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Rationalist Foundations and the Science of Force

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This chapter is a new account of natural philosophy in post-Leibnizian Prussia. Because mechanics was then natural science *par excellence* and my space here is limited, I restrict my view to foundations for mechanics from 1716 to 1786. Three conclusions emerge. First, mechanics in Enlightenment Prussia is far from Newtonian. Rather, its structure and basis are greatly fluid, and take decades to crystallize into three distinct paradigms; just one of them is meaningfully Newtonian. Second, Leibniz turns out to have influenced natural philosophy to a degree so far unappreciated. Third, in this age theoretical mechanics and its philosophy begin to drift apart, perhaps irretrievably.

I begin by presenting three views then dominant on the basic unit of matter (§ 1). Next, I explain the basic dynamical laws aiming then to unify mechanical phenomena (§ 2). Lastly, I examine some opposing views on inertial structure for Enlightenment dynamics in Prussia (§ 3).

The modern category 'German' is inapplicable to my area and cast of characters. They were not German citizens, nor did they publish just in German; some did not even speak it much.[1] Thus in selecting them I had to make a judgment call. I will examine here two philosophers, Wolff and Kant. The former deserves inclusion for his vast, original synthesis and his influence over a sizeable group of followers; the latter needs no justification. Both

[1] From a 1738 letter by Daniel Bernoulli for Euler, both Swiss: "Eben lese ich in dem Journal que Mess. Krafft et Delisle font presque tous les jours des expériences devant S[a] M[ajesté] I[mpériale]…. Vor ein Paar Tagen habe ich von dem Hr. König … ein programma gehalten, darin er den Geometris sieben problemata intra sex mensium spatium zu solviren proponirt. Die problemata sind profundae indaginis, und hat der Autor nicht wenig praestirt, wenn er sie alle recht solvirt."—Fuss 1843: 425f.

were fairly conversant with the exact science of their time, though as mechanics progresses Kant's grasp of it begins to lag behind.[2] Also, I present here some crucial contributions by Euler and Lagrange. Consecutively, each was for some two decades the supreme geometer at the Royal Academy of Sciences in Berlin. The Swiss developed much of what we know as Newton-Euler dynamics. Lagrange, who succeeded Euler after he moved to St Petersburg in 1764, used his long research sojourn in Berlin to prepare his 1788 masterpiece *Méchanique analitique*, the first non-Newtonian comprehensive theory of mechanics.

I limit my exposition to Prussia and its environs. Covering the western German states would take up space that I do not have here. And, I ignore here entirely the *vis viva* controversy. It has been over-studied, to the lamentable neglect of other, more important topics and results. Instead, and in the spirit of this publication, I will suggest and defend new lines of inquiry, by way of an opinionated introduction to my topic.

The upshot of my study is threefold. It should make us wary of assimilating 18th-century German natural philosophy with 'Newtonian science.' It justifies a call to arms: let us give more, careful attention to *Leibniz's* legacy for dynamics in the Age of Reason. Lastly, it makes a case for a new reading of Kant's natural philosophy: we ought to contextualize it to the science of his time rather than—as tradition has long had it—to Newton's *Principia*, a work that precedes it by about a century.

Ontologies for mechanics

In the 17th century an ambitious program, the mechanical philosophy, had set out to ground all science in basic facts about matter and motion, by explanatory reduction. Though Leibniz soon claimed that 'force' is more basic than extension and impenetrability—the mechanists' universal features of body—even he conceded that, once a mechanics has been articulated, all else must be reduced to it, as matter in motion.[3] Wolff and Kant, who fol-

[2] I cite Kant by volume and page number in Kant 1902-. Henceforth, '1' refers to Kant 1992, and '4' to Kant 2004. I mention his *Metaphysical Foundations of Natural Science* simply as '*Foundations*.' Unless otherwise noted, all translations are mine.
[3] Hertz too affirmed that ideal (1899: xxi): "All physicists agree that the problem of physics consists in tracing the phenomena of nature back to the simple laws of mechanics."

lowed him broadly in his 'dynamistic' account of matter, also took this program for granted:

> all corporeal action is from motion.... I acknowledge that all other phenomena of matter can be explained in terms of local motion. ([Leibniz] 1695: 145ff)
> The entire visible world is a machine..., a compound in which changes to its composition or structure always follow the rules of motion. (Wolff 1737: 66f.)
> The basic determination of something that is to be object of the outer senses had to be motion...., and so natural science, therefore, is either a pure or applied doctrine of motion. (Kant 4:477)

Implicit in the program was a shared assumption, which Enlightenment mechanics ultimately subverts. It was the thought that, when spelled out carefully, both 'matter' and 'motion' will be *univocal*. Namely, the unit of matter, whatever its final profile, will have a single kinematic structure and basic behavior, not multiple. However, as mechanics grew and philosophy strove to give it an ontology, 'matter' and 'motion' came to denote not one but three, wholly *distinct* species. I will put the point in modern terms, to see it clearly, and then illustrate it with examples from the time and place of my topic.

From Descartes onward, mesoscopic bodies were supposedly made up from components at subvisible scales, viz. the unit of matter. (Many called it a 'corpuscle' then.) Speaking anachronistically, these units come in three kinds: the mass point, the rigid body, and the deformable continuum. Now, these kinds are *irreducibly* distinct. In the first, mass is located at a point; in the other two, it is distributed over a volume, finite or infinitesimal, respectively. 'Motion' too has essentially different senses for these three kinds. A mass point can only translate, along the three direction of Cartesian space. In contrast, a rigid body can translate *and* rotate, around internal axes. And, a volume element (in a deformable continuum) can translate, rotate *and* deform.[4] Impenetrability is radically unlike for these ultimate objects. Mass points: no two points can overlap, or become superimposed. Deformable continua: they can overlap at a point, line or area (viz. their contact surface) but not over a finite volume. Rigid bodies: no overlap by a finite volume; and no volume previously taken up by one can become occupied by another. (*Sed*

[4] In our terms, a mass point has three degrees of freedom, a rigid body has six, and a deformable continuum has an infinity.

contra, a deformable continuous body can be compressed, or made to yield some of its 'space' to another body.)

