To appear in: *Collected Wisdom of the Early Modern Scholar: Essays in Honor of Mordechai Feingold*, eds. Anna Marie Roos and Gideon Manning.

Newtonianism and the physics of Du Châtelet's *Institutions de physique*

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This paper is about two things that cross paths. One is the many senses of the category 'Newtonian,' and their uses for exegesis. The other is the physics that Emilie du Châtelet grounded philosophically around 1740 in her book, *Institutions de physique*. I offer it as a tribute to Moti Feingold's magisterial work on how the century after Newton responded to his science. I begin with some context.

Many have described *Institutions* as a work of Leibnizian foundations for Newtonian science. An inspiring image, no doubt, but is it accurate? I argue here that it is not: little physics in her book is really Newtonian. Most of her physics is from figures before Newton; and even when she includes his results, du Châtelet filters them through demonstrably un-Newtonian ideas. So, we must abandon the conventional wisdom about her science.[1]

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[1] In the 20th century, the dominant view became that *Institutions* is concerned with "merging Leibnizianism and Newtonianism"; is a "marriage between Leibnizian metaphysics and Newtonian science"; is an "introduction to Newtonian physics" attesting du Châtelet's conversion to "Leibnizian metaphysics"; mediates "between Leibniz and Newton"; is a landmark document in the "history of French Newtonianism" and yet "framed according to Leibnizian principles." Allegedly it is a "fusion of Newton, Descartes, and Leibniz," and it synthesizes "Newtonian physics" and "Leibnizian metaphysics." So pervasive is it that it has seeped into the broader consciousness of Anglophone academia. A philosopher of modern physics: du Châtelet "was one of the first people to bring the Newtonian and Leibnizian traditions together." This collective modern verdict echoes the book's *approbateur*, Henri de Pitot, who gave it his imprimatur in 1738. See, respectively, Wade (1959:

Though he was not the only author of theory then—Descartes, Leibniz, and Huygens were too—Newton's science was especially successful and long-lived, to an extent that no early modern could match. As a result, the attribute 'Newtonian' has come to denote aspects and areas of science that go well beyond what Isaac Newton, the historical figure, taught or implied. For that reason, the historian must handle the term 'Newtonian' with caution and good sense. Its polysemic nature can easily ruin an argument, if the label has not been carefully inspected first.

I believe that sort of mishap has regrettably ruined most cases about du Châtelet's physics being Newtonian. So, to prevent further error and wipe the slate clean for future research, I untangle here the various senses of 'Newtonian science,' and determine which ones, if any, best capture the *physique* of her *Institutions*. I argue that no proper senses apply to it. On that basis, I conclude that 'Newtonian' is not a useful category for her science. Newton's importance notwithstanding, we need a different, better attribute for her physics.

To make my case, I distinguish three semantic regimes for the term 'Newtonian science,' viz. presentistic, contextual, and straddling senses. Next, I examine which of them apply to du Châtelet's physics, and how useful they are. Thus, a plain way to put my conclusion is, there *are* some senses of 'Newtonian' that apply to *Institutions*, but they are trivial and hollow, so they do no real interpretive work. And, there are some useful senses of 'Newtonian,' but her physics does not qualify for them.

1. Presentistic senses

From 1905 to about 1925, physics suffered very drastic changes that replaced most of its foundation wholesale. When modern science emerged from these upheavals, it referred to the one it had supplanted as "classical physics." Scientists with an eye to history (and some historians of science

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46), Barber (1967: 221), Janik 1982, Walters 2001, Gireau Geneaux 2001, Hagengruber 2011, Hutton 2004, Gauvin 2006, Moriarty 2006, and Weatherall 2015.

too) would often call classical physics by the onomastic term ‘Newtonian.’ So, when contemporary science calls a theory ‘Newtonian,’ it does so from a presentistic viewpoint: it takes itself to have overcome or corrected certain limitations in the physics that preceded it.

However, physics counts as classical in many senses, depending on which of its parts our science replaced. And, using Newton’s name for classical science has several, distinct aims. Some meant to credit him as the true author, some merely to honor his work, some used it for lack of a better onomastic, and some spoke out of militancy or deficient scholarship. Thus, referring to classical physics as ‘Newtonian’ because of its lacunas really denotes many limitations. Four, in particular, which yield four senses relevant to my aim:

i. Newtonian vs Minkowskian.
ii. Newtonian vs relativistic.
iii. Newtonian vs quantized.
iv. Newtonian vs modern.

Spelled out in brief, these senses denote distinct features, as follows. For Newtonian physics in sense (i), the metric and temporal properties of objects are borne by two ontologically *distinct* carriers, space and time. Minkowski in 1908 replaced this view by introducing a new entity, spacetime, qua single substratum for Special Relativity. A physics counts as Newtonian in sense (ii) if its kinematics is Galilean. Namely, it allows us to describe the same physical process from any frames that relate to each other by a Galilean transformation. In early-modern physics, that was known as the Parallelogram of Motions. Special Relativity replaced it with the Lorentz transformation, a different rule for describing mechanical processes across frames. In sense (iii), physical theory is Newtonian if it assumes that interactions are ‘continuous,’ not quantized. I.e., momentum and energy transfers in a system can take any value compatible with the relevant laws.

Quantum mechanics denies this assumption, and so it counts as post-Newtonian science.[2] Sense (iv) is a vehicle for the view that *all* of mechanics is grounded in Newton's Second Law, or the principle **f**=m**a**. Specifically, that for every mechanical setup treated by the theory, the Second Law is the dynamical principle used to derive the equation of motion for the setup at issue. That contrasts with post-1925 physics, broadly based in the Schrödinger Equation, for quantum mechanics; and the Einstein Field Equation, for gravitation theory.

Is it a good idea to use 'Newtonian' in any sense above for du Châtelet's science? Answer: No—there are two problems with that.

Senses (i) through (iii) are inadequate for it, because they are trivial and vacuous. These senses apply just as well to mechanics *before* Newton: they turn Galileo, Descartes, Gassendi, Huygens and Leibniz into makers of Newtonian science, because they too relied on the classical assumptions (i) through (iii). Moreover, *everyone* before 1905 counts as Newtonian in these respects. Even Newton's opponents, detractors, competitors and precursors accepted the above classical assumptions without discussion. No one then dreamed of denying them, for they had nothing to replace them with. That makes the term wholly trivial, if used for anything before 1905; but a label too easy to apply can't be the source of real understanding. Plus, the term fails to clarify what is specific about the science of *Institutions*: it is explanatorily empty, so it gives us no real insight into her science. To call her science 'Newtonian' in any of the senses above is as banal as saying that it was printed on paper.

