The Reception of Isaac Newton in Europe

Volume I

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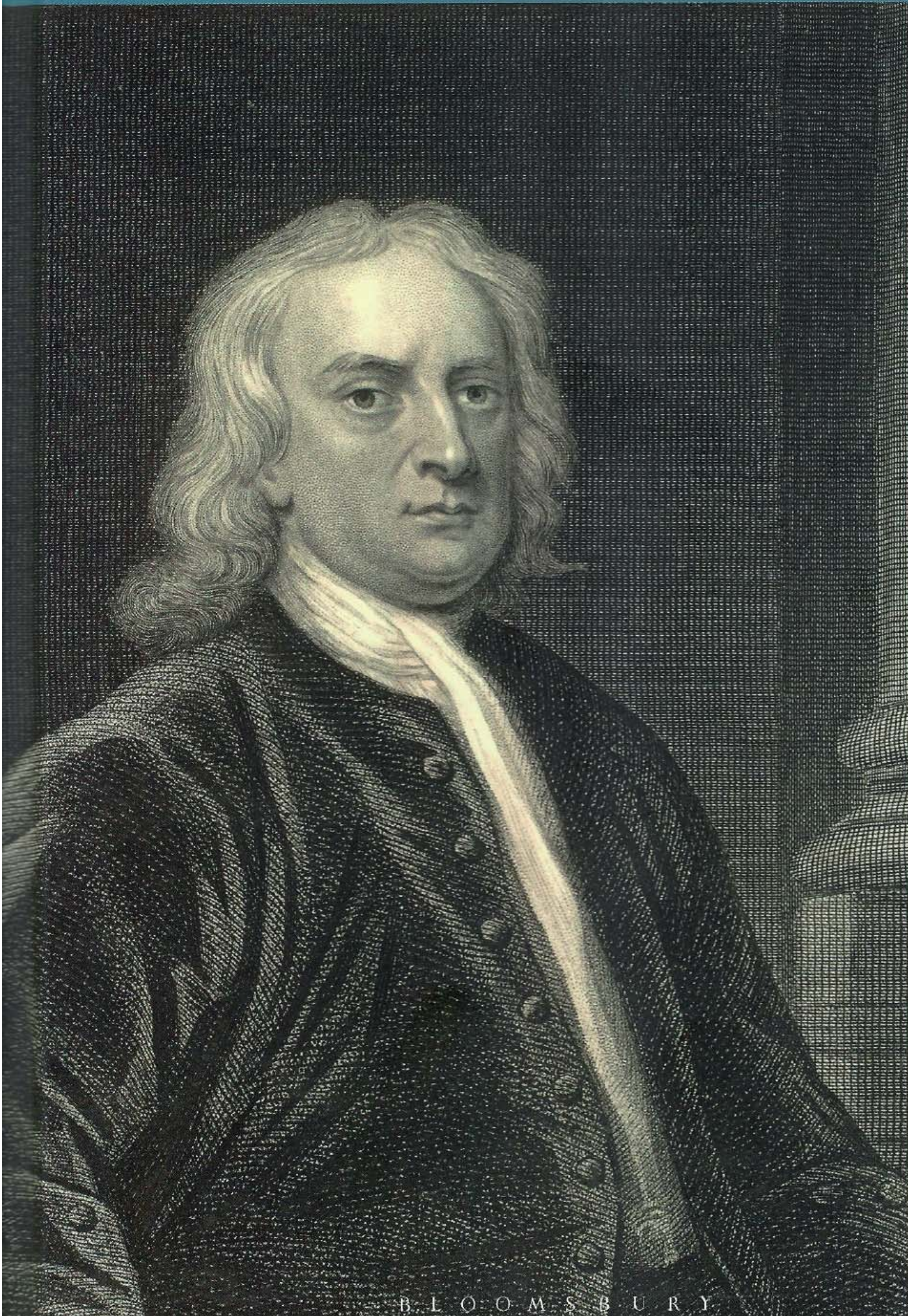

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CHAPTER 16
'TIS MUCH BETTER TO DO A LITTLE WITH CERTAINTY': ON THE RECEPTION OF NEWTON'S METHODOLOGY

Helmut Pulte

1 Introduction

Continuity of discontinuity seems to be one of the main characteristics of the growing 'Newtonian Industry' (Whiteside 1962; Westfall 1976; cf. Pulte 1993b). From the middle of the eighteenth century (see, e.g. Clairaut 1749, 329) to the present age (see, e.g. Cohen 1980; Westfall 1993) there is a remarkable prevalence of the view that Newton inaugurated a scientific revolution or brought about a totally new scientific worldview, i.e. a new way of doing science and thinking about science and its contribution to our understanding of reality. While the discontinuity thesis has great plausibility with respect to some of Newton's main scientific achievements such as his gravitational theory or his calculus, it seems difficult to defend regarding some other scientific areas of fundamental importance. Newton's principles of mechanics were anything but new, and classical mechanics in general can by no means be reduced to a 'Newtonian' mechanics which deserves this *epitheton ornans* in historical respect (see Truesdell 1960 and 1989; Pulte 1989). Moreover, the whole scientific view of Newton's masterpiece, the *Principia*, is strongly shaped by a traditional Euclidean structure and by epistemological claims which hardly characterize *modern* empiricism – a philosophical approach that took control of Newton's *oeuvres* more than any other in the later nineteenth and twentieth centuries (see, e.g. Blake 1966; cf. Pulte 2005 for a closer analysis).

Newton's methodology belongs to such a philosophical context, although it can hardly be disconnected from his scientific achievements (cf. Madden 1966, 119). Several questions have to be addressed right at the beginning. Did Newton develop an original methodology of science? If so, was his methodology received as sufficiently specific and inventive in such a way that this (strongly) influenced the reception of his work? Did Newton, as a working scientist, apply his methodological rules in a discernible manner? If not, were discrepancies noted and discussed by his readers and interlocutors, or did they perceive his science and methodology as being in some sense monolithic? Was his methodology meant to nod to a particular philosophical tradition, and to dissociate itself from others? If so, did it serve its purpose(s)? Was it perceived differently by different communities, for example those of 'working scientists' and of 'reflecting philosophers'?

Answering these and other questions concerning the role of Newton's methodology in the process of acquisition and transformation of his thinking is not an easy task, and will only be partly and tentatively ventured in this chapter. In order to do so, some framing restrictions, premises and perspectives should be disclosed at first.

Restrictions: This paper is about the reception of Newton's *methodology*, not about Newton's 'method' or 'style' in general. Both topics were recently investigated in erudite and comprehensive volumes (see, in particular, Achinstein 2013; Ducheyne 2012; Guicciardini 2009; Harper 2011), and both are often confused in the older literature. A study of the *reception* of Newton's methodology, however, should refrain from alleged 'implicit' methodological thoughts, which can be unveiled only by a study of his practised method, and also from unhistorical ascriptions such as, e.g. a theory of 'mathematical model-building' in Newton, which assumes a modern demarcation of mathematical form and empirical content that does not do justice to contemporary understandings of mathematical entities and formulas as being 'semantically laden'.

This chapter is, in more concrete terms, about the reception of Newton's *explicit* and, in most cases, normatively conceived reflections on topics like induction and deduction (respectively analysis and synthesis), about the misuse and use of hypotheses, about the correct and incorrect application of the 'mathematical method', and related subjects. I also take as *explicit* Newton's witting use of meta-theoretical terms like 'axiom', 'law', 'hypothesis' etc., the basic meanings and differences of those that were well established at that time. Second, I deliberately leave in the background what Newton said about the 'experimental method', because this important topic, linked to his experimental practice, deserves a self-contained investigation (see Friedrich Steinle's Chapter 20 in this volume). Third, although Newton's manuscripts and letters include very interesting methodological remarks, the only sources that are taken into account here are those that were accessible to readers of the eighteenth century. I will briefly comment on these sources in the next section.

Premises: Newton was, first and foremost, a working scientist and mathematician. I take it for granted here that his methodological reflections were subordinated to his scientific activities and were principally meant to defend his scientific results and the epistemic claims related to them. This premise does not imply that his methodology was or is *instrumental* in a strong sense, but that there is a strong tension between results and claims on the one hand and methodological rules on the other. In relation to this, I assume that Newton's methodology is – exactly because of this tension – specific and capable of demarcation by later readers, but not coherent, and therefore prone to criticism by the more critical minds among such readers. Newton's 'Hypotheses non fingo' is most revealing in this respect, but there are other striking features which will be sketched later, but cannot be developed in detail, because the focus here has to be on *reception*.

Perspectives: Following the scope of the volume, this survey of the reception of Newton's methodology is, by and large, restricted to the eighteenth century, although some perspectives on later developments will be ventured too. However, a cross-section topic like methodology, important both for scientists and philosophers, cannot be comprehensively represented for a whole century. Therefore, the later parts of this chapter offer different perspectives in order to identify different patterns of reception. These are *ideal types* in the sense that they are selected from the vast historical material and arranged according to historiographical criteria. The claim, however, is that the perspectives chosen will identify the most important strands in the reception of Newton's methodology in historical respect. First, I will put in perspective some important philosophers and geometers of the French Enlightenment (Part 3), followed by members of the so-called 'Geneva school of physics' (Part 4), the German tradition in (natural) philosophy until Kant (Part 5) and finally Kant's subsequent 'transcendental turn' and its continuation (Part 6). The 'central perspective', so to speak, to start with is defined by the core elements of Newton's methodology (Part 2). The general thesis of this chapter is that Newton's methodology, though not central for his own work or ideas, played a constitutive role for the creation of 'the eighteenth century's marble image' of Newton (Hall 1979, 405), or, in other words, that it enabled Newton's supreme reign – as uncontested hero of the Enlightenment – at the end of the eighteenth century.

2 The Gist of Newton's Methodology in Context

Which parts of Newton's methodology were accessible to a reader of the early eighteenth century? One might begin with his optical investigations in the late 1660s and early 1670s. In the published version of his 'New Theory about Light and Colour' (1672) some remarks on the mathematical and non-hypothetical character of his science of colours were omitted (Newton 1959–1977, 1: 96–97; cf. Newton 1958, 53), but similar statements might be found in the text of his *Optical Lectures*, published after Newton's death in 1728. Here, Newton declared that the basic propositions of his theory of colours should *not* be understood as mere hypotheses with only probable validity, but as finally demonstrated by experience; in the same breath he positioned the mathematical method against the misguided method that, as he claimed, characterized current natural philosophy: 'Nevertheless that from philosophising by geometers and the hard work of philosophers on geometry we may find, instead of conjectures and probabilities, which are sold cheaply everywhere, a science of nature secured by the strongest evidence'.[1] Newton's correspondence with Henry Oldenburg and Robert Hooke as well as other sources make it obvious that a typical and continuous aim of methodology in Newton's writings already underpinned an approach which claimed epistemic superiority, indeed mathematical certainty, and degraded the epistemic status of rival theories, i.e. the theories of colours of Robert Hooke, Christiaan Huygens, Ignace Gaston Pardies and others, who drew a sharp distinction between the certain knowledge of mathematics and the hypothetical or contingent knowledge of natural science (Shapiro 1993, 12–40; see also his Chapter 18 in this volume). Even when Newton appealed to empirical evidence for his theory of colours, his understanding of science was oriented towards a mathematical and deductive ideal in the tradition of Isaac Barrow and Christopher Wren. His rejection of fancied

[1] 'Verùm ut Geometris philosophantibus & Philosophis exercentibus Geometriam, pro conjecturis et probabilibus quae venditantur ubique, scientiam Naturae summis tandem evidentijs firmatam nanciscamur' (Newton 1984, 88).

hypotheses, which later became famous (that is, his banishment of scientific claims which neither described phenomena nor were 'deduced' from phenomena), was rooted in this tradition. This is one lesson that can be learnt already from Newton's early *Optical Lectures*. A second, also prospective lesson from this is that, given the importance for Newton of the mathematical ideal, it seems difficult to make sense of his later statements on induction within a Baconian framework, even if this has been frequently proposed.

