

ing the actual domination of the Etruscans over Latium. Servius is a Latin conception belonging to a solar cult, and associated with a group of legends to which belong Virbius of Aricia, and Hippolytus, Pelops, and Hippodamia.

All this is very alluring and most interesting, but it is not always convincing. In fact, Professor Pais's own teaching would lead us to view with incredulity many of his theories. Moreover, his style is not such as to attract the general reader or even to detain the scholar, for it is very involved, and is lacking in logical sequence; which tends to make a difficult subject still more difficult by reason of the ambiguity. The translation is very well done, although the paragraphing is often bad. There is little to suggest the mere transfer of Italian phraseology into English. On page 25 "semi-uncial" is contrasted with "Latin writing," where the evident intention is to refer to half-uncials and capitals. On page 142, the sentence, "Another divinity was still more intimately connected with Diana Aricina—namely the Nemus, that is, the grove of Aricia," is unintelligible. Probably Virbius is the divinity thought of. We note also *viscus* for *vicus* (p. 17), *Camasa* for *Camases* or *Camasese* (p. 143), *finestra* for *fenestra* (p. 149), *memorensis* for *nemorensis* (p. 144), cyclops "were" for "was" (p. 158), *Sanherib* for *Sennacherib* (p. 162), *examing* (p. 163), *word* for *work* (p. 226), and *Palatuar* for *Palatium* (p. 226). The book closes with sixty pages of notes in fine print which serve as valuable corroboration of the statements in the text. The index, which is indispensable in a work of this kind, has been omitted.

The Mecklenburg Declaration of Independence, May 20, 1776, and Lives of its Signers. By George W. Graham. New York: Neale, 1905.

The periodic revival of discussion of the Mecklenburg Declaration of Independence is once more upon us, and the zeal of the advocates on either side of the question has been quickened by the appearance of a clever forgery, somewhat stupidly put upon the market, and therefore denounced before disposed of at a high price. In his book Dr. Graham claims to present new evidence in favor of a Declaration. This evidence is (1) a poem, dated 1777, called the "Mecklenburg Censor," in which mention is made of a "rabble":

"In congress, they, the very first,
Their independence to declare."

(2) a child born twelve years after the alleged Declaration and called "my Independence Boy"; (3) deeds for land which date from 1775 and not from 1776 as the year of independence; (4) a schoolboy's declamation in 1809, mentioning the Declaration, and (5) a discussion of how Martin and Gardner obtained their information of the paper. Dr. Graham believes that the resolves dated May 31, which have been accepted by historians, were adopted May 20, and were additional and supplementary to a true Declaration of Independence.

However interesting as a discussion of authorities this plea in favor of a Declaration may be, it does not carry conviction. Because Martin and Gardner associated with men who were present at a convention in Mecklenburg in May, is no reason why

they should have discussed the matter with them or obtained original material. The resolves of May 31 suspended all royal commissions as null and void, and placed all legislation and executive powers in the Provincial Congress. This was in some degree a declaration of independence, and the reference in the "Censor" and in the schoolboy's oration could apply as easily to these sections as to a separate and distinct Declaration. Further, the schoolboy was a pupil of the Rev. Dr. Caldwell, who married the daughter of John McKnitt Alexander, secretary to the meeting, and had thus been under the influence of one interested in maintaining the existence of a Declaration. The fact that the resolves of May 31 were widely copied, while the more noticeable Declaration has left no trace in contemporary newspapers, is a difficult point against the passage of the latter, and is not to be lightly solved by general assertions. Nor does Dr. Graham's mode of treatment inspire confidence, for under his hands a supposition soon becomes a certainty. Martin, at the end of his chapter on the Declaration, gives as his authorities, "Records, magazines, gazettes." Martin told Dr. Hawks he had found a copy of the Declaration in the western part of the State prior to 1800. To this statement Dr. Graham adds: "Whether it was a manuscript or newspaper copy is not stated, but probably the latter." A few pages later, our author states positively that Martin had read the *Cape Fear Mercury* of June, 1775!

