

**On the role of Newtonian analogies in eighteenth-century life science:  
Vitalism and provisionally inexplicable explicative devices**

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Abstract

Newton's impact on Enlightenment natural philosophy has been studied at great length, in its experimental, methodological and ideological ramifications. One aspect that has received fairly little attention is the role Newtonian "analogies" played in the formulation of new conceptual schemes in physiology, medicine, and life science as a whole. So-called 'medical Newtonians' like Pitcairne and Keill have been studied; but they were engaged in a more literal project of directly transposing, or seeking to transpose, Newtonian laws into quantitative models of the body. I am interested here in something different: neither the metaphysical reading of Newton, nor direct empirical transpositions, but rather, a more heuristic, empiricist construction of Newtonian analogies. Figures such as Haller, Barthez, and Blumenbach constructed analogies between the method of celestial mechanics and the method of physiology. In celestial mechanics, they held, an unknown entity such as gravity is posited and used to mathematically link sets of determinate physical phenomena (e.g., the phases of the moon and tides). This process allows one to remain agnostic about the ontological status of the unknown entity, as long as the two linked sets of phenomena are represented adequately. Haller et. al. held that the Newtonian physician and physiologist can similarly posit an unknown called 'life' and use it to link various other phenomena, from digestion to sensation and the functioning of the glands. These phenomena consequently appear as interconnected, goal-oriented processes which do not exist either in an inanimate mechanism or in a corpse. In keeping with the empiricist roots of the analogy, however, no ontological claims are made about the nature of this vital principle, and no attempts are made to directly causally connect such a principle and observable phenomena. The role of the "Newtonian analogy" thus brings together diverse schools of thought, and cuts across a surprising variety of programs, models and practices in natural philosophy.

Eighteenth-century vitalists are not . . . impenitent metaphysicians but rather prudent positivists, which is to say, in that period, Newtonians. Vitalism is first of all the rejection of all metaphysical theories of the essence of life. This why most of the vitalists referred to Newton as the model of a scientist concerned with observation and experiment. . . . (Canguilhem 1977, 113)

In trying to make sense of the multiplicity of Newtonianisms in eighteenth-century natural philosophy, scholarship has been gradually progressing in sophistication and increasing the fine-grained quality of its interpretive categories.<sup>1</sup> For my part, I view the Newtonians as dividing into three large groups, which were always in interplay: *experimental* Newtonians, *metaphysical*, *ideological*, *methodological* Newtonians and *analogical* Newtonians, who combine elements of the first two groups. In what follows I shall focus on the third set, which I believe to be less studied, through a series of cases of ‘analogical Newtonianism’ in eighteenth-century life science. I thereby suggest a revision of Schofield’s “evolutionary taxonomy of eighteenth-century Newtonianisms” (Schofield 1978).

The role Newtonian analogies played in the formulation of new conceptual schemes in Enlightenment physiology and medicine is an aspect that has not received much attention.<sup>2</sup> And some self-proclaimed Newtonians in the life sciences are difficult cases because they also professed anti-mathematicism (see Sections 2 and 4). In contrast, the so-called ‘medical Newtonians’, like Archibald Pitcairne (who was Hermann Boerhaave’s professor in Leiden) and James Keill, have been the object of useful studies already (Brown 1977, 1981, 1987; Guerrini 1985, 1987); but they were engaged in a more *literal* project, seeking to directly transpose Newtonian laws into quantitative models of the body. I shall be interested in something different

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<sup>1</sup> From Schofield’s 1978 “evolutionary taxonomy” to Shank’s “thicker” description of, e.g., Leibnizian Newtonians in the Berlin Academy, such as Maupertuis (Shank 2008). Shank tried – successfully in my view (Wolfe and Gilad 2011) – to order Schofield’s extreme diversity, reminding us that if there were various interests at work in eighteenth-century French Newtonianism, nevertheless there were some unifying features.

<sup>2</sup> With the under-discussed exception of Hall 1968, or studies focusing on individual figures in which the Newtonian dimension is highlighted, such as Roe 1984.

here: less in *direct empirical* transpositions and more in the heuristic, *constructivist yet also empiricist* usage of Newtonian analogies – for the analogical Newtonian share with empiricists such as Locke, and ‘medical empiricists’ such as Sydenham, a suspicion of essences and ontology, favoring instead an ‘observational’ attitude towards phenomena.

By means of a series of variations on Newton’s method of positing an unknown entity (such as gravity) from which a series of mathematical equations are derived – for instance, equations linking together phases of the moon and tides – the figures I shall examine claimed that if the Newtonian physician or physiologist can posit an unknown called ‘life’ and derive from it various other phenomena, from the functioning of the glands to digestion and sensation, these will appear as interconnected, goal-oriented processes which exist neither in an inanimate mechanism nor in a corpse. But significantly, and in keeping with the empiricist roots of the analogy, no ontological claims were made about the nature of this vital principle; no attempts to causally connect such a principle and observable phenomena. This is not to say that the Newtonian analogy, or analogies had a single, clear-cut role in Enlightenment life science. But reflecting on the analogy brings together diverse schools of thought, and cuts across a surprising variety of programs, models and practices in natural philosophy; it is a story worth telling.

Newtonianism in Enlightenment life science at first appears to be a rather straightforward matter. Mainstream figures such as Hermann Boerhaave and Albrecht von Haller were self-proclaimed Newtonians seeking to apply the insights and methods of the great man to the newly emerging field we might call ‘biology’ (the term itself appeared in a usage we would recognize, in France and Germany in the late 1790s, with a few earlier uses in the previous decades, but not with a stable definition until approximately 1798<sup>3</sup>). For Boerhaave, e.g., amongst the “solid parts of the human body,” “some resemble Pillars, Props, . . . some Axes, Wedges, Leavers and Pullies, others Cords, Presses or Bellows,” Pipes, etc. (Boerhaave 1752, 81). The first line of Haller’s highly influential textbook *Elementa physiologiæ* sharply stated an analogy for the study of living beings (in whom the minimal unit of living tissue is the fibre): “the fibre is to physiology what the line is to geometry” (*Fibra enim physiologo id est, quod linea geometræ*).<sup>4</sup> In contrast, ‘heterodox’ figures such as John

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<sup>3</sup> Caron 1988, 231-232, McLaughlin 2002, Wolfe 2011a, 209-210.

<sup>4</sup> Haller 1757, I, 2. In Diderot’s discussion this becomes “the fibre is to physiology what the line is to mathematics” (*Éléments de physiologie*, in Diderot 1975-, XVII, 338).

Toland or Denis Diderot partly reject Newtonianism, either for ideological reasons, because of a different conception of matter, force and their relations, or due to a kind of tacit vitalism, according to which the science of living beings should not model itself on the science of gravitation (Guédon 1979).

Yet Newtonianism in eighteenth-century life science was more complicated: it didn't consist simply in either the literal-minded transposition of some quantitative and/or methodological tools, or the fierce rejection of design and physicotheology that was characteristic of the 'Radical Enlightenment'. As I will suggest, the appeal to a Newtonian analogy served as a stimulus to extremely diverse conceptual constructions in the ontology of Life in this period, constructions that appear quite distant from their source. Chief amongst these is the elaboration of what I will call, following an insight of T.S. Hall's, *provisionally inexplicable explicative devices*. Hall alludes to a comment of Robert Whytt's on how his method resembles Newton's with respect to gravity, and reflects on how we should understand Newton's influence on interpretive models in physiology. "One thing Newtonianism did," Hall suggests, "was to legitimize the adoption and use for interpretive purposes of what we may term 'inexplicable explicative devices', or 'physiological unknowns'."<sup>5</sup> As we shall see, a variety of thinkers - professors of medicine, natural historians, physiologists and naturalistically inclined philosophers - including figures we would term 'vitalists', put these "inexplicable explicative devices" to work, in ways I ranging from the more literal uses of Newtonian explanations to the more analogical uses of what are by that point "explicative devices."

Julian Martin has suggested that this can entirely be traced back to Roger Cotes' preface to the second edition of the *Principia* (Martin 1990, 130). Cotes notes there that "effects of the same kind - that is, whose known properties are the same - have the same causes and their properties which are not yet known are also the same" (Cotes, in Newton 1713/1999, 391). He also insists that gravity is not an occult quality (or cause): "occult causes are not those causes whose existence is clearly demonstrated by observations, but only those whose existence is occult, imagined and not yet proved" (*ibid.*). More boldly (because the former statement is, among other things, a standard statement of experimental philosophy), Cotes also says that "no mechanical explanation can be given" for the simplest cause. Yet for several

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<sup>5</sup> Hall 1969, vol. 2, 73; cf. Hall 1968, 14.

reasons, the real source is Query 31 of the *Opticks*.<sup>6</sup> Most importantly, this is where Newton himself states the idea that Cotes alluded to: “What I call Attraction may be perform’d by impulse, or by some other means unknown to me. I use that Word here to signify only in general any Force by which Bodies tend towards one another, whatsoever be the Cause” (Newton 1730/1952, 376). Newton also adds a very similar claim to Cotes’: that there is a great difference between arguments which rely on causes, the “principles” of which are not yet discovered, and “occult qualities” (401, 402), because such arguments do not claim to establish a link between phenomena and “the specifick Forms of Things” (401) but rather to articulate “general Laws of Nature by which the Things themselves are form’d,” laws we grasp phenomenally (empirically, we might say), even though their causes are not yet known (*ibid.*). In short, and to reiterate a very famous Newtonian *topos*, by postulating unknowns Newton arrives at the law of gravitation.

I examine here the different ways in which this methodological and model-building insight of Newton’s was applied, carried over, reformulated, and otherwise appropriated in eighteenth-century life science, in the formulation of what Hall termed “physiological unknowns,” which are also conceptual appropriations. I distinguish between (§ 1) the literal use of Newtonian methodology in ‘medical Newtonianism’ (Pitcairne, Boerhaave et al.), (§ 2) the non-literal transposition of his method in later physiology and medicine (Buffon, Maupertuis, Hartley et al.), which raises the question of whether or not this is a transposition of *models*, (§ 3) Albrecht von Haller’s so-called “Newtonian physiology,” which, I suggest, is really a physiology of ‘place-holders’ and mostly a weaker usage of the analogy, versus (§ 4) the heuristic, Newtonian-nourished vitalism of the Montpellier physicians, described as “prudent positivists” rather than “impenitent metaphysicians” in the epigraph to this essay (Canguilhem 1977, 113), and who make a *strongly* analogical usage of Newtonian methodology. Lastly (§ 5) I examine some more skeptical approaches to Newtonianism and mathematics, on the part of ‘vital materialists’ such as Mandeville and Diderot, before concluding with some general reflections on the Newtonian analogy in vitalism and its empiricist ramifications.