Lastly, when used in sense (iv), the term 'Newtonian' is honorific, *not* explanatory nor descriptive. It is meant as a tribute to one achievement (out of many) in physics before Schrödinger and Einstein. Because of that, it is dangerously misleading. It makes Newtonians out of theorists whose basic concepts and laws are wholly *unlike* Newton's, and competed with his.

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[2] For the classical assumption, the relevant laws are Conservation of Momentum and Conservation of Energy. In quantum theory, momentum and energy are transferred in discrete packets (multiples of the Planck constant, a finite quantity).

And so, if 'Newtonian' is used in sense (iv) above, the label is false. But, if we use a false label, we risk missing what is distinctive and important about her science. Why is it false? Historically, pre-1905 mechanics has some five different formulations, and Newton's impressed-force version is just one of them. Others are due to Leibniz and to figures around du Châtelet's time, like d'Alembert and Euler; yet others come from Lagrange, Hamilton and Jacobi, in the 19th century.

These facts suggest an interim conclusion: an important strand of usage for the category 'Newtonian' is inadequate for explaining du Châtelet's science as it stood around 1740. I move now to another semantic regime for the label at issue.

2. Straddling senses

Another semantic regime includes senses that straddle Newton's age and 21st-century science. In particular, some aspects of physics count as genuinely Newtonian because Newton discovered them, and gave them place of prominence in the *Principia*; but they also reach into our age, which thus regards them as his contribution. In effect, these aspects straddle early and late modernity, and retain the same content across three centuries of theory change. That semantic currency would give any interpretive category a good deal of value.

Thus understood, an area of science is Newtonian when it relies on either of these two assumptions:

v. gravity and inertia are distinct.

vi. all basic forces are central.

Now for some explanation and evidence.

Sense (v) is really two ideas. One is (1) the kinematic claim that gravitation and inertia correspond to distinct phenomena, or patterns of motion. The other is (2) the dynamical claim that gravitation and inertia are distinct causal properties of matter. Specifically, inertia is a 'force' or 'endeavor' to persist in a state—of uniform translation, or zero acceleration. Gravity is a genuine, distinct force; it is a species of *vis impressa* (the genus concept of mechanical agency in the *Principia*), and it causes *non*-uniform motion, viz. accelerated translation or some curved path. Einstein subverted Newton's distinction: in his General Theory of Relativity (GTR), gravitation and inertia are no longer distinct kinds of matter in motion.[3]

Sense (vi) is the thesis that, at explanatorily basic, microscale levels, all physical forces are actions at a distance exerted on the line between point-sized particles. Newton announced this view in *Opticks*:

> Have not the small Particles of Bodies certain Powers, Virtues, or Forces by which they act at a distance not only upon the Rays of Light ... but also upon one another for producing a great part of the Phaenomena of Nature? (Newton 1730: 350)

Admittedly, he did not endorse it as considered doctrine; he thought it was just more plausible than its competitors, and a promising heuristic.[4] Still, Newton did give it qualified assent. Then Laplace and his school adopted this exclusive view of force, and credited it to Newton:

> in the final analysis, the phenomena of nature reduce to actions *ad distans* between molecules. The consideration of these actions must form the basis of the mathematical theory of these phenomena. (Laplace 1898 [1809]: 295)

Many modern philosophers have called this picture 'Newtonian.'[5]

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[3] Lehmkuhl 2014 explains how Einstein thought his GTR unifies gravitation and inertia.

[4] Newton had a high evidential standard, viz. "deduction from phenomena." Not everything he pondered or gave some assent to cleared this high threshold of confirmation. The picture of *all* forces being inter-particle actions does not; only gravity does. Neither does the discrete-atoms picture of matter suggested in Query 31 of *Opticks* (or anything from those Queries, for that matter).

[5] An entire group of figures in physics, from Laplace to Du Bois-Reymond, subscribed to this picture of matter, and associated it with Newton; for evidence and discussion, see Fox 1974 and van Strien 2021. For a recent usage of 'Newtonian,' see Sebens 2015.

In regard to sense (v.1), du Châtelet's physics does count as Newtonian, but trivially: *everyone* before Einstein thought, without exception, that inertial motion and heavy fall are distinct kinds of motion—even Descartes, Leibniz, and Malebranche thought so, as did Gassendi. No wonder du Châtelet does too. Judged from sense (v.2), her physics does *not* count as Newtonian. This straddling sense regards gravitation as a genuine, irreducible force: an action at a distance, seated in bodies made of 'crass' matter. Du Châtelet rejected this premise, however. She regarded gravity as an as-if, or pseudo-force. The kinematic patterns of motion that Newton ascribed to real gravity, she argued, are caused by 'subtle' matter—not bodies—swirling in vortices around the earth, sun, and the planets. *Institutions* regards orbiting, free fall and projectile motion as epiphenomena of ether action by contact, not of a genuine force of body.[6] So, she was an *anti*-Newtonian.

Before I move to check *Institutions* in regard to sense (vi), a note is in order. That sense is very broad in scope. It applies to forces in *physics*, not just in mechanics. Newton and his followers meant it to cover forces responsible for trans-mechanical effects too—capillary action, light refraction, heat flow, electric currents, magnetic phenomena; and even some chemical reactions—not just the motion of inert masses. At the same time, du Châtelet promised a foundation for *physics*, not just mechanics. So, it would be no small matter if the science she grounded counted as Newtonian in this comprehensive sense.

Unfortunately, I don't think it does. Here is a short argument: to count as Newtonian in sense (vi), forces must be exerted at a distance, between point-sized masses standing in well-defined, determinate metric relations to each other (viz. facts like relative distance, angle, and curvature). But, du

[6] This raises the interesting question whether she was a *Cartesian* on this count. I tend to doubt it. Leibniz and Wolff (whom she followed) also rejected empty space, which they filled with ethers and vortices, all while rejecting many of Descartes' central doctrines. Moreover, du Châtelet did use to accept (Newtonian) empty space, until she read Leibniz's and Wolff's philosophical objections to it; cf. Gardiner Janik 1982, and also below (section 3b) for more details. I thank the editors for pressing me on this matter.

