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ARTICLE

MODELLING THE HISTORY OF IDEAS¹

Arianna Betti and Hein van den Berg

We propose a new method for the history of ideas that has none of the shortcomings so often ascribed to this approach. We call this method *the model approach to the history of ideas*. We argue that any adequately developed and implementable method to trace (dis)continuities in the history of human thought, or concept drift, will require that historians use explicit interpretive conceptual frameworks. We call these frameworks *models*. We argue that models enhance the comprehensibility of historical texts, and provide historians of ideas with a method that, unlike existing approaches, is susceptible neither to common holistic criticisms nor to Skinner's objections that the history of ideas yields arbitrary and biased reconstructions. To illustrate our proposal, we discuss the so-called Classical Model of Science and draw upon work in computer science and cognitive psychology.

KEYWORDS: methodology of the history of philosophy; history of ideas; models; concept drift; schemata; history of scientific methodology; Lovejoy; Kuukkanen

INTRODUCTION

The history of ideas has long been a discipline in disrepute. It has been criticized as involving an improper method that does not provide a proper understanding of historical texts. The aim of this paper is to propose a new method for the history of ideas that has none of the shortcomings so often ascribed to this approach. We call this method *the model approach to the history of ideas*. We maintain that any adequately developed and implementable method to trace (dis)continuities in the history of human thought (or *concept drift*) will require that historians use explicit interpretive conceptual frameworks. We call these frameworks *models*.

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Our proposal blocks holistic objections as well as Skinner's criticism that the history of ideas is a biased account that yields arbitrary and biased narratives. The novelties of our approach are fourfold. First, we construe ideas as (parts of) models. As a result, the ideas with which we can properly work will tend to be (highly) complex. Second, we counter holistic objections by adopting a neutral stance with respect to the ontological nature of ideas and by arguing that historians of ideas can trace continuities without assuming a notion of (strict) identity between ideas. Third, we counter Skinner's objection that the history of ideas yields arbitrary narratives by arguing that the results of applying interpretive models in the history of ideas are empirical hypotheses. We counter Skinner's objection that using models in the history of ideas results in biased accounts by arguing that the only defence against biases is to make them *explicit*. We submit that our use of *explicit* models is thus the best methodological defence against the risk of interpretive biases and support our findings with work in cognitive psychology. Our approach is more general than existing proposals because it allows us to detect continuity even in case of ideas that share no common features.

The paper is structured as follows. The second section examines criticisms of Lovejoy's method of the history of ideas, while the third section examines existing defences, in particular Kuukkanen's recently developed core/margin approach. The fourth section introduces our own proposed method for improving existing defences by way of an employment of explicit interpretive models. While adopting Kuukkanen's distinction between the core and margin of an idea or concept, we argue that any truly implementable method in the history of ideas will require that this core/margin vocabulary be applied to highly complex ideas conceived as (parts of) models. We also submit that not only the intension but also the extension of concepts must be considered when studying concept drift. In the fifth section, we illustrate our approach by examining an especially clear example of a model, the Classical Model of Science (CMS). In the sixth section, we show how the use of models as interpretive frameworks counters Skinner's biases objection by arguing that models function like cognitive schemata, in the sense of the schema theory of knowledge.

In the paper, we speak interchangeably of 'ideas' and 'concepts'. We remain neutral on a number of philosophical theories of concepts. For our purposes, it is sufficient that concepts are (expressible in language by) (categorical) terms, and that they are compositional; that is, if complex, they are composed of subconcepts. These latter, subconcepts, we sometimes call 'features', a term which, again, can be interpreted simply to mean (occurrences of meaningful) expressions.

LOVEJOY'S HISTORY OF IDEAS: OBJECTIONS

Lovejoy characterized the history of ideas as being concerned with *unit-ideas* that emerge throughout history. The historian must isolate certain

unit-ideas and trace their history in various contexts (Lovejoy, *Great Chain of Being*, 3–7, 15). Thus, for example, historians may isolate the idea of evolution and go on to trace the history of this idea in various periods and intellectual settings, and including different disciplines (in biology, then, but also astronomy, geology, literature, philosophy, and theology) (Lovejoy, ‘Historiography of Ideas’, 541–2). In this way, Lovejoy argues, we gain a unified perspective on a mass of historical facts and so render them intelligible (*ibid.*, 538–9).

It is well known that Lovejoy’s method has been subject to a number of criticisms, mainly directed against the notion of a unit-idea. Lovejoy construed unit-ideas as static entities that remain identical through time, although they enter into various thought-complexes. Consider the unit-idea *chosen people*. Lovejoy took it to be expressed by a propositional function ‘ $x(s)$ are the chosen people’. This idea has been entertained in many periods, yet was combined with ‘different assumptions as to what people x should stand for’ (Lovejoy, ‘Reply to Professor Spitzer’, 210). At different times, different values have been assigned to x , for example, the Jewish people, and thus different thought-complexes (wholes) have emerged. The unit-idea itself, however, in Lovejoy’s view, remained unchanged. Critics of Lovejoy have denied the existence of unchanging unit-ideas. They deny, in particular, that ideas or concepts retain their meaning over time and across contexts.

This kind of criticism has generally been taken to undermine the credibility of Lovejoy’s methodology for the history of ideas as a proper historical method (Wilson, ‘Lovejoy’s *The Great Chain of Being*’). In the rest of this section, we offer a systematic review of well-known arguments against Lovejoy’s method, and in the following section, review some common defences. Defenders of Lovejoy argue that the criticisms of his view do not discredit his methodology or the history of ideas more generally. Though we agree that the arguments are inconclusive, we think that their inconclusiveness has yet to be convincingly demonstrated. This applies in particular to the holist critique under (a) below, and to Skinner’s objections under (d). Because of this, we argue, we still lack an adequately developed methodology of the history of ideas; more work is needed to establish that history of ideas is a proper method. In the fourth section, we undertake this work.

Let us now consider the arguments against Lovejoy.

(a) *Holism*. The existence of unit-ideas is incompatible with *holism*. According to holism, a concept is fully determined by its relation to thought-complexes (wholes) (Spitzer, ‘Geistesgeschichte’; cf. Lovejoy, ‘Reply to Professor Spitzer’, 206–7). Consider, following Hintikka, Lovejoy’s prime example of a unit-idea: the principle of plenitude, that is (the idea) that every genuine possibility is eventually actualized. The meaning of this principle, says Hintikka, differs in different theoretical contexts (e.g. in Aquinas, Descartes, and Leibniz). If this is so, then plenitude is

not a *unit-idea*. It is a *conglomerate* of ideas, that is, many ideas, all different, and not one and the same idea. Given that Lovejoy's prime example of a unit-idea fails to be one, Hintikka continues, we can doubt the existence of unit-ideas altogether (Hintikka, 'Gaps', 26–8, 34). Put in general terms, for holists concept *a* in context or concept constellation *abc* does not have the same meaning as concept *a* in context *ade* (it is, indeed, *a-in-abc*, not *a-in-ade*). No part of these constellations remains unchanged; therefore, there is nothing like a unit-idea that remains identical.