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<sup>6</sup> Query 31 was added to the Latin edition of the *Opticks* (1706) and was then numbered 23; it became Query 31 in the newer English editions of 1717 and 1718. It is important that these Queries were composed during the final phase of Newton’s life, before the preface to the second edition of the *Principia*; they have a speculative – or at least exploratory – tone which distinguishes them from the rest of his work.

One can speak of an empiricist dimension here, for some of the hostility to mathematics is also an explicit, and recognizable empiricist posture, going back at least as to Bacon, Locke and Sydenham, with a complex pedigree composed alternately of Hippocrates, the medical *empirikoi* of antiquity and early modern empiricists (Wolfe 2010a). Notably, the way in which the Montpellier vitalists move away from an ‘ontologization’ of the vital principle can be considered a kind of empiricism, in keeping with their insistence on the primacy of observation over experiment.

### 1. Literal medical Newtonianism

Iatromechanism in its Scottish form, often referred to as ‘medical Newtonianism’, was a deliberate attempt to directly extend the power of Newtonian quantitative explanations to the medical realm. Such figures as Archibald Pitcairne, James Keill, William Cockburn, Bryan Robinson and George Cheyne lavished “quantitative and mathematical attention” on “the hydraulics and general mechanics of the animal oeconomy” (Brown 1987, 641). They held that medicine should be based on ‘mathematical physick’ (in fact modeled on astronomy), with the goal of finding absolute laws interconnecting empirically established phenomena. Much more restrictively quantitative than anything in, say, Harvey, these physicians sought to measure quantities of blood, force, and velocities; Pitcairne even wanted to weigh the *skin* of a corpse (Cunningham 1981, 93). More ambitiously, extending insights of iatromechanism, Pitcairne sought to construct a *certain* system based on elements which are themselves *certain* – fluids, velocities, dimensions of vessels, etc. In his 1692 Inaugural Lecture at Leyden, entitled “An Oration Proving the Profession of Physic Free from the Tyranny of any Sect of Philosophers,” Pitcairne emphasized the priority of mathematics over philosophy for physicians (Pitcairne 1715, 8); “Enquiries after physical causes as are generally proposed by the philosophers are entirely useless and unnecessary to physicians” (*ibid.*, 10). He later declared that “All Diseases of the Fluids consist either in a Change of their Qualities, or a Change of the Velocities of their Motions”; hence “The cure of every Disease, whether in the Vessels or Fluids, or both, is to be effected only by mechanical Laws.”<sup>7</sup>

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<sup>7</sup> *Elementa Medicinae* (1717), translated as *The Philosophical and Mathematical Elements of Physick* (1718), § LXXVIII, in Pitcairne 1718, 353; § LXXXVII, in Pitcairne 1718, 354. See also Brown 1987, 641.

Pitcairne criticized those who relied on fermentation for understanding vital processes and instead emphasized the expansion and contraction of muscles – a point on which he could rely on the authority of Newton himself, who in the *Opticks* explained fermentation and putrefaction (and thus life) by the *attraction of corpuscles* rather than by any irreducibly chemical process.<sup>8</sup> There is a kind of mechanistic reduction of chemical entities here (different, however, from a Cartesian-mechanical reduction), as can also be seen in works such as the 1702 *Mechanical Account of Poisons* by Pitcairne’s disciple Richard Mead: even poisons should be explainable in mechanistic terms (Mead refuted Boyle’s claim that the bite of a viper was more or less fatal depending on the *rage* of the viper). Similarly, George Cheyne also attacked chymistry in the name of a confident mechanism, writing in 1702: “All is nonsense, unless they first shew their systems and chymical effects to be necessary corollaries from the known laws of motion, i.e. unless their philosophy, and chymistry too, be first mechanically explain’d” (Cheyne 1702, 11, cit. in Gaukroger 2010, 334).<sup>9</sup>

However, in a typical confirmation that we are not dealing with neat, entirely separate categories here, but rather with interpretive distinctions, James Keill used the concept of attraction, not to reject the relevance of iatrochemical explanations, but rather to justify them: medicine works by “uniting and augmenting the attractive force of the particles which compose the humours,” given that the particles of some humours unite to the “particles of some medicines” more easily than others; and certain humours “require different purgative medicine” to carry them through the glands (Keill 1708, 65-66) ... an even balance between chemical language and that of Newtonian attraction. Overall, “the whole animal economy depends on attractive power” (*ibid.*, 8).

But more significantly for my comparative approach, we need to see just how literal the ‘medical Newtonians’ are in their way of being ... Newtonian. Consider Proposition XII of Bryan Robinson’s 1732 *Treatise on the Animal Oeconomy*: “The Velocities of the Blood in the corresponding Blood-Vessels of Bodies Situated alike

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<sup>8</sup> Of course, ‘Newton’ as referred to here and by the figures under discussion is the publicly visible Newton, not the author of alchemical notebooks in which a “fermental virtue” is posited which “accommodates itself to every nature [and] from metallic semen . . . generates gold, from human [semen] men etc. . . .” (a proposition dated approx. 1669, Keynes Ms. 12A, cit. in Iliffe 1995, 445).

<sup>9</sup> However, as Anita Guerrini has described, in later essays collected in the 1740 *Essay on Regimen*, Cheyne moved from a vision of the body as “nothing but . . . a Contexture of Pipes, an *Hydraulic Machin*” to an account in which circulation cannot be fully explained by “mere Mechanism, or the Laws of Motion which now obtain,” but instead requires a “primary *self-existent* cause” (Cheyne 1740, xiii, 2-3; Guerrini 1985, 263).

with respect to the Horizon, are in the subduplicate Ratios of the Diameters of the Vessels.”<sup>10</sup> Or proposition XIV: “If an animal Fibre, by a Force acting on it, be increased or lessened, either in Length or Thickness; its Length will be reciprocally proportional to its Diameter . . .” (Robinson 1732, 103). It may not be surprising that Alexander Monro *primus* described Robinson as “a blind follower of Sir Isaac Newton assuming with most of his countrymen Sir Isaac’s queries as axioms...,” what Brown described as “perpetrating mathematical excesses on the animal economy.”<sup>11</sup>

This kind of obsessively quantitative ‘mathematical physick’ was seen as particularly useful in the dosing of medicines. So, William Cockburn, in his 1694 *Oeconomia Corporis Animalis*, argued by “apodictic arguments” that if we assume two “postulates” – medicines can act only when they are mixed with the mass of the blood, and the general effect of a person’s peculiar “temperament” is only some alteration in the hydraulics of the circulating fluids – then we can conclude for bloods of equivalent thickness that

The doses of medicaments necessary to elicit a certain effect are proportional to the quantity of the blood; for if a particular dose were required to alter the thickness of, say, one pound of blood to a particular degree, then twice the dose would be necessary in order to alter two pounds to the same degree, . . . [G]enerally, if the quantity of blood  $b$  requires dose  $d$ , then the quantity of blood  $mb$  requires the dose  $md$ .<sup>12</sup>

Some self-proclaimed Newtonian natural philosophers with medical or physiological interests also turned the direction of proof back towards Newton’s theory itself, and claimed that their researches, e.g. in the hydraulics of circulation (such as Stephen Hales’ *Haemastaticks*, the subtitle of which describes it as *An Account of some Hydraulick and Hydrostatical Experiments made on the Blood and Blood-*

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<sup>10</sup> Robinson 1732, 113-114; Brown 1981, 351. Schofield, in his celebrated study of Newtonianism in English scientific and philosophical thought, thus completely misses the significance of the Newtonian analogy in the life sciences, when he writes that Robinson’s much respected work “leavens an otherwise commonplace mechanistic physiology with a section on the aetherial cause of muscular motion” (Schofield 1970, 108-109). Robinson’s hydraulics of the body may need “leavening” as writing, but it is anything but “commonplace mechanistic physiology,” with its mechanization of Newtonian forces!

<sup>11</sup> Monro *primus*, “The History of Anatomy,” MS 166, Medical Library, University of Otago, cit. in Guerrini 2006, 9; Brown 1981, 311 (referring to Cheyne and Mead).

<sup>12</sup> Cockburn and Southwell 1704, 2119-2220; Brown 1987, 633-634. Keill has a comparable, but less restrictive discussion of the ‘mathematics’ of the relations between humours and medicines (Keill 1708, 63-65). Mandeville, as I discuss below, strongly rejected this kind of argument, on empirical rather than a priori or otherwise ontological grounds, unlike Diderot, although the border separating these two attitudes is not clear-cut.



*Vessels of Animals*) could provide experimental proof of gravitational attraction, by using Newtonian theory “to explain the apparent rise of fluids inside capillary tubes” (Shank 2008, 404). Hales crosses several aspects of our story, if one considers the French translations of his two major works, the 1727 *Vegetable Staticks* and the 1733 *Haemastaticks*, by Buffon and the Montpellier professor of medicine François Boissier de Sauvages (1706-1767), respectively. Buffon’s translation was a major moment of the implantation of Newtonianism in France (after Maupertuis’ *Discours sur la figure des astres*), as Voltaire loudly proclaimed, referring to the “penetration of Newtonian truths inside the Paris academy despite the taste for Cartesianism that still dominates there.”<sup>13</sup> Sauvages’s case, which I discuss in section 4, shows how medical vitalism made use of explicitly Newtonian concepts. But what we should retain for now is (a) the literal character of this medical-Newtonian project, (b) its degree of ontological commitment, that is, the extent to which it is neither a heuristic model nor an epistemological claim about how we come to know the body. Instead, descriptions of the body as *just a “pure machine”* abound, as in James Keill:

The Animal Body is now known to be a pure Machine, and many of its Actions and Motions are demonstrated to be the necessary consequences of its Structure. The manner of Vision is shown in Opticks. BORELLI has given us the Mechanism of the Bones and Muscles for the moving of the joints. And since the Discovery of the Circulation of the Blood by the famous Dr. HARVEY, many useful Propositions concerning its Motion and Velocity have been determined by BELLINI. Dr. PITCAIRNE has explained the mechanical structure of the Lungs . . . many Phaenomena of the Animal Body which the Ages past thought inexplicable, have now by several [persons] been made the Subjects of Geometrical Demonstration.<sup>14</sup>

In addition to this literal transposition of Newtonian quantification, it is important to note that the iatromechanist project runs counter to a basic ‘empiricist’ inclination to be skeptical or at least agnostic about the nature of life itself: Pitcairne asserted unambiguously that “Life consists in the Circulation of the Blood produced by the Motion of the Heart and Arteries.”<sup>15</sup> Thinkers as different as Locke and Sydenham on the one hand, and the Montpellier vitalists on the other hand, shared a

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<sup>13</sup> Voltaire, *Réponse aux objections principales qu’on a faites en France contre la philosophie de Newton* (1739), in *Ceuvres complètes* (Paris, 1829-1840), XXIII, 71-72; cit. Shank 2008, 408.