Châtelet denies these premises. First, she rejects *all* action at a distance, not just Newtonian gravity; her reasons against it are well documented (for evidence and discussion, cf. Section 3). Second, it would be rash to credit *Institutions* with an ontology of point-masses bearing space relations. True, she did argue that perceptible bodies are grounded in *unextended* 'elements.' However, we have no evidence that she thought those elements carry mass, impressed force, and distance relations to each other. In her ontology, inter-element relations are at best an early version of topological facts: they are relations of serial ordering (being next in a series), not metric properties.

3. Contextual senses

In the contextual approach, we illuminate a historical work by the light that its age cast on it—to understand that work, we look at how *they* understood it. From this vantage point, interpretive categories come in two kinds, internal and external. Here I use 'Newtonian' as an internalist category that covers tenets, heuristics, theoretical foundations, and research programs that Newton himself endorsed, and then figures from 1700 to 1750 associated with him.[7] These two kinds differ in their respective power to explain particular things, and require different types of evidence to establish their presence.

Often, du Châtelet's breakthrough in her *Institutions* is presented as the insight that mechanics needs a philosophical basis to be properly grounded. But Newton knew that too.[8] Accordingly, his *Principia* came pre-equipped with conceptual foundations, which then acquired a life of their own. As a

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[7] In contrast, externalist approaches to 'Newtonian' single out aspects like personal loyalties, political allegiances, group self-identification, elements of material culture, or membership in networks of patronage, of professional advancement, and of the circulation of knowledge.

[8] I use the following abbreviation, followed by the section number in the work: *I* = *Institutions de physique* ([du Châtelet] 1740); *C* = *Cosmologia generalis* (Wolff 1737); *E* = *Elementa matheseos universae* (Wolff 1733). Unless otherwise noted, all translations and emphases are mine. I cite Newton from the second edition of the *Principia* (Newton 1713), abbreviated as '*P*2,' followed by page number; Newton 1999 is the best English translation.

result, by 1740 mechanical theory counted as Newtonian if it was based in one or more of these grounding assumptions.

vii.	Physics requires absolute space and time.
viii.	Mechanics is a theory of impressed forces.
ix.	Gravity is a universal action at a distance.
x.	Mechanics rests on 'hard bodies.'

Let us assess *Institutions* from these standpoints on Newtonian science.

Kinematic foundations. In exact terms, sense (vii) asserts that absolute space and time are indispensable kinematic foundations, *qua* the fundamental frame and chronometer, respectively. Namely, a force-free body moves uniformly straight ahead primarily in respect to absolute space—and, relative to a material frame, only *because* of this fact. Second, an inertial clock keeps true time just in case it takes equal stretches of absolute time for it to mark equal intervals. In a Scholium to his Definitions, Newton gave five arguments that his laws apply primarily to motions in absolute space. His Corollary V then proved that the laws also hold true—but *derivatively*, as a physical consequence of the Third Law—in any material frame at rest or in uniform translation relative to absolute space.[9]

It is unclear how closely du Châtelet had considered his inference. To refute "space distinct from matter," first she brings up Leibniz's point, contra Clarke, that absolute space allows Leibniz-shifts that would reduce God to choosing without a sufficient reason, which "is absurd" (*I* 73, 75). Then she rehearses Wolff's genetic account of our notion of space—by "abstraction," from considering "things existing outside each other"—to claim that space is *just* the intentional object of a mental representation common to us all. She infers, "it is certain that no space exists except insofar as there are real coexisting Beings; and without these things there would be no Space," which "does not subsist beyond things" (*I* 77, 79, 87). Then she makes a

[9] The argument is in *P*2: 5-11; Newton 1999: 408-14.

parallel argument against absolute time, and concludes, “Time in reality is nothing but the order of successive Beings... So, no Time exists without real successive Beings ordered in a continuous series” (*I* 102).

And, du Châtelet is explicit about the targets of her attack: Newton, and also Locke, Clarke, Raphson, and Keill, whose *Introductio ad veram physicam* had riled Wolff greatly. Thus, in respect to kinematic foundations she counts as *anti*-Newtonian; which disproves the received view.[10]

Dynamical laws. Spelled out precisely, sense (viii) is the view that mechanics is the science of motions caused by *vis impressa*, the type of force governed by Newton’s three laws, not just anything. The First Law, of inertia, specifies how a body behaves in the absence of impressed force. The Second codifies its nature and strength: impressed force is an action, and equals the “change in motion” of the body on which it acts. The Third fixes relations between impressed forces: they are interactions, viz. come in pairs, are equal and opposite, and act on different bodies. Newton had made the Law of Inertia into his first law of motion, and *Institutions* seems to follow suit. However, just stating the Law is no proof of Newtonianism, because the principle precedes Newton by five decades. It would make du Châtelet a Newtonian if she meant it as *he* does. But, she does not. For one, she does not claim—as she should, were she a Newtonian—that only *impressed* forces can change a body’s inertial state. For another, she followed Christian Wolff in his denial of absolute space; which cuts her off from this brand of Newtonianism. In the *Principia*, inertial rest and uniform translation are defined in respect to absolute space, the preferred frame for the true motions of bodies. Du Châtelet won’t endorse that thought, but offers no alternative to it. So, it is unclear, from *Institutions*, relative to what a force-free body stays at rest or moves inertially. Not only is her first law of motion not a Newtonian principle, but also it appears empirically empty.[11]

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[10] Wolff to his confidant, Count Manteuffel, 7 June 1741: “If you read what she says on space [in Chapter V], you will find that she speaks there no differently than I used to do, in my classroom lectures” (Ostertag 1910: 41).

