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Standing colossus: a critical study of J.B. Shank on Newton and the French

• Marius Stan •

Shank in this book aims to recount an infant stage in the growth of what he calls 'Newtonian mechanics.'[1] He does so in three parts, or eleven chapters. Part I sets the stage, with French science in the 1690s as the backdrop. Part II seeks to uncover the 'intellectual roots' of his topic; he claims them to be Newtonian, Leibnizian and Malebranchist. Part III is the longest, but also oblique to the history of any mechanics. It is a blend of externalist history (of institutional politics at the French Academy around 1700) and intellectual history centred on foundational debates in early calculus. The book's main figure is the mathematician Pierre Varignon, from whose output, spanning forty years, Shank picks eight papers (hereafter MV, for convenience). He concludes that 'ultimately, the contingencies of history produced what we now call classical Newtonian mechanics' (p. 370).

Unfortunately, this book misses the mark, by much. It combats a defunct view, and it ignores key sources. It fails on three counts: *evidential* — his project is unmotivated, he makes false promises and his facts are thin; *methodological* — his interpretive categories are inadequate, and he distorts Newton's legacy; and *explanatory* — his choice of context does not explain his topic, and he ignores better-supported explanations. I move now to defend my charges.

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[1] A discussion of J. B. Shank, *Before Voltaire: the French Origins of "Newtonian" Mechanics, 1680–1715*, University of Chicago Press, 2018. For lucid insights, I am indebted to George E. Smith and Katherine Brading; for helpful discussion, I am grateful to Noel Swerdlow, Moti Feingold, Jed Buchwald, Mary Domski, and Zvi Biener. I thank an anonymous referee for this journal for pressing me on two important points.

I. Aims and motivation

The book's objectives are unclear. A chief aim seems to be Shank's

> posing for historical scrutiny the claim that eighteenth-century mathematical mechanics, and by extension modern mathematical physics, was primarily a Newtonian creation, developed and announced in the *Principia* and then completed and established through the reception and acceptance of his work. (p. 11)

But no one thinks that any more. The consensus now is that Newton had an important *but far from exclusive* influence on eighteenth-century mechanics. Other, equally important, sources were the early Basel School and three giants (d'Alembert, Euler, and Lagrange) who developed Enlightenment dynamics from concepts, laws and methods *born after 1740*, not before (see below). Their transformation was comparable in 'impact on physical theory with the advent of quantum mechanics,' as experts agree now.[2] So, Shank's scrutiny is obsolete on arrival.

It is troubling to see so many misleading claims in this book; here are some egregious cases. *Before Voltaire* claims to map the 'scholarly terra incognita' of the years 1680–1715 in science at the French Academy (p. 18). But that land is far from unknown.[3] And, what we do not know, Shank leaves untouched, because he misses two real gaps in our knowledge of the period. The first regards the large output in two important fields at the Paris Academy between 1670 and 1725. One field is *mécanique* — their term for the statics of solids.[4] He fails to even raise

[2] See S. Caparrini and C. Fraser, 'Mechanics in the eighteenth century,' in *Oxford Handbook of History of Physics*, eds. J. Buchwald & R. Fox (Oxford, 2013), 358-405.

[3] Varignon's statics and geometry are studied in J. Fleckenstein, 'Pierre Varignon und die mathematischen Wissenschaften im Zeitalter des Cartesianismus,' *Archives internationales d'histoire des sciences* 2 (1948), 76-138 (which Shank ignores). Th. Hankins explained Malebranche's importance for dynamics, in 'The Influence of Malebranche on the Science of Mechanics During the Eighteenth Century,' *Journal of the History of Ideas* 28 (1967), 193-210. In the 1980s, Pierre Costabel and Michael Mahoney, mapped the Parisian debates on infinitesimals, as did J. Greenberg, 'Mathematical Physics in Eighteenth-Century France,' *Isis* 77 (1986), 59-78. Key episodes in *fin-de-siècle* dynamics at Paris are studied in C. Vilain, 'La question du "centre d'oscillation" de 1660 á 1690,' *Physis* 37 (2000), 21-51; and in Y. Fonteneau, *Développements précoces du concept de travail mécanique (fin 17e s.-début 18e s.)*, PhD diss. (Univ. de Lyon, 2011). For French reactions to Newton, see C. Borghero, *Les Cartésiens face à Newton* (Brepols, 2011).