<sup>14</sup> Keill 1708, iii-iv; Keill 1717, iii-iv (this is a revised version of Keill 1708, with new sections added: Guerrini 1985, Brown 1987). More generally, “the Animal Body is nothing but a Machine, whose Actions and Motions are all performed by Fluids” (Keill 1708, 66; Keill 1717, 182).

<sup>15</sup> Pitcairne, “Oratio qua ostenditur medicinam ab omni philosophorum secto esse liberam,” translated in Pitcairne 1715, 9-10, 16, 24; cf. *ibid.*, 95 and Pitcairne 1718, ch. VI, § 1, at 71. This is discussed in Guerrini 1987, 79-80 and Schaffer 1989, 177.

'Hippocratic' denial of or at least suspicion of this kind of definition and quantitative modeling of Life, along with a deep-rooted commitment to privileging observation over experiment (Wolfe 2010a); chemical-materialists like Mandeville and Diderot share this denial, which they motivate either on empiricist and skeptical grounds (Mandeville) or more ontologically terms (Diderot), but are not hostile to experiment.

A more complex but still literal form of literal Newtonianism is the sophisticated medical mechanism of Herman Boerhaave (1668-1738), Professor of Medicine, Botany, and Chemistry at Leiden. Boerhaave was widely viewed as the most influential lecturer in medicine in Europe, and taught figures including La Mettrie, and Haller. He frequently was described (and described himself) as a Newtonian, expanding the reach of mechanism beyond its Cartesian strictures, and incorporating chemical explanations of the microstructure of the body. Prior to 's Gravesande's appointment to the Chair of Physics in 1717, Boerhaave was the sole adherent of Newtonian physics in Leiden.<sup>16</sup> He wrote and lectured extensively on the value of mathematical and mechanical explanations in medicine, often metaphorically describing the parts of the body as pipes or vessels, "Pillars, Props, ..., some like Axes, Wedges, Leavers and Pullies, others like Cords, Presses or Bellows," adding that the functions of the body are "all performed by mechanical Laws."<sup>17</sup>

For Boerhaave the fibre was the basic unit of the solid parts of the human body, a unit defined in explicitly physico-mathematical terms: a fibre is a part of the human body considered as extended in length, but as having no parts in breadth. He thus compared it to a mathematical line, defined as length without breadth.<sup>18</sup> Further, his account of the nervous system has nerves "performing every action by vibration" (Boerhaave 1715, 109), depending on various states of tension in the fibres.<sup>19</sup> Another 'medical Newtonian', Henry Pemberton (a physician whose work

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<sup>16</sup> A.M. Luyendijk-Elshout, in her introduction to Boerhaave's "Discourse on the achievement of certainty in physics," in Boerhaave 1983, 145.

<sup>17</sup> Boerhaave [1708] 1751, 81, and see his "Discourse on the achievement of certainty in physics" (*Sermo academicus de comparando certo in physicis*), § VIII, Boerhaave 1983, 158.

<sup>18</sup> Van Swieten 1759, I, 38, cit. in Lindeboom 1970, 206. Van Swieten, a close student of Boerhaave's, edited Boerhaave's aphorisms with a commentary, a work Peter Hans Reill calls "the eighteenth century's bible of medical mechanism" (Reill 2005, 121).

<sup>19</sup> Bastholm 1950, 197. Sauvages reprised this theme of vibrations, and tied it to an (equally Newtonian-Boerhaavian) scheme of the body as a hydraulic machine. However, Sauvages added a Stahlian component, the soul as central explanatory principle in organic processes of self-maintenance (*Dissertation sur les médicaments*, in Sauvages 1770, II, 26-27; Sauvages 1763/1771, I

attracted the interest of Newton in the 1720s, to the extent that Newton entrusted him with editing the third edition of the *Principia*) tried to map out what sort of curve occurs in the fibres of muscular vesicles, in the Introduction on Muscular Motion he wrote for the new edition of William Cowper's *Myotomia Reformata* in 1723 (Brown 1981, 331-333). Updating the work of Keill, Borelli and others, and appealing to Newton's calculus of variations, Pemberton suggests contra Bernoulli that muscular vesicles do not become circular during distention, but rather take on the "Figure of the largest Cavity . . ." and "to determine this figure we must have recourse to the Problem concerning isoperimetrical Curves . . ." <sup>20</sup>

I now turn to the cases of three prominent figures in the mid-eighteenth century, Buffon, Maupertuis and Hartley, who no longer seek to literally extend the scope of Newtonian explanations, but instead integrate into them properties that were not strictly present in the original, thereby decreasing the degree of recognizably Newtonian laws and principles in use.

## 2. Non-literal transpositions of Newtonian method: Buffon, Maupertuis and Hartley

Voltaire notoriously described the great natural historian Buffon as the "head of the Newtonian party in France."<sup>21</sup> As Thierry Hoquet has reminded us, Buffon started out with no competence in natural history, but with a very good reputation in academic circles for his mathematical work, especially on the "jeu de franc-carreau" or "needle problem," on the strength of which he was admitted quite early on to the Paris Academy of Sciences, at the age of 26 (see Buffon 1733 and the discussion in Hoquet, forthcoming). Buffon translated Newton's *Method of Fluxions* in 1740, and entered into a polemic with Clairaut in which he defended the hypothesis of the unity of the law of attraction<sup>22</sup>; much later on, in the *Supplément* to his *Histoire naturelle*, he criticized the (Stahlian) chemistry of affinities for failing to

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("Prolégomènes"), 4, 10, 45). However, Sauvages more generally defended the pertinence of mechanical and mathematical explanations in medicine (referring notably to Newton), as I discuss below.

<sup>20</sup> Pemberton in Cowper 1724, xxxv; Brown 1981, 334.

<sup>21</sup> Literally, "je suis l'enfant perdu d'un parti dont M. de Buffon est le chef" (Voltaire, Letter XII to Helvétius, October 1739, in Helvétius 1818, 209; Besterman D2086); thanks to J.B. Shank for help finding this citation.

<sup>22</sup> Buffon, "Réflexions sur la loi de l'attraction," in Buffon 1749-1788, XXX (*Suppléments* I), 126f. (originally in *Mémoires de l'Académie royale des Sciences*, 1745; a response to Clairaut's paper "Du système du monde dans les principes de la gravitation universelle").

understand “the basic causal relation subtending [affinity], which is universal attraction.”<sup>23</sup> Buffon also spoke in familiar accents against hypotheses in the preface to his 1739 translation of Stephen Hale’s *Vegetable Statics*.

In a short slogan, one could say that Newton was useful to Buffon in conceptualizing an *anti-Cartesian* science of life. In the *Histoire naturelle*, he noted that his ideas would not convince those who (dogmatically, as it were) “only accept a certain number of mechanical principles,”<sup>24</sup> and in a familiar turn of phrase, added that he “supposes causes” (*ibid.*). Buffon was not a literal Newtonian who thought attraction could be extended to other phenomena, or that attraction was the general law of Nature (as seems to be the case in his polemic with Clairault). Instead, attraction for him was a means to widen the scope of science, away from strict mechanism. As Hoquet puts it, attraction “is useful less in what it poses than in what it opposes; it enables Buffon to make room for other causes, other general effects, applying to other law-bound sets of phenomena” (Hoquet 2005, 171). But in a number of relevant ways, the project (and contents) of Buffon’s *Histoire naturelle* are non-Newtonian – which does not mean they are *anti-Newtonian*.<sup>25</sup> As I discuss below (section 5), this takes the form of a hostility towards abstraction and mathematics, and a concomitant defense of the autonomy of the life sciences.

Maupertuis is a more complex case, for he was both a Newtonian – seeking to extend the explanatory scope and applicability of the force of attraction – and also considered that Newtonian attraction did not sufficiently account for organic phenomena such as the processes of generation, or even “the simplest chemical operations.”<sup>26</sup> In his *Système de la nature ou Essai sur les corps organisés*<sup>27</sup> he reflected on this at length, accepting that the same force of gravitation-attraction which controls the behavior of bodies in space governs the formation of organic bodies, but also reinterpreting the force of attraction as *affinity*. Thereby this force shifts from

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<sup>23</sup> Buffon, “Des Elemens,” in Buffon 1749-1788, XXX (*Suppléments* I), 75. In the “Seconde vue” of *De la nature*, Buffon corrects Newton for being inconsistent, and for not seeing that chemical affinities do not obey laws like that of gravitation; rather, they obey *the law* of gravitation (1749-1788, XIII, xiv).

<sup>24</sup> Buffon, *Histoire générale des Animaux* III, *De la nutrition et du développement*, in Buffon 1749-1788, II, 50.

<sup>25</sup> Hoquet 2005, 32-33 and *passim*, for excellent analysis of Buffon as Newtonian, or not; Hoquet is gently skeptical about the extent of Buffon’s Newtonianism.

<sup>26</sup> Maupertuis 1756/1965, § III, 141.

<sup>27</sup> This text first appeared in Latin in 1751 as *Dissertatio inauguralis metaphysica de universali naturae systemate*, under the pseudonym Dr Baumann; it was translated by Maupertuis in 1754 as *Essai sur la formation des corps organisés* and was later included in his 1756 *Ceuvres* under the title *Système de la nature*. I analyse this text in Wolfe 2010b.

being a strictly mechanical process understood in Newtonian fashion, and gains 'Leibnizian' qualities. His vision of matter sounds more monadic at this point, given that each minimal component of living matter (which he calls "molecules") possesses higher-level intellectual features:

A uniform, blind attraction spread over the parts of matter would not explain how these parts are arranged to form the most rudimentary organized bodies. If they all possess the same tendency and the same force to join them together, why then do some form the eye and others the ear? Why is there this wonderful arrangement? Why do they not join haphazardly? If an explanation is to be attempted, even if founded entirely on analogy, some principle of intelligence must be applied, something similar to what we call *desire, aversion, memory*.<sup>28</sup>

Thus, for Maupertuis, in order to explain the formation of organized bodies, intellectual (or 'psychic') properties must be added to the physical properties of matter. By endowing these "living particles" with a kind of memory, the regularity of the unfolding of organic processes and even the existence of trait heredity (precisely a kind of Newtonian unknown), can be explained.

