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THE SCIENCE OF MECHANICS

A CRITICAL AND HISTORICAL ACCOUNT OF ITS DEVELOPMENT

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VI.

NEWTON'S VIEWS OF TIME, SPACE, AND MOTION.

1. In a scholium which he appends immediately to his definitions, Newton presents his views regarding time and space—views which we shall now proceed to examine more in detail. We shall literally cite, to this end, only the passages that are absolutely necessary to the characterisation of Newton's views.

Newton's views of time, space, and motion.

"So far, my object has been to explain the senses "in which certain words little known are to be used in "the sequel. Time, space, place, and motion, being "words well known to everybody, I do not define. Yet "it is to be remarked, that the vulgar conceive these "quantities only in their relation to sensible objects. "And hence certain prejudices with respect to them "have arisen, to remove which it will be convenient to "distinguish them into absolute and relative, true and "apparent, mathematical and common, respectively.

Absolute and relative time.

"I. Absolute, true, and mathematical time, of it-"self, and by its own nature, flows uniformly on, with-"out regard to anything external. It is also called "*duration*.

"Relative, apparent, and common time, is some "sensible and external measure of absolute time (dura-"tion), estimated by the motions of bodies, whether "accurate or inequable, and is commonly employed "in place of true time; as an hour, a day, a month, "a year. . .

"The natural days, which, commonly, for the pur-"pose of the measurement of time, are held as equal. "are in reality unequal. Astronomers correct this in-

"equality, in order that they may measure by a truer "time the celestial motions. It may be that there is "no equable motion, by which time can accurately be "measured. All motions can be accelerated and re- "tarded. But the flow of *absolute* time cannot be "changed. Duration, or the persistent existence of "things, is always the same, whether motions be swift "or slow or null."

Discussion of Newton's view of time.

2. It would appear as though Newton in the remarks here cited still stood under the influence of the mediæval philosophy, as though he had grown unfaithful to his resolve to investigate only actual facts. When we say a thing *A* changes with the time, we mean simply that the conditions that determine a thing *A* depend on the conditions that determine another thing *B*. The vibrations of a pendulum take place *in time* when its excursion *depends* on the position of the earth. Since, however, in the observation of the pendulum, we are not under the necessity of taking into account its dependence on the position of the earth, but may compare it with any other thing (the conditions of which of course also depend on the position of the earth), the illusory notion easily arises that *all* the things with which we compare it are unessential. Nay, we may, in attending to the motion of a pendulum, neglect entirely other external things, and find that for every position of it our thoughts and sensations are different. Time, accordingly, appears to be some particular and independent thing, on the progress of which the position of the pendulum depends, while the things that we resort to for comparison and choose at random appear to play a wholly collateral part. But we must not forget that all things in the world are connected with one another and depend on one another, and that

General discussion of the concept of time.

we ourselves and all our thoughts are also a part of nature. It is utterly beyond our power to *measure* the changes of things by *time.* Quite the contrary, time is an abstraction, at which we arrive by means of the changes of things; made because we are not restricted to any one *definite* measure, all being interconnected. A motion is termed uniform in which equal increments of space described correspond to equal increments of space described by some motion with which we form a comparison, as the rotation of the earth. A motion may, with respect to another motion, be uniform. But the question whether a motion is *in itself* uniform, is senseless. With just as little justice, also, may we speak of an "absolute time"—*of a time independent of change.* This absolute time can be measured by comparison with no motion; it has therefore neither a practical nor a scientific value; and no one is justified in saying that he knows aught about it. It is an idle metaphysical conception.

Further elucidation of the idea.

