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Source: *Philosophical Studies: An International Journal for Philosophy in the Analytic Tradition*, Vol. 29, No. 1 (Jan., 1976), pp. 21-36
Published by: Springer
Stable URL: http://www.jstor.org/stable/4319006

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STATES, CAUSES, AND THE LAW OF INERTIA

(Received 22 November, 1974)

I

A paper by Richard Westfall entitled 'Circular Motion in Seventeenth-Century Mechanics', begins with the following statement.

> One prominent contemporary school of history and philosophy of science holds that the principle of inertia can only be understood as a convention that defines uniform rectilinear motion as a natural state which requires no causal explanation.[1]

Westfall is certainly right in thinking he has here expressed something recognizable as a (or possibly even *the*) received view about the law of inertia.[2] I do not think that the principle of inertia should be understood as a convention, but with that proviso, I accept the position in question, which I shall call the Received View.

Implicit in the Received View is the general claim that natural states require no causal explanation. For the force of the Received View is not that uniform rectilinear motion is a natural state which, *as it happens*, requires no causal explanation, but rather that uniform rectilinear motion is a natural state and *therefore* requires no causal explanation. Intuitively, the idea is that a natural state is what would obtain were no causes operative at all, and hence causes need only be cited in accounting for deviations from natural states. So it seems that the Received View is best understood as dividing into two separate theses: (i) natural states require no causal explanation, and (ii) uniform rectilinear motion is a natural state. My purpose will be to try to clarify these two theses.

II

It must be admitted immediately that there are senses in which it is quite correct to say that states are causally explained. First, there is a sense in which one is said to have causally explained a state if one has explained

Philosophical Studies **29** (1976) 21–36. *All Rights Reserved*

its *onset* as the effect of a certain cause, as when one explains why a 'frictionless disk' is moving with a constant velocity of eight feet per second along a level surface by pointing out that it began by sliding down an inclined plane one foot high. Second, there is a sense in which one is said to have causally explained a state if one has explained as the effect of some cause the neutralization of certain usual or expected disturbing factors, as when one explains the (near) constant velocity of a 'frictionless disk' by pointing out that pressurized gas escaping through a small hole in the concave bottom of the disk forms an air cushion (virtually) eliminating friction between the disk and the table surface.

So states can be causally explained, and in particular, a state of uniform rectilinear motion can be causally explained, as the examples show. Neither sort of explanation, however, features the explanadum as an effect of the cited cause. The explanations are causal, for essential appeal is made to a cause-effect connection: the change in the upward force exerted by the surface causes the onset of uniform motion; the escape of pressurized gas causes the elimination of friction. But the state explained is not an effect of the cause cited, and in fact is not a partner to the causal connection at all. As a first approximation, then, perhaps we should revise the first thesis to read: natural states require no *direct* causal explanation where a direct causal explanation is understood to be an explanation in which the explanadum is explained as the effect of a cited cause. More simply, we may tentively understand the first thesis as the thesis that natural states are uncaused.[3]

A state is a condition of changelessness. Intuitively, a thing changes during an interval *i* just in case the set of properties it has at the beginning of *i* is not the same as the set of properties it has at the end of *i*. But, even ignoring scruples over reifying properties, this idea is hard to sustain under pressure. What exactly is included in the set of properties a thing has at a time? Is the spirit of the suggestion compromised if we include purely relational properties? What about intensional properties? Is a gas at constant pressure, temperature and volume undergoing any physical change or not? The very familiarity of these questions is enough to indicate that no easy answers are forthcoming. Fortunately, however, it is possible for most purposes to salvage the core of the intuition by adopting a relativized conception. We may drop talk of change *simpliciter*, and speak instead of change relative to some antecedently specified set of properties. Relative to pressure, temperature, and volume, a system in thermodynamic equilibrium throughout an interval *i* undergoes no change during *i*. But this does not mean that the system undergoes no physical change of any kind of during *i*. From the point of view of kinetic theory, we have an enormous tangle of continuous processes certain selected effects of which 'balance out'. From a kinetic point of view, 'no change' in a thermodynamic system turns out to be a theoretical and practical impossibility.

