

On Absolute and Relative Motion

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The most apt discussions of the physical significance of the law of inertia and of the concept of absolute motion connected with it are due to Mach. According to him, in mechanics as well — just as already in geometry — the assumption of an absolute space, and hence of an absolute motion in the proper sense, is inadmissible. Every motion is intelligible only as a relative one, and what is commonly called absolute motion is merely motion relative to a reference system, a so-called inertial system, which is required by the law of inertia and which receives its orientation in some lawful manner from the masses of the world system.

Most authors today agree in essence with this view, in particular among the more recent ones Voss¹ and Poincaré². Boltzmann³, however, adopts a different standpoint, believing that an absolute space and thus an absolute motion cannot be denied outright. In what follows, however, I shall proceed from Mach's view and attempt to connect some further considerations with it.

Mach summarizes his reflections in the statement:⁴ “The natural standpoint for the investigator of nature remains to regard the law of inertia at first as a sufficient approximation, to relate it spatially to the fixed-star sky and temporally to the rotation of the Earth, and to expect the correction or sharpening of our knowledge from an extended experience.”

Now it does not seem entirely impossible to me that such an extended experience may now be available. In a recently published paper by K. R. Koch⁵ on the temporal change in the magnitude of gravity it is stated: “Accordingly, the assumption of a real change in gravity — or more precisely, in its difference between Stuttgart and Karlsruhe — appears to me to be required.” One must of course wait to see whether this claim is confirmed by further examination; but at the very least one must today reckon with the very close possibility that it is correct.

¹A. Voss, Die Prinzipien der rationalen Mechanik. Enzyklop. d. math. Wissensch., Band IY, 1, S. 39, 1901.

²H. Poincaré, Wissenschaft und Hypothese. Deutsch von P. und L. Lindemann, Leipzig 1904.

³L. Boltzmann, Prinzipien der Mechanik, II, S. 380, Leipzig 1904.

⁴E. Mach, Mechanik, 4. Aufl., S. 252, Leipzig 1901.

⁵K. R. Koch, Drude's Annalen der Physik, Band 15, S. 146, 1904.

An explanation of such a phenomenon, if it is real, on the basis of known causes would be very difficult. This circumstance encourages me to come forward now with a line of thought that I had already pursued earlier, and which had long since led me to the assumption that small periodic changes of gravity of measurable magnitude should be regarded as possible.

Experience first teaches that the inertial system required by the law of inertia can be fixed spatially with practically sufficient accuracy relative to the fixed-star sky. One is, of course, also free to use another reference system for the description of motions — for example, one fixed relative to the Earth. But in that case the Coriolis correction forces of relative motion must be applied at every material point in order to predict motions correctly. One may therefore say that the inertial system is distinguished from every other reference system by the fact that, for it, one can dispense with the introduction of those correction forces. A rectilinear, uniform translation of the chosen reference system may here be neglected as immaterial.

It is evident, however, that the fixation of the inertial system relative to the fixed-star sky cannot be regarded as accidental. Rather, it must be attributed to the influences — somehow coming into effect — of the masses of which the fixed-star sky is composed. One may therefore raise the question according to what law the orientation of the inertial system is regulated, if the instantaneous configuration and relative motion of the entire mass system — that is, the magnitudes of the individual masses, their distances from one another, and their time derivatives — are regarded as given.

The logical need for posing such a question, if one wishes to avoid the assumption of an absolute space, was also felt by Boltzmann, in that he incidentally points to the possibility⁶ that the three principal axes of inertia of the entire world system might supply the required orientation. This plausible conjecture would indeed overcome the conceptual difficulties, if it could be maintained. I do not believe, however, that it is admissible. For one may imagine a world system otherwise arranged like ours, with the sole difference that no forces at all act between the individual world bodies. Then all world bodies would describe rectilinear paths relative to the inertial system valid for this world system. But a calculation that is easy to carry out shows that, under this assumption, the principal axes of inertia of the whole system would in general execute rotations relative to the inertial system. One must therefore look for another condition by which the fixation of the inertial system can be rendered intelligible.