Newton's later statements on methodology in the various editions of the *Principia* and the *Opticks* were much more influential. In essence, they may be found in the 'Scholium' of the first edition of the *Principia* (1687), the 'Scholium' to the definitions and axiomata in Book I and the 'Hypotheses' in Book III, in the preface of the *Opticks* (1704), in the 'Queries' added in the later Latin edition from 1706, in the 'Regulae philosophandi' and the 'Scholium generale' to be found in the second edition of the *Principia* (1713) and also – in revised form – in the third edition from 1726. Without going here into the details of these various statements, a famous passage from the end of the *Opticks* (Query 31 in the last edition) allows one to highlight the essential features of Newton's developed methodology:

> As in Mathematicks, 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. For Hypotheses are not to be regarded in experimental Philosophy. And although the arguing from Experiments and Observations by Induction be no Demonstration of general Conclusions; yet it is the best way of arguing which the Nature of Things admits or, and may be looked upon as so much the stronger, by how much the Induction is more general. And if no Exception occur from Phaenomena, the Conclusion may be pronounced generally. But if at any time afterwards any Exception shall occur from Experiments, it may then begin to be pronounced with such Exceptions as occur. By this way of Analysis we may proceed [...] 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.
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> *Newton 1952 [1730], 404*

This is Newton's 'thickest description' of his own methodology, paraphrasing *inter alia* the 'hypotheses non fingo' of the 'Scholium generale' and the fourth rule of reasoning in philosophy from the *Principia* (cf. Newton 1999, 943 and 796). It includes all essential elements of Newton's doctrine of scientific method: *(a)* experiment and observation are the material basis of all scientific knowledge, and experiments yield at least 'certain truths'; *(b)* analytic induction is 'cause-revealing' and the most important method of scientific theory-building; the more general it is, the more reliable its results are; *(c)* synthetic deduction, starting from first principles, is 'phenomena-explaining' and theory-organizing; *(d)* the generality of induction may be restricted by the appearance of conflicting phenomena – however, conflicting phenomena only work as 'restrictors', not as 'rejectors' of inductively gained laws and principles. This point deserves special attention, because it demarcates Newton's understanding of science from a modern, hypothetical-deductive one, although the two have frequently been mixed up by critics (see, e.g. Blake 1966): conflicting observations or experiments cannot falsify general conclusions, but only restrict their range of application. Strict falsification is excluded, because, according to Newton's empiricism, both the conflicting phaenomenon ('Exception') and the inductive generalization ('Conclusion') are true, albeit in different respects. I will come back to this point later.

These are basically elements of an *empiricist* methodology, ostensibly belonging to a Baconian tradition often alleged to have been fostered by the Royal Society. The tension mentioned before does not result from internal incongruity in these elements, but from a linkage to mathematics that was alien to the Baconian tradition and revealed the influence of more mathematical-inclined writers such as Galilei Galileo and René Descartes or, in England, Isaac Barrow and Christopher Wren. The linkage in question becomes obvious from Newton's starting point ('As in Mathematicks ...'). It is also apparent from the parallel drawn between induction and deduction and the method of analysis and synthesis, introduced by Pappus for geometry and used by Descartes and others in the context of mathematics as well. And it is evident by the axiomatic-deductive ideal of science, advocated by Aristotle and exemplified in Euclid's *Elements,* which Newton pursued both in his optics (Shapiro 1993, 26–28 and 34–35) and in his mechanics (Pulte 2005, 66–75 and 119–31). Newton's use of the term 'axiom' and its demarcation from lower-level 'laws' and from 'hypotheses' best reflect both his claim for mathematical certainty in natural philosophy and the tension that resulted from this claim. This use was most prominent in the *Principia*, where he labelled his three laws of motion *axiomata sive leges motus*, i.e. as 'axioms, or the laws of motion' (Newton 1999, 416). The use of 'axiom', as coined in Euclid's *Elements*, unveiled Newton's epistemological claim for certainty to his contemporaries. Moreover, in private communication he explicitly stressed the 'highest evidence' provided by axioms in his philosophy and the fundamental difference of axiom and hypothesis. The latter was 'neither a Phaenomenon nor deduced from any Phaenomenon', and he was personally convinced that 'there is no such phaenomenon in all nature' which might contradict his axioms or laws of motion (Newton 1959–1977, 5: 396–97). The tension, not to say the antagonism, between Newton's empiricist methodology and his understanding of axioms in natural philosophy is obvious. He claimed that axioms were the most general results of induction, and therefore might be understood as universal laws of nature. But he in fact introduced a set of ingeniously chosen mathematical principles that functioned as axioms for the deductive structure of the *Principia*. Asserted truth was thus 'injected' into rational mechanics not from the bottom, but from the top, and its flow down to the level of phenomena could not be reversed by conflicting observations. For Newton, the material truth of axioms, inundating the whole system of propositions, stemmed from mathematics itself. That is the main reason why I regard his natural philosophy as a semi-empirical variant of what I call 'Mechanical Euclideanism' (for the historical relevance of this ideal of science, see Pulte 2005, esp. 66–75). A strong 'mathematical realism' (Jammer 1960, 100) lay at the core of this variant, and became most visible in Newton's ontology of 'absolute, true and mathematical time' and 'absolute space' (Newton 1999, 408). These were indispensable for the foundation of his scientific ideal, but could not be safeguarded by his empiricist methodology.

It has been well known, at least since Alexandre Koyré's *Newtonian Studies*, that the accentuated use of 'hypothesis' from the second edition (1713) of the *Principia* onwards, was intimately connected to Newton's attempt to support his epistemological claims by methodological means (cf. Koyré 1965, 25–52). Newton increasingly paid attention to separating 'good' (i.e. empirically promising) hypotheses from 'bad' (i.e. feigned and fictitious hypotheses). He renamed some of his own 'Hypotheses' of the first edition as 'Regulae philosophandi' in order to emphasize their role as truth-revealing methodological guidelines. As a sharp sword pointed at rival systems, he drew out his 'Hypotheses non fingo' in the new 'Scholium generale': 'For whatever is not deduced from the phenomena must be called a hypothesis; and hypotheses, whether metaphysical or physical, or based on occult qualities, or mechanical, have no place in experimental philosophy' (Newton 1999, 943). It is obvious that Newton's later (and most frequently noticed) remarks on methodology were made to challenge and to provide a demarcation from his rivals. As Koyré put it with respect to Newton's key methodological term: 'The expression "hypothesis" thus seems to have become, for Newton, toward the end of his life, one of those curious terms, such as "heresy", that we never apply to ourselves, but only to others. As for us, *we* do not feign hypotheses, *we* are not heretics. It is *they*, the Baconians, the Cartesians, Leibniz, Hooke, Cheyne and others – *they* feign hypotheses and *they* are the heretics' (Koyré 1965, 52).

It seems questionable whether Newton's methodological creed really shaped his practice of science – his use of conjectural hypotheses is an important point at stake here – and it seems also questionable whether his methodology may guide scientific practice in general, as has frequently been claimed (see, e.g. Harper 2011, 372–96). However, with respect to the reception of Newton's ideas, the question of methodological peculiarity and originality seems more important. Did this really distinguish 'believers' from 'heretics' and contribute to the formation of a 'community of faith'? Though Newton's thoughts about induction and deduction, analysis and synthesis and his reflections on hypotheses seem somehow eclectic and are by no means consistent, I am inclined to answer this question with a cautious 'yes', at least in a prospective sense. Newton promised a methodological 'silver bullet', a way of doing natural philosophy that reconciled empirical foundation with mathematical certainty. His claim was that this method distinguished experimental philosophy from the mere 'speculative philosophy' of his opponents; that is, of Descartes and Leibniz. While *his* system rested on firm, inductively gained principles, *their* systems merely stood on metaphysical fictions. This claim seemed, first and foremost, rewarding to Newton's British disciples, who echoed and strengthened the expectations raised by his methodology, and thereby advanced its (later) continental reception. Colin Maclaurin, for example, promised a smooth and steady growth of scientific knowledge (*epistemé* in the traditional sense) and an end of fruitless controversies, once Newton's methodology had been adopted: 'In order to proceed with perfect security, and to put an end for ever to disputes, he [Newton] proposed that, in our enquiries into nature, the methods of *analysis* and *synthesis* should be both employed in proper order [...]' (Maclaurin 1748, 8). In line with the dominating scientific worldview of the Enlightenment, that there can be only *one* true system of nature and *one* way to accomplish such a system, such British Newtonians stressed the uniqueness of Newton's methodology. William Emerson's statement can be taken as representative in this respect:

> Upon mechanics is [...] founded the *Newtonian or only true philosophy in the world. [...] never a philosopher before Newton ever took the method that he did.* For whilst their systems are nothing but hypotheses, conceits, fictions, conjectures and romances [...], he on the contrary and by himself alone set out upon a very different footing. [...] The foundation is now firmly laid: the Newtonian philosophy may indeed be improved and farther advanced, but it can never be overthrown [...].
>
> *Emerson 1773, V–VII*

Here, it seems particularly noteworthy *how* methodology was brought together with a Baconian vision, reflected in the Queries of Newton's *Opticks*, according to which natural philosophy might be a perennial task of exploring nature in all its varieties. Newton's methodology was, indeed, widely understood by his disciples as being like Ariadne's thread to Bacon's *natura sparsa*. A famous statement of Colin Maclaurin[2] that alluded to a draft of the preface of Newton's *Opticks* pointed to the modest but demanding path: 'Tis much better to do a little with certainty [...]'.[3] Statements like this were attributed to Newton personally (cf. Rob Iliffe's Chapter 26 in this volume), but referred more commonly to Newton's 'experimental philosophy' in opposition to the high-flying and dogmatic systems of metaphysics of his continental opponents. At the same time, they were demanding, because they could be understood to extend a 'new' methodology,

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[2] 'The variety of opinions and perpetual disputes amongst philosophers has induced not a few, of late as well as in former times, to think that it was vain labor to endeavour to acquire certainty in natural knowledge, and to ascribe this to some unavoidable defect in the principles of the science. But it has appeared sufficiently, from the discoveries of those who have consulted nature and not their own imaginations, and particularly from what we learn from Sir *Isaac Newton*, that the fault has lain in the philosophers themselves, and not in philosophy. A compleat system indeed was not to be expected from one man, or one age, or perhaps from the greatest number of ages; could we have expected it from the abilities of any one man, we surely should have did it from Sir *Isaac Newton*: but he saw too far into nature to attempt it' (Maclaurin 1748, 95–96).

[3] The partly similar passage in Newton's draft to the *Opticks* reads: 'And what certainty can there be in a Philosophy which consists in as many Hypotheses as there are Phaenomena to be explained. To explain all nature is too difficult a task for any one man or even for any one age. Tis much better to do a little with certainty and leave the rest for others that come after, than to explain all things by conjecture without making sure of any thing' (Newton Papers, Add MS 3970.3, f. 479; quoted from Westfall 1980, 643).

proven to be successful in physics, esp. celestial mechanics, to more and more areas, hence establishing a completely new scientific worldview, second to none (cf., e.g. Becker 1932; Buchdahl 1961; and Mordechai Feingold's Chapter 27 in Volume 3).