[11] In her book, the Law of Inertia is a claim about bodies having a ‘passive force’ of resisting; it is not (as Newton had it) a principle about endeavoring to remain in the same state

Still, one law in *Institutions* seems Newtonian beyond doubt: "the reaction is always equal to the action" (*I* 259). Surely this must be her statement of Newton's famous Third Law? And yet, I argue, this appearance deceives badly. Du Châtelet's principle is ultimately *Leibnizian*, and strictly incompatible with Newton's science. Her former allegiance to Newton, abjured once she discovered Wolff's philosophy, survives as an afterthought to her account of action and reaction. To prove my claim, I begin by showing the true genealogy of du Châtelet's law. It is not the *Principia*. Rather, it comes from *Phoronomia*, an influential 1716 treatise on mechanics by Jakob Hermann, a Swiss protégé of Leibniz. Take her twin claims that a body's force of inertia is the cause of its reaction; and that any interaction is a conflict of forces between a body that acts and one that resists:

> This resistance—which all Bodies put up when one tries to change their present state—is the foundation of the third Law of motion, according to which the reaction is always equal to the action…. Bodies resist by their force of inertia, and in reacting they tend to change the state of the Body that pushes them…. In every action, the Body that acts and the one against which it acts struggle with each other, and without this sort of struggle [*lutte*] there can be no action. For, I ask, how can a force act against that which opposes no resistance to it? (*I* 259; my emphasis)

Consider now its source in *Phoronomia*:

> In this force of inertia of matter is grounded the law of Nature whereby to every action there is an equal and opposite reaction. For in every action there is a struggle [*luctatio*] between an agent body and a patient one, and without such struggle no action, properly so called, of the agent upon the patient can be conceived…. Hence in all corporeal action there is a clash between an agent force and the resistance of the patient body, an application of the agent's force onto the body receiving the action; that is. (Hermann 1716: 3, 378).

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of motion. And, because she does not say relative to what material frames her Law holds, we cannot determine what empirical facts might make it true.

Now take her explanation that action consists in the spending of *active* force by the *agent*, or acting body. She phrases it as her answer to a supposed objection: if action equals reaction (e.g. when I pull on a body) they must balance each other. Then only rest, not motion, can result from them.

> Those who raise this objection fail to grasp that when I pull on this Body and I make it move, I do not use all my force to defeat its resistance. Rather, when I have defeated it, I have some force left over, which I use so as to proceed; and the Body proceeds by the force that I communicated to it. Hence, although the two forces are unequal, action and reaction are always equal. (*I* 259)

Consider, again, its source in *Phoronomia*:

> The force of a body is not the action itself. For, action is just the application of some force onto a subject capable of receiving it, or to which force can be applied. Hence, we must hold that the said force is applied to that body which resists, withstands, reacts.... Therefore, as we say that any action is equal and contrary to the reaction of the patient body, all we mean is: in all corporeal action, as much of the agent's forces is lost as it is gained by the body receiving the action. Action itself is equal and contrary to the resistance of the patient, which is its reaction, because this resistance—this force of inertia—by the patient body must be removed first, so that the patient might be set in motion by the agent (Hermann 1716: 378f.)

This view of action and reaction goes back to Leibniz, who had endorsed Hermann's account in a letter of 1715: "The *inertia* of matter that you discuss in § 11 is a wonderful topic, fit for the deepest research; few have grasped it so far.... [Without it] there is *no reason why* there would be a *struggle* between *agent and patient* [in impact]" (Gerhardt 1860: 398, my italics). Then sanctioned it in his anonymous review of *Phoronomia*: "Hermann notes that the rule 'to every action there is an equal reaction' follows from the *inertia of matter*, first discovered by Kepler" ([Leibniz] 1716: 2).

Wolff knew it, and in his *German Metaphysics* he took over Hermann's principle.[12] That may be where du Châtelet first found it.

Crucially, du Châtelet's law of action and reaction is un-Newtonian in essence too, not just in genealogy, because her law is not a statement about *impressed* forces. She regards interaction as an *asymmetric* clash between an agent and a patient deploying *heterogeneous* mechanisms: the agent acts by spending part of its active force so as to defeat the patient, whereas the latter reacts by exerting a *distinct*, passive power to resist, or 'force of inertia.' Moreover, because of her denial of action at a distance, du Châtelet restricts the law to contact interactions. And, due to her conversion to Wolffianism, her measure of action and reaction is unclear, and her distinction agent-patient is not well defined.[13] In radical contrast, Newton thinks of interaction as a symmetric exercise of homogeneous powers: Newtonian reaction just *is* action, not different in kind from it; actions and reactions just *are* impressed forces, not different from them.[14] His measure of both is the "change in motion" mutually induced by the interacting bodies, viz. two equal and opposite momentum increments. Newtonian mechanics neither supports nor needs a distinction between agent and patient. And, it allows *direct* action-reaction at a distance, which makes Newton's Third Law vastly broader in scope than du Châtelet's law.

In conclusion, her laws of mechanics are *not* Newtonian principles, despite their surface similarity with those. Her's and Newton's respective laws differ fundamentally in meaning, empirical measure, range of application,

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[12] "As Professor Hermann has [explained], we must *not* take the force of a body to be *the same* as its action—as those who see a problem in our law of action and reaction do. For a body does not act on another with all the *force that it has*, rather only to the extent that the other body *resists* it. The action of a body A on B consists in that A *breaks the resistance* of B. Then, when B resists it no longer, A pushes it along without any effort, insofar as it lies in the way of its motion."—Wolff 1720: §§ 669-71.

[13] Cf. Section 3b for thorough discussion, below.

[14] Newton defines impressed force as "*the action* exerted so as to change its state, whether of rest or moving uniformly straight ahead." Then he explains, "this force *consists in the action alone*, and does *not remain* in the body after the action. For, a body stays in any new state *solely* through the force if inertia" (*P*2: 2, my italics).

and explanatory payoff. Hence this part of the received view about Newtonianism in her treatise collapses as well.