(b) *Conceptual change*. If Lovejoy's unit-ideas are simple elements that do not undergo change, it is not clear how we could write a *history* of such ideas (Mink, 'Change and Causality', 9–14).

(c) *Scope*. Defining the scope of unit-ideas is problematic. Suppose we want to write the history of the idea of evolution in the nineteenth century following Lovejoy's approach. We would then need to specify features of this idea that enable us to attribute it to, say, Lamarck, as well as to Wallace and Darwin. But this raises a dilemma (Kuukkanen, 'Conceptual Change', 355–6). A narrowly Darwinian definition of evolution cannot properly be attributed to other historical actors or authors. A too broad definition of evolution, on the other hand, would overlook important differences among these authors (Bevir, *History of Ideas*, 203).

(d) *Arbitrariness and biases*. Skinner judged Lovejoy's methodology both (i) arbitrary, because it does not yield a proper understanding of historical texts, and (ii) biased, because it necessarily relies upon preconceived models in interpreting these texts. *Ad* (i): understanding the statements of authors requires insight into their *intentions*; yet studying unit-ideas does not provide such an understanding, for it abstracts from authors' intentions (Skinner, *Visions of Politics*, 79–86). If we do the latter, as Lovejoy allegedly does, we also cannot explain the *relevance* of certain ideas for certain authors in certain historical periods (*ibid.*; cf. Mendelbaum, 'History of Ideas', 37). *Ad* (ii): Skinner notes that by our 'mental set' we are 'set to perceive details in a certain way'. This, according to Skinner, is *not* conducive to the writing of any proper history. Insofar as we 'classify the unfamiliar in terms of the familiar', we run the danger of attributing some meaning to a historical text that the author could not have intended to convey (Skinner, *Visions of Politics*, 58–9).

LOVEJOY'S HISTORY OF IDEAS: REPLIES

Existing counterarguments to (a)–(d) go as follows.

Against (a) *Holism*. Recall Hintikka's point concerning the principle of plenitude: it is a conglomerate of many, different ideas, not a single, unchanging idea continuous over time. There are two standard responses to this holistic critique: (i) Gram and Martin's family resemblance account and (ii) Lovejoy's absurd consequence rebuttal.

(i) *Gram and Martin's family resemblance rebuttal*. According to Gram and Martin, unit-ideas can be saved by appeal to a notion of *family resemblance* ('Perils of Plenitude', 509–10). We classify, for example, the principle of plenitude as *one* idea because of a family resemblance among its various instances in different contexts. This account is considered unsatisfactory by some of Lovejoy's defenders because of the so-called problem of *wide-open texture*. Family resemblance, it is said, captures too much, and arbitrarily so, for there is always *some* resemblance among a concept *a*, and *any other* concept *b*, such that *a* and *b* may very well share no features at all. If *a* and *b* have no common features, we have no reason to treat them as *one* concept unified by family resemblance rather than *two* concepts (Kuukkanen, 'Conceptual Change', 365–6, building upon Anderson, 'Kuhn's Account').

(ii) *Lovejoy's absurd consequence rebuttal*. Lovejoy, followed by Kuukkanen, holds that the notion of unit-ideas can be retained because holism has absurd consequences. According to Lovejoy, the communicability of ideas requires that different minds *share* (at least in some cases) ideas, that is, that these ideas are held *in common* ('Reply to Professor Spitzer', 207). Now, according to Lovejoy, holism entails that no two thinkers can have the *same* idea, which is an absurd claim, for it implies that ideas are incommunicable; therefore, holism is false. Kuukkanen ('Conceptual Change', 360–2) adds that the very possibility of writing history requires that historians think of concepts employed by historical actors as instances of more general concepts *shared* by them.

Is this rebuttal against holism effective, and is it preferable to the family resemblance account? We do not think so. We claim in the fifth section that tracing continuities in the history of ideas does not require assuming a notion of (strict) identity between ideas; in particular, continuity can be detected even in case of ideas sharing no common features.

Against (b) Conceptual Change. This objection is usually met by giving up the simplicity of ideas, and thus arguing that ideas can change because they are *complex*: they have parts, and what changes are their parts. Although Lovejoy sometimes describes his discipline as the study of *simple* unit-ideas, many theorists have denied that the history of ideas requires assuming that unit-ideas are simple (Bredsdorff, 'Lovejoy's Idea of 'Idea''; Kvastad, 'On Method', 99). We agree: the ideas of which we can properly do history are in fact bound to be complex. Yet now a second problem arises: how is it possible to say that the *parts* of a (complex) idea can change, while the idea itself remains *the same*? Note that this objection differs from the holist's, though both point to the same general difficulty (what is many and different cannot be one and the same). Holists maintain that idea *a* in complex *abc* cannot be identical to idea *a* in complex *ade*; the present objection maintains that idea *a* composed of subparts $a_1a_2a_3$ cannot be the same as idea *a* composed of subparts $a_1a_2a_4$.

This problem can be solved, as Kuukkanen points out, by distinguishing between the *core* and *margin* of a concept. The *identity* of concepts is

defined in terms of their core, which is taken to comprise the concept's stable and context-unspecific features, that is, features that persist over some amount of time (Kuukkanen, 'Conceptual Change', 367). The margin of a concept comprises rather its variable and context-specific features. For example, Kuukkanen defines the core of the concept of *element* in terms of the subconcepts of a body that is (1) material and (2) indecomposable. Call the conjunction of (1) and (2) the *core* of the idea of element, and mark it with a_1a_2 . If historical actors accept a_1a_2 (that is, if they think of *element* as a material and indecomposable body) they can be said to accept the concept of element. The margin of the idea of *element* comprises certain subideas such as: (a_3) found in all (other) bodies, (a_4) responsible for observable qualities of (other) bodies, etc. (ibid., 367–8).

