

monias, amines, nitrites, etc., that do not contain oxygen; and here, too, we are upon a good solid ground of theory, relatively speaking. Oxygen would introduce him to more difficult questions, which have, however, in organic chemistry, been tolerably well answered. The separation of series of elements, such as F, Cl, Br, I, in this arrangement would force the student repeatedly to review, one by one, the facts that he had already learned, and would thus ensure the accuracy of his recollections. But, of course, the capital advantage would be the simplicity of the arrangement.

“Roscoe and Schorlemmer” suffers as much as any book we know from inconsistencies of arrangement. Its general idea, like that of many other works, is to treat the elements in one column of Mendeléeff’s table together. But no writer has ever adhered to that plan consistently. The result of doing so would be too atrocious. In this volume, oxygen follows after iodine. Boron is wedged between arsenic and carbon, simply because, at the time Roscoe studied chemistry, it was supposed to be allied to silicon through their non-volatile, glass-making acids—a circumstance which certainly does not affiliate boron with carbon. Nitrogen, phosphorus, and arsenic are treated in this volume; while antimony and bismuth go over to the third as being metals, although the metal tellurium is allowed a place here. A student who wishes occasionally to refer to this work along with a half-dozen other handbooks, all differently arranged, will be annoyed by the absence of any plain rule of arrangement in any of them.

The revision has been admirably performed. Its thoroughness and accuracy, and the sound scientific judgment shown wherever fact or theory is in doubt, are striking. A careful reading has disclosed but one or two slight errors. The historical statements are particularly careful, though we cannot assent to the credit allowed to Watt and disallowed to Cavendish as to the composition of water. The latter said that the two gases “are turned into water”—an expression of which the scientific caution at a time when there was no evidence whether that which was given off (which we now know to be heat-energy) was matter or not, ought to be commended in contrast to Watt’s unreflecting haste. It is absurd to treat *his* remark as a great discovery. What is supposed to have been the imperfection of the statement of Cavendish? Probably, his not explicitly recognizing that the imponderable something which escaped when the two gases were “turned into” water was not matter. But even Lavoisier in his chemistry, and all the treatises of his school, reckoned caloric among the chemical elements; so that really we cannot see that Cavendish conceived the fact otherwise than Lavoisier afterwards did.

### 81 (5 October 1905) 286-287

#### **La Nature et la Vie.**

Par Henry De Varigny. Paris: Armand Colin. 1905. 18mo, jésus, pp. 356.

CSP, identification: Haskell. *Index to The Nation*. See also: Burks, *Bibliography: List of Articles*.

Although it is the essential aim of physical science to explain phenomena without resort to any hypothesis of a direct action of mind upon matter (not but that

mind *does* act on matter in any common-sense understanding of the phrase, of course), yet many of us are attracted to certain results of biological study with a quality of interest only to be accounted for as being grounded upon some vague hope of getting light from them upon the mystery of the life we feel within us. It is to such readers that M. de Varigny addresses himself. His nineteen chapters, arranged in seven parts, wear, in the table of contents, a vague air of consecutive thought; and precisely so they appear when one comes to read them. They are composed of anecdotal facts of natural history strung upon a thread of reason without which they would lose half their charm, which is very considerable. In the way neither of observation nor of reflection has M. de Varigny much to communicate that is original; but he tells us much that is recent, and tells it with scientific accuracy.

By simply chatting about a few fair specimens of the work, we shall best convey an idea of its quality. The first chapter discusses the question of how life first came upon the cooling earth. Throughout, the author abstains from explicitly laying down any conclusions. As the advertisement puts it, his pages "n'ont rien d'un traité didactique." But his statement of the evidences pretty nearly amount to acceptance of Kelvin's theory that life was imported upon an aerolite; and perhaps that is the best opinion in the present state of science, though for quite different reasons from these which Varigny commends. To begin with, the author admits, as scientific men mostly do, that Pasteur proved that there is no such thing as spontaneous generation. Yet Clifford demonstrated to the satisfaction of every competent logician that Pasteur only refuted a particular argument, and left the general question untouched. For the hypothesis of spontaneous generation is that, under a certain complex combination of circumstances which may occur in the whole mass of the ocean once in a hundred centuries or so, living protoplasm will be produced; and all that Pasteur did was to show that in certain flasks of broth—say one or two hundred litres in all—no life was produced in several months. If Clifford were to meet a person who thought this a sound inductive argument against the possibility of spontaneous generation (say, M. de Varigny), the following conversation might ensue:

*Clifford*—Suppose that a little over a century ago, when the possibility of aerolites was disputed, a man had carefully prepared a quarter of an acre of ground so that nothing could fall upon it undetected, and had closely watched it for a whole day and night without catching a single stone from the skies. Do you think that that would have amounted to solid proof that no aerolite could have fallen in any civilized country during the previous four hundred years? Or do you think it would have been an argument of only moderate value, or how should you esteem it?

*Varigny*—It would have been a silly performance because of the insignificance of the area and of the duration of the experiment as compared with four centuries.

*Clifford*—Yet, estimating the value of the argument on that principle, it is hundreds of times as strong as the inference from Pasteur's half barrel of broth to the million and a half millions of millions of tons of sea water and the thousands of centuries of time that are in question.

*Varigny*—Well, you must admit that Pasteur proved that nobody had ever observed spontaneous generation; and reduced it to the rank of a bare possibility, a mere fancy. Now do you mean to say that scientific men should give up their time to the examination of the host of fanciful things that the idle from time to time amuse themselves by believing in?

*Clifford*—No; but I think that to argue that because it is not wise for a scientific man to bother with a certain supposed phenomenon, therefore that phenomenon does not exist, is uncommonly bad reasoning.

The probabilities seem at present to be decidedly against the theory that life came upon the earth by spontaneous generation, not because if such a phenomenon were possible it ought to have shown itself in Pasteur's flasks during his weeks of experimentation, but, on the contrary, because, even if the event be possible, the whole mass of the sea and the whole time since the solar system was a nebula fall short by millions upon millions of multiples of being sufficient to render the requisite combination of circumstances probable. For we know, what was not known in Clifford's time, that a molecule of protoplasm is composed of thousands of atoms; and very likely thousands of molecules must concur to produce life. These atoms, too, must all be arranged in certain ways. It would seem, then, though we really know next to nothing about it, that for such a combination to occur by pure chance must require the fortuitous satisfaction of at least a hundred independent conditions. Now how rapidly can we imagine these conditions to be satisfied? If we suppose that in every single molecule of water they are satisfied as frequently as light vibrates, the probability of the fortuitous occurrence of one molecule of protoplasm in the mass of the sea in a million million centuries would be so small that one might call it a miracle. The merit of Kelvin's idea is that it opens our prison doors. We are no longer limited to this puddle of an ocean or to the twinkling of an eye that has elapsed since the solar system was a nebula, but have the whole vast universe of space and time to draw upon, in which it may be that living protoplasm was produced fortuitously in sufficient quantity to reproduce itself. M. de Varigny shows that there is no serious objection to Kelvin's theory. As it is sometimes stated, it seems to make life inexplicable; and that is the greatest logical sin an explanatory hypothesis can fall into. But the true effect of Kelvin's theory is to open a wider field of possibility.

Among other entertaining chapters are those concerning an indispensable poison (carbonic acid and its effects upon climate), concerning the advantages of degeneration and concerning universal interdependence. In the last we learn that in New Zealand ill-managed lumbering spoiled the oysters, which were attacked by a parasitic worm that increased when more mud was washed down in the rivers. The penultimate part of the work is entitled "*Ce que veut la Nature.*" This was inevitable. This crystallizes the interest which was in solution throughout. But biology, as M. de Varigny rightly says, "will have none of final causes at any price whatever." Now, then, can it tell us "*ce que veut la Nature?*" It can give no other answer than that which we read in 1852 in a little brown book republished by Ticknor. The poet ought to know "*ce que veut la Nature.*" The man of science ought not to know; and if he does, it is surreptitiously.