CHAPTER I.

PRELUDE TO THE EPOCH OF GALILEO.

Sect. 1.—*Prelude to the Science of Statics.*

SOME steps in the science of motion, or rather in the science of equilibrium, had been made by the ancients, as we have seen. Archimedes established satisfactorily the doctrine of the lever, some important properties of the centre of gravity, and the fundamental proposition of hydrostatics. But this beginning led to no permanent progress. Whether the distinction between the principles of the doctrine of equilibrium and of motion was clearly seen by Archimedes, we do not know; but it never was caught hold of by any of the other writers of antiquity, or of the stationary period. What was still worse, the point which Archimedes had won was not steadily maintained.

We have given some examples of the general ignorance of the Greek philosophers on such subjects, in noticing the strange manner in which Aristotle refers to mathematical properties, in order to account for the equilibrium of a lever, and the attitude of a man rising from a chair. And we have seen, in speaking of the indistinct ideas of the stationary period, that the attempts which were made to extend the statical doctrine of Archimedes, failed, in such a manner as to show that his followers had not clearly apprehended the idea on which his reasoning altogether depended. The clouds which he had, for a moment, cloven in his advance, closed after him, and the former dimness and confusion settled again on the land.

This dimness and confusion, with respect to all subjects of mechanical reasoning, prevailed still, at the period we now have to consider; namely, the period of the first promulgation of the Copernican opinions. This is so important a point that I must illustrate it further.

Certain general notions of the connexion of cause and effect in motion, prevail at all periods of the developement of the human mind, and are implied in the formation of language and in the most familiar employments of men's thoughts. But these do not constitute a *science* of mechanics, any more than the notions of square and round make a geometry, or the notions of months and years make an astronomy. The unfolding these notions into distinct ideas, on which can be founded principles and reasonings, is further requisite, in order to produce a science; and, with respect to the doctrines of motion, this was long in coming to pass: men's thoughts remained long entangled in their primitive and unscientific confusion.

We may mention one or two features of this confusion, such as we find in authors belonging to the period now under review.

We have already, in speaking of the Greek school philosophy, noticed the attempt to explain some of the differences among motions, by classifying them into natural motions and violent motions; and the assertion that heavy bodies fall quicker in proportion to their greater weight. These doctrines were still retained: yet the views which they implied were essentially erroneous and unsound; for they did not refer distinctly to a measurable force as the cause of all motion or change of motion; and they confounded the causes which produce, and those which preserve, motion. Hence the study of such principles did not lead immediately to any advance of knowledge, though efforts were made to apply them, in the cases both of terrestrial mechanics and of the motions of the heavenly bodies.

The effect of the inclined plane was one of the first, as it was one of the most important, propositions, on which modern writers employed themselves. It was found that a body, when supported on a sloping surface, might be sustained or raised by a force or exertion which would not have been able to sustain or raise it without such support. And hence, *The Inclined Plane* was placed in the list of Mechanical Powers, or simple machines by which the efficacy of forces is increased: the question was, in what proportion this increase of efficiency takes place. It is easily seen that the force requisite to sustain a body is smaller, as the slope on which it rests is smaller; Cardan (whose work, *De Proportionibus Numerorum, Motuum, Ponderum,*

&c., was published in 1545) asserts that the force is double when the angle of inclination is double, and so on for other proportions; this is probably a guess, and is an erroneous one. Guido Ubaldi, of Marchmont, published at Pesaro, in 1577, a work which he called *Mechanicorum Liber*, in which he endeavours to prove that an acute wedge will produce a greater mechanical effect than an obtuse one, without determining in what proportion. There is, he observes, "a certain repugnance" between the direction in which the side of the wedge tends to move the obstacle, and the direction in which it really does move. Thus the wedge and the inclined plane are connected in principle. He also refers the screw to the inclined plane and the wedge, in a manner which shows a just apprehension of the question. Benedetti (1585) treats the wedge in a different manner; not exact, but still showing some powers of thought on mechanical subjects. Michael Varro, whose *Tractatus de Motu* was published at Geneva in 1584, deduces the wedge from the composition of hypothetical motions, in a way which may appear to some persons an anticipation of the doctrine of the composition of forces.

There is another work on subjects of this kind, of which several editions were published in the sixteenth century, and which treats this matter in nearly the same way as Varro, and in favour of which a claim has been made[1] (I think an unfounded one,) as if it

[1] Mr. Drinkwater's Life of Galileo, in the Lib. Usef. Kn. p. 83.

