considerable share of his attention in the long interval between the publication of the first and second editions of his book. As to acquaintance with the objects of his writings, he had none, I presume, with that horrid creature with the swim-bladder from which he believed we are all descended; nor with most of the animals which have formed the connecting links between that distinguished progenitor and man; but, nevertheless, his great collection of facts and evidences have been of sufficient weight to revolutionize the history of creation, and to pervert hundreds of thousands from their faith in the poetical narrative of Moses.

However, Dr. Guppy is correct in saying that there are certain upraised ancient reefs in the Lau group of Fiji. Lieut. Malan was no doubt alluding to the islands of Tavuthá, Naiiau, and Kambara, which have every appearance of atolls lifted out of the sea, for they are coral islands of considerable height, with an exterior rim and depression in the centre, between 150 and 200 feet deep. It does not follow that they may not form *now* part of a descending area; nor that the Bukatatanoa and Reid Reefs may not be participating in the movement. It is remarkable that only one reef has yet been discovered which connects the atoll awash with the island many hundreds of feet high which has been, to all appearance, an atoll in past ages. As far as I am aware, there is no island known, except Aldabra (which has a lagoon dry at low water, and is, therefore, an abnormal instance), of coral formation, with a lake in the centre and openings on the lee side; in such a stage as an atoll would be if it was elevated, say, 20 or 30 feet; nor do I believe that anyone has yet seen an upraised island *barrier* reef. The sight would be so phenomenal, that the news of its existence would speedily become public property. It must be remembered that, while proofs of upheaval are easily detected, direct evidence even, of subsidence, is most difficult to obtain. All searching under water is groping in the dark.

I may here remark, as it is our common object to arrive at the truth, and surveyors are not special pleaders, that there is an error in Mr. Dana's examples of subsidence (p. 310, "Coral Reefs," Darwin, third edition). Nanuku Islets are cays on a spur from a barrier reef, and Bacon Islets are formed of coral. It is unfortunate that these should have been inadvertently taken as types so late as 1885, three years after the charts were published, though it is easy to understand how Mr. Dana was mistaken in his first book when he had to rely simply on the excellent sketch made by the officers of Wilkes's expedition, to which he was attached; but these mistakes do not necessarily invalidate the conclusions of the experienced naturalist, who, had he thought of it, could have selected other and better examples from the same group.

It would be premature to discuss the cases of the Tizard and Macclesfield banks before Dr. Bassett-Smith records the results of his examination of the specimens collected, in pursuance of Captain Wharton's directions, chiefly by himself and Lieut. Parry, of the *Rambler*; but this much may be said—the existence of a submarine cliff is established in the former, and the sectional slopes have a great resemblance to those of mountains. The condition of the Itu Aba and its surrounding reef appears to favour Mr. Murray's views; but the general state of the reef does not, as far as I can see, disprove the theory of subsidence. The one Macclesfield bank section is not at all like that of a sinking mountain; and the condition of the coral on the top might indicate either a downward or upward movement, or neither. If we find, as in both these cases, a mixture of dead and live coral on the rim of an atoll, the inference seems more in favour of subsidence than elevation. It implies that a sudden descent to a depth beyond that in which the less hardy individuals of the species can flourish, has killed the growing insect; and that a new effort is being made to regain the surface. Specimens of reef-building corals were brought up from depths below even 30 fathoms; but, as far as the imperfect appliances which we have at present could help us, there was no evidence that masses of coral were at all common below 13 fathoms. The condition of the slopes could be safely compared to the banks of a river stream with the blades of grass growing thinner and thinner as the distance from the water was increased (Darwin, p. 111).

In the case of the Bukatatanoa, and other large reefs of a similar character, my difficulty with respect to Mr. Murray's theory is this: if corals commenced to grow on sediment which had lodged on a submarine inequality, why should nearly all parts have kept pace so evenly in their growth? The highest portion of the bank on which the Bukatatanoa Reef—according to the theory of Mr. Murray—rests, would surely not be around

the sides, but somewhere in the centre. There would be a summit of some kind, to which the sediment would be first attracted. Is it not most improbable that first the pelagic organisms which are dead, and then the coral polyps which are alive, should maintain an even contour around three sides of a bank and many miles away from the highest part of it, and arrive at the surface of the ocean much about the same time? If it is said, How do you know it arrived at the surface at the same time? I point to the connected form of the barrier. According to Mr. Murray, had it not done so, it would have formed innumerable rings instead of being connected, without a break, for so many miles.

