Response to Arthur, Mercer, Smith, and Wilson

by Philip Beeley, Universität Hamburg

In my introduction to *Kontinuität und Mechanismus*, I expressed surprise at the lack of work which was being done at the time on the young Leibniz in spite of the fact that conditions for investigating the period up to 1676 are almost ideal—certainly in Leibnizian terms. Most of the letters and papers from this period of immediate philosophical significance have now been published in the *Akademie-Ausgabe* so that there is here an incomparably better starting point for detailed studies than in the case of his later work.

Fortunately things have changed just in the space of the last two years:shortly after *Kontinuität und Mechanismus* left the press the long-awaited third and final volume of Konrad Moll's[1] major work on the young philosopher came out and soon an extensive study by Hubertus Busche[2] will appear. The English-speaking academic world has not been left behind: the collected papers of the recently-held Woburn conference on the philosophy of the young Leibniz will shortly be published, as will in-depth studies by Christia Mercer and Richard Arthur. Indeed they[3] along with Catherine Wilson[4] have been at the forefront of the emergence of important work on the young Leibniz in North America.

Together with the increasing interest in the young philosopher we are beginning to see the development of different approaches to his work, the focussing on different aspects. Nevertheless the spirit of Arthur Hannequin and Willy Kabitz, whose broad studies on the young Leibniz even today are justly regarded as valuable sources, is by no means dead. There are and will be those who likewise seek to present complete accounts of the German philosopher's early thought. Neither the one nor the other is I feel to be regretted; serious scholarship, whichever path it takes, will in the long run contribute to a greater understanding of this seminal period in Leibniz's thought.

In fact the question of approach or methodology takes centre stage in the contribution to this symposium from Christia Mercer and her co-reviewer Justin Smith. It also features fairly prominently in the comments made by Richard Arthur. However, in both cases methodological issues are closely tied in with specific criticisms regarding content and in my replies to them I shall endeavour to take up most if not all of the significant points they make. Catherine Wilson appears to have

less difficulty with this question. Her review demanding less of a reply, I shall refer to it in the course of my remarks on the other two reviews.

Leibniz and Aristotle: A Reply to Christia Mercer and Justin Smith

As we know from Leibniz's own reports, already as a student in Leipzig, soon after completing the trivium, he began to weigh up the arguments for and against the new mechanistic philosophy. His ultimate decision to follow the path of moderns represented the commitment to a philosophy in which the problem of the continuum plays a fundamental role already in respect of the three basic principles: magnitude, figure, and motion. *Kontinuität und Mechanismus* traces the development of Leibniz as a mechanistic philosopher from early youth up to 1672 when he departed for Paris, by which time he had already begun to make an impact on the scientific community abroad: his *Hypothesis physica nova*, published a year earlier, was to be the stepping stone to the much sought-after membership of the Royal Society (unfortunately his early attempt to gain membership of the *Académie des sciences* was not quite as successful).

As the title suggests, I focus in the book on the problem of continuity and seek to show how in its various aspects this problem runs like a thread through his early work and indeed draws together areas of his thought which might at first appear quite disparate: natural philosophy, metaphysics, mathematics, theology, law, and so on. The ties between the first three are undoubtedly by far the strongest and that is why in my introduction I state that there is scarcely a problem in the history of Western thought more suitable for showing the intimate relationship between philosophy, mathematics, and natural science than that of the continuum (p.12). This is also the reason why the larger part of the book is dedicated to investigating the young philosopher in regard to these areas.

Part of the aim of *Kontinuität und Mechanismus* is to show that the problem of continuity provides us with a very adequate means of approaching Leibniz's first philosophy precisely because it exhibits these interconnections—this being also in the spirit of his own concept of the ultimate unity of his thought—, while avoiding the difficulty of there today being simply too much material to make a global account feasible, at least within a limited time scale. I sought to make an objective study of an isolable problem.

The term 'continuity' was chosen for the title in preference to the term 'continuum' for two reasons. Firstly, because for my purposes 'continuum' was too narrow, since it refers essentially to a specific structure dealt with in mathematics and

physics. Continuity refers to this structure too, but concerns in addition questions like whether a plenum exists, the composition of bodies, the laws of motion, and so on. Secondly, on account of its ambiguity: it served as an oblique reference to the discussion on continuity and discontinuity in the history of science. I seek to show that both aspects of this topic are reflected in my study. As I point out in my introduction, we have on the one hand the continuing tradition of classical conceptions like that of the Aristotelian continuum into the seventeenth century, while on the other hand the introduction of new questions not only influenced the perception and interpretation of classical ideas, but also led to their being adapted to the new mechanistic point of view (p.11).

Kontinuität und Mechanismus situates the young Leibniz in the context both of the traditions and the contemporary discussions relating to the problem of continuity. This is necessary in order to bring out and fully understand the true significance of his own work on the topic. After all, he was dealing with a problem that had its own history; a history with which he was also well acquainted, particularly in respect of the Middle Ages, initially through scholastic text books and later through reading Libert Froidmont's *Labyrinthus sive de compositione continui.* And then of course other adherents to mechanism were also seeking to provide answers to similar questions. The context was necessary in order to see where similarities exist as well as where Leibniz's own approach is different.

