Part IV

HISTORY OF PHILOSOPHY OF SCIENCE

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Conventionalism and the Origins of the Inertial Frame Concept

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The obvious metaphysical differences between Newton and Leibniz concerning space, time, and motion reflect less obvious differences concerning the relation between geometry and physics, expressed in the questions: what are the invariant quantities of classical mechanics, and what sort of geometrical frame of reference is required to represent those quantities? Leibniz thought that the fundamental physical quantity was "living force" (mv^2), of which every body was supposed to have a definite amount; this notion violates the classical principle of relativity, since it makes a physical distinction between uniform velocity and absolute rest. But Leibniz did not try to represent this physical quantity in a spatio-temporal reference frame, assuming, instead, that all such frames are equivalent so long as they agree on the relative motions (changes in the mutual Euclidean distances) among bodies. Newton, in contrast, explicitly incorporated the relativity principle into his dynamics: he characterized the invariant quantity of force by acceleration rather than velocity, and recognized that the quantity of a body's moving force depends on the frame of reference in which it is described. Yet he tried to represent this dynamical conception in absolute space, which entails precisely the distinction between motion and rest, and hence the violation of classical relativity, that Leibniz's dynamics postulates. In the subsequent history of dynamics, serious methodological issues have been raised by the problem of expressing what is physically invariant in an appropriate geometrical structure.

As far as Newtonian mechanics is concerned, this problem was essentially solved by James Thomson in 1884, when he introduced the term "reference frame" and defined what we now call the inertial frame. His insight was that the laws of motion themselves postulate certain spatial and temporal structures, or, in his words, a certain kind of reference-frame and "dial-traveler" (time scale). He therefore proposed a "Law of Inertia," which I paraphrase: for any system of interacting bodies, it is possible to construct a reference-frame and a time scale with respect to which all accelerations are proportional to, and in the direction of, impressed forces. (Thomson 1884, p. 387.) To assert this as a law of nature is to assert, in modern language, that space-time is a flat affine space with a projection on time, and that deviations of a body's motion from the geodesics of the affine structure correspond to forces acting on the moving body. Of course we must add to this statement the demand that all of these actions are matched by equal and opposite reactions, so that a true impressed force

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can be distinguished from a pseudo-force, but Thomson's basic approach remains satistfactory; the main reasons to doubt it are the reasons to doubt Newton's laws generally.

Yet the end of the nineteenth century saw, not a recognition of the clarification that had been achieved, but an often bewildering philosophical debate over Newton's laws and the reference frame that they require, a debate that continued even after special relativity might have made the issues seem outdated. This is because to many of the participants in that debate, the (now seemingly straightforward) problem that Thomson addressed inevitably raised philosophical issues that affected the foundations of mechanics. Thus Ludwig Lange, for example, independently recapitulated Thomson's work (more or less) in his definition of "inertial system" (Lange 1885), but, while Thomson modestly thought he had clarified a point in Newtonian mechanics, Lange thought that he had set Newton's laws on an entirely new footing — a footing that purportedly replaced absolute notions with relative, real with ideal, and, especially, factual with conventional. Even though Lange's definition and his general point of view were given much attention by Ernst Mach and other physicists and philosophers, they seem to have had little influence on physicists working on the electrodynamical questions that led to special relativity, and so Lange's historical impact is difficult to assess. Yet something like Lange's general understanding of the role of reference-frames in mechanics became quite widespread in the philosophy of physics of the late nineteenth and early twentieth centuries, and it continues to make its presence known. It seems worthwhile, therefore, to look at the origins of this view and to see how a problem essentially concerning the invariant structure of Newtonian mechanics became a problem concerning a conventional choice of reference frames.

Lange developed his concept of inertial system from precedents in the German literature on the foundations of physics, especially Carl Neumann (1870), Heinrich Streintz (1883), and Ernst Mach (1883). Taking it as a basic principle that all motion is relative, these authors held that the classical law of inertia raised the fundamental "problem of the reference-system": relative to what system of reference is the motion of a free body rectilinear, and relative to what time-scale is it uniform? More generally, relative to what do the laws of motion hold? Neumann's infamous "Alpha body" was his answer to this question: he postulated that "at an unknown place in the cosmos," beyond our observation, sits a rigid and unchanging body, and that relative to this "Alpha-body" the motion of a free body is rectilinear (1870, p. 15). Streintz, attempting to improve on Neumann, proposed that the law of inertia holds with respect to *any* body found to be free of external forces, and named the appropriate reference-bodies "fundamental bodies." Both approaches had serious difficulties (see DiSalle 1988, especially chapter III), but the questions they were designed to answer seemed unavoidable once Newton's theory of absolute space was recognized to be unsatisfactory.