This relativized conception can be given a usefully simple abstract formulation if we are willing to tolerate some loss of generality. Given a system (or region of space) *s*, we may characterize a state sigma of *s* by specifying values for each of a number of antecedently chosen variables, $v_1(s) \ldots v_n(s)$, called state variables for *s*. *s* is thought of as being in sigma throughout an interval *i* relative to the $v_i(s)$ just in case each of the $v_i(s)$ has the appropriate constant value – a sigma value – throughout *i*. If one or more of the $v_i(s)$ is continuously changing values during *i*, we think of *s* as undergoing a process, or of a process occurring in *s*. We think of an event as occurring in *s* at *t* if one or more of the $v_i(s)$ changes value at *t* while the values of the $v_i(s)$ are stable immediately before or after *t*, i.e., if we have a change of state, or the termination or onset of a process.

This abstract account allows for any arbitrary choice of state variables. In many scientific contexts, however, we have in mind, or are searching for, a set of variables which is *complete* for certain purposes in the sense that, given those purposes, everything one needs to know about a system at a moment is given by specifying a value for each variable in the set for the system at that moment.[4] Thus we find it useful to talk about the mechanical state of a system, meaning the state of the system relative to a certain set of mechanical variables – momentum and relative position in the classical case where the system is several particles – because a significant amount of theory can be constructed utilizing those variables only.

We can now see why states are not naturally thought of as effects (or as causes either, for that matter). If we are thinking of *s* as in a state sigma of a certain sort (mechanical, thermodynamic) throughout *i*, then there is some set of state variables which we regard as *fully characterizing s for the purpose at hand*, and these do not change value during *i*. Hence, for the purpose at hand, nothing is happening in or to *s*, and so no cause is wanted to account for what is happening there: a cause which has no change in s as effect has no effect in s at all. A process or event in s, however, entails a change in the value of at least one of these state variables, and this change does require direct causal explanation as the effect of some cause.

To some extent, no doubt, this is a verbal matter: there is the question of how one chooses to distinguish between states on the one hand and processes and events on the other, and there is the question of how one uses the word 'effect'. Consider a block of stone being dragged up an inclined plane at constant velocity by a rope. Assuming for the sake of argument that the block is in a *state* of uniform rectilinear motion, isn't this state maintained by the constant pulling, and isn't this equivalent to saying that the continuation of the state is an effect of the pulling? Perhaps we should take a hint from Westfall's formulation and distinguish between natural states, which are not caused, and states which are not natural but enforced and hence effects.[5]

Actually, I think there is only the appearance of a problem here. Consider the block again. We do all this work pulling, and it certainly seems proper to say that the block continues to move up the incline with a uniform velocity as an effect of the pulling. Certainly any variations in the pulling will be answered by variations in the velocity of the block. But now recall the familiar textbook analysis. From the fact that the block is moving with uniform velocity in a straight line we can infer that there is no net force on it. Thus, the resultant of the weight of the block and the force exerted on the block by the surface of the incline, plus the forces due to friction and air resistance, must be equal in magnitude and opposite in direction to the force exerted by the rope. So the pulling produces a force the effect of which is to neutralize the other forces on the block, leaving it free to continue undisturbed in its state of uniform rectilinear motion! This sounds contrived, and of course it is. But only because it fancifully assumes that the block really does move with constant velocity along a straight path, and in practice this would not happen. But this is just to admit that, in practice, the block would not be in the mechanical state in question, but undergoing a certain irregular process or sequence of events. Of course, even if we *do* assume that the block moves uniformly, it is not *wrong* to say that it continues to so move as an effect of the force exerted by the rope rather than that its continuing to move uniformly is causally explained by the fact that the net force on the block is zero due to the force exerted by the rope. But sticking with the latter formulation does help to keep in focus the important difference between the way this situation is conceived in classical mechanics, and the way in which the matter was conceived in peripatetic theories. For in these theories, the force exerted by the rope had to do more than neutralize the other impressed forces; it had to maintain the motion *in addition*. Uniform rectilinear motion was conceived as a process, the paradigm case of continuous change, and hence cried out for causes. In the Newtonian conception, to effect a continuation of the motion it suffices to effect a neutralization or elimination of the other impressed forces; the rope could be replaced by elimination of friction and slope. From this point of view, what cries out for causes is not the motion of the block *per se*, but the continuing failure of the various impressed forces to produce accelerations. This difference comes into sharper focus if we use 'effect' in such a way that a motion can be an effect of a force only if it is a *net* force. (This is not, of course, to say that component forces have no effects whatever.)