Precisely in order to understand the origin of the problems which Leibniz and his contemporaries were concerned to solve, *Kontinuität und Mechanismus* begins with a chapter on the concept of the continuum in Aristotle. This prevents burdening later sections of the book with extensive discussions on the ancient problem and at the same time allows me to go into some detail on views to be found in the *Corpus Aristotelicum.* As I point out, later discussions largely revolve around Aristotle and subsequent interpretations of his work. But these are not the only reasons: Leibniz, as Mercer and Smith concede, took Aristotle seriously and never doubted the fundamental truth of Aristotle's concept of the continuum, even if he did on occasions (at least *prima facie*) significantly deviate from it. And although there can be no doubt that Leibniz in his early student days was heavily reliant on interpretations of the Stagirite found in scholastic text books or put across in lectures, we know that he soon, particularly on the recommendation of his teacher Jakob Thomasius, began to read Aristotle in the original.

One reason for the fairly extensive account of Aristotle's concept of the continuum, drawing from texts which would have been available to Leibniz, is, as I say, for purpose of later reference. Now since Mercer and Smith, who in the space of

a couple of lines call my first chapter a thorough presentation and a summary, claim that the internal connections are not apparent, let me give a few examples. The concept of number in Aristotle and its relation to the continuum, which I discuss in section 5, leads to the second chapter. There I take up the scholastic discussion on the topic, focussing on two key positions represented by Thomas Aquinas and William of Ockham, but bringing in also later commentaries by Francisco Suárez and Paolo Barbo, both of whom Leibniz quotes in this connection. This chapter in turn introduces a discussion of the concept of continuum and the foundation of number in the *Dissertatio de arte combinatoria* (chapter 3). The question of the relation between space, time, and motion treated of in section two of the first chapter features prominently in chapter 5 in relation to Thomas White's concept of universal continuity and later in connection with Froidmont (chapter 12) and Leibniz (chapter 13). The question of the passage from rest to motion (and *vice versa*), discussed in section three is taken up in chapters 5, 6, and 10. The concept of time itself, the topic of section four, plays a prominent role in chapter 6. The arguments for potential and against actual infinity and in this connection the temporal aspect of infinity itself, with which section six is concerned, is discussed again in chapters 10, 11, 12, and 13. Finally, Aristotle's two definitions of the continuum, his arguments against minima and for potentially infinite parts, are drawn upon in chapters 2, 4, 5, 7, 10, 12, and 13. Distinguishing the analytic and synthetic definitions was essential in order to make the presence of the *ta eskhata en* determination alongside the doctrine of infinite divisibility (or dividedness) comprehensible. All this, I should like to emphasize, ignores numerous other references to Aristotle and quotations from his works in later parts of the book.

Another reason for the account was to indicate the complexity of the discussion in Aristotle and to point out for example certain difficulties pertaining to the understanding of the nature of divisiblity. In the context of remarks on the analytic concept of the continuum for instance I suggest that Aristotle, in spite of his rejection of the existence of the infinite as a distinct reality, nevertheless can be seen to conceive a form of objective realisation of the infinite in the process of division itself. Thus we find a certain justification for the concept of the divided continuum that we find later in Ockham and Gregor of Rimini, both of whom felt they were interpreting Aristotle faithfully. Richard Arthur correctly notes this too, but does not mention my remarks on it (pp. 21-22).

A second criticism which Mercer and Smith make in respect to my account of Aristotle is that I have failed to put him in his historical context. This I will not dispute; on the other hand, to do this adequately would have meant writing a book

on ancient philosophy and that was not my task. And in any case I do on all relevant occasions—and that is to say, when it seemed that something was to be gained by it— indicate contextual aspects: for example, when Aristotle mentions that his doctrine of potential infinity would in no way hinder mathematical practice, I suggest that his remarks take up ideas expressed in Euclid's second postulate and the lemma of Eudoxus (p. 22); I refer to Aristotle's criticism of the Pythagorean and Platonic doctrines of number in the context of explaining his own views (p. 34); I put Aristotle's arguments for the continuum as a structurally-unifying element of space, time, and motion in the context of Zeno's paradoxes (admittedly without going into the paradoxes themselves; I felt I could assume knowledge of them). Later on, I mention some of the difficulties surrounding Aristotle's interpretation of Plato (p. 292). As I say, these are just a few examples. I could cite numerous others. In all cases, I restrict myself to claims which can be substantiated, and where there are uncertainties I phrase the contextual reference in a suitable manner.

I emphasize this, because when Mercer and Smith proceed to make their own suggestions as to the context in which Aristotle should be placed in the framework of a complete account—which needless to say I was not aiming at anyway—we get quite a few surprises. Aristotle, we are told, 'was responding to Zeno's paradoxes, which in turn were first presented in defense of Parmenidean monism as theoretical weapons in the denial of the reality of plurality and motion'. To this I would just say here that although Aristotle certainly does respond to Zeno, his views on the continuum are not reducible to this response. The evidence speaks against this. But then they continue by saying that Aristotle's theory of the infinite 'was on the one hand a denial of monism and on the other hand a denial of the Anaximandrean conception of the unlimitedness of the cosmos'. If things were really that simple!

Certainly, in this context Aristotle does refer to Anaximander and 'most of the *phusikoi*' (Phys. III, 4, 203b14-15), but when he comes to state the most important motive for developing the concept of infinity, he refers to a position most probably derived from the atomists, invoking mathematical arguments (203b23-25). There is a lot more that can be said here, but to keep it short, the history of the Presocratics and Aristotle's relation to them is not quite so clear-cut as Mercer and Smith seem to think. And their later proposal for a comparison between Aristotle and Leibniz in respect to their confrontation with monism—even allowing that it is a rough description—would I supect only work with a considerable over-simplification of the Aristotelian position. And I am not even sure that there are enough similarities to Leibniz's opposition to monism to make such a comparision worthwhile anyway. But there again maybe I have missed something.