Moreover, these units interact with their own kind and make up bodies in distinct, specific ways. Mass points exert *only* action at a distance—repulsion and attraction.[5] In this ontology, a body is a lattice of them in equilibrium configurations, i.e. at relative positions where their mutual forces balance each other. In turn, microscopic rigids exert body forces, like gravity; and also *contact* forces and torques.[6] They connect to form visible bodies either by internal pins and joints; or by external force closure, such as the pressure of an ambient fluid (traditionally, some imponderable, e.g. the ether). Lastly, deformable continuous bodies come pre-constituted. Volume elements dV, the unit of matter in continuum mechanics, are just potential parts. Each is the limit to which an 'Euler cut,' or arbitrary finite volume ΔV in the body, shrinks. Unlike rigid bodies, deformables bear stresses, i.e. *internal* contact forces (between parts).

These basic objects differ radically in all relevant respects. (i) Matter: mass points are discrete and zero-sized, rigid bodies and deformables are finite continua. (ii) Motion: deformation is meaningless for a rigid or a mass point. Spin is meaningless for mass points, but well-defined for the other two basic objects. (iii) Force too: internal stresses are meaningless for rigid bodies; and contact forces and torques are meaningless for mass points. And so, there is no prospect of reducing two of them to the third.

The better thinkers in our group are very much sensitive to these differences. For instance, Kant knows to distinguish "physical contact," or exercise of repulsive force, from "mathematical contact," i.e. kinematic overlap; "penetrating" from "surface" actions, i.e. body forces from contact forces; and "relative" from "absolute" impenetrability, viz. resistance to compression from rigidity. Euler seems even more acutely aware of the mathematical and physical differences between these basic entities.[7]

[5] In their case, 'contact action' is really a misnomer: just convenient shorthand for short-range repulsive acceleration.

[6] In continuum mechanics, a body force is one that acts directly on any point inside an extended body. (For instance, gravity or electromagnetism.) A contact force acts on the boundary of that body, thus indirectly—through the transmission of stress—on any point within it.

[7] E.g., he writes M, a finite number, for the mass element when he treats a body as a set of mass points; and dM when he regards the body as continuous; cf. Euler 1765b versus 1752. Likewise, he knows that the basic laws of rigid bodies or of elastic solids cannot be derived from the laws governing mass points; cf. Euler 1771 and 1776.

Foundations. All three ontological units are the explicit object of Enlightenment natural philosophy. Prussia was well at the forefront, where philosophers called these units respectively 'physical monad,' 'hard body,' and 'infinitely divisible matter.' Let us survey the academics first. Some have Boscovich be the author of modern mass points, but this entity really has two fathers. Kant in the 1750s espoused it too, though not without some ambivalence. In *Physical Monadology*, he mixed uneasily two pictures of matter. When properly purified, one of them—the official view, advocated early in the essay—is a doctrine of mass points:[8]

> Elements fill their determinate space by a certain activity that prevents other bodies from penetrating it.... The force of impenetrability is a repulsive force.
> In addition to the force of impenetrability, every element needs another force, that of attraction....
> There must be some point on the diameter where attraction and repulsion are equal. This point will determine the limit of impenetrability and the orbit of external contact; that is to say, it will determine the volume.
> The force of inertia of a body (which is called its mass) is the sum of the forces of inertia of all the elements of which it is composed. (1:482, 484, 485)

Thus, a physical monad, or Kantian 'element,' is inert, acts only by central forces (of repulsion and attraction), its mass is concentrated at a point, and its volume is really just an acceleration field.

As to the deformable continuum, it had long been Leibniz's official theory of matter (at least in public, in his later years). Wolff too takes bodies to be continuous, which he supports by a variety of considerations. One is a direct argument. A body is an "aggregate of elements," and material aggregates "are extended," and extended matter is continuous (1737: 169ff). Unfortunately, little is clear about the structure of "elements," the constituents of Wolffian bodies.[9] Each is endowed with two (purely qualitative, but not mentalistic) forces, "active" and "passive." Elements interact, but that mechanism is mysterious. Wolff infers that they must be partless and indivisible, for they are the simples that ground material composites (146). As they can-

[8] Many have long credited Kant's essay with a single, mass-point view; Holden 2004 is paradigmatic. Smith 2013 argues conclusively that the *Physical Monadology* oscillates between two theories of matter, not one. For the context of Kant's paper, see Leduc (forthcoming).
[9] He also called them, before Kant, 'physical monads,' to signal discreetly his departure from Leibniz. Cf. Wolff 1737: 148. Watkins 2006 is an acute discussion of how Wolff differs from Leibniz in regard to metaphysical 'simples.'

not be mass points, we might be tempted to assimilate them with volume elements dV, the element of integration in continuum mechanics. However, this is all conjectural; Wolff writes well before Cauchy clarified the physical continuum, so his account perforce is fuzzy. Eventually, he claims that elements stand in the relation 'outside each other,' *extra se invicem*, and thereby can make up continuous bodies by "aggregation," an operation left unexplained. This vexed problem soon spawned a family of post-Leibnizian monadologies, which then created its own opposition.[10]

Wolff hoped to draw more support for his ontology of matter from objections to competing views. The paradigmatic early-modern rigid body was the 'hard' atom, and he rejects it out of hand. Though tiny, atoms are extended, and so they have shapes. But, atomic shape "lacks a sufficient reason why it inheres in its subject," and thus it is an "occult quality," hence natural philosophy must banish rigid atoms (1737: 149). As for mass points, Wolff never bothered to dismiss them in writing. Coached by Leibniz, he always reviled *actio in distans*, and that is the only type of causal power they have. Had he lived to hear about young Kant's physical monads, he would have denounced them as an absurd monstrosity.[11]