[4] Shank leaves out Perrault, Huygens, Mariotte, Lamy, Amontons, Renau d'Eliçagaray, and Varignon himself (whose posthumous *Nouvelle mecanique* is a statics). And, of those he mentions, he just rattles off their names, book titles and biographical tidbits (for Cordemoy, he uses Wikipedia).

crucial questions about these works: what new results did they contain beyond Stevin and Wallis before them? In what important respects did these treatises differ from each other? Another field was fluid motion, where Edmé Mariotte, de la Hire and Varignon did important work, but Shank again leaves it untouched.[5]

The other gap he misses is Johann Bernoulli's effect. In the period that interests Shank, Bernoulli was determined to convert French science to a new brand of mathematics that he and his brother, Jakob, had developed from a terse paper by Leibniz. The Bernoullis saw this new brand as a total framework for exact science: equally well suited for integration theory, differential geometry, variational problems, theory of equations, optics, statics, fluid motion and particle kinematics. The French did not miss the power of Bernoulli's framework, for Johann taught them for thirty years. Read Varignon's oeuvre — not just the few papers Shank selected — and one sees how much it follows derivatively Bernoulli's research then. *Before Voltaire* misses the chance to ask: what research agenda, heuristics and methods did Bernoulli bequeath to Varignon and his peers, and how did they fit with Leibniz's effort to secure France for his cause?[6]

Even more misleading is Shank's claim (which he asserts without evidence) that after 1715

> analytical mechanics became an increasingly important centerpiece of French academic science, laying the foundation for the great eighteenth-century architects of modern mathematical physics: Maupertuis, d'Alembert, Clairaut, Lagrange, and, yes, Euler. (p. 36)

That is false. For one, French mechanics remained backward until the late 1730s, when the first three figures above took up problems and techniques that Varignon had not even touched, let alone solved or employed. For another, the basis for Enlightenment mechanics was the following. Mathematically: the concept of function, theory of differential equations and the

[5] Mariotte in the 1670s had drafted a *Traité du movement des eaux*, which de la Hire published posthumously in 1686. Varignon then wrote *Traité du mouvement et de la mesure des eaux coulantes et jaillissantes*, which contains results pertinent to hydraulics and managing waterways. This had been a topic of intense research also for the Bernoullis and Jakob Hermann, whose second half of *Phoronomia* is on fluid motion.

[6] Shank leaves out that from 1686 to 1716 Leibniz corresponded with everyone who was a name in French science: Fontenelle, Lamy, Malebranche, L'Hôpital, Varignon. Fontenelle gave him a 32-page obituary *éloge* in the Academy's transactions. And, Bernoulli had carried out an epistolary exchange with Bignon.

calculus of variations. Dynamically: the concepts of virtual work, action integral and the Lagrangian function. Without these — which owe *nothing* to Varignon and his age — you cannot even state the laws of analytic mechanics, let alone lay the foundation for it.

Also false is Shank's thesis that Varignon created a 'new science' of mechanics (pp. 25–8). It is not new — his mathematical innovations come from Johann Bernoulli. And, it is not a mechanics. A theory of mechanics requires concepts of mass, force, interaction; and dynamical laws, to enable predictions from momentum exchanges. Varignon's MV papers have none of that. They are just a semi-algebraic *kinematics* of one particle under central acceleration; which fact he must represent *geometrically*, by means of infinitesimal segments on imaginary lines all intersecting at one centre, because he lacks the tools to treat it analytically.[7] In reality, Varignon's MV is on a par with Leibniz, Huygens and Bernoulli's attempts to hijack Newton's key results in Book I and II. Shank follows Mahoney 1994 and Blay 1992, who called MV 'analytic mechanics.' But they are wrong, for the reasons I gave above (and below, under 'Method').[8] Then so is Shank, and *Before Voltaire* is not about mechanics. Hence it is misleading to tout it as a 'rigorously historicist scientific genealogy of analytical mechanics' (p. 115).