Turning to my presentation of Aristotle's position itself, Mercer and Smith question the remark I make toward the end of the first chapter that Aristotle treats of the continuum as an object of physics; that we do not have in his case a purely conceptual treatment such as we find later in the Middle Ages. To question this is rather surprising, since it is not a contentious issue amongst scholars of ancient philosophy, and the relevant texts make this quite clear. And this is not contradicted by the fact that for example in *De caelo* he considers the mathematical aspect. Nevertheless, Mercer and Smith do question it and to this end quote extensively from an article by Jonathan Lear[5]—where, one might ask, are their own arguments?—in which he is concerned to correct what he considers to be a confused interpretation of the nature of infinite divisibility in Aristotle. Having presented his main arguments, they refer to a remark in another section of the article, in which Lear states that the infinite for Aristotle 'is immanent in nature and not a transcendent principle'. There is, I think, no problem with this, and certainly not with the observation he makes immediately afterwards that matter as such, and that is to say prōtē hulē is merely a potentiality: the only way it can exist actually is as informed matter. From this uncontroversial statement of Lear's Mercer and Smith draw the incredible conclusion that infinity and matter are 'imaginary entities'. Aristotle did not say that, and no scholar of the Stagirite would say it either. They then follow from this that 'it is wrong to suppose that for Aristotle continuity is an exclusively physical problem'.

Well, that is certainly not something Lear is arguing for. In fact, to put the matter straight, in the article concerned, Lear is seeking to refute the thesis put forward earlier by Jaakko Hintikka in an article of the same title ('Aristotelian Infinity'), in which the latter argues in respect of the infinite that 'in the exact and proper sense in which, according to Aristotle, it exists potentially, it also exists actually'.[6] Lear thinks that Hintikka has overlooked the essence of the Aristotelian position, which is 'that there will always be possibilities which remain unactualized'.[7] In fact I refer to this discussion in *Kontinuität und Mechanismus*, pp. 21-22, note 55.

One of the aims of *Kontinuität und Mechanismus* is, as I have already mentioned, to situate Leibniz's arguments in their historical context and that is why I introduce his early discussion of the foundation of number in the *Dissertatio de arte combinatoria* by referring to two of the central positions held in the Middle Ages. This also gives me the opportunity to introduce the Ockhamist concept of the divided continuum as a forerunner to model we find in the young Leibniz (and which in fact is already suggested in the *Dissertatio* itself). I thereby bring in the scholastic commentators Suárez and Soncinas. Although short, this chapter took considerable

time to prepare since it involved sieving an immense quantity of text. With the exception of Ockham, there is very little good secondary literature on these topics in the authors concerned.

For Mercer and Smith on the other hand, this chapter (and presumably that on Aristotle as well) is nothing more than an historical summary or survey and moreover one that is not particularly original. Clearly, the charge of unoriginality is pretty damning for any author, and is not easy to refute. It is a charge which of course may be justified; if not, it seeks to do maximum damage. But in any case it demands a reply.

Well first of all I can refer back to what I have just said on the systematic role of the chapter and to the fact that the title makes clear that it is not intended to be a summary. Secondly, it should be fairly obvious that I would not in the space of some ten pages seek to summarize the wealth of scholastic literature on the continuum problem. One of the major figures is undoubtedly Thomas Bradwardine, but he is not even mentioned in *Kontinuität und Mechanismus*. Others like Walter Catton, Gabriel Biel, and Robert Grosseteste are simply mentioned in passing when I consider the arguments discussed by Froidmont (chapter 12). It is just not true that I at any time seek to survey the history of the continuum problem preceding the seventeenth century.

In fact, Mercer and Smith commend the chapter on Froidmont. Even here however there are some misunderstandings that need to be corrected. Firstly, although I indicate that there are significant points of agreement between Froidmont and Leibniz, I do not actually claim that the former influenced the latter—there is not enough evidence to support this. Nor do I claim that Froidmont provided the first early modern solution to the continuum problem. In fact, I show that he was decidedly reticent in this respect, claiming in his conclusion to his arguments against atomism that in all probability no one in future would be able 'to deduce a contradiction from the infinity of parts of a continuum'.[8]

As with Froidmont, my intention in the case of White and Digby was to provide a detailed account of their discussion of the continuity problem, since it deals not only with the philosophical side, but also brings in contemporary developments in mathematics and physics. In these respects I consider inter alia White's (and in part Digby's) arguments against Galileo, Torricelli, and Cavalieri as well as the relations to Descartes. Although I do go into the points of contact with the young Leibniz which are evidenced by the texts (concerning for example the concept of rarity and density, and the nature of the continuum) it is probably true that I could have made more of a direct comparison in the framework of the chapter itself.

