

on the glacial theory in his various writings on glaciers, is in marked contrast to the ignorance of the true state of the case usually displayed by English authors, who ascribe to Forbes the sole credit of all recent progress in the glacial theory.

Forbes's work commenced in 1841; it was in that year that he made his memorable visit to the Glacier of the Aar, and there found Agassiz, who had at that time already spent five summers in the study of glaciers, and published in 1840 the preliminary part of the investigations carried on by himself and his companions ("Études sur les Glaciers").

Agassiz with his usual freedom in dealing with his associates, which has so often made him appear as following the lead of his pupils, freely imparted to Forbes all he had seen, and certainly had no idea that the hospitality so freely proffered would be returned by the proceedings of Forbes, who appropriated what he could, and misrepresented the nature of his intercourse with Agassiz while his guest on the Glacier of the Aar.

To Tyndall we owe a thorough sifting of the claims of each investigator on the subject, and however unpalatable it may be to national prejudices that the name of Forbes should play a secondary part in these investigations by the side of those of Venetz and Charpentier, Rendu and Agassiz, the fact remains the same, and every fair-minded investigator will thank Tyndall for what he has done.

ALEX. AGASSIZ

Cambridge, Mass., April 15

Scientific Endowments and Bequests

IN the article on scientific endowments and bequests in NATURE for April 24, there is a statement, in reference to the Trinity Natural Science Fellowship, which perhaps requires a little correction.

Although there can be no doubt that the proposed new scheme for the selection of a fellow is in every way better than the old system of selection by routine examination, it is hardly right to speak of the election of a Natural Science Fellow, which took place in October 1870, as an "unsuccessful experiment."

It is certainly much to be regretted that circumstances have prevented the gentleman then chosen from strengthening the staff of scientific workers and teachers at Cambridge; but it is equally certain, that no system of selection that could possibly be desired, would have resulted in the election of a man possessed at once of more promising scientific abilities, and of a more genuine love for science.

The writer of the article seems to think that the examiners on that occasion were in search of what he is pleased to call a "genuine zoologist;" there is no doubt that there was then as there is now, a striking absence of young men of ability, devoting themselves to zoology; but though the college had announced a preference for a physiologist, yet the examiners were empowered to recommend either a zoologist, or one following any other branch of natural science.

F. M. BALFOUR

Trinity College, Cambridge, April 20

Permanent and Temporary Variation of Colour in Fish

ONE or two episodes in the annals of the Brighton Aquarium for the week just ended deserve a passing note.

Among the Plaice, *Pleuronectes platessa*, added to the general collection, is one remarkable example, having the posterior half of its under surface, usually white, coloured and spotted as brilliantly as the upper one; the line of demarcation between these two colours again, though sinuous, is most abrupt, there being no shading through from one to the other as might have been anticipated. This specimen may be turned to good account by advocates of the Darwinian theory, as affording a remarkable instance of the occasional tendency of a specially modified type to revert to its primeval state—the Pleuronectidæ being derived from ancestors originally possessing bilateral symmetry, and an equal degree of coloration on each side.

As the spawning season advances, many of the fish, and more especially certain of the Acanthopterygian order, undergo various important modifications in both their habits and appearance. During the last week or so, many of the larger examples of the Black Bream, or Old Wife (*Cantharus lineatus*), exhibited in tank 4 on the north side of the Western corridor, have afforded a striking illustration of these phenomena. Hitherto their prevailing tint has been a delicate silvery blue, varied by irregular longitudinal lines of pale yellow, a hue scarcely in harmony with

the name by which they are most popularly known. These light colours have now disappeared, or rather become absorbed, in a prevailing shade of deep leaden black, which, while deepest on the back, spreads itself over the whole surface of the fish with the exception of a few transverse lighter bands in the region of the abdomen. The males in particular are most conspicuous for this change, and these retiring from the remainder of the shoal, select certain separate and prescribed areas at the bottom of the tank, where they commence excavating considerable hollows in the sand or shingle, by the rapid and powerful action of the tail and lower portion of their body. A depression of suitable size having been produced, each male now mounts vigilant guard over his respective hollow, and vigorously attacks and drives away any other fish of the same sex that ventures to trespass within the magic circle he has appropriated to himself. Towards his companions of the opposite sex his conduct is far different; many of the latter are now distended with spawn, and these he endeavours by all the means in his power to lure singly to his prepared hollow, now discovered to be a true nest or spawning bed, and there to deposit the myriad ova with which they are laden, which he then protects and guards with the greatest care. Whether the aggregated produce of a large number of females is thus consigned to one bed, and whether the ova are guarded by the male until the young fish make their appearance, are points which, while awaiting confirmation, may be almost confidently inferred, reasoning from the very analogous nest-forming habits of the *Gasterosteus* or Stickleback family, already so familiar to every naturalist. The male of the Lump fish (*Cyclopterus lumpus*) is said to watch over the spawn of the female in a very similar manner, and at the particular time of the year, early spring, when it is deposited, assumes the most lively tints of red and blue, which disappear again after his paternal duties have been discharged, and are not retained through life as has been formerly supposed. On this point we have direct evidence from specimens confined within the aquarium walls. For yet another instance of change of colour in the male fish, associated with its nest-forming habits in the same Acanthopterygian order, I am indebted to a recent visit to the aquarium at the Crystal Palace, where Mr. Loyd directed my attention to a male example of the Cuckoo Wrasse (*Labrus mixtus*), which had formed a deep hollow in the sand of its tank, and was endeavouring in the most persuasive manner to induce a female of the same species to share it with him, swimming backwards and forwards between her and the completed nest, and plainly exhibiting the greatest anxiety for her to follow. The normal brilliancy of this fish was supplemented by a light opaque patch that extended over a considerable portion of the back of his head and shoulders, while the tints of the remaining portion of the body were more than ordinarily deepened.

