

THE NEGLECT OF THE WORK OF H. GRASSMANN.

IT must not be supposed that the neglect of Hermann Grassmann's mathematical work by his contemporaries is merely an incident of his biography. Its consideration involves a much larger question, because Grassmann's fate was shared by other mathematicians of the period in whose work stress was laid on form rather than content. The distinction between the two may be illustrated by reference to the mathematical treatment of quantity. As soon as analysis had generalized that idea so as to include complex quantities, a mathematics based on formal definitions and of a general character could be developed to include them. The meaning of the propositions of such a calculus need not enter into this study. The propositions would constitute a formal deductive series which could be developed without any reference to content. That Grassmann was a pioneer in the movement which made magnitude subordinate and posterior to a science of form was recognized by Hankel,¹ who says, "It was Grassmann who took up this idea for the first time in a truly philosophical spirit and treated it from a comprehensive point of view." In the Introduction (A) to the *Ausdehnungslehre* of 1844 Grassmann puts the matter thus: "The chief division of all sciences is that into real and formal. The former sciences

¹ *Theorie der complexen Zahlensysteme*, p. 16.

image in thought the existent as independent of thinking, and their truth consists in the agreement of the thought with the existent; the latter sciences on the contrary have for their subject-matter that which has been determined by thought itself, and their truth is shown in the mutual agreement between processes of thought." He goes on to consider mathematics and formal logic as branches of a general science of form, and seeks to dissociate this science from such real sciences as the geometry of actual space, although it must form the basis on which all such are built.

That the neglect accorded to Grassmann had nothing to do with any accident of birth or position is shown by the fact that Leibniz, whose name was famous in both mathematical and philosophical circles, shared the same fate in regard to his *Dissertatio de Arte Combinatoria* and later writings of the same kind, in which he sought to set up a formal symbolical calculus with similar aims. Of Grassmann's contemporaries who worked in the same field, we need mention only George Boole (1815-1864) who failed to obtain anything like a due recognition of his genius; and Sir. W. R. Hamilton whose early papers on quaternions were regarded as mere curiosities. Even when the applications of these generalized formal methods to the founding of a calculus of directed quantities of immediate value to physics had been made, we find the important work of Willard Gibbs waiting for years before it became known and made full use of. If, then, we are to explain the neglect of Grassmann's work we shall have to analyze the causes of the apathy and mistrust with which all such work has been received.

The view held by Carl Müsebeck is that in the almost exclusively philosophical form of representation, *which however was grounded in the whole system*, we have to seek the reason why the contemporaries of Grassmann

drew back in terror from deeper study of his early work. He says²: "Such a height of mathematical abstraction in which, with the help of a new calculus, laws are inferred in abstract regions about the mutual dependence of abstract constructions in which not even the character of the spatial is maintained, although at the conclusion of almost every section it is shown how the new method could be used with advantage, was never before known." That this has been a very important factor cannot be doubted. Dislike of the philosophical form of his work was expressed to Grassmann by the few mathematicians who noticed his first *Ausdehnungslehre*. He himself says in the preface to the second edition of this book that he expected the work to find fullest recognition from the more philosophically inclined reader. It is only necessary to refer to the application and extension of his ideas which have come from A. N. Whitehead³ in England and from G. Peano⁴ and C. Burali-Forti⁵ in Italy to show how well-founded this forecast was. But the analysis cannot rest there. We must inquire further how this dislike arose.

J. T. Merz⁶ in his chapter on "The Development of Mathematical Thought in the 19th Century," inclines to the view that a definite distaste for a philosophical form had set in among German mathematicians as a part of the reaction against the exaggerations of the metaphysical unification of knowledge in the schools of Schelling and Hegel. But mathematicians in modern times have, on the whole, been singularly unaffected by philosophical movements. Furthermore the calculus of extension and allied systems have not fully come into their own even in our own day,

² In his memoir of Hermann Grassmann, Stettin, 1877.

³ *Universal Algebra*, Cambridge, 1898.