But if the criticism of Mercer and Smith in this respect is not unjustified, this is not the case when they come to consider my discussion of Leibniz's opposition to materialism in the context of seventeenth-century microscopy. This discussion ties in, as they correctly mention, with his critique of Anaxagoras. As I show in *Kontinuität und Mechanismus*, Leibniz not only describes the contemporary microscopists Robert Hooke and Athanasius Kircher as 'Anaxagorists', he also quotes examples from their work in order to substantiate his claim that they did not proceed beyond providing analogies and that these simply point to material causes of ordinarily observable phenomena. Now that is a criticism Leibniz actually makes, so there can be little doubt about it. Mercer and Smith however completely ignore the evidence and simply claim that my remarks (which, as I say, come from Leibniz himself) are incorrect. To support this claim they refer to Catherine Wilson's highly acclaimed book *The Invisible World*, in which it is shown that 'the discoveries of the microscopists had an immense impact on the development of Leibniz's mature metaphysical system, a system anything but materialistic'. Of course this is a rather loose argument, since it refers to Leibniz's mature philosophy in order criticize observations made on his earlier work. But not only that, they completely miss the point: I at no time suggest that Leibniz's remarks in respect of causality constitute a general rejection of the work of microscopists, not even of Hooke or Kircher. Quite the contrary is true. Just a few pages earlier I note that Leibniz always had a high admiration for the work of Hooke (p. 200, note 104) and a few pages before that I talk of the importance Leibniz attached to the support his concept of matter received from the results of microscopists, mentioning explicitly Borel, Malpighi, and Hooke (Leeuwenhoek comes in later). In fact I also indicate the continuing importance of microscopy for Leibniz, and quote in this respect from a letter he wrote to Bierling in 1711, where he refers to the observation of animalcula in order to substantiate his view that '*subtilitas naturae procedit in infinitum*' (p. 200, note 104). All of which stands in no contradiction to Leibniz's emphasis on the need to investigate efficient causes in nature, and not material ones—something he continues to maintain in his mature philosophy, as I subsequently show (p. 206).

It seems Mercer and Smith seek to undermine the very foundations of the book by questioning the fundamental role I ascribe to the problem of continuity in Leibniz's early philosophy. In this respect they write that it was 'only one of several problems which interested Leibniz and not the most important one'. Precisely what they mean only becomes clearer later on when they say that although he was undoubtedly interested in the problem of the continuum in the early years 'he was

more concerned to solve theological problems'.

Now let me make it quite clear that I at no time deny the importance of theological concerns to the young Leibniz. In fact, I devote a considerable amount of space to discussing such questions as the Eucharist and Beatific Vision which have immediate relevance to the continuity problem (at least on Leibniz's view). Chapter four is entitled 'Theologie und Mechanismus'. Admittedly, I do not go into his irenical mission, but then *Kontinuität und Mechanismus* does not attempt to provide a complete account of the young Leibniz. I think also I should stress that the book analyses Leibniz's first philosophy in order to show the fundamental role which questions of continuity play in it. That is, I believe, not quite the same thing as saying that the problem of the continuum is the most important problem in the young Leibniz. I do not claim anything of that nature; indeed, I scarcely think it helps our understanding of the young philsopher to start evaluating problems in this way. Interestingly, neither Hannequin nor Kabitz, who both dedicate a considerable number of pages to dealing with theological issues feel that it is necessary to make such a claim. But that is perhaps more a question of style.

Although *Kontinuität und Mechanismus* does not turn on the proposition that the problem of the continuum is the most important in the young Leibniz, it does seek to show that with the increasing diversity of his thought from Leipzig to Paris and beyond, the ramifications of the continuity problem increase as well. That is why I say that it runs like a thread through his thought. The highly diversified, yet at the same time in some way unified philosophy which we find already in Mainz involves not only questions of law, metaphysics, ethics, and theology, but also physics and mathematics (or at least the rudiments thereof), in short, all that we find in the corresponding volumes of the *Philosophische Schriften* and the *Philosophischer Briefwechsel* in the *Akademie-Ausgabe*—and more besides. At least by the time we reach Mainz it is impossible, not to say absurd, to claim that theology is the overriding concern in all these areas; but that seems to be precisely what Mercer and Smith are claiming. Nevertheless, I would accept—and indeed be fully in agreement with this—that theological concerns are vitally important for the development of Leibniz's metaphysics. But perhaps that is what they mean anyway. If so, one wonders how this is to be reconciled with their bold remark towards the end of their review, that it is dangerous to isolate one problem from the others.

The criticism that I have focussed too much on the continuity problem is closely tied in with another charge, namely that I have concentrated on Leibniz's published works and that this has led to gaps (and inaccuracies) in the account given. Before turning to the examples which they cite in order to back this up, let me say that it

would be foolish for anyone to claim that he or she has seen everything which is going on in Leibniz's workshop of ideas—which is largely what the innumerable drafts he left to posterity are a reflection of. But on the other hand to claim that *Kontinuität und Mechanismus* somehow focusses on the published works at the expense of the unpublished drafts is (at best) a misunderstanding.

A close look at the book will show that my approach was to take the major texts up to 1672 as markers and to view them not only as they stand, but also in the light of unpublished material from the immediately preceding and following periods—and occasionally looking even further ahead. I did this primarily to see which ideas from the workshop were taken up and inwhat context, and also to see how problems inherent in certain texts influenced the subsequent path of Leibniz's thought. I chose this approach to avoid the danger of over-interpreting ideas—Catherine Wilson aptly calls them 'momentarily-adopted trial positions'—which can be found in the workshop, but which are not developed further. The danger is even greater, when one starts to talk in a strong sense of Leibniz adopting philosophical positions in the workshop. Even if, for example the term 'atomus' or 'indivisibile' (in a physical sense) occurs in a draft in Mainz or later in Paris, it is not in itself enough evidence to claim that Leibniz is suddenly 'committed' (or perhaps 're-committed'?) to atomism, as Mercer and Smith seem to suggest. As in any workshop, there is a lot of activity in Leibniz's workshop of ideas; there is, to use the expression of Mercer and Smith, a lot going on. Indeed, it would be surprising if there was not. And again, as in many workshops, things are being produced which pretty quickly land in the waste-bins, the bins of intellectual history, one might say. So while it is, staying for a moment with the analogy, good to look in on the workshop (and of course this is essential when we come to consider Leibniz's mature philosophy), to see what is going on, it is also important to note that quite a lot of the production is only provisional. We are also, or should be, interested in the results, and that is to say in the published texts. And certainly in regard to the *Theoria motus abstracti* and the *Hypothesis physica nova* I think we can say that they do not disappoint. It comes down to a question of balance: the letters and papers which were not published at the time are important, not least for the light they throw on the development of Leibniz's thought, the initial ideas he was working on, etc. But what he published was in many ways the culmination of his work at this time. In case there might be any doubt, let me refer to some of the main things in the *Theoria motus abstracti* and the *Hypothesis physica nova* which demonstrate why these writings (in spite of Leibniz's later disparaging comments on them) are so important.

