

Translation of Wenzel Hofmann's relative-inertia  
booklet (with AI tools)

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Critical Examination of the Two Fundamental  
Concepts of Mechanics:

**Motion**  
and  
**Inertia**

and the conclusions drawn therefrom concerning the  
rotation of the Earth about its axis  
and the  
Foucault pendulum experiment

by

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## Preface

From all domains of science the human intellect strives for progress. It is especially the natural sciences in which this striving finds the most rewarding field, and indeed on the one hand in the investigation of new, hitherto unobserved facts, and on the other hand in the examination of those theories and views that have hitherto been regarded as valid, with respect to their soundness.

In the latter direction, physics in particular offers an extraordinarily wide field for the pursuit of insight into what is truly real. It is precisely this direction that has always interested me quite especially, and I have made use of a large part of my leisure time in this sense. Very soon, however, I was compelled to discover that there still exist many theories and conceptions which do not appear to be completely well founded.

It will doubtless be held against me from many sides that I dare to call into question theories that were established by the greatest authorities and have for a long time been regarded as generally valid. Yet in the striving after truth one cannot allow oneself to be restrained by the judgment of an authority, however esteemed it may be, if one believes that one must raise well-founded doubts concerning the views it represents.

True knowledge cannot have its basis in belief in authority, but solely and exclusively in objective arguments.

In this spirit I have judged the various physical theories, and found, as already mentioned earlier, here and there occasion to cast doubt upon them, as has certainly happened not only to me, but also to many others.

Nevertheless, I did not feel the need to express these doubts publicly. Only the Foucault pendulum experiment, which was presented to the public in the Vienna Rotunda in the past month, prompted me to bring one of these doubts before the public in an article in the *Neues Wiener Tagblatt*.

The aforementioned article read as follows:

## The Foucault Pendulum Experiment

*(No proof of the rotation of the Earth about its axis?)*

The experiment mentioned in the title, first carried out by Foucault in the year 1851 and since then repeated in various places, was publicly demonstrated in Paris last year and, for several days now, has also been publicly presented here in the Rotunda as proof of the axial rotation of the Earth.

As a result, this experiment—which otherwise interests only the scientific world and students—has drawn the general attention of the public to itself. Under such circumstances it will therefore also be worthwhile to say a few words, by means accessible to the public, about the significance of this experiment, especially since a view is advocated here that contradicts prevailing conceptions.

The issue at hand is nothing less than the renewed uprooting of a question that has long been regarded as definitively settled, namely:

“Does the dome of the heavens (Sun, Moon, and stars), as we believe we perceive, rotate once around the Earth within a day, or is this perception really a sensory illusion that can find its sole correct explanation only in a daily axial rotation of the Earth?”

Already at the level of elementary instruction, young people are told that the former perception is in fact an illusion, that the observed motion of the Sun is merely an apparent one, while in reality the Earth completes an axial rotation within a day. At the upper level of secondary education, attempts are then made to prove this view, and as one of the supposedly most conclusive proofs, the Foucault pendulum experiment is adduced. The most recent repetitions of this experiment in Paris and Vienna are likewise presented to the public as proof of the axial rotation of the Earth.

Despite all this, I put forward the following propositions, which contradict the prevailing view:

1. The Foucault pendulum experiment is not a proof of the axial rotation of the Earth as the sole correct conception.
2. It is illegitimate to explain the daily observable motion of the Sun (and the stars) as a “merely apparent motion.”
3. The two conceptions—“daily circular motion of the Sun around the Earth” and “daily single axial rotation of the Earth”—do not stand in contradiction to one another, but are merely two different, yet fully equivalent, forms of apprehending one and the same fact.
4. The true significance of the Foucault pendulum experiment has, up to now, not been recognized at all.
5. The experiment in question can never bring about a decision between the two aforementioned views; rather, it provides very important insights into the hitherto only very incompletely understood and in part incorrectly defined law of inertia.

A thorough justification of these propositions undoubtedly requires more space than is available to me here; nevertheless, I promise to present the complete proof of them in a small work to be published in the very near future. Here I shall only briefly draw attention to a few weaknesses of the conception that is currently regarded as exclusively correct.

This conception simply explains the circular motion of the Sun that we observe daily as a “merely apparent motion.” In the face of such an assertion, does not the question arise almost irresistibly: wherein does the essential difference between a real and an apparent motion actually consist? What reliable criteria do we possess in order to distinguish one concept from the other?

It is in fact downright astonishing when one must then admit that up to the present essentially no one has seriously occupied themselves with defining these

two concepts in such a way that a strict distinction between them would be possible. How, under these circumstances, can one declare one motion to be “real” and another to be “apparent” when any criteria whatsoever for distinguishing the two are lacking?

I am well aware that most readers of this article will at once be ready to propose such a criterion; I already hear, in my mind, the response:

“Every motion is a relative one, referred to a particular system of position; if this system of position itself appears to be in motion in some way, then the motion first mentioned is only an apparent one.”

As an example of this view one might cite a motion observed inside a railway carriage: a person who walks inside the carriage in the direction opposite to the train’s motion with the same speed with which the train moves forward remains at the same place relative to the masses located outside the train. At first glance it thus really seems as though one were entitled to declare the motion observed inside the train to be an apparent one, since the person remains at rest relative to the objects outside the train.

But even a distinction between “real” and “apparent” motion attempted from this standpoint proves, upon closer consideration, to be entirely untenable—a point I shall demonstrate elsewhere.

If it is already in itself illegitimate to declare one motion real and another apparent without first establishing adequate criteria for distinguishing the two concepts, then in addition there is a logical error in the conclusions currently drawn from the Foucault pendulum experiment.

These conclusions rest, as their most fundamental basis, on the following experiential proposition:

A freely swinging pendulum does not change the direction of its plane of oscillation when its point of suspension is moved (rotated).

In the formulation of this proposition there lies an incompleteness that later leads to incorrect conclusions. Namely, the invariance of the direction of the plane of oscillation is stated without any comparison to a particular system of position—thus, as it were, as an “absolute” invariance—although it is in fact only a relative one.

The complete statement ought to read:

“The direction of the plane of oscillation remains unchanged *relative to the extraterrestrial masses.*”

By failing to relate the direction of the plane of oscillation to the extraterrestrial masses and by declaring its invariance to be “absolute,” these extraterrestrial masses are unconsciously assumed to constitute a system of position—and indeed one that is likewise taken to be in “absolute” rest. This, however, is an obvious error, since neither the concept of “absolute” motion nor that of “absolute” rest exists.

The logically correct conclusion can scarcely be anything other than this:

“The direction of the plane of oscillation is invariant relative to the extraterrestrial masses.”

If we now observe a change in the mutual configuration between the mass of the Earth and this plane of oscillation, we can draw no other conclusion from this than that the mutual configuration between the Earth’s mass and the extraterrestrial mass has also changed.

To conclude from this, however, that the extraterrestrial mass is the resting one and the Earth the moving one—there is not the slightest justification for this in the observation.

In order to observe such a mutual change between these two masses, we truly do not need the Foucault pendulum experiment; we can ascertain it much more conveniently from the daily rising and setting of the heavenly bodies.

The pendulum experiment must therefore be assigned an entirely different significance than that of serving as proof of the Earth’s axial rotation.

In conclusion, permit me to illustrate the current conception of the so-called “apparent” motion of the Sun by means of a very popular example. To this end I shall choose the form of a dialogue between two persons, A and B.

## Dialogue

**A:** When I take a leisurely drive in my automobile, am I really moving with my vehicle, or is it an illusion if I believe myself to be in motion?

**B:** There can scarcely be any objection raised against the reality of this motion.

**A:** If I were to have a road built along the equator, bridging the oceans, and were to walk along this road around the Earth, would the motion still be a real one?

**B:** Certainly; for there is no reason to declare a motion that is real on any arbitrary road to be apparent in this case.

**A:** If I now continuously increase the speed of my motion, can the real motion thereby pass over into an apparent one?

**B:** Certainly not!

**A:** I begin my journey precisely at noon (the Sun stands at the zenith) and choose such a speed from east to west that I traverse the entire equator in 24 hours; during this time I hold a rod with a golden knob vertically upward. Does not the rod, together with the knob, also really move, and indeed complete a circuit around the Earth in 24 hours?

**B:** Yes.

**A:** But my rod reaches all the way to the Sun, and the knob is just as large as the Sun. Does the motion of the rod or of the knob thereby lose its character of reality?