The far-reaching claims of Newton's methodology and the amplifying voices of the 'Newtonian choir' in Britain have to be taken into account in order to understand certain features of the (later) reception of Newton's methodology on the continent, to which I will turn now. In what follows, one should bear in mind that the earlier reception (until the middle of the eighteenth century) did not take place in a 'normal tradition' of Newtonian science in Thomas Kuhn's sense, but rather in a period of permanent foundational disputes between fundamentally different approaches to natural philosophy, i.e. (at least) those of Descartes, Leibniz and Newton, which were based on different scientific metaphysics (Pulte 2001, 61–64; 2012). In this period, controversies with regard to content – the origin and nature of forces, especially gravity, the conservation of *vis viva* or impulse, the discreteness or continuity of matter, the structure of space and time – partly overshadowed methodological issues, while later attempts to explain the success of Newton's approach ruled out possibilities of its extension to new areas and promoted *more influential* methodological discussions. This does not mean that methodology played no role in the early reception of Newton – quite the contrary, it has been shown that the transmission of Newton's doctrines via the Netherlands was accompanied by a discussion of his method in the works of Boerhaave, Musschenbroek, 's Gravesande, Desaguliers and others.[4] It means, in fact, that I will concentrate on later strands of reception because, according to my view, methodology here played a larger role for the shaping of the 'marble image' of Newton (cf. Hall 1979) and because here different strands of reception emerge or become richer in contrast.

3 From Cartesianism to Positivism: *Géomètres* and *Philosophes* of the French Enlightenment

Bernard de Fontenelle, in his influential *Éloge* on Newton from 1727, praised him for being as insightful as Descartes with respect to the 'Necessity of Geometry in Physicks'

[4] To a certain extent, these authors spread and defended Newton's methodology and its anti-hypothetical, certistic claims. Musschenbroek, for example, wrote: 'The eagerness to feign, so popular and widespread in former times, is very much restrained by the banishment of hypothesis; and they were replaced by exact observations, experiments, performed and described with industry and with certain intentions, as well as sound geometrical demonstrations. One discovered the true and certain manner of philosophising, by which certainty and truth in natural philosophy was achieved and science was expurgated from all fictions' ('Die Begierde zu dichten, die in vorigen Zeiten so beliebt und gewöhnlich war, ist durch die Verbannung der Hypothesen sehr gebändiget; und an ihre Stelle sind genaue Beobachtungen, Versuche, die mit Fleiß und in gewissen Absichten angestellet und beschrieben worden, wie auch tüchtige geometrische Demonstrationen gekommen. Man hat die wahre und sichere Art zu philosophieren erfunden, wodurch man zur Gewißheit und Wahrheit in der Naturlehre gelangen und die Wissenschaft von Erdichtungen reinigen kann'; Musschenbroek 1747, 'Vorrede'.). For the Dutch reception of Newton's physics and its methodology, see Brunet 1931; Cohen 1966; and Laudan 1968 (esp. 25–27 and the literature listed there); cf. also Chapter 27 by Mordechai Feingold in Volume 3 on the Enlightenment and Chapter 3 by Eric Jorink and Huib Zuidervaart on the Low Countries in Volume 1.

(Fontenelle 2006, 115), but paid no attention to Newton's methodology in more detail, despite describing Newton's scientific achievements extensively (see Iliffe's Chapter 26 in this volume). A century later, Jean-Baptiste Biot in his biography of Newton from 1822 (translated into English in 1833) was more specific in this respect. He discussed Newton's mathematical and experimental method in some detail as well as his method of synthesis and analysis. The latter was suggested as a reason for his scientific success, in the synthetic style of the *Principia*: 'It is hence evident [. . .] that Newton attained these great results by the help of analytical methods, of which he had himself so much increased the power; and this conclusion acquires certainty from the correspondence between Newton and Cotes, relating to the second edition of the Principia [. . .]' (Biot 2006 [1833], 39–40). The reference to Cotes's correspondence with Newton and his paraphrase of the methodology of analysis and synthesis (cf. Newton 1999, 385–86) were revealing, as was Biot's general commitment to Newton's inductivist philosophy of science in his earlier textbooks (cf. Ivor Grattan-Guinness's Chapter 17 in this volume).

Fontenelle and Biot were separated by a century. It seems quite natural that a contemporary appraisal of a scientific genius should concentrate on his scientific achievements and his personality, while the more distant assessment (also) tried to uncover 'deeper' causes for scientific success and groundbreaking changes in science. Here, epistemological and methodological questions became more relevant. This seems to have been characteristic for the French reception of Newton in general: Newton first gained ground in France as a mathematician and physicist, not as a philosopher; his theory of gravitation, the calculus and his mathematical physics in general were the dominant topics for discussion, not, in the first instance, his methodology (cf., e.g. Brunet 1931; De Gandt 1995; Fellmann 1989; Pulte 2005, 135–86). Early pathfinders of Newton's natural philosophy in France like Pierre Louis Moreau de Maupertuis, Voltaire or Émilie du Châtelet sweepingly praised Newton's 'experimental philosophy' and his application of mathematics to natural phenomena, and also sometimes the subtlety of his method of analysis, but did not pay much attention to his methodology in general (cf. Mary Terrall (Chapter 33), François De Gandt (Chapter 35) and Marta Cavazza (Chapter 38) in Volume 3).

Interest in this part of Newton's intellectual world grew in the middle of the century. The caveat of Anne-Robert-Jacques Turgot (1727–1781) in 1748 to some speculations of Georges-Louis Leclerc de Buffon on natural philosophy, not to complicate Newton's philosophy and fall back into the 'night of hypotheses'[5] is a typical echo to Newton's 'hypotheses non fingo' and a warning about a relapse to Cartesianism. Étienne Bonnot de Condillac (1714–1780), who was a great influence for the implementation of British empiricism in French epistemology and philosophy of language, presented Newton's methodology as a model for scientific inquiry in his *Traité des sistèmes* (1749). Although he implicitly rejected Newton's general ban on hypotheses, insisting that there are good and bad hypotheses and that the former are indispensable (Condillac 1749, 356–58), he praised Newton's system, permitted only by the *hypothesis* of gravitation, as superior to

[5] Letter from Turgot to Buffon from October 1748, quoted from Gay 1966, 136.

the metaphysical systems of the rationalist tradition, i.e. of Descartes, Leibniz, Malebranche and others. It was Condillac's preferential method of inquiry that led Newton to the unveiling of the true mechanism and system of the universe (see esp. Condillac 1749, esp. 376–78). Without mentioning Newton by name, he recommended an analytic approach, trained by the application of mathematics to outward nature in Newton's sense, as the methodological silver bullet for an empirical inquiry of the human mind and for metaphysics in general (Condillac 1749, 443–48). He endeavoured to avoid the synthetic method completely:

> The method I use in order to build up those systems, I call *analysis*. One can see that it comprises two operations, *decomposition* and *composition*.
>
> By the first method, one separates all ideas belonging to a subject and examines them, until one has discovered the idea which is the origin of all the others. By the second method, one arranges them [i.e. the ideas] by the order of their generation.[6]

Jean le Rond d'Alembert (1717–1783), both a *philosophe* and a *géomètre*, was influenced by Condillac's reflexions on scientific method (Hankins 1970, 80–84, 114–18) and discussed Newton's methodology in various of his writings, among them several articles for the great *Encyclopédie* (for these, see Koffi Maglo's Chapter 36 in Volume 3). In his *Traité de dynamique* (1743), D'Alemert still revealed strong Cartesian leanings and aimed at a rational justification of the first mathematical principles of nature, opposing all geometers who think that these principles might be contingent and who 'will ruin the certitude of mechanics and reduce it to nothing but an experimental science'[7] (D'Alembert 1743, xi–xii). Later, in his *Discours préliminaire de l'Encyclopédie* (Preliminary Discourse to the Encyclopédie; D'Alembert 1975 [1751]) from 1751 and his *Essai sur les éléments de philosophie* (Essay on the Elements of Philosophy; D'Alembert 1805) from 1759, D'Alembert was more in favour of Newton's methodological programme of experimental philosophy, but emphasized its mathematical-deductive and non-hypothetical character: Newton was the one who gave philosophy its final shape and recognized that 'the time was ripe to banish conjectures and uncertain hypotheses from physics or at least only to rate them at their worth, and that this science has to be established on experiments and mathematics alone'.[8] What Newton did not and perhaps could not do, according to

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[6] 'La méthode que j'employe pour faire ces sistêmes, je l'appelle *analise*. On voit qu'elle renferme deux operations, *decomposer, & composer*.

Par la premiere, on sépare toutes les idées qui appartiennent à un sujet; & on les examine, jusqu'à ce qu'on ait découvert l'idée qui doit être le germe de toutes les autres. Par la seconde, on les dispose suivant l'ordre de leur génération' (Condillac 1749, 440–41).

[7] '[. . .] nous ne l'adopterons pas non plus, avec quelques Geométres, comme de verité purement contingente, ce qui ruineroit la certitude de la Méchanique, & la réduiroit à n'être plus qu'une Science expérimentale' (D'Alembert 1743, xi–xii).

[8] 'Newton [. . .] parut enfin, et donna à la philosophie une forme qu'elle semble devoir conserver. Ce grand génie vit qu'il était temps de bannir de la physique les conjectures et les hypotheses vagues, ou du moins de ne les donner que pour ce qu'elles valaient, et que cette science devait être uniquement soumise aux expériences et à la géométrie' (D'Alembert 1975 [1751], 148, 150).

D'Alembert, was achieved by John Locke, who created an 'experimental physics of the soul' ('la physique expérimentale de l'âme'; D'Alembert 1975 [1751], 156) along the lines of Newtonian method. Though D'Alembert's understanding of analysis in empirical contexts carried the main elements of Newton's inductive method, it was strongly influenced by the meaning of 'analysis' in mathematics, and he detected one of the main reasons for Newton's scientific success in his application of the calculus to empirical phenomena, which led to a radical improvement of the ancient method of analysis (D'Alembert 1805, 317–23; cf. 360–66). Unlike Condillac, however, D'Alembert did not play down synthesis in comparison to analysis. Instead – probably influenced by Descartes's ideal of a deductive organization of scientific knowledge – he tried to balance both methods in order to achieve a universal methodology for all sciences, including mathematics (cf. Hankins 1970, 116–20). D'Alembert's positive evaluation of Newton's methodology mainly rested on Newton's 'mathematical way' of doing natural philosophy, which allowed him to combine his Cartesian heritage and his (later) more positivist leanings, in which Newton's (alleged) abandonment of a final explanation of gravitation could be smoothly integrated (cf. Guerlac 1977, 173–216). Even so, D'Alembert held tight to the conviction that the first mathematical principles of natural philosophy were not to be gained by a 'Newtonian induction', but were necessary principles revealed by reason and had to be considered as being epistemologically equivalent to the axioms of geometry (see Pulte 2005, 147–51).

Other influential representatives of the French Enlightenment were less in favour of Newton's mathematical approach (and sometimes overtly critical of it). To this group belonged the main 'materialists': Denis Diderot (1713–1784), Paul Henri Thiry d'Holbach (1723–1789) and Julien Offray de La Mettrie (1709–1751). In their chief works, they either turned to the 'experimental' side of Newton's natural philosophy and took no note of his explicit methodology (for example, in the *Pensées sur l'Interpretation de la Nature* (Thoughts on the interpretation of nature; Diderot 1754: cf. Friedrich Steinle's Chapter 20 in this volume)), or they were more or less ignorant of Newton's achievements (for example, *L'Homme machine* (The machine man; La Mettrie 1748, 16 and 143)), at times even hostile to his approach (as in the *Système de la Nature* (Systems of nature; D'Holbach 1770; cf. Catherine Wilson's Chapter 19 in this volume)).