It would not be difficult to show from the points of view of psychology, history, and the science of language (by the names of the chronological divisions), that we reach our ideas of time in and through the interdependence of things on one another. In these ideas the profoundest and most universal connection of things is expressed. When a motion takes place in time, it depends on the motion of the earth. This is not refuted by the fact that mechanical motions can be reversed. A number of variable quantities may be so related that one set can suffer a change without the others being affected by it. Nature behaves like a machine. The individual parts reciprocally determine one another. But while in a machine the position of one part determines the position of *all* the other parts, in nature more complicated relations obtain. These relations are best represented under the conception of a number, n, of quantities that satisfy a lesser number, n', of equations. Were $n = n'$, nature would be invariable. Were $n' = n - 1$, then with one quantity all the rest would be controlled. If this latter relation obtained in nature, time could be reversed the moment this had been accomplished with any one single motion. But the true state of things is represented by a different relation between n and n'. The quantities in question are partially determined by one another; but they retain a greater indeterminateness, or freedom, than in the case last cited. We ourselves feel that we are such a partially determined, partially undetermined element of nature. In so far as a portion only of the changes of nature depends on us and can be reversed by us, does time appear to us irreversible, and the time that is past as irrevocably gone.

Some psychological considerations.

We arrive at the idea of time,—to express it briefly and popularly,—by the connection of that which is contained in the province of our memory with that which is contained in the province of our sense-perception. When we say that time flows on in a definite direction or sense, we mean that physical events generally (and therefore also physiological events) take place only in a definite sense.* Differences of temperature, electrical differences, differences of level generally, if left to themselves, all grow less and not greater. If we contemplate two bodies of different temperatures, put in contact and left wholly to themselves, we shall find that it is possible only for greater differences of temperature in the field of memory to

* Investigations concerning the physiological nature of the sensations of time and space are here excluded from consideration.

exist with lesser ones in the field of sense-perception, and not the reverse. In all this there is simply expressed a peculiar and profound connection of things. To demand at the present time a full elucidation of this matter, is to anticipate, in the manner of speculative philosophy, the results of all future special investigation, that is a perfect physical science. (Compare Appendix, XIX., p. 541.)

Newton's views of space and motion.

3. Views similar to those concerning time, are developed by Newton with respect to space and motion. We extract here a few passages which characterise his position.

"II. Absolute space, in its own nature and with-"out regard to anything external, always remains sim-"ilar and immovable.

"Relative space is some movable dimension or "measure of absolute space, which our senses deter-"mine by its position with respect to other bodies, "and which is commonly taken for immovable [abso-"lute] space....

"IV. Absolute motion is the translation of a body "from one absolute place* to another absolute place; "and relative motion, the translation from one relative "place to another relative place....

Passages from his works.

".... And thus we use, in common affairs, instead "of *absolute* places and motions, *relative* ones; and "that without any inconvenience. But in physical "disquisitions, we should abstract from the senses. "For it may be that there is no body really at rest, to "which the places and motions of others can be re-"ferred....

"The effects by which absolute and relative motions

* The place, or *locus* of a body, according to Newton, is not its position, but the *part of space* which it occupies. It is either absolute or relative.—*Trans*

"are distinguished from one another, are centrifugal "forces, or those forces in circular motion which pro-"duce a tendency of recession from the axis. For in "a circular motion which is purely relative no such "forces exist; but in a true and absolute circular mo-"tion they do exist, and are greater or less according "to the quantity of the [absolute] motion.

"For instance. If a bucket, suspended by a long "cord, is so often turned about that finally the cord is "strongly twisted, then is filled with water, and held "at rest together with the water; and afterwards by "the action of a second force, it is suddenly set whirl-"ing about the contrary way, and continues, while the "cord is untwisting itself, for some time in this mo-"tion; the surface of the water will at first be level, "just as it was before the vessel began to move; but, "subsequently, the vessel, by gradually communicat-"ing its motion to the water, will make it begin sens-"ibly to rotate, and the water will recede little by little "from the middle and rise up at the sides of the ves-"sel, its surface assuming a concave form. (This ex-"periment I have made myself.)

The rotating bucket.