1. We find the fundamental question of the application of mathematics to the natural sciences discussed. He introduces in this context the concept of negligible error.
2. We have the distinction made between mechanical, physical and geometrical construction. This recurs later in the context of Leibniz's mathematical work. Here again he utilizes the concept of negligible error.
3. Leibniz talks of pure constructions in the interior of nature, clearly referring back to the *Dissertatio de arte combinatoria*. He suggests thereby that nature has at its core a mathematical structure, such as is expressed by the infinite dividedeness of matter.
4. We find an ingenuous attempt to save Cavalieri's geometry of the continua. Although Leibniz possesses only rudimentary knowledge of mathematics at the time, his insight into the nature of the continuum enables him to divise a concept of the Cavalierian indivisible which he is able to fall back on when he later develops the infinitesimal calculus.
5. Leibniz also provides an ingenuous model of the chemical atom, composed of cortex and nucleus, which he without difficulty is able to adapt to his theory of the infinite dividedness of matter, while at the same time serving to explain the chemical processes of decomposition and synthesis.

These are just a few points. The list could be considerably longer. And all this still excluding the more immediate philosophical or metaphysical significance.

What we should note is that Leibniz has no qualms about using the term *atomus* where he is clearly not talking about a strict sense of indivisibility at all. But this is only one of the difficulties in always understanding what he means. A major problem for him at the time when he was locating minds in points was the very concept of point he was working with. By conceiving points as having actual *partes indistantes* he was certainly able to provide a tenable solution to the mind-body problem, but only at the cost of making immortality on a theoretical level highly implausible. In exoteric writings like the letter to Duke Johann Friedrich, quoted by Mercer and Smith, he would sometimes simply skate round the problem by taking points ad hoc to be truly indivisible. But this was scarcely an answer and he struggled with the problem for many years, as the vortex model from the later Paris period clearly shows. I go into quite a lot of detail on this in chapter 14 of *Kontinuität und Mechanismus* (particularly in section 2, pp. 351-363). This seems to have escaped the notice of Mercer and Smith.

This leads me finally to the biggest axe which Mercer and Smith wield: the question of methodology. In part they criticize my approach in contradistinction

to that adopted by Mercer in her forthcoming book. Obviously, I am at a disadvantage here, since although I have seen drafts of it, I can hardly compare *Kontinuität und Mechanismus* to something which has yet to leave the press. What I can do, however, is to indicate some of the reasons I had for not adopting what they suggest as a more suitable approach.

It is clear from the remarks Mercer and Smith make that they are particularly interested in investigating the sources of and influences on Leibniz's first philosophy. In fact, they claim that it is 'not too difficult' to actually identify those contemporaries who did influence Leibniz's perception of ancient thought. As I have shown earlier in this reply, classical philosophy is one of their specialisms. Part of the key to this approach is, they say, to see who were Leibniz's teachers and to note which books were available to him. Well sure, no historian of ideas should neglect these things, and I think I can confidently say I did not when writing *Kontinuität und Mechanismus*. On the other hand, library lists, even if complete, do not always tell us an awful lot, expecially in dealing with someone like Leibniz, who would often just read the first few pages of books in order to get inspiration, or better, set his thought in motion (in many ways the typical method of the autodidact). Such details may as I say be useful, but what really counts is what Leibniz actually wrote on the authors, books, and articles he cites. Of course there are cases where ideas have a signature and can be fairly accurately traced, but with the problem of continuity this is by and large not so: we are dealing here with ideas not only of a high level of generality, but also which have been around for a long time.

What is really essential in the history of ideas is that only such claims are made as can be justified, and that is to say verified in the texts available. My fear with regard to any approach which makes its main goal the discovery of sources and influences is that it might (and probably will) dispense with these standards, because it is ultimately trying to reconstruct creative intellectual processes to which we have no privileged access. While I am not denying that source studies can be done scientifically using a fairly restricted number of available texts—this is one of the central tasks of classical philology—, the danger of applying this method to the history of ideas is that there are often simply too many possibilities. And of course one possiblity in regard to Leibniz should never be neglected: that he perhaps came upon a concept independently only to find it subsequently in some way in other authors.

One of the specific criticisms Mercer and Smith make in this respect is that I have not taken sufficient account of what Leibniz's contemporaries wrote on Aristotle's conception of the continuum. Well in fact my investigations did take in a lot of

authors on Aristotle or who discuss Aristotle in certain contexts. Although this search was not exhaustive—and probably could not be—I did not find any contemporary 'Aristotelian' who interpreted Aristotle as, for example, Ockham did. Nor did I uncover anything like the detailed writings on the nature of points such as are found in some late scholastic texts. It is perhaps intersting to note that despite her many years of work on seventeenth-century Aristotelians, Christia Mercer is not actually able to provide us with examples. She and Smith write simply 'it seems likely' that the views of Thomasius on Aristotle 'would have coloured the adoring student's perception of the topics related to matter and the continuum' or that a look at Stahl's *Compendium metaphysicae* 'might reveal a version of Aristotle's theory that particularly influenced Leibniz'.