If initially all world bodies were at rest relative to one another, except for a single material point which I imagine using to test the law of inertia and which I shall call the “test point,” then on the basis of the experiences already available there could be no doubt that the test point, if no forces acted on it, would describe a rectilinear path relative to a reference system firmly connected with the masses. In this case, the inertial system would therefore be spatially fixed immediately.

Now imagine further the case in which the world bodies consist of two groups:

⁶A. a. O. S. 333.

an “overwhelmingly dominant” group and a smaller group, such that the masses belonging to each group do not change their mutual distances, while the smaller group, considered as a whole, executes some motion — say a rotation — relative to the larger group at a given time. If only one of the two groups existed by itself, the inertial system would be fixed relative to it. Since both act together, and since one group is assumed to be far more “dominant” than the other, the inertial system will now still remain almost at rest relative to the first group, but will nevertheless execute a small motion relative to it, arising precisely from the influence of the second, smaller group.

How should one proceed most appropriately in such a case? I believe there can be no doubt. One would fix the reference system exclusively relative to the first, overwhelmingly dominant group and calculate as if this were the inertial system, while taking account of the influence of the second group by introducing, at every test point, the then very small correction forces of relative motion that the chosen reference system executes relative to the true inertial system. If one resolves to do this, these Coriolis forces then no longer appear as mere computational quantities arising from a coordinate transformation, but as physically existing forces exerted on each test point by the masses of the smaller group, and arising from the fact that these masses describe a motion relative to the chosen reference system.

To pursue this thought further, one might first investigate the case in which the second, smaller group just mentioned is represented by a single world body. One would then face the task of specifying, in magnitude and direction, the force — depending on the velocities of this world body and of the test point relative to the reference system fixed with respect to the other world bodies, and on the distance between them — that acts on the test point. If one imagines this task solved for a single world body, then on the basis of the principle of superposition the influence of an entire group of moving bodies would follow.

What has so far been securely established by observation is probably not sufficient to solve this fundamental problem; but one need not therefore doubt that further observations could lead to its solution.

After these preliminary considerations, I now turn to the case as it corresponds to reality. Making use of the fact that the configuration of the fixed-star sky changes little over the course of years or centuries, one may for the time being imagine a reference system fixed relative to three suitably chosen stars that nearly coincides with the inertial system. In order, however, to take account of the small deviations that still remain, one must imagine Coriolis forces applied at every test point which — as described earlier — are to be interpreted as forces depending on the velocities of the individual world bodies and of the test point.

With this, we are now also — and I place particular emphasis on this — in a position to specify a condition satisfying our need for causality, which the true inertial system required by the law of inertia must fulfill. It is namely that reference system for which all forces depending on velocity, originating from the individual world bodies, balance each other at the test point. Even if practically not much is initially gained by this statement, it nevertheless seems

to me to provide a very suitable basis for forming a clear concept of what is called absolute motion in mechanics. At the very least, it opens the prospect of a path which, after discovering the law governing the velocity-dependent forces, would lead to a determination of the inertial system. In other words: the absolute space spoken of by the law of inertia becomes constructible, without having to sacrifice the notion that, in the final analysis, all motions are only relative.

In all these considerations, my principal intention is to make it at least plausible that, in order to arrive at a satisfactory solution of the questions connected with the law of inertia, one must assume forces between world bodies that depend on their velocities relative to the inertial system. And if one concedes this, there follows further the task of seeking possible experiences that could stand in such a connection with the expected general law of nature that the law governing the velocity-dependent forces could be inferred from them. These forces, which for the sake of brevity I shall henceforth simply call “velocity forces,” have nothing to do with gravitational forces, which occur alongside them, and they can — and presumably will — obey an entirely different law with respect to mutual distance than the latter.

Here I would like to insert a remark that divides this paper into two quite separate sections. What I have set forth so far I believe I can defend with full certainty and confidence. What follows, however, I regard merely as an attempt, which may very easily fail; but nevertheless as an attempt that has at least some prospect of success and therefore must be made once.

