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THE METAPHYSICS OF VELOCITY

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ABSTRACT. Some authors have recently argued that an object's velocity is logically independent of its locations throughout time. Their aim is to deny the Russellian view that motion is merely a change of location, and to promote a rival account on which the connection between velocities and trajectories is provided by the laws of nature. I defend the Russellian view of motion against these attacks.

Most physicists and philosophers believe that an object's velocity is a simple function of where it is located at what time:

Velocity Principle (VP): An object's velocity can be identified with the first time derivative of its trajectory.

However, a number of authors have recently argued in favour of a rival account. Following Michael Tooley, they claim that an object's velocity is logically independent of its locations throughout time.[1] They admit that VP is true, but insist that this is only thanks to the way the laws of nature are. Given different laws, velocities and trajectories could be related in a way that makes VP false.

The ultimate target of the attack on VP is Bertrand Russell's (and everybody else's) view that motion is nothing more than a change of location:

> Motion consists merely in the fact that bodies are sometimes in one place and sometimes in another, and that they are at intermediate places at intermediate times. (1917, p. 84)[2]

If Tooley and his followers are right, then some facts about motion are not determined by where an object is when, and the metaphysics of motion is more complicated than Russell wants to admit.

My aim in this paper is to defend Russell's account against these attacks. In doing so, I shall assume that his opponents have to shoulder the burden of proof. If an object is first located at one point, and then at another, it has moved. Conversely, it is only true that

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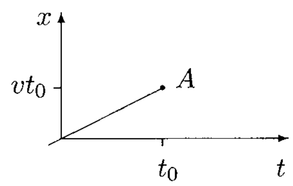
an object has moved if it has undergone such a change of location. Occupation of different places at different times is thus both necessary and sufficient for motion. It is incumbent upon Russell's critics to explain why we should think otherwise.

1. NON-DIFFERENTIABLE TRAJECTORIES

John Carroll (2002) presents the following counterexample to VP. Suppose an object A is moving with constant velocity v in one spatial dimension. It exists from the dawn of time until t_0, when it spontaneously goes out of existence. We can describe the motion of this object in terms of its *trajectory*, which is the partial function x_A that assigns the object's position to each time at which it exists:

$$(1) \qquad x_A(t) = \begin{cases} vt & \text{if } t \leq t_0 \\ \text{undefined} & \text{if } t > t_0 \end{cases}$$



Everybody agrees that the object has velocity v at all times before t_0. The question is what to say about its state of motion at t_0. Since x_A is not defined for any later times, it is not differentiable at t_0, and VP entails that A has *no* velocity at that time. Carroll thinks this is false. He regards it as obvious that A has the same velocity at t_0 as it has at all earlier times: v.

The trajectory is not differentiable at t_0, but it does possess a *left derivative*, which is the limit of its ordinary derivative as t approaches t_0 from below. The left derivative at t_0 indeed equals v, but Carroll argues that we cannot solve the problem by identifying velocities with left derivatives. If we did, we would run into problems with the mirror image of (1), where the object spontaneously comes into existence at t_0, and moves with constant velocity v ever after. In that case, the same reasoning would lead us to identify the object's velocity at t_0 with its *right* derivative.

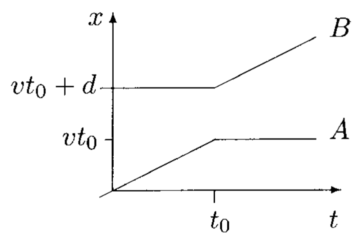
If a function is differentiable at a point then its left and right derivative both exist and are identical. In light of this, one might want to propose that an object's velocity is its right *or* its left derivative – whichever exists. But that can't be right, either. Suppose the object A continues to exist after t_0, but is no longer moving:[3]

$$(2) \qquad x_A(t) = \begin{cases} vt & \text{if } t \leq t_0 \\ vt_0 & \text{if } t > t_0 \end{cases}$$

In this case, the trajectory has both a left and a right derivative at t_0, but they are different. The left derivative equals v and the right derivative equals zero.

To make example (2) a bit more realistic, suppose that A is a solid sphere with diameter d. At time t_0, it elastically collides with a qualitatively identical sphere B that was initially at rest. According to classical mechanics, the trajectory of A's centre of mass is then given by (2), and that of B by:

$$(3) \qquad x_B(t) = \begin{cases} vt_0 + d & \text{if } t \leq t_0 \\ vt + d & \text{if } t > t_0 \end{cases}$$



Since both trajectories are non-differentiable at t_0, the challenge is to say what the two spheres' states of motion are at that time.

Carroll's claim is that neither VP nor any Russellian modification of it can give a satisfactory answer to this question. His counter-proposal is to abandon VP, and to claim that an object's velocity is logically (but not nomically) independent of its trajectory.[4] What velocity A and B have at t_0 is claimed to depend on what the laws of nature are. Let us say that a possible world is *fast* if the laws in that world are such that an effect is always present at the time of interaction t_0; a world is *slow* if the effect is present immediately *after*, but not at, t_0. In a fast world, Carroll tells us, A would have velocity v at t_0, and B would have velocity zero. In a slow world, matters would be the exact opposite.