This is all very loose and hypothetical and my suspicion is that arguments for influence on this level can only work with a severe over-simplification of the causal nexus, such as I have indicated with regard to their remarks on Aristotle and the Presocratics.

Against such an approach I would suggest that we should certainly seek to bring out the context in which Leibniz developed his first philosophy and to show that this context is deeply rooted in philosophical and scientific tradition; but we should be prepared to make only cautious references to possible sources and to remain silent when circumstances demand this. In *Kontinuität und Mechanismus* I sought to employ rigorous historical standards. For this reason I desist from speculating on sources of ideas when there is insufficient evidence to support such claims or where this would involve a severe over-simplification of a complex line of tradition.

Let me end this section of my reply with a variation of the motto with which Mercer and Smith concluded their contribution to this symposium. Leibniz, we know, had an insatiable appetite for knowledge, he called himself *Mortalium docillimus*.[9] On a number of occasions he tells us how he developed his ideas: he would pick up a book, read the first few pages, then put it down again and let his own imagination run. What he did not do was to pick up a book, take a cursory glance at it, and then set about misrepresenting its contents. As I say, Leibniz did not do this, and nor should we.

Leibniz, Ockham, and Gassendi: A Reply to Richard Arthur

One of the problems I see in the approach of Mercer and Smith emerges also in the first point of criticism raised by Ric Arthur in the course of his careful review. Having outlined my presentation of Ockham's doctrine of the continuum, he

questions my unwillingness to claim little more than that we have in the position of the *Doctor invincibilis* a forerunner to the Leibnizian conception of the continuum in 1671. The comparison would, he suggests, lose its point if one simply talked in this respect of similarities rather than of arguments for influence.

As I make clear in the course of my presentation, we have to be extremely cautious in such matters. I note that Leibniz considered himself a moderate nominalist, but that we nevertheless cannot be certain to what extent he was acquainted with Ockham's and Gregor's doctrine of the continuum (of course, strictly speaking Ockham and Gregor are not nominalists, but Leibniz considered them to be). Even if he adopts what prima facie are Ockhamist positions, even if he quotes Ockham and Gregor in the context of general remarks on nominalism (together with Biel, the young Luther, and 'the Augustinians'), this by no means proves that he actually worked directly on these authors. I note (p. 241) that he certainly refers to Ockham's concept of point in the *Disputatio metaphysica de principio individui*, but this reference is only second-hand. And, as in the case of all comparisons, we should not simply look for similarities, but also for differences. At the same place I suggest that Leibniz had different reasons for regarding the continuum as actually divided than Ockham and Gregor. For Leibniz, the deciding factor would appear to be the absence of sufficient reason for limiting the division, while Ockham and Gregor assume that since the continuum is real all its parts must also be real. Their position is supported by their conception of infinity as being nothing separate from finite, permanent things, but rather involving a progression of such things.

Arthur correctly notes that Aristotle, with whose position Ockham and Gregor felt they were in agreement, can also be read as countenancing the infinite by division. As I have already mentioned in my reply to Mercer and Smith, I present the textual basis for this interpreation in the first chapter of *Kontinuität und Mechanismus*. I therefore have nor principle difficulty with Arthur's saying that it is 'conceivable that Leibniz derived his early commitment to the actual division of the continuum from reading Aristotle in this way'. But then to say something is conceivable is a long way from claiming a direct line of influence. And as I say, there is one thing we must not forget: that Leibniz at any one time was seeking to answer a specific set of questions. Even if he sometimes did get inspiration from others, the results of his labours were always his own. This is why I heartily agree with Marie Boas Hall's analysis of Leibniz's approach, which I quote in my introduction (p. 9).

The questions Ockham, Gregor, and their late scholastic contemporaries were seeking to solve—and the same is obviously true of Aristotle as well—were quite different to those of the mechanistic philosopher Leibniz. But what about his

contemporaries? Arthur recognizes correctly that there is a considerable amount of common ground between Leibniz and Hobbes, even if the former rejects the latter's extreme nominalism (as he sees it) and his resolution of mind into body. In *Kontinuität und Mechanismus* some space is devoted to showing where Leibniz and Hobbes agree and where they differ, particularly in respect to points (pp. 245-250). It might therefore seem surprising that, as Arthur writes, I do not propose that Leibniz somehow derived his concept from Hobbes. This is however only surprising until one realizes that Hobbes himself did not propose a concept of the continuum. And even if one takes his remarks on quantity, these are framed very much in the traditional Aristotelian manner: that every quantity is divisible into parts that can be divided further.[10] It is precisely Hobbes's concept of point (and conatus) that is innovative, and it is at the same time precisely his concept of point to which Leibniz seeks to provide an alternative. As Catherine Wilson writes, Leibniz was deeply intrigued by Hobbes, because he alone among the physical atomists 'had interwoven infinitistic notions into his explanations and accounts'.

Probably Arthur's most serious criticism at this juncture is that I have somehow over-emphasized the role of Ockham in respect to the young Leibniz. It is certainly true that I place Leibniz (and Hobbes) in the Ockhamist tradition, even if it is difficult to pin down the exact points of contact. I think that the evidence I provide justifies his being placed in this tradition. But what Arthur finds particularly questionable about this aspect is that on his view Ockham was only a short interlude in Leibniz's thinking on the continuum. And to that I would say that while there is no doubt that Leibniz soon departs from the idea that the continuum is actually divided into infinity and soon dispenses also with his early concept of point, some of the original assumptions remain. The most obvious is of course the actual division of matter into infinity, which clearly reflects Leibniz's early continuum concept. As I have already noted, I think this is connected up with his view that there is a mathematical core to nature. There are however also traces of the early concept of point in the concept of the infinitely small: just as Leibniz felt that points could stand in relation to each other as the larger to the smaller, so also the infinitely small later on. And we should not forget his law of continuity which allows rest to be seen as a limiting case of motion, the circle to be taken as the limiting case of the ellipse, and so on. We find Leibniz stretching concepts in this way already in the *Theoria motus abstracti* in his attempt to solve problems raised by the Skeptics (*Kontinuität und Mechanismus*, pp. 330-335). This stretching of concepts is, I maintain, very much in the spirit of the Ockhamist tradition of the continuum.