Action at a distance. In sense (ix), science is Newtonian if it assumes that any two particles of matter act on each other by a force akin to terrestrial gravity; and they exert it *directly*, without any material intermediary, e.g. some subtle medium or ether. Is du Châtelet a Newtonian in the third sense above? To decide if she is, we must turn to Chapter 16 of *Institutions*, entitled 'On the Newtonian attraction.' It examines the view that gravity is a universal *actio in distans*, and that some phenomena—capillarity, some chemical processes, and optical refraction—are explainable as phenomenological effects of real action-at-a-distance forces. Namely, long-range in the case of gravitation, and short-range for the remaining cases. Du Châtelet does not even try to present the argument for universal gravity in Book III of *Principia*, let alone his most important result, viz. the proof that gravity is a central force, proportional to the mass of the interacting bodies divided by the square of their distance (*P*2 362-72). She just reviews a set of theses, which are Newtonian in asserting the existence of various *real* action-at-a-distance forces, including gravity.[15]

Decisively, du Châtelet introduces these theses only to *reject* them as false explanations. She attacks them twice, by the same strategy. Specifically, she argues that genuine action at a distance violates the Principle of Sufficient Reason, PSR, a fundamental metaphysical constraint. "This principle of sufficient reason ... which is impossible to give up, *destroys* that magical Palace erected on attraction" (*I* 395). Suppose two bodies A and B to be separated by a finite distance. Without contact action (by an intervening material medium), she claims, "there is no sufficient reason" why B should accelerate toward A. Also, there must be a sufficient reason why A would accelerate B (from a distance) in this direction rather than that, and

[15] Many of the results she presents are not due to Newton but to self-professed disciples: John Keill and John Freind in Britain, and her friend in France, Maupertuis. These results do not come from the *Principia*, are not derived by Newton's method there, and are not even natural extensions of his theory by means of the Second Law. Keill, Freind and Maupertuis were Newtonians in the senses (viii) and (ix) above.

by this amount rather than that. But, there is none, she contends. Ergo, attraction is merely apparent, not real. That is, apparent accelerations between distant bodies are just that—the illusion of action at a distance. In *reality*, they are effects of contact action, be it a vortex of subtle matter or impact by unobservable particles. In her words, the PSR entails that "these effects must be produced by *mechanical* causes," and so "the English" stand refuted. Ergo, "the supposition that all matter is heavy"—viz. gravitates toward any other piece of matter—"is entirely false." For, "by the principle of sufficient reason," heaviness is an "effect of *impact by a surrounding matter* that is not itself heavy" (*I* 399, 76).

Du Châtelet adopts here Wolff's case in *Cosmologia.* A fierce opponent of 'Newtonian' action-at-a-distance generally, and of universal gravity in particular, Wolff had argued that

> No change can be induced in a body except by another body contiguous to it.
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> A body does not act on another unless it presses against it [*in ipsum impingit*]. For a body A to act on another B, it is necessary that there be a reason why it acts on it rather than not. Because a body always acts by means of its active force, hence only if it is in motion: it is evident that there is no reason why A should act on B as long as A moves freely and there is nothing to obstruct its motion.
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> Action at a distance is impossible. ... Suppose—if it were possible—that body A acts at a distance. Let it act on B placed at some interval from it, say 20 feet. And, suppose it produces in B a motion. ... From the fact that A persists in its given place and is endowed with active force, we do not understand at all [*minime intelligitur*] why B, which sits away from A, must move. ... Consequently, B's motion lacks a sufficient reason, hence is sheer accident [*casus purus*], which is impossible (by §§ 56, 94-5 of my *Ontologia*). So, action at a distance is impossible. (*C* 128, 320, 322; his italics)

As early as 1709 Wolff had engaged in sharp polemics with Keill and Freind, the two defenders of 'attraction' that du Châtelet too chastises in Chapter 16.[16] Whatever her early opinions on distant action might have been, by 1739 she had come to believe that it could not be real. The cause

[16] Their polemic exchanges are briefly surveyed in Thackray 1970: 58ff.

was Wolff's teaching, as he boasted in 1740: "She had previously accepted the Newtonian attractions as true, whereas now she grants that one *cannot* admit them as anything but a *phaenomenon*." And also: "the Marquise declares herself ready to burn *all* her received opinions at the altar of my doctrine—as she has *already* done with the Newtonian attraction," i.e. universal gravity (Droysen 1909: 229). In conclusion Chapter 16, far from being an endorsement of Newtonian science, is in fact a sharp rebuke of Newton. It is really the last salvo in Wolff's thirty-year war against the Newtonians.

To sum up, in the sense (ix) above du Châtelet is an *anti*-Newtonian, because she rejects both the key thesis of the *Principia* and its extension to chemistry and optics.[17] In fact, the situation is even more severe. Du Châtelet's two commitments—to the denial of real action at a distance; and to the Hermann-Wolff law of action and reaction, which is a principle of *one-way* causation, not of mutual interaction—entails that she must *reject* as illegitimate most of the physical theory in Newton's treatise, except for the important but rather narrow results in Sections 2 through 10 of Book I in *Principia*. Her two commitments above entail that Sections 11 through 14—the remainder of Book I, where Newton treats interactions, perturbation theory, and the gravitational potential of spheroids—are nothing but mathematical models with *no* physical import. In sum, du Châtelet's acceptance of Wolffian dogma puts most of Newton's science out of reach for her. The conventional wisdom on her physics looks untenable.

Matter theory. In the sense (x), science is Newtonian if it supposes that the preferred matter theory for mechanics is the 'hard body' and empty space. In modern terms: that macroscopic bodies are composed of rigid atoms interacting *in vacuo* through action-at-a-distance forces and contact forces. Newton in the *Opticks* had asserted that rigid atoms are the most

[17] Newman 2017 is the standard account of the reception of Newton's particle-and-force chemistry in the early 18th-century.

plausible unit of matter: "all Bodies seem to be composed of hard Particles" (1730: 363). His followers in Britain endorsed this picture of matter.[18]

Does du Châtelet count as a Newtonian in this sense? No, she does not. First, she rejected rigid atoms, the Newtonians' preferred architecture of matter. She argues that atoms have shapes, and for atoms to be real, a sufficient reason must exist for them having this rather than that particular, determinate shape. But, no such reason can be given: "if small atoms were swimming in a vacuum, their size and shape would be without a sufficient reason" (*I* 73). So, atoms are not real. Wolff had dismissed them by the same move: "An *occult quality* is that which lacks a sufficient reason why it inheres in its subject, or even why it can possibly inhere. *The shape of material atoms is an occult quality*" (*C* 189-90; his emphasis).

In addition, du Châtelet also excluded empty space, which she thought physicists had adopted "on Mr. Newton's authority." To subvert the "absolute vacuum," she deploys the argument from Leibniz-shifts, which she considers "unanswerable" (*I* 73, 75). Having rejected real *actio in distans*, she then claims that all apparent effects of distance action must be caused by a subtle medium acting by contact. Clearly this medium must pervade all space up to cosmic scales—so as to account for the appearance of celestial attractions—hence du Châtelet's world is a plenum in which vortices carry continuous, deformable, gross matter around: the "hypothesis of a vacuum is false, and there is no such empty space in Nature" (*I* 75-6). In content and intent, this is *anti*-Newtonian.