David Hartley has been described as having a "Newtonian neuropsychology" in his 1749 *Observations on Man, His Frame, His Duty and his Expectations* (Smith 1987). This is an attempt at a 'vibratory' materialist account of mind, according to which small vibrations ("vibrunticles") are impressed in the solid filaments of the nerves by external objects; these sensations are transmitted by ætherial vibration to the infinitesimal particles that make up the substance of the brain. Indeed, Newton himself had suggested a 'vibratory basis of sensation' in some *Queries of the Opticks*,<sup>29</sup> and Hartley explained that he came to his idea of vibrations from reading Newton's *Principia* (the General Scholium), the æther queries, and Newton's Letter to Boyle.<sup>30</sup> Hartley also acknowledged some limitations in seeking to fill in Newton's blanks:

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<sup>28</sup> Maupertuis 1756/1965, § XIV, 146-147 (emphasis in original). Cf. Wolfe 2010b, Downing 2012 and Schofield 1978.

<sup>29</sup> Newton 1730/1952, *Queries* 14, 23, 31.

<sup>30</sup> Hartley 1749, 13-14; Guerlac 1977, 162; Schofield fails to correctly assess the import of the Newtonian analogy here: he thinks that Hartley's variant on æther theory "obtained an anomalous longevity" from its connection to the extension of Lockean philosophy known as associationist psychology (Schofield 1970, 198). But Schofield is missing a crucial dimension of Hartley's theory: its merit lies less in its 'truth' (i.e. claims about vibrations) than in the idea that complex intellectual operations can be resolved into simple ones by the laws of association, i.e., its contribution to methodology.

It seemed credible to Newton that a very subtle and elastic fluid, and hence very suitable for reception and communication of vibrations both lies hid in gross bodies and is diffused through the open spaces that are void of gross matter. . . I remain somewhat doubtful that I have sufficiently understood his views (Hartley 1746/1959, 4).

Hartley described these vibrations as

motions backwards and forwards of the small particles; of the same kind with the oscillations of pendulums, and the tremblings of the particles of sounding bodies. They must be conceived to be exceedingly short and small, so as not to have the least efficacy to disturb or move the whole bodies of the nerves of brain. For that the nerves themselves should vibrate like musical strings is highly absurd (Hartley 1749, I, 11-12).

(What vibrates are the infinitesimal medullary particles.) By their differences in degree, kind and place, these vibrations represent different primary sensations, or “simple ideas” in the brain. These become increasingly disposed to vibrate in any particular mode by each repetition of the sensation. Other vibrations, especially if they arrive at the brain simultaneously, may also induce this mode of vibration and become associated with it. The two (or more) vibrations modify one another, causing recollection of sensation, and by extension creating chains of induced vibrations called ideas, or more complex concepts.<sup>31</sup>

Hartley followed Newton extensively, in his account of sensation and the nervous system, but also in his methodology:

The proper method of philosophizing seems to be, to discover and establish the general laws of action, affecting the subject under consideration, from certain select, well-defined, and well attested phaenomena, and then to explain and predict other phaenomena by these laws. This is the method of analysis and synthesis recommended and followed by Sir Isaac Newton (Hartley 1749, I, 6).

In addition – and unlike any other of the thinkers discussed here – Hartley also expressed the familiar Newtonian denial that the vibratory theory of mind could imply or entail materialism:

It may be proper to remark here, that I do not, by thus ascribing the performance of sensation to vibrations excited in the medullary substance, in the least presume to assert, or intimate, that Matter can be endowed with the power of sensation (Hartley 1749, I, 33).

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<sup>31</sup> Hartley 1749, I, 13-16. See further Yolton 1983, 180-184; Smith 1987, 124.

Buffon, Maupertuis and Hartley extend the usage of Newtonian explanations or theoretical constructions far beyond what we saw in section 1, but in none of these cases is the Newtonian 'unknown' put to major use in theory construction: while these figures are not engaged in literal transposition, they are also not engaged in methodological innovation.

### 3. Haller: a physiology of 'place-holders'

A much more complex case of a medical Newtonian, or rather a *physiological* Newtonian, is that of the great physiologist and avid experimenter Albrecht von Haller (1709-1777). First, Haller was a Newtonian in a broad sense, stressing experimentalism, the search for laws rather than essences, and the rejection of hypotheses (even if the "real Newton"'s views on hypotheses were more complicated). But second, Haller was a Newtonian in a more sophisticated sense: on the *model* of Newton's presenting an explanation on a level of more complex structures than those of the atoms of attraction due to the inaccessibility of the cause of motion, Haller postulated *irritability* as a complex property which is not subject to the common laws of motion (Steinke 2005, 115), and also reflected on the status of hypotheses in a less dismissive way (*ibid.*, 75-77). (In a further move which will be central to my analysis here, vitalists will use this analogy to postulate 'Life'.) In his 1812 *History of the Royal Society*, Thomas Thomson observed that as the mathematico-mechanical model of how to do medicine began to lose its revolutionary fervor and promise of success in the eighteenth century, physiologists took hold of either of two "systems" with which to justify vital phenomena, "the more ancient [system] explaining every thing by the action of a *living principle*, and the more modern by a principle somewhat indefinite, to which they gave the name of *irritability*"<sup>32</sup>; the same insight had been stated – sometimes respectfully towards Haller, sometimes polemically as in Bordeu – by most of the Montpellier vitalists to whom I turn in the next section.

Irritability was a quantifiable, experimentally accessible property of the muscle fibres, to be studied mechanistically, through a correlation between a measurable degree of irritation and a degree of irritation of the fibres: between structure and function. There is no metaphysics of living matter here, at least superficially – and Haller goes out of his way to oppose his experimentally

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<sup>32</sup> Thomson 1812, 120; cit. Brown 1974, 183, n. 16.

grounded concept of irritability to his predecessor Francis Glisson's concept, which he portrayed (successfully in light of subsequent history of science) as an entirely speculative, metaphysical construct of matter as inherently appetitive (Gigliani 2008). For on the one hand, indeed, Haller wanted to define irritability so as to rule out 'speculative hidden qualities' (Steinke 2005, 106). But on the other hand, when pushed as to the reason why certain types of organic matter possess such properties, Haller first attributes it to the 'gluten' within the fibre ('Irritabilité est en vérité une force particulière à la glu animale'<sup>33</sup>, although he wavers on this), and then, coming dangerously close to just as metaphysical a vitalism as Glisson, attributes this 'vitality' to a hidden force, the *vis insita*:

The heart and intestines, also the organs of generation, are governed by a *vis insita*, and by stimuli. These powers do not arise from the will; nor are they lessened or excited, or suppressed, or changed by the same. No custom, no art can make these organs subject to the will, which owe their motions to a *vis insita*; nor can it be brought about, that they should obey the commands of the soul, like attendants on voluntary motion.<sup>34</sup>

Of course, for Haller forces were essentially linked to matter (as *vis insita*), while for Newton (at least the textbook picture of Newton) they were not so. Nerves don't move muscles according to a mechanical force, but convey or transmit (*fait parvenir*) to muscles, the force that makes them contract, "whether this force is a fluid, or whether we have as of yet no idea of what it is" (Haller 1756, ch. IV, § IX, 238-239). More generally, "Gravitation, attraction, elasticity, effervescence, and irritability are so many sources of movement, in which the soul has no part, and which produce their effects without one needing to invoke a thinking being as their author" (Haller 1772, 250). Unlike more analogical Newtonians, Haller didn't use the conceptual flexibility Newtonianism offers to postulate unknowns (or *almost* didn't do so, as I discuss below); instead, he promoted the geometrical method, although he admitted that it produced limited results in earlier generations of the life sciences:

I shall not insist on the usefulness of mathematics in the *animal economy*. It is evident in the functions of the eye, but is not with regard to the movements of the vital organs. Up until now, the calculators have arrived at such opposed results that they have put off modern physiologists from any use of geometry (Haller 1777b, 105a).

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<sup>33</sup> Haller to Bonnet, March 15 1755, in Sonntag ed. 1983, 63.

<sup>34</sup> Haller 1779, § CCCCIX, 198-199; 1786 edition, I, 237-238; the original Latin is in Haller 1747/1765, ch. IX, 'Motus muscularis', § CCCCIX, 184.



He added that it would, however, be the “perfection of science,” if the movements of the animal body and their “mechanical causes *could be subjected to calculation*” (*ibid.*, emphasis mine). But we have “not yet reached this point, which I so strongly desire.”

Haller did not literally transpose Newtonian explanations into physiology, like Pitcairne and others, discussed above. This has led some interpreters to doubt whether it really is appropriate to describe Haller as having a “Newtonian physiology,” as Roe claimed (Roe 1984).<sup>35</sup> But in my sense Haller is working with, or within a Newtonian conceptual space which is weakly analogical; whether or not irritability is *really like* attraction, or whether Hallerian forces are more ontologically material than Newtonian forces, is not the decisive point here. My concern is not to determine who was in fact a ‘real Newtonian’ but rather, to stress the role Newtonian concepts (or analogies) played in these theoretical articulations of Life (living systems, organisms, biological entities, etc.); “not what Newton said but what he enabled people to say” (Schlanger 1971/1995, 100). Most tellingly for my analysis, in a late entry written for the *Encyclopédie d’Yverdon* (the later, Swiss version of Diderot and d’Alembert’s *Encyclopédie*) entitled “Faculté vitale,” Haller used the ‘judge a cause by its effects’ principle to assert that we can posit vital properties as unknowns (he says “as *x*”), until the day comes when we can fill in their mechanisms and “erase” the “*x*.”<sup>36</sup> That Haller begins to resemble a vitalist is one outcome of this analysis, but we shall not explore it further here. What it would mean to be a vitalist in this specifically Newtonian context is the topic of the next section.

