

in this country to these equations, which are much more convenient than Cauchy's formula, and which much facilitate the reduction of measurements made with prism spectroscopes. The chapter concludes with a complete description of the various devices for compound and direct vision prisms. The combination of prisms to obtain great dispersion and resolving power has lost a great portion of its interest since the more general introduction of diffraction-gratings for spectroscopic purposes. We therefore turn with special interest to the fourth chapter, which deals with diffraction-gratings.

After a short history of the methods of ruling gratings, a discussion of plane gratings is given, which chiefly follows Rowland's and Cornu's investigations. About thirty pages are devoted to concave gratings. A very clear and elegant theory of these gratings, due to Prof. Runge, is, for the first time, published in full, and deserves to be widely read. It includes the very important practical question of the easiest method of adjusting the relative position of the slit, grating and camera, so that when the carriages roll along the beams, the spectrum should remain in focus and be displaced only in a direction parallel to the plane containing the two rectangular rails.

A disadvantage of concave gratings, which has been pointed out by Rowland in his first discussion, is its astigmatism, a point on the slit being drawn out into a line. It seems to me a curious fact that no one should have attempted to correct this astigmatism by means of cylindrical lenses. I was only waiting until the large concave grating of the Owens College was available, to try some experiments in this direction. Prof. Fitzgerald tells me that he has had the same idea, and has already determined by experiment the proper position of the two focal lines of the correcting lens. In looking over the pages of Prof. Kayser's book, I find that I had overlooked a paper by Mr. J. L. Serks, in the *Journal of Astronomy and Astrophysics*, in which the question is, in fact, solved theoretically. It is curious, however, that the author does not seem to have realised this application of his investigation, which he only applied to proving the possibility of finding a position for a comparison prism such that the horizontal edges of the prism should appear sharp on the spectrum plate. If the light coming from a luminous point is passed through a combination of a cylindrical and convex lens, placed so as to give a horizontal focal line in the position given by Serks, and a vertical focal line coincident with the slit, the astigmatism of the concave grating will be corrected.

The fifth chapter discusses the construction of spectroscopes, a good deal of space being devoted to the various devices for securing minimum deviation. The author seems to me to attach a somewhat exaggerated importance to the minimum deviation as regards its necessity to give definition. If the collimator is properly adjusted, and the faces of the prisms are plane, the spectra should be equally perfect whether the prisms are in the position of minimum deviation or not. When many prisms are used it becomes, of course, necessary that each prism should wholly take in the beam of light which has passed through the previous prism, and, in that case, the position of minimum deviation is most con-

venient. For the usual prism, cut so that its base is equally inclined to the faces, the position of minimum deviation is also that of maximum resolving power; but the prism may be turned considerably out of the symmetrical position without sensibly affecting its power of resolution.

The theory of the spectroscope, including the question of resolving power and purity, is fully discussed; but I venture to think that the treatment of the brightness of spectroscopic images might be made much simpler and clearer, and in some cases more correct, by starting from the following two very simple principles.

It is a well-known proposition, in the formation of images by lenses, that the brightness of the image, as deduced from the laws of geometrical optics, simply depends on the emitting power of the source and on the solid angle of the converging beam forming the final image. When the observations are taken by the eye, and the whole pupil is filled with light, the last solid angle is fixed; hence the brightness cannot be altered by any optical arrangement. The same proposition also holds when the light is refracted through prisms, provided the light is homogeneous. The second proposition, to which I have alluded, states that if the object is linear, the width of the central image, due to the finiteness of the wave-length of light, also depends only on the solid angle of the conical beam forming the last image.

These two propositions enable us to draw all the necessary conclusions without restrictions, such as that made by Kayser as to the position of minimum deviation of the prisms; and the results of § 508, derived from a paper by Wadsworth, will be found to need correction in some important particulars. The latter portions of this chapter deal with Michelson's researches, the applications of fluorescence, phosphorescence, and finally with photographic and bolometric methods.

The last chapter is devoted to spectroscopic measurements.