As Thomson's work shows, however, these questions are not really appropriate. The laws of motion do not hold "relative to inertial systems," or relative to anything. Again, by claiming that accelerations occur only in the presence of forces, the laws of motion assert the possibility of constructing systems in which every acceleration is traced to an interaction — in other words, they assert that inertial systems *exist* — and so one can't sensibly say that the laws are *true* only in such frames. True or false, the laws make *frame-independent* claims about the distinction between free motion and motion influenced by interactions. Ernst Mach recognized this when he pointed out that we could state the laws of motion without specifying any frame of reference at all, since the laws enable us to find a suitable frame (Mach 1883, p. 269); his chief objection to the laws was not that they are essentially relative, but that all of the *evidence* for them comes from motions relative to the fixed stars, and so there is no empirical reason to regard them as anything more than an inductive generalization about *those* relative motions. Even general relativity, while it suggests that one of the postulated Newtonian distinctions (between free motion and gravitational free fall) cannot be justified empirically, does not claim that such distinctions in general are relative to frames of reference. Yet by the end of the nineteenth century, a number of physicists and philosophers had come to regard the "relativity of the laws of motion" to a restricted class of reference frames as a fundamental principle, and by 1916 Einstein regarded this as the fundamental "epistemological defect" of the classical laws. It may be possible to gain some insight into the historical passage from Newton's physics to Einstein's from the nineteenth-century discussions of relativity and frame-dependence and the methodological issues that they raised.

Ludwig Lange arrived at the idea of "inertial-system" from Neumann's (1870) definition of equal times, which Lange called the "inertial time-scale": equal times are those in which a free particle travels equal distances. As Neumann pointed out (1870, p 18; see also Thomson and Tait, 1867, sections 247-48), this is an arbitrary stipulation, since one can always find a time-scale relative to which *any* particle motion is uniform. Given the stipulation, however, it is a factual claim that any *second* free particle will travel equal distances in equal times relative to the first. Since the comparison of distances and times for the two particles presupposes absolute simultaneity, and the two particles provide a dynamical definition of "equable flow" of time, Neumann's proposal is just an explicit dynamical version of Newton's absolute time. Lange's definition of the spatial "inertial system," and the corresponding law, are modelled on Neumann's formulation:

> **Definition I.** "Inertial-system" means any coordinate system with the characteristic that, with reference to it, the concurrent paths of three [material] points, simultaneously projected from the same point of space and then left to themselves (but which do not lie in a straight line) are all *rectilinear.*
>
> **Theorem I.** With reference to an Inertial-system, the path of *any fourth* point left to itself is also *rectilinear.* (Lange 1885a, p. 544-45.)

The requirement of *three* moving points follows from an argument analogous to Neumann's, presented more or less rigorously in Lange (1885a, 1885b). For three or fewer point motions, we can almost always construct a coordinate system in which these motions are rectilinear, and so it is a matter of convention whether three "points left to themselves" move in straight lines; we can first make a factual claim (Lange's "theorem") about the rectilinear motion of some *fourth* point. On these lines Lange believed he had solved the "problem of the reference-system": the laws of motion do not describe motion in absolute space, or even motion relative to the fixed stars, but motion relative to inertial systems.

The most important philosophical consequence of his work, according to Lange, was that one could capture the empirical content of what Newton called "absolute" in a relativistic treatment of motion. Since "Neumann's convention" had provided a "substitute" for absolute time, he mistakenly asserted, the concept of absolute time "has almost disappeared from present-day dynamics." (Lange 1885b, p. 336.) Since he viewed the inertial system as an analogous substitute for absolute space, he was convinced that his version of the law of inertia had accomplished the "avoidance of any *absolute* concept" (1885, p. 278). He therefore proposed some new terminology:

1. A point in uniform motion relative to an inertial system can also—with reference to another given inertial system—be treated as *inertially at rest.*

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2. A point with curvilinear motion relative to an inertial system cannot be called inertially at rest relative to any other, nor can one so designate a point whose motion relative to one inertial system is rectilinear but not uniform. (1885, p. 279)