2. Errors of method

Shank's tools and approaches in this book fail at critical junctures, which greatly subverts his overall case.

Mechanics. Shank makes 'analytic mechanics' into his main interpretive category, but he has no right to his term; here is why. He sets out by promising 'new commitment to precise, historicist rigor' and 'strict avoidance of retrospective' conceptualizations (p. 18). Then right away he refers

[7] Here is a truly analytic definition of central forces. Start with a potential P, given by a scalar field F, with the source at point C of coordinates (x, y). Let the acceleration of force be defined as the negative gradient of P (as Lagrange and Laplace did so in the 1780s). The force is central if the gradients of P are on lines given by equations for which (x, y) is a common solution.

[8] Craig Fraser had already pointed out (gently) that Blay was wrong about analytic mechanics, in reviewing Blay's 'Naissance de la mécanique analytique,' *Isis* 84 (1993), 386-7.

to MV as 'classical-', 'Newtonian-' and 'analytical mechanics.' But these terms *are* retrospective, a-historical, imprecise and anachronistic; and they are empirically wrong. To see why, let us fix ideas.

Classical mechanics has two versions.[9] One is Newton-Euler-Cauchy dynamics (NEC), built on the twin notions of impressed force and torque, with a Generalized Second Law for each.[10] The other is d'Alembert-Lagrange mechanics, in which the basic agency is the Lagrangian, not force. Its statics rests on the principle of virtual work, and its dynamics on d'Alembert's Principle, a heuristic for reducing moving systems to static ones in equilibrium. At first this version was developed to handle constrained systems, which NEC could *not* treat.

Second, in mechanics 'Newtonian' is honorific, not descriptive; and it is equivocal. In one sense, it is another name for 'non-relativistic': thus Galileo and Descartes already were doing 'Newtonian' science. In another, it is a shorter name for NEC dynamics.

Third, in the eighteenth century, 'analytic' was said of two theories. (1) Euler's 1736 dynamics for a mass point, free and constrained. Crucially, Euler claimed *primacy* for his 'analytic' approach, and for using 'mechanics' to name the theory of *moving* bodies; until then, 'mechanics' was the statics of rigid bodies. (2) Lagrange's mechanics of 1788, which counts as analytic in three ways. It deals primarily with constraints; its mathematics is the theory of analytic functions, with an analytic calculus of variations created by Lagrange around 1756; and its formal basis was analytic in the sense of *non-geometric*, which is not true of Leibniz-Varignon calculus.[11]

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[9] 'Classical' arose after 1905, to denote mechanics that is non-relativistic and non-quantized, so the term explains nothing about Varignon's results or approach.

[10] Historically, Newton created it for the special case of two free particles under mutual gravity; Euler produced most of NEC, though *not* by emulating Newton; and Cauchy in the 1820s found ways to extend it to continua (by creating the notions of strain and stress); other key makers of this theory were Navier, Saint-Venant, Truesdell, and Noll.

[11] Namely, Lagrange represented motions via functions, not curves; and derivatives (of motion functions) by coefficients of an algebraic object—viz. the Taylor-series expansion of the function at that point—not via *geometric* objects like infinitesimal sides of 'characteristic triangles,' as Leibniz and Varignon had done.

Against this background, Shank's thesis — that Varignon 'originated' analytical mechanics — is false in all respects. Varignon's MV is not analytic; it is no mechanics; and genuine analytic mechanics owes nothing to him. More generally, Shank missed the actual birth of analytic mechanics — on French soil no less, though from a Swiss father.