In conclusion, there is nothing about the grounding principles of du Châtelet's mechanics that we may call Newtonian legitimately and usefully. The standard view once again looks untenable.

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[18] Newton had committed to void space already by the time of *De Gravitatione*, written no later than 1684. In *Principia*, he made a strong case that interplanetary space must be empty, not filled with the Cartesians' ether vortices. There is some debate about whether Newton admitted real action at a distance; Henry 2011 persuaded me that he did. For his matter theory among his followers in Britain, see Heimann and McGuire 1971.

3b. Newtonian mechanics: empirical theory

There is yet another sense to 'Newtonian science.' This particular meaning is robust, illuminating, and far from trivial. It takes some detailed argument to establish its presence, so I have set it aside for special treatment here. It is as follows:

> **xi.** Originating in the *Principia* and explained as it does: from the notion of impressed force, the basic concept in Newton's dynamics.

This sense of 'Newtonian' is powerfully explanatory, and would cast much needed light on her book. It would explain why each result is in there: because it follows from the basic notions. And, it would show the physics of *Institutions* to be a unified theory—built on a single, common basis—not a patchwork of juxtaposed doctrines. Then let us examine it more closely.

To say that her science is Newtonian in this way is really a conjunction of two claims. First, that some of it comes directly from the *Principia* as Newton had treated it there. Second, that whatever in her book is not from Newton is an extension of his theory by means of the Second Law, his general principle. I postpone the first claim for later. Is the second true? By the time *Institutions* came out, there had been just two extensions of the Second Law to novel setups beyond the *Principia*. One was a treatment of hard-body impact, viz. the frontal collision of two translating rigid spheres, in a prize essay that du Châtelet must have read.[19] The other was a 1736 paper on the collision of rotating bodies, which she could not have seen (Euler 1744). But, there is *no* collision theory in *Institutions*, and thus no trace of these parts of Newtonian science properly so-called. As to other extensions of the Second Law—to fluid motion, rigid-body dynamics, elasticity and celestial mechanics—they are all subsequent to du Châtelet's treatise, sometimes by decades.

[19] That was MacLaurin [1724] 1732, a paper published in the proceedings of the Paris Academy, which she followed closely.

And so, the last hope for the received view is to prove that du Châtelet's science is Newtonian because her empirical results come directly from his book. I now examine the relevant chapters in *Institutions*, to see if they meet this criterion. However, first I must introduce a distinction between two types of acceleration in a region, or neighborhood:

Laminar: the acceleration is equal and parallel, at every point.

Central: it points to a single center, and varies with distance from it.

The distinction seems trifling, but it does real work. Gravity-like effects and motion patterns (orbits, trajectories and configurations) had been studied *before* Newton. But, pre-Newtonian science had always supposed the acceleration of gravity to be laminar. Newton innovated significantly, by supposing it to be central as defined above (and then showing universal gravitation to *vary* as the inverse-square distance from the center of force). This matters greatly for my case, to which I return.

Chapter 11 is a kinematics of straight-line motion, uniform and accelerated. These results come from Galileo, not Newton. In Parts I and II of Day Three in his treatise, *Two New Sciences*, Galileo had quantified the relations between speed, time, and distance for a body in uniform translation and free fall under laminar acceleration (Galilei 1638: 150-77). Soon after that his student, Torricelli, re-derived these relations in *De motu gravium* (1644: 97-126). Then Huygens reprised them in Part II of *Horologium oscillatorium*, his masterpiece (1673: 21-31). Admittedly, these figures show their results in terms of Eudoxean ratios (between homogeneous magnitudes) whereas du Châtelet gives them as algebraic expressions. That is because she took them from Christian Wolff's *Elementa matheseos*, an international bestseller reissued throughout the 18th century.[20] In sharp con-

[20] Cf. *E* 1-110. Wolff cites *Varignon* as his precursor in the "analytic," or algebraic, account of motion "in the Galilean hypothesis," i.e. the assumption of *laminar* gravity (*E* 103).

trast, Newton's theory of free fall is about bodies accelerated toward a *center*, by a force *varying* as the distance to *it* (*P*2 105-11). It is not restricted to *laminar* acceleration, as Galileo-Huygens theory is. But, it is the latter that du Châtelet presents, so her Chapter 11 is not Newtonian science.

Chapter 12 is an outline of "composite motion," i.e. curvilinear trajectories. First, du Châtelet presents a basic kinematic principle for it: the Parallelogram Rule for accelerations and forces. The principle is old. Applied to powers in static equilibrium, its mention and use goes back to Aristotle's Hellenistic followers. Galileo had explained projectile motion as a "resulting composite motion" [*motus quidam emerget compositus*] from horizontal translation and laminar fall (1638: 237ff, 250). So had Huygens, who followed him on this topic (1673: 21). Newton derived the Parallelogram Rule in the *Principia*, as Corollary I and II to his laws of motion, and applied it to planetary orbits to show that they obey Kepler's second law (*P*2 13f., 34ff). But Newton made clear that the Rule was not his discovery, and du Châtelet does not claim that either. Hence the Parallelogram Rule, and by extension Chapter 12, is not Newtonian science in the sense at issue.

There follows a diptych of theory and confirmation. Chapter 13 outlines a theory of free fall and vertical ascent near the surface of the earth without air resistance. Du Châtelet adds to it some key relations between speed, distance, and time of fall. The theory is not Newtonian in any meaningful sense. For one, she has a Leibnizian conception of the cause of fall.[21] Moreover, her quantitative relations come from Galileo, who first derived them in *Two New Sciences* (1638: 156-72). Critically, just like the rest of her science, this particular theory too has a limitation that makes it essentially *non*-Newtonian: it is restricted to laminar gravity. The "cause that makes a body fall is supposed to act *equally* in every instant," she declares (*I* 305, my italics). So, it does not vary with distance. The accompanying Chapter 14 surveys the experimental evidence for *Galileo's* theory of free fall—again, not Newton's science. If we trace the findings and experiments that

[21] It is Leibnizian because she adopts his view in *Specimen dynamicum* of 'heaviness' as a *dual* agency causing 'dead force' in constrained bodies and 'live force' in free ones.

