A Few Further Remarks on "The Principle of Relativity"

K. Kroman

Fysisk Tidsskrift 15 (1916–17), pp. 192–205

Translated from the Danish

Fysisk Tidsskrift opens its 15th volume with no fewer than three treatises on the *Einstein*ian principle of relativity. Since all three of them take up a position towards my article on the same subject in the 14th volume, pp. 1—30, I should like to be allowed to bring forward a few further remarks on the question.

With the author of the first treatise, Mr. Editor Helge Holst, I am, I believe, in agreement on each and every point. His introductory remark about the practical applicability of the Einsteinian formulae I can also readily subscribe to; indeed I have said the same thing myself at the top of p. 24 of my article.

Nor am I, with the author of the second treatise, Mr. Engineer C. E. Walsøe, at bottom in disagreement so far as the Einstein problem is concerned. When he (at the foot of p. 15) finds that Einstein's doctrine has been condemned as a scientific hypothesis, I am completely satisfied. To calling it an unscientific hypothesis I have no objection.

With the author of the third treatise, Dr. H. M. Hansen, I am on the other hand, epistemologically regarded, in great disagreement. Dr. H. appears as a warm defender of Einstein's doctrine, which he does not find very different from Lorentz's, and he holds that I have misunderstood the latter. I assume that this is chiefly to mean that I have not sufficiently distinguished between Lorentz's original and his later, more fully worked out hypothesis. But to this my article gave me no occasion whatever. Dr. H. overlooks that I expressly emphasised that I did not wish to write a theory of electricity, but merely to examine the Einsteinian hypothesis, particularly on its epistemological side. Neither did I wish to give an exposition of Lorentz's works, the less so as I am not decidedly certain that they give us precisely the right solution of the Michelson riddle[*)].

[*)] It really continues to be my hope that M.'s result will one day be explained by an alteration of the

Lorentz has presented both his hypotheses — the latter no fewer than three times — in the writings I have cited. He has there discussed the various experiments akin to Michelson's which Dr. H. also mentions. I can therefore hardly have overlooked that he had both an original and a more fully worked out hypothesis. But common to them both is the contraction, and I needed nothing else and nothing more for my examination of Einstein's train of thought. I have moreover also emphasised the community of formulae between Einstein and Lorentz, but at the same time stressed that in spite of this there is nevertheless a most essential difference between the two. In what follows I shall once more try to give grounds for holding that Lorentz's hypotheses are at all events possible assumptions, while Einstein's must in my view be stamped as impossible.

Dr. H. calls Einstein's doctrine simpler and (in an earlier treatise) bolder than L.'s. That the simplicity must be of a rather

superficial kind seems, however, to appear sufficiently from the uncommonly artificial results concerning space and time in which it issues, and boldness is after all no unmixed scientific virtue. In Einstein it appears, as we shall see, chiefly as courage for exaggerations and vague determinations of concepts, courage to defy all reasonable probability, and indifference towards fundamental epistemological demands. I have, in my judgement, brought this last point to light in a number of places in my treatise. Dr. H. has sought to remove some of the difficulties pointed out; but it does not seem to me that he has really succeeded in doing so. I shall however not dwell further on these particulars, since I believe I can illuminate and justify my view still more clearly by following Einstein's train of thought critically through its development, and pointing out where its incorrectnesses come forward one by one and give occasion to the various contradictions and great improbabilities in which his doctrine issues.

First, however, I must permit myself a couple of general remarks.

Dr. H. defends various peculiarities in Einstein's exposition with the statement that Einstein is a phenomenist. That is however a little reassuring defence; for it is anything but fortunate for an investigator to be a phenomenist. By a phenomenist one understands an investigator who has formed the view that it is most scientific to keep to the phenomena, to that "which shows itself", and to let causes, forces and the like look after themselves. It is thus sensation that is hereby set in the high seat, while thought, as

theory of light. Dr. H. finds (p. 18) that this prospect is barred by an utterance of Poincaré's. To me, however, it seems that this utterance — at least in Dr. H.'s formulation — is quite unwarranted. By what right could one demand that L.'s hypothesis should take account of other marvels than M.'s, before any others had yet shown themselves? If others of the same nature turn up, the same hypothesis will of course be able to explain them too, and if they are of a new nature, new hypotheses will presumably be required as well. But one should not, as is well known, sell the skin before one has shot the bear.