#### 4. Eighteenth-century vitalism as “prudent positivism” rather than “impenitent metaphysics”

‘Vitalism’ here refers specifically to the ‘Montpellier vitalists’, that is, the group of physicians and professors of medicine (and anatomy, botany, etc.) at the

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<sup>35</sup> For criticisms of this view Duchesneau 1982, 154 and Steinke 2005, 115f. Hall has a different way of denying that Haller is a Newtonian: he thinks that the biological ‘analogs’ of attraction are not Haller’s quantifiable concepts such as the irritability of muscular fibres, but rather, more archaic concepts such as sympathies and vital flame, as in Fernel and Paracelsus. It should be clear that my approach is different; regardless, Schofield is also mistaken to deny that there is any Newtonianism in Haller! (Schofield 1978, 184).

<sup>36</sup> Haller 1772, 244b; cf. Haller 1752, VI, § 150, XII, § 395.

Faculty of Medicine at Montpellier, beginning in the mid-eighteenth century. The term 'vitalist' was applied to this group from approximately 1800, and indeed served as a self-description during those decades, although some, like Paul-Joseph Barthez, declared that he did not want to be considered as "le Chef de la Secte des Vitalistes" (Barthez 1806, 98, n. 18). Given their shared insistence on sensibility as the sole, defining property of living beings (against Haller's basic distinction between irritability and sensibility), the vitalists could just as easily have been called 'sensibilists'; although Henri Fouquet, when reflecting on their movement in an 1803 work, simply stated that the terms amount to the same thing, since "whatever is sensitive is vital" (Fouquet 1803, 78). Sensibility was thus presented as the primary and general property of living beings (tantamount to life, as Fouquet says), so that the distinction between irritability and sensibility was jettisoned, contra Haller.

Montpellier vitalism did not rely on an idea of vital force or substance as something distinct from the physical, causal world; its concepts of 'animal economy' and *organisation* were distinct from classical mechanistic concepts without being thereby anti-mechanistic (or 'organismic' like the concepts of Leibniz or Stahl). I have referred to this conceptual status elsewhere as 'expanded mechanism' and its explanations as 'structural-functional' (Wolfe 2011b; for the latter term see also Duchesneau 1982). In fact, this very distinctive feature of Montpellier vitalism, which contrasts with animism and *Naturphilosophie* but also with later forms of vitalism, relies strongly on the Newtonian analogy, as a means for dismissing metaphysics and pleading for 'safe science'. Consider Canguilhem's bold statement which serves as my epigraph:

Eighteenth-century vitalists are not . . . impenitent metaphysicians but rather prudent positivists, which is to say, in that period, Newtonians. Vitalism is first of all the rejection of all metaphysical theories of the essence of life. This why most of the vitalists referred to Newton as the model of a scientist concerned with observation and experiment. . . . (Canguilhem 1977, 113)

Canguilhem's assertion extends beyond the historical record, but it was indeed the case that various eclectic, hybrid and innovative figures in Enlightenment 'biomedicine' explicitly made use of different versions of the 'Newtonian analogy'.<sup>37</sup> Haller is only one of the more prominent examples: "The measure of forces consists

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<sup>37</sup> Gaissinovitch 1968, 105-106; Brown 1974 and 1981; Roe 1984, 288-290. For dissenting views, cf. Hall 1968, esp. 13-20 (the use of analogy goes back to ancient medicine and is thus not specifically Newtonian) and Duchesneau 1982, 154 (the physiological invocation of an 'unknown x' is not an explanation which reduces down to the level of atoms of attraction, and hence is not really Newtonian). For a reassessment of Haller as Newtonian, see Steinke 2005, ch. 3.

in their effects,” and “One will never know the mechanical source from which the movements that follow irritation arise, but one will approach this ... in measuring the effect.”<sup>38</sup>

The principle that ‘the first cause shall be posited and then studied through its effects’ is repeated in various versions throughout this period, from Haller to Caspar Friedrich Wolff’s “it is enough that we know that it is, and that we know it according to its effects,” Bichat’s praise of Newton for being the one who brought together “the simplicity of causes and the multiplicity of effects,” or Bonnet’s comment that he will “grant” irritability like the Newtonian grants attraction: “as a certain fact, the cause of which I ignore, without reasoning any less well on its consequences.”<sup>39</sup> Buffon’s version is more complicated: if we know the “general cause,” phenomena could easily be “deduced from it” if “*the action of the forces producing them were not so complex*” (although in fact, the “system of the world” is too chaotic for that: “*quel cahos on a eu à débrouiller*”).<sup>40</sup> If anything, being an *anti-Newtonian* (like Johann Hatzfeld or John Toland) – whether out of ideological motivations, and/or because of a particular focus on vital matter, or at least a chemically laden concept of matter – was rare. The point is, Newtonian science was ‘safe science’. In Query 31 of the *Opticks*, Newton had suggested that, just as the ordinary course of Nature is controlled by the attractions of gravitation, magnetism and electricity, there may be other kinds of attractions in other areas:

As in mathematics, so in natural philosophy, the investigation of difficult things by the method of analysis, ought ever to precede the method of composition. This analysis consists in making experiments and observations, and in drawing general conclusions from them by induction, and admitting of no objections against the conclusions, but such as are taken from experiments, or other certain truths . . . . By this way of analysis, we may proceed from compounds to ingredients, and from motions to the forces producing them: and in general, from effects to their causes, and from particular causes to more general ones, till the argument end in the most general. This is the method of analysis: and the synthesis consists in assuming the causes discover’d, and establish’d as principles, and by them explaining the phaenomena proceeding from them, and proving the explanations.<sup>41</sup>

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<sup>38</sup> Haller 1757, I, 426; Haller 1777b, 105, cit. and trans. in Roe 1984, 282.

<sup>39</sup> Wolff 1764 /1966, 160; Bichat 1801, “Considérations générales,” xxxii-xxxiii; Bonnet, *Tableau des Considérations sur les corps organisés*, in Bonnet 1783, VII, 56.

<sup>40</sup> *Preuves de la théorie de la terre*, article I, in Buffon 1749, I, 130.

<sup>41</sup> Newton 1952, 401-402. On the significance of Newton’s Query for Boerhaave and subsequent physician-philosophers, see Duchesneau 1997, 302-304.

Canguilhem suggests that eighteenth-century vitalists were not “impenitent metaphysicians but rather prudent positivists, which is to say, in the language of the period, Newtonians.” An *imprudent* metaphysician would posit the existence of a substance or force; a prudent vitalist (leaving aside the term ‘positivist’) would, following Query 31, “not seek for other causes of phenomena besides experimental causes, i.e., causes which determine the order of succession of these phenomena by the results of experiment,” in Barthez’s words (Barthez 1858, “Discours préliminaire,” 10n.) – we could also call this empiricism.

With the goal of ‘modeling’ vital properties of which we do not know the ultimate structural cause, various Newtonian biologies and biomedicines emerged, recurrently claiming like Barthez that “we cannot know the essence of causes, but only the regular lawlike relations among phenomena; phenomena governed by the same laws can be considered effects of the same cause” (Lesch 1984, 25), a clear echo of Newton’s rule II in the *Principia*. Barthez also used the language of analogy. He acknowledged that the credibility of facts is undoubtedly “proportional to the intellectual ability and truthfulness of the observer,” but stressed that a fact is only truly credible if its articulation “displays an intimate relation to many other facts that are already known, but have been imperfectly observed,” and ultimately “falls under the heading of certain *essential analogies* with other, numerous facts that themselves have not been challenged by learned men” (Barthez, *op. cit.*, 33). Most overtly, Barthez said he would analyze the ‘something’ that differentiates living bodies from dead bodies *like one analyzes the “unknown quantities” of the geometricians:*

That element found in living beings which is not found in the dead, we shall call Soul, Archaeus, Vital Principle, X, Y, Z, like the unknown quantities of the geometricians. We only need to determine the value of this unknown, the assumption of which facilitates and shortens the calculation of phenomena.<sup>42</sup>

From a different corner of Europe, at the avant-garde of Newtonian biomedicine, Haller had stated the point in perhaps the most definitive way, in the “Faculté vitale” entry discussed above:

Every time we see effects, the mechanical cause of which is unknown to us, we can refer to this cause as a *faculty*, like we refer to an unknown quantity as *x*. If luminous experiments or perfected anatomy [enabled us to] discover the mechanism which produces this effect, we would then erase the *place-holder* name [*nom d’attente*, emphasis in original], as one erases the character marking an unknown quantity.<sup>43</sup>

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<sup>42</sup> Barthez 1806, vol. 1, 16; Barthez 1858, vol. 1, 18n.

<sup>43</sup> Haller 1772, 244b; cf. Haller 1752, VI, § 150, XII, § 395. Barthez’s comment about the ‘unknown

The idea is that the relevant biological property – in Haller’s case, irritability – will be epistemically treated in the way that Newton treated gravity, as “a (provisionally) inexplicable explicative device” (Hall 1968, 14). Hartley treated his own object, aether, in precisely these terms: “Let us suppose the existence of the aether, with these its properties, to be destitute of all direct evidence, still, if it serves to explain a great variety of phenomena, it will have an indirect evidence in its favor by this means.”<sup>44</sup>

The Montpellier physician François Boissier de Sauvages asserted a characteristically vitalist identification between the “laws of organic sympathies,” the existence of which he believed to be confirmed, and a Newtonian-type unknown: “the detail of these laws is only known to us through abstraction” (Sauvages 1771, § CCVXXXVII, 276). Ménéret described nosology as “following the path Newton suggests to the physicist,” rather than as a “stubborn search for causes” (Ménéret 1765a, 232a). In his entry “Observation,” he opposed Newton the physicist, who was close to observation, to Descartes the experimenter (1765b, 314b), and La Caze praised Newton as the author of a system rather than as a man of experiment (La Caze 1755, 19): whether or not all of these statements are fully consistent with one another, they testify to the existence of a ‘vitalist Newton’, i.e., a Newton constructed by the vitalists. Indeed, this has been described as a basic feature of Montpellier vitalism, particularly

its concepts of the specifically vital in living things that made use of analogs to Newtonian gravitation. Sauvages . . . thought the key to medicine lay in nosology, or the classification of diseases according to their symptoms. He [argued for] a ‘philosophical nosology’ that would explain the bodily processes underlying disease states by faculties peculiar to the living body. These faculties were no less efficacious for being, like gravity or elasticity, unknown in their essence (Lesch 1984, 25).

I have already noted the various iterations of the principle that ‘the first cause shall be posited and then studied through its effects’. Some of these versions place more stress on the unknown dimension of the “provisionally inexplicable explicative device,” some less. Some, like Cabanis, present the claim in the language of *facts*: “general facts are what they are; and today there is no more point in explaining *sensibility* in animal physics and rational philosophy than there is in explaining

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quantity’ sounds suspiciously like Haller’s earlier remark.