In the later course of the century, Newton's methodology served as a repository for very different concepts of science and scientific worldviews among the French *géomètres* and *philosophes*. In mathematical physics, Joseph Louis Lagrange (1736–1813) in a sense radicalized Newton's 'mathematical way' of doing science. There can be no doubt that he understood his *Méchanique Analitique* (Analytical mechanics) from 1788 to be a continuation and refinement of the *Principia* (cf. Pulte 1989, 232–40). 'Newtonian philosophy' in his understanding was a synonym for a mathematization of nature without any metaphysical presuppositions and that is why, for Lagrange, Newton's method of analysis had no longer to do with conceptual work about basic notions of mechanics, but with the application of (mathematical) analysis (that is the calculus) to nature alone. Although the two parts of his masterpiece, statics and dynamics, were presented in a perfectly synthetic style, expressing Lagrange's claim to unify the efforts of a whole century of mechanics from Newton onwards by a reduction to (and development from) a *single* principle (that of virtual velocities), mathematical analysis became the one and only legitimate method of research in mechanics: 'All who love analysis will see with pleasure that mechanics is about to become a new branch of it, and will thank me for extending its domain like this'.[9] The consequences of Lagrange's 'methodological monism', as I call it, are grave and are – at the end – one reason why classical mechanics, strongly shaped by Newton, ran into a crisis in the early nineteenth century (cf. Pulte 2005, chs. VI, VII).

Pierre-Simon de Laplace (1749–1827) offered a more balanced and comprehensive reception and transformation of Newton's methodology at the end of the century in his *Exposition du système du monde* (System of the World, 1796). Though not always conceptually precise, he acknowledged both Newton's method of analysis and synthesis and had a clear grasp that they might be related to a gradual process of induction and deduction. Unlike Lagrange, Laplace held that the method of analysis was not exhausted by the application of the calculus, the important role of which was nevertheless fully acknowledged by him, especially as a means to gain certain and coherent scientific knowledge (cf. Henrich 2010, 83–88). Rather, analysis for Laplace was a stepwise process, starting with observations and their description, the comparison of observations, including the formation of analogies, and then proceeding to the inductive establishment of laws of motion and, on their basis, the uncovering of general laws which might govern the behaviour of bodies under the influence of forces (in the case of astronomy the law of gravitation). Synthesis consisted in descending from the first causes that may be discovered (e.g. gravitation) to the phenomena by the application of general laws of force (Laplace 1884 [1835], 393–94, 455–62). Hypotheses might be allowed as heuristic devices, but were converted into scientific truths by 'successive inductions well effectuated' ('par une suite d'inductions bien ménagées'; Laplace 1884 [1835], 6). Laplace claimed that the *Exposition* offered not only a great number of scientific truths thus discovered, but also the 'true method that has to be followed in the investigation of the laws of nature'.[10] According to him, this methodological doctrine accorded perfectly with the method propagated and applied ingeniously by Newton like no other scientist before; it is 'the most certain method that can guide us in the investigation of truth'.[11]

Without exaggeration, it may be said that French mathematical physics at the end of the eighteenth century interpreted Newton's methodology as a guarantee of scientific truth and as a 'universal key' to unlock the remaining secrets of nature. The increasing positivism of French philosophy, often realigned to mathematical physics as a prototype

[9] 'Ceux qui aiment l'Analyse, verront avec Plaisir la Méchanique en devenir une nouvelle branche, & me sauront gré d'en avoir étendu ainsi le domaine' (Lagrange 1788, vi).

[10] '[. . .] et la vraie méthode qu'il faut suivre dans la recherché des lois de la nature' (Laplace 1884 [1835], 1).

[11] 'Telle est la méthode la plus sûre qui puisse nous guider dans la recherche de la vérité. Aucun philosophe n'a été, plus que Newton, fidèle à cette methode; aucun n'a possédé à un plus haut point ce tact heureux qui, faisant discerner dans les objets les principes généraux qu'ils recèlent, constitute le véritable génie des sciences, tact qui lui fit reconnaitre dans la chute d'un corps le principe de la pesanteur universelle' (Laplace 1884 [1835], 462–63; cf. 455–56).

of science and convinced by an 'essential *unity of scientific method*' (Kolakowski 1972, 17), took Newton's 'hypotheses non fingo' mistakenly as a commitment to an anti-metaphysical and merely phenomenon-orientated philosophical attitude. Jean Baptiste Joseph Fourier's theory of heat was exemplary in this respect (see Grattan-Guinness 1990, 3: 588, 627–31 and his Chapter 17 in this volume).

With less specificity than French (natural) philosophy and mathematical physics, other sciences in the second half of the eighteenth century frequently referred loosely to Newton's method as being the paradigm of scientific method in general, under headings such as 'experimental philosophy' or 'Newtonian philosophy'. This applied not only to natural sciences (see the respective chapters in this volume), but also to more remote disciplines like the human sciences (as they are now called), including economics, history and sociology (see Buchdahl 1961; Guerlac 1977 and 1981; Dierse 1986). To give just one example, at the turn of the century, Charles Fourier (1772–1837) claimed, in his *Théorie des Quatre Mouvements* (Theory of the four movements), to be the first to apply an 'analytic and synthetic calculation' to social systems in analogy to Newton's theory of attraction – 'nobody dreamt of this, not even those in the eighteenth century who wanted to cram analytical methods in everywhere [. . .]'.[12] The mere fact that Fourier, in trying to exploit an analogy between attraction in the social sphere and 'material attraction', ascribed the explanation of the latter to both Newton and Leibniz (Fourier 1808, 12) makes it clear that such incoherent references were not based on a serious reflection about Newton's methodology, but instead show that 'Newton's method' was in the air in all kinds of scientific enterprises at that time. The cultic worship of Newton in the late French Enlightenment had a strong *ideological* drift[13] under the surface of rationality and scientism, which was fostered by Newton's methodology. If Ernst Cassirer was right that the Enlightenment in general was coined by a principle of *immanence*, that is by a striving to understand the natural and cultural world without recourse to any transcendent epistemic authorities like theology and traditional metaphysics, this worship implied *anti-Enlightenment* features. The intention to extend Newton's methods to the spheres of history, morality, politics and social life was evoked by the promise

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[12] 'Je pensai dès-lors que l'attraction tant décriée par les philosophes était interprète des vues de Dieu sur l'ordre social, et j'en vins au *calcul analytique et synthétique* des *attractions* et *repulsions passionnées*; [. . .]. On aurait donc découvert les lois de l'association sans les chercher, si l'on se fût avisé de faire l'analyse et las synthèse de l'attraction: c'est à quoi personne n'a songé, pas même dans ce 18.ᵉ siècle, qui, voulant fourrer partout les methods analytiques, n'a pas essayé de les appliquer l'attraction' (Fourier 1808, 20; accentuation in the original).

[13] Cf. Agassi 1979, 427–28. Agassi uses 'ideology' in a slightly different meaning not to be discussed in this chapter. I use 'ideology' here in the sense of a collective consciousness that is not backed by sufficient experience and rational analysis (cf. Pulte 1993a). It is, with respect to Newton, indirectly addressed by Isaiah Berlin, when he writes: 'The impact of Newton's ideas was immense: whether they were correctly understood or not, the entire programme of the Enlightenment, especially in France, was consciously founded on Newton's principles and methods, and derived its confidence and its vast influence from his spectacular achievements. And this, in due course, transformed – indeed, largely created – some of the central concepts and directions of modern culture in the west, moral, political, technological, historical, social – no sphere of thought or life escaped the consequences of this cultural mutation' (Berlin 1980, 144).

implied in the slogan 'Tis much better to do a little with certainty', but without any doubt enormously exceeded Newton's original inductive justification and bore the characteristics of religious faith (cf. Pulte 2000, 103–06).

4 'The True Heirs of Newton': Methodology in the Geneva School of Physics

There is one strand of reception of Newton's methodology in French-speaking science that deserves a distinct treatment, because it did not perpetuate a classical understanding of science, ending in first and certain principles by the application of an inductively conceived analysis and synthetically building up a coherent scientific system by deduction from such principles. Instead, it stressed the fundamental role of hypotheses as the means *and* the end of science. This strand has been labelled the 'corpuscular-kinetic physics of the Geneva school'.[14] The two main representatives of this school were George-Louis Le Sage (1724–1803) and his disciple Pierre Prevost (1751–1839). Both are known in the history of physics as advocates of a mechanical explanation of gravity by the movement of extremely small and fast particles, atomic in character and not constituted by 'ordinary' matter. Their approach was similar to Leonhard Euler's explanation of gravity (see Pulte 1989, 110–21, 161–76) and Le Sage developed it partly in exchange with Euler (see Kleinert 2016). However, he did not advertise his theory as an 'anti-Newtonian' return to a Cartesian-like mechanism (as Euler did), but linked it to the well-known and futile attempts of Newton himself to explain the force of gravity by an aether or other mechanical cause, thus supplementing Newton's original doctrine of gravitation. Nicolas Fatio de Duillier (1664–1753), for a while Newton's closest friend, had some influence on Le Sage's explanation of gravity, though both approaches differed in detail.[15]

The Geneva reception and criticism of Newton's methodology has to be interpreted within this context: Le Sage was well aware of the fact that his explanation of gravitation was speculative and hypothetical and that other hypothetical explanations were possible. By about 1755 he had developed his hypothetical and speculative method in a paper 'Sur la mèthode d'hypothèse' ('On the method of hypothesis'), which was eventually published after his death by Prevost (Le Sage 1805). In this paper and in later letters and notices, Le Sage criticized Newton for his 'hypotheses non fingo' on two grounds. First, Newton himself obviously feigned hypotheses, such as atomism, which were constitutive for his physics (Le Sage 1805, 260–76). Second, Newton did not acknowledge that in many cases hypotheses *remain* utterly conjectural and cannot be converted into indubitable scientific truths by stepwise induction and analogies; the investigation of

[14] I am referring here to the subtitle of Weiss 1988, the full title of which is *Zwischen Physikotheologie und Positivismus. Pierre Prevost (1751–1839) und die korpuskularkinetische Physik der Genfer Schule.*

[15] Cf. Weiss 1988, 100–17, 146, 212; for a more detailed analysis of Fatio's theory of gravitation see Zehe 1980. His relation to Le Sage and interesting aspects of his biography are also described in Mandelbrote 2005.

causes cannot be reduced to these procedures alone, but depends on other methods as well (Le Sage 1798, 129).

Prevost pursued Le Sage's 'method of hypotheses' in his scientific research and developed it in his reflections on scientific method. He insisted, contrary to contemporary methodologists like David Hartley or Joseph Priestley (cf. Laudan 1981, 226–35), that there was no automatic way to improve hypotheses in the course of investigation and thereby to approximate truth (Prevost 1805, 2: 195–96). As Burghard Weiss has shown, both Le Sage and Prevost understood their scientific achievements and their methodology to be a continuation and perfection of Newton's achievements. They felt themselves to be the 'true heirs of Newton' ('Newtons wahre Erben'; Weiss 1988, 371). The history of methodology features a long and unhappy tradition, nursed *inter alia* by twentieth-century logical empiricism, in which it is claimed that Newton was the founder of the hypothetical-deductive method (see, e.g. Blake 1966). However, if this method is to be understood in a modern sense (as a basically fallibilist method of trial and error), and if it is to be understood as a general method (including mathematical physics as an alleged repository of certain scientific knowledge), then instead of Newton himself, his 'Geneva heirs' properly deserve such a title.