One consequence of this view is that classical mechanics, even if true, would underspecify what the laws of nature are. The trajectories (2) and (3) are what we get by solving the equations of motion for the two interacting spheres. Since that is *all* classical mechanics gives us, a world in which classical mechanics is true could be either slow or fast; there's no way to tell.

Carroll does not claim that VP is *actually false*. Given the way the laws of physics are, there are no perfectly elastic collisions of type (3), no discontinuous processes like (1), and the problem cases never arise. The point Carroll wants to make is that there *could* be such interactions and that that shows that VP is not an acceptable analysis of 'velocity'. The connection between trajectories and velocities would thus have to be more complicated than the Russellian admits, and Carroll offers his own account as a proposal for what we might put in place of VP.

Carroll's complaint about VP was that any acceptable account of velocity has to entail that in example (1) the object A has velocity v at the last moment of its existence. VP falls short of this requirement, but the same is true for Carroll's own view. If the connection between velocities and trajectories is not part of the meaning of 'velocity', but something that needs to be provided by the laws of nature, then it does *not* follow from A's trajectory before t_0 what velocity it has at t_0. Its velocity could be anything you like – it all depends on what the laws are. In particular, there could be *gappy* worlds in which the laws fail to ensure that every object *has* a velocity at every time of its existence. It is therefore compatible with Carroll's view that the laws are such that VP gives the *correct* account of the examples (1) and (3).

It is thus not apparent why we should ever have accepted Carroll's initial claim that it is *obvious* that A has velocity v at t_0 in example (1). According to his own view, A's velocity at t_0 depends partly on the highly non-trivial, and empirically undecidable, question of whether the world is slow or fast. Yet if it is merely *unclear* whether A has a velocity at t_0, and what it is if it does, then we don't have a problem for VP. It is not an argument against an analysis that it legislates unclear cases – that is part of what an analysis is supposed to do. A counterexample to VP would be a *clear* case where an object's velocity is not what VP says it is. Carroll does not provide such an example.

Still, one might feel some force in Carroll's claim that VP's consequence that *A* and *B* are neither at rest nor in motion at t_0 is counterintuitive. I am inclined to agree that more needs to be said about this case, but I think that Carroll is wrong in supposing that this requires tinkering with VP. Let us distinguish an object's *left velocity* (defined in terms of its left derivative) and its *right velocity* (defined in terms of its right derivative). If an object has a non-zero left velocity then it has just completed a change of location; if it has a non-zero right velocity then it is about to begin a change of location.[5] As Carroll's examples show, an object can be left moving without being right moving, in which case there is nothing that can be said about its velocity *tout court*. It only makes sense to speak of a momentary state of motion if left moving is always accompanied by right moving, and that need not be the case. Only if an object moves on a differentiable trajectory does its left velocity always coincide with its right velocity, and only in this case can we speak of its velocity simpliciter.

Hence I think that Carroll got it backwards: what needs to be given up in non-differentiable cases like (1) and (3) are momentary states of motion, not VP. All that can and needs to be said about *A*'s state of motion at t_0 in example (3) is that its left velocity (defined in terms of its left derivative) equals v, and that its right velocity equals zero.

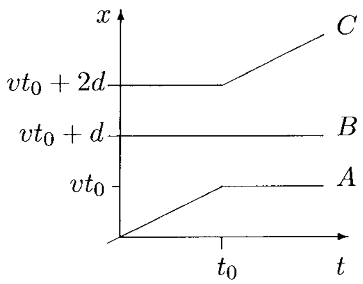
To inquire about its velocity simpliciter is to fail to appreciate that talk about instantaneous states of motion presupposes differentiable trajectories. It is *correct* to say, as VP does, that the spheres *A* and *B* have no velocity (simpliciter) at t_0.[6]

2. NEWTON'S CRADLE

John Bigelow and Robert Pargetter (1990) raise a different objection to VP. They claim that there are cases where the trajectory is differentiable, but where its derivative does not equal the object's velocity.

Their example is a version of Newton's Cradle. It can be obtained from our earlier example by adding another sphere, *C*. Initially, *B* and C are at rest with locations $vt_0 + d$ and $vt_0 + 2d$, respectively. (Since all spheres have diameter d, this means that B and C touch.) Now suppose that, at t_0, A hits B. After time t_0, spheres A and B are then at rest while C moves with constant velocity v:

$$(4) \qquad x_A(t) = \begin{cases} vt & \text{if } t \leq t_0 \\ vt_0 & \text{if } t > t_0 \end{cases}$$
$$x_B(t) = vt_0 + d$$
$$x_C(t) = \begin{cases} vt_0 + 2d & \text{if } t \leq t_0 \\ vt + 2d & \text{if } t > t_0 \end{cases}$$



Since the middle sphere B never changes its position, VP entails that it has zero velocity at all times. Bigelow and Pargetter disagree:

> The velocity of A is transferred from A, through B, to C. There is a moment in time when B has velocity v – even though there is no appropriate time series of past or future positions for B which will yield velocity v as a limit. (1990, p. 67)

What is assumed here is that in an interaction *velocity* has to flow from one object to another. But this "velocity transfer theory" is neither a theorem of classical or relativistic mechanics, nor is it a conceptual truth about velocity. As any competent physicist will tell you, it is simply false. What is true is that there is an *energy* flow through B at t_0. But since that is a flow of potential energy, and not of kinetic energy, this does not support Bigelow and Pargetter's case.