Some of the most interesting and valuable comments in Arthur's review concern

the relation between Leibniz and Gassendi. Indeed, Gassendi's view of the continuum is one of the candidates Arthur puts forward as having influenced Leibniz. To this end he quotes a number of passages from the *Animadversiones*, where Gassendi refers to the limits of the senses in regard to assessing continuity in nature or where he distinguishes continuous and discrete quantity: that the parts of the former can be separated (but are not in fact separated), whereas those of the latter are actual or really separated. As is well known, there are nominalist strands in Gassendi's thought and we can see this here. Nevertheless, Gassendi is too much of an eclectic to wholeheartedly adopt one position, and in his more specific remarks on the continuum (which Arthur surprisingly does not cite) we see clearly that he is only partly in the Ockhamist camp.

In fact Gassendi, who correctly emphasized the need to distinguish between mathematical and physical bodies when dealing with the question of divisibility[11]—this was one of his central criticisms of Sextus Empiricus—called into question the Aristotelian distinction between potential and actual infinities. On his interpretation—corresponding to the generally accepted view—Aristotle had denied that a continuous magnitude can be actually divided into infinite parts, holding rather that the parts are potentially infinite. But this, Gassendi suggests, is no solution; it also implies that the continuous magnitude is infinite. He therefore inclines in the *Animadversiones* to the position that the continuum is actually divided, but that the parts are inexhaustible—and finds support for this view in classical antiquity, namely in Archimedes' *Arenarius*.

Superficially this is similar to Leibniz, but on the other hand there is an essential difference: Gassendi rejects both the orthodox Aristotelian viewpoint and the position adopted by Ockham, that the continuum is actually divided into infinite parts. In place of infinity he puts the concept of inexhausibility.

As the long tradition of Skepticism makes clear, dogmatic philosophers like the atomists must fall back on the limits of the senses in order to justify their view. Gassendi is no exception here. His suggestion—and we should emphasize that it is such—that motion is composed of intervals of rest serves to explain differences in velocity, by removing continuity. On the other hand, according to Gassendi, continuity, be it in motion or bodies, exists only for our senses. Again, there would appear to be a similarity to Leibniz, but we should not forget that these are things which have been discussed since ancient Greece—the bent or broken stick, half-immersed in water is a good example. And what is particularly important regarding the limits of the senses in the young Leibniz is the use he makes of this in order to develop a sophisticated concept of error; a concept, which fist appears in the context

of the application of mathematics to natural science and later re-appears in the context of mathematics itself. As far as I can see there is nothing like this in Gassendi, especially since Leibniz's aim with the concept of negligible error is primarily to save the the senses: to leave no doubt how important empirical evidence is in scientific activity.

Arthur is also correct to remind us that Gassendi distinguished mathematical and physical points rather like Leibniz did. But here again, we should not forget that such distinctions were all part of the philosophical traditions with which both men were acquainted—although Leibniz better than Gassendi. In fact scholastic authors distinguished not just mathematical and physical, but also metaphysical points, and a lot more besides: *puncta inflata*, *puncta continuantia*, *puncta terminantia*, etc.[12]

Now I do not want to appear here as if I have got something against Gassendi. On the contrary, I accept that I could have taken greater consideration of his views in *Kontinuität und Mechanismus* than I in fact did. But what I am suggesting is that it is not sufficient simply to point to superficial similarities—and moreover similarities of a highly general nature—and then to claim that one has discovered a line of influence. We must, as I say, look at the differences as well, because they often tell us more.

Let me turn now to Arthur's remarks concerning Leibniz's famous letter to Thomasius of 30 April 1669. This letter quite clearly has two parts in which Leibniz also argues from two different sets of assumptions. When introducing the second part he writes '*Probandum autem est, nulla dari entia in mundo: praeter Mentem, Spatium, Materiam, Motum*'.[13] I should have thought that anyone introducing an argument like this is trying something out. And that is in fact precisely what Leibniz is doing here, although admittedly in a more concrete sense than at other times in his workshop of ideas. Quite why Arthur should oppose so vehemently my statement that Leibniz is here less interested in expounding one particular point of view than arguing the case for mechanism against the scholastic philosophy, I have to confess I do not understand.

But be that as it may, I am not convinced that placing the nature of motion at the centre of Leibniz's philosophical development in the later 1660s and early 1670s makes this development more understandable. In fact, it partly becomes less so. If, as Arthur argues, it is simply a defect of mechanism in the *Confessio naturae contra atheistas* of 1668 that matter does not have an internal source of motion, then this hardly explains why in 1669 a position should be adopted in which this defect is so to speak compounded. And if the aim of continuity explains why he adopted the position he did in the *Theoria motus abstracti* of 1671, why was this aim not a

determining factor earlier? The point I am making is that providing a theoretically successful role for God in the context of mechanism, together with the aim of producing a coherent mechanism itself, were overriding concerns for the young philosopher.