". . . . At first, when the *relative* motion of the wa-"ter in the vessel was *greatest*, that motion produced "no tendency whatever of recession from the axis; the "water made no endeavor to move towards the cir-"cumference, by rising at the sides of the vessel, but "remained level, and for that reason its *true* circular "motion had not yet begun. But afterwards, when "the relative motion of the water had decreased, the "rising of the water at the sides of the vessel indicated "an endeavor to recede from the axis; and this en-"deavor revealed the real circular motion of the water, "continually increasing, till it had reached its greatest

Relative and real motion.

"point, when *relatively* the water was at rest in the "vessel. . . .

"It is indeed a matter of great difficulty to discover "and effectually to distinguish the *true* from the ap- "parent motions of particular bodies; for the parts of "that immovable space in which bodies actually move, "do not come under the observation of our senses.

Newton's criteria for distinguishing absolute from relative motion.

"Yet the case is not altogether desperate; for there "exist to guide us certain marks, abstracted partly "from the apparent motions, which are the differences "of the true motions, and partly from the forces that "are the causes and effects of the true motions. If, "for instance, two globes, kept at a fixed distance "from one another by means of a cord that connects "them, be revolved about their common centre of "gravity, one might, from the simple tension of the "cord, discover the tendency of the globes to recede "from the axis of their motion, and on this basis the "quantity of their circular motion might be computed. "And if any equal forces should be simultaneously "impressed on alternate faces of the globes to augment "or diminish their circular motion, we might, from "the increase or decrease of the tension of the cord, "deduce the increment or decrement of their motion; "and it might also be found thence on what faces "forces would have to be impressed, in order that the "motion of the globes should be most augmented; "that is, their rear faces, or those which, in the cir- "cular motion, follow. But as soon as we knew which "faces followed, and consequently which preceded, we "should likewise know the direction of the motion. "In this way we might find both the quantity and the "direction of the circular motion, considered even in "an immense vacuum, where there was nothing ex-

"ternal or sensible with which the globes could be "compared . . . ."

4. It is scarcely necessary to remark that in the reflections here presented Newton has again acted contrary to his expressed intention only to investigate *actual facts*. No one is competent to predicate things about absolute space and absolute motion; they are pure things of thought, pure mental constructs, that cannot be produced in experience. All our principles of mechanics are, as we have shown in detail, experimental knowledge concerning the relative positions and motions of bodies. Even in the provinces in which they are now recognised as valid, they could not, and were not, admitted without previously being subjected to experimental tests. No one is warranted in extending these principles beyond the boundaries of experience. In fact, such an extension is meaningless, as no one possesses the requisite knowledge to make use of it.

The predications of Newton are not the expression of actual facts.

Let us look at the matter in detail. When we say that a body K alters its direction and velocity solely through the influence of another body K', we have asserted a conception that it is impossible to come at unless other bodies A, B, C . . . . are present with reference to which the motion of the body K has been estimated. In reality, therefore, we are simply cognisant of a relation of the body K to A, B, C . . . . If now we suddenly neglect A, B, C . . . . and attempt to speak of the deportment of the body K in absolute space, we implicate ourselves in a twofold error. In the first place, we cannot know how K would act in the absence of A, B, C . . . .; and in the second place, every means would be wanting of forming a judgment of the behaviour of K and of putting to the test what we had

Detailed view of the matter.

predicated,—which latter therefore would be bereft of all scientific significance.

The part which the bodies of space play in the determination of motion.

Two bodies K and K', which gravitate toward each other, impart to each other in the direction of their line of junction accelerations inversely proportional to their masses m, m'. In this proposition is contained, not only a relation of the bodies K and K' to one another, but also a relation of them to other bodies. For the proposition asserts, not only that K and K' suffer with respect to one another the acceleration designated by $\varkappa\,(\overline{m + m'}/r^2)$, but also that K experiences the acceleration $-\varkappa m'/r^2$ and K' the acceleration $+\varkappa m/r^2$ in the direction of the line of junction; facts which can be ascertained only by the presence of other bodies.