The value of the book is increased by the fact that the author has not been satisfied with a statement of results, but in many cases has added his own criticisms. I entirely agree with the statement made in the preface, that a mere compilation without critical discussion is of very little value. In the present volume there has not been so much opportunity of touching on tender spots as will arise in subsequent divisions of the subject; but Prof. Kayser's evident fairness and knowledge of his subject render it certain that no one need be afraid of placing himself under the judgment of so competent an authority. While congratulating Prof. Kayser on the successful accomplishment of the first portion of his task, we conclude with the hope that we may soon be able to welcome a second volume. ARTHUR SCHUSTER.

#### LIFE AND WORK OF C. GERHARDT.

*Charles Gerhardt: sa Vie, son Oeuvre, sa Correspondance: 1816-1856. Document d'Histoire de la Chimie.* Par M. Édouard Grimaux et M. Charles Gerhardt. (Paris: Masson et Cie.)

A BIOGRAPHY which involves the history of the turning-point of a science is always interesting; and this one in particular, which tells the tale of the

struggles of a young Alsatian, who came to Paris against the desire of his father to fight his way to recognition and fame, is almost dramatic in the way in which it enlists the sympathy of the reader. The story loses nothing by being told by the distinguished son of the subject of the memoir, and by the unfortunate Edouard Grimaux, whose recent death was—at least, in part—due to the jealousy of the Government of France when any attack on its action is made by men in its official pay. Indeed, it may be surmised that M. Grimaux found in the recital of Gerhardt's combats with those in power some consolation for his own recent dismissal from office.

Charles Gerhardt was born at Strassburg on August 22, 1816; he passed his schooldays at the Gymnasium there, and his father, in order to prepare young Gerhardt for the charge of a white-lead works which had fallen into his hands as the result of an unfortunate speculation, sent him to Carlsruhe, where, from 1831 to 1834, he studied chemistry and allied subjects. But, on his return to Strassburg, he found the monotonous existence of a works-manager far from his taste; and after stormy interviews with his father, at which he declared his intention to devote himself to the pursuit of pure chemistry, he entered the army as a preliminary step. This step, however, was far from leading him to the desired goal; and, deciding to abandon the calling of a soldier as rapidly as he had formed the intention of taking it up, he applied to relatives in Germany, requesting help to buy a substitute. The help was furnished by no less than Liebig, who had heard of his ability from his teacher Erdmann, and thought it worth while to secure a promising assistant by payment of 40*l.*—the necessary sum.

Needless to say, the money was afterwards refunded by his relatives.

It will be gathered from this short sketch of Gerhardt's youth that he was a young man of very decided character, and that he did not always take the surest way of gaining his desires—that, in fact, he had more of the *fortiter in re* than the *suaviter in modo*.

After having studied for two years at Giessen, then rising rapidly into repute as the first school of chemistry, Gerhardt made his way to Paris armed with a letter of introduction to Dumas and with authorisation to translate Liebig's "Organic Chemistry" into French—a task for which his bilingual education eminently suited him. At first all went well. Liebig's introduction opened to him the doors of the chemists of the day; but he failed, in spite of all efforts, to obtain a junior post. His repeated endeavours to secure a place in a laboratory where he could continue his researches were met with the advice—impossible to follow, under the circumstances—"Do some work, and you will find a place." But in order to continue his researches a laboratory was necessary; and this *impasse* barred his way for months. At last Cahours obtained leave from Chevreul for him to occupy a bench in the laboratory of the *Jardin des Plantes*. Here he carried on investigations on hellenine; and, at the same time, he published a note on the constitution of salts of organic acids and their connection with salts of ammonia.

