shortly before that of the Ulloa's ring, 38° 48′ ± 48′, and 38° 28′ ± 22′ Thus the agreement between theory and observation is singularly perfect. JAMES C. MCCONNEL.

St. Moritz, Switzerland.

"The Teaching of Elementary Chemistry."

In reply to Prof. M. M. P. Muir's letter, I wish to say that, judging from his answer, Prof. Muir does not seem to consider it necessary in books of which he is senior author to secure that accuracy of which, from his criticisms of the writings of others, one would expect to find him the champion.

The first extract from the books mentioned sounds curiously to chemists. I consider the statement misleading inasmuch as it appears to convey an idea as to the constitution of caustic soda which is not that generally entertained by chemists; that this is not the intention of the authors, however, is manifest from p. 247 of the "Elementary Chemistry," where the usual view is stated.

It is utterly untrue and misleading to state that, "inasmuch as the result of passing chlorine over yellow mercuric oxide dried at about 100° is to evolve oxygen without forming chlorine monoxide, ... it may still be justly said that in making chlorine monoxide 'we carry out a reaction in which oxygen is produced in presence of chlorine.'"

The facts are briefly these:—

(*a*) When chlorine gas is passed at ordinary temperature over yellow mercuric oxide, which has been previously heated to 300°–400°, chlorine monoxide is obtained.

(*b*) When a large quantity of chlorine gas at ordinary temperature comes rapidly into contact with yellow mercuric oxide which has been previously dried at ordinary temperature, a violent reaction, accompanied with evolution of light and heat, ensues, and nearly pure oxygen is the only gaseous product. If both the chlorine and the mercuric oxide be kept cool by means of a freezing mixture, chlorine monoxide is the only gaseous product obtained. With intermediate conditions of temperature, &c., mixtures in varying proportions of oxygen and chlorine monoxide are obtained. (Pelouze, *Annalen der Chem. und Pharm.* Bd. xlvi. 196.)

The formation of oxygen in the second case must therefore be due to the decomposition of already formed chlorine monoxide, or to the occurrence of a reaction the conditions of which render the existence of part of the chlorine monoxide impossible. I think the majority of chemists will agree with me that the appearance of oxygen under conditions which insure the non-existence of (or as itself a product of the decomposition of) chlorine monoxide, can scarcely be admitted as in any measure explaining the *formation* of the latter.

I do not consider it a "verbal quibble" to object to the use of the term "volatilized" as applied to the mechanical removal of particles of a solid substance.

As to the chemical properties of chlorine, bromine, and iodine, I should indeed be open to the gravest charges of non-acquaintance with chemical classification, had I suggested anything so idiotic as that, say, potassium hypobromite and potassium hypoiodite (if the latter exists) could be *identical*.

I called the passages I quoted misleading, because some of them at least were inaccurate. What amount of inaccuracy is required to make a statement misleading may be a matter for difference of opinion. Apparently it is so.

Prof. Muir states that he will decline to take any notice of my anonymous communications. This, at least, is safe ground: but I can wait for the second editions of the two books, and see if the inaccuracies are eliminated. In the second edition of "Elementary Chemistry" I hope Messrs. Muir and Slater will also describe the methods (omitted on p. 19) for removing air from oxygen. Whilst these methods remain unpublished, I prefer to remain Z.

"Kinematics and Dynamics."

MAY I ask a short space in your columns to refer to a few points in Prof. Greenhill's review of my book on "Kinematics and Dynamics," published in your issue of February 16 (p. 361). I shall be as brief as possible.

(1) "In questions involving the size of the earth (pp. 74 and 80), it is the circumference and not the diameter which should be given in metres, the circumference being 40,000,000 metres," the reason being, I suppose, that in illustrative problems round numbers should be employed as data, with the object of facilitating arithmetical calculation. There are doubtless advantages in this course, and in many problems I have adopted it. But should it be made an invariable rule? Problems based on exact data, such as the ones referred to, on pp. 74 and 80, have for many students a greater interest than those based on approximations.

(2) "The expression 'knots an hour' (p. 60) is irritating to a sailor." But the expression "knots" simply would be either misleading or puzzling to a student unacquainted with nautical abbreviations.

(3) "The formula $\frac{1}{2}v^2 = \frac{1}{2}v_0^2 + as$ is to be preferred to that on p. 34, $v^2 = v_0^2 + 2as$; in all cases the factor $\frac{1}{2}$ should go with the v^2 in the equation of energy." The formula quoted is not an equation of energy, but a kinematical equation. Equations of energy (see pp. 253, 256, 328) have in all cases the form approved by Prof. Greenhill.