An unadulterated view of matter as continuous at all scales is on display in Kant's *Foundations* of 1786: "Matter is divisible to infinity, and, in fact, into parts such that each is matter in turn" (4:503). The view is sophisticated too, not just pure. Like modern continuum mechanics, Kant lets matter be governed by Conservation of Mass, which he derives from his First Analogy. He has a good grasp of mass density in a continuum. And, he endows matter with "penetrating" and "surface" actions, i.e. body forces and contact forces.[12] In one respect, he stands alone in his age: his 'penetrating' force (of attraction) is *direct* action at a distance, which he always defended valiantly from common objections. Then, like his predecessors, Kant too moves to

[10] E.g., G. Ploucquet, who started out with monads, strove to understand how they make up a continuum, and gave up on them as "chimeras, whose nothingness is sufficiently proven" (1753:355). Another, merciless opponent was Euler 1746b. In private, these debates could get rather acrimonious; see Ostertag 1910: 50-150. Keen accounts of the warring positions is De Risi (2007: 301-14) and Leduc 2015.

[11] I examine Wolff's arguments against action-at-a-distance, and Leibniz's influence on him, in my (forthcoming b).

[12] See 4:541f and 516. For thorough discussion, see Friedman 2013. Still, Kant's actuality has limits. He could never bring himself to recognize the existence of shear forces in his continuum. Cf. Wilson (2013: 89) and Stan (2014: 435ff).

delegitimize rigid bodies, though by a new argument. And, he bids good-bye to the mass points he used to advocate (4:502, 504f).[13] Or so he says. Unwittingly, he had already taken it back, in his foundations of kinematics:

> Since in phoronomy nothing is to be at issue except motion, no other property is here ascribed to the subject of motion, namely, matter, aside from movability. It can itself so far, therefore, also *be considered as a point*, and one abstracts in phoronomy from all inner constitution.... If the expression "body" should nevertheless sometimes be used here, this is only to *anticipate* to some extent *the application of the principles* of phoronomy to the more determinate concepts of matter that are still to follow. (4: 480; my italics)

In effect, Kant here quietly has mechanics adopt a mass-point view, despite his own official doctrine that matter is continuous. He has just argued for treating an *extended* body as a *point*-sized entity, which he then endows with mass. Ultimately, this reverberates through his entire grounding of mechanics, with adverse effects (Stan 2014).

Though everyone—Leibniz, Johann Bernoulli, Wolff and Kant—rejected rigid bodies emphatically, it is doubtful that any professional philosopher in Prussia endorsed this ontology. Lambert might fit the bill, but he was no mainstream figure in academia. Kant in *Foundations* suggests, darkly, that it was the default matter theory of the "mathematical investigators of nature," and yet he leaves them unnamed. Were they all reproving a domestic straw man? Or were their attacks directed abroad?

Before we move on, note how little Newtonian this all is. Newton's preferred ontology of matter was rigid atoms and empty space; his many followers in 18th-century Britain favored it too (Heimann & McGuire 1971). And yet, if there is one thing that unites Leibniz, Wolff and Kant, it is their stark rejection of rigid matter and the void.

Superstructure. As an edifying sequel, let us now turn to some local *mécaniciens*. They articulated exactly the respective mechanical theory of each of the three basic entities above. It is thanks to their researches that the fundamental differences between these objects become clear. Still, I do *not* mean to imply that they posited any of these objects as ontologically basic. If they had any such commitment, it is quite hard to discern.

[13] For a lucid explanation of both points, see Friedman 2013.

Euler is their standard bearer, but also the most confounding figure. He contributed equally to the mechanics of all three objects above. The mass point—single and free, or kinematically unconstrained—is the main topic of his youthful *Mechanica* of 1736. However, much in it was just a unification of earlier results in 1-particle dynamics by Newton and Jakob Hermann. In the 1740s, he started breaking new ground, by deriving equations of motion for *systems* of mass points; and for a *constrained* mass point.[14] That was his masterful *De motu corporum in superficiebus mobilibus*, where he determined the trajectory of a mass point forced to move on rigid surfaces, whether fixed or freely movable (Euler 1746).

As to rigid-body dynamics, Euler created it more or less single-handedly.[15] His decisive breakthrough came in 1750, with *Découverte d'un nouveau principe de Mécanique*, whose significance is comparable to Newton's book. There, Euler obtained equations of motion for a rigid body moving around a fixed point under external forces. A few years later, he had another insight (Euler 1765a). In *Recherches sur la connoissance mécanique des corps*, he explained that, in addition to mass, or resistance to *translation*, matter has another basic property. It is the moment of inertia, i.e. resistance to rotation around some instantaneous axis. He unified all his results on this fundamental object in *Theoria motus corporum solidorum* (1765b). His last breakthrough was in *Formulae generales pro translatione quacumque corporum rigidorum*. There, he had a deep kinematic insight: any rigid transplacement—the most general sense of motion for a rigid body—is equivalent to a translation and a finite rotation (Euler 1776b). He was inspired to take up this problem by Lagrange, who at Berlin had handled this topic in his (1773). Like d'Alembert before them in France, they studied rigid motion so at to extend Newton's program in celestial mechanics. The Briton in 1687 had modeled the planets as *particles*. But, that made it impossible to treat the gravitational phenomena they exhibit as *extended* bodies: precession, nutation, libration, and tidal lock-

[14] Hepburn 2007 and (forthcoming) study Euler's early dynamics in the context of early 18th-century natural philosophy. Stan (forthcoming b) explains Euler's complicated relation to Newton, and his creation of rigid dynamics.

[15] There *was* research on rigid-body motion before Euler, but it had a severe limitation. Lacking a proper kinematics and sufficient dynamical laws, theorists were confined to studying the motion of just one, representative point in the body—the so-called 'center of oscillation.' A survey of results is Vilain 2000.

ing.[16] To explain these, Euler and Lagrange had to model a planet as a rigid body.