an unreliable counsellor, is put in the corner. But of course such an epistemology is just as impossible to carry through as it is false. Without the help of thought there could of course be neither calculation nor inference. A real denial of thought therefore becomes an impossibility for man, and every phenomenist therefore becomes of necessity an *inconsistent* investigator, who lets thought play its part as usual the 99 times, while suddenly the 100th time it is quite arbitrarily denied, so that one permits oneself, for example, to set up effects without stating or being able to state a trace of causes, and the like. Of this kind of step we shall meet a number in Einstein, and that such liberties are not fortunate for scientific character surely needs no demonstration.

Let us then begin by looking a little more closely at the not very clear concept and the not very fortunate expression "the principle of relativity", and let us first ask: did Newton (and the older physics generally) have anything that could rightly and reasonably be called a principle of relativity? What, in general, is a principle?

In most physical textbooks one finds various quite respectable propositions obstinately entitled principles: "Archimedes' principle", "Doppler's principle", and so on. This evidently arises solely from linguistic indifference and unfortunate inertia. By a principle is understood, more exactly speaking, a starting point, a fundamental proposition, i.e. a proposition which is laid at the foundation of the whole following structure of doctrine, a first proposition which is not derived from earlier ones, which has no proof before it, since one has found it self-evidently correct, and of whose validity no one has dared to doubt. Of such principles or fundamental propositions Newton, as is well known, set up three: the law of inertia, the law of force and the law of interaction. A "fundamental proposition of relativity", on the other hand, he did not set up. In the law of force, and already earlier, he defined a force not as a cause of motion, but as a cause of *change* of motion, thus not by the aid of motion but by the aid of change of motion, that is, change of velocity, of direction, or of both. The logical consequence of this is, among other things, that if a body has a certain motion, and another like it has the same motion plus some uniform rectilinear velocity, the equations of force for both must of course become the same; for unaltered motion requires no force.

Further, he maintains that a body can have motions of all kinds, relative as well as absolute, and that just as there is a difference between uniform rectilinear motion with the velocity 5 and uniform rectilinear motion with the velocity 10, just so must "motion with the velocity 0" be something by itself. This too every sober person is compelled to assume. No one can think anything by the expression motion without at the same time having to think, more or less distinctly, of a resting; and no one can think a motion

as relative without at the same time having to think an absolute rest or motion. These ideas stand and fall with one another like the ideas up and down, and in every rational mechanics one handles these determinations quite securely and naturally.

Widely different from this is however the question: can we human beings always determine what motion a body has in a given real case? With absolute exactness we can of course never do it. With approximation, on the other hand, we can often determine its relative motion. In certain cases we shall also be able to determine that it has such and such an absolute motion. But if the motion is uniform and rectilinear, the case will — so far as we yet know — not be distinguished by any sensible mark from an unaltered motion in the same direction with another velocity or with none at all, and therefore it will, as Newton says, often be *hard* to determine the true velocity.

Now why may Newton have used so vague an expression? Why has he not said outright that in the face of unaltered motion a determination of velocity is simply and plainly impossible? Of course he was quite clear that here no help was to be expected from manifestations of inertia, as there is with changes of velocity or of direction. He had, it is true, the *conjecture* that the centre of gravity of the solar system was absolutely at rest in space. But it was only a conjecture. He has also said that perhaps there was no such fixed point at all that we could lean upon. He has therefore, rather, presumably thought that matter, which had so lately revealed itself from a new side in the enigmatic gravitation, might perhaps also one fine day disclose *certain* differences according as it was at rest or in uniform rectilinear motion, and that the physicists might learn to control these differences, so that a computation of velocity would thereby become possible. If Lorentz's hypothesis is correct, we have of course in *the contraction* precisely something in the direction indicated.

When later the wave theory of light had come to power, and the phenomenon of interference had been more closely understood, a number of physicists have surely thought that as soon as one merely obtained sufficient technical mastery of the interference experiments, one would thereby have a means of distinguishing between absolute rest and uniform rectilinear motion. Precisely this was of course Michelson's assumption.

If one wishes to express oneself as exactly as possible, I therefore think that one must confine oneself to saying of Newton and the older physicists:

1) They conceded that they were for the present unable *experimentally* to distinguish between absolute rest and uniform rectilinear motion.

