But is it true? Brilliance of parts is not always, I am almost inclined to assert not commonly, accompanied by a disinterested love of knowledge, though often enough by ambition, which is a very different thing; nor, unfortunately, is a love of knowledge always associated with the capacity to gratify it. To many men, again, opportunities fail, or health, or energy of character, or perseverance, or the means enabling them to wait for success in the career chosen, or, lastly, circumstances may have compelled them to adopt an unsuitable career, and so their intellectual lives are wrecked. It is only in respect of the residue remaining after elimination of these cases that the reproaches addressed to the examinational system are capable of being justified. What proportion that residue may bear to the totality of brilliant failures it may be difficult to determine. My impression is that it is a very small one. At any rate, it is so in the University to which I belong—the University of London. So large a proportion of the men who have taken high degrees at that University have in after life fully maintained, to say the least, the distinction of their University record, that the failure of the residue—if such failure there be—may be justly ascribed to causes of the nature above indicated rather than to any ill effect of the examinational system. The assertion may easily be verified by reference to the Honours Lists, more especially in the Faculties of Science and Medicine. I mention these Faculties because it is much easier to trace the after life of graduates in them than in the other Faculties. But on looking over the list of M.A. medallists, I do not find a single name which suggests any lack of after-life response to earlier promise.

Finally, on turning to the University record of many, probably of most, of the eminent men of the day, the very reverse of the alleged disparity between promise and performance upon which the protest is based will be found to exist.

I am, for my part, fully convinced after several years' daily familiarity with the working of a purely examinational system, that in examinations we have the best means yet devised of testing the general ability and attainments of young men and women. And I have shown above that competition does not produce any of the evil results complained of in the protest. On the contrary, I believe it to be in most cases—but certainly not in all—a most useful discipline.

But I have no faith whatever either in piecemeal examinations, or in examinations in technical or special subjects, or in mere manipulative matters. I admit, too, that nothing like sufficient attention is paid to the progressive improvement of examinations in accordance with the advance and increasing volume of knowledge. In particular, the range of optional subjects at higher examinations should be greatly extended, that the test applied to each candidate may better correspond with his opportunities and with his mental structure. Above all, the tendency which unfortunately exists to increase beyond measure the difficulty of examinations requires to be carefully guarded against. Too highly pitched syllabuses necessarily involve a low standard of performance, with the result that the successful candidate and the public are equally deceived. F. VICTOR DICKINS.

Burlington Gardens, November 6.

Gresham College.

THE communication of Mr. R. D. Roberts states that the article of Prof. Ray Lankester "is based entirely upon a misapprehension as to the purpose and function of the London Society for the Extension of University Teaching and its position with regard to Gresham College."

I beg to be allowed to state that I can indorse Prof. Ray Lankester's statements with regard to the London Colleges. It is nine years since my connection with King's College ceased, but for nine years I was a lecturer in the Evening Class Department of the College. I know that the College staff, often at great personal sacrifice on the part of some of its members, threw such energy into the teaching of evening classes that their efforts can best be described as thoroughly educational in the highest sense. The number of lectures in the winter courses were twenty-five to thirty, in the summer ten. They were, as a rule, as complete and advanced as similar courses in the Universities, some of them more complete than such courses elsewhere.

At the time when the Society for the Extension of University Teaching was first established, it appeared to me to be a superfluous and mischievous institution. The leading idea which it communicated to the public by very extensive advertisements and reports of meetings was, that there was no such form of education in London, and that the teachers subsidized by the Society were bringing enlightenment from the Universities of Oxford and Cambridge. For several years there were courses consisting of only three or four lectures, delivered in districts widely separated, as, for instance, in Mile End, Kensington, and Dulwich, while a long course consisted of only ten lectures. There was no curriculum in any one centre in either arts or science. The courses of lectures were not even grouped into Departments or Faculties, such as modern languages and literature, Latin and Greek, ancient history and archæology, pure and applied mathematics, experimental science, or biological science. Desultory instruction, not education, appeared to be the object of the Society. The lecturers were, as a rule, qualified for the duties they undertook; some were eminent men, even of the highest eminence; but I do know that others from the Universities should certainly not have been intrusted with the duty of public lecturers until they had undergone an additional term of instruction and training of at least three to five years as assistants to Professors. The Society provided employment for a number of unemployed graduates from Oxford and Cambridge; and at the time, no doubt, it was considered politic and conciliatory to make an assumption of carrying culture to the masses. The young men were willing to take up such duties, for they gained opportunities for practice in the art of teaching which led to possibilities in the way of promotion. There is little doubt that the Society has improved latterly, and it may or may not deserve to be supported by public subscriptions; but it would be a monstrous injustice to King's and University Colleges to place the funds of Gresham College in the hands of this Society. The injustice would be the greater in the case of King's College, because, as I understand, University College has discontinued its Evening Class Department; but for at least twenty years before this London Society for the Extension of University Teaching was in existence or thought of, the staff of King's College, without endowment, were teaching by night as well as by day, and with inadequate remuneration doing more than fulfilling the intentions of Sir Thomas Gresham. The City clerks, engineers, and manufacturers left their places of business to attend these lectures, and obtained sound theoretical and practical instruction in art and science, ancient and modern literature and languages. I have in mind many who have risen to distinction; and there are, no doubt, thousands who are ready to acknowledge the benefit they derived from the evening classes of King's College.