she reviews, we find Riccioli and Grimaldi in the 1650s; Huygens and Mariotte in the 1670s; and figures closer to her time, like Desaguliers, Frenicle de Bessy and Pitot, who refereed her *Institutions*. Newton finally makes an appearance, but just as a figure among many. She reports his pendulum experiments, but the conclusion she draws from them is ambiguous. For Newton himself, they were proof that a body's amount of matter [*copia materiae*] is always proportional to its weight (*P*2 365f).[22] However, du Châtelet explains them as evidence that weight is proportional to bodies' "amount of *proper* matter," and also to their "mass" (*I* 323). But, she leaves the first term unexplained and the second undefined—so, it is unclear how they relate to each other, and whether she understands mass as Newton does, viz. qua measure of resistance to changes of state. What *is* clear is that her science in these two chapters is Galileo's kinematics of free fall—not Newton's dynamics of gravity, in which the acceleration is central, not laminar (*P*2 105-14).

Chapter 16 is a digression on metaphysical foundations, not empirical theory. I have discussed it above (in § 3), so I will not examine it further except to say that it is not genuinely Newtonian science.

Chapter 17 presents a qualitative theory of constrained fall on an inclined plane (simple and compound) and an immovable surface. In two respects, these results are not Newtonian. First, they come from figures before Newton. Galileo devised the modern theory of the inclined plane, in Day Three of *Two New Sciences* (1638: 177-235). Building on an idea by Simon Stevin, Torricelli soon refined and extended Galileo's theory (1644: 127-53). Then Huygens in *Horologium* made it rigorous, then extended it to compound inclined planes and—by letting the planes shrink to infinitesimal lengths—also to bodies falling on an immobile curve, viz. a cycloid (1673: 31-59). Second, these results suffer from a common limitation. Namely, they assume the acceleration of gravity is laminar. In contrast, Newton had treated constrained fall (on epicycloids and hypocycloids, in

[22] In modern terms, Newton produced experimental evidence that inertial mass is equal to *passive* gravitational mass.

Section 10 of Book I) under central accelerations varying as some distance to the force center, *posita cuiuscunque generis Vi centripeta* (*P*2 132-9). The sheer generality of Newton's analysis turns the Galileo-Torricelli-Huygens results into special cases valid just in the limit case of the center of acceleration being at infinity. But, *Institutions* takes up just these limit cases—yet *without* showing them to be special consequences of Newton's gravity—and so Chapter 17 is not Newtonian science.

Chapter 18 outlines a theory of the simple pendulum, including two conditions for isochronism, and ends with a qualitative account of the compound pendulum. This account comes from Huygens, not Newton. In Sections I and IV of *Horologium*, Huygens had presented mathematical treatments of the mass-point and the compound pendulum, respectively.[23] Again these results are un-Newtonian. The first theory lacks the generality of Newton's treatment of isochronous oscillation (*P*2 139-47). And, the second is simply *unobtainable* from Newton's principles. A fact often overlooked is that some setups of classical mechanics cannot be treated from the dynamical laws given in the *Principia* (cf. Smith 2007). Huygens' compound pendulum, which du Châtelet takes over in *Institutions*, is one such setup, and so her Chapter 18 cannot possibly be Newtonian.

Chapter 19 gives a theory of projectile motion without air resistance. It too comes from Galileo, who first treated the topic in Day Three of *Two New Sciences*. There he obtained the key results of du Châtelet's chapter—based on a striking proof that the projectile path is a semi-parabola, if the projection is horizontal (Galilei 1638: 237ff; *I* 507). Her Chapter 19 has the same drawback that makes it pre-Newtonian: it limits the account to motion in laminar gravity—as Galileo had done—instead of Newton's motion under arbitrary central forces (*cogente Vi quacunque centripeta, P*2 114-20).

[23] The former, also known as the 'simple' or 'mathematical' pendulum, models the bob as a *point* mass attached to a weightless rod. The compound pendulum models the bob as an *extended* rigid body, attached to a flexible string. To treat the latter, Huygens had employed an energy principle—a claim about the total *vis viva* in the bob—not Newton's laws of force, which cannot yield a solution.

Lastly, Chapter 20 is a doctrine of 'dead' forces. That is Leibniz's coinage in *Specimen dynamicum*, where he distinguished it from 'live' force. Du Châtelet imports his terminology and concept, including his examples.[24] Again following Leibniz, her paradigm is the forces exerted by bodies in static equilibrium. In every conceivable respect, this chapter is *not* Newtonian science. Statics, the science of 'dead' forces, long precedes Newton; its key results come from Archimedes, the medieval 'science of weights,' and Stevin. And, du Châtelet's two conditions for static equilibrium come from Torricelli and Varignon, who worked before Newton and outside the Newtonian tradition, respectively. There is *no* statics in *Principia*, whether of 'dead' forces or anything else.

Chapter 21 is outside the scope of my topic here: it explains and defends *vis viva*—a Leibnizian idea irrelevant to her Newtonianism, except perhaps as evidence against it.

The missing Newton. Because Chapter 15 presents some of Newton's results in the science of motion, I set it aside here for extended scrutiny. Du Châtelet's ostensible topics in it are *elliptical* orbits under inverse-square accelerations directed to a fixed center; Newton's 'Moon test' and theory of *terrestrial* gravity; and a digression on the Earth's shape at the North Pole, which a 1736 French expedition to Lapland confirmed.[25] These results do come from the *Principia*, though flattening at the poles is predicted in it, but not proved empirically. Still, that is not enough to justify calling them instances of Newtonian science. That is because the results presented are just kinematic: they are facts about accelerations, trajectories, and shapes. For them to be Newtonian, du Châtelet should have attached *Newton's* causal mechanism for them, viz. universal gravity acting from particle to particle.

[24] Cf. Leibniz (1989: 119f.) versus *I* 519-23. However, du Châtelet innovates by distinguishing between active and passive dead forces, which Leibniz never did (*I* 528).

[25] Significantly, du Châtelet does not discuss inverse-square orbits in *general*, viz. conic-section trajectories, as Newton had done. This too militates against calling her science Newtonian; see below.