This *Cape Fear Mercury* is not known to exist in any collection; but it is entirely gratuitous to suppose that a copy was abstracted from the British records by Andrew Stevenson, when minister plenipotentiary at the Court of St. James. There is nothing in his official correspondence to show that he had made any discovery on so important a question, and to justify belief that in 1837 the followers of Jefferson were so jealous of his fame that they would employ Stevenson to destroy evidence of an early Mecklenburg Declaration. It would be much more to the point to discuss contemporary records, which have not yet been entirely exhausted.

For instance, why did it require only twelve days for the news of Lexington to reach Williamsburg, Va., and nearly twenty-one days to go further south and west to Mecklenburg, N. C.? Yet the news is said to have reached Mecklenburg while the convention was sitting, and led to the framing of the Declaration. Then, too, the supporters of the Declaration say that four copies of this important paper were sent by special messenger to the Continental Congress, and the receipt was acknowledged and encouragement given by Hancock and by a joint letter of the Carolina delegates. Yet the letter-book of John Hancock does not show any such letter, the joint letter has never been traced, and the two men in Congress most interested in obtaining every support for independence, Adams and Jefferson, had never seen or heard of the Declaration until it was first printed in 1819. Jefferson believed it to be a forgery, and in an unpublished letter to him Adams echoed his opinion:

"It appeared to me utterly incredible that they should be genuine; but there were so many circumstances calculated to

impose on the public that I thought it my duty to take measures for the detection of the imposture. For this purpose I instantly inclosed the *Essex Register* to you, knowing that if you had either seen or heard of these resolutions, you would have informed me of it. As they are unknown to you, they must have been unknown to all mankind. . . . But who can be the Demon to invent such a machine after five and forty years, and what could be his motive? Was it to bring a charge of Plagiarism against the Congress in '76, or against you, the undoubted acknowledged draughtsman of the Declaration of Independence? Or could it be the mere vanity of producing a *jeu d'esprit*, to set the world agasp and afford a topic of conversation in this piping time of Peace? Had such Resolutions appeared in June '75, they would have flown through the Universe like wildfire; they would have elevated the heads of the inhabitants of Boston, and of all New England, above the stars, and they would have rung a peal in Congress to the utter confusion of Toryism and Fidelity, for a full year before they were discomfited [discomfited?]."

And a few days later he again wrote:

"If these resolutions were genuine, they ought to be published in every Gazette in the world. If they are one of those tricks which our fashionable men in England call hoaxes and bores, they ought to be printed in all American journals, exposed to public resentment, and the author of them hunted to his dark Cavern. For although you and I should as easily believe that a flaming Brand might be thrust into a Magazine of Powder without producing an explosion, as that these Resolutions could have passed in 1775, and not been known to any Member of Congress in 1776; and if they were not known to you, as I am very sure they were not, it is impossible they could have been known to any other Member."

The absence of any record in the journals of the Congress of their receipt is not conclusive, as Thomson had not yet perfected his method of noting papers and reports coming to the Congress. We may therefore accept the tradition that Captain Jack was sent to Philadelphia. It does not follow, however, that he was the bearer of a Declaration or even of the resolutions of May 31, and it is surprising that attention has not been called to an entry in the Journals of June 26, of resolutions on the State of North Carolina, recommending the good people of the colony to associate, establish a militia, and support the "American Association." In the original Journal the entry does not show that it was on North Carolina, and the corrected Journal supplies that information. This action must have been suggested from North Carolina; and as Congress had been for days sitting as a committee of the whole on the state of America, the delay in adopting the resolves has no significance. Could not Jack have brought this suggestion, and after time his journey have become associated with the alleged Declaration? Could not Jack have taken back to North Carolina the joint letter, dated June 19, of the delegates of that colony in the Continental Congress, urging the people to greater exertions and to embody a militia? And would not Hancock have transmitted the Congress resolves of the 26th in a letter? These are quite as reasonable suppositions as those put forward by Dr. Graham, and answer to known dates and circumstances more definitely and satisfactorily.