(4) "In dealing with rotation, the author would do well to study Maxwell's geometrical representation of the direction by means of the screw, right-handed or left-handed." I have done so; but I find that students more readily grasp a specification of the direction of a rotation when it is made by reference to the face of a clock; probably because few of them are so familiar with right-handed and left-handed screws as they are with clock-faces.

(5) "In a linear strain the increment of distance of two points in the line of the strain is properly their *elongation*; while the ratio of the elongation to the original distance is called the *extension*, not the *elongation*, as on p. 167." And yet Thomson and Tait ("Elements of Natural Philosophy," § 139), Clifford ("Elements of Dynamic," p. 158), Minchin ("Uniplanar Kinematics of Solids and Fluids," § 78), and Ibbetson ("Mathematical Theory of Elasticity," § 53), all define elongation exactly as I have done.

(6) "The author, disregarding the vernacular use of the word 'weight,' defines the weight of a body as the force with which it is attracted by the earth" [I don't (see § 290); but let that pass], "but is at variance with his own definition in the statement of the majority of the subsequent examples, relapsing into the language of ordinary life." No references are given to these instances of backsliding. I have looked pretty carefully through the subsequent examples, and can find no case in which I have used the term referred to in any other sense than that given it by definition. I should be glad to have such slips pointed out to me, if there are any.

(7) "A collection of 500 different ways of spelling the name of the town of Birmingham has been made, and a similar collection could be made from the present treatise of different ways of expressing the simple ideas of the pound *weight* and the pound *force*." It is true that these ideas are expressed by English writers in various ways. And it seems to me desirable that a student should be made acquainted with them. Surely in holding that I should choose one phrase and stick to it, your reviewer is blaming me for not being one of the "mathematical precisionists" at whom he sneers.

(8) "This terminology culminates in the solecisms that on p. 477 we must suppose pressure to be measured in poundals on the square foot in hydrostatical problems; and that if the equation $w = mg$ is supposed to be used with absolute units, the weight of a body is measured in poundals; as if a mathematician asked in a shop for 'half a poundal of tea, or tobacco.'" It is not quite correct to say that, in the hydrostatical equations referred to, pressure must be supposed to be measured in poundals per square foot. In fact it may be supposed to be measured in terms of the unit of pressure of any derived system, as, *e.g.*, the dyne per square centimetre, or even the pound-weight per square foot, provided only the density be measured in terms of the corresponding unit. I am aware that this mode of expressing hydrostatical equations is unusual, but it seems to me to have great advantages, and it was adopted both for this reason and for the sake of making the section on hydrostatics uniform with the rest of the book. With regard to the units in which weight should be measured, the practice of the tobacconist or the tea merchant is surely not our best guide.

(9) "Thus a mathematical precisionist, to express the simple idea of a force of 10 pounds, to be consistent should call it 'a force equal to the weight of the mass of 10 pound weights,' the absurdity of which is evident." The phrase inclosed in quotation marks is not quoted from my book. In my terminology the most precise of mathematicians would express the idea referred to

by the phrase "a force equal to the weight of 10 pounds," which is neither clumsy nor absurd.

(10) "Except for the parts criticized above, on the units of weight, mass, and force, the present treatise shows that the author has read with profit and discrimination the most recent treatises on dynamics." I have been under the impression that in my treatment of these units I had, in the main, followed the most recent treatises on dynamics. May I ask in which of them units are treated in what Prof. Greenhill considers the proper way?

I would like to say also that the elementary proofs of the chief properties of the common catenary, which are given by me, are, with slight modifications, those given in Prof. Goodeve's "Principles of Mechanics." My indebtedness to his book is acknowledged generally in the preface.

I fear my desire to be brief may have made me appear curt. Let meexpress, therefore, my appreciation of the trouble Prof. Greenhill has taken to form a just estimate of the merits of my book, and of the kindly way in which he has spoken of it.

J. G. MACGREGOR.

Dalhousie College, Halifax, N.S., March 1.

Coral Formations.

I AM glad to see the theory that the internal lagoons of coral atolls are excavated by the chemical action of sea-water and the removal of carbonate of lime in solution is now being brought to the test of figures.

Mr. J. G. Ross (NATURE, March 15, p. 462) calculates from his experiments that in this way a sheet of carbonate of calcium half an inch thick can be removed annually from the surface of a lagoon, but strangely adds, "In other words at the same rate it would require about a century to deepen the lagoon one fathom." According to this method of calculating, 144 years is "about a century."

These figures no doubt suit the theory of the formation of coral lagoons very well, but they appear to me quite destructive of the other and co-relative view that the platforms upon which atolls have been formed have been built up by the accretion of the dead shells of pelagic organisms showered down from the surface of the ocean together with the shells of those organisms which have lived on the bottom. I believe that at no place on the surface of the globe are such dead shells being supplied at a rate that would even balance this supposed rate of chemical destruction.