**B:** No—on the contrary; the longer the rod, the more rapid the motion of the knob must be, since it has to traverse an ever larger circle.

**A:** Since I began my journey exactly at noon, let the rod reach to the Sun, and let the knob be identical with the Sun; then the knob is identical with the

Sun and must remain so throughout the entire journey, since I circle the Earth in 24 hours. If the real motion of the knob is conceded to me, can the Sun now be denied this “real” motion?

**B:** I must also concede this final conclusion; but there must surely be some error somewhere in the entire chain of reasoning, since the result of the final conclusion stands in direct contradiction to the theory of the “apparent” motion of the Sun that is nowadays regarded as generally valid.

**A:** This contradiction, however, does not find its cause in an error in my reasoning, but rather lies in the entirely unjustified distinction between “real” and “apparent” motion.

**B:** But what, then, is to be said of the Foucault pendulum experiment? How is one to interpret the phenomenon of the rotation of the plane of oscillation that appears there?

**A:** Under no circumstances may it be regarded as proof of the apparent character of the daily motion of the Sun, because there is in general no difference whatsoever between real and apparent motion. The fact observed in this experiment is a phenomenon of inertia; it directs our attention to the general law of inertia and urges us to pass more critical judgment upon it than has hitherto been the case. One soon arrives at the insight that this law has so far been only incompletely understood and in part incorrectly defined.

I am fully aware that in this brief essay the propositions set forth at the outset have not yet been conclusively demonstrated; a complete proof is not possible at this place, where there is scarcely room even for a brief indication of all that one would like to say. I nevertheless repeat my promise to treat my theories more fully and to prove them in detail in a work to be published shortly for this specific purpose.

Professor Wenzel Hofmann

Vienna, November 1903

## Motion

At first glance it may well appear superfluous to devote many words to this concept; for surely everyone believes themselves to be fully clear about what is to be understood by motion.

And yet this is not the case. Precisely because it is generally assumed that the concept of “motion” is one that stands clear in and of itself, instruction often neglects to devote the necessary attention to it; unclear conceptions, false judgments, and consequently false conclusions then arise.

The entirely unjustified distinction between “real” and “apparent” motion, as well as the dispute: “Does the Sun rotate around the Earth, or does the latter rotate once about its axis in 24 hours?”, are consequences of an incomplete grasp of the concept of motion.

It shall therefore be attempted here to examine this concept somewhat more thoroughly than is usually done, and then, on the basis of these considerations,

to render a definite judgment as to whether one may draw a distinction between “real” and “apparent” motion.

In this way it will of itself become clear in what manner the question: “Does the Sun rotate around the Earth, or does the Earth rotate about its axis?” must be answered.

First of all, for this purpose it appears necessary to establish an unassailable definition of the concept “motion of a material point.”

This seems quite easy; I believe no one could formulate this definition otherwise than as follows:

“By the motion of a material point one understands any change of its position.”

However simple this definition may sound, it nevertheless contains a difficulty that is not to be underestimated, which consists in the fact that it introduces a further concept that itself requires more precise definition—namely, the concept of “position”.

Before giving a final definition of this concept, I would first like to emphasize that anyone, whatever their level of education may be, cannot comply with the request to specify a definite position in any other way than by relating the point whose position is to be determined to a series of other sensibly perceptible points (lines, surfaces, or bodies).

For example, one fixes the position of a point by stating its distances from three other points or from three planes, etc.

Let us call the points, lines, or planes to which we relate a point when determining its position “position-elements,” and the totality of the elements used for such a determination of position a “system of positions.” The concept “position of a point” is then defined as the relation (distances) of this point to the elements of this system of positions.

It is clear that the determination of the position of a point must always be preceded by the choice of a system of positions. In fact, this always occurs, even though in many cases unconsciously.

The question now arises: how many elements are necessary for a determination of position, and what conditions must be required of them?

Geometry answers this question on the one hand by stating that three elements in mutually invariant relation are required. On the other hand, however, it appears necessary that these position-elements be sensibly perceptible, for only to such elements can definite relations actually be ascertained.

For the decision of the questions to be treated here, it is by no means necessary to pursue the geometric field any further; rather, it appears important to become clear about the following: every observation of a motion must be preceded by the conception of definite positions. This conception must be preceded by the choice of some system of positions. This choice occurs either consciously or unconsciously; in most cases the latter is true.

I must now remark here that the conception of the concept of motion developed in this way encompasses only so-called “relative” motion, and that in

what follows, whenever the simple designation “motion” is used, relative motion is always meant.

An important question must now be decided: is the choice of a system of positions really bound to nothing other than the sensuous perceptibility of its elements and their invariant relation to one another, or are there perhaps circumstances that render the choice of a system of positions unjustified despite the fulfillment of the previously mentioned conditions?

Some of my readers will probably answer this question with a definite “yes.”

I almost hear the words with which one will object to me in this regard: it depends on the choice of the system of positions whether a motion is to be regarded as real or apparent. A person who is inside a moving railway train is at rest or in motion relative to a system of positions chosen inside the carriage; but this choice of system of positions is illegitimate, and the state of rest or motion observed on its basis is only an apparent one, because the chosen system of positions is itself in motion relative to a system of positions chosen outside the train.

Through this objection, the circumstance would indeed be regarded as decisive for determining whether a motion is real or apparent—namely, whether the chosen system of positions is itself at rest or in motion relative to another system of positions.

If, however, we were to establish the principle that a motion is to be regarded as apparent whenever the system of positions to which it is referred is itself in motion relative to another system of positions, we would very soon arrive at the most peculiar conclusion that there can be no real motions at all, but only apparent ones.

A simple example will easily show us the untenability of this principle.

Let us imagine in infinite space only three masses  $A$ ,  $B$ , and  $C$ , which stand in mutually variable relation to one another. Let  $A$  be in motion relative to  $B$ ; this motion would then have to be declared apparent, because  $B$  itself is in motion relative to  $C$ . But likewise the motion of mass  $B$  relative to  $C$  would have to be declared apparent, because  $C$  in turn is in motion with respect to  $A$ .

This simple example therefore yields, under the validity of the aforementioned principle, a self-returning chain of conclusions that completely excludes the possibility of any real motion.

How much more must this be the case if we presuppose more than three masses standing in mutually variable relations to one another, as indeed corresponds to the actually existing circumstances?

I can now calmly assert that the view according to which a motion is to be declared apparent when the system of positions to which it is referred is itself in motion relative to a second system of positions must be abandoned, because under this view we would have to concede only apparent motions in general.

But in order to become clear as to whether perhaps the magnitude of the mass of a system of positions could be decisive for the interpretation of a motion observed relative to it as real or apparent, let the following example be considered.

Let us imagine in space nothing other than two completely identical spheres  $A$  and  $B$ , whose mutual distance is continually increasing. If we choose the system of positions on  $A$ , then  $B$  must be declared to be in motion, and conversely, if the system of positions is chosen on  $B$ .

In this case it is entirely undisputed that one choice of system of positions is just as justified as the other, and that in both cases the motions observed must either both be declared real or both be declared apparent motions; but there is not the slightest reason for adopting the latter view.

We are thus confronted in this example with a case in which one and the same phenomenon—namely, the increase of the mutual distance between the two spheres—admits two different but evidently equivalent interpretations as real motions.

Let us go one step further and assume that one of the two spheres, say  $A$ , is enlarged by a single molecule. Am I now still as free as before in the choice of the system of positions, or does there now exist a “compelling” reason to choose the system of positions on  $A$  and to be obliged to declare  $B$  as the moving mass, or may I even now still choose the system of positions on  $B$  and declare  $A$  to be in motion?

I believe that there will be hardly anyone who would wish to assert that the motion which previously had to be granted the character of real motion can now be denied that character merely on account of the addition of a single molecule.

And if now a second, third, etc. molecule is added to  $A$ , will one ever be in a position to say, at a definite moment during the growth of  $A$ : now, at this moment, the free choice of the system of positions ceases; from now on only the motion of  $B$  relative to  $A$  may be regarded as a “real” motion, while the motion of sphere  $A$  observed relative to  $B$  must henceforth be declared an apparent motion?

Surely, after these considerations, everyone will arrive at the conviction that when two masses stand in mutually variable relation to one another, it is indifferent which of the two is declared to be the system of positions and which the moving mass.

It may indeed serve a particular *purpose* to regard the larger mass  $A$  as the system of positions and the smaller  $B$  as the moving mass; nevertheless, no obstacle whatsoever stands in the way of the opposite conception, and it would certainly be a logical error to designate, in the latter case, the motion of  $A$  observed relative to  $B$  as an apparent motion, as though it were a sensory illusion.