Congress of Arts and Sciences, Universal Exposition, St. Louis, 1904. Edited by How-

P 1133

ard J. Rogers, Director of Congresses. Volume I: History of the Congress, by the Editor; Scientific Plan of the Congress, by Hugo Münsterberg. (Philosophy and Mathematics.) Boston: Houghton, Mifflin & Co. 1905. 8vo, pp. 527.

In every future history of the human mind the signal success of the St. Louis Congress must be commemorated. Justly to distribute the credit for it would probably be impossible at present. We certainly shall not attempt such a task. Speaking in the rough, as men usually speak of the credit for great achievements, the idea was the creation of Professor Münsterberg, and it was a creation such as few men are privileged ever to make. The history of its evolution will be found narrated in this volume as minutely as possible by Mr. Rogers, while the plan is explained by Professor Münsterberg. This first volume contains twenty-five papers read to the philosophical and mathematical sections of the Congress by some of the men of all the world whose words on such subjects best commend themselves to our attention.

The introductory address, by Professor Newcomb, is very properly a blend between an ordinary presidential address and an oration, and is very accurately in taste. Custom calls for the suggestion of a scientific idea in a presidential address, and an idea of wide scientific appeal. If Professor Newcomb had a hundred such jotted down in his note-book, he could not have selected one of more pressing importance or of more evident truth than the idea he chose, while in its whole philosophical scope it has certainly been treated hitherto with something like disdain; so that those who for many years have been preaching its salutary and consequences must be gratified to find their idol taken up by a man so prominent in the world of science as Professor Newcomb. The idea is, that while it may be true that all evolution, be it physiological or physical, intellectual or spiritual, individual or social, proceeds without any strict breach of continuity, yet it is universally found that in every development there are at least two extraordinary leaps. Professor Newcomb only mentions one, which he illustrates in the launching of a ship; but we venture to point out that the laying of the keel is another such great step. So in the development of an individual animal, one step takes place when the unit-cells of two progenitors unite, a third life suddenly appears, while another step takes place when the new being is launched and breaks its way into the element it is to inhabit. Now the evolutionists never cease to tell us that we are to look at the life-history of the individual to find there a miniature record of the past history of its race. If this be so, the logic of science commands us to begin with the hypothesis that there have been at least two cataclysmic epochs in the development of the race, and forbids us to surrender this hypothesis until inductive inquiry has fairly ascertained its truth or falsity. There is a natural presumption in favor of something like the doctrine of universal continuity, but upon uniformitarianism, which goes further, there lies a heavy burden of proof.

We can here mention only one or two among at least a dozen strikingly instructive papers that the reader will want to per-

use long before he will have sucked their juice. It is a curious classification which adds mathematics to the usual list of normative sciences, aesthetics, ethics, and logic; and since a reviewer's *matter* is infallibility, we will curiously say that it is a confusion of thought to class mathematics with the *theory* of reasoning simply because its business is to say what conclusion would necessarily follow from each given assumed premiss or premisses. One of the most interesting communications in the volume is a discussion of the definition of mathematics, by Professor Bocher of Harvard University. Like all others who have discussed the question, he seems to have quite overlooked a definition which one would think would have been insured against neglect by being embedded in one of the most famous of all philosophical writings. We refer to the definition of mathematics as the science of order. It is Descartes who puts it forth. He, indeed, says "measure and order"; but it has been for many years well known that quantity is nothing but ordinal sequence. We mean that this is well known to those who are versed in the subject, but not that it is well known to all those who ought for their own sakes to understand it; nor that it is well known to the whole indolent breed who call themselves "thinkers."