That was Jakob Bernoulli, with his terse *Démonstration générale*, printed in the Paris Academy's transactions. His paper had two seminal insights: rigid constraints do zero net virtual work (which he stated as a generalized Lever Principle); and in a constrained system the kinetic reactions balance the impressed forces.[12] The former insight is the seed of d'Alembert's 'general principle' in his *Traité de dynamique*; and the latter is Lagrange's heuristic for reducing dynamics to statics, in *Méchanique analitique* (1788: 184).[13]

That refutes Shank's final claim that 'the deployment of the Leibnizian calculus (…) is the most important continuity flowing through the whole history of the eighteenth-century French engagement' with Newton's book (p. 365). There is not a shred of truth in this. From Clairaut to Navier, the French practically re-created mechanics from the ground up. Research into their vast recreation is ongoing, but it is clear that Shank is mistaken.[14] Which turns into false advertising his to focus 'precisely and attentively on the actual historical steps that led from Newton's mathematical mechanics as published in 1687 to the later science that came to be associated with his name and legacy. (p. 10)

Newton. Another serious concern is that Shank's book is undermotivated. Specifically, Varignon was much less, and Newton immensely more, important than he thinks they were.

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[12] See Jakob Bernoulli, 'Démonstration générale du centre de balancement,' *Histoire de l'Académie royale des sciences* (Paris, 1703), 78-84 (p. 80, 82).

[13] Incidentally, this confirms Truesdell's old claim that Jakob's paper above was 'second only to the *Principia* itself in influence on the later growth of the discipline.' See 'Program toward rediscovering the rational mechanics of the Age of Reason,' in his *Essays in the History of Mechanics* (Springer, 1968), 85-137 (p. 104).

[14] Some synopses are C. Fraser, 'Lagrange's Early Contributions to the Principles and Methods of Mechanics,' *Archive for History of Exact Sciences* 28 (1983), 197-241; and I. Grattan-Guinness, *Convolutions in French Mathematics, 1800-1840* (Birkhäuser, 1990).

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Take Newton first. Of his mathematics in the *Principia*, Shank alleges that it was ‘opaque’, ‘cumbersome’, ‘recondite’, ‘abstruse’ (pp. 131, 5f). But that is very misleading. Newton did not write for a general audience — not even for a knowledgeable but broad readership. His tract was not a college textbook; it is a long research paper — for it began as one — in advanced theoretical mechanics. He wrote it with just two people in mind: Wren and Huygens, after whose *Horologium oscillatorium* he modelled the *Principia*. And, they did understand it — well enough that Huygens (on a 1689 visit to England that Shank ignores) suggested to Newton an alternative measure of force, based in local curvature instead of deflection from inertial path. A few others also understood it well: among them David Gregory and Roger Cotes; Johann Bernoulli, who then gave a stronger solution (to the inverse Kepler Problem) than Newton had; Jakob Hermann, who further developed Newton’s kinematics of motion under central acceleration; and Leibniz, who grasped the *Principia* enough to reverse-engineer some key results (while lying that he had not read it). Shank is wrong to motivate a study of Varignon by claiming that Newton’s mathematical methods were ‘only partially understood, even by the few’ who read it (p. 5).

Now take Varignon. It is also misleading to suggest, as Shank does, that Varignon’s reformulation of some Newtonian proofs helped anybody better understand Newton’s science. There *was* a Continental effort to explain it, but Varignon was absent from it.[15] And, his papers MV were historically inert: no major figure used them for further research. For example, d’Alembert, a father of real analytic mechanics, only used Varignon’s (unpublished) lecture notes on calculus from his days at *Collège Mazarin*. Then what *is* the proof that Varignon originated anything important, let alone analytic mechanics? Answer: none. Thus it is wrong to claim,

> How did it happen that such a dauntingly recondite treatise [viz. the *Principia*] (…), that *this of all books* became within a matter of decades the widely perceived agent of a revolutionary transformation in modern science and even of modernity tout court?

[15] The effort unfolded at two levels of difficulty: ‘s Gravesande and Musschenbroek wrote for intermediate readers, and advanced researchers had du Châtelet’s commentary (1756) or the 4-volume *Principia* by LeSeur, Jacquier and Calandrini (1739–42). The figures they found helpful for explaining Newton were the Basel School: Hermann, Euler and the Bernoullis. Varignon earns a mere three footnotes in their commentary to Newton; du Châtelet does not mention him at all.