Points that are not "inertially at rest" are "inertially accelerated" or "inertially rotating." But since the latter quantities are independent of the choice of an inertial system and so correspond exactly with what Thomson called absolute rotation and acceleration — even Lange conceded that "all the same, there remains in the concept of absolute motion a valuable core consisting of that which it has in common with the concept of inertial rotation" —it should be obvious that Lange is proposing only a verbal change. Furthermore, in one respect in which his proposed changes are *not* entirely verbal, they are seriously misleading. They suggest since the laws of motion themselves make sense only with respect to a certain kind of coordinate system, acceleration, rotation, and uniform motion are meaningful only in such coordinate systems. Obviously, however, the possibility of inertial frames assumes the possibility of dynamically distinguishing these states of motion; what is "relative to a coordinate system" is just what cannot be so distinguished, namely position and velocity. Thus, Lange's "relativistic" way of thinking about inertial systems obscures a crucially important aspect of the transition from absolute space to inertial frames: the abandonment of the search for a background against which dynamical laws are supposed to be valid, and a corresponding focus on the spatio-temporal structure intrinsic to Newtonian dynamics.

The methodological implications of his work are also difficult to gather from Lange's own statements. For one thing, his talk of a *procedure*, projecting material points from a given spatial point, seems to suggest that he is trying to base the law of inertia on an operational definition. (For this interpretation see, for example, Barbour 1989.) Yet Lange's stated philosophical aim was not to describe operational procedures, but to find the most abstract formulation possible of the law; for example, he disagreed with Mach's expression of the law by reference to the fixed stars, precisely because "abstract mechanics" demands a form of the law that "does not rest on any given object of physical astronomy, but rather consists of *purely dynamical* concepts." (1885, p. 269.) Moreover, he repeatedly emphasized that the inertial system was supposed to be an "ideal construction" which "could never find immediate application," but from which all practical methods could be derived (e.g., 1885a, p. 544). He described the "material point left to itself" on which the construction is based as a "mathematical abstraction" and a "requirement that is never completely fulfilled"; on that account, the content of the law of inertia is "never factually given, but rather *assumed*" in order to comprehend mechanical phenomena. Lange's project, therefore, was only a conceptual "completion" of the law, a "purification of the hypothesis of superfluous elements" (1885, p. 270), and was not *intended* to be operationalistic.

We can get a clearer idea of Lange's methodology, and of the consequences it had for his broader philosophy of space and time, from his understanding of the conventional stipulation that he placed at the core of the law of inertia. First, he saw Neumann's convention for equal times as an illustration of a profound and general methodological prescription, which he called the "principle of particular determination": a scientific "theorem" should be expressed

> so that the theorem is conventionally valid for the smallest possible part of its objects; the contents of the theorem relate, then, insofar as they are more than mere convention, insofar as they are new empirical results, only to all of the rest [of the objects to which the theorem refers].(1885, p. 278.)

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In other words, given a hypothesis that generalizes about a class of objects, we ought to determine how much arbitrariness there is in the generalization, and to express the non-arbitrary, empirical content of the hypothesis with the minimal number of objects. Lange's determination that the law of inertia is a convention for three or fewer particles, and an empirical claim for more than three, was intended to be a direct application of this principle. Indeed, he saw his recognition of this "partial convention" as the crucial difference between his formulation of the law inertia and James Thomson's (Lange 1885b, p. 351).

Considering his place in the history of the philosophy of space and time, Lange elevated his "partial conventionalism" into a metaphysical challenge to the Newtonian belief in the existence of absolute space. To Newton's argument that centrifugal effects reveal the absolute rotation of a sphere in absolute space, Lange responded that

> For the satisfaction of our epistemological need it suffices completely to introduce the inertial system as an *ideal convention* and to refer the motion of the sphere to this. We are not in the least advanced by the assumption of a *really-existing* [emphasis added] immaterial coordinate system.

Like some contemporary commentators, Lange blamed Newton's metaphysical realism concerning space and time on his "religious conception of nature," according to which space and time are "creations of the Eternal and Omnipresent." The modern scientist, however, more accustomed to the "systematic separation between belief and knowledge," does not need to claim real, "transcendent" existence for the mental constructions that are created in order to give coherence to the phenomena. Thus Lange viewed absolute space as Newton's realist answer to the "problem of the reference system," and considered that he himself had solved the problem in a conventionalist way, showing that the law of inertia is essentially founded in a coordinate system chosen by convention.