But, to return to the juxtaposition here of philosophy and of mathematics, the comparison that it compels between the general state of intellectual development of the two groups of students is one of the most impressive lessons of the whole volume. Yet mathematics is not as well represented in the volume as is philosophy; and particularly the expression of all that semi-logical department of mathematics which keenly interests every variety of pure intellect is, excepting in Bocher's paper, distinctly weak. In applied mathematics, Boltzmann and Poincaré continued at St. Louis a controversy—a very uncontroversial controversy, it is true—which was not really of a mathematical nature and which had been quite exhausted, as far as they were concerned, years before. The subject should lie fallow until some new point of view is found.

A congenital defect of such a congress, in the province of philosophy at any rate, is that the principal speakers must be very few. At St. Louis there were two only for each section. To select the best two, the selector should add to a superhuman insight an absolutely complete and thorough acquaintance with all the young philosophers. For it must be the young men, if any, who are to open our eyes. Their elders have been tried and found wanting. The selection that naturally gets made is that of the men who, in the later of the previous years, have put forth the most prepotent ideas. But it is just these men that we have no need of hearing. The old tune is still running in their heads; they will harp on the one old string. The man who in silence and obscurity has been creating some strange, beautiful, and illuminating conception is the man from whom we desire to hear, but from whom there is little chance of hearing in such a congress. Some of the old leaders, and some only, express themselves in this volume, together with a few who, if not old leaders, have recently so expressed themselves that all philosophers would know what they had to

say. The truth is, that the personal *rencontres* were of liveliest interest, but the record of what was said is considerably less so.

Geology: Earth History, Vols. II. & III. By T. C. Chamberlin and R. D. Salisbury, Professors of Geology and Geography at the University of Chicago. Henry Holt & Co. 1906.

That geology is not yet an exact science, and that many of the commonly accepted views of the earth's history rest in the last analysis upon undemonstrated assumptions, is emphasized in a striking manner in the above work. Contrary to the plan of other text-books on the same subject, marked attention is paid to the hypotheses of the origin and early phases of the earth, and several alternative views are discussed in considerable detail. For a number of years past, Professor Chamberlin's studies have led him to consider these fundamental problems connected with the origin of the earth, and have convinced him that neither the commonly accepted Nebular Hypothesis of Laplace nor the Meteoric Hypothesis of Lockyer and Darwin can possibly meet the demands that later discoveries put upon them. For example, these hypotheses presuppose an original crust with somewhat definite chemical and physical characteristics, such as no great basal formations are now known to possess. Indeed, recent studies in Canada, the United States, Great Britain, Scandinavia, and Finland have demonstrated that great granitoid areas of the Archaean rocks, which were previously supposed to be a part of that crust, were intrusive, and that they had been forced into rocks which were formed on the surface at a period much later than the original crust. Professor Chamberlin has been led, therefore, to develop the "planetesimal hypothesis," in which "it is assumed that the parent Nebula of the solar system was formed of innumerable small bodies, planetesimals, revolving about a central gaseous mass, much as do the planets today." He finds in the numerous spiral nebulae, with coiled arms or streamers, with luminous centres and knots of light on the streamers, and which present a *continuous* spectrum, some reason for assuming that such planetesimal systems exist in great numbers. By this hypothesis, "the evolution of the system consisted in the aggregation of these innumerable small bodies into much fewer larger ones." "The earth is supposed to have started as a nebular knot, acting as a nucleus, and to have grown gradually to its present mass by accretions" of the scattered planetesimal masses.

This hypothesis calls for a relatively slow growth of the earth—cold at first, but with a rising internal temperature developed in the central portion, chiefly through compression, and creeping outward. The young earth is believed to have had no atmosphere until it attained about one-twentieth of its present mass, owing to its inability by the force of gravity to attract and hold to itself the light atmospheric gases against their high molecular velocities, which would carry them off into space. But when the growing earth reached the requisite mass, an atmosphere was gradually accumulated by contributions from the free atmospheric

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