> The only possible answer is through a process of translation, and it is this process in its initial iterations that is the focus of this book. (p. 5)

If anyone thinks Varignon mattered to Enlightenment dynamics, she ought to prove it, not assume it. It is a very steep hill.

Regrettably absent from *Before Voltaire* is a crucial fact: Newton set the agenda for virtually all celestial mechanics in the eighteenth century. In Book III of *Principia*, Newton had listed and explained several open problems in his gravitation theory.[16] Solutions to these problems, he rightly thought, would accrue to his dynamics as evidence for its truth. And indeed, the Enlightenment took up Newton's agenda and solved it — or even added to it, when new phenomena became known after his death.[17] To read his agenda in Book III next to the list of prize-essay competition topics at the Paris Academy is to grasp how much Newton marked the century after him. Not Varignon. Then why is he worth a book?[18]

Shank insists at length that, from Newton, the French took *just* his mathematical results, while ignoring his physics and empirical agenda (for example p. 126). This is false. Like the Leibnizians, the French combatted Newton early and resolutely. They rushed to defend Descartes's programme, and compete with Newton, at two levels. Conceptually, Malebranche re-argued that extension is the essence of matter (thus Descartes is right and no vacuum exists, contra Newton); and that *tourbillonaire* theory is sound and consilient — leading to Privat de Molières's comprehen-

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[16] Among them was the motion of the lunar and planetary lines of apsides; the horizontal parallaxes of the Moon and Sun; solar orbiting around the planetary centre of gravity; gravimetric variation with latitude; lunar motion; issues in perturbation theory, e.g. deviations by Jupiter, Saturn and their satellites from exact Kepler orbits; the motion of the tides; comet trajectories; and the precession of the equinoxes; see Newton 1687, Book III, Prop. 3 through 42.

[17] E.g. the Earth's nutation in 1748; which allowed d'Alembert to best Euler and the latter's 'Newtonian', impressed-force approach to nutation and precession.

[18] Niccolò Guicciardini's researches have taught us that Varignon's published papers (in kinematics) amount merely to a reformulation in Leibnizian terms—with a faint move at generalization—of some elementary kinematic results in Books I and II of the *Principia*. Shank adduces no new facts in this regard, and so Guicciardini's assessment (which Costabel seconded *avant la lettre*, as it were), stands unchanged; see his *Reading the* Principia (Cambridge University Press, 1999), on pp. 199-205. I thank an anonymous referee for urging me to make this clear. To make matters worse for Shank's book, whatever Varignon had obtained in his papers got quickly superseded by Jakob Hermann's *Phoronomia* (a comprehensive handbook of 1716) and then by Euler's *Mechanica* of 1736.

sive system of fluid-vortex physics (1742) and Fontenelle's 1752 philosophical defence of them. Empirically, French science issued predictions that competed with Newton's; replicated some of his force-at-a-distance mechanisms from Cartesian, contact-action premises; and re-proved (from differential mathematics) the Newtonian results with key *empirical* import. Here, Varignon did much: he re-derived Newtonian theorems critical to securing *empirical* evidence for the *Principia*'s astronomy, as Fontenelle saw clearly but Shank missed.[19] And, he spent years re-proving Newton's predicted orbits in resisting media — an *experimentum crucis* for deciding between Newtonian and Cartesian astronomy — all the while remaining alert to Newton's signalled uncertainty about the actual strength of resistance.[20]

There are errors of method beyond the ones above. Shank invents categories (for example 'liberal science', 'mechanical mathematics' and 'Euclidean'), but they are not univocal. Then what good are they as interpretive tools? Another term he uses is 'physico-mathematics.' But he never explains its meaning and scope, nor justifies its use — for example, Halley and Borelli used it differently from L'Hôpital and each other. Also, he claims to do the 'archeology' of analytic mechanics, with no explaining what it is; perhaps Foucault's shiftiness about his 'archeology' was forgivable in the Sixties, but in 2018 Shank's evasion about his is not.