To understand clearly the philosophical significance of Lange's work, however, his own interpretation notwithstanding, we have to examine the precise role played by convention. His mathematical arguments make it a matter of convention whether three particles are moving in straight lines, but what does this imply about the dynamical case of free particles in rectilinear and uniform motion? One such particle can always be represented as moving uniformly in a straight line by suitable adjustments of the coordinates and the time-scale, but two or three particles whose mutual distances increased and decreased could not be represented as moving each in a straight line in the same sense. What we can say about the requirement of three free particles is more restricted than the "conventionalist" result about arbitrarily moving points: a reference-system in which one, two, or three free particles move uniformly may not be one in which all free particles move uniformly. Lange's illustration of this supposes, first, that all free particles move uniformly relative to some reference-system (which we can call the inertial frame). Obviously there will be systems relative to which some one of these particles moves uniformly, but which rotate (for example, about the path of the particle) relative to the inertial frame; we can even find systems in which two of the particles move uniformly, but which rotate (for example, about the line joining those two particles) relative to the inertial frame — and, evidently, relative to any system in uniform motion relative to the inertial frame. After the third particle, however, the freedom to adapt the frame comes to an end: as long as the particles do not travel collinearly or in parallel paths, any movement of the frame that preserves the uniform rectilinear motion of these particles must do the same for all the free particles in the inertial frame. So the *dynamical* significance of Lange's minimal "partial convention," as he pointed out (1885b, pp. 344-46), is that if there is at least one coordinate system and time scale in which arbitrarily many free particles move uniformly, any three free particles satistfying Lange's conditions define another system and time scale in uniform motion relative to the first.

All of this indicates that Lange did not really provide a conventionalist answer to the "problem of the reference system." In fact he answered a different sort of question, one more nearly related to Thomson's approach than one might have thought: *assuming* that inertial frames exist, in what circumstances can we say that any given frame is inertial? If such a class of frames exists, it is not a matter of conventional choice whether a given frame belongs to the class; what Lange has shown is that a factual determination can indeed be made, but only with at least three free particles. Far from supporting the relativity of motion, then, Lange's spatial "particular determination" (that three particles are required) postulates an objective distinction between the state of free motion and other possible states: for particles moving freely, frames are always possible in which aritrarily many move uniformly in straight lines, while for particles moving anyhow, such frames are generally possible for any three particles. Lange recognized this point in 1902, when he published a reconsideration of his 1885 work in light of critical reactions to it (1902, pp. 9, 37). Now, instead of presenting the inertial system as that relative to which the law of inertia holds, he claimed (analogously to James Thomson) that "the pronouncement of the law...is simply reduced to the assumption of the *phoronomical possibility* of a system in which *arbitrarily many* (n > 3) points left to themselves" move uniformly; the three particles projected from a point, formerly part of the definition underlying the law, Lange now recognized as "the simplest possible prototype of all practical real methods" of constructing an inertial system (1902, pp. 38-39).

Thus in the course of Lange's philosophical development, his "ideal construction" lost its foundational role in the expression of the law of inertia and became a way of considering the possible application of the law. The law itself, meanwhile, became something that Lange had criticized Newton for expressing, namely a kind of "existence-hypothesis," insofar as it asserts the existence of reference systems in which free particles move uniformly. Lange himself did not fully appreciate this point, and never abandoned the view that he had established some form of relativism combined with conventionalism, and it is not difficult to understand why (especially in light of the fact that even contemporary physics texts occasionally say that the laws of motion hold relative to inertial systems). When we speak of the "kinematical possibility" of constructing a certain kind of reference system, namely one in which every acceleration is proportional to an impressed force, we seem to be naming just one of (obviously) many possibilities for the assignment of coordinates; this leaves an opening for the popular but vague statement that inertial frames are merely the simplest possible ones — a statement that evidently contains a certain element of truth, but that ignores the grounding of inertial frames in the lawlike dynamical structure of the Newtonian universe. Therefore the assertion that they are possible seems scarcely comparable, in its "metaphysical" significance, to a claim about the affine structure of Newtonian spacetime. For similar reasons it is frequently said that Minkowski's space-time formulation of special relativity has essentially different metaphysical implications from Einstein's original formulation. In the time before the invariant four-dimensional version of Newtonian mechanics was developed, then, it was (at first glance) comparatively easy to suppose that the abandonment of absolute space for an equivalence class of possible frames was really the abandonment of a realistic picture of spatio-temporal structure for a conventionalist one.