W. SAVILLE KENT

On Approach caused by Velocity and Resulting in Vibration

PROF. J. CLERK-MAXWELL, in his recent paper on "Action at a Distance," has brought under notice again the experiments of Prof. Guthrie "On Approach caused by Vibration," and has so well summarised in popular language the facts investigated and the conclusions arrived at, that fitting opportunity appears to present itself to me for calling the attention of the scientific world to phenomena closely allied to those under review although more complex in their manifestation, since in these velocity is independent of, yet initiates vibration. That they have not been referred to in the experiments either by Prof. Guthrie, Challis, and others who have taken part in the discussion is probably to be accounted for in the unfortunate although convenient habit indulged in by experimentalists of using the tuning fork as the agent for demonstration.

The following passage from Prof. J. Clerk-Maxwell's paper alluded to will best introduce my own observations—"Here is a kind of attraction with which Prof. Guthrie made us familiar. A disc is set in vibration and is then brought near a light suspended body which immediately begins to move towards the disc as if drawn towards it by an invisible cord. What is this cord? Sir W. Thomson has pointed out, that in a moving fluid the pressure is least where the velocity is greatest. The velocity of the vibratory motion of the air is greatest near the disc. Hence the pressure of the air on the suspended body is less on the side nearest the disc than on the opposite side; the body yields to the greater pressure and moves towards the disc. The

disc therefore does not act where it is not. It sets the air next it in motion by pushing it, this motion is communicated to more and more distant portions of the air in turn and thus the pressures on opposite sides of the suspended body are rendered unequal, and it moves toward the disc in consequence of the excess of pressure. The force is therefore a force of the old school, a case of *vis a tergo*, a shove from behind."

It has been customary with me for several years, when occasion invited it, to demonstrate to my musical friends the physical action existing in the sounding organ-pipe, to show them (taking up a chance wood-shaving lying on the floor of the workshop or a strip of tissue paper) that, heterodox though the teaching be, the stream of air at the mouth of the organ-pipe constitutes a free-reed—visibly, before them the film-like wood-shaving is drawn into the motion of the air, and the beautiful curve of the reed's swing displays itself beyond dispute; then to show them that the air-moulded tongue obeys every law of the free-reed, has its own definite rate of vibration, that the current is so directed that it shall pass not strike the lip, that it is an air-moulded or *aëroplastic* reed as definitely fashioned in substance, strength, proportion, and form, as metal reeds are to produce a required and determinate rate of vibration. First, the velocity of current, a constant upward force; then, the periodicity of vibration as a secondary mode of its activity. The *aëroplastic* reed forming with the pipe a system of transverse vibration associated with longitudinal vibration, and possibly another phase of vibration across the width of the reed enabling it to synchronise with the harmonic range of the pipe; the principle of action of the whole being termed, in my non-academic phraseology, suction by velocity; but if a more exact expression is found its explanation should imply, or better still, include the axiomatic phrase of Sir W. Thomson, "in a moving fluid the pressure is least where the velocity is greatest." To state the existence of an air-moulded free-reed is to give the key to its nature. Flutes, flageolets, whistle-pipes, disc-whistles, form one group with organ-pipes; all are of one type. Then there is another group of free-reeded instruments including the vocal organs, the trumpet, bassoon, oboe, harmonium, and the like, the only distinction between the two groups being that the one possesses reeds of air of definite pitch; and the other possesses reeds of grosser substance, whether it be membrane wood or metal, alike of definite pitch, but in every one the degree of elasticity or pliancy in the substance determines how much of that pitch shall be maintained as the work is done. Velocity is power, and in every conjunction of reed and pipe the reed is the dominant. Most distinctly it should be recognised that the air-reed does work and expends power in doing it. A rod or a string delivers up under a single blow the whole vibrating energy it is capable of—not so the air-column in the organ-pipe, which needs to be beaten the precise number of blows requisite for the pitch of tone elicited.