We will require causation (and not merely causal explanation) where we see events and processes as opposed to states. But what we see as an event or process rather than a state will depend on the state variables we employ, and this will depend in turn on tradition, ingenuity, and relative success in formulating satisfying theories employing the variables at hand. Given an inclination toward atomism, it is not surprising that science should often proclaim apparently static systems to be undergoing processes or sequences of events. For the fact that the chosen state variables for s remain constant during i is no guarantee that the variables for the ultimate parts of s will remain constant, especially since we may find it useful to characterize parts of s in terms foreign to the characterization of s. Evidently, if we do proclaim that a system s, previously thought to be static, is really undergoing a process or sequence of events on the grounds that the parts of s are undergoing change during the intervals in which s is in a state relative to the $v_i(s)$, then we must be thinking that the characterization of s in terms of the behavior of its parts is in some sense superior.[6]

Thus it is more or less obvious that atomism will lead us to see events and processes where we saw only states before, and hence to see effects and require causes where we previously did not.[7] What is not so obvious is that science can lead us to see a state where before we saw a process or event.

An example of this more surprising case is the classical idea that for certain purposes a body in space is completely characterized by its momentum.[8] Mere changes in position, providing they do not affect momentum, do not represent changes in the state of a body. Thus a body in unaccelerated motion throughout *i* is in a fixed state throughout *i*. Unaccelerated motion is not a process or sequence of events, and hence its continuance throughout *i* needn't be explained as the continued effect of anything.[9]

This shift of allegiance to a new set of state variables allows one to represent unaccelerated motion as a state of a body, and hence to dispense with the old question, 'Why do things continue moving?' A similar shift deals similarly with 'Why is there motion?' A universe of things in motion represents no change from some previous state of the universe which we suppose obtained at some time. There is no event – the onset of motion – which needs explaining. More or less motion is a nother matter: if we think of the universe as, in part, characterized by total kinetic energy at a moment, then changes in this magnitude will force us to find causes, or failing this, to argue for a new way of characterizing matters.[10]

III

Our views about what does and does not call for direct causal explanation depend on our views about what does and does not count as a state, and this in turn depends on the state variables we consider. Uniform circular motion is a state if we consider only speed, and if this is all we consider, we have no need of causes. But if we consider velocity, we have a process of continuous change of direction, and this wants explaining as the effect of a corresponding continuous change in the direction of the net impressed force. More broadly, whether and where causes are required depends on our taxonomic resources. We have seen how a scientific problem (continued motion) can be solved, or anyway made to disappear, by altering those resources in such a way as to replace an apparent process by a state. The question naturally arises as to what, if anything, justifies such alterations. This is both a question about the thesis that uniform rectilinear motion is a mechanical state, and about the meaning of the thesis that states are uncaused.

The natural response to the question is to say that the solution to the problem justifies the alterations which made it possible. This is no doubt true, but unhelpful. If the solution really *is* a solution, then *of course* it justifies the alterations which yield it. But some alterations in taxonomy simply make a problem invisible – literally indescribable – without making it clear why this is a good idea. Some critics of classical mechanics no doubt felt that just this sort of slight of hand was involved in the stance taken toward the problem about continued motion, and perhaps some theists would have felt this way about the proposed dissolution of the question concerning the existence of motion. Evidently we need to distinguish the real solutions from mere hand-waving, and this is just the original problem slightly, and misleadingly, relocated.