In order to clear this matter up, and to understand from a methodological point of view what Thomson and Lange really accomplished, we need to recognize exactly where convention really enters into the construction of an inertial frame. That an inertial frame is possible is evidently a strong dynamical claim: it means that every component of every acceleration within a dynamical system is objectively traceable, by virtue of the equality of action and reaction, to some source within the system. This implies, for example, in the case of our solar system, that of all the possible resting points we could choose in order to "frame" the system, those are distinguished *by law* — not necessarily by simplicity or any other kind of "convenience" — in which all accelerations are determined by Newtonian interactions. (The dynamical analysis of all of these interactions warrants Newton's assertion that, assuming the laws of motion and astronomical phenomena, we can "demonstrate the frame of the system of the world." 1729, p. 323.) Thus, as was noted above, Thomson's and Lange's claim is precisely equivalent to the claim, in the four-dimensional picture, that of all the geometrically possible worldlines, those are distinguished which represent physically possible trajectories of free particles. However it is formulated, this structure provides a definition of matter in its passive inertial state, and so it is the necessary foundation for the Newtonian account of active forces as causes of accelerations. Whatever conventionalist challenge affects the proposed geometrical structure, then, affects at the same time the entire classical picture of "fundamental forces of nature."

The opening for conventionalism occurs precisely with the principle through which the commitment to the Newtonian program (and its accompanying spatio-temporal structure) is first made, a principle which Lange, for all his stated conventionalism, was content to take for granted. This is just the principle that there is an objective distinction between free motion and motion under the influence of a force. Lange spoke of the free particle as the "element" of dynamical theory, in the sense that constructions (like his construction of the inertial system) are "derived from" this element just as constructions in pure geometry are derived from the geometrical point (1885, p. 350). But he did not see that, while this ideal physical thing, the free particle, is indeed the element of a physical theory, its spatio-temporal *path* is the element of a spatio-temporal affine geometry in *precisely* the sense in which spatial length is the element of spatial metrical geometry; the worldlines of freely moving particles thus provide a "coordinative definition" of the affine structure of space-time just as the rigid body provides a coordinative definition of Euclidean length. We can give empirical arguments for or against the suitability of these definitions: for example, Newton's Scholium on space, time, and motion argues empirically that the definition of true motion through force and acceleration makes sense and has a clear application, while Einstein's argument about the equivalence principle suggests that the traditional definition cannot be unambiguously applied, and therefore (in effect) that the affine structure of space-time is best coordinated to the paths of freely falling particles. In either case, the justification for the definition extends only as far as the successful application to the phenomena that the theory is supposed to address.

Of course it was impossible to say all of this explicitly in the language available to Lange, before the concept of space-time was developed, but, in a review of Lange's work written in 1891, Gottlob Frege made essentially the same point. He criticized Lange for exaggerating the difference between his "inertial" concepts and those that Newton called absolute, and suggested that the question whether motion is "real" was only a verbal dispute: the important question, he pointed out, was whether there is a real distinction between accelerated and unaccelerated motion. The distinction is real, he asserted, "in the same sense in which the constancy of a length is real": "in both cases we have arbitrary stipulations, which however are so closely linked to the lawfulness of nature that they are thereby distinguished from all other stipulations which are mathematically and logically equally possible." (Frege 1891, p. 157.) Both stipulations connect a physical process (respectively, measuring forces by accelerations and measuring lengths by rigid measuring-rods) with an aspect of geometrical structure, and the success of the stipulation suggests its connection with some regularity of nature. This sort of argument follows a pattern familiar from discussions of the measure of time: it resembles the claim that a pendulum clock is a better choice to measure time than my heartbeat because the rate of the clock stands in a simpler relation to natural laws than the rate of my heart. We therefore make no special ontological claim when we call the quantities thus defined "absolute, true, and mathematical," as long as we mean, "as opposed to relative, apparent, or common," insofar as only the former are imbedded in a coherent system of laws.