3. Problems of evidence and explanation

Evidential shortcomings cause *Before Voltaire* to give a distorted view of its topic. First, Shank refuses to discuss *any* content from the works he mentions — despite claiming to rest his case on a 'mix of institutional and *in-*

[19] These were Newton's theorem on the precession of the line of apses (for a planet orbiting under inverse-square force), which Varignon handled algebraically in O39, in the Paris Academy's proceedings for 1705; and a key result in perturbation theory (viz. when a planet is attracted by the Sun and also by another planet), which Varignon treated algebraically in O33, published *ibidem* in 1703.

[20] Newton started from the simplifying assumption that the drag force on a spherical planet by a (supposed) Cartesian medium could be in proportion to the planet's speed (v), its speed squared (v^2) or some linear combination of them (i.e., as $\mathrm{m}v+\mathrm{n}v^2$). Each case yields a different orbit shape for the body moving in such media; cf. *Principia*, Book II, Sections i-iii. Varignon treated orbits under resistance as v in O46, 48, 51 (written 1709-11); as v^2 in O52-3, 56 (of 1711); and as ($\mathrm{m}v+\mathrm{n}v^2$) in O60, 62 (of 1712, 1714).

tellectual changes' (p. 358; my italics). Second, Varignon wrote much else beyond Shank's chosen papers, MV; and he wrote much on topics closely related to MV. Shank never says why he chose *those* papers above and how they relate to Varignon's oeuvre. His choice of primary sources is arbitrary and very problematic for his project.

The third shortcoming was to leave out of the book some crucial facts. Beginning in 1689, figures on the Continent hurried to publish 'mechanical' theories of gravity — in reality, just kinematics of 1-particle orbits under inverse-square acceleration directed to a fixed point, with and without resistance.[21] First were two papers by Leibniz, *Tentamen* and *Schediasma*, which Johann Bernoulli brought to Paris and explained to Malebranche's group. That led Varignon to publish his *Nouvelles conjectures sur la pesanteur*, of 1690. In the same year, Huygens too wrote a *Cause de la pesanteur*; it contains a predicted value for the bulging of the Earth that differed from Newton's, and competed with it.[22] A key geometric result in Huygens's short tract was a proof that the orbits of particles in media resisting as their speed are logarithmic spirals. Varignon re-proved this result, from Leibniz-Bernoulli calculus, in O47 of 1709.[23] Like Varignon, around 1704 Jakob Hermann began to study orbits under central accelerations; his results ended up in *Phoronomia*, a treatise that major figures read and learned from (including Euler and Lagrange). Varignon's work on orbits in resisting media begins after 1709 — by then the Bernoullis had studied much of it, and Johann was coaching him, in correspondence that Shank ignores. Then Bernoulli in 1710 gave an analytic proof that, *in vacuo*, inverse-square orbits are conics; Varignon re-proved this result two years later. In Part III of *Horologium*, Huygens had created an embryonic theory of evolutes, which Leibniz then expanded, as did Bernoulli (under L'Hôpital's name); Varignon contributed to it around 1714, with his pa-

[21] These are just embryonic treatments, based in the supposition that the central acceleration on the particle comes from some contact action, whose precise mechanism they leave mysterious.

[22] Huygens's predicted value for the Earth's oblateness was 1/578; Newton's was ca. 1/230. The Dutchman's preferred assumption was that gravity varies as the distance from the center of the Earth. In contrast, Newton had derived his value from universal gravity (inverse-square, and acting from particle to particle inside the Earth). I thank an anonymous referee for a helpful correction on this point.

