deduced for Jupiter's mass 1053·92. Encke, from fourteen oppositions of Vesta, between 1807 and 1825, made its value 1050·36, in a paper published by the Berlin Academy of Sciences in 1826.

Sir George Airy's observations at the Cambridge Observatory, alluded to by M. Leverrier in his recent notice, are next in order of time. They were commenced in 1832 and continued till 1836. The final result appears in vol. x. of the Memoirs of the Royal Astronomical Society; it is 1046·77, and depends upon observations on thirty-three nights. Details of the earlier Cambridge observations will be found in vols. vi. and viii. of the same memoirs. Sir George Airy considered it very improbable that there could be an error of a single unit in the denominator of the fraction expressing the mass, being led to this opinion by the close agreement of the separate results.

In the year 1835 Prof. Santini, the present venerable director of the Observatory of Padua, by sixteen nights' measures of the distance of the fourth satellite from both limbs of Jupiter, obtained for the mass 1049·2 (*Ricerche intorno alla Massa di Giove*, Modena, 1836).

Bessel's elaborate series of measures of distances of the four satellites commenced in October 1832 and were completed in the middle of 1839. They are fully discussed in his very valuable memoir, *Bestimmung der Masse des Jupiter*, in vol. ii. of his *Astronomische Untersuchungen:* the definitive value of the mass (p. 64) is 1047·879. Bessel's mass, which has been generally adopted in the calculation of the perturbations of minor planets and comets, and which is so close a confirmation of that deduced by the Astronomer Royal, has received much additional support from recent and, as regards method, essentially different investigations. Thus Krueger, of Helsingfors, from the perturbations of Themis, one of the minor planets which approaches nearest to Jupiter, assigns 1047·16; Axel Möller, by his masterly researches on the motion of Faye's Comet, 1047·79; while Von Asten, from his last investigations relating to Encke's Comet, finds 1047·61.

THE HOPKINS UNIVERSITY, U.S.

THE munificent bequests made by wealthy Americans for the promotion of education in the United States frequently excite our astonishment, for they are unparalleled in Europe at the present time. One of the most unique and well-devised of these bequests has lately occurred. Last year there died a Mr. Jonas Hopkins, a rich citizen of Baltimore, who, after providing for his relatives and leaving various minor benefactions, bestowed the chief part of his estate to found a university with an affiliated medical school and hospital. Both the university and the hospital receive separate landed and other property of such a substantial character that the value of the total amount is over three millions of dollars. Each institution is to be controlled by a board of nine trustees, and the same persons are to be on both boards. The university will have no ecclesiastical or political character or supervision, and will be modelled as far as possible after all that is best in similar American and European institutions. It is intended to give the highest instruction that can be obtained, and the trustees are to act in accordance with the most enlightened experience of the day. The scientific and literary departments will first be organised, and then will follow the departments of Medicine and Law.

No permanent buildings will be erected till all the Faculties are in working order and the wishes of each professor can be carried out; meanwhile a building has temporarily been secured in Baltimore, on the outskirts of which city are the grounds Mr. Hopkins has left for the hospital and university which in future will bear his name. The trustees have already selected the President of the University, and an admirable head they have found in Mr. Henry Gillman, formerly the Principal of the San Francisco University. Mr. Gillman is now in England, maturing his plans and gaining information from various universities in Europe. The dominant wish of the new president is to gather round him a body of professors and lecturers devoted to original research in their different spheres. Only one chair has yet been filled, namely, that of Mathematical Physics, and to this Mr. H. A. Rowland has been appointed. Though still quite a young man, the good work Mr. Rowland has already done in magnetism has made his name well known among English physicists, and in his new position a brilliant career lies before him. It is hoped that students will be received in 1876, and we heartily wish Mr. Gillman every success in his noble work.

SCIENCE IN GERMANY

(*From a German Correspondent.*)