⁴ *Calcolo Geometrico secondo l'Ausdehnungslehre di H. Grassmann*, Turin, 1888.

⁵ *Introduction à la géométrie différentielle, suivant la méthode de H. Grassmann*, Paris, 1897.

⁶ *History of European Thought in the 19th Century*, Vol. I, p. 243.

when wide syntheses are eagerly sought. It seems to the present writer that it is in the attitude of the plain anti-metaphysical mathematician that we must seek for the explanation of the want of understanding which leads to mistrust of philosophical form. An immense amount of prejudice barred the way to the full development of a general science of form—prejudice due to non-realization of the purely formal claims of such a calculus.⁷ And if we could get at the bottom of this not altogether unreasoning mistrust it might be possible to clear away some of the hindrances to a proper understanding of the fundamental importance of Grassmann's work.

To do this we must push our analysis a step further. What steady cause can have been operating over such a long period which could so affect the attitude of the individual as to create what amounts almost to a general blindness to the importance of a whole body of contributions to thought? I believe that the root of the matter lies in wrong principles of instruction. It may be that this at first sight appears too small an influence to have such consequences; but so did the minute geological influences of the uniformitarians to those who sought for explanations in more dramatic cataclysms. It is as unscientific to neglect the unobtrusive but persistent influences of educational methods on pure thought as it would be to treat of the social conditions of a people without taking into account their mind-development.

We will only give one well-recognized example of the importance of methods of exposition on mathematical history. Merz places Gauss at the head of the critical movement which began the nineteenth century. He adds,⁸ however, that it was not to him primarily that the great change

⁷ Cf. the article below on "The Geometrical Analysis of Grassmann and its Connection with Leibniz's Characteristic," § 2.

⁸ *Op. cit.*, Vol. II, p. 636.

which came over mathematics was due, but to Cauchy. Gauss, while issuing finished and perfect though sometimes irritatingly unintelligible tracts, hated lecturing; in contrast to this Cauchy gained the merit, through his enthusiasm and patience as a teacher, of creating a new school of thought—and earned the gratitude of the greatest intellects, such as Abel, for having pointed out the right road of progress. But it is not so much upon the manner of exposition of original mathematicians themselves that stress must be laid. It has without doubt often happened that writers of great analytical insight have failed to see that it is no more a descent to a common level to seek out and use the best methods of enforcing consideration of their work, than it is to use a printing-press instead of a town crier the more effectively to reach their audience. Grassmann himself, however, did all that was humanly possible in this way, although Jahnke is of the opinion that he was inclined to the belief that even first instruction should be rigorous; and kept back applications until too late. It is rather that teaching methods in general during the nineteenth century have always lagged too far behind discovery. And so they have left the students of one generation, who are the potential original workers of the next, with minds unreceptive to newer and more delicate methods. It might be urged that this would affect equally all branches of mathematics, but I think it can be shown that it is on the reception of such fundamental analytical methods as Grassmann's that its evil influence more particularly falls.

It is quite obvious that the subject must be limited if we are to deal in detail with the suggested effects of inadequate educational methods. So I shall confine myself in what follows to the consideration of the difficulties which beset the path of the teacher who has to explain the ordi-

nary concepts of mechanics; and attempt to show how failure to realize the nature of those difficulties tends to produce an unreceptive attitude to modern analysis. I have chosen this subject for two reasons. Firstly, it seems to me that if the concepts of mechanics were properly treated they would finally appear to the pupil as useful constructions instead of as the dogmatically asserted existents they are still commonly held to be; and so the formal science underlying the real science of mechanics would naturally arise for him as the final result of analysis, and not as the unreal fabric of a philosopher's dream. And secondly, it is the domain to which the various "extensive algebras" have peculiar applicability, as Grassmann himself felt strongly. It is highly significant therefore that it is precisely Grassmann's suggestive applications to mechanics whose neglect is the most noticeable. That this is so is, on my view, because sounder and more philosophical notions of geometrical as opposed to mechanical concepts were already coming into exchange in Grassmann's own day so that geometrical applications were thereby rendered more understandable.