In regard to the mechanics of continuous bodies, here too Euler created large swathes of it. (D'Alembert too, in France.) He found the equilibrium conditions for a thin rod (the elastica), in an appendix to a pioneering tract on variational methods (Euler 1744). In the 1750s, he unified and generalized the statics and dynamics of 'Newtonian' inviscid fluids in laminar and vortex flows. As part of that, he articulated the concept of internal pressure in a continuous fluid (Darrigol & Frisch 2008). Another key result was his memoir *Principes généraux du mouvement des fluides*, the birth of 'Euler's equations' for compressible flow (1757). In a letter to Lagrange, he devised what we call the 'Lagrangian' description of motion for a continuum (1762). The deep dynamical insight he obtained in *Découverte* above slowly led him down the path to equations of motion for manageable, lower-dimensional continua: the elastica and the thin plate, or lamina (1771, 1776a).[17] The former paper is important because Euler shows there for the first time how to compute the shear stress in a continuum—a type of force that has no meaning or reality in rigid bodies or mass points (1771: 384).

Fracture. As the 1780s draw to an end, the growth of mechanics reveals, somewhat *ex post facto*, a facet that should have unsettled our philosophers, had they kept up with it. Throughout the century, mechanical theory strove to become general: to explain the full range of mechanical phenomena known then, by deriving equations of motion for them. But, it turned out, that task eventually required it to resort to three kinds of building blocks for modeling bodies: the mass point, the rigid body, and the deformable continuum. As I have explained, these objects are deeply unlike and mutually irreducible. Thus mechanics had to give up on the philosophers' dream to anchor it in a single, monolithic base ontology.

Euler and Lagrange, it appears, did not miss that. They saw, more clearly than the philosophers, that in mechanics the need for generality can trump ontological unity. So, the theorists turned understandably quietist about the

[16] These are all phenomena associated with a planet's axis of rotation and its angular velocity about it. But, a mass point has no such axis.
[17] Due to a lack of adequate kinematics, three-dimensional elastic solids remained in good part out of his reach. They first get treated in post-Napoleonic French mechanics, chiefly by Cauchy, Poisson, Navier and Saint-Venant.

ultimate constitution of body.[18] Philosophers, in contrast, rushed in to fill that silence. As we have seen, they sought to avoid ontology fracture by a dual strategy. First, they gave a priori arguments for a preferred unit of matter; witness Wolff's and Kant's proofs that body is a physical continuum. Second, they tried to rule out, again on a priori grounds, competitor ontologies—rigid bodies and mass points. However, they seem to have missed that, by keeping ontology unified, they fail to give a sufficient foundation for mechanics. Clinging to one picture of matter at all costs will fail to yield a mechanics demonstrably general.

Thus, by the last third of the century, mechanics had fractured ontology. The early modern hope for a single, univocal material basis yields to a protracted struggle for foundational supremacy that extends well into the *Spätklassik*, enmeshing Lord Kelvin and Helmholtz, Duhem and Mach, Hertz and Hilbert. In deep, subtle exchanges, these figures debated whether mechanics ultimately rests on discrete or continuous matter, contact forces or distance actions, bodies with finite or infinite degrees of freedom. In retrospect, we see that they continued a dialog that began with Wolff and Kant, d'Alembert and Euler, Boscovich and Lagrange.

Dynamical laws

Primed by Kuhnian intuitions about paradigms, we might expect that, after Newton's *Principia* came out, its three mechanical principles became *the* laws of motion for everyone, our protagonists included. Not long ago, Eric Watkins discovered this to be a mistake. The canonical laws of motion in academic Prussia differed significantly—in content and intent—from Newton's principles. Subsequently, others proved that these schools also refused to embrace the Briton's understanding of inertia, force and interaction. Together, these findings point to an unexpected conclusion: in Prussian universities before the late 1750s, Newton's own conceptual basis for mechanics is mostly invisible. Instead, local figures were in thrall to a foundational program bequeathed by *Leibniz*.[19] Wolff and his followers carried it out, and it left deep marks on Kant's thinking too. I will make that clear as I explain

[18] Despite his teachings in *Letters to a German Princess*, it is unclear what Euler's considered ontology of matter was. I explore this question in my (forthcoming a).
[19] This is the result of Watkins (1997; 1998) and Stan (2012; forthcoming b).

another way in which natural philosophy falls apart at that time. Like the breakup I examined in Section I, it too ends in fracture: between philosophical reflection and scientific theory.

The distant source of that is Descartes. Recall his program in *Principia Philosophiae* of using 'laws of motion' to ground 'rules' of motion.[20] Wolff keeps this Cartesian duality and the program it underwrote:

> The general principles of the rules of motion are called *laws of motion*.... Implicit in the rules of motion there are general principles, from which we can derive these rules.... Mathematicians assume these laws without proof; but it behooves the Metaphysician to demonstrate them. Hence we deem it our business to establish them here. (Wolff 1737: 228)

Now, there was a deep source of tension in Descartes' project. To secure absolute certainty for mechanics, he let it flow—by deductive argument—from indubitable facts about God and the essence of matter, *through* the laws of motion, to the rules. But, he did not foresee that the final list of 'rules of motion' might in the end be *longer* than the seven rules he had on record. Thus, conflict can ensue: the full list of rules, or equations of motion, might need for their derivation a wholly *different* set of dynamical laws than Descartes' three principles. Critically, these laws might be impossible to anchor by any a priori argument in metaphysical resources.

Founders. Nearly a century after him, Wolff too was oblivious to this danger. To make good on his promise above, he gave two laws of motion, grounded in metaphysics: a Law of Inertia, and a Law of Action and Reaction (1737: 229, 252). Wolff insisted that these principles are necessary, and so he tried to produce a priori evidence for their truth. To that end, he invented a confirmation strategy that would loom large in the Enlightenment.[21] It is a mixture of metaphysics of body, conceptual analysis, and the Principle of Sufficient Reason, PSR.[22]

[20] The laws were basic principles of 1-body motion and interaction; the rules, kinematic predictions of outcome in 2-body collision—ancestors of equations of motion.