<sup>44</sup> Hartley 1749, I, 15.

attraction in the physics of masses" (Cabanis 1802, 157). Slightly more sophisticated, but also closer-sounding to Haller's "place-holder," is Johann Friedrich Blumenbach's formulation of the principle, which returns explicitly to the analogy between vital force and Newtonian explicative unknowns in order to articulate his concept of a *Bildungstrieb* or formative drive (*nisus formativus*):

the term *Bildungstrieb* . . . explains nothing itself, rather it is intended to designate a particular force whose constant effect is to be recognized from the phenomena of experience, but whose cause, just like the causes of all other universally recognized natural forces, remains for us an occult quality.<sup>45</sup>

Yet another instance of the Newtonian analogy in the search for vital properties occurs in a somewhat unexpected author, Claude Bernard, who is sometimes presented as the 'Newton of a blade of grass', the existence or possibility of which Kant had denied (Kant 1787, § 75, B 337-338, in Kant 1987, 282-283): i.e., the thinker who modeled the mechanisms and processes powering organic beings, which Bernard called "living machines." This is not the place to determine whether there ever can be a Newton of a blade of grass in Kantian terms (likely not, even if Cuvier declared that there was no reason why "natural history should not have its own Newton" [Cuvier 1825, 4]), but Bernard stated his own version of our analogy, in marked anti-essentialist terms:

[N]either physiologists nor physicians must imagine it their task to seek the cause of life or the essence of disease: that would be entirely a waste of time, pursuing a phantom. The words "life," "death," "health," and "disease" have no objective reality. These are literary expressions which we use because they represent the appearance of certain phenomena to our minds. In this we must imitate the physicists and say what Newton said about attraction.<sup>46</sup>

The analogy was put to work more concretely as well. Ménuret applied this kind of reasoning to sphygmology – the branch of medicine concerned with the pulse (Terada 2006) – because it is a case where a direct structural or otherwise substantival analysis completely misses the target: the functioning of the pulse

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<sup>45</sup> Blumenbach 1797, 18, cit. in Lenoir 1980, 83.

<sup>46</sup> Bernard 1865, Part II, chapter 1, section iv, 114-115. Bernard then includes a passage in quotations which sounds 'Newtonian' but is not by Newton, not least because it includes anachronistic formulations such as "here is the fact, here is the real." He gives the example of bodies falling according to the force of gravity without our knowing the exact existence of this force, with the Latin phrase *quasi esset attractio*, which Bernard may have found in the works of the Baron Massias (either his 1825 *Problème de l'esprit humain* or his 1830 *Traité de philosophie psycho-physiologique*), which provide a methodological summary of Newton on attraction, "quasi esset attractio" (Massias 1825, 129; Massias 1830, 357).

requires an expectant, observational attitude which is not an inductive attitude which makes inferences from particulars to general laws, but rather, as regards methodology, an “extrapolation, interpolation and other combinations of partial generalisations” (Kolak and Symons 2004, 214). This methodology of extrapolation and interpolation is the opposite of an interventionist and ontologically committed approach. Similarly with the functioning of the glands, the topic of Bordeu’s best-known work, the *Recherches anatomiques sur la position et la fonction des glandes* (1751). Their secretory action poses a problem to the mechanist, and the vitalist approach involved positing of a kind of ‘sensation’ within the glands. As Elizabeth Williams puts it,

Mechanists had long attributed glandular action to the compression of glandular bodies by surrounding muscle and bone, but by 1750 it was widely recognized that this approach did nothing to explain why particular glands secreted particular fluids. Indeed it was in regard to this problem that vitalists first made inroads against mechanists, denying the explanatory power of such a model for glandular action and substituting for it a view based on the ‘internal sensations’ alluded to earlier, specifically the ‘taste’ or ‘desire’ of the gland that determined which components of blood it drew to itself and acted upon in furtherance of its specific function.<sup>47</sup>

A higher-level instantiation of this method for describing phenomenal regularity through these kinds of syntheses of different generalisations is the concept of the “animal economy” as Ménéret described it: “this term, taken in the most exact and common sense, refers only to *the order, mechanism, and overall set* of the functions and movements which sustain life in animals” (Ménéret 1765c, 362a). The Newtonian analogy is an important component of the vitalist construction of the animal economy as a structural-functional model of living being; it is a particular instance of what Rom Harré described in general about the formation of scientific theories: “in a creative piece of theory construction, the relation between the model of the unknown mechanism and what it is modeled on is also a relation of analogy. Thus, at the heart of a [scientific] theory are various modeling relations which are types of analogy.”<sup>48</sup>

Of course, one can also be skeptical about the concrete significance of the Newtonian analogy, and ask what it does beyond being an analogy? This can be called the ‘where are the equations?’ objection. In fact, plenty of physiologists in this period *did supply* equations. Some medical Newtonians, like James Keill, Stephen

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<sup>47</sup> Williams 2012, 398.

<sup>48</sup> Harré 1970, 35, reprinted in Harré 2004; Duchesneau 1982, 490 n. 13.

Hales or Buffon, *extended the scope* of Newtonian explanations, whether literally (§ 1 above) or non-literally (§ 2), whereas others, such as the vitalists, articulated their claims on the basis of an *analogy*, a “transfert de méthodologie” as Rey calls it (Rey 1992, 402).

But this distinction is not absolute either, as we also find Sauvages attempting to extend Newtonian calculations, and deliberately imitating the form of Newton’s *Principia* (axioms, corollaries, lemmas, etc.) in his 1740 treatise on vital motions; Sauvages sought to apply Newtonian laws of motion across the board. He asserted that he was a Newtonian because he did not reduce all corporeal phenomena to a particular *substance or entity* (be it a fluid, electricity, or a specific mechanism). In his 1752 *Dissertation sur les médicaments* he called this “following physical causes” rather than “mechanical causes”; the latter include shape, size and mass, whereas physical causes are “general phenomena, the mechanical causes of which are not investigated; rather, [these phenomena] are used to immediately account for many other phenomena.”<sup>49</sup> Sounding again more like a literal-extension Newtonian than an analogical Newtonian, Sauvages also explained that he was willing to eliminate terms like ‘vital force’ from his vocabulary, if others eliminated ‘elasticity’ or ‘gravity’, since in all these “we do not know the essence of these terms.”<sup>50</sup> Closer to Boerhaave or Keill, Sauvages also defended the pertinence and indeed inevitability of mechanical explanations in medicine in his *Nosologia*, citing Newton and Bernoulli (and stressing in the “Prolegomena” to this work that medicine needs mathematics if it is to make successful conjectures, referring to ‘s Gravesande among others). In his earlier work on diseases, Sauvages explicitly related his work to the “beautiful discoveries of Baglivi, Bellini, Pitcairn, Keill, Newton, Boerhaave, Michelotti, Bernoulli” who by “applying mathematics” to physics and anatomy, have revealed “many secrets of nature.”<sup>51</sup> However, Sauvages also thought there were processes specific to living beings, which he detailed in chemical terms (e.g. fermentation and putrefaction). That is, the “faculties” of the body are equivalent to the properties of matter in general (e.g. gravity, elasticity and attraction) but *within the organism* these faculties produce processes of fermentation and putrefaction which seem to be restricted to living beings.<sup>52</sup>

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<sup>49</sup> Sauvages 1770, vol. 2, 3; Sauvages 1752, 11, 12, 13, 14, 26, 33-35; Sauvages 1771, vol. 1, 60-61, 89; Martin 1990, 131; Williams 2003, 84, 88-89.

<sup>50</sup> Sauvages 1763/1771, I, § 209, 64.

<sup>51</sup> *Ibid.*, I, 492; I, 9; Sauvages 1731, 2.

<sup>52</sup> Sauvages 1763/1771, I, §§ 150-154, 261, 266.



Regardless, one should distinguish between the vitalist *analogy* and the *extension of equations* in Newtonian biomedicine, as the former is more *systemic*, while the latter is more rigidly *mechanical*, in the sense that systemic concepts (often credited to Newton through the nineteenth century) produce a model of a diverse set of phenomena, a more or less abstract picture of how such phenomena, whether they be planetary, economic, or biological, can be expected to behave at each instant and over time,<sup>53</sup> as is notably also the case in Bernard's physiology. At any rate, if vitalism 'transfers methodology' by analogical reasoning, rather than extending the quantitative approach, then the question 'where are the equations?' is not the relevant one. My point is both that the usage of 'unknowns' as a basis for bringing together a cluster of phenomena was a key feature of physiological thinking in this period, and that the vitalists employed this in such a way as to conceive of vitality *without locating it in a special substance*.

Consider again the case of Barthez. After having initially asserted the existence of an independent vital force, he appeared chastened by some reactions to his ontological fervour, and added a chapter to the second edition of his book entitled "Skeptical considerations on the nature of the vital principle" (Barthez 1858, III, 96f.). Barthez warned that one should follow an "invincible skepticism" (32) or a "reasonable Pyrrhonism" (274) when it comes to the vital principle. He only "personified" the vital principle, he explained, for ease of argument (126). What does it mean to investigate the nature of life skeptically? Contrary to what one might expect, it does not mean to approach vital phenomena with a demystifying, deflationary attitude, but rather, to attribute properties to the vital principle "that result immediately from experience" (*ibid.*). Now, while it is not the case that Barthez was always so cautious (and to be fair, which 'natural philosopher' of the previous hundred years gave a wholly consistent articulation of the experimental and programmatic sides of their work?), we need only retain his Newtonian insistence that the nature of the vital principle itself is not at stake. For Barthez, it was logically possible to replace this idea of an 'entity' with something more "abstract," a "faculty of the human body, the essence of which is unknown to us, but which possesses motor and sensitive force" (*ibid.*). Thus he could assert that the vital principle is not a substance (113) and that the "science of man" he is defining is precisely not an ontology (129): "I am as indifferent as could be regarding *Ontology* considered as the

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<sup>53</sup> Depew and Weber 1996, Introduction.

science of entities.”<sup>54</sup> This sounds quite close to that other ‘vital Newtonian’, Haller, who used various terms such as *vis insita* but tended to keep them at a distance; indeed in his late article “Faculté vitale,” Haller refers, as we saw, to such terms as “place-holders” (*noms d’attente*).<sup>55</sup>

The diverse usages of the Newtonian analogy in eighteenth-century life science were not, to be sure, the extension of a quantitative, experimental set of methods directly transposed from mechanics and the physical sciences (with Haller being something of an exception here, as he both engaged in some of this transposition, and worked with the theory-modeling dimensions of the analogy). But nor are they, as was sometimes presented in scholarly literature, mere window-dressing, putting new ideas in Newtonian garb “to retain [its] prestige for newer views.”<sup>56</sup> However, it would be a mistake to imagine that Newtonian models seamlessly replaced earlier models, or that avant-garde natural philosophers in a Radical Enlightenment context were all necessarily Newtonian. I now consider one type of reaction which, instead of taking Newtonian methodology or theory-modeling insights as fruitful stimuli, frowned upon the encroachment of the physical sciences onto the complex of medicine, natural history, physiology and overall what we would be tempted to call ‘biology’.<sup>57</sup> Two particular instances of this reaction, which combined a chemical conception of matter with philosophical materialism, are Mandeville and Diderot.