Reeds of the oboe are as truly free-reeds as are the vocal cords. The stream of air does not necessarily pass down the organ-pipe, but in the oboe it is essential it should pass down the pipe. The action of this orchestral instrument is best explained under the law of "least pressure," showing an identity in principle but with difference of mode; instead of the stream with a lapping action as an air-tongue at the mouth of the organ-pipe, we have an air-current passing between two sensitive reeds down a narrow straw-like tube into the main body of the pipe. The velocity in the little tube immediately causes "least pressure" in the interior, effecting approach and closure of the pair of lip-like reeds, and so on, a perpetual renewing and breaking of contacts, the periodicity of such movement being determined by the sensitiveness of the reed in relation to the air-tube through which the impulses must move before the "dispersion of the vibrations" into the air relieves the reed and fixes the period of its stroke. In further proof that the flue organ-pipe is a free-reed instrument, compare the flute, its representative, with the oboe and clarinet. So little is understood concerning the nature of these wind instruments, that, whenever in the science of acoustics they are referred to, it is stated that the clarinet is a closed pipe, and the oboe an open pipe; that the former produces the series of uneven harmonics and the latter the even series, and the explanation given is that the tube of the one is cylindrical, and the tube of the other is conical. The explanation does not really explain. It is true that the clarinet gives in relation to its length the pitch corresponding to that of a closed pipe, whilst the oboe, though of similar length (scale of key allowed for), is of the pitch of an open pipe, with relative harmonics; yet this difference

arises not in any degree from the shape of bore cylindrical or conical. As well denominate the oboe "a closed pipe" if structure is compared; the one is not more a closed pipe than the other, the true cause of the diversity is in the rate of *reed-vibration* of the clarinet being only half the rate of that natural to the oboe. The proof is clear and open to anyone intent to observe. Place the oboe head on the clarinet-tube, and you will get from this same tube only the two-foot tone instead of the four-foot tone, and with this transformation of pitch the series of harmonics previously wanting. Place the flute-head on the clarinet-tube and the same results follow; showing that the velocity of vibration originates with the reed, and that the flute rightly considered is a free-reeded instrument.

The experience of years justifies me in presenting these conclusions, and should they not be disproved, questions will suggest themselves whether physicists should not look to the disturbance of the equilibrium of air-pressure as the chief element in determining the pitch of sounds produced in organ pipes; whether the long conserved doctrine of "the column of air within being alone the cause of sound" has not been detrimental to investigation as was in older times the doctrine that "nature abhors a vacuum," which, as Whewell points out, retarded science a century by pre-occupying men's minds against observation; and whether it is not through the presence of the law of "least pressure" that vibration of any kind becomes possible.

HERMANN SMITH

The Hegelian Calculus

YES, YESTERDAY evening a copy of NATURE for the 10th instant, sent to my late address at Pierhill, reached me here. The sender annexes the initials W. R. S.—those, presumably, of Mr. W. R. Smith. It was only thus that I became aware of that gentleman's letter on "The Hegelian Calculus," in said issue; and, as I am called upon by name therein, I should be obliged if, in an early number of the valuable publication referred to, you would kindly allow me insertion of this explanatory word in return.

In my rejoinder, mentioned by Mr. Smith as appearing in the current number of the *Fortnightly*, and which (rejoinder) treats, as Mr. Smith truly says himself, his own paper in the same pages "as a virtual concession of the entire case," I speak thus:—

"He that, with whatever tincture of mathematics, will but cast a single glance into the situation as it veritably is, will perceive at once that Mr. Smith's present paper is of such a character as not to demand any further answer from me. It is of such a character, however, that it may be put on the level of a business transaction, and if Mr. Smith can persuade any competent mathematician—say the greatest alive, Sylvester, he being at once mathematician, metaphysician, and German scholar, and at the same time wholly unknown to myself—if, I say, Mr. Smith can persuade any such competent expert to see in this matter with Mr. Smith's eyes, I shall consent to be mulcted in what pecuniary penalty this expert may please."

Of course with reciprocity in the other event. I hope Mr. Sylvester will kindly pardon me for having thus, almost involuntarily, made free with his name; but, if I could say the above then, certainly not less can I say the above now—after this letter of Mr. Smith's. The "character" in allusion is one, I believe, hitherto unexampled in literary controversy, and such that, as I also believe, the most important interests call forth thorough understanding of it. It is in consequence of this "character" that, as I have intimated, I cannot, with any respect to myself, enter into further direct relations with Mr. Smith, and that I must confine myself to what has been said above. All, for that part, may be confidently left to time. Napoleon snipped off, and put in his pocket the alleged gold tassel, assured that use would disclose the tinsel in suspicion. So, as regards the—to me—extraordinary operations of Mr. Smith—not but every *Kenner* must see what is concerned at a glance—I can leave them fearlessly to the intrusions of the public.

Further proceeding, let me intimate in conclusion, however formidable it may look, must, so far as I am concerned, be arranged by a friend on the one part, and a friend on the other. Longer to trouble the public with these altercations can only seem to it impertinent. I, at least, shall be satisfied if it will but consider the result in the end.

Edinburgh, April 18

J. HUTCHISON STIRLING