Let us therefore return to our example and ask why the classical resolution of the problem of continued motion isn't simply a case of putting on blinders. The problem can be made to look serious this way. The discussion so far has emphasized the dependence of the state/process-event distinction on the available taxonomic resources: nothing is a state but your descriptive machinery makes it so. But what we need, it seems, to vindicate the classical attitude is to find that an unaccelerated body 'really is' in a *state of motion*,[11] and not undergoing a process or sequence of events. For this would justify the adoption of a set of state variables which do not change values in the case of unaccelerated motion. We seem to be caught up in a circle: a body in unaccelerated motion is in a state if good mechanical theory ignores position in characterizing the motion of a body. But a theory which does this is a good theory only if bodies in unaccelerated motion really are not (in so far) undergoing a mechanical process or sequence of events.

There *seems* to be a rather simple way out of this: we simply point out that, as a matter of fact, the character of the motion of a body at $t+\varepsilon$ does not depend on its position at t. Now the mechanical problem is to show how the character of the motion of p at $t+\varepsilon$ depends on the features of p at t. Since the character of the motion of p at $t+\varepsilon$ does not depend on the position of p at t (or any other time), we may ignore position (for these purposes) in characterizing p at t.

Taken literally, this won't do, for it assumes what needs to be shown, viz., that specifying the position of p at $t+\varepsilon$ is no part of characterizing the motion of p at $t+\varepsilon$. If we do not assume this, then, of course, the 'motion' of p at $t+\varepsilon$ *will* depend on its position at t.[12]

Perhaps, however, this idea can be patched up. The controversy with the peripatetics was whether there could be motion without a corresponding net impressed force. (This is not to rule on 'inner' or 'inherent' forces.) The problem was whether a continued impressed force – e.g., the movement of the column of air moved by the hand in throwing the stone – was required for continued motion.[13] One might argue that, as a matter of empirical fact, net forces produce *accelerations*. Consider this passage from Galileo.

Furthermore, we may remark that any velocity once imparted to a moving body will be rigidly maintained as long as the external causes of acceleration or retardation are removed, a condition which is found only on horizontal planes; for in the case of planes which slope downwards, there is already present a cause of acceleration, while on planes sloping upward there is retardation; from this it follows that motion along a horizontal plane is perpetual; for, if the velocity be uniform, it cannot be diminished or slakened, much less destroyed.[14]

It is difficult to know just how Galileo meant this. But one fairly natural way of taking the passage, a way which has been curiously overlooked, is that it is an attempt to hoist the peripatetic on his own petard. Imagine a ball rolled down an inclined plane onto a plane everywhere normal to g. Once the slope ceases (i.e., becomes normal to g), there are no net external forces on the ball in the direction of motion, yet the motion continues. But, *cessante causa cessat et effectus*. Therefore, the motion is not an effect at all! The idea is that the only possible changes in the system – alterations of slope – will produce accelerations, whereas no change in the system argues no change in the motion. Since the motion continues in the absence of any available causes, it follows by the peripatetic principle that the motion is not an effect.[15]

Now the determined peripatetic might insist that in the situation imagined, the ball would in fact stop, since there is nothing to keep it moving, But it seems that the peripatetic conception can be refuted empirically as follows. In the case previously discussed of a block pulled up an inclined plane at constant velocity, the peripatetic theory requires the force exerted by the rope to exceed the net force in the opposite direction by some positive amount, say f. This a a measure of the force required to maintain a mass of the size in question at the velocity in question. Imagine now a ball of the same mass placed on a plane inclined at an angle A from a plane normal to g such that the resultant of g and the force exerted on the ball by the surface equals f. According to the peripatetic theory, this ball should move with a uniform velocity v down the plane. But in fact, for any detectable angle, and reasonably large and heavy balls, there will be a detectable constant acceleration right through v (which is fairly small in this case). One might be tempted to reply that this glibly assumes that the direction of g can be established independently of the sort of experiment in question. Now it is true that we cannot in this context establish that a plane is normal to g by observing the behavior of a ball placed on it. But we need not do this. We may utilize a plumb, or place a spring balance on the plane and weight a known weight, or use a level.[16]