2) But they were by no means certain that this would remain impossible.

3) Still less did they set up the impossibility of it as a principle, that is, as a proposition

one *was to* start from.

4) And they have surely all been agreed with Newton that absolute rest and uniform rectilinear motion were something different in themselves, and not simply dependent upon which coordinate systems one used.

But if this is how the matter stands, it becomes not merely misleading but quite incorrect to use expressions like "a Newtonian principle of relativity". Everything is then said far more clearly and sharply in the simple words that Newton built upon the law of inertia. —

More characteristic than to call Newton a relativist would surely be to call him an absolutist. In Einstein's doctrine, on the other hand, we have nothing less than an attempt to relativise not merely rest and motion, but even space and time as well.

It is in particular Michelson's result that leads him into these endeavours. Michelson started from the assumption that light, independently of the light source's rest or motion, constantly had in the world-space and in air the constant velocity c, as he relied on the current theory of light generally. So did Lorentz and Einstein, and I can therefore frame the following remarks on the basis of the inviolability of this presupposition.

Michelson now found by his experiment that light traversed the two paths s and s_1 in one and the same time.

For human reason there is nothing else to remark upon this than that the two paths consequently *must* have been equally long. This was also Michelson's first thought, in that he assumed that the earth's translation had been counteracted by the motion of the whole solar system. When this way out was found barred, Lorentz set up his contraction hypothesis. If we leave aside that he might of course also have assumed the shorter path to be lengthened, and if we hold fast to the complete correctness of the theory of light, there was, as has been said, no other way out, unless the phenomenon was to come under the purely irrational.

Einstein has surely known this first contraction hypothesis of Lorentz's when in 1905 he came forward with his doctrine. This rests, he says at the beginning of his treatise, upon two presuppositions: the principle of relativity and the "only apparently irreconcilable"

presupposition of the aforementioned constant velocity of light. These two presuppositions are *sufficient*, he adds. Of contraction he says not a word. On the other hand he declares it superfluous to assume an ether.[*)]

Concerning "the principle of relativity" he remarks that he has arrived at the conjec-

[*)] Lorentz, Einstein, Minkowski: Das Relativitätsprincip, pp. 27—28.

ture that to the concept of absolute rest there corresponds, neither in the mechanical nor in the electrodynamic domain, any peculiarity of the phenomena. This conjecture he will now "raise to a principle" and call the principle of relativity.

But it really will not do to raise conjectures to "principles" in this way without further ado. For the present we know only that in a few cases results corresponding to Michelson's have shown themselves. Einstein can therefore only be permitted to set up as a *hypothesis* that his conjecture holds generally, and he can thereupon set himself the task of testing this hypothesis's possibility and its eventual scientific worth. But he thereby incurs quite other obligations than if he could at once raise his conjecture to a principle.

His first scientific obligation would thus become that of giving the hypothesis up again on account of its impossibility. For it is impossible, since it stands fast that, when the correctness of the theory of light is presupposed, the contraction is the only conceivable way out. But if bodies are contracted by the motion, then there corresponds after all a definite peculiarity, namely uncontractedness, to real rest. That we cannot for the present measure the contraction *directly* is, practically regarded, tiresome enough. Scientifically regarded, however, it is already sufficient that we can establish that it must take place, indeed even compute its magnitude in relation to any presupposed velocity.

But with this, as we shall see, all real relativising of rest and motion, of space and time, is annihilated, and the many corresponding exaggerated expressions are to be rejected[**)]. Already here, then, it is seen that Einstein's hypothesis is an impossibility. What he could with scientific

right have set up and sought to justify is something widely different from what he has stated. He could rightly have set up the hypothesis that there is a certain *technical* equivalence between absolute rest and uniform rectilinear motion.

Without noticing anything of the contradiction, Einstein however goes further. He thinks of two coordinate systems with mutually parallel axes and a common X-axis, the one ($O\ x\ y\ z$) at rest, the other ($O'\ x'\ y'\ z'$) with the constant X-velocity v. In each there is an observer (A and B) furnished with perfectly alike clocks and measuring rods and a consummate ability to use them. They both rely on the theory of light and on the constant velocity of light c. Dr. H. calls them physicists and assumes that they know Michelson's experiment. All this we can presuppose without misgiving.