## 5. Anti-mathematicism: Mandeville and Diderot

Programmatic ideas for how to conceptualise the life sciences – their scope, their method, and their boundaries – in the mid- to late-eighteenth century often appealed to Newtonian insights, as we have seen. From Haller to the Montpellier vitalists, this kind of approach sought to capitalize on the power of the Newtonian analogy, without any metaphysical or experimental claim to be doing a ‘different kind of science’. But some other approaches, which also had a strong affinity to vitalism, albeit in the form of a ‘vital materialism’ (Reill 2005, Wolfe 2009), were

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<sup>54</sup> Barthez 1806, 96, note 17.

<sup>55</sup> Haller 1772, 244b; see also Haller 1777a, 663f.

<sup>56</sup> Schofield 1970, 191; a similar view is found in Schlanger 1971/1995, 105.

<sup>57</sup> For further discussion of the status of these sciences prior to the emergence of this nomenclature, see Wolfe 2011a.

more opposed to physico-mathematical encroachment onto the territory of the life sciences. And they are empiricist.

One form of anti-mathematicism in life science was the physician Bernard Mandeville's skeptical attitude (itself reminiscent of Sydenham's hostility to mechanism-friendly anatomical experimentation) towards quantitative, numerical approaches in medicine. In his *Treatise of Hypochondriack and Hysterical Diseases* (1711, revised 1730), which is in dialogue form, Mandeville addressed the pertinence of Newtonianism in medicine rather skeptically (Mandeville 1730, 175, 201). The character Philprio, who various hints identify as Mandeville, specifies that it is in the realm of *practice* that he cannot see the usefulness of mathematics. The other character, Misomedon notes that it may be a matter of time:

But the Scheme of bringing Mathematicks into the Art of Medicine is not of many Years standing yet. The *Newtonian* Philosophy, which I believe has in a great measure been the Occasion of the Attempt, was not made publick before the latter End of the last Century: And considering the vast Extent the Art of Physick is of, both as to Diseases incident to human Bodies, and the Medicines that are made use of, great length of time must be required before an entire System can be form'd, that shall be applicable to all Cases, and by the Help of which; Men shall be able to explain all *Phenomena* that may occur, and solve all the Difficulties and Objections that may be made (Mandeville 1730, 181)

Obviously, in the mechanical approach to the structure of the body, we need mathematics. "All Fluids likewise are subject to the laws of Hydrostaticks" (179). But if we do not know the exact nature of the elements of these entities, calculations are pointless (183). What physicians want to know and they lack is (a) the *causes* of diseases and (b) the properties ("virtues") of each remedy in the *materia medica* (*ibid.*). An exact mathematico-mechanical model in which the dose of the remedy is proportionate to the quantity of blood in the individual (*ceteris paribus*, Mandeville says!) is false, since temperaments or individual natures as encountered by the physician do not obey such laws (187).

Recall that this was a core claim of the Scottish iatromechanist (and medical Newtonian) William Cockburn, some decades earlier: that "The doses of medicaments necessary to elicit a certain effect are proportional to the quantity of the blood" in the individual (Cockburn and Southwell 1704, 2119). A similar objection to the 'medical Newtonians' was made by a late figure of Montpellier vitalism, Jean Charles Marguerite Guillaume de Grimaud, whose medical thesis on irritability was published only under his initials ('D.G.') in 1776. Grimaud explicitly targeted Keill

and others on their claims to quantify muscular action, combining mathematical criticisms with appeals to empirical evidence, ranging from the bizarre feats of muscular strength in the animal world to King Augustus II of Poland's ability to bend horseshoes with two fingers (Grimaud 1776, 33, 35). Mandeville gives the example of water: the difference between cold water, which we drink with pleasure and is necessary to our survival, and hot water, which makes us vomit, is not a difference that can be measured in its mass (Mandeville 1730, 192-194).

If he was not (quite) a mechanist, how does Mandeville account for the physiological processes which apparently underly our mental life? In *chemical* terms, appealing to "ferment" concepts in medicine (17), naming "Concoction" as "that which is the basis of the whole Oeconomy" (84). Of course there is no absolute historical or conceptual opposition between Newtonianism and chemistry (Keill, Friend, Cheyne and later Boerhaave, the author of the *Elementa Chemiae* (1732), would certainly not approve of opposing them, either because of more pluralistic approaches to attraction, or because of the belief that "chymical operations" could be accounted for mechanistically: Metzger 1930, Franckowiak 2003). But thinkers such as Mandeville and Diderot did so, the first on practical, falsifiable grounds, and the second for reasons involving matter theory and broader ontological commitments.

Diderot offered a much sharper, and perhaps more 'categorical' form of Mandeville's objection. Where Mandeville was skeptical about mechanical methods but allowed for their content to be gradually filled in by successful experiments (rather like Haller conceding that "up until now, the calculators have arrived at such opposed results that they have put off modern physiologists from any use of geometry"), Diderot hinted at a profound ontological divide between the two kinds of sciences:

We are on the verge of a great revolution in the sciences. Given the taste people seem to have for morals, *belles-lettres*, the history of nature and experimental physics, I dare say that before a hundred years, there will not be more than three great geometers remaining in Europe. The science will stop short where the Bernoullis, the Eulers, the Maupertuis, the Clairaut, the Fontaines and the D'Alemberts will have left. . . . We will not go beyond.<sup>58</sup>

Diderot uses 'geometricians', as he often does, as a generic term for mathematicians. His crucial claim, whether or not it was historically validated, is that mathematics

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<sup>58</sup> Diderot, *Pensées sur l'interprétation de la nature* § IV, in Diderot 1994, 561. I discuss this in Wolfe 2009 and at greater length in my forthcoming paper on Diderot's biologicistic Spinozism.

will just drop off or stay where it is, whereas the ‘life sciences’ (the “history of nature” or natural history was a term designating the cluster of activities we might today call biology) will take off. He meant this both as a fact about scientific activity and as an ontological claim, that the processes and entities life scientists seek to understand are not to be understood in mathematical terms.

Similarly, Buffon spoke of an “overreliance (*abus*) on mathematical sciences,” given that mathematical truths are merely “definitional truths”: “exact and demonstrative” but also “abstract, intellectual and arbitrary.”<sup>59</sup> As noted in section 2, this is where Buffon is less of a Newtonian – when he seeks to define and delimit the realms of “natural history and particular physics” (*physique particulière*), as *non-mathematical*. In natural history, Buffon declared, “the topics are too complicated for calculations and measures to be advantageously applied.”<sup>60</sup> And Buffon had translated Newton (*Méthode des fluxions*, 1740), just as Diderot published works on probability theory and attempted an analysis of Newton in his *Mémoires sur différents sujets de mathématiques*.<sup>61</sup> In fact, Diderot’s bold claim about a “revolution in the sciences” follows shortly after a passage referring to Buffon’s criticism of abstraction.

Diderot makes two points in the above passage: first, a claim about the revolutionary dimension of life science in contrast to the ‘static’ situation of the mathematical sciences (and this is both an empirical claim and an ontological commitment to a materialist metaphysics of Life, Wolfe 2011a); second, a critique of mathematical abstraction. The latter point comes up again in a short piece of natural philosophy Diderot composed in 1770, the *Philosophical Principles on Matter and Motion*. There, he puts forth much the same critique with a more explicitly chemical reference:

You can practice geometry and metaphysics as much as you like; but I, who am a physicist and a chemist, who takes bodies in nature and not in my mind, I see them as existing, various, bearing properties and actions, as agitated in the universe as they are in the laboratory where if a spark is in the proximity of three combined molecules of saltpeter, carbon and sulfur, a necessary explosion will ensue (Diderot 1975-, XVII, 34).

In his lecture notes from Guillaume-François Rouelle’s chemistry course in the 1750s (which Diderot attended for three years), Diderot also criticized the abstractions of

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<sup>59</sup> Buffon 1749, I, “Premier discours,” 54.

<sup>60</sup> Buffon, *De la Manière*, in Buffon 1749, I, 62; Hoquet 2005, 175.

<sup>61</sup> On Diderot’s mathematical ability (his capacity to follow differential calculus but not the work of Euler or D’Alembert, and his work in probability theory), see Dhombres 1985.

“physics” and insisted that “it is from chemistry that it learns or will learn the real causes” of natural phenomena.<sup>62</sup>

Newtonian-type unknowns do appear in Diderot, in his late manuscript on physiology: “How many highly certain phenomena are there, whose first cause is unknown? Who know how movement or attraction resides in bodies? . . . These are facts, and the production of sensibility is another fact” (Diderot 1994, 1283). But the attitude that we saw in the two earlier quotations is predominant, whether in the form of the first claim (the autonomy of the biological with respect to mechanical and mathematical explanations) or of the second (an appeal to irreducible chemical properties). This is what Guédon meant by “Diderot’s anti-Newtonianism” (a rather over-inflated phrase, in the end): a reliance on a chemical conception of matter as possessing active properties, over and against Newton, and drawing on Rouelle’s (Stahlian) chemistry of mixts.