If we now imagine in infinite space three spheres  $A$ ,  $B$ , and  $C$ , of which  $A$  and  $B$  stand in invariant relation to one another, while sphere  $C$  exhibits variable relations to both, then it must surely be permitted to regard the two spheres  $A$  and  $B$ , despite their spatial separation, as a rigid whole.

In doing so, however, we have arrived at the same standpoint as before; once again there are two unequal masses—( $A+B$ ) the larger,  $C$  the smaller; once again there is no compelling reason to have to choose the system of positions on ( $A+B$ ); once again the motion of ( $A+B$ ) relative to  $C$  must still be regarded, with full justification, as a real motion.

The conclusion does not change even if we imagine ever so many bodies rigidly connected with one another and only a single body standing in variable relation to them.

The choice of the system of positions must still, with full equality of right, be made either on the totality of the many rigidly connected bodies or on the single body.

I should like to formulate this insight as an important law and give it the name "*Reciprocity between the moving mass and the matter of the system of positions.*"

The law would read:

If a relative motion of a mass  $A$  with respect to a second mass  $B$  is ascertained, then with full equality of right  $B$  may also be declared to be really in motion with respect to  $A$ .

It now appears not entirely impossible that a whole series of my readers will agree completely with the considerations presented thus far, but will nevertheless object as follows:

All of these examples and the conclusions drawn from them refer only to relative motions; they cannot, however, find application when a point is in absolute motion.

I meet this objection with the assertion:

"There is no such thing as absolute motion at all."

The "absolute" motion of a mass would have to be a motion that could be ascertained—or at least conceived—entirely without reference to a second mass.

One would then also have to be able to ascertain, or at least to conceive, a motion of a mass that is completely alone in infinite space.

But under this presupposition there can be no talk of ascertaining a motion, since motion can be ascertained only on the basis of sensuous perceptions, for which, in the complete absence of sensuously perceptible objects of comparison, no possibility is afforded.

There will still be many who maintain that even if one can never ascertain an absolute motion, one can nevertheless conceive such a motion.

But even those who believe that they can truly conceive a body in motion in an infinite space containing only that one body are in a state of self-deception.

They believe they can first imagine the body in its original position and then imagine the same body at a certain distance in a new position; in order to obtain an intuition of the magnitude of the path traversed by the mass, they imagine the original mass simultaneously with that same mass in its new position; they thus fictitiously introduce a second mass that now occupies the original position of the first, and thereby, albeit unconsciously, once again enter the domain of relative motion—only now they are comparing the real mass not with a real one, but with a fictitious one.

But can such a motion, existing merely in imagination, not sensuously perceptible, yet still bearing the character of positional comparison and thus still

relative motion, lay claim—over against sensuously perceptible motions—to the title of a real motion?

Certainly not!

Rather, one must render the judgment: this so-called “absolute” motion is only a subjective conception of individuals, which lacks the most important characteristic of reality, namely perceptibility.

Incidentally, I shall return once more to this “absolute” motion in the second chapter of this work and will there show that the experiment upon which Newton based this concept was an inadequate one.

After I had arrived at the insight that there are in general only relative motions, and that one cannot deny a motion the character of a real motion merely because it is a “relative” one, I searched for some characteristic by means of which perhaps some motion would nevertheless have to be regarded as an “apparent” motion. Despite all my efforts, I was unable to find such a characteristic; what struck me as particularly strange in this context was the circumstance that so many authors in their works assume the existence of the concept of “apparent motion” without having made even the slightest attempt to define this concept or to specify a characteristic by which a motion could be recognized as an “apparent” one.

It would also be of the greatest interest to me if anyone were able to specify such an infallible characteristic.

I now wish to set down, in concise form, what has been discussed thus far, in a few propositions:

1. All perceptible motions are relative motions, *i.e.*, they always relate to some material system of positions.
2. All of these motions permit the reversal of their interpretation, *i.e.*, one may interchange the moving mass and the system of positions in such a way that the former is declared to be the system of positions and the mass of the latter the moving mass (law of reciprocity).
3. Every ascertainment of a motion must, if the statement is not to suffer from the defect of incompleteness, specify with full definiteness the system of positions to which it refers.

For example, the statement: “The Earth rotates about its axis” is an incomplete one; the statement must be completed as follows: “The Earth rotates about its axis with respect to an extraterrestrial system of positions.”

4. No motion expressed in this complete form can be denied the character of reality if the moving body actually undergoes a change of position within the specified system of positions.

Example: the Sun is in real relative rotation around the Earth with respect to a system of positions assumed on the Earth, because it actually changes its position within such a system.

5. One and the same mass may, when referred to different systems of positions, exhibit different motions; nevertheless, each of these must be accorded the character of reality.

The conception of the Earth's motion that is generally accepted today provides an example of this. The Earth is in rotating motion with respect to the Sun; an arbitrary point of the Earth completes one revolution in 24 solar hours with respect to the Sun; during this time, an arbitrary point of the Earth describes a complete circle about the Earth's axis. With respect to the remaining fixed-star sky, however, the individual points of the Earth do not describe circles but cycloidal paths. Moreover, the Earth completes, with respect to the Sun, 365 rotations within a year (leaving aside the fractional part of the 366th rotation), while with respect to the fixed-star sky it makes, in the same time (again disregarding the last fractional part of a rotation), 366 rotations; for the year comprises 365 whole solar days but 366 whole sidereal days.

If one were now to adopt the standpoint that only a motion observed with respect to the fixed-star sky (excluding the Sun) is a real motion, then the phenomenon mentioned would have to be explained as follows: the circular motion of the various points of the Earth would be an apparent motion, while the real motion of these points would proceed along cycloidal paths. The observation that the Earth makes 365 rotations about its axis in a year would have to be regarded as an illusion. One would have to declare: the Earth performs 366 rotations in one year in the course of cycloidal motion of the individual points of the Earth.

In this way, at one stroke, the daily rotation of the Earth in 24 solar hours, as well as the circular motion of the individual points of the Earth about the Earth's axis, would be degraded to apparent motions.

I am by no means of this view; rather, I assert the proposition: each of the two observations is ascertained on the basis of an admissible system of positions; hence both motions must also be regarded as real motions.

I deliberately designate the relevant systems of positions only as "admissible," because their elements do not, after all, fulfill the requirement of mutual invariance perfectly, but only approximately; a purely terrestrial system of positions does not suffer from this deficiency.

Before it is raised against me, I would like myself to advance and refute an objection to the assertion made above.

According to this assertion, the motion of objects located outside a railway train as observed from within the train—for example, the rapid passing-by of telegraph poles—would also have to count as a real motion, since relative to a system of positions chosen inside the train these telegraph poles undeniably undergo a continuous change of position.

It will surely surprise most readers that, according to the propositions stated earlier, this phenomenon too must be regarded as a real motion; I already hear the words in my mind: that really is going too far to make such a claim.

My reference to the fact that all the conditions for the essence of a real motion are fulfilled—the system of positions possesses sufficiently many elements in

rigid connection, and the variable relation of the telegraph poles to the system of positions cannot be doubted—will convince the opponents less than if I demonstrate to them that even the “real motion” of the Earth about its axis, which they certainly do not doubt, leads to the same conclusion; if I compel them, on the basis of their own conviction, under certain circumstances to declare the telegraph poles rushing past the train to be really in motion.

For this purpose I imagine a railway built along the Earth’s equator and invite my opponents to undertake a journey with me upon it from east to west. We depart exactly at noon and give the train the same speed with which the Earth moves from west to east; the train then keeps the Sun continuously at the zenith, and is therefore at rest with respect to the Sun, while the Earth beneath the train moves with tremendous speed. We now again see the telegraph poles rush past the windows; this time, however, my companions will have no alternative but—on the basis of their own conviction regarding the Earth’s axial rotation—to declare: “Yes, the rushing telegraph poles are really in motion.”

Why should the interpretation of a phenomenon, which in this case must be regarded by my opponents not as illegitimate but as downright necessary, be considered impossible in other cases?

The principled difference between the observation in this case and that during an ordinary railway journey consists solely in the following: in the first case the Sun stands in invariant relation to the train and thus, as it were, forms part of the chosen system of positions, whereas in the second case (in an ordinary railway journey) it stands in variable relation to the system of positions chosen in the train and therefore does not belong to it.