The motion of a body K can only be estimated by reference to other bodies A, B, C.... But since we always have at our disposal a sufficient number of bodies, that are as respects each other relatively fixed, or only slowly change their positions, we are, in such reference, restricted to no one *definite* body and can alternately leave out of account now this one and now that one. In this way the conviction arose that these bodies are indifferent generally.

The hypothesis of a medium in space determinative of motion.

It might be, indeed, that the isolated bodies A, B, C.... play merely a collateral rôle in the determination of the motion of the body K, and that this motion is determined by a *medium* in which K exists. In such a case we should have to substitute this medium for Newton's absolute space. Newton certainly did not entertain this idea. Moreover, it is easily demonstrable that the atmosphere is not this motion-determinative medium. We should, therefore, have to picture to ourselves some other medium, filling, say, all space, with respect to the constitution of which and its kinetic relations to the bodies placed in it we have at present no adequate knowledge. In itself such a state of things would not belong to the impossibilities. It is known, from recent hydrodynamical investigations, that a rigid body experiences resistance in a frictionless fluid only when its velocity *changes*. True, this result is derived theoretically from the notion of inertia; but it might, conversely, also be regarded as the primitive fact from which we have to start. Although, practically, and at present, nothing is to be accomplished with this conception, we might still hope to learn more in the future concerning this hypothetical medium; and from the point of view of science it would be in every respect a more valuable acquisition than the forlorn idea of absolute space. When we reflect that we cannot abolish the isolated bodies A, B, C . . . ., that is, cannot determine by experiment whether the part they play is fundamental or collateral, that hitherto they have been the sole and only competent means of the orientation of motions and of the description of mechanical facts, it will be found expedient provisionally to regard all motions as determined by these bodies.

Critical examination of Newton's distinction of absolute from relative motion.

5. Let us now examine the point on which Newton, apparently with sound reasons, rests his distinction of absolute and relative motion. If the earth is affected with an *absolute* rotation about its axis, centrifugal forces are set up in the earth: it assumes an oblate form, the acceleration of gravity is diminished at the equator, the plane of Foucault's pendulum rotates, and so on. All these phenomena disappear if the earth is at rest and the other heavenly bodies are affected with absolute motion round it, such that the same *relative* rotation is produced. This is, indeed, the case, if we start *ab initio* from the idea of absolute space.

But if we take our stand on the basis of facts, we shall find we have knowledge only of *relative* spaces and motions. *Relatively*, not considering the unknown and neglected medium of space, the motions of the universe are the same whether we adopt the Ptolemaic or the Copernican mode of view. Both views are, indeed, equally *correct*; only the latter is more simple and more *practical*. The universe is not *twice* given, with an earth at rest and an earth in motion; but only *once*, with its *relative* motions, alone determinable. It is, accordingly, not permitted us to say how things would be if the earth did not rotate. We may interpret the one case that is given us, in different ways. If, however, we so interpret it that we come into conflict with experience, our interpretation is simply wrong. The principles of mechanics can, indeed, be so conceived, that even for relative rotations centrifugal forces arise.

Interpretation of the experiment with the rotating bucket of water.

Newton's experiment with the rotating vessel of water simply informs us, that the relative rotation of the water with respect to the sides of the vessel produces *no* noticeable centrifugal forces, but that such forces *are* produced by its relative rotation with respect to the mass of the earth and the other celestial bodies. No one is competent to say how the experiment would turn out if the sides of the vessel increased in thickness and mass till they were ultimately several leagues thick. The one experiment only lies before us, and our business is, to bring it into accord with the other facts known to us, and not with the arbitrary fictions of our imagination.

