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

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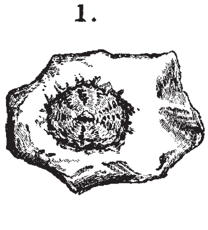

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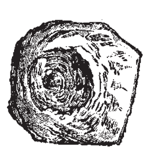

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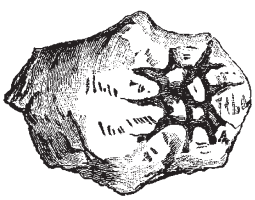

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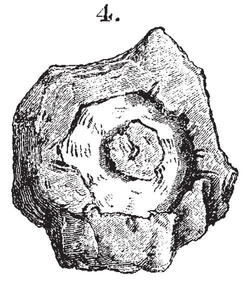

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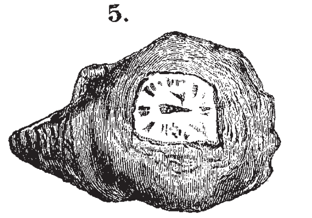

their narrow end towards the centre, and here and there in the ring are specks of grit or dust.

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 then they themselves are able to declare themselves to be when properly plotted.

Liverpool, June 29. OLIVER J. LODGE.

[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.

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-