

Leibniz on Motion – Reply to Edward Slowik

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In his contribution to this volume of *The Leibniz Review*, “Another Go-Around On Leibniz on Rotation,” Edward Slowik critically discusses some aspects of my essay “Leibniz on Motion and the Equivalence of Hypotheses,” published in the previous volume of the *Review*. I am grateful for Slowik’s comments, and I appreciate the opportunity to reply to them here.

The project of my paper was to clarify Leibniz’s views on motion and his doctrine of the equivalence of hypotheses (EH), and to establish that, contrary to popular belief, Leibniz is not hopelessly confused about these matters. More specifically, in addition to shedding light on the grounds and meaning of EH by, among other things, examining in detail Leibniz’s main proof for EH and his theory of solidity, my concern was to show that Leibniz’s position on motion is not internally contradictory. Leibniz is both an absolutist *and* a relativist about motion, as the textual evidence suggests, but this is no problem, because he is an absolutist about motion *at the dynamical level of reality* and a relativist about motion *at the phenomenal level* (or, as I prefer to say, the merely phenomenal level). Call this reading ‘level interpretation’. In his critique, Slowik argues that “there are problems” with both of the two specific versions of my level interpretation that I consider in my paper (Slowik, 132). Not all of the problems discussed by Slowik are the kinds of problems that could raise doubts about the tenability of my readings *as interpretations* of Leibniz. Some of them call attention to difficulties with the views on motion that I attribute to Leibniz *as views about how to explain the phenomena*, including the question of how somebody who adopts General Relativity can meaningfully talk about uniform rectilinear motions, the problem of how to reconcile EH with the fact that Leibniz’s fundamental conservation law does not hold in all frames of reference, and the question of how Leibniz’s account of solidity could be further fleshed out in order to explain the seemingly mysterious communication between the different parts of a solid object—all of which are explicitly acknowledged and discussed in my paper. Although I might be more optimistic than Slowik about the prospects for satisfactorily addressing these problems in the framework of Leibniz’s natural philosophy—and in my paper I try to show that and how Leibniz could respond to them—I agree, as stated at the end of my essay, that one might deem these difficulties and the generally unpolished

character of Leibniz's theory of motion as sufficiently unattractive to pledge one's allegiance to Newton's theory instead. But, as I see it, the listed problems do not provide any grounds for denying that Leibniz endorsed the views on motion that I want to ascribe to him. Accordingly, I will not revisit these problems here but focus my attention on those problems identified by Slowik that potentially cause trouble for my interpretation *qua* interpretation.

Before considering Slowik's objections, two brief clarifications are called for. The first concerns Slowik's characterization of the two readings that I develop in my paper as saying "(1) that the structure of spacetime is Leibnizian at the phenomenal level and Galilean at the dynamical level, or (2) that it is Galilean at both levels" (Slowik, 131-32). This characterization is not quite accurate. Slowik's presentation of my readings is correct as far as their descriptions of the phenomenal level are concerned. Both of them agree that EH applies to the phenomenal level, but on what I call the 'strong relativist reading', EH is understood as General Relativity, and the spatiotemporal structure of the phenomenal level is identified with the structure of (so-called) Leibnizian spacetime, while on what I call the 'weak relativist reading', EH is understood as Galilean Relativity, and the spatiotemporal structure of the phenomenal level is identified with the structure of Galilean spacetime. But Slowik mischaracterizes how my readings describe the structure of the dynamical level. On both of these readings, the structure of the dynamical level is rich enough to support absolute accelerations *and* absolute speeds, which means that it must be richer than the structure of Galilean spacetime, in which absolute speeds are not well-defined. As indicated in my paper, there are good textual and systematic reasons for ascribing such a richer structure to the dynamical level—reasons, I might add, that cause problems for Slowik's own interpretation, according to which the proper and "privileged" structure in dynamical contexts is the structure of Galilean spacetime (Slowik, 134).¹

The second clarification concerns Slowik's claim that my weak relativist reading allows "two possible interpretations...the first being that there are accelerated motions at the phenomenal level but that EH only applies to inertial, non-accelerated motions at this level," the second being "that there are simply no rotational or accelerated motions at the phenomenal level at all" (Slowik, 133). Perhaps I did not express myself as clearly as I thought, so to remove any lingering uncertainties let me state again that, according to my weak relativist reading, the only motions that are physically possible at the phenomenal level are uniform rectilinear ones.² This thesis is not only well-supported textually, it also allows us to view

Leibniz as a (general) relativist about motion at the phenomenal level on the weak relativist reading, despite the fact that this reading ascribes the structure of Galilean spacetime to the phenomenal level. That is, even though, on the weak relativist reading, absolute motions (namely, absolute accelerations) are well-defined at the phenomenal level, it is still the case that all actual and physically possible motions at this level are relative, precisely because they are uniform and rectilinear, and because Galilean relativity obtains. To my mind, if there is any element of Leibniz's theory of motion that is close to non-negotiable on the basis of the texts, it is the doctrine that all motions are relative as far as the phenomena are concerned.³ Any adequate interpretation must accommodate this (general) relativism in some way.