The system of positions thus possesses, in the first case, a few molecules more than in the second.

That the magnitude of the mass inherent in a system of positions cannot be decisive for the decision of the question whether the observed motion is to be regarded as real or apparent, I believe I have shown clearly enough in what precedes.

A further objection, which has been raised to me in oral discussions with specialists, I shall also address immediately.

It was objected to me as follows: by adopting an extraterrestrial system of positions, certain phenomena—such as, for example, the motion of the planets—receive a simple explanation; the latter then appear to revolve in elliptical orbits around the Sun, whereas relative to a purely terrestrial system of positions they describe very complicated paths. From this one might conclude that the extraterrestrial system must indeed be the more correct one.

To this I reply that for the simplest representation of the motions observable in the various celestial bodies, not a single system of positions suffices, but rather that, depending on which phenomenon one wishes to apprehend in the simplest manner, different systems of positions must be assumed.

The conception of the motion of the planets in elliptical orbits around the Sun, the circular motion of the Moon around the Earth, and the rotating motion of any point of the Earth around the Earth’s axis cannot be developed, in their simplest intuitive form, from one and the same system of positions.

This has not generally been appreciated, because one is accustomed to speak of these motions without accounting for the system of positions to which the respective motions are referred.

Thus, for example, the motion of the planets—if one truly wishes to conceive their paths as ellipses—may by no means be referred to a system of positions formed by three points of the fixed-star sky (with exclusion of the Sun), because under this assumption, owing to the progressive motion of the Sun, the paths of the planets are cycloidal curves.

In order to be able genuinely to declare the planetary orbits to be ellipses, the system of positions must be composed of the two poles of the Sun and of a fixed star that stands in invariant relation to these.

Analogous considerations apply if one wishes to explain the path completed by the Moon within one lunar month as a circular (elliptical) orbit around the Earth. In this case as well, the Moon's path would have to be regarded as cycloidal if one were to assume a system of positions consisting of three points of the fixed-star sky (this time even including the Sun). The observed lunar orbit shows its simplest form only when one takes as its basis a system of positions consisting of the two poles of the Earth and a fixed star standing in invariant relation to these.

The same considerations apply analogously to the rotating motion of the individual points of the Earth about its axis. Relative to a completely extraterrestrial system of positions, the individual points of the Earth describe cycloidal curves. If one wishes to regard their motion as genuinely circular, the system of positions must be conceived as consisting of the two poles of the Earth and of an extraterrestrial mass-point that stands in invariant relation to the Earth's axis.

Furthermore, it should be emphasized that a motion that is ascertained as rectilinear relative to a purely terrestrial system of positions (for example, a motion proceeding along a given Earth radius) must appear as a curvilinear path when referred to an extraterrestrial system of positions. For such motions, therefore, the purely terrestrial system of positions leads to the simplest conception of them.

From this it should be sufficiently evident that different motions require different systems of positions for their simplest representation, and that it must therefore appear entirely unjustified to regard precisely that system of positions which renders the planetary orbits simplest as the only correct one, and to allow only motions referred to "this" system to count as real motions.

With the considerations presented thus far, I believe the problem of motion—so far as it can be discussed at all without taking account of the law of inertia—has been sufficiently illuminated; the influence of inertia upon the motions of masses I have for the time being deliberately avoided.

On the subject of "Inertia" I shall express myself in more detail only in the second section of this work.

Before, however, I proceed to this chapter, I would like to ask my esteemed readers to review once more the views expressed by me thus far as objectively as possible, that is, as free as possible from the conceptions that have become

habitual through previous instruction.

I therefore ask all readers to answer the following questions for themselves:

1. Is the definition: “By the motion of a material point one understands any change of its position” in any way open to objection?
2. Can anything be objected to the definition: “By the position of a point one understands its relation to a sensibly perceptible system of positions”?
3. Can the law of interchangeability of the system of positions with the body that is in motion relative to it be doubted?
4. Is it legitimate to declare a motion apparent merely because the system of positions to which it is referred is itself in motion relative to a second system of positions?
5. Can the concept of “absolute motion,” after absolute motion is “never ascertainable,” indeed not even “conceivable” in any reasonable sense, be regarded as a genuinely valid concept?

Despite my previous expositions, there will still be many readers who adhere to the distinction between real and apparent motion and therefore believe they can declare my views to be incorrect on the basis of their conviction regarding the legitimacy of the concept of “apparent motion” or that of “absolute motion.”

Such readers would then, however, if they wish to maintain their standpoint with justification, have to be in a position to give a clear account of the following points:

1. A definition of the concept of “apparent motion,” or
2. criteria for distinguishing a real from an apparent motion,
3. a definition of the concept of absolute position,
4. a definition of so-called “absolute motion,” or
5. criteria for the ascertainment of an absolute motion.

## **Inertia**

The concept of motion, insofar as it is considered independently of the influences of inertia—as was done in the preceding chapter—is essentially a purely geometrical concept and, with respect to the judgments of geometry, comparatively easy to elucidate.

It becomes substantially more difficult to bring the conclusions of geometrical kinematics into agreement with the currently prevailing views concerning the law of inertia. In the attempt to bring exact geometrical conceptions into accord with present-day inertia theory, quite definite contradictions emerge.

These contradictions first prompted me to cast doubt upon the law of inertia itself, and upon closer examination I found that, from a physical standpoint as well, it does not meet the requirements of an unassailable principle.

In what follows, an attempt shall be made to approach the concept of “inertia” more closely.

As a rule, the law of inertia is expressed in the following manner: every body exhibits the tendency to preserve unchanged the state of rest or of motion; the moving body exhibits the tendency to continue its motion in a “straight line.”

As already mentioned earlier, I initially found the formulation of this law to be in contradiction with the judgments of geometry; for from the latter it is easily seen that a motion which is ascertained as “rectilinear motion” with respect to some system of positions can appear as curvilinear motion with respect to another system of positions.

In order to demonstrate this, I shall in what follows work through two examples constructively. In the first it shall be shown that a motion which proceeds rectilinearly with respect to the Earth must appear curvilinear with respect to an extraterrestrial system; in the second example, a point shall be assumed which moves rectilinearly with respect to an extraterrestrial system and must, as a consequence, appear to be in curvilinear motion with respect to the Earth.

**Example I.** In Fig. 1 let the circle  $E$  represent the Earth’s equator, which rotates, in the sense of the arrow, with respect to a system of positions composed of the Sun  $S$  and two fixed stars  $F_1$  and  $F_2$ . Let  $P$  be a mass-point which is in rectilinear motion with respect to the Earth, namely in the direction of an Earth radius whose initial position is assumed to be  $OS$ . If the Earth were at rest relative to the system of positions  $S, F_1, F_2$ , then after a sequence of equal time intervals the point  $P$  would occupy the positions 1, 2, 3, etc. But since the Earth rotates with respect to  $S, F_1, F_2$  in the sense of the arrow, the Earth radius mentioned will assume the positions  $O1, O2, O3$ , etc., and therefore the point  $P$  will occupy, in these different positions of the Earth radius, the locations  $1', 2', 3'$ , etc. Relative to the system of positions  $S, F_1, F_2$ , the point  $P$  will therefore have described the curvilinear path  $1', 2', 3'$ , etc.; relative to a terrestrial system of positions, however, the motion of point  $P$  must decidedly be regarded as rectilinear, since throughout the entire time it remains on one and the same Earth radius.

**Example II.** Again let the circle  $E$  represent the Earth’s equator rotating in the sense of the arrow; this time, however, let it be assumed that the point  $P$  moves entirely independently of the Earth in a straight line from  $S$  toward  $O$ , and in doing so occupies, after a sequence of time intervals, the positions 4, 3, 2, 1, and finally  $O$ . During this time the Earth radii  $O4, O3, O2$ , and  $O1$  will have passed through the position  $OS$ . An observer situated on the Earth, who refers this motion to a terrestrial system of positions, will ascertain the various positions of this point at  $P4, P3, P2, P1$ , and  $PO$ , and will necessarily have to declare the motion to be curvilinear.

I must emphasize here, in order not to be misunderstood, that I do not present these two examples for the purpose of once again raising the question of whether the line of motion observed from the extraterrestrial system or that ascertained from the standpoint of the terrestrial system represents the “real” path of point  $P$ . In this regard I believe I have already shown clearly enough in the preceding discussion that both interpretations are equally justified.