I doubt whether these facts were placed before Mr. Goschen on the occasion when he made his speech on the subject of this Society and Gresham College. It seems to me that the matter should be put before him and all others concerned in a true and proper light, and without partiality.

In conclusion, I will state it as my carefully-considered and deliberate opinion that the Lecture Society called the London Society for the Extension of University Teaching has done no educational work which for extent or solidity is worth consideration in comparison with that of King's and University Colleges.

W. N. HARTLEY.

Royal College of Science, Dublin, November 9.

Divergent Evolution.

SOME of your readers may possibly remember a paper of mine on "The Variation of Species as related to their Geographical Distribution," which appeared in NATURE, vol. vi. p. 222. About the same time I prepared a paper on "Diversity of Evolution under One Set of External Conditions," which was published in the Linnean Society's Journal—Zoology, vol. xi. pp. 496–505. I refer to these papers simply to say that the problems there discussed have occupied my attention more or less ever since.

Part of my paper relates to the subject discussed by Mr. Romanes in his paper on "Physiological Selection"; but as it has been independently worked out, I believe it will be of interest to all who have followed the discussion on the "Origin of Species." The abstract of Mr. Romanes's paper given in NATURE, vol. xxxiv. pp. 314, 336, 362, did not come into my hands till the following January, when my theory of "Divergent Evolution through Cumulative Segregation" was, for the most part, written out in its present form. Since then, and with reference to the discussion on "Physiological Selection," I have worked out the algebraic formulæ given in the fifth chapter, and have introduced explanations of the same.

My "segregate fecundity" and Mr. Romanes's "physiological selection" are the same principle; and our theories still further correspond in that we both insist on the prevention of intercrossing as a necessary condition for divergent evolution. This conclusion was reached by me through investigations made many years ago, and was maintained in my paper on "Diversity of Evolution under One Set of External Conditions," and in still stronger language in articles in the *Chrysanthemum* (Yokohama), January 1883, and in the *Chinese Recorder* (Shanghai), July 1885. In the first of these papers I used the word "separation" to indicate the phase of the principle that results from migration; but for a fuller discussion of the subject I found it necessary to introduce "segregation" as the more significant term; and in the second paper I maintain that "While external conditions have power to winnow out whatever forms are least fitted to survive, there will usually remain a number of varieties equally fitted to survive; and that, through the law of segregation constantly operating, ... these varieties continue to diverge till separate species are fully established, though the conditions are the same throughout the whole area occupied by the diverging forms;" and in the third paper I said, "I am prepared to show that there is a law of segregation rising out of the very nature of organic activities, bringing together those similarly endowed," and causing "the division of the survivors of one stock, occupying one country, into forms differing more and more widely from each other." Since then, my nomenclature of the subject has been worked out with that word as the central symbol of my theory. It is therefore a pleasure to find that Mr. Romanes uses the same word to express the same general idea, giving to his theory the alternate name of "segregation of the fit" (Linnean Society's Journ.—Zool., vol. xix. pp. 354, 395), and in one place at least describing it as "physiological segregation" (see letter on "Physiological Selection," NATURE, vol. xxxiv. p. 408).

As I have explained in chapter iv., I at first thought of using "physiological segregation" in place of "industrial segregation," but finally concluded that it was a term of such wide significance that it could not be well used as the name of any one kind of segregation, while at the same time it was not broad enough to serve as a general term for all kinds. I therefore greatly prefer the term "segregation of the fit." I would, however, so define it as to cover all forms of segregation.

Though our use of this fundamental word is undoubtedly due to our having the same general truth to express, several divergences appear in the development of our respective theories, tending, we may hope, to a fuller elucidation of the subject.

76 Concession, Osaka, Japan. JOHN T. GULICK.

Alpine Haze.