The difficulty Lange's contemporaries had in seeing Frege's point brings out an interesting difference between the inertial frame postulate and its four-dimensional counterpart. In the latter case, the connection between the elementary physical process and its geometrical representation (between the path of a free particle and the affine geodesics of spacetime) has (*modulo* the essential difficulty of having four-dimensional intuitions in the first place) the same intuitive obviousness as the connection between the congruence of rigid rods and Euclidean length: the physical invariant "looks like" the geometrical invariant. In the former case, however, we can only picture the projection of a geodesic in a given inertial frame (a rectilinear motion in a three-dimensional coordinate system), and we know that this is just one of an equivalence class of allowable projections. Before there was an invariant representation of what is common to members of the equivalence class, it was perhaps a natural mistake to look here for some conventional element in the theory of motion. And so it seems to me even more remarkable that Thomson should have recognized that the intrinsic spatio-temporal structure of Newton' laws could be expressed in nineteenth-century language.

More recently, misunderstanding of the fundamental coordination underlying space-time structure has created difficulties for us, who are accustomed to the four-dimensional picture. In the modern version of the metaphysical debate between Newton and Leibniz, spatial relations are assumed to be epistemologically unproblematic, because the geometry of these relations is easy to think of as the structure of possible distances between objects — as a set of rules governing the motion and comparison of rigid bodies. Space-time, by contrast, represents states of bodies *through time*, and bodies are therefore thought to have states of motion "relative to" it; inertial forces arising from different states of motion are thought to be *caused* by it. Therefore space-time seems to take on the aspect of a thing, where space alone could be thought of as an order; the *existence* of space-time therefore seems to require some special metaphysical argument. But space-time is no more the cause of differences in states of motion than Euclidean space is the cause of differences in length. Both are "structures of possibilities" coordinated to elementary physical processes; space embodies a set of laws governing possible momentary distances among bodies, space-time a set of laws governing possible dynamical evolutions. Neither one can be coherently reduced to a set of relations, since both structures *govern* possible relations. Yet such a structure is not really a thing either, at least not like the things to which its structure is coordinated. The history of the theory of space-time structure, beginning with its origins in the three-plus-one dimensional account of inertial frames, helps us to see that the structure is an aspect of our laws of motion, and that the important philosophical questions about it are not ontological, but methodological after all.

References

Barbour, J. (1989), *Absolute or Relative Motion?* v. 1. Cambridge: Cambridge University Press.

DiSalle, R. (1988), *Space, Time, and Inertia in the Foundations of Newtonian Physics*, 1870-1905. Ph.D. dissertation, University of Chicago.

Frege, G. (1891), "Uber das Tr gheitsgesetz." *Zeitschrift fHr Philosophie und philosophische Kritik* 98: 145-61.

Lange, L. (1885), "Ueber die wissenschaftliche Fassung des Galilei'schen Beharrungsgesetzes." *Wundt's Philosophische Studien* 2: 266-97.

______. (1885a), "Nochmals ueber das Beharrungsgesetz." *Wundt's Philosophische Studien* 2: 539-45.

______. (1885b), "Uber das Beharrungsgesetz." *Berichte der Koniglichen Sachsischen Gesellschaft der Wissenschaften zu Leipzig, Mathematisch-physische Classe* 37: 333-51.

______. (1886), *Die geschichtliche Entwickelung des Bewegungsbegriffes und ihr voraussichtliches Endergebniss. Ein Beitrag zur historischen Kritik der mechanischen Principien*. Leipzig: Wilhelm Engelmann.

______. (1902), "Das Inertialsystem vor dem Forum der Naturforschung. Kritisches und Antikritisches." *Wundt's Philosophische Studien* 2O, v. 2: 1-71.

Mach, E. (1883), *Die Mechanik in ihrer Entwickelung, historisch-kritisch dargestellt*. Leipzig: F.A. Brockhaus. 9th edition, 1933.

Neumann, C. (1870), *Ueber die Principien der Galilei-Newton'schen Theorie*. Leipzig: B.G. Teubner.

Newton, I. (1729), *The Mathematical Principles of Natural Philosophy*. Translated by Andrew Motte. London. Reprint: New York: The Philosophical Library, 1964.

Streintz, H. (1883), *Die physikalischen Grundlagen der Mechanik*. Leipzig: B.G. Teubner.

Thomson, J. (1884), "On the Law of Inertia; the Principle of Chronometry; and the Principle of Absolute Clinural Rest, and of Absolute Rotation." *Proceedings of the Royal Society of Edinburgh* 12: 568-78.

Thomson, W. and P.G. Tait. (1867), *Treatise on Natural Philosophy*. Cambridge: Cambridge University Press.