about £1,600, were distributed among various competitors, almost exclusively of French nationality.

THE following table gives the mumber of students from the different medical schools who passed the licensing examinations now required in the State of New York for the year ending in 1896:

Schools.	Number of Candidates.	Honor.	Per cent. accepted.
University of the State of New York	2	1	100
New York Medical College and hospital for women	7	0	100
College of Physicians and Surgeons, Columbia University	142	11	92.9
Syracuse University	24	0	91.6
Bellevue Hospital Medical College	47	3	89.3
University of Buffalo	36	0	89.2
Long Island College Hospital	64	0	87.5
Niagara University	15	0	86.6
Woman's Medical College of the New York Infirmary	7	2	85.7
Albany Medical College	47	0	85.1
Eclectic Medical College	20	3	85
New York Homeopathic Medical College	27	3	81.4
New York University	79	2	75.9

THE report of the special commission appointed by the Dutch government to discuss the scheme of draining the Zuyder Zee has been submitted. According to the *Railway Review* it states that such an undertaking is quite possible. The work would take 31 years for completion, and every year 10,000 hectares of land would be restored to cultivation. A dike 30 miles in length would have to be constructed, extending from the extreme end of South Holland to the eastern coast of Friesland. The building of this dike, 35 meters wide at the base and six meters high, will take nine years. The total cost of the work is estimated at £26,000,000, which includes the amount to be paid in indemnities to the fishermen of the Zuyder Zee. The total value of the land thus reclaimed from the ocean is estimated at £27,000,000, so that the Dutch treasury net a profit of £1,000,000, without reckoning the substantial gain to the public wealth and a corresponding increase in the annual revenues from duties and taxes.

UNIVERSITY AND EDUCATIONAL NEWS.

THE Rev. Thomas J. Conaty will be installed as Rector of the Catholic University, Washington, on January 19th. It is expected that Dr. Conaty and Cardinal Gibbons will make important speeches outlining the policy of the University.

EX-SENATOR SAWYER, of Wisconsin, has added $5,000 to his recent gift of $25,000 to the endowment fund of Lawrence University, in Appleton, Wis.

PROF. CHARLES F. CHANDLER has retired from the professorship of chemistry and medical jurisprudence in the College of Physicians and Surgeons, but retains the professorship of chemistry in Columbia University. Prof. Thomas Egleston has retired from the chair of mineralogy and metallurgy in Columbia University, and has been made professor emeritus.

DR. KLEBS, the German pathologist, has been made professor in the Rush Medical College, Chicago, and will also occupy a position in the post-graduate medical school of the University of Chicago.

A SCHOOL of science, with twenty-seven professorships, has been founded at Madrid.

THE following appointments are announced: Dr. G. H. Bryan to be professor of pure and applied mathematics in the University College of North Wales, Bangor; Prof. E. Pringsheim to be professor of physics and Dr. Karl Friedheim to be professor of chemistry in Berlin University; Prof. Paul Staeckel, of Königsberg, to be professor of mathematics in the University of Kiel; Dr. Franz Nissl to be docent in anatomy in the University of Heidelberg.

DISCUSSION AND CORRESPONDENCE.

ON CERTAIN PROBLEMS OF VERTEBRATE EMBRYOLOGY.

I CRAVE your permission to rectify certain mistakes into which the reviewer of my recent work has fallen in his notice in SCIENCE of November 20th (p. 763). Your reviewer makes the following statements:

(1) "So far as has yet appeared, this theory (of antithetic alternation of generations in Metazoan development) rests upon the author's observation that the epidermis contributes, in early embryonic stages, to the production of nerve cells. The transformations of these cells *he has not followed*," etc., (2) "hence the whole embryo is a transient structure," etc.

(3) "The author discourses at length upon the well-known fact that in all vertebrates there is an embryonic period, at the close of which the anlages of all the principal organs are present, but not yet differentiated."

(4) "We have been unable to see that the elementary facts, which the author has collated, are anything more than what is commonly taught beginners in embryology, (5) nor to recognize that they afford any arguments to support the author's theory of 'antithetic alternation.'"

As regards the heading 1, the reviewer is apparently unaware of the existence of the completed part of my work published in April, 1896, in the Zool. Jahrb. under the title, 'The history of a transient nervous apparatus in certain Ichthyopsida—an account of the development *and degeneration* of ganglion cells and nerve-fibres.'

The work contains some of the results of six years' investigation of the matter; it was carried out on upwards of 120 embryos (out of a collection of 500 or more), whose sizes ranged from 5 mm. to 19 cm., and it is illustrated by 8 double plates containing more than 131 figures. I assert, and challenge your reviewer to prove the contrary, that in the work in question *the whole of the transformations* of these cells *to their complete degeneration* are described, illustrated and established. I have asked many competent embryologists what was their opinion on this point, and not one of them expressed anything but the conviction that I had completely established the transient nature of the ganglion cells. Hence I conclude that the reviewer must have been dreaming when he wrote "it may be questioned whether *a failure to study the fate of certain cells* in an embryo is a sufficient basis to construct a revolutionary theory upon." When I wrote my work upon them I had *only studied them for six years!*

(2) No such foolish idea as that mentioned in the review, to wit, that 'the whole *embryo* is a transient structure,' has ever entered my head as any time. What I believe, and what I have written more than once, is that there is in the development of every vertebrate a more or less reduced larva or '*phorozoon*' of a transient nature, that an embryo or sexual generation could be transient has never yet occurred to myself, or to anyone else, so far as I am aware.