Slowik's first objection to my strong relativist reading can be reconstructed as follows: If the structure of the phenomenal level were Leibnizian, rotations would be no problem for Leibniz. But Leibniz does recognize that rotations are a problem for him. Therefore, he cannot hold that the structure of the phenomenal level is Leibnizian (Slowik, 132). This objection can be answered rather quickly. Strong relativism does not entail that rotations are "not a problem," but merely that they, like all other motions, are relative. So, if an opponent, e.g., Newton, claims that the phenomena establish that rotations are not relative, it is incumbent on the strong relativist to tell a story about why this alleged counterexample to his theory is not a genuine counterexample at all. And this is exactly what Leibniz does; no surprise there.

Slowik's second objection seems to be the following: If Leibniz thought that the structure of the phenomenal level were Leibnizian, he would also have to think that in the case of the rotation of a body with respect to the "plenum" the assignments of motions and of the effects of the centrifugal forces must be "reciprocal" in the sense (I take it) that the phenomena are compatible both with ascribing the motions and force effects to the body and with ascribing them to the plenum. But, in cases like these, Leibniz always ascribes the motions and force effects only to the body, not the plenum. Therefore, Leibniz cannot hold that the structure of the phenomenal level is Leibnizian (Slowik, 132). Before evaluating this argument, I would like to voice a terminological quibble concerning Slowik's choice of the term 'plenum' in describing the scenario under consideration. Usually, 'plenum' is taken to refer to a certain kind of physical universe, namely, a universe that is chuck-full and contains no vacua. On this understanding, bodies are *part* of the plenum. So, in order to forestall potential confusions a better way of characterizing rotations of

bodies from a Leibnizian point of view would be to describe them as rotations with respect to the ambient medium, or, even better and more accurately, as rotations with respect to the center of mass frame (CMF) of the ether.

Regarding the second premise of Slowik's argument, I agree that in rotations of bodies with respect to (the CMF of) the ether, Leibniz attributes the *force effects* to the body, not the ether—and, indeed, it would be a problem if he did not do this, given that the empirical evidence clearly confirms this attribution. But I disagree with the claim that he attributes the *motion* to the body, not the ether. To be sure, in describing examples of rotational motions Leibniz usually says things like 'imagine a spinning rod', and not 'imagine the ether spinning around a rod,' but, of course, this is no evidence that he regards the description that the rod is spinning as literally true, and the description that the ether is spinning as literally false. It is merely a reflection of the fact that the first description is much simpler, which, according to Leibniz, explains why it is the one that is ordinarily used and why it is commonly treated as if true, even though, strictly speaking, it is only one among several equivalent hypotheses, as I explain at length in my paper.⁴ If we want to know what Leibniz really thinks about rotations, we have to look to those passages where he explicitly discusses their status. And, to my mind, the textual evidence leaves no doubt that he takes rotations and circular motions to be no exceptions to his general relativism about motion.⁵ This means that the second premise, as stated, is false. In order to be true, it needs to be reformulated as saying that in cases where a body rotates with respect to (the CMF of) the ether Leibniz always attributes the effects of the centrifugal forces only to the body, not the ether; and, in order for the argument to remain valid, the first premise needs to be adjusted accordingly.

So, the crucial question in evaluating the first premises becomes whether somebody who agrees that (a) in all rotational motions of a body with respect to (the CMF of) the ether the centrifugal forces only affect the body, not the ether, must reject that (b) all rotations are relative motions. A moment's reflection reveals that there would be a problem about holding both (a) and (b) only if yet another claim were assumed as well, namely, that (c) exhibiting the effects of centrifugal forces (or being acted upon by centrifugal forces) entails, or is a reliable indicator for being in absolute motion. Newton certainly endorses (c), but, on my view, Leibniz clearly does not. (c) is a non-trivial claim that requires an argument, or, at least, some kind of plausibility consideration in its support. As I read Newton, part of the point of his thought experiment of the lonely pair of rotating globes