6. We can have no doubts concerning the significance of the law of inertia if we bear in mind the manner in which it was reached. To begin with, Galileo discovered the constancy of the velocity and direction

The law of inertia in the light of this view.

of a body referred to terrestrial objects. Most terrestrial motions are of such brief duration and extent, that it is wholly unnecessary to take into account the earth's rotation and the changes of its progressive velocity with respect to the celestial bodies. This consideration is found necessary only in the case of projectiles cast great distances, in the case of the vibrations of Foucault's pendulum, and in similar instances. When now Newton sought to apply the mechanical principles discovered since Galileo's time to the planetary system, he found that, so far as it is possible to form any estimate at all thereof, the planets, irrespectively of dynamic effects, appear to preserve their direction and velocity with respect to bodies of the universe that are very remote and as regards each other apparently fixed, the same as bodies moving on the earth do with respect to the fixed objects of the earth. The comportment of terrestrial bodies with respect to the earth is reducible to the comportment of the earth with respect to the remote heavenly bodies. If we were to assert that we knew more of moving objects than this their last-mentioned, experimentally-given comportment with respect to the celestial bodies, we should render ourselves culpable of a *falsity*. When, accordingly, we say, that a body preserves unchanged its direction and velocity *in space*, our assertion is nothing more or less than an abbreviated reference to *the entire universe*. The use of such an abbreviated expression is permitted the original author of the principle, because he knows, that as things are no difficulties stand in the way of carrying out its implied directions. But no remedy lies in his power, if difficulties of the kind mentioned present themselves; if, for example, the requisite, relatively fixed bodies are wanting.

The relation of the bodies of the universe to each other.

7. Instead, now, of referring a moving body K to space, that is to say to a system of coördinates, let us view directly its relation to the bodies of the universe, by which alone such a system of coördinates can be determined. Bodies very remote from each other, moving with constant direction and velocity with respect to other distant fixed bodies, change their mutual distances proportionately to the time. We may also say, All very remote bodies—all mutual or other forces neglected—alter their mutual distances proportionately to those distances. Two bodies, which, situated at a short distance from one another, move with constant direction and velocity with respect to other fixed bodies, exhibit more complicated relations. If we should regard the two bodies as dependent on one another, and call r the distance, t the time, and a a constant dependent on the directions and velocities, the formula would be obtained: $d^2r/dt^2 = (1/r)\ [a^2 - (dr/dt)^2]$. It is manifestly much *simpler* and *clearer* to regard the two bodies as independent of each other and to consider the constancy of their direction and velocity with respect to other bodies.

Instead of saying, the direction and velocity of a mass μ in space remain constant, we may also employ the expression, the mean acceleration of the mass μ with respect to the masses $m, m', m'' \ldots$ at the distances $r, r', r'' \ldots$ is $= 0$, or $d^2(\Sigma mr/\Sigma m)/dt^2 = 0$. The latter expression is equivalent to the former, as soon as we take into consideration a sufficient number of sufficiently distant and sufficiently large masses. The mutual influence of more proximate small masses, which are apparently not concerned about each other, is eliminated of itself. That the constancy of direction and velocity is given by the condition adduced, will be seen at once if we construct through μ as vertex cones that cut out different portions of space, and set up the condition with respect to the masses of these separate portions. We may put, indeed, for the *entire* space encompassing μ, $d^2(\Sigma mr/\Sigma m)/dt^2 = 0$. But the equation in this case asserts nothing with respect to the motion of μ, since it holds good for all species of motion where μ is uniformly surrounded by an infinite number of masses. If two masses μ_1, μ_2 exert on each other a force which is dependent on their distance r, then $d^2r/dt^2 = (\mu_1 + \mu_2)f(r)$. But, at the same time, the acceleration of the centre of gravity of the two masses or the mean acceleration of the mass-system with respect to the masses of the universe (by the principle of reaction) remains $= 0$; that is to say,

The expression of the law of inertia in terms of this relation.

$$\frac{d^2}{dt^2}\left[\mu_1 \frac{\Sigma mr_1}{\Sigma m} + \mu_2 \frac{\Sigma mr_2}{\Sigma m}\right] = 0.$$