The reader must peruse the memoir itself if he wishes to become acquainted with Gerhardt's struggles—how his too direct expression of his opinions, in words calculated

to irritate rather than to gain converts to his views, hindered his progress. Indeed, his relations—afterwards so intimate and so inseparable—with Laurent, began with an encounter. But both soon found that their ideas of the necessity of a reform in chemistry, and of the manner in which it was to be carried out, were nearly identical; and they joined forces in a campaign against the ruling powers. These powers were not mollified by the manner in which the campaign was conducted. Even Liebig, his old master, might be excused for resenting words such as the following, relating to a nitrogenous substance, a derivative of cyanuric acid, to which Liebig had given the name "mellon":—"Ce n'est pas une partie seulement du *memoire* de M. Liebig qui est fausse, mais toute l'histoire du mellon, toutes ses transformations, toutes ses reactions." Still, that did not excuse Liebig from saying that Gerhardt reminded him of a highwayman, who attacks and robs travellers and, after having stolen their clothes and ornaments, wears them with effrontery in the streets.

These were, however, days of hard hitting; and had the contest been confined only to words, little harm would have been done. But, unfortunately, the positions of instructors in the provinces and at Paris were so badly paid (and it is scarcely improved yet) that many offices were held by one individual, and places which gave command of several laboratories were occupied by those who were disinclined to abandon any one of them. Thus the best paid of the Government offices—that of Director of the Mint—had a salary attached to it of 600*l.* a year; many of the chairs were worth little over 100*l.*; and the emoluments sometimes became the gift of an elder to a younger member of a family, and were dispensed with little regard to scientific fitness or eminence. Moreover, it is the unfortunate custom in France that if a man wants a position he must ask for it—nay, he must personally supplicate those in power to bestow it on him. Thus, a candidate for admission to the Institute must canvass the members, hat in hand, and report has it that the reception accorded to a candidate is not always flattering to his *amour propre*. But we in England have little reason to criticise; for, though admission to the Royal Society's Fellowship is, fortunately, free from such disagreeable incidents, the candidature for a chair, with its system of testimonials and interviews, might well be reformed.

To return to Gerhardt. After several years of disappointment, he was finally appointed (through Dumas' influence) to the chair of chemistry at Montpellier, at one time renowned for its medical school; here he had only 6*l.* a year to spend on apparatus, and 12*l.* to provide specimens! Moreover, he found his colleagues occupying sinecures, and anxious to retain their chairs, as such, by discouraging the attendance of students. It is exceedingly galling to a "new broom" not to be allowed to make a clean sweep; and it is difficult to keep on terms of sufferance—not to speak of friendship—with what may be disrespectfully called the old besoms. So Gerhardt's new chair was by no means a comfortable seat; and after some years he applied for, and obtained, leave of absence on half-pay—another arrangement which sounds strange in our ears. In the meantime, however, he had married Miss Jane Sanders, a Scottish lady, resident with her mother and sister at Montpellier; this union was,

from every point of view, a happy one. During his leave of absence, Gerhardt came to Paris; and again, it is strange to us on the other side of the Channel, many of whom are content with an occasional visit to the capital, to see how absence from Paris is, to a Frenchman, absence from civilisation. "La vie du province"—there is no expression which so fitly renders the *ennui* of banishment from "ce cher Paris." But to live at Paris was not necessarily to find an official position; and, after many disappointed hopes, Gerhardt finally accepted two chairs at Strassburg: "Le cumul," as pluralism is termed, has attractions, it appears, to those to whom it is offered.

Gerhardt was not long at Strassburg, however, before he succumbed to an attack of peritonitis; and, after a few days' illness, during which he regretted nothing more than the cessation of work, he passed away.

Gerhardt shared with other reformers absolute belief in his own theories, and want of patience with conservatives who would not be convinced. Patience and a more gracious manner would have not only given him a happier and more prosperous career, but would also have accelerated the acceptance of his doctrines. Still, it is difficult for us to judge. Suffice it to say that the formulæ which we still use are, for the most part, Gerhardt's. While Gerhardt referred the formulæ of compounds to the volume occupied by the molecular weight in grams contained in 22.4 litres of the gas, Laurent extended the same numerical conception to the "formulæ" of elements; while Gerhardt wrote  $H_2O$  for the formula of water, but  $O$  for that of oxygen, Laurent introduced Avogadro's and Ampère's view that the molecular formula of oxygen should be  $O_2$ . They united their forces in advocating the adoption of "types," such as that of  $H_2$ ,  $H_2O$ , and  $NH_3$ ; and Williamson supplemented their ideas by the addition of "double types"—substances derived, for example, from two molecules of water by replacement of an atom of hydrogen in each. Later, as every one knows, this conception developed into structural formulæ. But the idea of a homologous series, too, was first introduced by Gerhardt; and it has proved one of the most fertile in the whole domain of organic chemistry.