Reid Reef, to the north of Bukatatanoa, is a still more remarkable instance. Here, there is a feature, which Mr. Murray mentions, quite apparent, viz. the paucity of coral heads in a lagoon where the barrier is uninterrupted; but the difficulties which I have ventured to place before him are greater than ever: (1) Why should wave-action distribute a bank on the weather as well as the lee side of the land? (2) Why should the insect reach the surface simultaneously on all sides, as shown by the continuous reef and the uniformity of its breadth? To the best of my recollection, the islets within this barrier are of volcanic origin, but the point is not material.

With reference to Mr. Murray's explanation of the deficiency of reef under Mbuksu Levu (Mount Washington), may I point out that it is of great assistance in showing why, when discussing subsidence, it is not necessary to hesitate for examples at those islands which have a barrier on one side and a fringe on the other. On a cliffy, steep side, the sinking fringe would, of course, remain very close to the new coast-line, and, if it assumed the barrier form at all, would rapidly become filled with pieces from the land.

The fragmentary character of the Great Barrier Reef of Australia does not seem to have excited the attention it deserves in this controversy. I believe that there are innumerable passages through it, and that, instead of being termed a "barrier," it might better be described as a collection of large patches.

W. USBORNE MOORE.

8 Western Parade, Southsea, July 9.

The Hailstorm in Liverpool.

Sunday, June 2.—A very sultry afternoon, with heavy thunder-clouds in the south-west and west, and continual rumblings from the same direction, the storm evidently passing over the Dingle and Toxteth Park. Soon after 3.30 p.m. lightning became visible and the storm broke.

At first a few large drops of rain fell, making patches of $1\frac{1}{2}$ to 2 inches in diameter on the cement. In a minute or two, large lumps of ice came slowly down, rebounding to an astonishing height. As the stones became more numerous they decreased considerably in size and fell with greater force. This lasted for ten or twelve minutes. The rain had nearly ceased during the height of the storm, but began again towards the end for a short time. I immediately collected some stones off the grass, and placed them in a dish on some blotting-paper.

They were evidently of two classes; the one having clear ice kernels, the other white misty ice. If the stone was large the kernel was surrounded with another coating of the opposite kind of ice, a dark line intervening between the two, and here and there I noticed a third or fourth layer.

Many in themselves spherical had a pear-shaped appendage, which, however, soon melted away. One or two (Fig. 4) had the second layer thinner than the others, making a hollow between the centre and exterior layer, both above and below.

I found one of a most curious construction (Fig. 3). The stone was all composed of white ice except at one end. The white ice seemed to terminate like the petals of a flower, closing in round a centre of clear ice. This construction was entirely formed below the surface, the stone being otherwise smooth. I could not take accurate measurements owing to the absence of a correct scale, but approximately the stones I drew measured as follows:—

	Breadth.	Width.	Thickness.
(1)	$1\frac{1}{2}''$	$1''$	$\frac{3}{8}''$
(2)	spherical, about $1\frac{1}{2}''$	$1\frac{1}{8}''$	$1\frac{1}{8}''$
(3)	$1\frac{1}{8}''$	$1\frac{1}{8}''$	$1\frac{1}{8}''$
(4)	$1\frac{1}{8}''$	$1\frac{1}{8}''$	$1\frac{1}{8}''$
(5)	$1\frac{1}{8}''$	$1\frac{1}{8}''$	$1\frac{1}{8}''$

The stones had all a metallic taste and also a flavour of ozone.

I have examined some of the stones under the microscope. All have an air-bubble at the centre, and I thought in some I could distinguish a speck of sand or grit as well. The kernel appears to have infinitesimal cracks in the ice, going round the central bubbles in circles. Sometimes these are not spread out all round, but run up to the centre in spokes, widening out as they reach the edge. The dark line between the coatings appears to be composed of small pear-shaped air-bubbles lying with

their narrow end towards the centre, and here and there in the ring are specks of grit or dust.

In the pear-shaped prominences the minute ice cracks appear to be formed in waving lines.

In some (Fig. 4), the air-bubbles are formed near the surface round the second or third layer, and are much larger; in others (Fig. 5), they appear in the kernel instead of the spoke-like formation of cracks.