in the *Principia* is precisely to make (c) plausible. Since, by assumption, there is nothing else but the two globes and the rope connecting them, the motion that gives rise to the centrifugal forces that tighten the rope can only be attributed to the globes-system. This, in turn, might be viewed as making it sufficiently plausible to assume that whenever a body exhibits effects of centrifugal forces, the body must be in absolute motion. But from Leibniz's perspective, this argument is not the least bit convincing. Given Leibniz's account of solidity, a world with nothing but two rotating globes connected by a rope is nomologically impossible (Jauernig 2008, 26-29). So, from Leibniz's point of view, there is no reason to accept (c), which means that he can admit (a), without having to give up (b). In other words, Slowik's first premise (in the original and the adjusted formulation) is false. To be sure, by admitting (a), Leibniz agrees that the presence of effects of centrifugal forces indicates that a special kind of motion is going on—but being special does not directly translate into being absolute. Rotations and circular motions of bodies are relative motions, namely, relative to (the CMF of) the ether, and they are special in that they give rise to centrifugal forces that, as a matter of law, only act on the bodies, not the ether, which is not generally true of all relative motions. In short, it is correct that the phenomena associated with rotations exhibit a certain asymmetry as far as the effects of the centrifugal forces are concerned, but this asymmetry by itself is not enough to destroy the relativity of the situation as far as the motions are concerned.

Slowik's criticism of my weak relativist reading targets the claim that there are no accelerated motions at the phenomenal level, which is implied by the thesis, discussed above, that the only physically possible motions at the phenomenal level are uniform rectilinear ones. Slowik objects that "there is very good evidence to accept real accelerations (i.e., non-instantaneous change in velocity) at the phenomenal level" (Slowik, 133). This evidence is said to consist in "Leibniz's proof of the *vis viva* law in the *Discourse on Metaphysics* (AG, 49-50)," which, Slowik claims, "relies upon the incremental build up of speed, i.e., their [sic] non-instantaneous accelerations" (Slowik, 133). Several comments are in order.

First, I am somewhat puzzled by Slowik's explication of the notion of real acceleration as 'non-instantaneous change in velocity'. Given that the acceleration of a body corresponds to the rate of change of its velocity, it is natural to think that a necessary condition for a body to be really accelerated over a period of time is that at every instant during this period one can attribute a non-zero rate of change in velocity to the body, which one might also put by saying that during this period

the body is undergoing a series of instantaneous changes in velocity. To my mind, a more helpful way of characterizing the difference between real accelerations and the ersatz-accelerations of a theory that allows only complex uniform rectilinear motions would be to say that the velocity changes over time involved in the former are truly *continuous*, while the velocity changes involved in the latter, although very small (or, even, as small as one wishes), are not truly continuous.

Second, in the discussion in the *Discourse* that Slowik refers to—which, incidentally, does not contain a proof for Leibniz’s law of the conservation of *vis viva*, strictly speaking, but an argument against the identification of (what the Cartesians call) quantity of motion with moving force, and, accordingly, against using the Cartesian measure of quantity of motion, mv , as a measure of moving force—Leibniz does not say anything that commits him to acknowledging the existence of real accelerations. The (rather familiar) argument proceeds by assuming that it takes the same amount of force to elevate a body A of one pound to a height of four fathoms as it takes to elevate a body B of four pounds to a height of one fathom, and by showing that, given Galileo’s law of free fall, the (Cartesian) quantity of motion that A acquires by freely falling four fathoms is different from the quantity of motion that B acquires by freely falling one fathom. Assuming that the force to elevate a body to a certain height equals the moving force that the body acquires by freely falling from this height, this shows that moving force and (Cartesian) quantity of motion must be different quantities. At no place in this argument does Leibniz assume or claim that the velocity of the two falling bodies changes truly continuously; he merely considers their speeds before the fall (both of them are at rest) and after the fall (A’s speed is twice the speed of B). To be sure, by making use of Galileo’s law of free fall Leibniz acknowledges that the velocity of the two falling bodies changes uniformly, but this does not imply that he must accept real accelerations. This leads me to my third comment.

Third, even if we grant Slowik that Leibniz sometimes appeals to incremental build-ups of speed in the derivations of his laws, it is not clear to me on what grounds Slowik takes this to be tantamount to admitting that there are real accelerations. Surely, a speed-build-up that grows by discrete increments is still an incremental build-up of speed. And, of course, this is exactly how somebody who rejects the existence of real accelerations conceives of, say, free fall, to use Slowik’s example, i.e., as consisting in a series of very brief (or as-brief-as-one-wishes) elements of non-accelerated motions with velocities that monotonically increase by very small (or as-small-as-one-wishes) increments from one element to the next such that in

equal time intervals the velocity changes by the same increment. I do not see why this ersatz-accelerated motion should not count as an ‘incremental build-up of speed’.