5 *Vernünftige Gedanken*, Mathematics and Newton's Growing Influence in the German-speaking Lands

The general reception of Newton's work as well as the admission and transformation of his methodology in the German-speaking lands provides evidence of an even more heterogeneous and complex reception of his ideas than in France. This has to do with political fragmentation and the institutional disparities and differential chronologies of reception that resulted from it. For example, until the middle of the eighteenth century the Berlin Academy was effectively in hibernation and not comparable to academies such as those in Paris and London. Natural philosophy ('Naturlehre', later 'Physick' resp. 'Physik') was not well established at German-speaking universities in the first half of the century, and had – as an experimental discipline – to replace traditional 'dogmatic physics', which was dominated by rationalistic school philosophy; it also had to deal with varying role models, switching between an experimental discipline and applied mathematical science (Stichweh 1984, 318–45). Additionally, in the course of the eighteenth century, various methodologies, which stemmed from mathematics or were oriented towards mathematics (including Newton's), were considered as possible prototypes for philosophy. This led to an extensive discussion of 'philosophy as analysis' ('Philosophie als Analysis'; Engfer 1982, see esp. 68–121). For these reasons, the function and value of mathematics in natural philosophy were intensely debated. Among the central questions that arose were considerations of how far philosophy might adapt methods like analysis and (secondary) synthesis from mathematics, and, whether mathematics might be able to replace metaphysics (Tonelli 1959, esp. 49–64), especially as the foundation for a reformed (basically experimental) natural philosophy. Metaphysics as understood in the tradition of Gottfried Wilhelm Leibniz and Christian Wolff was at stake here, and books like Wolff's *Vernuenfftige Gedancken von Gott, der Welt und der Seele des Menschen* (*Reasonable thoughts on God, the world and the human soul*; Wolff 1719) set a pedantic and grave tone in this part of the academic debate (cf. Mittelstraß 1970, 109). Finally, it should be kept in mind that the controversy between Leibniz and Newton about the calculus created friction in the reception of Newton's work (see Herbert Breger's Chapter 30 in Volume 3, Hall 1980 and Meli 1993). In what follows, the focus will be on the reception of Newton's methodology in philosophy, paying special attention to a discussion of the role of mathematics. However, as (school) philosophy cannot be clearly delineated from a developing natural philosophy ('Naturlehre'), this discussion will also take into account relevant textbooks of physics.

As Thomas Ahnert shows in Chapter 2 in Volume 1 on the general reception of Newton in the German-speaking lands, the acceptance of Newtonian ideas was not necessarily slow and reluctant, but rather sparse and eclectic (cf. Waschkies 1987, 348–73). There were early adherents of Newton and his methodology such as the Swiss Johann Jakob Scheuchzer or Georg Matthias Bose in Wittenberg, but they were few in number and not altogether very influential. Despite the remarks of previous historians, there was *no* genuine 'Newton school' at the Berlin Academy, whether led by Leonhard Euler and Pierre Louis Moreau de Maupertuis or not (Pulte 1989). Instead, there was only eclectic reference made to parts of Newton's doctrine, without much attention to his methodology. Euler, for example, in his first important work *Mechanica sive motus scientia analytice exposita* (Mechanics, or analytical presentation of the science of motion) from 1736 praised the 'synthetic' achievements of Newton's *Principia*, but at the same time criticized its 'analytic' shortcomings (Euler 1848–1850, 1: 3). In Euler's famous *Lettres à une Princisse d'Allemagne* (Letters to a german princess) 'the great Newton' ('der große Newton') was praised for diverse scientific discoveries, but not for his scientific method (Euler 1986 [1768–1772], esp. 21, 29, 58–60). Euler's *magnum opus* on natural philosophy, the posthumously published *Anleitung zur Naturlehre* (Guidance to natural science, written around 1755), made it quite clear that observation and experiment should accompany the investigation of nature, but that neither these methods nor induction represented its basis, which lay instead with the rational analysis of concepts like matter, extension, impenetrability and inertia.[16]

While in textbooks on rational mechanics like Euler's *Mechanica* the 'mathematical way' of Newton's *Principia* necessarily played a role, the great number of common textbooks on natural philosophy or physics (see Lind 1992) discussed neither the methodological role of mathematics nor Newton's general method in detail; Newton's methodology was instead mentioned sporadically and in an eclectic manner. One might, in a loose chronological order, distinguish here two groups with respect to the reception of Newton's ideas:

[16] See Euler 1926; cf. Pulte 1989, 112–17, 171–81. The same can be said of other more philosophical works of Euler on space and time, foundations of mechanics and the constitution of matter. See, e.g. Euler 1748, 1750a and 1750b.

The first of these was still strongly influenced by rationalistic metaphysics in the tradition of Leibniz and Wolff. It included authors like Georg Bernhard Bilfinger (1693–1750), Christian August Crusius (1715–1785), Georg Erhard Hamberger (1697–1755) or Johann Heinrich Winkler (1703–1770) (see Lind 1992, 123–45, for further representatives). In general, these thinkers followed Wolff's attitude to Newton (for this, see Thomas Ahnert's Chapter 32 on Wolff in Volume 3 and Lorenz 1985). Wolff was not an opponent of Newton's 'experimental philosophy' in the sense that he did not acknowledge observation and experiment to be important methods of inquiry; his rationalism did not imply a dispraise of experience (Kreimendahl 2007). Wolff also thought that metaphysics might profit from the methods of concept formation and inspection used in mathematics (Dunlop 2013, 463–68). He esteemed both experimental and mathematical methods, and he was – at the beginning of the eighteenth century – perhaps the most important German-speaking scholar with respect to the dissemination of Newton's achievements (see Lorenz 1985, esp. 92). However, what Wolff did *not* accept was the epistemological pretention that he found in Newton's specific combination of experimental and mathematical method, in particular Newton's claim to gain general and certain principles of natural philosophy by these methods *alone*. For Wolff, metaphysical reflection was essential in order to detect the true and first mechanical causes of nature and the corresponding mathematical laws of nature (see Wolff 1737, esp. 108–43, 228–392; cf. also Steinmann 1913, 55–69).

The authors who constituted the first group (and who are mentioned above) shared in this criticism. They basically accepted or even admired Newton's mathematics, they respected his physical achievements, but they rejected their foundations and therefore were – where this topic is mentioned at all – critics of his methodology. They argued that Newton himself *did feign* hypotheses, and his method of induction was *not* sufficient to found first and certain principles of nature, but was in need of a metaphysics of nature in order to do so. Moreover, Newton did not sufficiently distinguish between mathematical assumptions (for example, the law of gravity) and real mechanical causes (see Bilfinger 1742; Crusius 1749; Hamberger 1741; Winkler 1738 and 1754). The first of these objections aimed at Newton's scientific practice (and charged him with inconsistency with respect to his methodology), the second raised doubts about Newton's epistemological claims and the last two were meant to undermine the entire ontology of his natural philosophy: Newton's house was built on sand, because it was built upon experience and mathematics *alone*. At best, not Newton himself, but his adherents were charged with making a philosophically unsound mingle-mangle of mathematical assumptions and causal claims. Crusius may serve as an example for this interpretation:

> from the mix-up of both [mathematical and philosophical abstractions] results immense confusion. One introduces the concepts of mathematics, which are only imagined, into philosophy, and assesses them as real things. One confuses mathematical forces, which are only general concepts, and the basic forces of efficient causes, which have to be considered in philosophy. Instead of explaining real causes, one exhibits only specious calculations, which are based on assumed hypotheses. [...]

Newton himself did not set out his attractive force as a physical one, but according to his view, it should be only [understood as] a mathematical one.[17]

Crusius, however, was not simply critical of certain 'Newtonians'. He also showed sympathy for their empirical orientation when he attacked the epistemology of Leibniz and Wolff for its formal and relational determination of the first concepts of knowledge and asked them to seek empirical completion from sense experience. His idea that the true method of philosophy was not formally to copy the analytical method of mathematics, but to gain at first its concepts from an analysis that began with given, empirical facts later became important for Kant's precritical discussion of Newton's method (Cassirer 1922–1923, 2: 527–29, 586–88; cf. also Part 6). Crusius was certainly the most influential among the first group of those philosophers who, by and large, still accepted Wolffian metaphysics. He might be joined by Johann Christoph Gottsched (1700–1766), who in his *Erste Gründe der gesamten Weltweisheit* (First grounds of all philosophy) included an extensive treatment of natural philosophy ('Naturlehre'; Gottsched 1733, 1: 321–544). The more empirical character displayed there, however, did not hide the fact that Gottsched was still ruled by the dogmatic *Vernunftlehre* and its methods developed earlier in his work.

Some representatives of 'dogmatic' physics, including (natural) philosophers like Johann Georg Heinrich Feder (1740–1821), Samuel Christian Hollmann (1696–1787) and Johann Heinrich Winkler, were more moderate and paid little attention to 'foundational' shortcomings in Newton's methodology. Hollmann may be characterized as the most conservative of this group. He was a sceptic with respect to Newton's 'mathematical way' (Hollmann 1737, for him see Cramer 1988). Feder, later well known for his dispute with Kant, followed Hollmann's philosophy and shared explicitly his and Crusius' critique of 'certain philosophers who demonstrate everything by mathematical method, but whose arbitrary definitions already entail the embryos of the following conclusions'.[18] Feder's logic (that is his treatment of the philosophical discipline that included methodology) had strong psychologistic leanings and did not clearly separate epistemic capacity from logical processes like induction or deduction. His outline of the history of logic did not mention Newton at all (Feder 1775, 233–40).

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[17] 'Zur Auflösung der natürlichen Begebenheiten habe ich überall physikalisch-mechanische Ursachen gesuchet, und bey Gelegenheit die Unterschiede der mathematischen und philosophischen Abstractionen sorgfältig bemerket. Denn durch die Verwechselung dieser beyden entsteht ungemein viele Verwirrung. Man bringet die bloß eingebildeten Begriffe aus der Mathematik in die Philosophie hinüber, und setzet sie als wirkliche Dinge. Man verwirret die mathematischen Kräfte, welche blosse General-Begriffe sind, mit den Grundkräften der wirkenden Ursachen, welche man in der Philosophie zu betrachten hat. [. . .]

Newton selbst hat seine anziehende Kraft nicht vor eine physikalische ausgegeben, sondern sie hat nur nach seiner Absicht eine mathematische seyn sollen' (Crusius 1749, Vorrede, 42–43).

[18] 'Wenn es in der Philosophie nötig wäre, sich von jemanden zu nennen; so würde ich den Namen dieses Philosophen [Hollmann] bekennen. [. . .] *Crusius* ist vortrefflich, um die Sätze gewisser Philosophen beurteilen zu lernen, die alles nach mathematischer Methode beweisen, aber deren willkührliche Definitionen schon die Embryons von den nachfolgenden Sätzen enthalten' (Feder 1769, 44).

A second group of readers of Newton, which gained ground in the second half of the century, was more distant from 'dogmatic' physics in the tradition of Leibniz and Wolff and more in favour of experimental philosophy in Newton's sense. This had been conveyed to them mainly by 's Gravesande and Pieter van Musschenbroek (Lind 1992, 145–75). For these readers, Newton's doctrines were mostly accepted and eclectically consulted in explanatory contexts, though his methodology usually did not play an important role in their textbooks. Johann Gottlieb Krüger (1715–1759), Johann Andreas Segner (1704–1777) and Gottfried Sell (1704?–1767) were strong proponents of Newton's physics (see Krüger 1740–1774; Segner 1746; Sell 1738), albeit with reservations about Newton's 'mathematical way'.[19] The last decades of the eighteenth century witnessed the spread of such perceptions among authors who included Johann Peter Eberhard (1727–1779), Johann Christian Polycarp Erxleben (1744–1777) and Christian Gottlieb Kratzenstein (1723–1795). They referred more eclectically to Newton's doctrine and his methods (see Eberhard 1753 and 1755; Erxleben 1772; and Kratzenstein 1787; with respect to method also Lind 1992, 182–85). Erxleben's *Anfangsgründe* was one of the most popular textbooks on natural philosophy in the German-speaking lands. There, Erxleben implicitly rejected Newton's attitude to hypotheses, which he held to be unavoidable in research and to serve scientific progress, but at the same time propagated a quasi-phenomenalist interpretation of natural laws in general and of Newton's laws of gravitation in particular that became important for the spread of Newton's natural philosophy towards the end of the century (see Erxleben 1772, esp. 4–8, 84–86). Georg Christoph Lichtenberg (1742–1799), who reworked later editions of Erxleben's *Anfangsgründe*, and is today better known as an aphorist than as a natural philosopher, saw the intellectual world in the last third of the century as being shaped by steady 'trade winds', among which the Newtonian one was dominant:

> Today, my mind follows the thoughts of the great Newton through the building of the world, not without the excitement of a certain pride; after all I am of the same stuff as that great man, because his thoughts are not incomprehensible for me [. . .] and what God called to posterity through this man, is heard by me, although it slips over the ears of millions unheard.[20]

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[19] 'To expel all geometry from natural philosophy is impossible, but it can be limited [. . .]' ('Alle Geometrie aus der Naturlehre zu verbannen ist unmöglich, aber man kann sie einschränken [. . .]', Segner 1746, 'Vorrede'). Even at the end of the century the mathematician Abraham Gotthelf Kästner, teacher of the young Gauss, warned that a certain type of mathematics, i.e. pure mathematics, should not be used in natural philosophy, because its propositions were as worthless as some propositions of scholastic philosophy (see Mehrtens 1990, 405). In general, however – and certainly influenced by the great impact of French mathematical physics on natural philosophy – concerns about the 'mathematical way' faded away at the end of the century. Nevertheless, the growing mathematization of natural philosophy brought about a discussion on the role of hypothesis (Lind 1992, 233–50).