The most promising path for detecting the postulated velocity forces and uncovering their law of action seems to me to lie in the most precise possible observation of terrestrial motions that occur with relatively large velocities. Just as the discovery of gravitation began with the observation of falling motion, so here too the first step toward solving the riddle could be taken through experiences concerning terrestrial motions and their correct interpretation. The immediate proximity of the Earth’s mass offers some prospect of detecting the presence of velocity forces — which, as experience teaches, are under ordinary circumstances certainly only very small — with greater precision than would be possible in even the finest astronomical observations.

This consideration led me at the time to carry out the gyroscope experiments about which I reported to the Academy nearly a year ago.⁷ I then expected — as I explicitly stated — to be able to establish a behavior of the gyroscope not in agreement with the usual theory, in the hope of attributing the observed deviation to the sought-after velocity forces and thereby making them accessible to experimental investigation. Certain indications of a deviation were indeed discernible; but as a cautious and conscientious experimenter I was not permitted to attach weight to them, and I had to declare — as I did — the result of the experiment negative in the direction that was primarily pursued. I have since carried out a few further experiments with the same apparatus, though only a few, since they are very laborious and time-consuming. The result, however, has only strengthened me in the view that the accuracy attainable with this

⁷Sitzungsberichte 1904, S. 5.

experimental arrangement does not suffice to detect the velocity forces, if they exist at all.

More success might reasonably be expected from a further continuation of fall experiments, whose results so far may already be described as quite encouraging. The usual theory, which takes no account of velocity forces, predicts on the northern hemisphere, alongside an eastward deviation of falling motion from the plumb line, a southward deviation of such extraordinarily small magnitude that its experimental detection would be entirely excluded. Nevertheless, observers have repeatedly found southward deflections of measurable magnitude, which are of an entirely different order of magnitude — several hundred times and more greater — than those expected from theory. The most recent observations in this field, due to the American physicist E. H. Hall, known with distinction as the discoverer of the “Hall phenomenon” and as an experimenter,⁸ have once again confirmed this experience. Hall does, however, regard further experiments on a larger scale (with greater fall heights) as necessary, and he has announced such experiments. One may expect very valuable insights from them. Perhaps the further execution of such experiments may also be promoted if theoretical considerations such as those I have presented here strengthen the hope for a positive result. For it truly requires no small courage to undertake laborious and lengthy experiments when the unanimous opinion of all theorists is that they cannot possibly lead to the expected result. This consideration has also chiefly prompted me to come forward with my views, although I must tell myself that they still lack far too much in the way of adequate experimental foundation to have much prospect of meeting with approval.

I now come to the — as I must admit — most doubtful conjecture that I have formed in connection with the foregoing, and which is connected precisely with the observation by Koch mentioned at the beginning. It is readily understood that I must also expect velocity forces arising from the motion of the Earth relative to the Sun. The Sun is a fixed star like the others, and it too contributes its share to the fixation of the inertial system; or, in other words, it exerts velocity forces when we consider motions relative to a reference system established without regard to it. Even though nothing is yet known about the dependence of these forces on distance, one may nevertheless regard it as probable that the influence of a nearby body is greater than that of a much more distant one. Nothing is therefore more natural than the assumption of velocity forces of this kind, which could bring about small periodic changes in gravity, with both a daily and an annual period.

A difficulty — and indeed a very serious and perhaps insurmountable one — arises only if one assumes that these velocity forces could be of such magnitude as to be measurable at the Earth’s surface, and that Koch’s observation could be interpreted in this sense. One then necessarily encounters the objection of astronomers, who, despite the great precision with which they are able to predict the motions in the solar system, have noticed nothing of the occurrence of such

⁸Edwin H. Hall, *Physical Review*, XVII, S. 179 und S. 245, 1903; ferner *Proceedings of the American Acad.* XXXIX, Nr. 15, S. 339, 1904.

forces.

This objection is so compelling that one might almost abandon hope of being able to silence it. One might indeed retreat to the position that, so long as nothing is yet known by other means about the law of action of velocity forces, there remains at least a remote possibility that this contradiction could later be resolved. And in this hope one might initially wait calmly to see what consequences emerge — while provisionally disregarding this contradiction — from observational results such as those found by Koch. If, for example, not only Koch's original observation were confirmed, but also a daily period of gravity fluctuation, expected on the basis of the views presented here, were actually found, then despite all objections one could see in this a certain confirmation of the proposed theory.