[21] Even his philosophical opponents adopted it. For instance, d'Alembert, whose dynamics rests on three explanatory principles, or 'laws of motion,' and a general heuristic, nowadays called 'd'Alembert's Principle.' Like Wolff, d'Alembert grounds his laws in conceptual analysis and the PSR; cf. Firode 2001. Kant too succumbs to this Wolffian strategy, even in his Critical years; cf. Stan 2013.

[22] Quite unlike Leibniz, Wolff and argued that PSR was necessary—and so facts grounded in it are themselves necessary. A sharp account of Wolff's grounding of PSR is Look 2011.

As illustration, consider his defense of his second law of motion. First, he analyzes the concept 'interaction' as denoting the encounter between an agent and a patient body. Then, from his ontology of body, he takes that there are active and passive forces. He assigns the former to agents and the latter to patient bodies. Next, he invokes PSR to infer, there is no sufficient reason why an agent should exert more active force (needed to break the patient's resistance) than the latter puts up passive force to oppose the agent. So, he concludes, in any interaction the active force spent by the agent equals the passive force put up by the patient—no more, no less. This is his Law of Action and Reaction (234-8; 251-5).

This seems broadly Newtonian, but that appearance misleads badly. Wolff's dynamical laws, and the concepts behind them, differ sharply from Newton's. In fact, we ought to wonder if they are at all compatible with the *Principia*. That book neither supports nor requires Wolff's heterogeneous dualities agent vs. patient, active vs. passive force, and action vs. reaction. Newtonian action and reaction are *homogeneous*, not different in kind: both are *vires impressae* ruled by the Second Law—and Wolff lacks all that. Thus, Newtonianism is again conspicuously absent from his system. The reason, I show elsewhere, is that Wolff's mechanical foundations really come from Leibniz. Jakob Hermann, another Leibnizian, likewise shaped his basic laws of dynamics, which never aimed to ground Newton's theory (Stan, forthcoming b).

It is hard for us to grasp how influential Wolff and his views were then. Like much else he wrote, his doctrinaire natural philosophy swayed many in Germany: Thümmig, Gottsched, Winckler, Stiebritz, Baumeister, Burkhäuser, Hausen, Kahle, Formey, and the compendious Hanov, who set out in the 1760s to rival his master in output. Many of them just paraphrase his physical teachings when they do not repeat them verbatim. Nowadays deservedly forgotten, their sheer number then made Wolff's account into the received consensus, and made them a powerful presence.

So powerful, in fact, that the young Kant began his *New Theory of Motion* in 1758 hoping the Wolffians would forgive his dissent from their dogmas (2:15). And yet, Kant does not write it to roll back Wolff's foundations in the name of Newton, as one would expect from a recent convert.[23] Rather, he merely corrects that dogma, by subverting the Wolffian distinction agent/patient. Otherwise, he keeps an astonishing amount of their ultimately Leibnizian views. Like them, Kant thinks that the basic laws of motion are two (not three, as Newton had it); they are derivable a priori; impact is the paradigm of interaction, and is a mutual exercise of "force of motion," a descendant of Wolff's *vis motrix*, not Newton's *vis impressa*. Remarkably, these Leibniz-Wolffian commitments and agenda survive almost unscathed in Kant's mature philosophy of physics.[24]

Builders. While the philosophers aimed at absolute certainty, the theorists' driving force was the search for truly *general* laws of motion, i.e. proven to entail equations of motion for all types of bodies and mechanical systems. Despite our initial expectations, Newton and his contemporaries did not bequeath such laws to the Enlightenment. And, this became quite clear before too long. Of the mechanical principles handed down by the Seventeenth Century, the young Euler says:

> These principles are of no use in the study of motion, unless the bodies are infinitesimally small, hence *the size of a point*—or at least we can *regard* them as such without much error: which happens when the direction of the soliciting power passes *through the center of gravity*.... But if it does not pass through that center, we *cannot* determine the entire effect of these powers. That is all the more so when the body to be moved is *not free*, viz. is constrained by some obstacle, depending on its structure. (Euler 1745, §17; my emphasis)

In our terms, his complaint is that the laws of the late 1730s could not predict the motion of extended bodies and constrained systems—a very large class of behaviors, to be sure. That realization set off a quest for ostensibly general principles of mechanics. However, as Euler and his peers set out to uncover them, no one foresaw they would end up with not one but three *distinct* and *independent* sets of basic laws. This makes the foundations of mechanics ca. 1780 overdetermined, a situation that continues today.

[23] Many take Kant's *Theory of Heavens* (1755) to signal his move to Newtonianism. The truth is complicated, of course. Watkins 2013 untangles carefully the various (anti-)Newtonian strands in Kant's thought at that time. Jauernig 2011 explains his stance toward the Wolffians beyond natural philosophy.

[24] For explanation and argument, see Stan 2013. Friedman 2013 also endorses these claims. These results ultimately vindicate the main insight of Watkins 1997.

The first general principle originated with Maupertuis, who found that when two bodies collide directly or balance each other on a lever, the integral $\int Mvds$ tends to a minimum.[25] This was evidence for a new dynamical law, which he called the Principle of Least Action (1740, 1744). At Berlin, his colleague Euler extended the insight to the motion of a particle attracted by one or more central forces, in his *Harmonie entre les principes généraux de repos et de mouvement de M. de Maupertuis* (1753a). Euler then claimed optimistically that, "with an easy and natural addition," Maupertuis' law "extends with the greatest success to the whole Science of motion" (1753b: 217).

Still, it was no proof that the principle was *demonstrably* general. That breakthrough came from the young Lagrange, then an obscure adjunct professor known outside Italy to no one but Euler. Though his feat happened at Turin, it earns mention here for developing results Lagrange had first had in a memoir (submitted to the Berlin Academy) that greatly impressed Euler, who brought Lagrange to Berlin, where he became the leading innovator in mechanics (Galletto 1991).