We have witnessed as great, if not a greater change in chemical theory during recent years. Fortunately, it has not aroused the same passion, although it has been resolutely opposed by a conservative faction. At the meeting of the French Association at Havre, the writer remembers well a discussion of which the central point was whether the formula of barium sulphate should be written  $BaO.SO_3$  or  $BaSO_4$ . Is it possible that, twenty years hence, we shall still find a remnant for whom the ionic theory has no value? W. R.

#### MONISM FOR THE MULTITUDE.

*The Riddle of the Universe at the Close of the Nineteenth Century.* By Ernst Haeckel, Ph.D., M.D., LL.D., Sc.D., and Professor at the University of Jena. Translated by Joseph McCabe. Issued by the Rationalist Press Association, Ltd. Pp. xvi + 398. (London: Watts and Co., 1900.)

THERE is a twofold pathos in this book, for with it the author—whom to know is to love—draws, he says, "a line under his life-work," and with it he once

more illustrates the sad fact that a great investigator may not be convincing as a philosopher. The book begins with a reproach that philosophy is ignorant and that science lacks consistency, and we end it with a sigh for the same reasons. As a few readers may remember, Haeckel projected, almost a generation ago, the scheme of a "System of Monistic Philosophy"; but the shadow of age has fallen upon him while his early ambition was still not within sight of being realised. Therefore he has given us in this, "which has something of the character of a sketch-book," only a hint of what might have been. For the non-fulfilment of his dream of youth, the order of things is more responsible than the author, for there are few who have worked harder and, at the same time, more brilliantly for their day and generation.

But although Haeckel speaks of the volume as a sort of sketch-book, this is not meant to suggest that its conclusions are mere *obiter dicta*. On the contrary, as he tells us, he has been meditating for fully half a century on the problems of evolution, and now, in his sixty-sixth year, he gives us "the ripe fruit of his tree of knowledge." If this is not an altogether happy metaphor, it may serve to remind the unsympathetic that we have here, at least, the sincere voice of "a child of the nineteenth century," who is conscious of no dogmatism, though the suggestion of it seems painfully frequent, who blinks no facts so far as he is aware, who is impelled by no motive but a love of truth.

"My 'Monistic Philosophy' is sincere from beginning to end—it is the complete expression of the conviction that has come to me, after many years of ardent research into Nature and unceasing reflection, as to the true basis of its phenomena."

Impulsive the author certainly is, as he has always been—impulsive, for instance, to champion Darwinism in the early days of its unpopularity, and impulsive in his confidence in genealogical trees which many a Jack has hewn at while the giant climbed—but ignorant no one will venture to call the zoologist who has laid so many solid blocks in the scientific edifice, and to whom the Royal Society has lately awarded its Darwin medal. There is, perhaps, no important idea in this volume, admirably translated by Mr. Joseph McCabe, which is not to be found in that wonderful work of 1866, the "*Generelle Morphologie*"; but the ideas are now illumined with a wealth and confidence of illustration which only a big personal share in the scientific progress of the last forty years could give.

The book, as we have said, begins with a reproach. Scientific workers "do not see the wood for the trees"; the metaphysicians "trouble not about the individual trees, and are satisfied with the mere picture of the wood"; betwixt the two is the multitude, still oppressed by "the riddle of the painful earth." But this incoherence, this ignorance, this oppression result from that blindness to the open secret of unity which is the lasting defect of Western thought. There is but one fact, and, as a writer in the *Monist* recently remarked, it is an evidence of human frailty that the word ever got a plural; there is but one science, the science of the order of nature; there is but one comprehensive riddle, the problem of substance; and there is but one hopeful attempt at solution, namely, of course, scientific monism.