But I see that such a position could not be maintained. If it does not succeed even now in making it at least somewhat credible that the interpretation of Koch's observation which I regard as possible need not necessarily stand in contradiction to astronomical experience, then no one will pay any attention to my interpretation, and there could then arise the danger that Koch's observation would meet the same fate as the southward deviation of falling bodies — namely, that one would not seriously concern oneself with it and would be inclined from the outset to attribute it to observational error, because it does not agree with accepted theory.

Only for this purpose — and by no means in order to present the individual possibilities now to be discussed as particularly probable — I add the following.

Let one imagine a planet that orbits its central body in accordance with the first two of Kepler's laws. Let the law of velocity forces be such that the planet experiences an attraction from its sun that is proportional to the component of velocity perpendicular to the radius vector and inversely proportional to the first power of the distance. One immediately sees that under these circumstances no gravitational force at all would be required alongside the velocity force in order to explain the planetary motion given by observation. The astronomers of a solar system with only a single planet would in fact have no means of deciding whether Newton's gravitational force or the velocity force assumed in the manner described were valid, if they restricted themselves to observations of the orbit alone. The difference would, however, immediately become apparent if they included observations made on their planet.

According to Newton's law of gravitation, a daily period of gravity fluctuation is also known to occur, which gives rise to the portion of tidal motion originating from the Sun, but which is too small to be detected by pendulum observations. If, however, the astronomers of that solar system attempted to replace Newton's law of gravitation by the mentioned law of velocity forces, they would have to expect a far larger daily period, which under conditions like those between our Earth and the Sun would amount to about 180 times that expected in the other case.

It should further be noted that the arbitrarily selected velocity law is only one of infinitely many that would all accomplish the same thing, namely explaining the motion of a single planet around its sun in accordance with the first two

Keplerian laws, without requiring a Newtonian gravitational force to act alongside it. One would merely need to allow the velocity component in the direction of the radius vector — which was previously assumed to be without influence — to participate according to some arbitrary law, and could then determine the law, after the component perpendicular to it acts on the attractive force, such that the desired motion results. Nor would one need to restrict oneself to the first power of the velocity; one could invoke the second or other powers.

If a solar system has more than one planet, it becomes, of course, much more difficult to explain all planetary orbits using velocity forces alone, because the third Keplerian law must now also be satisfied. One would then, at least so far as I can see, have to resort to quite artificial assumptions. And even if this should nevertheless succeed more easily than now appears to me, it would still remain doubtful whether the perturbations of planetary orbits, the motions of moons, and so forth could then also be explained.

But one should not forget the purpose of these considerations. I have no intention whatsoever of replacing Newton's law with a law of velocity forces. I merely wish to make it plausible that velocity forces could, under certain circumstances, by themselves produce effects quite similar to those of gravitational forces. If one concedes this as possible, it immediately follows that, should such a case occur, it would be very difficult to disentangle from astronomical observations the portion attributable on the one hand to gravitational forces and on the other to velocity forces.

On the basis of this consideration, I regard it as best not to allow oneself to be deterred by the — in themselves admittedly very weighty — objections of astronomers from searching for phenomena that could be brought into connection with velocity forces. If it succeeds, on this entirely independent path of investigation, in deriving a law of action for the velocity forces, then afterward there still remains, as the best touchstone for the admissibility of the result, a precise comparison with astronomical observations, taking into account the error margins that must be considered there.

I would naturally not recommend such a procedure if I did not expect with greater confidence that velocity forces exist at all — even if it must be left open whether they are of such magnitude.

Whether they will ever be detectable in the motions accessible to our perception remains, of course, doubtful. If one is willing to admit an absolute space, then indeed every reason for assuming velocity forces disappears. But at least in this one point — that I do not recognize an absolute space — I find myself in agreement with the majority of natural scientists, and I therefore hope that at least the conclusions drawn in the first part of this paper will find consideration among them.