The purpose of presenting these two examples was solely to show that a motion which is ascertained as rectilinear with respect to one system must appear as curvilinear with respect to another system, indeed must do so if the one system is in curvilinear motion with respect to the other.

Now the universe most certainly contains infinitely many systems of positions that are mutually in curvilinear motion with respect to one another.

Any given motion can therefore, according to the foregoing considerations, be regarded as rectilinear at most with respect to one of these systems; relative to all others it must, with compelling necessity, appear as curvilinear.

And now I return once again to the law of inertia itself.

Let us imagine a moving point which, deprived of any other influences, is to follow solely its inertia. The point would then, if it were to obey the law of inertia as it has hitherto been regarded as valid, find itself in some embarrassment. With respect to which of the infinitely many systems of positions is it now supposed to continue to move rectilinearly?

If it attempts to move in a straight line with respect to the Earth, then it describes curvilinear paths with respect to all extraterrestrial masses; but if it assumes rectilinear motion with respect to some extraterrestrial system (the fixed-star sky contains very many such systems), then it moves not only with respect to the Earth but also with respect to all other extraterrestrial systems along curvilinear paths.

The previously cited formulation of the law of inertia must therefore certainly be regarded as inadequate; it would have to specify unconditionally the system of positions with respect to which the moving body, by virtue of its inertia, strives to preserve “rectilinear” motion. By failing to do this, it commits a serious incompleteness.

It therefore appears decidedly inadmissible to assume rectilinear inertia without distinction as to the system of positions to which it refers.

One will now object to me that this rectilinear inertia has nevertheless been confirmed by countless examples.

This is by no means the case. The motions of extraterrestrial masses all proceed along curvilinear paths and therefore cannot furnish an example for the stated law of inertia; indeed, precisely because they do not conform to this law, they induced Newton to assume a deflecting force.

From the motions of celestial bodies we therefore by no means can derive with certainty rectilinear inertia, since in order to bring this theory into agreement with fact a second assumption, adapted to the first—namely that of mass attraction—must be made.

The situation is then such that one theory is supported by the other and conversely; but in that case neither the one nor the other is proven.

Even less tenable are those examples that are drawn from observations of terrestrial masses. The rectilinear path of a ball rolling on a horizontal plane is not a conclusive example, because the path is a constrained one owing to the presence of the material plane, and we therefore cannot know to what extent, in free fall, inertia contributes to the production of the parabolic path. The agreement of the theoretically calculated path—composed of rectilinear inertia and attraction—with the actually described path cannot be regarded as a confirmation of the present law of inertia, since Newton’s law of attraction itself arose only from the “assumption” of rectilinear inertia.

But even if one were to suppose that experiments conducted on Earth actually led to the conclusion of rectilinear inertia with respect to a terrestrial system of positions, this would then be all the more proof that rectilinear inertia must not be assumed for observations with respect to an extraterrestrial system of positions, since geometry teaches with compelling necessity: motions that proceed rectilinearly with respect to the Earth must appear as curvilinear paths with respect to an extraterrestrial system of positions.

It might, after these considerations, occur to a defender of the concept of “absolute motion” to advance the assertion: the system of positions within which a body manifests its inertia in the striving for rectilinear motion is precisely the “absolute” one.

But even thereby the “in itself impossible” concept of “absolute motion” would not be rescued.

In the first place, considerable difficulties would stand in the way of identifying such a system, since under the presupposition of universally acting attractive forces of masses there exists no example in which one could observe a mass-point subject solely to inertia.

But even if such a system were found, all motions ascertained with respect to it would still once again be “relative” motions.

I would like to emphasize at this point that hitherto one has generally not become clearly aware of the gap in the currently accepted law of inertia that has been illuminated above; rather, rectilinear inertia has been assumed for all motions, regardless of the system of positions to which they are referred—wherein, however, there lies a very definite error.

To the objection that, in view of the relatively small distances which we usually subject to observation, only infinitely small, scarcely measurable inaccuracies could thereby arise, I readily concede this insofar as it concerns motions of terrestrial masses referred to a terrestrial system of positions.

It appears to me questionable, however, to extend this same tolerance also to the motions of extraterrestrial masses, which we do not observe merely for short times and small distances.

In no case, however, may I allow myself to be influenced here by the smallness of any possible inaccuracies, since what is at issue is not greater or lesser inaccuracies, but rather whether the concept of “inertia” is securely defined or not.

After I had gained the conviction that the presently accepted law of inertia does not conform to the incontrovertible conclusions of geometrical kinematics, I

was prompted to subject this law also to a closer examination from the physical point of view.

From this standpoint as well, I believe I must raise decisive objections against it.

I cannot accord to the previous concept of inertia—which is conceived as “absolute,” because it is defined without any reference whatsoever to any body other than the one under consideration—this character of absoluteness. Rather, I am of the view that the concept of inertia, just like that of motion, must be conceived exclusively as “relative.”

A body can be in a state of rest or motion only with respect to another body. If we now state that a body has the tendency to preserve its state, this can be understood in no other way than that it wishes to remain in the same state of rest or motion *with respect to that same other body*. Thus, in the inertia of a body there lies a relation of that body to the one with respect to which the state of rest or motion was ascertained.

In order to explain my ideas in this regard by means of an example, I imagine in infinite space nothing other than two material points *A* and *B* that are continually moving away from one another. (It is evident that for these two points the law of inertia can be understood in no other way than that both strive to maintain their mutual motion.)

I must therefore be able to assert: the inertia of *A* consists in continually moving farther away from *B*, and conversely, *B* strives to move away from *A*.

By this conception, the inertia of each of the two points is revealed as a relation to the other.

If I now imagine several material points *B*, *C*, etc., then, for example, *A* will wish to follow the inertial tendency with respect to each of the other points; the behavior of point *A* must then result as the *resultant* of the individual inertial tendencies.

This consideration may of course also be transferred from material points to whole groups of material points, that is, to bodies. I shall therefore permit myself to state the following proposition:

Every body is subject, with respect to all other bodies present in space, to the law of the preservation of the mutual state of motion (or rest); its actual behavior is then the resultant of all the individual influences.

To what extent, however, this resultant depends upon the mutual distances, upon the magnitudes of the various bodies, and upon their mutual grouping can scarcely be investigated by speculation, but only on the basis of experience (perhaps through experiments arranged specifically for this purpose). But if one adopts the view that the inertia of a mass must be conceived relationally, then one must all the more abandon rectilinear inertia, since the path of a body would then depend upon so many variable conditions.

I wish to emphasize once again that, in my view, inertial effects are relations of masses to one another, that they therefore occur reciprocally, and that—much

like the mutual motions of masses—they bear the character of reciprocity; that is, body  $A$  manifests its inertia with respect to  $B$  in the same manner as  $B$  does with respect to  $A$ .

A characteristic difference between the ascertainment of the mutual behavior of bodies with respect to motion and with respect to inertia consists, however, in the fact that when ascertaining the relative motion of a point one may refer it only to *one* arbitrarily chosen, freely selectable system of positions, whereas in observing the inertia of a body one must always take into account the simultaneous influence of *all* masses present in space outside the body.

It is an extraordinarily wide—but also difficult—field to investigate the laws according to which masses exert their mutual inertial tendencies. The greatest difficulty lies in the fact that in all experiments that might be undertaken one must never make the observed mass dependent upon a single opposing mass, but always upon the totality of all existing masses.

It does not occur to me to wish to develop here a complete theoretical edifice of inertia; this cannot be the work of an individual within a few weeks. What is intended here is merely to draw attention to the inadequacy of the law of inertia as it is presently regarded as valid, and at the same time to provide a stimulus indicating in what direction investigations toward a better grounding of the laws of inertia might be undertaken.

Nevertheless, some basic principles can already be established even now.

For this purpose, I return once more to the example in which I assumed in infinite space nothing other than two masses that are in mutual motion and, by virtue of inertia, strive to maintain this motion. If I choose mass  $A$  as the system of positions, then  $B$  must exhibit inertia with respect to mass  $A$ ; conversely, if the system of positions is chosen on  $B$ , inertia must become manifest at mass  $A$ .

The mutual configuration of the two, however, must at every moment be the same in both cases, regardless of which system of positions is taken as the basis of observation, since the behavior of the two masses cannot depend upon the interpretation that an observer assigns to the phenomenon.

The reciprocity of the inertial effects of two masses upon one another may also be illuminated from another standpoint.

Every moving body, by virtue of its inertia, has the capacity to perform work; the magnitude of this capacity we call its “kinetic energy.”