[**)] Dr. H. too uses these misleading expressions. Repeatedly it is said: rest and uniform rectilinear motion are one and the same, or physically one and the same; there is no sense in distinguishing between them. And yet he then distinguishes so vigorously between them that he maintains that the motion makes bodies contract, clocks go more slowly, people age more slowly; indeed he even adds (p. 22) that every observer can establish this.

But Einstein then continues: they both assume that they are at rest.

This seems a remarkably frivolous and dogmatic assumption to assign to them. Physicists so excellently equipped ought surely rather to have repeated Michelson's experiment critically, and in any case to have built upon the more reasonable presupposition that each of them had a uniform rectilinear motion with a certain unknown velocity (perhaps = 0 for one of them), which they would have either to keep as an unknown in their calculation or to try to determine as well as could be done, for example by the aid of a sky of fixed stars, or whatever else they had that was most serviceable.

We may, however, suppose that Einstein has assigned them these arbitrary assumptions with a definite end in view, perhaps in order to show quite clearly that rest and uniform rectilinear motion are entirely alike, or the like; and of course he is at liberty in his thought-experiment to introduce whatever determinations he pleases, provided only he informs the reader what he is doing.

But at the same time the reader is of course also at liberty to note what happens: at liberty to note that here Einstein introduced a contradiction into the situation, in that he gave A a correct presupposition and B a false one, in direct contradiction with what had been said about the two coordinate systems.

A thereupon regulates the clocks he has set up round about in the system O by the aid of light signals, so that they all get the same setting and a correct rate, 1 second while the light runs the path c. This now goes easily enough. But then B is to do the same from his contradiction-laden presupposition.

And here one must remember that Einstein has hitherto not said a word about contraction, as indeed he could hardly do, since he has on the contrary said that the phenomena were to behave alike under rest and under uniform rectilinear motion. We must therefore for the present reckon without any contraction. Einstein has of course possibly thought that it would present itself of its own accord as a consequence of his presuppositions.

But suppose now, first, that the "perfect" clocks have also had a correct rate (1 second while the light really runs the path c) already from the manufacturer's hand, and that B has relied on this as well as on the constant velocity of light. What will then happen?

B would then simply discover that if he sent a light signal the path l there and back perpendicular to the X'-axis, the light would not be $\frac{2l}{c}$ seconds on the way, but $\frac{2l}{c}\,a$ seconds[*)]; and if he sent the signal the path l there and back along the X'-axis, it would take the time $\frac{2l}{c}\,a^2$. But from this B would surely conclude: my system therefore cannot

[*)] Compare, concerning this and the following expressions, my earlier article.

be at rest; it must have the uniform rectilinear velocity v.

Assuming next that B does not rely on his clocks' going correctly, but will set them himself, we get a somewhat altered result. Let him again begin perpendicular to the X'-axis and again use the measured path l. If he now arranges his clock to advance $\frac{2l}{c}$ second-marks while the light is on its way, his seconds will be a times too large; for the light has in reality then run the path $2\,l\,a$. His clock thus comes to go a times too slowly, of which however he for the present has no inkling. But if he then sends a signal along the X'-axis, he will to his surprise find that the regulated clock has now advanced $\frac{2l}{c}\,a$ "seconds" while the light was on its way. And if he has thought the matter over a little, he will surely also conclude from this that he not only has the aforementioned velocity v, but that he has moreover arranged his clock to go a times too slowly.

Thus must the results shape themselves when we reckon without contraction.

But these results Einstein cannot use; for they stand in conflict with the Michelson result.

Could he introduce a real contraction, B's self-deception could on the other hand succeed. If all B's distances in the X'-direction became a times as small, he would be able to arrange all his clocks to advance $\frac{2l}{c}$ second-marks while the light, on his presupposition, ran the path $2l$, but in reality the path $2la$. His clocks would thereby come to go a times too slowly, and his incorrect presupposition would further entail that they got local time, or, in relation to the clock at O', deficits of clock-reading proportional to their X'-distances from O', without his noticing anything. All this is easily obtained by quite elementary calculation, and in the same way one easily obtains what A and B would on the whole be led to think about each other's computations, holding fast as they do to their mutually conflicting presuppositions.

In this way, then, we should be able to arrive at "the Einsteinian transformation formulae".