At the very outset of our discussion we are faced with the difficulty that so much difference of opinion exists between teachers of mechanics that many have been forced into the conclusion that, since the enthusiast with an unphilosophical method of his own can yet reap good results, method is unimportant. This, of course, is only partially true. If it were wholly true it would mean an end to all possibility of coordination—an end, in fact, to the claims of education to be a science. To grant that education is an art is not to forego all its claims to be a science. For we must regard all art as applied science "unless we are willing, with the multitude, to consider art as guessing

and aiming well.”⁹ Beneath the apparent chaos of opinion on the teaching of mechanics there is however some order if one can avoid certain sources of confusion which have led to superficial differences of opinion where nothing deeper exists.

One source of confusion is the absence of a clear idea of the difference in educational theory between an impersonal *principle* and the more personal element—the *method* of applying the principle. This distinction is insisted on by Mr. E. G. A. Holmes,¹⁰ and seems a real one. If once we realize it we can see how it is possible for there to be fairly well accepted scientific principles of teaching at the same time as a wide divergence of method in use by different teachers under differing conditions. And indeed if one looks carefully into much of the polemical writing on mechanics teaching it is seen to be caused less by fundamental differences of principle than by differences of method. It is still more necessary to clear away a second source of unsatisfactory discussion. A superficial glance through the mass of controversial writing on science teaching in recent years would lead one to suppose there was a sharp division of principle between those who believe in a logically ordered course with emphasis on what one may call the instructional method, and those who prefer a looser, more empirical, treatment usually embodying heuristic methods. It would be possible, however, to reconcile many of the combatants if they could be persuaded to see that so direct an opposition is far too simple a statement of the problem, and that each may be partial statements of the real solution. And this becomes possible, I think, if once the disputants grant the importance of the biogenetic or embryonic principle as applied to education—the principle, that is to say,

⁹ Reference to Plato, *Philebus*: G. Boole, *The Mathematical Analysis of Logic*, note p. 7.

¹⁰ E. G. A. Holmes, *The Montessori System of Education*, English Board of Education Pamphlet, No. 24, p. 3.

that the development of the individual is a recapitulation of the development of the race. It seems strange that it should be necessary at this stage to call attention to a principle so well known¹¹ and so much applied, and yet one often has the spectacle of a successful teacher of higher classes urging the claims of logical order against an equally successful empiricist whose experience has been with younger pupils. The truth is, of course, that no one method is applicable to all ages. If the biogenetic law holds, then the natural principle would be to use, in general, modes of teaching a subject similar at each stage to those by which the race has gathered its knowledge of that subject. In mechanics this would mean that a more rigidly logical course would follow empirical experiments and the handling of simple machines.

We will now pass on to our main investigation of the factors which must be taken into account in avoiding the creation of an atmosphere uncongenial to a final abstract analysis. In doing so I will indicate what appear to be the general principles by which one must work in giving to beginners living ideas of the entities of mechanics, and failure to comply with which leads to the production of passively instructed, rather than of irritable and responsive, organisms. The concepts of mechanics are produced from the raw material of experience by the process of abstraction, and a beginner must therefore pass through an experimental stage before he is introduced to the logically defined concepts themselves. In fact he must first use and handle rough ideas and thence be led to build up the more rigidly exact definitions of them for himself. It follows from this that any information we can glean

¹¹ It is a very remarkable thing that De Morgan in his *Study and Difficulties of Mathematics*, first published in 1831, or 28 years before the *Origin of Species*, should have stated this principle so concisely in the words (p. 186) referring to discussions of first principles: "the progress of nations has exhibited throughout a strong resemblance to that of individuals."

about the actual historical process by which man came to form and use concepts may be of vital importance to a teacher. In mechanics particularly, where the concepts are less obvious than in geometry (the first ideas of force, mass, acceleration and energy, regarded however not as constructions but as real entities, were only developed to any clearness after Galileo—that is at quite a late stage in man's history) any fogginess about their nature and use means endless confusion; and that accounts for most of the difficulties commonly experienced.