C. D. HOLT.

Sefton Park, Liverpool.

Use or Abuse of Empirical Formulæ, and of Differentiation, by Chemists.

PROF. THORPE'S review of the work of Mendeleeff suggests to me a question I have several times previously thought of putting, viz. whether chemists are not permitting themselves to be run away with by a smattering of quasi-mathematics and an over-pressing of empirical formulæ. I do not make the accusation; I merely put the question as one suggested by an incomplete and superficial perusal of one or two recent memoirs.

To make my meaning clear, I will state a few facts, and if they are unnecessarily obvious I shall be glad to find them so.

Take percentage composition (p), and specific gravity (s); s is a function of p , and the question is, whether it is a continuous or a discontinuous function. To obtain an answer to this question, the best determinations of s should be plotted on a large scale in terms of p , with the probable limits of inaccuracy laid down, and then the curve should be examined to see whether it possesses, at the points of definite constitution, any kind of discontinuity, whether of slope or curvature. The answer may come out, either that such discontinuity certainly exists, or that it possibly exists, or that, if it exists at all, it must be below a certain specifiable order of magnitude. One of

these is the definite kind of statement that can be made, and nothing else.

In order to assist the eye in forming a judgment, some form of mechanical integrator or differentiator might legitimately be run over the curve, provided due care were taken to avoid the creeping in of errors; but I doubt whether anything could be certainly detected in the derived curves that ought not to be visible in the original curve itself.

The process adopted by chemists seems a less satisfactory plan. I speak under correction. They assume some elementary form of empirical expression for the function, say a quadratic expression with three arbitrary coefficients, and they determine these coefficients to suit three points on the curve, first for one portion and then for another, taking these portions in the stages between one definite constitution and another; they thus obtain a set of quadratic expressions for s in terms of p , each with a more or less different set of coefficients: in other words, they find bits of parabolæ which more or less fit successive portions of the actual curve. They then differentiate each of these, and plot $\frac{ds}{dp}$, and they appear to be struck with the fact that, for each portion, these plottings come out precisely rectilinear; while with the observation that discontinuities exist between successive portions they seem quite pleased.

They sometimes go on to plot $\frac{d^2s}{dp^2}$, and to deduce fresh support for their facts by means of it.¹

Now, were it not that eminent persons appear to lend their names to this kind of process, one would be inclined to stigmatize this performance as juggling with experimental results in order to extract from them, under the garb of chemistry, some very rudimentary and commonplace mathematical truths.

I would not be understood as casting any doubt on the results which may, by ingenious and clear-sighted persons, have been arrived at, even by so questionable a process: I would not be so understood, partly because those results lie out of my province, partly because the hypothesis of definite constitution for solutions or for alloys seems a very probable one, partly because I have myself plotted the s p curve for dilute ethyl alcohol, and clearly perceive the varieties of slope and curvature detected by Mendeleeff, though the changes are scarcely so sharp and definite at definite points as one might wish them to be in order to support the *a priori* improbable hypothesis of actual discontinuity. But what I want to assert, perhaps unnecessarily, is, that no juggling with feeble empirical expressions, and no appeal to the mysteries of elementary mathematics, can legitimately make experimental results any more really discontinuous than they themselves are able to declare themselves to be when properly plotted.

Liverpool, June 29.

OLIVER J. LODGE.

CHEMICAL AFFINITY.

IN the older days, chemists were willing to think that, when they had said of a chemical occurrence, "It is a manifestation of the affinities of the reacting bodies," they had given a fair explanation of the occurrence. Nowadays, we rather avoid the term affinity. The modern chemist is not comforted by the word as his fathers were. Phrases, he knows, have a way of decoying a man to destruction. But, although he does not use the word affinity so much, the chemist is more eager than ever to understand the modes of action of affinity.

Since the latter part of the last century, the prevalent views regarding affinity have fluctuated between the doctrines of Bergmann and Berthollet. Bergmann taught that the causes of chemical action and gravitative attraction are identical; this cause being manifested, in one case, in an attraction between minute particles, and, in the other case, between comparatively large masses, of bodies. Further, he said that the result of chemical attraction between different kinds of particles is a change of com-

¹ Although Prof. Thorpe's review suggested the writing of this letter here is nothing contained in that review which prompts these remarks. Prof. Thorpe does not appear to have fallen into the errors which, in the writings of some chemists, I fancy I detect.