But Einstein himself cannot (legitimately) arrive at them. For he can neither *tolerate* nor *dispense with* nor *introduce* the contraction.

He cannot *tolerate* the contraction; for it would annihilate his first presupposition, which stands in conflict with it, since that presupposition does not permit a difference between the phenomena under absolute rest and under uniform rectilinear motion.

But even if Einstein could also *tolerate* the contraction, he still cannot *introduce* it. For where should he get it from? Since he has rejected the ether, he would be quite unable to find any cause for this alteration of the phenomena. The contraction would become a purely miraculous phenomenon; and surely one is not permitted in science to

invent miracles in order to prop up an otherwise tottering or unworkable hypothesis?

On the other hand we have just seen that the doctrine cannot at all *dispense with* the contraction. It is quite necessary if *B* is to be able in the long run to remain in his false assumption. And if this does not happen, Einstein will not merely come into conflict with Michelson; his train of thought will positively come to establish the opposite of what he set out to prove.

There is therefore hardly anything else to be done than to give up the Einsteinian doctrine as *impossible*.

Perhaps one or another of his adherents will remark that if there is nothing more amiss than this, then the matter does not stand so badly for Einstein. For we need then merely restrict his relativity presupposition a little, let him keep the ether, and let him add the contraction; then all is in order.

Quite so, certainly. But then it is simply no longer Einstein's doctrine; then it is Lorentz's that we have arrived at. And Lorentz's view is, as has been said, a possibility, but in return, as we shall see, a view very different indeed from Einstein's.

The difficulty with the contraction Einstein has evidently not had clearly in view at all. When he comes to compute *B*'s *moved* system, it goes quite easily for him, "indem man das Prinzip der Konstanz der Lichtgeschwindigkeit im ruhenden Systeme anwendet." But that he has of course no right whatever to do. He thereby commits a circular inference and proves nothing at all, or at most that rest and motion are one and the same, provided one boldly presupposes that they are one and the same. The error hangs together with his "raising" to a principle, without further ado, what is only a hypothesis. If light, according to his second presupposition, has a constant velocity in space and thus in the system at rest, then it is *immediately regarded unthinkable* that it should also have a constant velocity, i.e. the same velocity in all directions, in a moved system. Anything of the kind first becomes thinkable if the moved system and all its contents have, on account of the motion, contracted according to certain definite laws. Such an alteration, however, Einstein cannot get introduced without having to give up his first presupposition and to assume the ether again. His two presuppositions are thus *really* in contradiction with each other, and not merely apparently so, as he supposed.

Einstein could with every scientific warrant have introduced his problem with the following question:

Can one, without coming into contradiction with sound reason, assume 1) that the phenomena are always alike under absolute rest and uniform rectilinear motion, and 2) that light always has in space (and in air) the same constant velocity *c*?

And to this the answer would then have to be: no! For if the phenomena are to be *even merely apparently* alike under absolute rest

and uniform rectilinear motion, then light must also in a uniformly rectilinearly *moved* system apparently have the velocity c in all directions. But this is unthinkable unless the system and all its contents are contracted during the motion according to certain laws (for example the Lorentzian ones). And if this is the case, then the phenomena are of course not alike under absolute rest and uniform rectilinear motion. If it should still be objected that the contraction cannot be reckoned to "the phenomenal", to that which "shows itself", then one forgets that it has of course shown itself precisely through the position of Michelson's interference fringes.

But Einstein sees nothing of all this. He therefore keeps his presuppositions without suspecting anything of their contradiction. He raises his impossible hypothesis to a principle and takes the contraction along on his own free authority. If he can assume the theory of light without assuming an ether, it would of course also be unreasonable to make a fuss about such a trifle of a contraction; if one is a phenomenist, such things cause no stoppages. And thus he arrives at the result that he has really established that rest and uniform rectilinear motion are, both in the mechanical and in the electrodynamic domain, one and the same. For he has of course "proved" that A and B both arrive at the same results concerning the phenomena, indeed concerning each other, although A starts from a presupposition correct by the old belief and B from one completely wrong by the old belief. Consequently it must after all be quite indifferent whether one presupposes oneself to be at rest or in uniform rectilinear motion, and one can thus simply say to Michelson: you need only presuppose that the earth stands still, and your result at once becomes explicable. If anyone says: yes, but the earth certainly does move!, you answer merely: rest and uniform rectilinear motion are quite the same. There is no sense in distinguishing between them.