The core/margin distinction allows Kuukkanen to give a nice account of *conceptual stability*, *conceptual change*, and *conceptual replacement* (ibid., 368–71). In the case of conceptual stability, neither the core nor the margin of a concept changes (in our schematic terms, $a_1a_2a_3a_4$ is present in more than one author at more than one time). In the case of conceptual change, the margin of a concept changes, whereas the core remains the same (you might have $a_1a_2a_3$ at time t_1 , and $a_1a_2a_4$ at time t_2). Finally, if the core of a concept changes, we can speak of conceptual replacement (you have a_1a_2 at time t_1 , say, but at t_2 there is only a_1). In cases of conceptual stability, historical actors accept *identical* concepts; in cases of conceptual change, they accept *varieties* of concepts; in cases of conceptual replacement, *different* concepts. In this way, we can identify *continuities* throughout history by referring to the stability of the (same) core in different varieties of a concept, and *discontinuities* by referring to the varying margins of these varieties.

Against (c) Scope. Kuukkanen's solution to (b) is also a solution to (c). Recall that the alleged problem is that we risk defining an idea too broadly if we define it in such a way that it encompasses the views of several different authors, and too narrowly if we try to define it in such a way that it encompasses the view of only one particular author. On Kuukkanen's account we can do both at the same time: we can specify a set of features of a concept (core) that is shared by a variety of thinkers. We can then *contextualize* this concept by describing its varying margins in different authors. In this manner, we can identify concepts adopted by various authors (by referring to its core) and highlight the peculiarities of an author's use of a concept (by referring to its margins).

Against (d) arbitrariness and biases. As we have seen, Skinner rejects Lovejoy's method because, he thinks, (i) it does not allow us to understand the meaning and relevance of historical texts, for it abstracts from the study of authors' intentions; (ii) it is necessarily biased. One might find this criticism not entirely fair, for Lovejoy himself argues that historians should take an author's intentions into account while performing careful empirical and textual research, and this entails placing texts in their historical context (Lovejoy, 'Reflections', 13–14; Wilson, 'Lovejoy's *The Great Chain of*

Being', 202; Knight, 'Unit-Ideas Unleashed', 213). Yet the question is, of course, how we should *do* this concretely. There is no full specification of the method of the history of ideas that shows Skinner's worry to be unjustified. And if *this* is the objection, then it has not yet been countered adequately.

Let us take stock. There is still work to be done to provide an adequate defence of Lovejoy's methodology. Two points, (a) and (d), have not yet been adequately countered. In particular, Kuukkanen's successful rebuttal of objections (b) and (c) cannot be also used to address (a) and (d). For Kuukkanen's core/margin distinction, however correct, gives us only a *vocabulary* of conceptual change. It does not tell us how the general idea should be *implemented*. Once we counter (b) and (c) by saying that there is a core to a concept, and it is *that core* that shows continuity through time, how do we identify this core in such a way that, against Skinner, it avoids arbitrary and biased narratives (d), and in such a way, against the holists, that the continuity of such cores can be adequately accounted for (a)?

In the following three sections, we develop an implementable methodology for the history of ideas that accounts adequately for both holist objection (a) and Skinner's objection (d), in part by improving on Kuukkanen's core/margin distinction *ad* (b) and (c).

As to (a), we take a middle position, so to speak, between holists and their opponents: *with the holist*, we say that it is trivially correct to say that ideas are different in different authors, for ideas do change; yet, *against the holist*, we say that this observation does not jeopardize the method of the history of ideas, for in case of continuity, we say, there is *something* that *remains constant* that we can trace. But, *against Lovejoy and Kuukkanen*, we think there is no need to construe 'something remaining constant' as (strict) *identity among ideas*. We maintain in particular that in certain cases, the *something* that *remains constant* across contexts is not (strict) identity among ideas, but structural isomorphism among (specific embodiments of parts of) conceptual models. This means that our proposal enables us to detect continuities in some cases of conceptual replacement as well (cases where both the core and margin of a concept change) because sometimes the continuity does not even regard ideas, but the *place, role, or function* that a certain idea occupies in the context of certain conceptual structures.

As to Skinner's objections *ad* (d), we counter them by arguing that interpretive models must do historical justice to the manner in which authors construe concepts, and that the use of *explicit* interpretative models is the best defence against the risk of interpretative bias.

THE MODEL APPROACH TO HISTORY OF IDEAS

In this and the following two sections, we propose a method for the history of ideas or *concept drift* (conceptual change and replacement) based on what we call *models*. In our model approach ideas or concepts are construed as

(parts of) models, that is, complex conceptual frameworks. The ideas we can do history of, we maintain, are (highly) complex.²

We proceed as follows. In this section, we deal with the *first* of Skinner's objections under (d), namely the arbitrariness objection. We argue that concept cores must be singled out in a way that reflects the manner in which authors construe that concept; we also claim, with the support of work in computer science, that historians must consider not only how the authors they study construed a concept's *intension* (core and margin), as Kuukkanen indicates, but also how they construed its *extension*. In the fifth section, we explain how the method of history of ideas can best be implemented by using explicit and clearly designed interpretive models. In the fifth section, we also consider the holist's critique (a). For the time being, we disregard (a), and take for granted the legitimacy of the claim that concepts or ideas retain their identities through time. In the sixth section, we deal with the *second* of Skinner's objections under (d), namely the biases objection.

Versus Skinner's Arbitrariness Objection (d): Historians Must Commit to the View that Concept Cores are *relevant* for the Authors They Study

Skinner's arbitrariness objection at (d) can be countered as follows. In identifying the core of a concept, we may include *only* features that, next to being ascribed to the concept by historical actors themselves, are also *relevant* features of the concept for the authors in question. Historians must commit to affirm that what they identify as the core of the concept is also *significant* for the authors they study. This, we submit, prevents us from foisting arbitrary narratives upon the authors we study – at least *in principle*, from the methodological point of view. The point is that we must be able to be proven *wrong*: the historian must commit to an objective reconstruction. In singling out the core of a concept, we generate empirical hypotheses, albeit hypotheses necessarily constrained by the historian's interest. In our approach, historians of ideas attempt thus to provide realistic (objectively constrained) insightful interpretations. Importantly, our position does *not* require that the historical actors literally and explicitly *say* they hold a certain concept *x* with such and such significant features. Historical actors may hold such concept cores only implicitly. In the latter case, it is for the historian to make concept cores explicit and provide non-literal evidence that their authors themselves actually did hold those concept cores in the same way. This can be done, for instance, by inferential means, that is, by

²An anonymous reviewer has pointed us to Gracia's valuable 'framework approach' for the history of philosophy (*Philosophy and Its History*, 279–88). A comparison between Gracia's and our approach would deserve a separate paper; here we just note that Gracia's frameworks, which include formulations of philosophical problems and criteria for evaluating philosophical arguments, are more general than our models, which are highly specific conceptual frameworks designed explicitly to study *concept drift*.

employing cogent arguments. We return to the notion of relevance in the following section and consider the explicitness constraint again in the sixth section.