It was Locke who first plainly showed how concepts arise from the material of immediate perception. If we think of the flux and confusion of our perceptions—the colors, sounds, smells, sensations of touch, at any instant we find our attention drawn to some more insistent parts of that flux. When these continually recur we use nouns, adjectives and verbs to identify them. Such is the beginning of the formation of concepts. These are regrouped to form other concepts. Thus a wide experience of animals would lead us to group them and to speak, for example, of a class "dog." Once classed we can treat all instances as having the general properties of the class. The practical advantages are obvious. "The intellectual life of man consists almost wholly in his substitution of a conceptual order for the perceptual order in which his experience originally comes," says William James.¹² Once concepts are formed they enable us to handle our immediate experience with greater ease. And by building up more and more complex concepts and tracing the connections between them we create our mathematics and our sciences.

Even animals may form rough concepts.¹⁸ A dog by experience comes to know the difference between "man"

¹² *Some Problems of Philosophy*, p. 51.

¹⁸ This treatment of the origination of concepts is founded largely on that of E. Mach in his chapter on Concepts in the volume *Erkenntnis und Irrtum*.

and other animals. Furthermore if he met a dummy man he would soon find out that the reactions he ordinarily associated with "man" failed to be reproduced, and so would reject that experience for his man-class. In a similar way man must have formed concepts becoming more and more complicated but more firm in outline as his experience became richer. But it is to be noticed that the growth of concepts in a body of experience depends on the number and interest of our observations in the region concerned. For this reason interest in, and consequent familiarization with, simple machines and mechanical toys may well be the child's best introduction to mechanics. Model monoplanes, an old petrol engine from a motor cycle, pumps, a screw, levers, a jack, Hero's turbine model—all these can easily be got at; few young children will show no interest, while many of them will possess already in these days of mechanical toys a considerable knowledge of manipulation. Simple explanations of the working of such apparatus are absorbed with astonishing readiness. In larger schools where there is an engineering workshop this method of introducing young boys to mechanics by way of machinery has been tried with considerable success. Knowledge gets picked up as it were "by contact." The concepts which arise at this stage are necessarily crude—general ideas of force, speed, work and friction; this latter is, of course, one of the first things to notice—not the last to be dealt with as is usually the case. Simple as these considerations are, they are not yet fully appreciated. The London Mathematical Association's *Report on the Teaching of Elementary Mechanics* suggested some time ago that the phrase "Mechanical Advantage" be replaced by "Force-Ratio." For beginners neither of these is intelligible; but they very soon know "how much stronger" a machine makes you. And that conception is quite good enough for them to use.

In introducing simple mechanical concepts to beginners, therefore, the principle to use is that the concepts must arise naturally from experience and not be handed out as definitions. Dictated definitions not founded on sufficient knowledge of facts are flimsy constructions ready to fall at the first breath of difficulty. They do not perform that primary function of concepts of helping one to classify and handle facts, because the facts to be handled are not in the mind when the concept is formulated. "How much stronger a machine makes you" is a phrase which reminds the hearer at once of the assistance it gives him in grouping machines and using them intelligently for different purposes. A note-book definition of "mechanical advantage" is likely to present another arithmetical puzzle instead of serving to remind the learner of the solution of old ones. The principle here advocated was well expressed in the discussion on mechanics teaching at the British Association in 1905 by the president of the section, Professor Forsyth. He said, "What you want to do in the first instance is to accustom the boys to the ordinary relations of bodies and of their properties, and afterward you can attempt to give some definitions which will be more or less accurate; but do not begin with the definitions, begin with the things themselves." And the philosophical basis for the principle is, that the significance of concepts is always learned from their relations to perceptual particulars, their utility depending on the power they give us of coordinating perceptual facts. From this it follows further that concepts and names should never be introduced where there is no direct and immediate gain in so doing. Such terms as "centrifugal" and "centripetal" forces, and the endless discussion to which they lead, are thus beside the mark. "Force toward, or away from, the center" does all that is necessary without introducing new words of really less precision.