Given the way the laws of physics are in the actual world, the velocity transfer theory is false. In other worlds, the laws are different, and in some of them the theory might be true. This much we can grant Bigelow and Pargetter. What they need for their argument, however, is a world in which the velocity transfer theory is true and in which (4) correctly describes the motion of the three spheres. But that the Russellian will deny. For the Russellian, case

(4) is a *counterexample* to the velocity transfer theory: since the sphere *B* never changes its location it always has zero velocity, and there is no velocity-flow though it.

To assume that the velocity transfer theory and (4) can both be true is to assume the very point that the Russellian denies. The claim that sphere *B* has velocity v at t_0 is thus either based on bad physics, or it is begging the question. Hence there is no reason for the Russellian to reconsider his position in light of example (4).

3. VELOCITY AND EXPLANATION

Let me now turn to two objections that are concerned with the explanatory role of velocities. The first one is due to Michael Tooley (1988), who asks us to imagine a possible world in which objects' positions are perfectly random, and where an object's location at one time puts no constraint on where it might be at any later time. In this world, the trajectories of objects tend to be highly discontinuous: they jump around. It might happen, however, that for some stretch of time an object's locations accidentally form a smooth curve. Tooley suggests that in this case the object has a differentiable trajectory, but no velocity:

> It seems to me that one is very hesitant to attribute a velocity in such a case, and I would suggest that the reluctance to do so derives from the feeling that the velocity of an object at a time should be *causally* relevant to its positions at later times. (1988, p. 244)

I disagree. What velocity explains, and what its causal relevance is, depends on what the laws of nature are. In the actual world, an object's velocity is a good guide to its positions at later times. Not so in Tooley's world: there the laws are different, and velocity is not causally relevant to an object's trajectory the way it actually is. But this lack of causal relevance does not show that objects don't *have* velocities in that world.

Many philosophers think that we ought to postulate only entities that are doing some explanatory work. Since velocities wouldn't explain anything in Tooley's world, one might take this as reason for denying that there are any. But this already assumes that velocities need to be postulated *in addition* to an object's trajectory, which is precisely what the Russellian denies.

Similar remarks apply to Bigelow and Pargetter's complaint that the Russellian view of motion cannot account for the explanatory role of velocities even in the fully deterministic case described by classical mechanics:

> [Russellian] velocity cannot explain an object's sequence of positions: we cannot say an object is now located to the right of where it was a moment ago because it was in motion a moment ago. (1990, p. 66)

It is a mistake to assume that the Russellian wants to *eliminate* talk about velocities in all contexts. There is no reason for him to do so. He does not need to deny that velocities are bona fide properties of objects; he only claims that they are functions of their trajectories. Neither does the Russellian disagree about what the laws of nature are. Yet it is only thanks to them that an object's velocity is relevant to its future position. There is no reason why the Russellian should not give exactly the same explanation as Bigelow and Pargetter: given the way the laws are, an object's present location is partially determined (and hence explained) by its previous velocity.

According to Russell, an object's velocity at a time only depends on its trajectory immediately before and after that time. His opponents deny this, and advocate a metaphysically more complicated relationship between the two. But this difference does not matter here, for the explanatory function of velocities is safeguarded by the role they play in the laws of nature, and not by their metaphysical status (as Bigelow and Pargetter seem to assume).

4. CONCLUSION

Tooley and his followers claim that the connection between an object's velocity and its trajectory depends on the laws of nature. I have argued that they have given us no compelling reason for accepting their view. What is true is that the causal and explanatory role of velocities is dependent on what the laws are, and that is a dependence they do not seem to appreciate enough. While the velocity principle might need some clarification in the non-differentiable case, the Russellian view itself strikes me as analytically true: necessarily, an object moves if and only if it changes its location.

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NOTES

[1] See Tooley (1988), Bigelow and Pargetter (1990), and Carroll (2002).
[2] See also Russell (1903, p. 473).
[3] This example is due to Mortensen (1985).
[4] Tooley (1988, p. 246) and Bigelow and Pargetter (1990, p. 70) defend the same view.
[5] The converse does not hold. An object can have zero left velocity even though it has just completed a change of location. As an example, consider

$$x_A(t) = \begin{cases} (t_0 - t)^2 & \text{if } t \geq t_0 \\ 0 & \text{if } t > t_0. \end{cases}$$

The left derivative of x_A at t_0 equals zero even though $x_A(\text{t}) \neq x_A(t_0)$ for all $t < t_0$.
[6] Frank Jackson and Robert Pargetter (1988) make a similar point, but conclude (incorrectly, in my view) that an object's velocity is *indeterminate* whenever its left and right derivatives disagree.

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