The kinetic energy of a moving mass is therefore an inertial phenomenon, and it is therefore natural to judge the law of inertia also from this side.

For this purpose, let me imagine in space nothing other than two unequal masses  $M$  and  $m$  that are in a state of mutual approach, such that their distance diminishes by the length  $v$  per unit time.

If I choose the system of positions on  $M$ , then  $m$  is the moving mass and can therefore manifest a definite kinetic energy in the system of  $M$ , that is, perform work of a definite magnitude.

If, however, I reverse the interpretation and choose  $m$  as the system of positions, then mass  $M$  possesses kinetic energy with respect to  $m$ .

It is now of great interest to pose the question: in what relation do these two kinetic energies stand to one another, which are produced by different masses at equal velocities (for in both cases the velocity is equal to the distance  $v$ )?

According to the familiar formula  $E = 12mv^2$ , one would have to decide: the larger mass, since the velocity is unchanged by the interchange of the system of positions, also produces the greater kinetic energy.

And yet this is not the case, as the following consideration will show.

Let us assume that the two masses  $M$  and  $m$  eventually come into collision as a result of their mutual approach, so that the capacity for work of both masses can actually become manifest. Let us imagine at the place of collision an apparatus that consumes the existing kinetic energies and at the same time registers them—for example, an elastic spring that is compressed by the two masses and fixed in this state; the apparatus then directly indicates, for instance in the achieved tension of the spring, the measure of the work performed.

If we now first let mass  $M$  serve as the system of positions, then the energy stored in the spring is the work performed by  $m$  in the system of  $M$ .

In the other case, however—namely when one regards  $m$  as the system of positions and  $M$  as the moving mass—the same energy of the compressed spring must be regarded as the measure of the work performed by  $M$  in the system of  $m$ .

Thus, the kinetic energies produced mutually by the unequal masses  $M$  and  $m$  are equal.

From this follows the correct proposition:

If two material systems  $M$  and  $m$  are in mutual motion, then the kinetic energy of  $M$  with respect to  $m$  is equal to that of  $m$  with respect to  $M$ .

Since the validity of this law is independent of the magnitudes of the two masses  $M$  and  $m$ , it must remain valid even when, for example,  $M \rightarrow \infty$ ; that is, the kinetic energy which an arbitrary body can exert through its motion with respect to all masses external to it is equal to that which these masses can exert with respect to the body when the latter is taken as the system of positions.

From all that precedes it follows that all inertial phenomena are to be sought in mutual relations of masses to one another, so that the effects produced by inertia are independent of which mass is taken as the system of positions.

I wish to designate this law by the name “*Reciprocity of inertia*.”

The equation  $E = 12mv^2$  appears to stand in contradiction to these considerations; this contradiction, however, is resolved by the following reflection.

The inertial effect of a mass  $M$  with respect to another mass  $m$  is a function of both masses; the equation for the kinetic energy that they mutually manifest must therefore contain both masses.

Let us call the kinetic energy that two units of mass are able to exert upon one another at a mutual velocity of one unit of length  $k$ ; then the kinetic energy between the two masses  $M$  and  $m$  at a mutual velocity  $v$  is expressed by the equation

$$E = k M m v^2,$$

where I have assumed the dependence on the velocity to be proportional to the square of  $v$  as established.

Moreover, in setting up this equation no account has been taken of a possible—indeed perhaps even probable—influence of the distance  $r$  between the two masses. Should such an influence be demonstrable by experiment, then the equation would take the form

$$E = k M m f(r) v^2.$$

If I now consider the case in which different masses  $m_1, m_2, \dots$  are always opposed to the same mass  $M$ , then in the equation

$$E = k M m v^2$$

the product  $kM$  is constant and represents the kinetic energy of the mass  $m = 1$  with respect to the constant mass  $M$  at a relative velocity  $v = 1$ .

For the actually existing conditions it is self-evident that the inertia of any mass  $m$  must always be referred to the totality of all other masses, which may be regarded as constant.

It is known that a mass of one gram, at a velocity of one centimeter per second relative to the total mass, develops a kinetic energy of 12. Hence the product  $kM$  contained in the earlier formula amounts for us to 12, and from this results the special equation

$$E = 12mv^2.$$

Whereas in the general equation

$$E = k M m v^2$$

the character of reciprocity between  $M$  and  $m$  is expressed, in the special equation

$$E = 12mv^2$$

the influence of the mass  $M$  is contained in the constant factor 12, whereby the expression for kinetic energy now appears to be independent of the mass  $M$ .

That this, however, is only an appearance is sufficiently clear from the preceding considerations.

Particularly interesting are the conclusions that may be drawn from this principle for the phenomenon of centrifugal force in rotating masses.

Let us imagine some rotating body  $K$ ; we must regard the experimentally verified phenomenon of centrifugal force manifested by it as an inertial relation of the rotating mass to all masses external to it that do not participate in the rotation.

According to the principle developed above, the same inertial tendencies must then arise if I take the body  $K$  as the system of positions and imagine all other masses rotating about it.

In this case as well, the individual mass points of the body  $K$  must develop exactly the same centrifugal force as before; only now one would have to draw

the further conclusion that mass points external to the body  $K$  should also exhibit centrifugal phenomena—phenomena that, however, have not hitherto been observed.

This circumstance appears at first glance to stand in contradiction to the principle previously stated; nevertheless, I maintain this principle, and indeed with the following justification.

An arbitrary mass point of the body  $K$  during its rotation is obliged, by inertia, both to the body  $K$  itself and to all masses external to it: with respect to the former it must preserve its position, while with respect to the latter it must manifest inertia in a continuation of motion—whether in a straight or in some curved path we do not know.

From these two tendencies there then results the resultant that we can experimentally ascertain and from which we derive the concept of centrifugal force; in doing so, however, one must not forget that the first influence is attributable to a comparatively small mass, the second to an infinitely large mass.

If we now reverse the interpretation and take the system of positions to be fixed on the body  $K$  itself, so that the entire remaining mass appears to rotate about  $K$ , then owing to the reciprocal inertial effects of the masses we must still expect centrifugal phenomena to occur at  $K$ ; but it is now natural also to expect centrifugal phenomena in the other masses rotating about  $K$ .

We must, however, analogously to before, take into account that the individual mass points of the latter must also obey two inertial obligations: with respect to the body  $K$  they should manifest centrifugal force, whereas with respect to all other infinitely large masses they must strive to preserve rest.

The resultant must now clearly favor the latter, which is why we are unable to observe any measurable centrifugal phenomena in those mass points.

Newton himself had certainly undertaken similar considerations, as follows from his experiment with the rotating vessel; only because he conceived inertia as “absolute” did he fail to include in his reckoning the ratio of the masses that were to impart centrifugal force to the masses inside the vessel to the masses external to the vessel, and he simply rendered the judgment:

“Relative rotations do not produce centrifugal force.”

It will perhaps not be entirely superfluous to recount the experiment performed by Newton.

Newton took a vessel filled with water, suspended it by a cord, and then repeatedly twisted the vessel about its vertical axis, thereby strongly torsioning the suspension cord; when he then left the vessel to the action of the torsional force of the cord, it entered into rotational motion, which at first affected only the rigid mass of the vessel, while the liquid was only gradually set into rotation through friction with the vessel walls.

The observed phenomenon was the following: at first the water exhibited no centrifugal phenomena; only to the extent that it participated in the rotation did such phenomena appear, in the elevation of the water level at the walls.

From this Newton drew the conclusion:

“As long as only the vessel moves, the water is in relative motion with respect to the mass of the vessel; and since no centrifugal phenomena appear during this time, it is thereby proven that relative rotations produce no centrifugal force.”

After this account it should now be clear that Newton, through his conception of inertia as “absolute,” failed to take into consideration the following: as long as the liquid is only in relative motion with respect to the vessel, only a possible centrifugal phenomenon arising from the inertia of the water mass with respect to the comparatively negligible mass of the vessel is to be expected, whereas the invariance of the position of the liquid particles is to be regarded as the effect of inertia with respect to the infinitely large remaining (external) mass.

The experiment was evidently inadequate and by no means justified the conclusion:

“Relative motions produce no centrifugal force.”

The decision of the question whether centrifugal force is developed in relative rotations or not would have to be sought in an experiment in which the mass that stands opposite the mass in relative rotation constitutes a system of positions containing as much mass as possible.