It should be noted that some of the crude concepts arrived at in the early stages are really, when one comes to analyze them, very complex, and Ostwald's warning¹⁴ against the error of supposing that the less simple concepts have always been reached by compounding simple ones has application here. As he says, complex concepts often in origin have existed first. We can now see more clearly why the teacher of mechanics so often complains of the difficulty of giving the average child a satisfactory notion of force.¹⁵ The difficulty is largely due to the teacher who knows the concept to be complicated, and seeks to define it in terms of mass-acceleration—thus involving two more concepts, one of which (mass) is at least as difficult to understand as force. A rough idea of force, considered simply as a "push" or a "pull," can be assimilated at a very early stage; that of mass-acceleration must come very much later.

The bearing of this preliminary stage in the formation of concepts on our main thesis may now be traced. It is quite evident that the individual has very limited powers of absorbing the logically ordered account of a science in which stress is laid on abstract notions before such notions have grown up naturally by use. Now this difficult step for the beginner from the perceptual to the conceptual is very similar to that which leads from ordinary mechanics to such a treatment of the subject as that of Grassmann. Both lead into regions of greater abstraction. In the latter case we can get rid of concepts in so far as they relate to the existent, and reach a statement of mechanical principles in terms of a generalized form-theory. We may illustrate, roughly, the meaning of this by the following analogue. At different stages in the history of physics various the-

¹⁴ Ostwald, *Natural Philosophy*, p. 20.

¹⁵ Cf. C. Godfrey, *Brit. Association Report on Mechanics Teaching*, p. 41.

ories of light have been held. The concepts used in these theories (corpuscle, elastic-solid ether, electro-magnetic medium) have possessed widely different "qualities"; but the equations expressing the relation between the conceptual elements have throughout possessed similarity of form. A science of form would hence lay emphasis on the invariant relations, refine away the particular concepts, and leave a much more abstract and generalized science.

But if racial development is in the main similar to the progress of the individual this will explain the great difficulty experienced by whole generations of mathematicians in understanding work of the type of Grassmann's.

Furthermore, it is at this point that defective scientific training looms into importance. For unless great care has been taken in avoiding the too early definition of concepts, a rigid view of them is promulgated. The older dogmatic and orderly methods of teaching tended inevitably to this. The consequence was that when the time came for polishing and development of the concepts obtained, and for the deliberate building up of more complex ones—it was found that the capacity for subtle generalized views had been destroyed. A mind forced into passivity and filled with inert knowledge cannot suddenly be brought to discard it in response to the stimulus of a tentative generalization. To take a simple example, the idea of a new kind of addition, applicable to vectors, shocks and confuses a pupil who has been dogmatically instructed in algebra as though it were a sacred rite. As with the child under such a system, so with the generation of which he forms a part. Jahnke states that many mathematicians were put off by meeting in Grassmann's work a product which equals zero without either factor doing so. Formal logical development often leads to conclusions which are not capable of any mental image.¹⁸ Such abstractions are

¹⁸ Cf. F. Klein, *The Evanston Colloquium*, Lecture 6.

out of reach of those who have never been freed from the confines of the existent world.

Cajori¹⁷ in a notice in 1874 of the publication called *The Analyst*, Des Moines, Iowa, said that it bore evidence of an approaching departure from antiquated views and methods, of a tendency among teachers to look into the history and philosophy of mathematics. My thesis is that such a movement, which certainly has not yet been realized, would remove the main cause of the neglect of Hermann Grassmann's work, which even in these days is often granted the kind of recognition accorded to certain literary classics, which are famous but never read. Perhaps it is an earnest of the future that the copy of *The Analyst* referred to by Cajori contained a brief account¹⁸ of the essential features of Grassmann's *Ausdehnungslehre*.

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¹⁷ *Teaching and History of Mathematics in the United States.*

¹⁸ Translated by W. W. Beman of the University of Michigan.