contained the true principle of this problem. The work is "Jordanus Nemorarius *De Ponderositate*." The date and history of this author were probably even then unknown; for in 1599, Benedetti, correcting some of the errors of Tartalea, says they are taken "a Jordano quodam antiquo." The book was probably a kind of school-book, and much used; for an edition printed at Frankfort, in 1533, is stated to be "Cum gratia et privilegio Imperiali, Petro Apiano mathematico Ingolstadiano ad XXX annos concesso." But this edition does not contain the inclined plane. Though those who compiled the work assert in words something like the inverse proportion of weights and their velocities, they had not learnt at that time how to apply this maxim to the inclined plane; nor were they even able to render a sound reason for it. In the edition of Venice, 1565, however, such an application is attempted. The reasonings are founded on the usual Aristotelian assumption, "that bodies descend more quickly in proportion as they are heavier." To this principle are added some others; as, that "a body is heavier in proportion as it descends more directly to the centre," and that, in proportion as a body descends more obliquely, the intercepted part of the direct descent is smaller. By means of these principles, the "descending force" of bodies, on inclined planes, was compared, by a process, which, so far as it forms a line of proof at all, is a somewhat curious example of confused and vicious reasoning. When two bodies are supported on two inclined planes, and are connected by a string passing over the junction of the planes, so that when one descends the other ascends, they must move through equal spaces on the planes; but on the plane which is more oblique (that is, more nearly horizontal,) the vertical descent will be smaller in the same proportion in which the plane is longer. Hence, by the Aristotelian principle, the weight of the body on the longer plane is less; and, to produce an equality of effect, the body must be greater in the same proportion. We may observe that the Aristotelian principle is not only false, but is here misapplied; for its genuine meaning is, that when bodies *fall freely* by gravity, they move quicker in proportion as they are heavier; but the rule is here applied to the motions which bodies *would* have, if they were moved by a force extraneous to their gravity. The proposition was supposed by the Aristotelians to be true of *actual* velocities; it is applied by Jordanus to *virtual* velocities. This confusion being made, the result is got at by taking for granted that bodies *thus* proved to be equally *heavy*, have equal powers of descent on the inclined planes; whereas, in the previous part of the reasoning, the weight was supposed to be proportional to the descent in the vertical direction. It is obvious, in all this, that though the author had adopted the false Aristotelian principle, he had not settled in his own mind whether the motions of which it spoke were actual or virtual motions;—motions in the direction of the inclined plane, or of the intercepted parts of the vertical, corresponding to these; nor whether the "descending force" of a body was something different from its weight. We cannot doubt that, if he had been required to point out, with any exactness, the cases to which his reasoning applied, he would have been unable to do so; not possessing any of those clear fundamental ideas of pressure and force, on which alone any real knowledge on such subjects must depend. The whole of Jordanus's reasoning is an example of the confusion of thought of his period, and nothing more. It no more supplied the want of some man of genius, who should give the subject a real scientific foundation, than Aristotle's knowledge of the proportion of the weights on the lever superseded the necessity of Archimedes's proof of it.

We are not, therefore, to wonder that, though this pretended theorem was copied by other writers, as by Tartalea, in his *Quesiti et Inventioni Diversi*, published in 1554, no progress was made in the real solution of any one mechanical problem by means of it. Guido Ubaldi, who, in 1577, writes in such a manner as to show that he had taken a good hold of his subject for his time, refers to Pappus's solution of the problem of the inclined plane, but makes no mention of that of Jordanus and Tartalea. No progress was likely to occur, till the mathematicians had distinctly recovered the genuine idea of pressure, as a force producing equilibrium, which Archimedes had possessed, and which was soon to reappear in Stevinus.

The properties of the lever had always continued known to mathematicians, although, in the dark period, the superiority of the proof given by Archimedes had not been recognised. We are not to be surprised, if reasonings like those of Jordanus were applied to demonstrate the theories of the lever with apparent success. Writers on mechanics were, as we have seen, so vacillating in their mode of dealing with words and propositions, that they would be made to prove anything which was already known to be true.

We proceed to speak of the beginning of the real progress of mechanics in modern times.

Sect. 2.—*Revival of the Scientific Idea of Pressure.—Stevinus.—Equilibrium of Oblique Forces.*

THE doctrine of the centre of gravity was the part of the speculations of Archimedes which was most diligently prosecuted after his time. Pappus and others, among the ancients, had solved some new problems on this subject, and Commandinus, in 1565, published *De Centro Gravitatis Solidorum.* Such treatises contained, for the most part, only mathematical consequences of the doctrines of Archimedes; but the mathematicians also retained a steady conviction of the mechanical property of the centre of gravity, namely, that all the weight of the body might be collected there, without any change in the mechanical results; a conviction which is closely connected with our fundamental conceptions of mechanical action. Such a principle, also, will enable us to determine the result of many simple mechanical arrangements; for instance, if a mathematician of those days had been asked whether a solid ball could be made of such a form, that, when placed on a horizontal plane, it should go on rolling forwards without limit, merely by the effect of its own weight, he would probably have answered, that it could not; for that the centre of gravity of the ball would seek the lowest position it could find, and that, when it had found this, the ball could have no tendency to roll any further. And, in making this assertion, the supposed reasoner would not be anticipating any wider proofs of the impossibility of a perpetual motion, drawn from principles subsequently discovered, but would be referring the question to certain fundamental convictions, which, whether put into axioms or not, inevitably accompany our mechanical conceptions.

In the same way, if Stevinus of Bruges, in 1586, when he published his *Beghinselen der Waaghconst* (Principles of Equilibrium), had been asked why a loop of chain, hung over a triangular beam, could not, as he asserted it could not, go on moving round and round perpetually, by the action of its own weight, he would probably have answered, that the weight of the chain, if it produced motion at all, must have a tendency to bring it into some certain position; and that when the chain had reached this position, it would have no tendency to go any further; and thus he would have reduced the impossibility of such a perpetual motion, to the conception of gravity as a force tending to produce equilibrium, a principle perfectly sound and correct.

Upon this principle thus applied, Stevinus did establish the fundamental property of the inclined plane. He supposed a loop of string, loaded with fourteen equal balls at equal distances, to hang over a triangular support which had a horizontal base, and whose sides, being unequal in the proportion of two to one, supported four and two balls respectively. He showed that this loop must hang at rest, because any motion would only bring it into the same condition in which it was at first; and that the festoon of eight balls which hung down below the triangle might be removed without disturbing the equilibrium; so that four balls on the longer plane would balance two balls on the shorter planes, or the weights would be as the lengths of the planes intercepted by the horizontal line.

Stevinus showed his firm possession of the truth contained in this principle, by deducing from it the properties of forces acting in oblique directions under all kinds of conditions; in short, he showed his entire ability to found upon it a complete doctrine of equilibrium; and upon his foundations, and without any additional support, the mathematical doctrines of Statics might have been carried to the highest pitch of perfection they have yet reached. The formation of the science was finished; the mathematical developement and exposition of it were alone open to extension and change.