[20] 'In den Gedanken gibt es gewisse Passatwinde, die zu gewissen Zeiten beständig when, und man mag steuern und lavieren, wie man will, so werden sie immer dahin getrieben. [. . .] Mein Verstand folgt heute den Gedanken des großen Newton durch das Weltgebäude nach, nicht ohne den Kitzel eines gewissen Stolzes, also bin ich doch auch von dem nämlichen Stoff wie jener große Mann, weil mir seine Gedanken nicht unbegreiflich sind [. . .], und was Gott durch diesen Mann der Nachwelt zurufen ließ, wird von mir gehört, da es über die Ohren von Millionen unvernommen hinschlüpft' (Lichtenberg 1963, 92–93).

The contribution of textbooks for such a 'silent' implementation of Newton's doctrine in general, including his views on method, notwithstanding, the original development of methodology itself must also be considered. Johann Heinrich Lambert (1728–1777), whose logic was influenced by Descartes and by Wolff and would later become influential for Kant, was the most important German-speaking philosopher in this respect in the later eighteenth century next to Leibniz, Wolff and Kant. His posthumously published *Abhandlung vom Criterium veritatis* (Treatise on the criterium veritatis; written around 1761) was an attempt to develop a philosophy of science, namely a methodology, for the axiomatic-deductive sciences. The fundamental problem ('Basisproblem') for Lambert was to determine how the truth of the first principles of such sciences – and above all of metaphysics as a science – might be guaranteed (see Wolters 1980, 15–28, 51–67). Though the systems of Descartes and Wolff were his main reference points, Lambert considered Newton's methodology to be highly relevant because he focused on the methods of analysis and synthesis and obviously saw them well exemplified in Newton's physics (Lambert 1915 [1761], esp. 35). Lambert elaborated his first attempt into a fully fledged methodology of science in his *Neues Organon* (New Organon) from 1764; the title explicitly referred the reader to Aristotle and to Francis Bacon (Lambert 1990 [1764], 1: IX). Although Lambert linked his project with Wolff's *Begriffslogik*, thereby integrating elements of John Locke's theory of ideas, his logic was – contrary to earlier logics of that time like Feder's – explicitly anti-psychologistic (Wolters 1980, 104–14, esp. 109). In fact, Lambert sought a universal *Vernunftlehre* ('doctrine of reason'), by which he meant to form the methodological basis of *all* sciences (in a quite general meaning: 'Wissenschaften'). In this sense, for Lambert, the 'sciences are only an applied doctrine of reason'.[21] Lambert's methodology, his theory of analysis and synthesis, and his epistemology cannot be discussed here in detail. Nevertheless, they were highly relevant for the reception of Newton's rational mechanics as a well-founded science. They revealed interesting parallels to Newton's own methodology, particularly with regard to the leading function of mathematics as an epistemological ideal in a deductively organized science, as well as in the epistemic aims that might be attained by at least *some* empirical sciences, or in the causal role of the method of analysis, and the possibility of verification through induction. Lambert's esteem for the method of synthesis as a means of specification and demonstration and finally his criticism of feigning arbitrary hypotheses underlined his respect for Newton.[22] It was no accident that Lambert repeatedly referred to Newton's work – not as an explicit source of his own methodological reflections, but as an example of the successful application of an accurate scientific method that might lead to true scientific knowledge:

[21] 'Die Wissenschaften überhaupt sind eigentlich nur eine *angewandte Vernunftlehre*, eben so, wie es eine *angewandte Mathematik* gibt. Man sollte daher allerdings jede *Aufgabe in den Wissenschaften auf bloß logische Aufgaben reduzieren können*. Wir haben aber noch wenige Beispiele davon' (Lambert 1990 [1764], 2: 225–26; §444).

[22] See Lambert 1990 [1764], 1: esp. 225–26 (§444), 304–09 (§§607–14), 288–91(§§581–86), 285–88 (§§576–80b) and 280–84 (§§567–71); for a detailed analysis of these features see Neemann 1993–1994, 2: 127–245.

> In this way, *Newton* stood in his room, and determined from some truths known to him the figure of the earth, the mechanical laws of heavenly motions etc. – discoveries which would be considered as revelations, if *Newton's* mind and the means of measurement were unknown.[23]

Both Lambert and Kant regarded Newton's mechanics as a sublime form of a priori knowledge of nature – 'a priori' being interpreted in a wide and somehow 'relative' sense in Lambert's system and in an 'absolute' sense in that of Kant (Neemann 1993–1994, 2: 185–99, 258–60). Therefore Lambert, like Kant, rejected David Hume's scepticism on epistemological and methodological grounds, and did so in a sarcastic manner.[24] Both tried – although in quite different manners – to integrate the rationalist and empiricist traditions of their time into new, all-embracing philosophical systems, and both tried to save the 'best of both worlds' in order to underpin philosophically the best available science of their time, that was mathematical physics. While Lambert perpetuated some traditional, even Aristotelian, elements in his methodology, Kant understood his 'Copernican Revolution' *inter alia* as a revolution of epistemology *and* philosophical methodology, guided by Newton's doctrine of method for natural science.

6 The 'Transcendental Turn' of Newton's Methodology

Among German philosophers, Immanuel Kant (1724–1804) was one of the greatest (if not the greatest) admirer of Newton's science, although his critical philosophy must be understood as a sharp rejection of the official empirical epistemology of Newtonianism. The importance of Newton's physics for the formation of Kant's transcendental philosophy has been known since the publication in the late nineteenth century of Alois Riehl's *Der Philosophische Kritizismus* (The philosophical criticism) at the latest.[25] In recent times Ernan McMullin asserted:

> The *Principia* evidently played a fundamental role in the shaping not only of Kant's philosophy of science but of his entire philosophical system. Without it to serve as paradigm for what natural science, and more broadly the human powers of

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[23] 'Die wissenschaftliche Erkenntnis deckt uns demnach den Reichtum unseres Wissens auf, indem sie uns zeigt, wie eines von dem anderen abhängt, wie es dadurch gefunden werden könne, und was mit dem Gegebenen zugleich gegeben ist, und folglich nicht erst für sich gefunden werden müsse. Auf diese Art blieb *Newton* in seinem Zimmer, und bestimmte aus einigen ihm bekannten Wahrheiten die Figur der Erde, die mechanischen Gesetze der himmlischen Bewegungen etc. Entdeckungen, die man für Offenbarungen halten würde, wenn *Newtons* Geist und die Wege der Meßkunst unbekannt wären' (Lambert 1990 [1764], 1: 304–05 (§607); cf. also 277–88 (§§562–63), 284–85 (§573), 306 (§611) and 309 (§616)).

[24] See Wolters 1980, 19–20, who quotes unpublished remarks on Hume from Lambert's *Nachlass*.

[25] Riehl 1876–1887, 1: 221–65, esp. 234: 'I think I will be able to show that the influence of Newton's natural philosophy on the formation of Kant's critical philosophy was no lesser than even Hume's influence'. ('Ich glaube zeigen zu können, dass der Einfluss der Naturphilosophie Newtons auf die Entstehung der kritischen Philosophie Kant's kein geringerer war, als selbst der Einfluss Hume's'.)

understanding, could achieve, it seems doubtful whether Kant's transcendental turn would ever have taken place, or at the very least, could have claimed the credibility that it did.

McMullin 2001, 306

Some chapters in this publication consider Newton's impact on Kant's philosophy in various respects: Michael Friedman has traced Newton's general influence on Kant with a special emphasis on his theory of space and related metaphysical and theological issues; Thomas Ahnert has tracked the relationship of Newton and Kant with regard to the development of *Naturphilosophie*; Eric Watkins has considered that relationship in the context of mechanical principles; and Catherine Wilson has done so with respect to matter theory and atomism. In what follows, I will accompany these investigations with some observations concerning Newton's role in Kant's methodology of science and philosophy.

In his precritical period, Kant showed great interest in Newton's physics in general (Calinger 1979), and particularly in his cosmology (Falkenburg 2000), his physicotheology (Waschkies 1987) and, of course, in the possible benefit for metaphysics of taking Newton's science and its methods seriously (see, e.g. Schmucker 1951; Röd 1976; Friedman 1992; Pulte 1998b). In fact, Kant's demand for a *methodological reform of metaphysics* was the *leitmotif* and recurrent theme of his occupation with Newton's method, and his *Allgemeine Naturgeschichte und Theorie des Himmels* (Universal natural history and theory of the heavens) was the first publication that bore witness to his practical knowledge of this method. Several arguments there are to be understood as causal reasoning in the sense of Newton's analysis (cf. Falkenburg 2000, 80–86). Kant, since he was not able to present his approach mathematically, was at this point still hesitant with respect to Newton's synthetic method: 'One might sweepingly, by a series of subsequent conclusions in the manner of the mathematical method, with all its pomp, and even more illusion [...] arrive at the plan by itself which I would like to present of the origin of the building of the world; however, I prefer to bring forward my opinions in form of a hypothesis [...]'.[26]

Omitting here some other early and methodologically relevant publications of Kant, it can be said that his first significant transmission of Newton's methods to philosophy is to be found in the prize essay *Untersuchung über die Deutlichkeit der Grundsätze der natürlichen Theologie und der Moral* (Inquiry concerning the distinctiveness of the principles of natural theology and morals) from 1762. This essay is even more important since it revealed the *leitmotiv* of Kant's interest in methodology, which ran through his whole philosophical *oeuvre* up to his main critical works. That was his desire to end for metaphysics once and for all what he later, in the second edition of his first *Critique*,

[26] 'Man könnte, wenn man weitläufig sein wollte, durch eine Reihe aus einander gefolgerter Schlüsse nach der Art einer mathematischen Methode mit allem Gepränge, das diese mit sich führt, und noch mit größerm Schein, als ihr Aufzug in physischen Materien gemeinhin zu sein pflegt, endlich auf den Entwurf selber kommen, den ich von dem Ursprunge des Weltgebäudes darlegen werde; allein ich will meine Meinungen lieber in der Gestalt einer Hypothese vortragen und der Einsicht des Lesers es überlassen, ihre Würdigkeit zu prüfen, als durch den Schein einer erschlichenen Überführung ihre Gültigkeit verdächtig machen und, indem ich die Unwissenden einnehme, den Beifall der Kenner verlieren' (Kant 1910 [1755], 263; cf. 235–36).