Finally, I should like to address once more the claim made by Arthur (as well as by Mercer and Smith) that Leibniz for a time in Paris embraced atomism. I think they are wrong here and that this mistake in part results from the over-interpretation of work-shop drafts. Leibniz is, I would suggest, by the time he leaves Mainz well aware of the source of atomism, namely in over-reliance on the imagination. This is some-thing which can quite naturally happen and is therefore understandable. Thus he later speaks later of the 'plausible errors of the atomists'.[14]

In Leibniz over-reliance on the imagination is very closely tied up with mathematics. It was, as he tells us in *Phoranomus*[15] his lack of insight into geometry which in his early days led him to atomism, not least in regard to motion. The mathematical papers from Paris are full of references to the need to adopt an arithmetical approach to the infinite and thus to overcome reliance on the imagination. But on the other hand he does at this time in numerous philosophical drafts refer to atoms, sometimes distinguishing—on a hypothetical basis—the atoms of physics from those of metaphysics or mathematics. And he does put forward the vortex theory, at first sight comparable to ancient atomist explanations for how similar (hard) particles come together to form larger bodies, but conceived by Leibniz to explain how a type of atom could come about in which the mind could be situated—an atom held together by motion. Was he here embracing atomism? This might at first seem the case. But what he was I my opinion really trying to do was to to solve the fundamental problem with which the *Theoria motus abstracti* and the *Hypothesis physica nova* had left him: how to integrate minds into the system. It seemed he needed physical indivisibles, but so much spoke against this.[16] Soon this model vanished as well. Paris, like Mainz, really was a remarkable workshop of ideas.

Philip Beeley
Institut für Geschichte der Naturwissenschaften
Mathematik und Technik (IGN)
Universität Hamburg
D-20146 Hamburg Germany
e-mail: fm6a021@math.uni-hamburg.de

---

[1]Konrad Moll, *Der junge Leibniz III. Eine Wissenschaft fuer ein aufgeklaertes Europa: Der Weltmechanismus dynamischer Monadenpunkte als Gegenentwurf zu den Lehren von Descartes und Hobbes* (Stuttgart-Bad Cannstatt: Frommann-Holzboog, 1996).

[2]Hubertus Busche, *Leibniz' Weg ins perspektivische Universum. Eine Harmonie im Zeitalter der Berechnung* (Hamburg: Meiner, forthcoming).

[3]See for example Richard T. W. Arthur, 'Cohesion, Division, and Harmony: Physical Aspects of Leibniz's Continuum Problem (1671-1686)', *Perspectives on Science* 6 (1997, forthcoming); Christia Mercer and R. C. Sleigh, Jr., 'Metaphysics: The Early Period to the *Discourse on Metaphysics*', in *The Cambridge Companion to Leibniz*, ed. Nicholas Jolley (Cambridge: Cambridge University Press, 1995), pp. 67-123.

[4]See for example Catherine Wilson, 'Motion, Sensation, and the Infinite: The Lasting Impression of Hobbes on Leibniz', *British Journal for the History of Philosophy* 5 (1997), pp. 339-351.

[5]Jonathan Lear, 'Aristotelian Infinity', *Proceedings of the Aristotelian Society* N.S. 80 (1979-80), pp. 187-210.

[6]Jaakko Hintikka, "Aristotelian Infinity," *Philosophical Review* 75 (1966), pp. 197-218; p. 199.

[7]Lear, p. 191.

[8]Libert Froidmont, *Labyrinthus sive de compositione continui liber unus, Philosophis, Mathematicis, Theologis utilis et iucundus*, Antwerp 1631, p. 196: '*quam (sc. contradictionem) nemo hactenus ostendit, et (licet vates non sim) ausim asserere, olim ostendet nemo*'.

[9]See the eventually discarded postscript to Leibniz's letter to Jacob Bernoulli of April 1703 (GM III, 72).

[10]Thomas Hobbes, *Opera philosophica quae latine scripsit omnia*, 5 Vol., ed. William Molesworth, London 1839-1845 (reprint: Aalen 1961), IV, p. 391: '*[...] cum omnis quantitas divisibilis sit in semper divisibilia*'. See also pp. 244 and 384.

[11]As a means to sanctioning modern mathematical work with indivisibles Gassendi distinguishes the '*regnum quam-liberrimum*' of mathematics from the material domain of physics. See Pierre Gassendi, *Syntagma philosophicum* Phys. I, 3, ¶5, *Opera Omnia*, 6 Vol., Lyon 1658 (reprint: Stuttgart-Bad Cannstatt 1964), I, p. 264. See *Kontinuität und Mechanismus*, p. 265.

[12]See my 'Points, Extension, and the Mind-Body Problem. Remarks on the

Development of Leibniz's Thought from the *Hypothesis phyica nova* to the *Système nouveau*', in *Leibniz's 'New System' (1695)*, ed. Roger S. Woolhouse, Florence 1996, pp. 15-35; p. 19.

[13]A II, 1, 21.

[14]See for example Leibniz's letter to Pierre-Daniel Huet of March 1679: '*sed et eum* (sc. Aristotle) *in physicae acroaseos libris veram continui notionem vindicasse a plausibilibus Atomistarum erroribus, quantivis pretii est*' (A II, 1, 467).

[15]*G.W. Leibniz: Phoranomus seu de potentia et legibus naturae*, ed. André Robinet, in *Physis* 28 (1991), Dial. I, pp. 429-541, Dial. II, pp. 797-885; p.803.

[16]I consider this development in more detail in 'Points, Extension, and the Mind-Body Problem' and *Kontinuität und Mechanismus* pp. 351-362.