What moved Euler to awe was Lagrange's creation of a new mathematic, the calculus of variations. He then exported it to mechanics, in *Application de la méthode précédente à la solution de différentes problèmes de dynamique* of 1762, which he started with a "General principle." Let a set of masses M, M′, etc. interacting by central forces cross the spaces s, s', etc. in a time t. Let u, u', etc. be their instantaneous velocities. "Then the formula $M\int uds + M'\int u'ds' + M''\int u''ds'' +$ etc. will always be a minimum or a maximum" (Lagrange 1762: 198). More exactly, Lagrange's law says that the variation of a certain quantity, viz. the action integral, is null:

$$\delta \int Mvds = 0 \qquad [1]$$

This is the Maupertuis-Euler Principle of Least Action, restated by Lagrange in variational terms. From it he derived equations of motion for a large class of mechanical setups, including systems of constrained particles, a gas, a fluid in laminar flow, and a rigid body. It was proof that his principle was a general law of dynamics.

[25] For each body in the system, M is its mass, v the instantaneous velocity, ds the differential arc element of its trajectory. The integral is with respect to time.

The second principle was born abroad, though from a seed sown by Johann Bernoulli, a vocal Leibnizian. It was this: the virtual work of the applied forces on a system in equilibrium vanishes. His friend, Varignon, made it public and used it to outline a statics of rigid bodies, in *Nouvelle Mécanique, ou Statique* (1725: 176). Another seed was planted by d'Alembert in his great dynamical treatises of the 1740s. To find equations of motion for constrained systems, he reasoned as follows. The motions acquired in fact by the system do not coincide with the motions impressed on it by the external causes; some impressed motion is lost to the constraints.[26] To find the acquired motions, i.e. the actual accelerations, d'Alembert proposed a heuristic: the acquired motions, *if their sign were reversed*, would balance the impressed motions. That is, if all these motions were given to the dynamical system at issue, it would be in equilibrium. At this point, it can be handled with the tools of statics.[27]

It was the young Lagrange who unified the two insights above, into a principle he knew quite well to be general. His thought was this. Take the masses in a system, and multiply them by their respective *actual* accelerations. Suppose that forces, equal to these products but *opposite* to them, acted on the system in addition to the *real* forces already acting on it. (This is Lagrange recasting d'Alembert's idea in terms of forces, not 'motions.') Then the system would be in static equilibrium. Thereby, it comes under the jurisdiction of the Bernoulli-Varignon statical law above: the virtual work of all these forces is zero. We call it the Principle of Virtual Work:

[2] $$\sum (\mathbf{F}_i + \mathbf{J}_i) \cdot \delta\mathbf{r}_i = \mathbf{0}$$

To make a *dynamical* system reveal its equations of motion, Lagrange subjects it to a twin *Gedankenexperiment*. In addition to the actual forces $\mathbf{F}_i$ acting on it, he introduces a set of fictive forces $\mathbf{J}_i$ such that they balance the combined $\mathbf{F}_i$. With the system now reduced (in thought) to rest, he gives each part (again in thought) a virtual displacement $\delta\mathbf{r}_i$ compatible with the con-

[26] Think of a body on an inclined plane. Part of the acceleration of gravity—viz. the component normal to the plane—is never acquired actually by the body.
[27] Admittedly, this is rather opaque. Explaining it sufficiently requires much more space than I have here. A lucid account of d'Alembert's idea is Fraser 1985; clear expositions of its modern role are Duhem 1903 and Hamel 1949.

straints. His principle is that $\mathbf{F}_i$ and $\mathbf{J}_i$ are such that their net mechanical work along these displacements is null. From this, he derived a "general formula containing the solution to all the problems on the motion of bodies," i.e. the so-called 'Lagrangian' equation of motion[28] (1878[1763]: 12).

Lagrange first applied and advertised this law in *Recherches sur la libration de la lune*, 1763. Soon after that he moved to Berlin, where he spent twenty years figuring out how to extend his law to all of mechanics. That comprehensive effort produced his *Méchanique analitique*, in which he called his law the "General Principle of Virtual Velocities." Though the work came out in France, it soon appeared in German as well (Lagrange 1797). Its principle became the norm for some key German figures too; e.g. Gauss in his work on hydrostatics (1830). Elsewhere, it was *the* basic dynamical law for much of the next century (Capecchi 2012).

The third general principle was twofold. Because of its explanatory power, we know it now as Euler's Laws of Motion (Truesdell 1991). However, unlike Lagrange's two consecutive achievements above, Euler did not write a comprehensive treatise to demonstrate the range of his two laws. Rather, he piecewise proved that they are general (Stan, forthcoming a). The first law by itself enabled him to determine the motion of a rigid body around a fixed point (1752); the linear flow of inviscid fluids (1757); and later, mass points with constraints (Euler 1781; 1783). However, as he expanded his scope, Euler came to realize that his First Law was generally not enough to determine *completely* the mechanical behavior of extended bodies. Rather, a second basic law is needed, analogous to the first but logically independent from it. This insight emerges clearly in Euler's later work in elasticity, rigid-body dynamics, and constrained motion (1771, 1776b, 1781). There, he always starts "from first principles," by setting down two laws:

[3a] $$\mathbf{f} = \mathrm{m}\mathbf{a}$$

[3b] $$\mathbf{h} = \mathrm{i}\alpha$$

The equation of motion for the particular system at issue follows in every case from them. If Euler's two laws above look Newtonian, it is because

[28] To learn how he did that, see Fraser 1983 and Barroso Filho 1994.

they generalize Newton's *Lex Secunda*. The laws assert that there are two fundamental kinds of mechanical agencies and effects. The first is forces, which impress linear accelerations; the second is torques, or causes of angular acceleration (around some instantaneous axis of rotation). Bodies resist the first in proportion to their mass, the second in proportion to their moment of inertia.

Against this backdrop, it may seem as if the passage of time takes mechanics toward its broadly Newtonian version we learn early in college. We should resist that impression; it is a side effect of my having to end my account with the 1780s. In the evolution of mechanics, that is an artificial cutoff point. From *that* perspective, Newton-Euler dynamics is the minority view in the two centuries after the *Principia*. Lagrange's Principle of Virtual Work becomes the basic law in French mechanics for well over half a century after his move to Paris in 1783. In Britain, Stokes embraced it too. And, the Principle of Least Action becomes the fundamental law in Hamilton-Jacobi theory, a brand of mechanics that takes hold in Germany and much of Britain from the 1830s onward. We recognize these two formulations as the ancestors of our analytic mechanics.