THE peculiar haze mentioned by Prof. Tyndall is no doubt identical with what is commonly met with in some parts of the Mediterranean. During the hottest and driest weather of the summer, and when no wind is blowing, perfectly horizontal strata of haze can be seen occupying the Gulf of Naples. The peaks of the Sorrentine Mountains, with Solara of Capri, Ischia, Vesuvius, Camaldoli, &c., stand out above this haze. The height of the strata rarely reaches 2000 feet, and is more often about 1500 feet. The same facts that led Prof. Tyndall to consider it other than water vapour, and of micro-organic nature, had produced in my mind similar conclusions. This haze, when looked at near the sea, has often a beautiful pink tint, due, no doubt, to a complementary effect from the sea-water colour, as the colour is more marked on the limestone rocks, where the white sea-bottom makes the water look much greener When, however, the observer is cut off from a view of the green sea for some time, the haze has then a light buff colour. The opacity of this haze is so great as sometimes to resemble a slight London fog.

Anyone who would count the number and study the characters of the organisms and other solid contents of the air here at different times would soon settle the question what this phenomenon is due to, and whether there is any truth in the old *blight*. H. J. JOHNSTON-LAVIS.

Naples, November 4.

The Astronomical Observatory of Pekin.

IN your number of November 8 (p. 46), you gave an account of a lecture by Mr. S. M. Russell, of Pekin, on the instruments in the old Observatory there. May I mention that the late Alex. Wylie, about nine or ten years ago, published a full account of them (with illustrations) in the "Travaux de la 3me Session du Congrès International des Orientalistes," vol. ii. Having had my attention drawn to them by some photographs kindly sent me by Mr. Russell, I pointed out the scientific interest of Ko Show-King's instruments (which anticipated the ideas of Tycho Brahe by three hundred years), in a paper published in the Proceedings of the Royal Irish Academy, vol. iii., 1881, and in *Copernicus*, vol. i. J. L. E. DREYER.

Armagh Observatory, November 12.

AN HISTORICAL AND DESCRIPTIVE LIST OF SOME DOUBLE STARS SUSPECTED TO VARY IN LIGHT.

THE light-changes of double stars are, for the most part, of an intermittent character. Unmistakable at one epoch, they may completely evade detection at another. Hence observations of them which, by the nature of the case, cannot be repeated are apt to incur discredit for lack of confirmation. They should, on the contrary, if properly authenticated, be carefully borne in mind, as testifying to an incident in the history of the stars they refer to which, however apparently isolated, must be extremely liable to recur. We have therefore thought that it would be useful to put together, as concisely as possible, a few facts bearing on the supposed variability of some stars which we may reasonably consider to be physically double, referring those of our readers who desire fuller information on the subject to the original authorities we shall cite for their convenience.

γ Virginis = Σ 1670.—The first observation is by Bradley in 1718. The components, normally of the third magnitude, were regarded as equal by all observers until W. Struve, May 3, 1818, noticed the preceding star as slightly the fainter. It continued so for several years; the difference was obliterated from 1825–31, and reversed, doubtfully 1832–33, certainly in 1834 ("Mensuræ Micrometricæ," pp. lxxii. 4). O. Struve's observations, 1840–74, showed decided variability in a double period, oscillations of half a magnitude in a few days being superposed upon a fluctuation extending over many years. An investigation of the law of change, begun in 1851, led to no result, owing to the low altitude of these stars at Pulkowa ("Obs. de Poulkova," ix. 122). Dawes found them equal, 1840–47; but each alternately about a quarter of a magnitude brighter than the other, 1847–54 (Memoirs R. Astr. Soc., xxxv. 217–19). Similar swayings of lustre were constantly apparent to Dembowski (*Astr. Nach.*, Nos. 1111, 1185, 1979). Each star is given as of 3·5 magnitude (combined 2·8) in the "Harvard Photometry" (see also "Harvard Annals," xiv. 454). Gould assigns to them the combined magnitude of 3·1, Pritchard of 2·67; Gore thought them nearer to the second than to the third magnitude, April 5, 1883 ("Cat. of Suspected Variables," p. 362). (The combined magnitude of two third magnitude stars is 2·25.) Owing to their uncertainty of shining, the angle has often been reversed in measuring these stars. They are of a pale yellow colour, and show a spectrum of the Sirian type. They revolve in a highly eccentric orbit in a period of 180 years, and emit fully sixteen times as much light proportionately to their mass, as the sun.

44 (*i*) Boötis = Σ 1909.—On June 16, 1819, Struve noted a difference of two magnitudes between the components; of one invariably 1822–33, but of only half a magnitude 1833–38. Argelander found them exactly equal, June 6, 1830 ("Mens. Microm.," p. lxxii.). To Dawes, in April 1841, the attendant star seemed a shade brighter than its primary, which was rated as of fifth magnitude (Mems. R. A. Soc., xxxv. 232). Dunér's observations at Lund, 1868–75, confirm their relative variability, causing the disparity between them to range from 0·4 to 1·3 magnitude; and he points out that they appeared to Herschel consider-