When we reflect that the time-factor that enters into the acceleration is nothing more than a quantity that is the measure of the distances (or angles of rotation) of the bodies of the universe, we see that even in the simplest case, in which apparently we deal with the mutual action of only *two* masses, the neglecting of the rest of the world is *impossible*. Nature does not begin with elements, as we are obliged to begin with them. It is certainly fortunate for us, that we can, from time to time, turn aside our eyes from the overpowering unity of the All, and allow them to rest on individual details. But we should not omit, ultimately to complete and correct our views by a thorough consideration of the things which for the time being we left out of account.

The necessity in science of a consideration of the All.

8. The considerations just presented show, that it

The law of inertia does not involve absolute space.

is not necessary to refer the law of inertia to a special absolute space. On the contrary, it is perceived that the masses that in the common phraseology exert forces on each other as well as those that exert none, stand with respect to acceleration in quite similar relations. We may, indeed, regard *all* masses as related to each other. That *accelerations* play a prominent part in the relations of the masses, must be accepted as a fact of experience; which does not, however, exclude attempts to *elucidate* this fact by a comparison of it with other facts, involving the discovery of new points of view. In all the processes of nature the *differences* of certain quantities u play a determinative rôle. Differences of temperature, of potential function, and so forth, induce the natural processes, which consist in the equalisation of

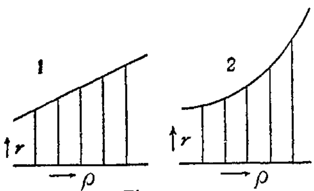

Fig. 143.

Natural processes consist in the equalisation of the differences of quantities.

these differences. The familiar expressions d^2u/dx^2, d^2u/dy^2, d^2u/dz^2, which are determinative of the character of the equalisation, may be regarded as the measure of the departure of the condition of any point from the mean of the conditions of its environment—to which mean the point tends. The accelerations of masses may be analogously conceived. The great distances between masses that stand in no especial force-relation to one another, change *proportionately to each other*. If we lay off, therefore, a certain distance ρ as abscissa, and another r as ordinate, we obtain a straight line. (Fig. 143.) Every r-ordinate corresponding to a definite ρ-value represents, accordingly, the mean of the adjacent ordinates. If a force-relation exists between the bodies, some value d^2r/dt^2 is determined by it which conformably to the remarks above we may replace by an expression of the form $d^2r/d\rho^2$. By the force-relation, therefore, a *departure* of the r-ordinate from the *mean of the adjacent ordinates* is produced, which would not exist if the supposed force-relation did not obtain. This intimation will suffice here.

Character of the new expression for the law of inertia.

9. We have attempted in the foregoing to give the law of inertia a different expression from that in ordinary use. This expression will, so long as a sufficient number of bodies are apparently fixed in space, accomplish the same as the ordinary one. It is as easily applied, and it encounters the same difficulties. In the one case we are unable to come at an absolute space, in the other a limited number of masses only is within the reach of our knowledge, and the summation indicated can consequently not be fully carried out. It is impossible to say whether the new expression would still represent the true condition of things if the stars were to perform rapid movements among one another. The general experience cannot be constructed from the particular case given us. We must, on the contrary, *wait* until such an experience presents itself. Perhaps when our physico-astronomical knowledge has been extended, it will be offered somewhere in celestial space, where more violent and complicated motions take place than in our environment. The most important result of our reflexions is, however, *that precisely the apparently simplest mechanical principles are of a very complicated character, that these principles are founded on uncompleted experiences, nay on experiences that never can be fully completed, that practically, indeed, they are sufficiently secured, in view of the tolerable stability of our environment, to serve as the foundation of mathematical deduction, but that they can by no means themselves be re-*

The simplest principles of mechanics are of a highly complicated nature and are all derived from experience.

garded as mathematically established truths but only as principles that not only admit of constant control by experience but actually require it. This perception is valuable in that it is propitious to the advancement of science. (Compare Appendix, XX., p. 542.)