Generalizing Galileo's argument (as I have construed it) we seem to have the following principle: Given a system s, we may not choose a set of state variables such that there is some interval during which only one variable changes. If this condition is not met, then one of three things must hold: (1) the system is not closed, (2) the set of state variables is not complete, i.e., there is a 'hidden variable', or (3) the changing variable is spurious, i.e., changes in the value of the variable do not represent changes in the sort of state under consideration. In the Galilean thought experiment, regarding position as a state variable leads to a violation of the principle, and since explanations (1) and (2) are implausible – the thought experiment is constructed so as to yield this result – it is concluded that position is a spurious variable.

This principle, which I will call the Isolated Variation Principle (IVP), is a dressed up version of the peripatetic principle (*cessante causa cessat et effectus*) which Galileo's argument (as I have construed it) turns against the peripatetic account of continued motion. It is thus a principle *shared* by Galileo and his peripatetic opponents. This is important: he wants to establish that uniform motion is not subject to direct causal explanation, and so must be treated as a mechanical state. If his argument is to amount to more than the bald assertion that uniform motion is treated as a state in his theory, he must appeal to a principle the satisfaction of which is a desideratum of all parties to the dispute. Seen in this way, Galileo's claim is that the peripatetic theory is not consistent with a universally received condition of good direct causal explanation.

I have been arguing that there can be a real as opposed to merely verbal dispute over whether unaccelerated motion should be represented as a state. Representing unaccelerated motion as a state commits one to the view that mere change of position is not an effect. This is at least partly an empirical matter, for it is at least partly an empirical matter whether suitable correlative causes can be found. Specific candidates for the correlative cause may be dismissed on empirical grounds, as Galileo dismissed the peripatetic account of the continued motion of projectiles, and empirical considerations can be brought forward to support a general rejection of correlative causes, as Galileo does (if I understand him correctly) in the argument lately rehearsed. There were, at one time, good empirical grounds for treating unaccelerated motion as a natural state requiring no direct causal explanation, for there were good empirical grounds for thinking that this was the only way to construct a theory satisfying IVP.

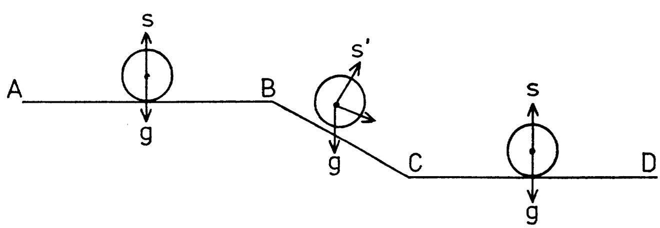
It might seem that our old problem concerning how to distinguish between enlightening re-orientations and putting on blinders will simply re-arise here, for what is to prevent someone from always taking the 'spurious variable' line? Can't we always satisfy IVP by throwing away enough information? And, so if, doesn't this show that satisfying it one way rather then another is really a matter of choosing a convenient convention?

Fortunately, *calling* a variable 'spurious' isn't enough to *make* it spurious. Throwing away information may seriously weaken a theory in either of two ways. (i) There may be other cases besides the one creating the problem in which the crucial variable is needed to explain something. In situation *A*, it seems that only *x* varies. But in situation *B*, variations of *y* seem to be best explained as consequences of variations in *x*. Bringing *A* to heel by throwing out *x* will simply create the same problem *vis-à-vis* *y* in situation *B*. Of course, we could throw out *y* too, but eventually, perhaps immediately, (ii) throwing out variables may rob a theory of any interest. Someone whose only interest is the explanation of uniform motion will certainly be unimpressed by the classical gambit, and rightly so. Any approach which throws out most or all of the variations which constituted the motivating target explananda in the first place is bound to be rejected, not indeed as false, but as irrelevant. Thus it is that the installation of a state in place of what was thought to be a process or sequence of events is rightly regarded as revolutionary, and is correspondingly rare. This is simply a healthy unwillingness to take nothing for an answer, even though it is sometimes the right answer.