This is really Einstein's explanation of Michelson.

But if rest and uniform rectilinear motion are thus one and the same, then there is of course no sense either in calling A's presupposition correct and B's false. Then they are both correct, and the contradiction between them is something that will surely disappear with a more correct conception of the nature of space and time. And it is thus not correct either that B's seconds are false. They are just as correct as A's, although they are a times as large. The truth is merely that there are many kinds of seconds in the world, although one can at the same time quite well say that a second is the time in which light with its constant velocity c runs the path c.

This is no real contradiction either; but space and time have hitherto been conceived so incorrectly that it looks like one. As soon as they are conceived in their right relation to each other, everything comes into the most beautiful order. —

Thus does Einstein's doctrine issue. The contradictions in it are pushed further and further out into the distance, as criminals were once sent to Botany Bay. In the end it is the fundamental determinations space and time themselves that suffer for it. Further than that one cannot go, unless one were to take one very last step and maintain that contradiction and non-contradiction are also at bottom one and the same.

Let us next look a little at Lorentz's doctrine.

One can, as has been indicated, quite well arrive at this doctrine by starting from Einstein's and gradually removing all its contradictions and exaggerations. Lorentz has however gone another way. Since he *relied on the theory of light* and found it necessary to assume a resting ether, he explains Michelson's result to himself in the only way still possible, spoken of before: by conjecturing that the motion has brought with it a contraction of the apparatus in the direction of the translation. And as soon as the kindred experiments mentioned by Dr. H. had come in, he sought further to explain to himself the origin of the contraction in such a way that precisely the results obtained must come about. Thereby arose his more elaborated hypothesis, whose germs are however already indicated in the first. He puts forward his assumption with great reserve, constantly calls it merely a hypothesis, and states ‚that one cannot yet know at all how generally the contraction in question will set in; about that only experience will gradually be able to instruct us. His investigation has merely shown that such a contraction *would be able* to come about by the ways indicated without giving contradiction; but it has not shown that it *must* come about. Neither does Lorentz set up any dogma about the velocity of light as the greatest in the world; he says quite simply that his hypothesis holds only for velocities smaller than that of light. He proceeds, in other words, constantly in a *correctly scientific* manner.

What now are the consequences of his assumptions?

First and foremost this, that everywhere where the conjectured contraction of things on account of their motion really sets

in — and it is of course *possible* that it will *always* set in — there it will go with us as it went with Michelson: if we work in the electrodynamic or optical domain in a uniformly rectilinearly moved system without taking into account that it possibly moves, we shall on account of the contraction apparently get the same results as we should have got if

the system had been at rest.*)

Further, the transformation formulae between two such systems will of course, as purely mathematical consequences, get the same *form* in Lorentz as in Einstein.

But they will get a widely different *significance*.

Lorentz is not a relativist. He constantly says "the Einsteinian principle of relativity" when he speaks of it. He himself holds absolute rest and unaltered motion to be two different states, and the system at rest in relation to the ether is for him the really resting one, the "distinguished" system which the relativists deny because we cannot for the present point it out *experimentally*. If *A*'s system is such a one, then for Lorentz *A*'s time is the true time and *B*'s only an apparent or conditioned one, and the corresponding holds of course of their measurements. There is therefore in Lorentz no question of the *contradiction* that *both A and B are right. With him truth is only one*, even if we cannot always find out where it lies. We know in any case that at most only one of the two can be right. Lorentz thus denies neither thought's demands nor its results, and he constantly distinguishes clearly between rational truth and what is experimentally attainable. Space, time and simultaneity therefore preserve with him undisturbed their former character, and what in Einstein appeared as the artificial and the paradoxical, precisely because it was conceived as something absolute or solely valid, presents itself in Lorentz as the quite intelligible and natural, being clearly stamped as the expression of the *composite* situation which arises partly from the contraction that plays its part and partly from the deliberate disregard of the system's possible or real motion.

---

*)In the face of questions where time also plays a part, this will however strictly speaking only occur provided that we either ourselves set our clocks to an apparently correct rate by the aid of light signals, or that the contraction itself will really bring it about that they get this rate, even if the system has previously been at rest and their rate then corresponded to rest. This question has hardly yet been sufficiently investigated.