The position we have just outlined seems to be in part shared by Kuukkanen himself. However, Kuukkanen argues that the *main criterion* for identifying concept cores as such selected features is the research interest of the historian (Kuukkanen, ‘Conceptual Change’, 367–9, 371), and the idea of historians *choosing* a concept core *according to their interest* leads Kuukkanen to describe his own position as a form of conventionalism. Were we to take seriously Kuukkanen’s talk of conventionalism, we would not be able to counter Skinner’s arbitrariness objection because a conventionalist position on selecting concept cores would not prevent us from foisting arbitrary narratives upon our actors. For if, in identifying the core of a concept, the main criterion is our research interest, we may end up disregarding the features of a concept that historical actors themselves deemed most important – and be methodologically legitimated in doing so.

Extensional Shifts Must Also be Part of Studying Concept Drift

In line with a standard technical position in philosophy, Kuukkanen identifies the meaning of a concept with its *intension*, which in turn he identifies with its core and margin. However, related research in computer science and information technology, where the topic of concept drift is hotly debated, suggests that the *extension* of concepts is also relevant to historians’ practice. In a recent paper, Wang, Schlobach, and Klein (‘Concept Drift’) provide an account of conceptual change which is almost identical to the one proposed by Kuukkanen, yet differs from Kuukkanen’s in approaching concept drift in terms of intension *and* extension. The reason is that changes in the extension of a concept are often indications of changes in its intension; the former, that is, are often helpful in detecting the latter. Consider the concept of an EU country, that is, a country belonging to the European Union. The extension of this concept differs through time. When confronted with certain changes in extension in the data, one likely conjecture is that the *meaning* of the concept of an EU country has changed. Wang, Schlobach, and Klein is of great value for the working historian because historians often reason to shifts in the meanings of concepts by detecting shifts in their extensions. Think of your texts as data: the data will often show *only* shifts in concept extensions. By regarding this as (often) evidential of a shift in concept intensions, you can formulate an initial hypothesis of concept drift. And if we directly hypothesize intensional shifts, we can then check our hypotheses by identifying extension shifts.

Consider the following example. Historians have noted that Kant did not apply the concept of life to plants, though he did apply it to animals and humans. Yet for Kant, humans, animals, and plants are all organisms; therefore, the concepts of *life* and *organism* in Kant are not co-extensive (Zammito, ‘Teleology Then and Now’, 762–3). The reason for this lies in their different

intensions: according to Kant, life is the capacity to move oneself, while organisms are objects in which every part is both an end and a means. Unlike Kant, many other philosophers and scientists at the end of the eighteenth century did apply the concept of life to plants, though their construal of organism was similar to Kant's. Alexander von Humboldt, for example, described *life* as a physico-electrical force that maintains bodies in their original form (Richards, *Romantic Conception*, 316–21). This phenomenon indicates a complex shift in the intension of the concept of life, which can be recognized by studying the change in its extension. If we wish to describe the history of the concept of life from Kant onwards, we can thus first reconstruct the intension and extension of Kant's concept of life, and subsequently employ this account to study theorists after Kant, highlighting continuities and discontinuities in the meaning (intension) and application (extension) of this concept.

We conclude that if historians wish to study concept drift, they need to specify how the authors they study construe both the intension of the concept in question (its core and margin, or meaning) *and* its extension (reference). A reliable method in the history of ideas should incorporate this consideration explicitly.

Let us sum up. The method we propose requires that historians of ideas (1) distinguish between the core and margin of a concept, identifying the core by selecting significant features of the concept in question; (2) specify the extension of concepts, and thus study concept drift by examining shifts in both intension and extension. The question that now arises is this: how can we effectively accomplish (1)–(2)?

One important thing to realize is that in order to do this we must find a way to represent many (shifting) relations among the features (subconcepts) of complex concepts, which will often form a network. In our view, this is best accomplished by using *models*. We take models here to be explicitly stated conceptual frameworks developed intentionally to achieve insightful interpretive goals. Models are abstract relational structures or networks of (sub)concepts including, crucially, determinables.

In the next section, we illustrate our proposal in detail by presenting a model of the complex concept of *proper science*, used by scholars within the history of philosophy and science. We will show in what sense the history of ideas can be properly conducted by using models such as these, that is, conducted in such a way as to answer the holists' and Skinner's objections at (a) and (d).

MODELS, BY EXAMPLE

The Classical Model of Science

We will illustrate our model approach to the history of ideas by discussing a model recently applied in the history of philosophy and of science: de Jong

and Betti's Classical Model of Science (CMS). This model has been developed by improving on previous systematizations of Aristotle's theory of science (Scholz, 'Die Axiomatik'; Beth, *Foundations*, 31–2), and set up with the aim of specifying in an interpretive framework a historically influential ideal of axiomatic science. This ideal is reconstructed on the basis of Aristotle's *Analytica posteriora*, *The Logic of Port Royal*, and Bolzano's *Wissenschaftslehre* among other important philosophical works (de Jong and Betti, 'Classical Model').

The CMS is articulated in seven conditions that capture the concept of proper science according to a certain ideal. These conditions, for any system *S* of propositions and concepts (terms), are as follows:

- (1) All propositions and all concepts (or terms) of *S* concern a *specific set of objects* or are about a *certain domain of being(s)*.
- (2a) There are in *S* a number of so-called *fundamental concepts* (or terms).
- (2b) All other concepts (or terms) occurring in *S* are *composed of* (or are *definable from*) these fundamental concepts (or terms).
- (3a) There are in *S* a number of so-called *fundamental propositions*.
- (3b) All other propositions of *S* *follow from* or *are grounded in* (or *are provable* or *demonstrable from*) these fundamental propositions.
- (4) All propositions of *S* are *true*.
- (5) All propositions of *S* are *universal* and *necessary* in some sense or another.
- (6) All propositions of *S* are *known to be true*. A non-fundamental proposition is known to be true through its *proof* in *S*.
- (7) All concepts or terms of *S* are *adequately known*. A non-fundamental concept is adequately known through its composition (or definition). (de Jong and Betti, 'Classical Model', 186).