This condition is sine qua non for Newtonian science properly so-called, because the kinematic facts of her Chapter 15 can, and *had been*, explained also from *non*-Newtonian principles in competition with Newton's science. In particular, Leibniz in 1689 had modelled elliptical orbits (around the Sun) from contact pressure by an oscillating vortex. Huygens in 1690 had elaborated a contact-action theory of terrestrial gravity, with *Discours de la cause de la pesanteur*. And, he had matched Newton's prediction of the Earth's flattening at the poles, which, however, Huygens explained from action by contact, not from gravity at a distance.[26]

More generally, as of 1740 ether-vortex theories of planetary and terrestrial gravity were still being worked out, as sophisticated updates to the Descartes-Leibniz-Huygens alternative to Newton.[27] Thus, if we juxtapose du Châtelet's empirical account of gravity effects with her denial of action at a distance (and concomitant support for ether-vortex actions) it turns out that Chapter 15 is an *alternative* to Newton. It is not real Newtonian science.

Absent structure. So far, I have subverted the received consensus on du Châtelet by showing an absence of evidence for it. Now I move to strengthen my case: I use absence *as* evidence. For *Institutions* to be genuine Newtonian science, it *ought* to present and endorse the two elements that set Newton's dynamics apart from all competitors: the claim that gravity is a *universal interaction* exerted from *particle to particle*; and the *novel* results that his claim entails in *Principia*. The following results depend on Newton's weaker thesis that, at celestial scales in our system, gravity is an

[26] Huygens' causal mechanism was a pressure gradient induced on falling bodies by an ether vortex rotating around the Earth's center; see Huygens (1690: 131).

[27] After *Principia* came out, Leibniz outlined an alternative account of motion in elliptical orbits, with his 1689 *Tentamen de motuum coelestium causis*. Janiak (2015: 118ff) explains lucidly how Leibniz's physics in that paper differs from Newton's. As to the Earth being oblate—not oblong, as some Cartesians then said—Hermann had predicted that too, from non-Newtonian principles. So had Huygens, from the assumption that "in the spherical space comprising the Earth and the bodies around it up to a great distance, there is a *fluid matter* consisting in very small parts diversely agitated in every direction with great speed," which, unable to escape, begins to rotate "in spherical surfaces centered around the center of its space," hence also of the Earth (Huygens 1690: 135). For discussion, see Todhunter (1873: 28-63).

interaction: the theory of the Moon's motion under gravity from the Earth and the Sun jointly; the theory of the tides; a theory of comets orbiting in conic sections around the Sun; a theory of Jupiter and Saturn perturbing each other's orbit by mutual gravity; the theory of the Sun's motion around the common gravity center of the Solar System. Another result (Newton's predicted ratio of the Earth's bulging) depends on the stronger thesis that gravity acts between the *particles* of bodies.[28] However, her book neither endorses universal gravity nor treats its specific consequences above.

My second piece of evidence is from lack of relevant structure. Suppose that *Institutions* were a presentation of Newton's dynamics. Then we should expect it to match his argument structure, which is as follows. 1. A kinematics of one particle in central-acceleration fields. 2. The gravitational dynamics of two free particles in a vacuum. 3. A theory of spherical bodies moving under gravity in a viscous medium resisting as the first or second power of the body's speed. 4. An application of these results to the orbits of planets and satellites in the solar system, culminating in an inductive inference to universal gravity, followed by a set of predicted effects that would further confirm Newton's theory.

That is not at all the expository structure of du Châtelet's physics. For instance, nothing in *Institutions* corresponds to parts (2), (3) and (4) above. In reality, the order of her empirical theory mirrors Wolff's presentation in his compendium, while covering a narrower range of topics.[29] The implication of these facts is that her physics is not Newtonian in any useful way. It is not even "Newtonian in its basic mechanical principles," as some thought (Iltis 1977: 31). The received consensus appears untenable.

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[28] Separately, Newton, Huygens, and Hermann had not just inferred to equatorial bulging, but had also given numerical predictions for its amount—as a ratio of two earth radii, to the North Pole and the equator, respectively. Newton's predicted ratio was 229/230, while Huygens' was 577/578. These values differed because they worked from different assumptions about the source of terrestrial gravity: Huygens from ether action by contact, and Newton from action at a distance acting *from particle to particle.*

[29] Cf. Wolff, *Elementa matheseos* (*E* II, 1-330).

Orthogonal senses. Lastly, the category 'Newtonian' has two more contextual senses pertinent to natural philosophy, which was a field broader than mere mechanics:

xi. The right mathematics for physics is the geometry of fluxions.
xii. Light consists in an emission of special particles.

We can dispatch these two senses quickly. *Institutions* does not contain a philosophy of mathematics (to which sense (xi) speaks) and so, that brand of Newtonianism is irrelevant here. Further, du Châtelet did have an optics—to which sense (xii) would be relevant—but she expounded it elsewhere, not in *Institutions*.[30]

4. Conclusions

I have argued that the *physique* of *Institutions* is not Newtonian in any useful sense. Its core is a heavily kinematic theory of 1-particle motion, which du Châtelet found in Galileo, Torricelli, and Huygens. In light of these facts, the ruling scholarly consensus—that she gave Leibnizian foundations for Newtonian science—is untenable.

That consensus is being challenged on other fronts as well. Elsewhere, I have argued against its other half, viz. that du Châtelet gave us a *Leibnizian* foundation for science. Recently, a better-argued account of her foundational project has emerged (Brading 2019). Du Châtelet really aimed to solve certain related problems in the fundamental physics of her time, irrespective of its authorship. Specifically, she aimed to elucidate the nature of bodies, the mechanism of the causal actions they exert, and the epistemic constraints on theory-building in physics. She did not think that Newton alone was worth foundational attention; nor that he had displaced all physical doctrines that preceded him.

[30] Gessell 2019 analyzes her theory of optics, and connects it to her views from *Institutions*.

Acknowledgments. I am grateful to Katherine Brading for long and illuminating exchanges on du Châtelet; and to Alison Peterman for very helpful comments on an early draft. I have much benefitted from discussions with Anne-Lise Rey, Andrea Reichenberger, Qiu Lin, Ruth Hagengruber, Sarah Hutton, Marcy Lascano, Karen Detlefsen, Jeffrey McDonough, and Andrew Janiak. For useful questions, I thank audiences at Boston University, Columbia, and Harvard. I wish to thank the editors for their very helpful work on my paper.

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