This anti-Newtonianism is not the standard ideological opposition to Newton as the patron saint of a Boyle Lectures-type natural theology, but rather an opposition to the ontology of action at a distance, which is also not in favor of a Cartesian physics, like earlier oppositions. The idea is that the project of tables of affinities, which is central in the chemistry of Rouelle and Venel (Pépin 2012), was ontologically opposite to the idea of a system of Newtonian attraction. Rouelle explicitly connected the idea of *affinities* to the older idea of *sympathies*:

The ancient chemists noticed that certain bodies placed at a certain distance attracted one another. They named the cause producing this effect . . . *sympathy*, a term which modern chemists have replaced with *affinity* or *relation*, which does not follow the universal law of gravity . . . but that of the homogeneity of surfaces.<sup>63</sup>

This can be seen as a commitment to the unbroken continuity of matter – philosophically a kind of materialist position – if we supplement this passage with some of Diderot’s commentaries on Rouelle, extending into his idea of a universally sensing matter. But more recent examination suggests it is an overstatement to call Rouelle an “anti-Newtonian” as well (Franckowiak 2003). Indeed, the opposition between a chemically ‘rich’ conception of matter and a more ‘crude’ mechanistic picture is . . . specific to a given program: one could also cite chemists of the period for whom Newtonian attraction was a liberation from strict mechanism.

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<sup>62</sup> Diderot 1975-, IX, 209. His lecture notes were first published in 1887, and are now available in the standard edition of his works: *Cours de chimie de Mr Rouelle* (1756), in Diderot 1975-, IX.

<sup>63</sup> Rouelle, *Cours de chimie, 1754-1758*, ms., cit. in Franckowiak 2003, 244; see also Guédon 1979, 191.

Moreover, Diderot's conception of active matter (or vital matter, since all of matter is potentially alive in his view), his commitment to sensibility as an inherent property of all matter, can be traced to other sources than Rouelle's chemistry. Schofield finds that Diderot's vision of matter "resembles at worst a neo-Platonic living macrocosm and at best a Leibnizian pre-established harmony of self-sufficient monads" (Schofield 1978, 187). Leaving aside the judgmental tone, he has noticed something important: the Leibnizian dimension in Diderot, which marks a limit in the pertinence of the Newtonian analogy. Very summarily, one could say that the analogy (and the usage of unknowns as explicative devices) is quite useful for theory-building (as can be seen, e.g. in Haller, Barthez, Sauvages and all the way to Bernard), but not for handling specific phenomena such as generation (Maupertuis and Diderot would concur here).

Diderot's attitude towards Newton is not easy to make out clearly, but one can summarize his overall relation to the issue as follows: he has an *ontological* opposition to the mathematical treatment of life, whilst he thinks that probability theory does not do violence to the nature of organisms the way that, say, iatromechanism did. The figure of Saunderson in Diderot's 1749 *Letter on the Blind* expresses strong hostility to 'Newtonian deism' or natural theology (an *ideological* hostility which colors some of his objections against action at a distance, both in an "Observation" at the end of the *Interprétation* and later in the 1761 *Réflexions sur une difficulté proposée contre la manière dont les newtoniens expliquent la cohésion des corps*<sup>64</sup>). Lastly, Diderot has a pragmatic or utilitarian attitude towards both mathematics and life science: "in a few centuries, it will be utility (*l'utile*) which will serve as a constraint for experimental physics [*sc.* life science, CW], as it now serves as a constraint on geometry" (*Interprétation*, § VI, in Diderot 1975-, IX, 33). This is neither a belief in the future success of mechanism (filling in place-holders, as Haller might have had it), nor a categorical rejection of this possibility. In this sense it is much too strong to call Diderot "the supreme anti-Newtonian of the High Enlightenment," as Jonathan Israel does (Israel 2006, 222; as noted above, Jean-Claude Guédon put forth this view twenty-five years earlier).

## 6. Conclusion

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<sup>64</sup> In Diderot 1975-, IX; a text printed anonymously in the *Journal de Trévoux* in April 1761, in which he also presents attraction as a "general property of matter" (Diderot 1975-, IX, 341).

I have suggested a series of distinctions between types and usages of Newtonianism in eighteenth-century life science, from the more literal usage found notably in the ‘medical Newtonians’ in the earlier part of the century, which is more quantitative and hardly analogical, to the *very* analogical Newtonianism of Montpellier vitalism. Several noteworthy figures can be located between these two extremes, such as Haller, who on the one hand employed the analogy and the usage of ‘physiological unknowns’ (the *nom d’attente*), but on the other hand also considered the application of geometrical methods to physiology and medicine to be one of the greatest “perfections” of the science. Similarly, if we take the thinkers I surveyed in section 2, Maupertuis and Hartley are closer to the first type, whereas Buffon, especially as he moves further and further into the consideration of the categories and concepts of natural history, is closer to the non-quantitative approach. One could say that these different Newtonian-inspired approaches to life are tantamount to different conceptions of the animal economy: in some cases (Pitcairne et al., Haller, possibly Mandeville) there is a search for physiological laws, whereas in other cases (Barthez, Bordeu, possibly Diderot) there is more of an ‘observational’ attitude, an empiricism which frowns upon providing ultimate definitions of life.

In this sense, the variety of Newtonianisms and here, Newtonian analogies for concepts specific to the life sciences indicates that Schofield, Brown and others are mistaken to state that Newtonian physiology “died” at the end of the seventeenth century,<sup>65</sup> or that “much of what we know [of eighteenth-century biology], once we leave England, escapes our Newtonian net” (Coleman 1967, 269). Newtonian physiology is both extended, as in the case of Haller, and conceptually transformed rather than abandoned through the analogy, with its heuristic potential. Of course, one could object that this potential of the Newtonian analogy is not unique: it is also a feature of all mechanical analogies more broadly, for mechanism in general (not just Newtonianism) functions on the basis of analogies. Indeed, as Hall observed, machines in Renaissance and early modern science had a dual status. On the one hand, they were “facts of life,” “practical realities [such] as waterwheels, looms, pumps, and presses and the windmills,” which were “in view everywhere from the British Midlands to the Peloponnesus.” But on the other hand, they were also

*hypothetical constructs* used for explaining everything from atoms, to animals, to the cosmos. These abstract constructs had, in turn, two sources. They arose,

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<sup>65</sup> Schofield 1970, Brown 1977, Brown 1981; for Brown, “*Principia*-style physiology, by and large, died with him [Newton]” (Brown 1987, 644).



first, as extrapolations from familiar, concrete machinery and, second, as extensions or applications of abstract mathematical mechanics (Hall 1969, vol. 1, 219).

Richard Westfall was thus mistaken in his assertion that iatromechanism “contributed almost nothing to understanding what was seen,” being “for the most part . . . simply irrelevant to biology” (Westfall 1971, 104). For mechanical models of Life, as well as their syntheses with chemical and other models, are powerful heuristics in seeking to understand Life.

But there is at the very least a crucial *theoretical* difference between mechanical and Newtonian analogies, or one might say, a difference in theory construction: despite the variety and flexibility of ‘machine models’ of Life (Des Chene 2005, Wolfe 2012), they are not ‘systemic’ in the sense described above (e.g. with the examples of the animal economy or the functioning of the glands), comprising the inclusion of combinations of partial generalizations. Indeed, a few decades later, the *Encyclopédie méthodique* recommended discontinuing the usage of the term ‘machine’ to describe the body, as had been current in early modern French, and opposed the complexity of causal processes in the “animal economy,” “vital economy” or “organism,” to the “system of causes and effects” as presented in mechanics (“Machine,” in *Encyclopédie méthodique*, 1808, 310).

The difference between mechanical and Newtonian analogies is most marked in cases of *strongly* analogical Newtonianism. Bordeu was one of the few authors of the period who reflected self-consciously on the heuristic role of metaphors or analogies in scientific theory construction (not just as fictions to be discarded, which is of course a more common view). In his masterpiece, the 1751 *Recherches anatomiques sur la position et la fonction des glandes*, when discussing the problem of whether the secretory process of the glands can be reduced to a type of *sensation* or not, he gently criticized Stahl’s notion of an *anima* controlling the body, while noting that both his own idea of sensation and Stahl’s *anima* are *metaphors*:

This is again one of these *metaphors* which I must be allowed; . . . It is difficult . . . to explain myself, when it comes to speaking of the force which so carefully directs a thousand singular motions in the human body and its parts; what terms should I use to describe them? . . . Stahl claimed that the soul directed everything in the animal body. . . . I can state that all living parts are directed by an ever-vigilant self-preserving force; does this force belong, in certain respects, to the essence of a part of matter, or is it a necessary

attribute of its combinations? . . . I can only suggest a *way* of conceiving things, *metaphorical* expressions, comparisons.<sup>66</sup>

To say that the Stahlian concept of soul is a metaphor (which Stahl did not say!) is essentially to say that the concept has *functional* value (or not) depending on how well it models phenomena – rather than making a claim about what sorts of things exist. If Bordeu were writing sometime after the 1970s he would quite likely have spoken of such images as ‘heuristics’. The popularity of the analogy between vital force and Newtonian gravitation, in the eyes of the vitalists, lay in the combination this offered of explanatory power *and* the absence of obligations to provide an account of vitality in terms of micro-structure (given that iatromechanists, whether Descartes, Borelli, Baglivi or Boerhaave, consistently affirmed that micro-structural explanations dispelled ambiguities inherent in chymiatric language and subsumed the variety of functions under a fixed number of mechanical, indeed mathematical laws).

An interesting effect of the Newtonian analogy in eighteenth-century life science is that it allowed its users to focus on the properties unique to living beings without either substantializing them, dissolving them into pan-mechanism, or positing a transcendental ground of Life which is not itself accessible to scientific study, as in Kant’s influential and rather sibylline pronouncement that there will “never be a Newton of a blade of grass.”<sup>67</sup> Neither Diderot nor the vitalists are ontologizing Life the way Kant does – they are heirs of empiricism. Granted, thinkers like Bordeu and Ménéuret, and Diderot in his own context, present the organism in terms of self-organization (with a particular emphasis on epigenesis in Diderot, Wolfe forthcoming), but not in a sense that Kant would approve of, since in the end organisms are defined as self-organizing entities constituted by a special kind of matter (organized or living matter), producing particular levels of organization, without any other special principle. Of course, if we consider Diderot’s statement that “we are on the verge of a revolution in the sciences,” it can be taken to mean that the entities studied in the life sciences are fundamentally different, or more modestly that one should not focus on mathematics at the present time, but instead on areas such as natural history, a.k.a. biology (Wolfe 2009, Wolfe 2011). Support for the former view emerges if we consider his writings more generally,

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<sup>66</sup> Bordeu 1751, § CVIII, 163.

<sup>67</sup> That Kant does not always live up to his own strictures (e.g. in his fascination for Stahl’s account of organisms) is not something that can be addressed in the present paper.

with their focus on living matter, or sensibility as a basic property of matter. But here we have arrived at a point further removed from Newtonianism.

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