Drift. And so we see how by the mid-1780s mechanics and philosophy have grown apart, which brings Descartes' program to a tragic end of sorts. Excessively concerned with certainty for mechanics, the philosophers end up with dynamical laws that may be safe from doubt but are explanatorily too narrow: they cannot determine by themselves *all* the possible motions of *all* the possible bodies. Regrettably, philosophers—even great ones, like Kant—did not seem quite alert to this problem then.

The theorists, in turn, seem afflicted by a converse problem. Having secured true generality for their laws, they are at a loss about how to show their absolute certainty, which they continue to believe in, swayed by philosophy. They either state it blankly, like Euler in the *Letters*, who asserts without proof that the Principle of Least Action is "perfectly founded in the nature of body, and those who deny it are very much in the wrong" (1802: 303). Or they proffer basic dynamical principles while bracketing entirely the question of evidential support for them, as Euler does with his two laws in his late papers. Or, finally, they flail about ineffectually as they try to derive these basic laws from deeper, supposedly more certain premises. Such is the case of Lagrange and a host of others, ca. 1780-1825. Having left Berlin for Paris, he and his new confrères at the *Ecole Polytechnique* spend decades seeking in vain to ground the Principle of Virtual Work, our statement [2] above, in something absolutely certain or self-evident (Bailhache 1975). After Fourier they give up in frustration, and the question becomes dormant. But it just makes the split more painfully obvious to those who care to look.

Kinematic foundations

Lastly, there were interjections on whether motion is 'relative' or there are 'absolute' motions. However, these terms are highly misleading and laden with anachronistic connotations. It is best to present this debate from within.

Early modern mechanics presupposed an (often implicit) distinction between *true* and *apparent* motion. For instance, if taken to be about apparent motions and rest, both Copernicanism and the Law of Inertia are trivially false.[29] So, it was assumed, the dynamical laws are statements about the true motions of bodies. Inevitably, a metaphysical question arises now: what is the *nature* of true motion? What does *motus in re vera* (as Descartes called it) consist in? Newton notoriously claimed that true motion *is* absolute motion. That is, it consists in velocity in absolute space, a rigid immobile frame metaphysically distinct from bodies. His opponents retorted that true motion is relational. Namely, it consists in a distinguished relation of matter to matter, not to space itself.[30]

Newton did not just declare that true motion was absolute, but he also gave a very powerful argument that it *must* be so.[31] Briefly, its logic was as follows. (i) Bodies have true motions: for any given body, there is a fact of the matter as to whether it really moves or rests. (ii) True motion is either absolute or "relative," i.e. relational; *tertium non datur*. (iii) Any correct account of true motion must satisfy the "properties, causes, and effects" of true

[29] The Earth indisputably *appears* to rest and the Sun to move. Given an initial impulse, a ball rolling on a smooth, long, flat table will describe a curve—a cycloid—not as straight line, as the Law of Inertia predicts. (This is evidently because the table, and the Earth to which it is fastened, is a rotating frame, hence is not inertial.)

[30] A third, small group—the later Huygens, the young Leibniz—asserted that motion was *relativistic*. I.e., they denied that bodies have *any* true motions. Confusingly, they phrased this as a claim that motion is 'relative.' Thus, interpreters must use extreme caution with the term 'relative motion.' It was a linguistic vehicle for logically contrary views, viz. relationism and relativism. I explain these facts at length in Stan 2015.

[31] Newton's case for absolute motion is in the Scholium to the Definitions, in his *Principia*. I follow here the reading defended in Rynasiewicz 1995.

motion. (iv) Relative motion fails to meet these criteria. (v) Absolute motion always meets them. (vi) So, true motion is absolute motion, i.e. velocity in absolute space (Newton 1999: 408-12).

In the century after Newton, the most striking fact is how little understood, and so directly addressed, his argument was. Though Leibniz admitted to Clarke that each body has an "absolute true motion," he avoided explaining *precisely* what it consists in, all the while denying that it is velocity in absolute space, as the Newtonians had it. Instead, Leibniz sought to preempt their conclusion by arguing that there is no absolute space, hence there can be no motion in it. This move ultimately solves nothing; Leibniz still owes the world his own account of what true motion is, and a proof that it supports a mechanics of inertial forces. Still, in Germany the post-Leibnizians followed his tactic. Led by Wolff, they rehashed his genetic account of our representation <space>. First, they alleged, we perceive direct metric relations between material objects. Then, by "abstraction," we form the concept of a system of situations for the class of all actual bodies: space (Wolff 1730: 455-61). Motion, in turn, is nothing but a body's change of situation relative to some set of bodies (1730: 493ff.). Possibly true, but insufficient. At any instant, a given body changes infinitely many kinematic relations to others. And yet, both the new mechanics and his natural philosophy assume that bodies have *unique* true motions, which he ought to define and defend. Regrettably, Wolff always remained oblivious to this deep, difficult problem. So did his disciple, Thümmig, who went on to write a useless, repetitive "sixth letter to Clarke" after Leibniz's death, without ever grasping that true motion all but inexorably requires absolute space (Sharpe 1744).

Against this background of general obliviousness to Newton's point, Euler in 1748 made a strong kinematico-dynamical case for absolute space, with his *Réfléctions sur l'espace et le tems*.[32] Here is a capsule. (i) True motion and rest obey the Law of Inertia: only external forces can change a body's true uniform translation or rest; and they always change them if applied to it. (ii) 'Relative' motion does not satisfy the Law. That motion can be changed without applying a force to the *body*; and a net force can *fail* to change it. (iii) In contrast, motion in absolute space cannot but obey the Law. Therefore true motion, i.e. change of *true* place,

[32] For context, elaboration, and immediate reception of Euler's case, cf. Stan 2012.