Actually, far more serious problems arise in connection with the other two alternatives: how are we to rule out hidden influences from within and without? This is a large question, and I will only touch on one aspect of it that bears directly on the law of inertia.

Gravitational Forces as Hidden Variables

Consider again a ball rolling along a level surface and then down an incline to another level surface. Along the incline *BC* we have a net force on the ball along the surface which is the resultant of g and s', the force exerted by the surface on the ball. Assuming g is constant, this force will be constant as well, and the ball will accelerate uniformly along *BC*. Now



what accounts for this change in velocity? The standard answer is that it is a net force on the ball as it rolls along *BC* which accounts for the change in velocity. But this force does not change during the interval (t_B, t_C) in which the ball traverses *BC*. Indeed, there is no continuous change in the system during (t_B, t_C) to which we could refer the continuous change in velocity. On the other hand, the net force does change at t_B and again at t_C, but these events correspond to changes in the acceleration of the ball. Application of IVP here suggests that only changes in acceleration can be taken as effects: the cause cited for the continuous change in velocity during (t_B, t_C) appears no more real than the cause the peripatetic theory required for continuous change of position. It might seem, then, that we should suppose a body experiencing uniform *acceleration* is in a mechanical state and regard uniform *velocity* as merely a special case $(a=0)$.

However, the matter can be taken a step further. Actually, there will be an increase in acceleration during (t_B, t_C). The law of gravitation tells us that this is due to a corresponding increase in g. But this is only inferred from changes in acceleration on the assumption that force is directly proportional to acceleration. Why not suppose instead that the only genuine changes of state are deviations from 'gravitational' acceleration, since these are what correlate with the changes at t_B and t_C?[17] On this supposition, the mechanical state of a small mass in the vicinity of the earth is given by $a = \mathrm{km}_e/d^2$. (More generally, the mechanical state of a body of mass m in a two body system is given by $a = \mathrm{kmm}'/d^2$.) From this point of view, genuinely uniform acceleration between B and C would be *unnatural*; something (air resistance? friction?) must be inhibiting the motion of the ball.

This is substantially the suggestion developed by Ellis to show that the choice of uniform rectilinear motion as the mechanical *status quo* is conventional. The suggestion is applauded by Hanson who takes it as an improvement, though apparently agreeing with Ellis' contention that the issue is one of choosing conventions.

I agree with Hanson that this is an improvement, but I think IVP explains why. Gravity is very real, but as a cause of motion it has always had an overcooked smell. Relativity recognizes this by treating universal gravitation as a universal condition, and not, except figuratively and anachronistically, as a cause. Hanson puts the matter well.

> *Paths of body pairs, then, will be geodesic toward each other in space*. Thus planets will move along geodesics quite naturally when their associated spacial frameworks have been determined by very large masses, such as our sun. The door to general relativity is now well ajar. The paths of planets are no longer to be explained in terms of fundamental forces and dynamical laws – ghosts in the celestial machinery. Rather it is simply a kinematical fact that small bodies will move along geodesic paths through spaces determined by large bodies.[18]

There are lingering suspicions, of course. One concerns the motion of the ball on the surfaces AB and CD: it is *un*accelerated along those surfaces, so doesn't the new perspective require a cause?

Assuming AB and CD are normal to g, there will, of course, be no acceleration along the surface. Now the new perspective says that the ball is in a mechanical state when $a = \mathrm{km}/d^2$. This gives the magnitude of a along lines connecting the gravitational centers of the ball and the earth. Since d^2 remains constant, we must infer a force exerted by the surface to account for *this* deviation from the *status quo*. But since the surface is everywhere normal to the natural acceleration, we should *expect* no component along the surface, so we need find no cause to explain the observed uniformity of speed along that surface. Galileo's thought experiment falls out as a special case. And the issue retains its empirical cast:

given that we want our theories to satisfy IVP, it is an empirical question which of two competing theories fares better in this respect.[19]