XX.

(See page 238.)

Of the treatises which have appeared since 1883 on the law of inertia, all of which furnish welcome evidence of a heightened interest in this question, I can here only briefly mention that of Streintz (*Physikalische Grundlagen der Mechanik*, Leipsic, 1883) and that of L. Lange (*Die geschichtliche Entwicklung des Bewegungsbegriffes*, Leipsic, 1886).

The expression "absolute motion of translation" Streintz correctly pronounces as devoid of meaning and consequently declares certain analytical deductions, to which he refers, superfluous. On the other hand, with respect to *rotation*, Streintz accepts Newton's position, that absolute rotation can be distinguished from relative rotation. In this point of view, therefore, one can select every body not affected with absolute rotation as a body of reference for the expression of the law of inertia.

I cannot share this view. For me, only relative motions exist (*Erhaltung der Arbeit*, p. 48; *Science of Mechanics*, p. 229), and I can see, in this regard, no distinction between rotation and translation. When a body moves relatively to the fixed stars, centrifugal forces are produced; when it moves relatively to some different body, and not relatively to the fixed stars, no centrifugal forces are produced. I have no objection to calling the first rotation "absolute" rotation, if it be remembered that nothing is meant by such a designation except *relative rotation with respect to the fixed stars.* Can we fix Newton's bucket of water, rotate the fixed stars, and *then* prove the absence of centrifugal forces?

The experiment is impossible, the idea is meaningless, for the two cases are not, in sense-perception, distinguishable from each other. I accordingly regard these two cases as the *same* case and Newton's distinction as an illusion (*Science of Mechanics*, page 232).

But the statement is correct that it is possible to find one's bearings in a balloon shrouded in fog, by means of a body which does not rotate with respect to the fixed stars. But this is nothing more than an *indirect* orientation with respect to the fixed stars; it is a mechanical, substituted for an optical, orientation.

I wish to add the following remarks in answer to Streintz's criticism of my view. My opinion is not to be confounded with that of Euler (Streintz, pp. 7, 50), who, as Lange has clearly shown, never arrived at any settled and intelligible opinion on the subject. Again, I never assumed that remote masses *only*, and not near ones, determine the velocity of a body (Streintz, p. 7); I simply spoke of an influence *independent* of distance. In the light of my expositions at pages 222–245, the unprejudiced and careful reader will scarcely maintain with Streintz (p. 50), that after so long a period of time, without a knowledge of Newton and Euler, I have only been led to views which these inquirers so long ago held, but were afterwards, partly by them and partly by others, rejected. Even my remarks of 1872, which were all that Streintz knew, cannot justify this criticism. These were, for good reasons, concisely stated, but they are by no means so meagre as they must appear to one who knows them only from Streintz's criticism. The point of view which Streintz occupies, I at that time expressly rejected.

Lange's treatise is, in my opinion, one of the best that have been written on this subject. Its methodical movement wins at once the reader's sympathy. Its careful analysis and study, from historical and critical points of view, of the concept of motion, have produced, it seems to me, results of permanent value. I also regard its clear emphasis and apt designation of the principle of "particular determination" as a point of much merit, although the principle itself, as well as its application, is not new. The principle is really at the basis of all measurement. The choice of the unit of measurement is convention; the number of measurement is a result of inquiry. Every natural inquirer who is clearly conscious that his business is simply the investigation of the interdependence of phenomena, as I formulated the point at issue a long time ago (1865–1866), employs this principle. When, for example (*Mechanics*, p. 218 et seq.), the negative inverse ratio of the mutually induced accelerations of two bodies is called the mass-ratio of these bodies, this is a *convention*, expressly acknowledged as arbitrary; but that these ratios are independent of the kind and of the order of combination of the bodies is a *result of inquiry.* I might adduce numerous similar nstances from the theories of heat and electricity as well as from other provinces. Compare Appendix II.