MUCH as may have been written about bone-formation, yet this theme seems still to be inexhaustible, as in the current series of the "Archiv für mikroskopische Anatomie" (of which we gave the contents in a former report) no less than three papers are published on this subject. Two of these, those by Strelzow and by Stieda, speak of the ossification of cartilage and of bone-growth, and arrive at quite contradictory results. The older view on bone-growth starts from the supposition that the bones once formed undergo no further plastic change, that their single parts cannot displace each other, that therefore an insterstitial growth cannot be imagined. If the growing bone, as usual, does not merely show a uniform increase in size, but little by little changes its shape too (the bent bones for instance, the bends of which change during growth), this naturally leads to the supposition that besides the deposit of fresh material, a solution or absorption of those older materials took place, which did not fit the new shape. In opposition to this view, which Stieda also defends, Strelzow tries to prove that the bone grows interstitially, that therefore it can change its shape in an outward direction without reabsorption of any of its parts, that it is useless therefore to suppose the latter to take place, and that there is no reason for such a supposition. Now, with regard to the change from cartilage to bone, it has certainly been proved, for most cases, that the cartilage is first destroyed before in its place a bone grows from fresh materials. But while Stieda thinks this the case everywhere, Strelzow observes that the lower jaw and the shoulder-blade form exceptions to the general rule, the cartilage there passing immediately from its softer state to bone. Hertwig's observations, which he makes with regard to his investigations of the teeth of Reptilia, have a much more extensive range. In Hemibatrachia the teeth form earlier than any other bones of the head, and starting from this basis those bones in the oral cavity are destroyed, which only cover the exterior of the original cartilage skeleton, and are therefore called covering bones. In frogs these bones certainly form without the help of the teeth, which only appear at a later stage; but as frogs (Batrachia) and salamanders (Hemibatrachia) are of the same order, and particularly as the former are the more recent family, Hertwig thinks that in their ancestors the formation of teeth took place in the same way as in the salamanders now, but that in course of time they lost the primitive bone-forming teeth and retained only the bones resulting from them. The formation of teeth now observed in frogs is therefore a secondary phenomenon. Just as the bones of the oral cavity have their origin in the teeth, Hertwig supposes the covering bones on the exterior of the head to result from scales, and states that this is still very evident with certain fishes. What is a rule for lower vertebrata may also be applied to the higher orders, so that *all* covering bones may be derived from scales or teeth, which in sharks and rays are still equivalent and homologous formations. Therefore sharks and rays must be looked upon as the oldest forms of Vertebrata provided with bones; they are succeeded first by salamanders, then by frogs, and finally by the remaining reptiles, birds, and Mammalia.

It is a well-known fact that the gland-cells only absorb certain materials from the blood in order to convey them, more or less changed, into the hollow interior of the gland organ, and thus to furnish useful substances to the organism (secretions), or to remove useless ones from the same (excretions). Wittich demonstrates these relations in a particularly clear manner ("Archiv für mikroskopische Anatomie," 1875). After the injection of differently coloured solutions (carmine ammonia, indigo-sulphate of soda) into the blood of living rabbits, these colours are again excreted by the kidneys. If the animals are killed during this excretion, and the glands are examined, the carmine is only found in the gland vessels, not in their cells; the indigo, however, in the cells also. Such experiments evidently show that the gland-cells have a sort of selective affinity for the two colouring materials, letting the one pass entirely, and partly retaining the other in their interior.

In the same journal Neumann acquaints us with an interesting property of the cells which coat the abdominal cavity of a frog. It is known that some of these cells in female frogs are furnished with cilia, by the motion of which the ova ejected from the ovary into the abdominal cavity are introduced into the openings of the oviduct. Waldeyer, in his book, "Ovary and Ovum," had maintained that as the essential parts of the female genital organs result from the coating of the embryonal abdominal cavity, those ciliated cells physiologically connected with them result from the same basis, viz., the germ-epithelium; while the whole remaining coating of the *later* developed abdominal cavity, with its entirely different physiological signification, must be a formation genetically different from the former. Goette had already proved ("Entwickelungsgeschichte der Unke") that all those formations, together with several others, result from the uniform cell-coating of the abdominal cavity of the embryo. Neumann now specially proves their genetic identity by the observation that these ciliated cells only occur at the time of sexual maturity in the uniform epithelium of the abdominal cavity, and that therefore they represent local transformations of the same. This again confirms the theory, which Goette (*l.c.*) defends for the whole organism, that each embryonal part is not unconditionally intended for certain formations (which has been an accepted belief since Remak), but that from one single and uniform part in the embryo quite different tissues and organs can and may result, solely depending on the locally changing conditions of development. For instance, the coating of the embryonal abdominal cavity, besides the parts already mentioned, also furnishes the fibrous tissue of the intestines, the kidneys, and the heart.

THE LAWS OF STORMS*

Recent Criticism and Contrary Theories.—The rules referred to in last article are only empirical and are derived from no theory. Mechanics ought to take them in hand and explain them; but it has not been able to do so, for the circulatory movements of both liquids and gases are as yet a closed letter to that science. They are to-day in the same position as were Kepler's laws before

* Continued from p. 405.