IV

A decision about how to characterize – a choice of state variables – imposes distinctions between states and non-states, and hence determines what is and what is not construed as an effect. Such decisions are not arbitrary, in part *because* they have this consequence. Effects require direct causal explanation; if there is none to be had which satisfies, then we shall alter our taxonomy. This sounds like metaphysics, and indeed it is. But we can give it a domestic appearance: the goal of good theorizing is to codify and increase understanding; the IVP simply records one condition under which there will be a willingness to abandon a theory on the grounds that it does not conduce to this basic goal. All this presupposes a certain explanatory strategy which is the essence of classical mechanics and its kin. Though a detailed analysis requires the formulation and discussion of its normative principles – e.g., IVP – this strategy is easy to state in outline: what requires explanation is change, and changes are to be explained as effects, the trick being to characterize matters in a way which makes this possible, i.e., in a way which distinguishes genuine changes from states.

This strategy seems to be breaking down at the quantum level. The pressure has produced two schools of thought. There are those who would save the strategy, by opting for one of the alternatives provided by IVP. This school has two factions. The hidden variable group looks to (1) or (2): there are hidden influences. Others argue that the best alternative is (3): we are conceptualizing the matter in a way which makes us see changes where there are none. But there is another school of thought which holds that the basic strategy needs junking. If our most satisfying theories do not satisfy the old constraints, then those constraints are dead. After all, they were supposed to be constraints on satisfactory explanation. Should this school of thought prevail, a revolution will have occurred which runs far deeper than that effected in the 16th and 17th centuries. For Galileo's revolution was a revolution 'under a principle': in some form or other, the IVP was a common court of appeal for both parties. That principle has a normative force precisely because it does (or did) describe our disposition to abandon certain theories as unexplanatory. Its overthrow would mean we no longer had the disposition it describes. Such a change might be rationally engineered, but it has a strange 'feel' to it: reactionaries are likely to complain that it is cheating to produce intellectual satisfaction by making people more easily satisfied.

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NOTES

[1] Richard Westfall, 'Circular Motion in Seventeenth Century Mechanics', *Isis* **63** (1972), 184.

[2] Westfall cites Brian Ellis ('The Origin and Nature of Newton's Laws of Motion', in R. G. Colodny, *Beyond the Edge of Certainty* (Englewood Cliffs, N.J.: Prentice Hall, 1965), pp. 29–68) and Dudley Shapere ('The Philosophical Significance of Newton's Science', *The Texas Quarterly* **10** (1967) 201–15) as examples of philosophers who have advocated this view.

[3] This is how Shapere sees the matter (*op. cit.*, p. 205). Ellis is much less clear about this. He sticks to a formulation in terms or causal explanation, but stipulates that "the behavior of a given system is considered to require causal explanation if and only if we feel that this behavior is not sufficiently explained by its subsumption under a law of *succession*, "where" a law of succession is any law that enables us to predict the future stages of any system (or given class of systems) simply from a knowledge of its present state, assuming that the conditions under which it exists do not change." (Ellis, *op. cit.*, p. 45.) As examples of laws of succession, Ellis mentions the law of radioactive decay, the law of free fall, and Kepler's laws of planetary motion. All of these laws apply to cases in which we would ordinarily think to causes as being very definitely at work. Apparently, Ellis is thinking that none of these laws by itself *sufficiently explains* the behavior ıt applies to. Perhaps this is true. But it seems much more helpful to ask whether free fall is caused, then to ask whether it is 'sufficiently explained' by the free fall law.

[4] A more precise account of the notion of completeness involved here would be desirable. The natural suggestion is this: given a closed system s, the state of s at any moment t can be exhibited as a function of its state at any previous moment. Sympathetically read, this is not too bad. But there is one fundamental problem at least which even sympathy will not dispel; what counts as a closed system? The inevitable answer seems to be that a closed system is one in which all changes of state are internally caused, but this will return us in a circle. In practice, what happens is that conditions are identified under which a given set of state variables can be expected to represent only changes generated by other representable changes. These conditions are then taken to define 'closed' for systems characterized by those variables.

[5] This seems to be Shapere's line. Cf., *op. cit.*, footnote 22, p. 214.