> "is governed by the idea of place as conceived of in Mathematics, and not at all by the body's relation to other bodies. Now, one cannot say that [the Law of Inertia] rests on something that subsists only in our imagination. Hence, we must absolutely conclude that the mathematical idea of place is not imaginary, but that there exists something real that corresponds to this idea. Therefore in the world, beyond the objects that constitute it, there is some reality that we represent by the idea of place." (Euler 1750: §13).

In essence, this is the very same logic as Newton's. Recall Newton's premise (iii) above: true motion has certain "properties, causes, and effects." The causes are "forces impressed upon bodies to generate motion" (1999: 412). Now, the Law of Inertia governs these forces: they *alone* change true inertial translation; and they *always* change it. But, to change a body's relative motion, forces are neither necessary nor sufficient, in flat contradiction with the Law. Thus, Newton and Euler concluded separately, true motion cannot consist in a relation of matter to matter. This raises a fascinating question, as yet unsolved. Did Euler in *Réfléctions* just restate a part of Newton's argument without acknowledging him as the originator? Or did he rediscover it independently? His powerful intellect and peerless grasp of the conceptual foundations of mechanics certainly point to the latter.

As Euler was for absolute motion at that time—its sole yet ablest defender—so was Kant for relationism. He first advocated it in *New Theory of Motion and Rest*, a little-read but very important paper from 1758. Recall that a relationist grants that any body has a unique true motion, and explicates it as a distinguished relation to other bodies or matter. Kant is no exception. For him, *wahrhafte Bewegung* is the kinematic relation between any two interacting bodies. (In keeping with the Wolffians' paradigm process, Kant in the 1750s restricts his account to collision.) Again like any standard relationist, he explains why that relation is privileged: in his view, because it results in dynamical effects, unlike any other kinematic change relative to bodies outside the interacting system. Further, the two colliding bodies share in this mutual relation—viz., relative motion—to the same extent: "tell me if one can infer, from what happens between them, that one is at rest and only the second moves, and also which of them rests or moves. Must we not ascribe the motion to both, namely in equal measure? Their mutual approach may be attributed to the one just as much as to the other" (2:18). So, he infers, in any impact one body is in true motion relative to the other, and vice versa. Quantitatively, this motion is its momentum with respect to the mass center of the 2-body system. Thus each body has a true velocity, which consists in a relation to a material system of reference, the so-called center-of-mass (CM) frame of the collision.

What is more, Kant keeps this version of relationism into the 1780s, modulo his switch to transcendental idealism. In *Foundations*, he reasserts his early relationist credo: "all motion is relative only.... That is, matter can be thought as moved or at rest solely in relation to matter, and never with respect to mere space without matter" (4:559). However, a body has (infinitely) many such relations to 'matters' outside it. Which is its *true* motion? Kant explains, true motion is an "*active* relation of matters in space" (4:545). A 'matter' is in that relation if it interacts with another by exerting a "moving force." His paradigm is again direct collision, for which he argues that each body has a true velocity relative to the CM-frame. (In this case, its 'moving force' is linear momentum, or capacity to accelerate objects in its path.) Then he claims without really explaining that his analysis applies to interactions by attractive forces as well, e.g. gravitation.[33] And, in a novel development, he extends his relationism to circular motion—specifically, to a spinning body. Allegedly, that too is relative motion, because any two "opposite parts" in the body—diametrically across, normal to the axis of rotation—endeavor to *recede from each other* (4:561f.). Thus Kant convinces himself that all true motion is a relation of matter to matter, and so there is no need for Newton's absolute space as the fundamental frame of reference.[34]

Kant could not have known it, but his key insight rediscovered an idea the young Leibniz had, but left fallow. Around 1677, Leibniz too had surmised that true motion is a mutual relation between colliding bodies, such that each has a true momentum relative to their CM-frame:

> If *space* is a certain thing consisting in a supposed pure extension... and *motion* is change of space, then motion will be something absolute. But in reality... motion is not something absolute, but consists in *relation*. And therefore if two bodies

[33] Friedman (2013: 494ff.) is a reconstruction of Kant's reasoning for this sort of process.
[34] This account is based on results obtained in Stan 2009 and 2015. There is an alternative reading, on which Kantian true motion is motion relative to an inertial frame designated by matter. (Ultimately, the global CM-frame of the physical universe.) Friedman 1992 and 2013 has defended it with great sophistication.

> collide, *the speed* must be understood to be *distributed between them* in such a way that *each* runs into the other with the *same force*. Thus... all the phenomena consistent with experiments will be at once deduced from this fact alone. (Leibniz 2001: 225; A.iv.359; my italics)

Combine this with the fact that Kant's relationism was originally an engagement with Wolff's foundations of mechanics, not with Newton or Euler (Stan 2011). Then it turns out that, in this period, there is a strong Leibnizian strand in kinematic foundations too, not just in mater theory or dynamical principles, as I have explained.

Conclusions

Natural philosophy in post-Leibnizian Prussia is a greatly diverse milieu, rich in seminal developments that shaped mechanics and its philosophy into the late modernity. Notably, Newtonianism—in both theory construction and conceptual foundations—long remains a minor presence. And, when it becomes established it never gains full supremacy. Strong Leibnizian elements always remain in place to challenge its rule, as do other, novel mechanical ideas and constructions genuinely born out of the Age of Reason, not grown from early modern seeds.

At the same time, this diversity of foundational perspectives defies any attempt to show that post-Newtonian mechanics is a unified theory. Three, mutually irreconcilable ontologies are offered as material basis for its models. The epistemic status and identity of basic dynamical laws become split beyond easy reconciliation. And, the 17th-century schism between absolutists and relationists about motion remains in place. Together, these disagreements should give us an edifying glimpse into the discord that reigned in much of 19th-century mechanical foundations.

Hopefully this preview is enough to inspire us to give Enlightenment natural philosophy in Germany the attention and respect it deserves.

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