Taking it in its simplest and most perspicuous form, the law of inertia, in Lange's view, would read as follows:

Three material points, P_1, P_2, P_3, are simultaneously hurled from the same point in space and then left to themselves. The moment we are certain that the points are not situated in the same straight line, we join each separately with *any* fourth point in space, Q. These lines of junction, which we may respectively call G_1, G_2, G_3, form, at their point of meeting, a three-faced solid angle. If now we make this solid angle preserve, with unaltered rigidity, its form, and constantly determine in such a manner its position, that P_1 shall always move on the line G_1, P_2 on the line G_2, P_3 on the line G_3, these lines may be regarded as the axis of a coördinate or inertial system, with respect to which every other material point, left to itself, will move in a straight line. The spaces described by the free points in the paths so determined will be proportional to one another.

A system of coördinates with respect to which three material points move in a straight line is, according to Lange, under the assumed limitations, a simple *convention.* That with respect to such a system also a fourth or other free material point will move in a straight line, and that the paths of the different points will all be proportional to one another, are *results of inquiry.*

In the first place, we shall not dispute the fact that the law of inertia can be referred to such a system of time and space coördinates and expressed in this form. Such an expression is less fit than Streintz's for practical purposes, but, on the other hand, is, for its methodical advantages, more attractive. It especially appeals to my mind, as a number of years ago I was engaged with similar attempts, of which not the beginnings but only a few remnants (*Mechanics*, pp. 234–235) are left. I abandoned these attempts, because I was convinced that we only *apparently* evade by such expressions references to the fixed stars and the angular rotation of the earth. This, in my opinion, is also true of the forms in which Streintz and Lange express the law.

In point of fact, it was precisely by the consideration of the fixed stars and the rotation of the earth that we arrived at a knowledge of the law of inertia as it at present stands, and *without these foundations* we should never have thought of the explanations here discussed (*Mechanics*, 232–233). The consideration of a small number of isolated points, to the exclusion of the rest of the world, is in my judgment inadmissible (*Mechanics*, pp. 229–235).

It is quite questionable, whether a *fourth* material point, left to itself, would, with respect to Lange's "inertial system," uniformly describe a straight line, if the fixed stars were absent, or not invariable, or could not be regarded with sufficient approximation as invariable.

The most natural point of view for the candid inquirer must still be, to regard the law of inertia primarily as a tolerably accurate approximation, to refer it, with respect to space, to the fixed stars, and, with respect to time, to the rotation of the earth, and to await the correction, or more precise definition, of our knowledge from future experience, as I have explained on page 237 of this book.

I have still to mention the discussions of the law of inertia which have appeared since 1889. Reference may first be made to the expositions of Karl Pearson (*Grammar of Science*, 1892, page 477), which agree with my own, save in terminology. P. and J. Friedlander (*Absolute und relative Bewegung*, Berlin, 1896) have endeavored to determine the question by means of an experiment based on the suggestions made by me at pages 217–218; I have grave doubts, however, whether the experiment will be successful from the quantitative side. I can quite freely give my assent to the discussions of Johannesson (*Das Beharrungsgesetz*, Berlin, 1896), although the question remains unsettled as to the means by which the motion of a body not perceptibly accelerated by other bodies is to be determined. For the sake of completeness, the predominantly dialectic treatment by M. E. Vicaire, *Société scientifique de Bruxelles, 1895*, as well as the investigations of J. G. MacGregor, *Royal Society of Canada, 1895*, which are only remotely connected with the question at issue, remain to be mentioned. I have no objections to Budde's conception of space as a sort of medium (compare page 230), although I think that the properties of this medium should be demonstrable physically in some other manner, and that they should not be assumed *ad hoc*. If all apparent actions at a distance, all accelerations, turned out to be effected through the agency of a medium, then the question would appear in a different light, and the solution is to be sought perhaps in the view set forth on page 230.