Yet if these figures be correct we shall have to reckon upon the removal from such platforms of more than half an inch annually in consequence of the quicker action which it is said takes place through greater pressure at greater depths.

If, therefore, we accept the dissolution theory of the origin of coral lagoons, it seems impossible to believe in the building up of platforms of calcium carbonate on volcanic or other peaks from varying and unknown depths to the levels necessary for the growth of reef corals. If, on the other hand, we believe that platforms are so built up, it appears equally destructive of the dissolution theory of the lagoons.

Dr. Darwin indicated this difficulty in his letter to me, published in NATURE, November 17, 1887, p. 54, but the figures we are now supplied with enable us to realize it much more vividly.

T. MELLARD READE.

Park Corner, Blundellsands, March 16.

The Movements of Scree-Material.

I PERUSED with interest the abstract of a paper on the above, read by Mr. Davison at the meeting of the Geological Society on the 29th ult.

The phenomenon seems somewhat akin to the movements in the "Stone Rivers" of the Falkland Islands, though another reason has been suggested by Sir Wyville Thomson as the cause of their progress.

Might it not be possible for motion to be produced in loose materials, and in the molecules of certain coherent substances situated at a high angle of slope, by continual though imperceptible vibrations in the earth's crust?

Apart from the changes wrought by alternating temperature, might not the "downward creep" in the lead on the roof of Bristol Cathedral—as observed by Canon Moseley—be due to a "settling down" of the molecules by the constant vibrations of sounds transmitted through the structure, and having their origin within and without?

CECIL CARUS-WILSON.

Bournemouth, March 15.

Were the Elephant and Mastodon contemporary in Europe?

MR. HOWORTH asks this question in NATURE for March 15 (p. 463). Perhaps this extract from a translation of a note from Prof. d'Ancona, of Florence, will satisfy Mr. Howorth: "The soil of the upper Val d'Arno is ascribed to formations of the Pliocene period." In it have been found "*Mastodon avernensis*, *Elephas meridionalis*." Twenty-four other animal remains are identified, all differing from the remains of the bone-caves. In both places respectively these relics belong to contemporary animals.

9 Sinclair Road, W., March 15. H. P. MALET.

EXPERIMENTS IN MOUNTAIN BUILDING.[1]

THE primary object of these experiments was to explain on what mechanical principles the remarkable rock-structures recently discovered by the Geological Survey in the North-West Highlands might have been produced. In experimenting on the behaviour of strata when subjected to horizontal pressure, it has been usual to regard large rock-masses as practically plastic bodies, and to imitate in the laboratory the great flexures and plications of Nature by compressing layers of clay, cloth, and other plastic or flexible substances. It was, however, evident, as soon as the true structure of the North-West Highland area was unravelled, that the rocks had, to a very large extent, behaved like rigid bodies under the enormous lateral pressure to which they had once been subjected. Instead of following the usual method of using plastic materials, the author therefore set to work to devise strata sufficiently rigid to snap rather than bend and become folded on the application of lateral pressure. It is to this peculiarity in the character of the materials, rather than to any great novelty in the methods, that the interesting results obtained are mainly due.

The experiments were of three distinct kinds. The first series was designed to explain the behaviour of strata when thrust horizontally over an immovable surface, and thus to throw light on the phenomena of "thrust planes," such as are now known to occur abundantly in the North-West Highlands between Loch Eriboll and Skye (see NATURE, vol. xxxi. p. 33). To simulate natural strata, layers of damp sand, foundry loam, or in a few cases clay, with laminæ of dry stucco powder between, were employed. In a few minutes the anhydrous powder absorbed enough moisture from the damp beds to enable it to "set" into tolerably rigid sheets. The rock which had thus solidified *in situ*, was next compressed horizontally, by pushing in, by hand, or with the help of a screw, the movable end of the long box in which the strata were formed. One side of the box could be removed at pleasure, and at the end of each experiment it was lifted off, and the section inside revealed, so that it could be photographed or copied if desired.

Fig. 1, which is drawn to a scale of $\frac{1}{12}$ of the original, shows the character of the section produced after the end had been pressed in 20 inches. The central light-coloured band, bounded by stiff stucco laminæ, has undergone no folding, but has become heaped up by means of a series of slightly inclined reversed faults, along which the constant pressure from the right found relief. For this structure the author has proposed the name "wedge structure," as the advancing mass is really raised by being forced over a series of wedges of undisturbed rock.

After pushing the piled-up mass a certain distance

[1] Abstract of a Paper by Henry M. Cadell, B.Sc., F.R.S.E., H.M. Geological Survey of Scotland, read before the Royal Society of Edinburgh, February 20, 1888.