These conditions are highly abstract. Together they are meant to capture the basics of an axiomatic view of science that was held by multiple thinkers in various periods. In the core/margin terminology laid out in the previous sections, conditions (1)–(7) fix the *core* of the concept of proper science according to the axiomatic ideal. As is clear, the concept of proper science articulated here is also highly complex: its core is described as a relational structure of seven conditions containing, in turn, many (sub)concepts.³

If (1–7) are the core, what are the *margins* of the concept of proper science? To see this, keep in mind that, as noted, conditions (1–7) are highly abstract. First and foremost, the sense in which (1–7) are abstract is

³Mind that de Jong and Betti do not claim that theirs is a normative model of what science should be. They merely claim descriptively that the above conditions offer an articulation of what some philosophers and scientists throughout history have themselves regarded as the ideal characteristics of a proper science (the CMS is a 'description of a norm', cf. Betti, 'On Haslanger's').

that they are general, due to the fact that the (sub) concepts they contain are *determinables*. This is for instance the case of the subconcept *proposition* in conditions (1), (3), and (4–6). We will have more to say about this determinable later on. In a second sense, conditions (1–7) are abstract for having been *abstracted* or being *made* general (to be precise, set up via a combination of abstraction and abduction) taking a cue from, among others, influential views on science found in Aristotle, *The Logic of Port Royal*, and Bolzano. Now, as de Jong and Betti point out, these general conditions (1–7) can be *specified* in different manners, that is, the determinables contained in conditions (1–7) might be determined – embodied, instantiated, encoded – by different authors in different ways (‘Classical Model’, 189–93). These specific determinations give us the margin of the concept of proper science.

In terms of the core/margin terminology, de Jong and Betti’s claim is that whereas many thinkers adhered to the core of the concept of proper science as fixed by (1–7), this concept may differ in different thinkers in its margins, that is, authors may apply or interpret those conditions differently. By specifying the margins of the concept of proper science, we thus indicate its variable features. And thus, in applying the Model to individual thinkers by specifying conditions (1–7), we are able to *contextualize* the model. In this way, we can maintain that various authors, say Bolzano and Kant, have adhered to *the same* conception of proper science and still allow for variation. For if we were to specify Kant’s ideal conception of proper science, it would be *wrong* to say that he thinks it requires fundamental *propositions*, but *right* to say that he thinks it requires some *other* fundamental items, namely *judgements*. In other words, the (sub)concept of some item x that is *fundamental* (3a), from which other items $x_1 \dots x_n$ *follow* and which is such that it can be said to be *true*, belongs to the core of this concept of proper science, that is, axiomatic science. Item x can be indicated more generally, using the terminology of analytic metaphysics and philosophy of language, as a *truthbearer*: being a truthbearer is a *role* that can be played by different items, among which one may count Kant’s judgements, Bolzano’s propositions in themselves, or Frege’s thoughts. There is no axiomatic science without axioms (core), but there is still axiomatic science when the axioms are determined as propositions rather than judgements or thoughts (margins). To make this clearer, let us consider the model in its most stripped down version, one that reveals only its core:

- (1) S has items x_s and items y_s that are **about** a .
- (2a) Some y_s in S are **fundamental** (call them Fy_s).
- (2b) All non-fundamental y_s in S are **composed or definable** from Fy_s .
- (3a) Some x_s in S are **fundamental** (call them Fx_s).
- (3b) All non-fundamental x_s in S **follow from** [are **grounded in** or are **provable** or **demonstrable from**] Fx_s .
- (4) All x_s in S are **true**.

- (5) All x s in S are **universal and necessary in some sense**.
- (6) All x s in S are **known to be true**. Non-fundamental x s are **known to be true through their proof or demonstration or grounding proof** in S .
- (7) All y s in S are **adequately known**. Non-fundamental y s are **adequately known through their composition or definition**.

In bold you see features of the core, which is stable, while the variables x , y are place-holders for determinables. Note that the determinables x , y do not admit just *anything* as substitutes: for instance, x can only stand for (expressions referring to) something that can be *true* (4), is known to be *true* (6), etc., while y can only stand for something that can be *defined*, etc. Consider now condition (3b). Condition (3b) captures the axiomatic ordering of propositions, judgements or any other truthbearing item in a science. It requires that non-fundamental items *follow from* fundamental ones (core), but this concept of *following from* can have a strong construal (margin) as *explanative demonstration or grounding*, which makes – by determination – *following from* a notion corresponding to the traditional Aristotelian notion of a scientific syllogism or *demonstratio propter quid* (de Jong and Betti, ‘Classical Model’, 190). One clear example of this strong interpretation in modern times is Bolzano, though this strong construal has been abandoned by later authors. Note that the latter claim, that is, that Bolzano had a strong interpretation of (3b) and that this construal was later abandoned, is, in keeping with what we said in the fourth section, a (complex) empirical research hypothesis. The CMS allows us to test this hypothesis in the following way. By using this model in their interpretations, historians can make explicit, first, that 3b belongs to the core of the notion of proper science they are considering. They might then hypothesize that specific actors, for example, Bolzano and some later authors, thought of proper science as an axiomatic ideal (continuity, core). Second, since 3b is general, it allows for both stronger and weaker construals. Our historians can, therefore, put forth the hypothesis that, within the *same* conception of axiomatic science, Bolzano and later authors have *different* construals of the same notion of *following from* (which is a discontinuity, and regards the margins). This same procedure is adopted for all conditions.

Recall now our reply to Skinner’s arbitrariness objection *ad* (d), that is, that in identifying the core of a concept, we may include *only* features that, in addition to being acknowledged as belonging to the concept by the historical actors themselves, are also *relevant* for them. By applying the Model to reconstruct historical actors’ views of science, we indeed commit ourselves to the following four claims (two about the concept’s core, one about its margins, and one about neither).

First (core), we claim that the concept of science according to (1)–(7) is accepted by these historical actors; that is, it is construed by them in the manner indicated by the bold (stable) part of these conditions.

Second (core), we claim that no *other* condition plays a more or equally significant role in shaping the notion of proper science; that is, that there is no eighth feature of the concept of proper science, missing from our list, that is at least equally relevant to the thought or philosophical system of our authors. The conditions listed, then, are both necessary and sufficient. This is a matter of empirical research: hence (1)–(7) allows us to formulate empirical hypotheses concerning what philosophers and scientists themselves took science to be.