Fourth and finally, Leibniz never tires of reassuring us that the applicability of mathematical expressions to physical quantities is not jeopardized by the fact that the mathematical entities to which these expressions refer are truly continuous, while physical quantities are not. As long as all physical quantities sufficiently approximate being continuous, there is no problem.⁶ Similarly, Leibniz is comfortable appealing to infinitesimals, instantaneous increments, imaginary roots, and other mathematical entities in the derivations of his laws and of his measures of various forces, while at the same time insisting that he “wouldn’t want to claim on these grounds that these mathematical entities are really found in nature” and that he “only wish[es] to advance them for making careful calculations through mental abstraction” (*Specimen Dynamicum*, GM VI, 238, AG, 121). So, even if in some derivations of his laws Leibniz relies on the incremental build-up of speed, and even if this incremental build-up is represented mathematically as a truly continuous build-up, this still does not show that Leibniz accepts real accelerations in the physical world. In sum, I remain unconvinced by the evidence that Slowik presents for his claim that Leibniz is committed to the existence of real accelerations at the phenomenal level.

By way of concluding, allow me to say a word about Slowik’s alternative interpretation, according to which Leibniz holds that “EH allows different perspectives,” depending on whether or not forces are taken into consideration (Slowik, 134). If motion is considered dynamically, EH is to be understood as Galilean Relativity, corresponding to a Galilean spacetime structure; if motion is considered kinematically, EH favors no reference frames and “allows a Leibnizian spacetime interpretation” (Slowik, 134). I do not have space to discuss this proposal in any detail, but I would like to register that I do not consider it a friendly amendment to my level interpretation, despite Slowik’s characterization of his reading as a “potential prospect for upholding something like Jauernig’s suggestion” (Slowik, 134). The main motivation for my level interpretation is that it allows us to clear Leibniz from the charge of endorsing an internally contradictory position about motion without having to play down any of the textual evidence, which, to my mind, leaves no option but to conclude that Leibniz is committed to both relativism and absolutism about motion, understood as theories about what the physical world is like and what kinds of motions exist in it. According to my level interpretation,

the seeming conflict is no problem because Leibniz holds that there is a level of reality at which all motions are relative—a level that, incidentally, also includes forces, albeit only phenomenal (relative) ones—and there is another level of reality at which absolute motions exist. Slowik’s interpretation, by contrast, does not do justice to both Leibniz’s absolutism and his relativism. Slowik explicitly notes that the observation that “EH allows different perspectives, Leibnizian or Galilean, is not meant to be taken as supporting any sort of ontological or epistemological equality of these different structures,” and goes on to single out the Galilean structure as “privileged” (Slowik, 134). In other words, on Slowik’s view, only the Galilean perspective, which embodies Leibniz’s absolutism, captures what the physical world is really like, while the Leibnizian perspective, which embodies Leibniz’s relativism, merely captures what the physical world appears to be like when some crucial elements, namely, the forces of bodies, are intentionally ignored. As I understand Leibniz, he intends his relativism about motion to be much more ontologically robust than that.

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References

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Notes

- ¹ Slowik himself acknowledges that there are passages that are problematic for his reading (Slowik, 137, note 3).
² Slowik quotes me expressing this thesis right before he claims that my reading can be interpreted as saying that there are accelerated motions at the phenomenal

level, which flatly contradicts the statement he just quoted.

³ Cf. *Specimen Dynamicum*, GM VI, 247. Also see note 5.

⁴ Cf. *A Specimen of Discoveries*, A VI.4B, 1620.

⁵ Cf. *New System*, G IV, 486-87, L 459: "...no matter how many bodies one takes, one may arbitrarily assign rest or some degree of velocity to any one of them we wish, without possibly being refuted by the phenomena of straight, circular, or composite motion." Cf. *Phoronomus*, C 590; *Dynamica*, GM VI, 508; AG 125, note 173.

⁶ 'Reply to the thoughts...', G IV, 568-69, L 583: "I acknowledge that time, extension, motion, and the continuum in general, as we understand them in mathematics, are only ideal things... It is true that perfectly uniform change, such as the mathematical idea of motion, is never found in nature anymore than are actual figures which possess in full force the properties which we learn in geometry, because the actual world does not remain in this indifference but arises from the actual divisions or pluralities whose results are the phenomena which are presented in practice and which differ from each other down to their smallest parts. Yet the actual phenomena of nature are arranged, and must be, in such a way that nothing ever happens which violates the law of continuity... Although mathematical thinking is ideal, therefore, this does not diminish its utility, because actual things cannot escape its rules." Cf. Letter to Varignon, February 2, 1702, GM IV, 91-95.