[23] I cite Varignon's papers by their index number in the list from *Briefwechsel von Johann I Bernoulli*, Bd. 2, ed. D. Speiser (Birkhäuser, 1988), 387-408.

pers O64-5. Huygens also started research on curvature; Varignon worked much on this topic, for example in O42, of 1707. In 1674, Malebranche began refurbishing Cartesian physics wholesale, by supplying it with new ontologies of body and force, new laws of motion, and an emended impact mechanics. Under attack from Leibniz, Malebranche kept revising both ontology and mechanics, with new collision theories in 1692, 1700 and 1712.[24] Varignon was an early contributor to this neo-Cartesian renewal, followed by Louis Carré, Rohault, Saulmon, Mazière and Privat de Molières. Thus already by 1692 in France two programmes, Newtonian and neo-Cartesian, competed for supremacy over the new science of motion. A third, Leibnizian programme vied to penetrate Paris and displace both competitors.

Here is how this matters. Even if Shank's premises were true, his explanation is not the only one available; consider this

> **Alternative explanation:** The French took up Newton because Huygens, Leibniz and the Bernoullis had done so, thereby signalling that in orbital astronomy (on which much Cartesian science depended) Newton had set the new standard, so it had to be taken up.[25] Ergo, to explain how and why the Continent received Newton's dynamics, we must look to Huygens and the Basel School first, to the Malebranchists' programme in natural philosophy second, and only then to Varignon.

For his book to be good, Shank first ought to defeat this alternative conclusively. He does not do that. Instead, he just puts forward a thesis — that 'liberal' science, coupled with Malebranche's influence in *mathematics*, was the 'origin' of 'analytic mechanics' — without even mentioning alternatives. That weakens his case in the book not insignificantly.

Finally, there are some regrettable errors in *Before Voltaire*. It is false that Descartes gave 'rigorous natural philosophical demonstrations' (p.

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[24] With his paper O11, Varignon sought to improve Malebranche's theory of hardness (*dureté*), who in the same year (viz. 1692) had published a revised account of his laws of motion, with a new theory of hard-body impact. For details, cf. P. Mouy, *Les lois du choc des corps d'aprés Malebranche* (Paris: Vrin, 1927).

[25] Having lost Huygens and Roberval, in the 1690s the French regarded Leibniz and the elder Bernoullis as their mathematical superiors. Costabel was not afraid to call Varignon a mathematician *de second rang*, with Bernoulli and Leibniz as the true masters; cf. his 'Introduction' to D. Speiser (note 20), (p. 8, 14).

124). Cartesian physics following his laws of motion is *all* presented as 'conjectures' from qualitative premises; conjectural physics was Varignon's approach too.[26] Shank extols L'Hôpital as the 'equal of Huygens, Newton, Leibniz, and Bernoulli' (p. 142), but we have known since 1923 that *none* of his research post-1692 was by him; he bought it from Johann Bernoulli, whom he bound to a non-disclosure agreement that frustrated the young Swiss to no end.[27] Shank does not seem to know what rectification and quadrature were, which causes an unintelligible account of Archimedes's approach to integration (p. 153).[28] It is false that Maupertuis learned mechanics from a '*Traité de méchanique*' [sic] by Varignon (p. 366): that book does not exist; the one that exists, *Nouvelle mécanique*, has no mechanics, being in fact a posthumous work in statics.

In sum, Shank is wrong to call Varignon the origin of any mechanics, be it Newtonian, analytic or otherwise. He and his peers did little of note, and thus Shank confirms *sans le savoir* the older verdict of them as a *Zwischenzeit der Epigonen*, as Fleckenstein put it. In the meanwhile, the story of mechanics in France ca. 1700 remains to be written.

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[26] Varignon published conjectural accounts of gravity, hardness, and solid resistance; see his works O7 (1690), O11 (1692), and O28 (1704), respectively.

[27] Bernoulli also dictated to him the differential-calculus treatise that made L'Hôpital famous; cf. R. Bradley, S. Petrilli, and C. Sandifer, *L'Hôpital's* Analyse des infinimens petits: *Annotated Translation with Source Material by Johann Bernoulli* (Birkhäuser, 2015).

[28] Answer: they were early-modern terms for definite line- and area integrals, respectively (for volume integrals, it was 'cubature' or 'solidity.').