Third (margins), we claim that when we describe the way in which determinables get determined in the thought of certain authors, we describe the author's intentions; what they really meant.⁴

While the first claim accords easily with Kuukkanen's account, the last two claims are novel. We maintain that the addition of these last two claims serves to address the first of Skinner's criticisms at (d), namely arbitrariness. For although the systematization of *this* particular complex concept (proper science) via the CMS, in particular via the core captured by the determinables at (1–7), does represent a particular historian's choice, this choice is not arbitrary. De Jong and Betti developed the Model because they regarded this particular view of ideal axiomatic science as interesting and highly useful in framing insightful historical accounts of deep and long-standing issues in the history of philosophy (not only in the history of science or in the philosophy of science). This concept of science, they submit, has been dominant for two millennia, and been adopted by a wide variety of thinkers such as Spinoza, Wolff, Kant, and later Bolzano, Husserl, and Frege. But it is an historical claim, and an explicit and revisable hypothesis that authors did adhere to (a certain determination of) the CMS.

Fourth, by using the CMS to do the history of ideas, we claim that certain (sub)ideas occupy a certain *place* in the conceptual relations established by this model. The important advantage of this over Kuukkanen's original core/margin approach is that the latter cannot account for conceptual continuities through history unless they regard either the core or the margins of a concept. Our model approach enables us to do more, insofar as the use of models allows us to trace the continuity of ideas not only with regard to their core/margin but also with regard to the structural *place* that a certain concept occupies in a model. The model approach thereby affords a more realistic and general methodology than is available from any other existing account.

Like the second and the third, this fourth point is also novel and important to counter holistic objections at (a). We will substantiate it by considering a

⁴It is sometimes argued that historians cannot know the intentions of authors. Although a discussion of this issue would exceed the scope of this paper, we wish to stress two points. First, *if* the intentions of an author cannot be known, Skinner's claim that Lovejoy's methodology results in arbitrary narratives is unjustified. Second, *even if* we cannot know the intentions of an author, this would not invalidate our approach. For we can then still use models to make explicit our interpretations and to reconstruct how concepts are used in historical texts, as our discussion of the CMS elucidates.

particular application of models in the history of ideas: de Jong's use of the CMS to study *analyticity* in Kant and Bolzano (de Jong, 'Bernard Bolzano'; de Jong, 'Analytic-Synthetic Distinction').

Kant was the first to apply the analytic–synthetic distinction to statements (which Kant called 'judgements'). Before Kant, this distinction had been applied only to *methods*. In his *Wissenschaftslehre*, Bolzano credits Kant for recognizing the applicability of this distinction to *statements*, which Bolzano called 'propositions' (Bolzano, *Wissenschaftslehre*, 83). Given this, one would perhaps expect to find some continuity between Kant's and Bolzano's views on this topic. Yet this is hardly the case. Kant and Bolzano in fact construe the concept of analyticity in quite different ways. Recognizing this, the historian of ideas will want to know why this is so: why did Bolzano's modify Kant's account of analyticity?

To explain Bolzano's motives, de Jong employs the CMS ('Bernard Bolzano', 329–33, 340–9). Both Kant and Bolzano, de Jong argues, (I) accept (1)–(7) in some determination, and both, crucially, hold that (II) analytic statements play only a marginal role in a science. Bolzano, de Jong continues, has a strong take on (3b): scientific proofs should be explanatory; that is, the relation of *following from* between fundamental (axioms) and non-fundamental (theorems) statements of a science is such that theorems are grounded in axioms on the basis of *explanatory proofs*. He thinks that sciences are ordered by explanatory proofs in such a way that some statements (grounds) are more *general* than other statements (consequences), and takes this idea to suggest that in a science some analytic propositions *should be grounded by and derived from* more general synthetic propositions pertaining to that science (*ibid.*, 345–9). This fits nicely with (II) above, that is, the view that analytic statements play only a marginal role in any science, because in this way, they turn out to be somewhat trivial consequences of synthetic propositions. For example, Bolzano regards the more general *synthetic* statement (A), 'the angles of a triangle are together equal to two right angles', as a *ground* for the more particular *analytic* statement (B), 'the angles of *this* particular triangle are together equal to two right angles'. It is here that the differences between Kant's and Bolzano's views on analyticity become clear. For whereas Bolzano takes (A) to be synthetic and (B) to be analytic, Kant takes both (A) and (B) to be synthetic. In addition, Kant thought that mathematical statements had to be proven by means of what he called a constructive procedure in pure intuition. This procedure was not recognized by Bolzano as a scientific demonstration because in such a procedure we prove more general mathematical statements (A) on the basis of more particular statements (B). According to Bolzano, then, Kant did not understand the order of proof proper to mathematics (i.e. that one should move from the more general to the more particular) (*ibid.*).

In short, by applying the CMS, de Jong manages to reconstruct the relations among the concepts of *analyticity*, *grounding* or *scientific explanation*, and *generality*. Now note, in connection with the fourth point

above, that while *grounding* (as a strong take on condition (3b)) belongs to the core of the concept of science as presented by the CMS, *analyticity* does not: it is a (sub)concept belonging neither to the core of the concept nor to its margins. But Bolzano's views on analyticity, conceived as a critique of Kant's view of the order of proof within mathematics, are understandable only in light of a reconstruction of the manifold of relations between the (sub)concept of analyticity and the concepts belonging to the core of the notion of proper science as systematized by (1–7) as well as other concepts. By applying the CMS, de Jong brings Bolzano's views more clearly into focus. This shows that the model approach allows us to study concept drift in a manner at once more realistic (first three points above) and general (fourth point) than afforded by other existing approaches.

We have now seen in what sense the method of the history of ideas can be implemented by using models. Two points remain to be considered: the holists' objection, and Skinner's biases objection.

Against (a): Unit-Ideas Need Not Be One in Number and Holism Need Not Be False

Our method, though fundamentally in line with Lovejoy's, differs from his on two important points. First, we do not think that we must hold that holism is false to do the history of ideas legitimately because we do not think that we must assume that the ideas we are working with retain their identity through time. Second, we think there is no reason to regard the ideas we are working with as simple. Quite the contrary, we think that these ideas will often be highly complex.

Let us examine the first of these points. What would a holist say to the claim that (1)–(7) capture a *shared* core of the concept of proper science, enabling us to detect continuities? A holist can simply point to the fact that Kant, Frege, and Bolzano all interpreted the axioms of a science to be different objects, namely judgements, propositions, and thoughts, and so conclude that they held *different* concepts of axioms, and thus too of science. This observation is correct: strictly speaking the holist is right. Yet we do not think the historian of ideas should be troubled by this. That is, contrary to Lovejoy and Kuukkanen, we think that historians of ideas need not prove the holist wrong to continue doing their job. What they need is a workable way to trace discontinuity within continuity, and, we maintain, this is possible by using models such as the CMS, for it is the *abstract relational nature* of conditions (1)–(7) that allows historians to do so.