For example: around a vessel filled with water a very heavy ring could be set into the most rapid possible rotation; if one then chooses the system of positions in the mass of this ring, the liquid would be in relative rotation with respect to this system of positions; perhaps, with sufficiently large mass of the ring and sufficiently rapid rotation, the occurrence of a centrifugal phenomenon in the liquid could be ascertained, thereby demonstrating “relative” inertia.

A negative outcome of such an experiment would nevertheless still not justify the conclusion that relative rotations produce no centrifugal phenomena, because even then the mass of the ring would be infinitely small in comparison with all remaining masses.

Much more suitable than this water-vessel experiment for such investigations, however, would be the Foucault pendulum; and to this refers the statement made in my article published in the *Neues Wiener Tagblatt*:

The Foucault pendulum can never be regarded as proof of the apparent character of the daily motion of the Sun and stars; rather, it can serve to provide important insights into the law of inertia, which has hitherto not been sufficiently understood.

An experiment to be carried out for this purpose would have to be arranged in such a way that beneath a freely oscillating (Foucault) pendulum a mass as large as possible could be set into very rapid rotation. Should it then be found that the rotation of the plane of oscillation of the pendulum is altered by the rotating mass (naturally with air currents carefully excluded), then the “relative” inertia—of which I am in fact fully convinced even without such an experiment—would thereby be experimentally demonstrated.

The advantage of this experiment over that with the vessel of water, as conducted by Newton, lies in the fact that in the latter case, for the anticipated elevation of the water level at the vessel walls, the weight of the water must be overcome, whereas in a pendulum experiment only the inertia of the plane of oscillation of the pendulum with respect to the extraterrestrial masses would have to be overcome.

Perhaps, through this suggestion, a body or an institute may be found willing to carry out such an experiment; I would very much wish this to be the case.

I must note here that precisely the experiment conducted by Newton with the rotating vessel—whose inadequacy I believe I have sufficiently emphasized—was taken as corroboration of the concept of so-called “absolute motion,” and indeed on the basis of the following reasoning: result of the experiment—relative rotations produce no centrifugal force; but every body in rotation exhibits the phenomenon of centrifugal force; we cannot attribute this to relative motion, therefore the cause must be sought in a motion simultaneously carried out by the body with respect to an unknown (absolute) system of positions.

From this it may be seen that the in itself untenable concept of absolute motion finds no support even in this line of reasoning, since it is built upon an inadequate observation.

With the discussions presented here, the field of “inertia” is by no means exhaustively illuminated; rather, they are intended only as an encouragement to devote greater attention to this law. At the same time, I wish to point out that with altered conceptions of the law of inertia, alterations in the views concerning the concept of “force” will also likely take place.

## The Axial Rotation of the Earth

The daily rising and setting of the Sun, or respectively of the stars, is a phenomenon that has found different interpretations at different times.

Today, the only correct conception is generally held to be that this phenomenon can be explained only by the axial rotation of the Earth, while the older view—that the Sun, Moon, and stars revolve around the Earth—is regarded as erroneous.

I believe, however, that both conceptions can be acknowledged as correct, and I shall attempt to show that the two interpretations by no means stand in contradiction to one another, but are fully equivalent.

In order to demonstrate this, I shall proceed from the presently accepted view that the Earth rotates about its axis.

It is perfectly clear that this rotating motion of the Earth must then be conceived as a “relative” motion of the Earth, referred to an extraterrestrial system of positions.

Now I believe that in the first section of this work I have sufficiently clarified the law of reciprocity of every relative motion. In accordance with this law, it must under all circumstances be permissible to interchange the system of

positions with the moving body in such a way that the latter is conceived as the system of positions and the mass of the former as the moving mass.

On this occasion I have also justified the fact that the differing magnitudes of the masses do not impair the validity of this reciprocal relation.

I was therefore compelled, on the basis of the insights gained into the geometrical laws of motion, to say to myself:

The former view—that the Sun, Moon, and stars revolve around the Earth—is, if one starts from the axial rotation of the Earth, nothing other than a not merely permissible but actually self-evident consequence of this law of reciprocity; just as, conversely, for one who proceeds from the conception of the daily revolution of the celestial bodies around the Earth, the assumption of the Earth’s axial rotation cannot appear as a contradiction to that conception, but only as a permissible interchange between moving mass and system of positions.

Had one been fully clear, at the times when this question gave rise to passionate disputes, about the concept of relative motion and the law of reciprocity that is logically necessarily connected with it, then many a controversy would surely have been avoided.

Nevertheless, even then a doubt would have arisen regarding the equal legitimacy of both conceptions, namely at the point of reconciling both views with the meanwhile established law of inertia.

At least this was my own experience; on the basis of the purely geometrical laws of motion I had arrived at the principle of reciprocity mentioned earlier, but I must confess that it caused me some difficulty when I perceived that this principle stood in contradiction to the presently prevailing law of inertia and the centrifugal phenomena derived from it.

I asked myself: how is it possible, under the assumption of a purely terrestrial system of positions (the Earth thus conceived as a body at rest), to reconcile the phenomenon of the Earth’s flattening, as well as the proven and centrifugally explained weaker effect of gravity at the equator compared with other geographical latitudes, with this assumption? And further: what colossal centrifugal forces would have to develop at the Sun and the other celestial bodies if, under the assumption of a terrestrial system of positions, the entire universe were to revolve about the Earth with tremendous speed?

I therefore once again examined for myself the concept of motion as defined in the first chapter, as well as the reciprocity law derived from it, but I could find not the slightest flaw either in the former definition or in the latter law.

I was thus compelled to draw the conclusion that the law of inertia as presently accepted, in its current formulation, as well as the laws of centrifugal phenomena connected with it, must be deficient—without at that time yet knowing in what these deficiencies actually consisted.

I therefore subjected for myself the concept of inertia, or rather the current conception of it, to a sharp critique.

In the course of this critique, it first struck me that the assumption that a moving body tends to continue its motion in a “straight line” is untenable, since a motion that is rectilinear with respect to some system of positions must

appear as curvilinear when referred to another system of positions that is itself in rotational motion with respect to the first.

Now, however, the universe contains infinitely many systems of positions which—even if often imperceptibly to us—are in mutual (rotational) motion with respect to one another.

If, therefore, the law of rectilinear inertia were to lay claim to validity, it would at the very least have to specify the system of positions to which this rectilinear inertia is to be referred.

The formulation of the law of inertia that is customary today, however, fails to specify such a system of positions; perhaps the fixed-star sky is tacitly assumed to be such a system.

But even against this one would have to object that the fixed-star sky cannot constitute a single system of positions, but rather contains innumerable systems of positions standing in mutually variable relations to one another.

With the exposure of this deficiency of the law of inertia, however, I was still by no means satisfied, because it did not yet provide an explanation of the contradiction between the reciprocity of relative motions and centrifugal phenomena.

Through further reflection it then became clear to me that it is also inadmissible, from the physical standpoint, to conceive inertia as an “absolute” property.

For the reasons set forth in the second chapter, I was compelled to arrive at the conviction that the inertia of a mass consists in its relation to all other masses external to it, from which then followed the views developed there concerning inertia and centrifugal phenomena.

Let us apply these views to the relative motion of the Earth with respect to extraterrestrial masses and attempt, on their basis, to form a judgment concerning the inertial phenomena that would have to arise if the interpretation were reversed (the Earth conceived as the system of positions and the extraterrestrial mass as moving).

The eastward deviation of a body falling from greater height from the vertical, as well as the centrifugal influences upon terrestrial gravity, must then—if one once admits relative inertia and, consequently, its reciprocity—occur in exactly the same manner as they do under the assumption of a rotating Earth and a fixed stellar sky.

In order to meet the objection that, under the assumption of the Sun and stars rotating around the Earth, these bodies would nevertheless have to continually move away from the Earth as a consequence of the developed centrifugal forces, I remark the following.

Any one of the many celestial bodies would, under this assumption, be in rotational motion with respect to the Earth, but in invariant relation with respect to all other celestial bodies. This celestial body would therefore, by virtue of inertia, have to manifest centrifugal force with respect to the Earth, while with respect to the remaining mass of the universe it would strive to preserve its position. The former influence would have to be regarded as vanishingly small in comparison with the infinitely large extraterrestrial mass.

Nevertheless, one could not deny this first influence entirely, and in the infinite course of time one would have to expect a noticeable effect through the integration of the infinitely small centrifugal effects.