[6] Notice that we needn't endorse any reductionist talk here. We *might* say that sigma turned out to be a process, meaning, at least in part, that the $v_i(s)$ do not provide the most fundamental characterization of s. But we might say instead that the processes occurring at the micro-level explain why s remains in a certain state. Thus the fact that a gas at constant temperature in a closed container exerts a constant pressure – i.e.,

is in a state relative to temperature, volume and pressure – is explained by the fact that the molecules are involved in certain processes.

[7] If we thought of gasses as continuous substances, then *changes* in volume rather than stability would be the problem: how can a continuous substance expand or be compressed? What is diffusion?

[8] Of course, the mechanical state of a *system of bodies* is not completely characterized by specifying the momentum of each body. Relative positions, at least, are required. The state of a system is not, in general, fixed by giving the state of each component.

[9] Definition IV in Newton's *Principia* reads: "An impressed force is an action exerted upon a body, in order to *change its state*, either of rest, or of uniform motion in a right line." (My emphasis.)

[10] Lucretius seems to have been confused on this point. He held that the natural tendensy of every body was to move straight downward with a certain fixed velocity (the same for each body). Thus, were every body to assume the natural motion, no collisions would occur. Since they obviously do occur, Lucretius introduced a random swerve in the natural motion. Now a swerve could cause a change of state in the universe from a state in which all bodies have their natural motion, to a state in which collisions occur. But Lucretius apparently held that there was no time in which all bodies had their natural motion. If so, then there is no event here which needs a cause to explain it. Cf., *De Rerum Natura*, Bk. II, lines 217–251, and 294–308.

[11] Such a phrase would have seemed a near contradiction to the peripatetics who boggled at the idea of motion without change.

[12] Position may, of course, be predicted, for it is determined by antecedently specifiable factors. But a body's coming to have such-and-such a position is not an effect, except in an indirect sense. To see this, it is enough to remember that a body may predictably assume a certain position precisely because no causes operate at all. When we do treat position as an effect it is either because we are treating a certain trajectory as an effect, and that trajectory includes, as it happens, the position in question, or because we are treating a *system* of bodies, the state variables for which include relative positions of constituent parts.

[13] Cf., Galileo, *Dialogue on the Great World Systems*, The University of Chicago Press, Chicago, 1953, pp. 163–65.

[14] *Dialogues Concerning Two New Sciences*, Dover, New York, 1914, p. 215.

[15] A good deal is made sometimes of the apparent fact that Galileo thought of uniform circular motion as the relevant natural state. This may be true, but it does not effect the argument as I have interpreted it. The point is that motion continues with uniform speed when there is no net impressed force in the direction of the motion. For the purposes of this argument, any surface everywhere normal to g will do as well as any other.

It seems to me unlikely in any case that Galileo thought of uniform circular motion as free of all forces, however *natural* he thought it was. He certainly did think of it as unaccelerated, but this is harmless in the present context which concerns only accelerations in the direction of motion.

[16] Of course, the peripatetic has not quite exhausted his supply of objections yet. He might complain that the accelerations being measured are only accelerations relative to the earth. But *that* is what the dispute was about. Or he might hold that the required maintaining force is so small that a ball on a plane at a detectable angle from a plane normal to g will experience a greater force than one simply required to maintain uniform speed on the plane. He could say this. But who would listen?

[17] The distance *h* between the ball and the earth varies with the acceleration *a*, and it might be suggested that changes in *h* cause changes in *a*. This seems intelligible only on the assumption that changes in *h* correspond to changes in the gravitational force f_G. But, as Ellis points out (*op. cit.*) changes in f_G are not independently accessible.
[18] N. R. Hanson, 'A Response to Ellis's Conception of Newton's First Law', in R. G. Colodny, *op. cit.*, p. 71.
[19] Under this new perspective, some of the peripatetic's questions regain their cogency, e.g., what keeps a projectile or satellite from assuming its natural motion? And certain peripatetic answers turn out right, e.g., a net force *is* requred in the case of the block being pulled up an inclined plane at uniform velocity.