called a 'mere fumbling around' ('ein bloßes Herumtappen') and to pave for it the way of 'the secure path of science' ('den sicheren Gang der Wissenschaft'; Kant 1911 [1787], 7). His early prize essay made quite clear from the beginning that a secure foundation of 'higher philosophy' (metaphysics), can be gained only by following the firm rules of method that Newton had established for natural science:

> If the method is fixed, by which highest certainty for this [philosophical] kind of knowledge can be gained [. . .] then, instead of the eternal change of opinions and school sects, thinking minds have to agree upon a firm doctrinal rule for common endeavours; as *Newton's* method in natural science turned the looseness of physical hypotheses into a secure procedure, following experience and geometry.[27]

Kant's implementation of this programme followed Newton's example only in parts and featured elements of the methodological separation of mathematics and metaphysics to be found in the work of Crusius and other representatives of the Wolffian school, and which were not backed up by Newton's understanding ('As in mathematicks . . .', cf. Part 2). Thus, although Kant argued that mathematics proceeds synthetically, starting from few arbitrary definitions and basic principles ('Grundsätze'), the main business of metaphysics was, for him, to examine the variety of unclear concepts which are given by experience and to proceed analytically to basic concepts (Kant 1912 [1762], 276–90). Kant connected *only* the latter method with Newton's name: 'The true method of metaphysics is basically identical to the one introduced by Newton into natural philosophy, and which had so expedient consequences in that area.'[28] Perhaps, Kant never came closer to Newton's recorded methodological doctrines. He seemed to propagate a metaphysics based on experience which, by analysis and induction, might uncover basic principles of understanding. Therefore, it is not by accident that this period ended with his short essay *Von dem ersten Grunde des Unterschiedes der Gegenden im Raume* (On the first ground of the distinction of directions in space), in which Kant, who was also influenced by Euler, laid claim to a philosophical demonstration of the existence of Newton's absolute space independent of matter (Kant 1911 [1768], 383). In the end, the argument of this essay undermined Kant's earlier dichotomy between the synthetic method (of mathematics) and the analytical method (of metaphysics). The ontological status and epistemological function of space became *one* initial point of his transcendental turn (cf. Riehl 1876–1887, 2(1): 237–65) – which necessarily also turned the tables of his methodology in some respects.

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[27] 'Die vorgelegte Frage ist von der Art, daß, wenn sie gehörig aufgelöset wird, die höhere Philosophie dadurch eine bestimmte Gestalt bekommen muß. Wenn die Methode fest steht, nach der die höchstmögliche Gewißheit in dieser Art der Erkenntniß kann erlangt werden, und die Natur dieser Überzeugung wohl eingesehen wird, so muß an statt des ewigen Unbestands der Meinungen und Schulsecten eine unwandelbare Vorschrift der Lehrart die denkenden Köpfe zu einerlei Bemühungen vereinbaren; so wie *Newtons* Methode in der Naturwissenschaft die Ungebundenheit der physischen Hypothesen in ein sicheres Verfahren nach Erfahrung und Geometrie veränderte' (Kant 1912 [1762], 275).

[28] 'Die ächte Methode der Metaphysik ist mit derjenigen im Grunde einerlei, die *Newton* in die Naturwissenschaft einführte, und die daselbst von so nutzbaren Folgen war' (Kant 1912 [1762], 286).

The critique of Newton's 'official', empiricist philosophy that accompanied this turn is well known and not in need of detailed discussion here. Cognition of the empirical given is not certain (neither in Newton's sense nor in the critical sense of apodictic certainty), the application of mathematics in natural science – which *can* yield the desired certainty – is not sufficiently enacted by Newton and is in need of a conceptual structure depending on a metaphysics (existence of synthetic principles a priori), and no science proper can be founded without a transcendental reflection on the conditions of the possibility of experience, such as the category of causality (see, e.g. Kant 1911 [1786], 467–79). Despite these criticisms, Newton's thoughts on method maintained their relevance for Kant; to a certain extent they became even more important for him. He described the methodological guides of natural science *and* of mathematics as being (historically) necessary for metaphysics to overcome the mere 'fumbling around' and as 'prescribing' instances for nature – instances that first of all made possible the 'Copernican Revolution' which Kant himself proclaimed in the second edition of the *Kritik der reinen Vernunft* (Critique of pure reason; Kant 1911 [1787], 9–13). Abridged and schematized, one might characterize the transformation in question as a *transcendental shift* of Newton's methodology, which included both his methods of analysis *and* synthesis. Metaphysics was in need of the method of analysis which starts from given experience and, understood as philosophy of science, from existing science as a given *factum* and which ends (now) in the disclosure of the transcendental principles of human knowledge. But metaphysics also made use of the method of synthesis in a twofold sense. First, beginning from the conditions of the possibility of experience as principles, it expounded that human knowledge is systematic in character and that science is framed by indisputable theoretical premises. *In concreto*, Kant tried to outline a synthetic metaphysical framework for Newton's mechanics by his *Metaphysische Anfangsgründe der Naturwissenschaft* (Metaphysical foundations of natural science; Kant 1911 [1786]). Even though this attempt failed in several respects, its turn towards a synthetic ideal of science, which presented such foundations in the manner of Euclid's *Elements*, was obvious[29] – and also one reason *why* it failed (Pulte 2005, 228–36). Second, and more generally, Kant presented the structure of the core of his first *Critique*, the 'Transcendental Doctrine of Elements', as being one great 'experiment of reason' that was *synthetic* in character.[30] Compared to Kant's precritical thinking, where (synthetic)

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[29] At the end of the famous preface of this work, Kant remarked: 'In this treatise I have not followed the mathematical method in all severity [. . .], but imitated it, not in order to gain a better reception of it by a pomp of thoroughness, but because I believe, that such a system would be capable of it and that this perfection certainly could be reached betimes [. . .]'. ('Ich habe in dieser Abhandlung die mathematische Methode, wenn gleich nicht mit aller Strenge befolgt [. . .], dennoch nachgeahmt, nicht um ihr durch ein Gepränge von Gründlichkeit besseren Eingang zu verschaffen, sondern weil ich glaube, daß ein solches System deren wohl fähig sei und diese Vollkommenheit auch mit der Zeit [. . .] wohl erlangen könne [. . .]' (Kant 1911 [1786], 478).

[30] 'This experiment of pure reason is quite similar to the experiment of *chemists*, sometimes described by them as the experiment of *reduction*, generally as the *synthetic process*. The *analysis of the metaphysician* separated pure knowledge a priori in two quite disparate elements, i.e. those of the things as appearances, and those of the things in themselves. *Dialectics* combines both again in unanimity with the *unconditional* as the necessary idea of reason, and finds that this unanimity can never be reached without this separation, which therefore is the true one'. ('Dieses Experiment der reinen Vernunft hat mit dem der *Chemiker*, welches sie mannigmal den Versuch der *Reduction*, im Allgemeinen aber das *synthetische Verfahren* nennen, viel Ähnliches. Die *Analysis*

mathematics was separated from (analytic) physics and metaphysics, the transcendental shift brought mathematics and metaphysics – the latter proceeding now in an analytic *and* synthetic manner – closer together. However, and though Kant from time to time was tempted to 'imitate' the mathematical method in philosophy, the 'Transcendental Doctrine of Method' of his first *Critique* made quite clear that mathematics and philosophy must be separated because philosophy always has to start from concepts, while mathematics may be entitled to start with concepts, defined by construction in pure intuition (Kant 1911 [1787], 468–83).

More evidence from Kant's works might be adduced in order to underpin the strong impact of Newton's thoughts on scientific method on Kant's transcendental methodology.[31] However, the previous remarks may be sufficient to suggest how strongly Newton's methodology influenced Kant and how strongly Kant in turn transformed this methodology.

The broad and lasting influence of the Kantian transformation both in 'school philosophy' and the natural sciences cannot be traced here.[32] Instead, this chapter will now turn briefly to Jakob Friedrich Fries' methodology. For a long time marginalized in history of philosophy and science (cf. Pulte 1998a), Fries was the most important philosopher of science in the German-speaking lands in the early nineteenth century. He was also strongly committed both to Kant's transcendental philosophy *and* to Newton's physics. The title of his chief work in philosophy of science from 1822 was reminiscent of Newton's masterpiece: *Die mathematische Naturphilosophie, nach philosophischer Methode bearbeitet. Ein Versuch* (Mathematical philosophy of nature, arranged by a philosophical method. An attempt). While the first part of this work offered the first fully fledged philosophy of mathematics in the German language ('Philosophie der Mathematik' – a term introduced by Fries), the second part can, to a certain extent, be understood as a continuation and modernization of Kant's *Metaphysical Foundations of Natural Science*. Fries' aim was to give a philosophical foundation not only for mechanics, but also for the theoretical physics of his time that followed Kantian lines and did justice to the conceptualization of new empirical findings. The first part of Newton's rational mechanics – 'pure doctrine of motion' ('reine Bewegungslehre') in Kant's and Fries'

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des Metaphysikers schied die reine Erkenntniß a priori in zwei sehr ungleichartige Elemente, nämlich die der Dinge als Erscheinungen und die der Dinge an sich selbst. Die *Dialektik* verbindet beide wiederum zur *Einhelligkeit* mit der nothwendigen Vernunftidee des *Unbedingten* und findet, daß diese Einhelligkeit niemals anders, als durch jene Unterscheidung herauskomme, welche also die wahre ist' (Kant 1911 [1786], 14, note). It should be noticed that Kant drew a similar parallel in the footnote before, when he accentuated a metaphysical method similar to the first one mentioned above: 'This method, imitated the method of the natural scientist [. . .]' ('Diese dem Naturforscher nachgeahmte Methode [. . .]') (Kant 1911 [1786], 13, note).

31 Kant's warning not to 'feign' ('dichten') hypothesis might be mentioned here (Kant 1911 [1786], 502) or his subsequent remark on the explanation of phenomena, which strongly resembled Newton's doctrine: 'For the explanation of given phenomena no other things or explanatory grounds can be brought forward except those which are connected to the given by already known laws of appearances' (Kant 1911 [1786], 503).

32 For philosophy see Cassirer 1922–1923, 2; Hennemann 1975; Butts and Davis 1970; Giere and Westfall 1973; Bonsiepen 1997; Pulte 1998b; and Falkenburg 2000, to mention only a few. For the natural sciences and its textbooks, see Stichweh 1984; Lind 1992; Olesko 1991; Jungnickel and McCormmach 1986; and Clarke 1997. Pollok 1997 offers a good introduction to Kant's *Metaphysical Foundations* and a quite detailed bibliography of sources and secondary literature.

transcendental form – retained an exceptional, 'towering' role, as the a priori laws that it presented are valid for any objects of outward experience. However, the different sensory qualities of physical bodies (like sound, heat, colour etc.) might force one to establish particular physical theories 'under' its heading, the laws of which cannot necessarily be reduced to those of mechanics. This meant that, from Kant's point of view, Fries made an 'empiricist concession' that allowed a *plurality* of physical theories under the towering system of *one* rational mechanics (for more details, see Pulte 2005, 269–73). Starting from this differentiation of (one) system of mechanics and (many) theories of physics, Fries built up a fully fledged methodology of science that picked up important characteristics of Newton's methods of analysis and synthesis, which Fries described as 'regressive' and 'progressive' (cf. Fries 1979 [1822]; 1971 [1827]; 1967 [1828-1831]). The regressive method started from the analysis of complex phenomena and might be described as a rational induction that was oriented towards the mathematical laws of a physical theory – and in the end to the a priori laws of motion which here served as highest heuristic maxims of induction. The progressive method began from the principles of a theory, i.e. a system of mathematical laws of nature, and was in most cases only of limited 'deductive range' (and thus incomplete) due to the complexity of phenomena. Both methods were mutually dependent, and a methodology of the empirical sciences was needed to regulate the interplay of the synthetic 'top-down' and the analytic 'bottom-up' procedure. Fries' description distinctly argued how these synthetic and analytic methods should interact:

> In each mathematical system we can actually develop the system from the highest principles by putting together each complex [*Komplexion*] out of its elements; but with these developments we always reach only a certain point where the composition of the complexes will be too large. Here we follow the reverse way of observation, regard the complex as a whole and just try to organise the complexes at large by an involution without completing the evolution down to the last detail. The latter method of induction demands a development of constitutive laws as precisely as possible in order to obtain certain heuristic principles; however, it remains indispensable in its own sphere as all theoretical compositions always treat only general laws without finding the way to a particular story.[33]

The interplay of both methods raises different questions concerning the formation and justification of scientific theories, the respective status of heuristic maxims as guidelines

[33] 'Wir können nämlich in jedem mathematischen System von den obersten Principien aus vorwärts das System entwickeln, indem wir jede Complexion selbst aus ihren Elementen zusammenstellen; wir kommen aber mit diesen Evolutionen immer nur bis an eine bestimmte Gränze, wo uns die Zusammensetzung der Complexionen zu groß wird. Hier schlagen wir den umgekehrten Weg der Beobachtung ein, und fassen das Zusammengesetzte als Ganzes auf, und versuchen nur die Complexionen im Großen in einer Involution zu ordnen, ohne die Evolution bis ins Einzelne zu vollenden. Das letztere Verfahren der Induction fordert eine möglichst genaue Entwickelung der constitutiven Gesetze, um bestimmte heuristische Maximen zu erhalten; es bleibt aber in seiner eignen Sphäre unentbehrlich, indem alle theoretischen Zusammensetzungen doch immer nur allgemeine Regeln behandeln, ohne sich bis zur einzelnen Geschichte durchfinden zu können' (Fries 1967 [1828–1831], 5: 312–13).

of rational induction, the way in which the regulative and constitutive function of these maxims can be differentiated, etc. These are questions that cannot be followed up in this chapter (for more information, see Pulte 2006, 106–13). Here, a final hint concerning Fries' methodology for the empirical *sciences* – in contrast to Kant, the plural form is appropriate here – must suffice. Fries was very keen on demonstrating that his methodology was not only in line with Kant's doctrine, but also with Newton's general rules of philosophizing – and that it was therefore directed 'against the phantasm of philosophical hypotheses and against the arbitrariness of mathematical fictions'.[34] However, he was more in line with Kant than with Newton when he discussed the role of *pure* mathematics as a device for creating hypotheses which have to be checked by experience (Fries 1979 [1822], 10). The verifying function of mathematics in Newton's *Principia* was, due to his mathematical realism, a *direct* one. Fries emphasized, following the spirit of his time, the autonomy of mathematics, and therefore had to accentuate even more than Kant that the verifying function of mathematics could only be an *indirect* one, which was dependent on the mediation of scientific metaphysics:

> As a result of all these considerations we gain this methodological rule for mathematical physics: Kant's metaphysical natural science is not so much about directly introducing metaphysical principles into physics, but about securing a firmer application of the pure mathematical principles by an elucidation of the fundamental metaphysical ideas. According to *Newton's* mathematical rules the mathematics of his natural philosophy had to expect the knowledge of the nature of the moving forces, and had to offer its services afterwards.
>
> We, however, are better informed by *Kant's* elucidations of the fundamental concepts and vindicate sovereignty to pure mathematics. *Pure mathematics* prescribes for all natural sciences with necessity the first laws of motion and the forms of fundamental forces, by which everything is brought about, as well as the highest forms of all processes, by which physical materials interact.
>
> This right of pure mathematics I would like to defend here by philosophical expedients.[35]

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[34] 'gegen das Phantastische philosophischer Hypothesen und gegen die Willkührlichkeit mathematischer Fictionen' (Fries 1979 [1822], 21; see 17–32 for Fries' broader discussion of Newton's methodological rules).

[35] 'Als Folge aus allen diesen Betrachtungen ergibt sich uns die methodische Regel für die mathematische Physik: durch Kants metaphysische Naturwissenschaft sollen uns weniger unmittelbar metaphysische Principien in die Physik eingeführt, als durch die Aufhellung der metaphysischen Grundgedanken eine festere Anwendung der rein mathematischen Principien gesichert werden. Nach *Newtons* methodischen Regeln mußte die Mathematik seiner Naturphilosophie nur von der Erfahrung die Kenntniß der Natur der bewegenden Kräfte erwarten und dieser hintennach ihre Dienste anbieten.

Wir hingegen sind durch *Kants* Aufklärungen der Grundbegriffe besser verständigt und vindicieren der reinen Mathematik die Herrschaft. *Reine Mathematik* schreibt allen Naturlehren mit Nothwendigkeit die obersten Gesetze der Bewegung und die Formen der Grundkräfte, durch welche alles bewirkt wird, so wie die obersten Formen aller Processe, unter denen die körperlichen Stoffe in Wechselwirkung kommen, vor. Dieses Recht der reinen Mathematik will ich hier mit philosophischen Hülfsmitteln zu vertheidigen suchen' (Fries 1979 [1822], 32).

7 Concluding Remarks

Kant's philosophy worked as a large traction engine for the further discussion of Newton's science and its methodology. Newton's achievements were discussed more critically during the Romantic period (cf. Cunningham and Jardine 1990; Williams 1973) – in the philosophical systems of German Idealism he fell back compared with Johannes Kepler, whose philosophical outlook was estimated as a 'holistic' one in opposition to Newton's alleged mechanistic and materialistic leanings. Yet Kant's transformation made Newton's science a lodestar for most philosophers and many scientists and mathematicians in the German-speaking lands up to the Neo-Kantian school philosophy that dominated the late nineteenth and early twentieth centuries and left its mark even in later logical empiricism. In France, the *Critique de la Science* and, later, conventionalism were influenced by a related philosophical current. Before the revolutions in physics in the early twentieth century, it continued the claims for certainty that could be detected in Newton's methodology and that were transformed by Kant's philosophy of science. The French reception of Newton's methodology up to the end of the eighteenth century (traced in Part 3) was likewise devoted to epistemological certism, but based itself instead on an inductive justificationism which was scarcely fleshed out. As I tried to show, its general orientation was broader and more political, with some ideological undertones. It remained influential via the ideas of Jean Baptiste Joseph Fourier (1768–1830), Auguste Comte (1798–1857) and others in nineteenth-century positivism. By contrast, the Geneva school (cf. Part 2) was the first to develop a *modern* understanding of Newton's methodology, that is an understanding that broke with the assumption that stepwise induction might lead to first and indubitable mathematical laws of nature (or 'axioms'). It is *here* where we find traces of a modern hypothetico-deductivism which has mistakenly been ascribed to Newton himself.

The systematic differences of the four versions of methodology presented in this chapter certainly have to do with the different philosophical backgrounds of their protagonists, especially in Cartesianism or Leibnizianism. However, they also reflect the vagueness and interpretability of Newton's remarks on scientific method that made it difficult clearly to separate, for example, his own doctrine of analysis and synthesis from older ones such as that of Descartes. It was often the case that analysis as a method in Newton's own sense and the analytic produce of the calculus were treated as more or less the same.

Despite the differences between the outcomes of such divergent approaches, some common points can also be detected. One of these is the ambiguous role played by Newton's statement: 'hypotheses non fingo'. Experimental scientists (see also Friedrich Steinle's Chapter 20 in this volume), philosophers and geometers were aware that Newton, in his scientific work, definitely feigned hypotheses, even if his methodological article of faith said otherwise. Another is the perception and critique of Newton's 'mathematical way', best visible in the German reception of his work (described above in Parts 5 and 6). A degree of primacy for the method of analysis, often brought about by the successful application of the calculus, can thus be detected in the reception of his methodology. In this respect, Newton was characteristic of the scientific thought of the

Enlightenment, to which the label 'Age of Reason' has often been attributed in an undifferentiated manner. But this is too simple: 'It is much better, I think, to insist on the importance of deductive-empirical analysis than it is to settle for the vaguer notion of an "age of reason"' (Ford 1968, 24).

Newton's combination of inductivism and mathematical realism however was, by many savants, perceived correctly as being unconvincing or even contradictory. The 'semantical unloading' of mathematics in the course of the eighteenth century (cf. Pulte 2001) made this combination more and more implausible.

From a chronological perspective, it seems that a strong intellectual interest in Newton's methodology by and large succeeded a strong interest in his mathematics (which was accepted by most scholars early on) and in his physics (which was more controversial as a result of his theory of gravitation, but was not seriously contested in the second half of the eighteenth century by most natural philosophers). This may not be surprising. Acceptance that Newton had achieved unique scientific success preceded interest in the *reasons* for this success. A sound methodology provided one decisive reason for scientific success, or, in more traditional philosophical terms, for *episteme* as certain, infallible knowledge about the world. As Imre Lakatos put it: 'before Newton the problem was whether it is possible at all to arrive at *episteme*; after Newton the problem became *how* it was possible to arrive at *episteme*, and how one can extend it to other spheres of knowledge' (Lakatos 1978, 1: 221). It was certainly a bold exaggeration of Lakatos to say that Newton created modern philosophy of science,[36] yet it might be affirmed that interest in the reasons for Newton's scientific success, and therefore in his methodology, did in fact considerably promote the development of philosophy of science during the late eighteenth and the nineteenth centuries (cf. Pulte 2015), even if this does not deal with the meaning of Newton's methodology for the age of Enlightenment sufficiently. The varied (and in part heterogenous) findings of this chapter instead suggest that Newton's methodology was taken as a *new* promise for the validation of an *old* aim (i.e. *episteme*), and as a proclamation that this aim, in the end, might be expanded to all areas of human knowledge. Ernst Cassirer's description in his *Philosophie der Aufklärung* (Philosophy of the enlightenment) does not necessarily present a contemporary estimation of Newton's methodology, but it can be understood as giving the gist of what that methodology meant for the Enlightenment:

> The whole eighteenth century [...] adored Newton as great empirical researcher. However, it did not leave it at that but stressed again and again and more and more forcefully that Newton did not only lend firm and permanent rules to nature, but also to *philosophy*. No less important than the results of his research are his *maxims*

[36] 'In this sense one may say that, while Newton's method created modern science, Newton's theory of method created modern philosophy of science' (Lakatos 1978, 1: 221). 'In this sense' here refers to the two large philosophical research programmes he detects in modern certistic-oriented philosophy: 'justificationist philosophical psychology' and 'inductive logic'. This, again, is a crude simplification of what a historically more erudite analysis of modern methodology has to tell.

for research, the 'Regulae philosophandi' he asserted with respect to the knowledge of nature and engraved on it forever. The unlimited admiration and reverence the eighteenth century showed to Newton is based on this understanding of his *oeuvre*. This work appears important and incomparable not only in its objective and its yield, but even more because of the route which it has taken to this objective.[37]

[37] 'Das gesamte achtzehnte Jahrhundert [...] verehrt in Newton den großen empirischen Forscher; aber es bleibt hier nicht stehen, sondern es betont immer wieder und in wachsender Eindringlichkeit, daß Newton nicht nur der Natur, sondern auch der *Philosophie* feste und dauernde Regeln gegeben habe. Nicht minder wichtig als seine Forschungsresultate sind die Forschungs*maximen*, die er aufgestellt hat, sind die "regulae philosophandi", die er in der Naturerkenntnis zur Geltung gebracht und die er ihr für immer eingeprägt hat. Die grenzenlose Bewunderung und Verehrung, die das achtzehnte Jahrhundert Newton entgegenbringt, gründet in dieser Auffassung seines Gesamtwerkes. Nicht ausschließlich um seines Ertrags und um seines Zieles willen erscheint dieses Werk bedeutsam und unvergleichlich, sondern noch mehr um des Weges willen, der zu diesem Ziel geführt hat' (Cassirer 1932, 43–44).