If, however, we imagine two celestial bodies lying diametrically opposite one another with respect to the Earth, then the situation presents itself as follows: both bodies would, owing to their rotation about the Earth, strive to move away from the Earth and thus also from one another by virtue of the centrifugal force thereby developed. Opposed to this tendency, however, stand both the attraction exerted by the Earth upon the two bodies and the mutual attraction between the two bodies themselves. If neither an approach nor a separation of these bodies from one another occurs, then we must draw the necessary conclusion that the individual tendencies cancel one another out.

I shall now conclude this chapter, as before, by summarizing its content in concise form.

The Earth and the fixed-star sky stand in the relation of mutual rotation. The present conception assumes the fixed-star sky to be at rest and the Earth to be rotating about its axis. Purely geometrical kinematics not only permits an interchange between system of positions and moving mass, but indeed presents it as something entirely self-evident. The objections raised from the standpoint of the law of inertia against such an interchange are, I believe, removed in a completely natural manner if one abandons the law of inertia as presently regarded as correct and instead subscribes to the law of “relative inertia.”

## The Foucault Pendulum Experiment

The factual phenomenon of this experiment consists in the circumstance that the position of the plane of oscillation of a pendulum suspended so as to be rotatable changes continuously with respect to the Earth.

Experiments at various geographical latitudes show different deflections of the plane of oscillation, which led to the assumption that a pendulum swinging at the pole will show, relative to the Earth, a full revolution of its plane of oscillation in one day. This assumption is confirmed by the fact that the rotation of the plane of oscillation at an arbitrary geographical latitude, inferred theoretically from it, corresponds with fairly good accuracy to the actually observed deflections of the plane of oscillation.

This phenomenon is interpreted on the basis of prior experiments which showed that, despite rotations performed at the suspension apparatus, the position of the pendulum’s plane of oscillation remained unchanged.

From this, the conclusion is drawn that the position of the plane of oscillation of a pendulum suspended so as to be rotatable, insofar as external circumstances do not forcibly change it, is to be regarded as an “absolutely fixed” one, so that any change that occurs in the mutual position between this plane of oscillation and its surroundings must necessarily be conceived as a motion of the latter.

For the pendulum swinging at the pole, the practical application of this conclusion is then made: a forced change of the plane of oscillation, as it occurs

at the equator or even at arbitrary geographical latitudes, cannot be assumed at the pole; therefore the mutual change of position between the plane of oscillation and the Earth must *necessarily* be regarded as a rotational motion of the Earth.

This motivation appears so clear that one can indeed easily allow oneself to be taken in by it.

Nevertheless, it is not only not free of objection, but on sharper criticism it even shows quite considerable defects.

It is especially to be emphasized in this regard that all the experiments preceding the Foucault experiment, from which one drew the conclusion “the plane of oscillation of the pendulum does not change its position without external influences,” were, properly speaking, themselves already Foucault pendulum experiments, since they too always presupposed a pendulum suspended so as to be rotatable; only one did not take the time for a thorough observation, or did not have the opportunity to observe the pendulum over a longer duration, for otherwise one would already have had to observe, *before* Foucault, the phenomenon of the deflection of the plane of oscillation.

I can therefore claim: all experiments carried out before Foucault and also perhaps later, from which one wished to infer the invariability of the plane of oscillation of a pendulum, are themselves Foucault experiments; only they were either defectively carried out or defectively observed. Nevertheless, one uses the insights obtained from them—insights that are naturally only incomplete—to interpret the complete experiment.

That is surely not logical.

The other imperfections of these experiments and of the conclusions drawn from them should not remain unmentioned for that reason.

If, on the basis of only a very brief observation of the pendulum, one issued the judgment that the pendulum plane preserves its position unchanged in spite of the rotation of the suspension apparatus, then this statement initially contains an imperfection in that it does not expressly state whether this invariability is referred to a terrestrial or to an extraterrestrial system of position. It is certainly to be assumed with determinateness that in all these experiments the position of the pendulum plane was brought into relation with its immediate surroundings, that is, with a terrestrial system of position; nevertheless, one applies the judgment obtained in this way, in the interpretation of the complete experiment, to an extraterrestrial system.

A further imperfection of the experiments preceding the Foucault experiment and used for its interpretation consists in the fact that one contented oneself with rotating the suspension apparatus without considering whether the *magnitude of the mass* that is rotated around the pendulum could not have an influence on the phenomenon.

In this neglect there lies the same imperfection that I already emphasized in a previous chapter with respect to Newton’s experiment with the rotating vessel.

From all this it may be seen that the experiments used to interpret the Foucault pendulum experiment—experiments which were supposed to demonstrate the invariability of the plane of oscillation of a rotatably suspended

pendulum—were incompletely conducted and incompletely observed (Foucault) experiments, and that it is therefore not permissible to interpret a completely conducted experiment by means of the results of a principally identical but incomplete experiment.

If, therefore, we imagine that the pendulum experiment has been carried out at the pole and that in fact a complete revolution of the plane of oscillation has been observed over the course of one day, then for the explanation of the phenomenon we find in the preceding experiments no support whatsoever; the phenomenon must therefore be interpreted from itself.

First, one must then ascertain that the plane of oscillation of the pendulum is in motion in comparison with a terrestrial system of position, but stands in an invariant relation to an extraterrestrial system of position.

But there is no more a compelling reason to choose precisely the latter system of position than there is in judging the daily observable phenomenon of the rising and setting of the heavenly bodies; here too, with the same right, I must be able to say: the pendulum plane rotates with respect to the Earth, or the Earth rotates with respect to the pendulum plane. Thus the experiment can certainly bring no decision as to whether the Earth rotates about its axis, or the pendulum plane and with it the fixed-star sky rotates about the Earth.

Nevertheless, the experiment is an extraordinarily stimulating one; only, its significance must be sought in something other than the impossible decision of the questions mentioned above.

For the question presses itself at once: why does the plane of oscillation maintain its position precisely with respect to the fixed-star sky and change it in comparison with the Earth?

The view I set forth in a previous chapter—that inertia is a relation of masses to one another—provides a quite unforced explanation of this.

Perhaps it will be good if I repeat here once again the motivation of this view of mine. It runs as follows: every observed motion or rest of a mass is a relation of it to a second mass (the mass of the system of position); the inertia of the mass first mentioned consists in the striving to maintain the state of motion or rest, that is, to remain in the original relation to all masses outside it. In this there is quite clearly expressed that inertia is a relation of masses to one another, and that the behavior of a body must be regarded as the resultant of all inertial obligations to which a body is subject with respect to all other masses.

If we apply this view to the pendulum swinging at the pole, then we must conclude: the pendulum must at the same time manifest inertia with respect to the Earth, the Sun, and all other heavenly bodies; the predominant influence is to be ascribed to the infinitely great mass of the fixed-star sky, while the inertial influence of the Sun and that of the Earth must be conceived as subordinate. Still, however, it is to be assumed that the mutual distances of the masses also exert an influence and therefore that the Sun and the Earth, by reason of their smaller distance from the swinging pendulum, cannot remain entirely without effect.

The various inertial influences to which the pendulum is subject would there-

fore have to manifest themselves individually in such a way that the pendulum, if opposed solely to the influence of the Earth's mass, should show an invariant position with respect to the Earth; the influence of the Sun's mass would, however, have to bring about the single revolution of the plane of oscillation within a *solar* day, while, as a consequence of the inertial effects of the remaining fixed-star sky, the complete rotation of the pendulum plane would have to take place already within a *sidereal* day.

Under these circumstances it is naturally to be assumed that, on the one hand, the Sun, and on the other hand, the Earth, must exert a retarding effect upon the rotation of the pendulum plane. One would therefore expect that for the complete revolution of the pendulum plane a time is required that is somewhat greater than that of a sidereal day.

It would therefore surely be highly interesting to observe the Foucault pendulum experiment from this standpoint, in order thereby to learn, by experimental means, the influence of the Earth, the Sun, and the other masses upon the pendulum.

Indeed more than that: the thought even suggests itself of wishing to learn the influence of smaller terrestrial masses upon the freely swinging pendulum.

For this purpose, beneath a Foucault pendulum one would have to set a mass as large as possible into as rapid a rotation as possible; the rapid rotation could then, to a certain degree, paralyze the predominant influence of the mass of the Earth and of the remaining heavenly bodies.

If it were possible, in this way, to alter the speed of rotation of the plane of oscillation of the Foucault pendulum, then not only would a proof of the *relative* "inertia" thereby be provided, but a means would also be found to determine experimentally to what extent these inertial effects depend on the magnitude of the masses, on their mutual distances, and on the velocities of the motions.