Let us look at this a bit more closely. The question of what concepts are and how they retain their *identity* over time is fundamentally a philosophical one. Its answer will depend on philosophical theories of meaning and identity, and there is no conclusive reason to take the belief that concepts retain their identity over time to be true. Recall that Lovejoy and Kuukkanen both respond to holistic criticisms by arguing that holism is *false*, because it

absurdly implies that ideas are incommunicable. But no holist would accept this argument. To do justice to the communicability of ideas one needs only to establish their *similarity of meaning* (content similarity), and meaning similarity seems fully consistent with holism. Such similarity-based theories of concepts are on offer (see e.g. Decock and Douven, ‘Similarity’). So, historians of ideas who wish to base their methodology on the claim that holism is false are committed to a philosophical position on which no consensus has emerged. This is problematic. Of course, since no consensus has emerged either concerning the nature of holism, it is equally problematic to take holism as a basis for *rejecting* the history of ideas as a legitimate project. The question of whether concepts retain their identity over time is simply an unsolved philosophical question. Where does this leave us?

We propose that to do the history of ideas, we only need to assume that *something* serves as a constant, under which (or within which, if you prefer) discontinuities can be traced. ‘Something remaining constant’ is construed by Lovejoy and Kuukkanen as (strict) identity among ideas (that is, we have *one* concept and not two), but this seems too strong. History of ideas can be based on weaker notions, such as similarity, subsumption of some concepts under more general concepts, family resemblance, or structural isomorphism. Questions such as these do not need to be settled in order to get to work. We have explained our proposal in terms of a general abstract relational framework (1–7); a model, that is, which allows us to determine determinables seen as variables. This notion of ‘determination’ can be defined in various ways, and we see no point in insisting that it must mean, for example, that there is numerically *one* concept of truth-bearer represented in the CMS, determined by numerically *three* different concepts in Kant, Bolzano, and Frege, namely judgement, proposition, and thought. Talk of ‘determinables’ here has no ontological significance: if one prefers an additive account like Kuukkanen’s (one which, for example, identifies concepts by addition, adding core + margin 1, or core + margin 2 + margin 3, etc.), our models can be freely reformulated in that style (e.g. by formulating the conditions disjunctively, such as: (1) S has judgements or propositions or thoughts or ... which are **about** *a*). In fact, we do not even see a point in insisting that specifically *concepts* (in the technical philosophical meaning) compose the core of the notion of science according to (1–7), namely *follow from, truth*, etc. Not only, then, do we see no need to prove the holist wrong, we also think that using models allows us to incorporate nicely some of the holistic objections into our own method while maintaining, against the holist, that *something* can remain common through time. As we saw, this something can also be a structure, or the place of certain ideas in a model. This is where our second point of difference with Lovejoy becomes clear.

There is no reason to regard the concepts we investigate using models as *simple*. On the contrary, we hold that the ‘ideas’ in ‘the history of ideas’ are bound to be concepts that are highly complex, as well as abstract, if we want

to practise the history of ideas in the most fruitful way. Recall again the concept of proper science as articulated in the CMS. Our method implies that the history of ideas is a discipline concerned with describing concept drift by describing relations among (sub)concepts that form the intension of other, highly complex concepts; as complex as the concept of proper science in our example. And here is where the holists' point may in fact be illuminating.

When a concept x is, say, in the concept constellation xyz , the holist would maintain, x is really no longer x , but rather x -in- xyz ; when x is in xwr , it is in fact not x (not 'the same' x), but rather x -in- xwr . Therefore, in these cases x can at most be *similar* (there are *two xs*), not the same (it is not *one x*). This is not only perfectly accountable in terms of our method, but also simply correct and in tune with how our method really works. For the CMS is a complex abstract constellation that is, strictly speaking, not really as such endorsed by *anybody*: only its specifications are endorsed. When we work with the Model, we reconstruct *specifications* that are at most *similar* to each other, similar with respect to, indeed, (their function as fixed by) the Model. Such is the case with Bolzano's axioms as propositions *versus* Kant's axioms as judgements. This situation applies also to doing the history of concepts that do not share any features in Kukkanen's sense, but which occupy a similar *place* in the network of concepts specified in a certain model. Suppose that, for example, x stands for analyticity, that is, a (sub)concept that, as we have seen, belongs neither to the core of the concept of proper science nor to its margins (it does not appear in 1–7), and bearing this in mind consider again de Jong's study of Bolzano and Kant. When we study x using (1–7), we reconstruct x as embedded in the complex network of subconcepts in (1–7) which itself contains determinables: the concept of analyticity in Kant and Bolzano differs, but its *place* in the complex relational network of concepts, as fixed by the Model, is similar: analytic statements occupy a functionally similar place in the *same* conception of proper science held by both Kant and Bolzano. And this conception is the same in the sense that its specifications have the same or a similar structure; it is with respect to this structure that they are *similar*. This means that our approach allows us to detect continuities even in some cases of conceptual replacement (that is, also when both core and margin change).

SKINNER'S BIASES OBJECTION (D): MODELS AS COGNITIVE SCHEMATA

We may now address the last remaining objection: Skinner's biases objection. Using models, Skinner argues, is not conducive to proper historical writing, for models are a representation of our own mindset, which, being

our own and not that of the authors we study, is necessarily biased. Models may make us see things in historical texts that are not really there.

We do not think Skinner can be proven wrong. But his objection can be turned on its head: exactly because the risk of biased reconstructions is real, biases must be made *as explicit and precise as possible by turning them into models*. If historians employ models such as the CMS in interpreting texts, their presuppositions are there for all to see. Moreover, by using models, the object of investigation is more clearly defined and delimited, for example, proper science *as* articulated in the CMS. Hypotheses developed on the basis of models are, besides, readily open to falsification.⁵

We thus submit that, far from being a danger, the use of models in fact provides a better methodology in the history of ideas than one that does not use them. To further substantiate this claim, we will argue that models, next to providing a defence against interpretative biases, also increase the understanding of historical texts. Our claims in this section are of broader interest, since they support the use of models in history in general, not only in the history of ideas. We support our case by reference to work in cognitive psychology.

Can models such as the CMS be considered, as Skinner suggests, a representation of our mindset? Yes. We argue that the CMS is (a representation of) a *cognitive schema* in the sense of the so-called schema theory of knowledge, yet that precisely this circumstance, far from damaging our position, in fact supports it.