(3) This is supposed to be a criticism on what I have discribed as the 'critical stage' or phase. The reviewer considers that there is no new discovery in this, but he adduces no evidence for his belief, that it is 'a well known fact,' and that what I have brought forward is nothing more than what is taught beginners in embryology.

If it be a well-known fact, this existence of a corresponding period in the development of vertebrates, and (4) if the facts I have described be such as are usually taught beginners in embryology, surely there must be evidence of it in some of the text-books of embryology. There is certainly no evidence of this 'well-known fact' in the *literature* of embryology, beyond the few lines I have cited from His regarding the human embryo, and, although I am pretty well acquainted with the more important current text-books, I have never been able to find anything in any of them, not excluding Minot's volume, bearing on the matter.

Why, moreover, within the past few years, should Keibel and Oppel, who are well versed and skilled, working and teaching embryologists, have spent so much time and labor in searching for corresponding phases in embryos of different vertebrates, and why should they have failed to find my critical phase, if it were already known to exist? Founding on these researches and my own, I state that there is only one such corresponding phase in the development and that it was first described in the pamphlet under consideration. If your reviewer questions the truth of this, let him produce his evidence. I believe, and the contrary has still to be proved, that my pamphlet contains the following novelties, fate of the yolk, yolk-sac and merocytes, discovery of a corresponding critical phase in the development of

various vertebrates, explanation of the premature birth of the opossum, the main outlines of the history of the mammary glands and placenta and a number of other items.

It was published and appeared as a separate work, because it was felt that the facts described were sufficiently novel and important to warrant such a course, and not merely 'to secure attention' to my theory. The 'note of personal exultation' is no such theory; it is intended to be an invitation and challenge to scientific men to examine and, if possible, to refute my arguments. The latter is something the reviewer has carefully avoided to attempt.

The last paragraph cited (5) is merely the expression of the critic's personal opinion and, unless supported by argument, can carry no conviction. The reviewer offers no reasons for his adverse judgment; until he does so, it is out of question to consider, or even to understand, what standpoint he takes up. A mere denial of the truth of my theory is not argument, but dogmatism.

J. BEARD.

UNIVERSITY OF EDINBURGH.

MR. BEARD'S paper on the transient nervous system was well known to me, and I consider my statements correct, and I made them deliberately. There are two methods, and only two, that enable the investigator to trace the forms and connections of nerve-cells, the Golgi method and the methylen-blue method. Mr. Beard employed neither of these, and, as a matter of fact, has not traced, and was necessarily unable to follow, all the essential transformations of these cells. His research is an extremely interesting one, and, in my opinion, important, but the results are not available in support of the 'Phorozoon' theory. The number of embryos and of years devoted to the research do not make up for the exclusive use of an insufficient method.

The facts referring to the condition of the organs at the 'critical stage' are given more or less fully in all embryological text-books. The occurrence of the critical stage is not specially mentioned, because no embryologist, except Beard, has heretofore regarded it as more or less important than preceding and subsequent stages. The error of Mr. Beard's, which I note, is the assumption that this stage is 'critical,' for stages before it and after it could equally well be selected and traced through the vertebrate series as accurately as the 'critical' stage. If Mr. Beard knows the contrary of this he must have compiled the characters of various classes of vertebrates at other stages and found that they do not agree. The detailed publication of such a compilation must precede a claim for serious attention to his hypothesis. As stated in the notice of Beard's work, the compilation of characteristics in vertebrate embryos of various classes and all approximately at the same stage is useful, and if corresponding tables of other stages were also compiled they would also be useful. It would be interesting to know what proof establishes the 'critical' nature of the stage selected by Mr. Beard. He has not as yet published any such proof, although the *onus probandi* is his. He calls for disproof, but proof from him must come first. Mr. Beard states that he compiled the facts, yet complains that I say the facts are well known. Does he mean that the facts he quotes from various authors were unknown? My review is a protest against two tendencies: first, to solve, embryologically, morphological problems without sufficient regard to histogenesis; second, to push speculation indefinitely beyond observation. Both tendencies have been marked in Mr. Beard's previous papers, as well as in the one under discussion.

I regret that my criticism cannot be more favorable.

CHARLES S. MINOT.

SCIENTIFIC LITERATURE.

The Survival of the Unlike. A collection of Evolution Essays suggested by the study of domestic plants. By L. H. BAILEY. New York, The Macmillan Company. 1896. 515 pp. 8vo.

Whatever Professor Bailey writes is interesting reading. He has the rare gift of an entertaining style, and what he writes people want to read. All his previous books have been widely read, and this will prove to be no exception to the well-established rule. The secret of this popularity, if there be any secret about it, is that when he writes he has some-