In what sense are models like the CMS cognitive schemas? Stressed by cognitive scientists and educational psychologists from the 1970s onwards, schemata are mental frameworks for organizing information (Anderson, 'Notion of Schemata'; Rumelhart, 'Schemata'; Rumelhart and Ortony, 'Representation of Knowledge'). They can be represented by *networks of (sub)concepts*. When schemata are *activated*, as is said in the literature, they increase a reader's capacity to comprehend passages in texts. In fact, it is argued, without schemata understanding is impossible. As Anderson remarks in a discussion of reading comprehension, 'text is gobbledygook unless the reader possesses an interpretative framework to breathe meaning into it' (Anderson, 'Notion of Schemata', 423).

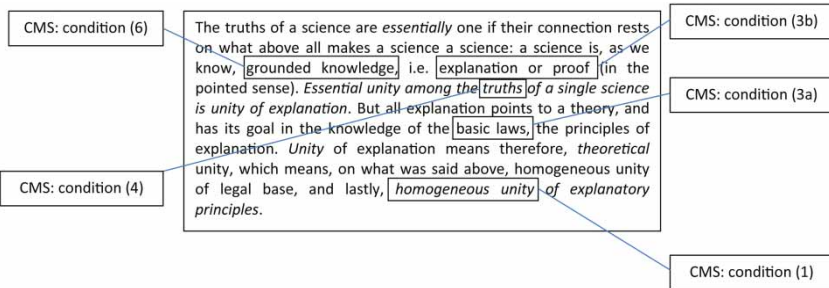
Importantly for the comparison with our models, schemata are characterized as being *abstract*. By virtue of being abstract, they can be applied to a variety of cases; or, we can say, they contain *determinables* or *variables* that can be associated with various (sub)concepts (Rumelhart, 'Schemata', 35). In addition, schemata are structured, insofar as they represent *relationships* among sub(conceptual) parts. They also provide *context* and *vocabulary* for what we read, and are taken to *organize* experience. Schemata are also *dynamic* – and in fact, in order to accommodate new experiences, they

⁵These virtues of models have been stressed by socio-economic historians (Lorenz, *De Constructie*, 243–81).

must be so. When new information is comprehended in terms of an already existing schema, schema theorists speak of *assimilation* or *accretion*; if new information is used to create a new schema, schema theorists speak of *restructuring*; if, finally, new information is used to modify an existing schema so as to make it more accurate or useful, schema theorists speak of *tuning* schemas (Rumelhart and Norman, ‘Accretion’).

Consider now again the CMS as described in the previous section. It is easy to see that the salient characteristics of schemata apply to it. The CMS analyses the concept of proper science as a conjunction of seven *abstract* conditions containing determinable (*variables*) subconcepts. It represents a *network of relations* among these subconcepts, and provides a *vocabulary* and *context* for interpretation. By applying the CMS to the writings of various thinkers, historians *determine* or *contextualize* this model, on which basis they *interpret* and *comprehend* the views of various philosophers on science. Is the CMS dynamic? Yes: Betti and de Jong have *restructured* earlier versions of a similar model by Beth and Scholz, partially in light of new evidence. Indeed, insofar as the Model enables the formation of empirical hypotheses, it should always be open to restructuring and tuning.

These similarities between the CMS and schemata are evidence that it *is* a schema. We may therefore expect that it furthers the comprehension of certain philosophical texts. It is not customary in philosophy to test hypotheses of this kind experimentally. We ourselves did, however, conduct a small qualitative experiment in a class on the work of Edmund Husserl. The following passage from Husserl (*Logische Untersuchungen*, §64) was presented to a small group of students with an elementary knowledge of Husserl’s thought:



For many readers lacking prior knowledge, as for the students in our present case, this text is, again, gobbledygook. Yet if we apply the various conditions of the Model as indicated, that is, if we *activate* a schema, the reader can begin to organize, interpret and make sense of the passage in question. This is a process of both contextualizing a schema and placing a text in context.

CONCLUSION

We have presented a new and implementable methodology for the history of ideas. Lovejoy's history of ideas has been rejected in the past on the ground that (i) the existence of unit-ideas is not compatible with holism. It has also been argued that it is neither clear (ii) how the meaning of unit-ideas can change, nor (iii) how one should define the scope of unit-ideas, and (iv) that the history of ideas often yields arbitrary and biased narratives. These criticisms are taken to show that the history of ideas involves an improper historical method.

We have shown that Kuukkanen's defence of Lovejoy's approach, by distinguishing between the core and the margin of ideas or concepts, is able to counter two of these objections ((ii) and (iii)), yet remains vulnerable to (i) and (iv). Given this lingering vulnerability, Kuukkanen's defence does not manage to show that the history of ideas is a proper historical method, nor does it provide an implementable methodology for the history of ideas.

By contrast, our model approach does provide an implementable methodology for the history of ideas that counters all four of the objections mentioned above and allows us to show in what sense the history of ideas is a legitimate method. While we adopt Kuukkanen's vocabulary of concept core/margin to describe concept drift in the history of ideas, we argue that concepts must be seen as (parts of) models, that is (complex, interpretive) conceptual frameworks. Through models such as the CMS, we can make explicit the stable core and variable margin of highly complex concepts such as the concept of science. We have shown that our approach can counter (iv) because of two characteristics of models. First, models can be set up in such a way as to do justice to the manner in which authors construe concepts. Granting this, our approach is not susceptible to Skinner's criticism that the history of ideas yields arbitrary narratives. Second, models are *explicit* interpretative frameworks that represent a mindset or schema in the sense of the schema theory of knowledge in cognitive psychology. Making an interpretive framework explicit in fact provides the best defence against the risks of interpretative biases in the writing of intellectual history, and furthers the comprehension of texts. Furthermore, we have shown that the model approach can counter the holistic objection *ad* (i) by remaining neutral concerning the ontological nature of ideas and by arguing that our approach allows us to trace continuities in history even without assuming a notion of (strict) identity between ideas.

The use of models is virtually nonexistent in the history of ideas. We submit, however, that historians of ideas should use models as schematic representations to study the development of ideas because models enable us to represent concept drift as change in a network of (shifting) relations among subideas, that is, elements of a given complex idea or concept. Since our models are highly abstract, and can thus be properly applied to different periods and the work of different thinkers, they provide efficient

tools for organizing and classifying information and enable fine-grained analyses of conceptual (dis)continuities, that is, conceptual shifts throughout history.

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