Knowledge and knowers of the past:

A study in the philosophy of evolutionary biology.

Submitted by Thomas Bonnin to the University of Exeter

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I certify that all material in this thesis which is not my own work has been identified and that no material has previously been submitted and approved for the award of a degree by this or any other University.

Signature: …………………………………………………………..

ABSTRACT

This dissertation proposes an exploration of a variety of themes in philosophy of science through the lens of a case study in evolutionary biology. It draws from a careful analysis and comparison of the hypotheses from Bill Martin and Tom Cavalier-Smith. These two scientists produced contrasted and competing accounts for one of the main events in the history of life, the origin of eukaryotic cells. This case study feeds four main philosophical themes around which this dissertation is articulated. (1) *Theorizing:* What kind of theory are hypotheses about unique events in the past? (2) *Representation:* How do hypotheses about the past represent their target? (3) *Evidential claims:* What kind of evidence is employed and how do they constrain these hypotheses? (4) *Pluralism:* What are the benefits and the risks associated with the coexistence of rival hypotheses? This work both seeks to rearticulate traditional debates in philosophy of science in the light of a lesser-known case of scientific practice and to enrich the catalogue of existing case studies in the philosophy of historical sciences.

Table of Contents

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CHAPTER 1: EVOLUTIONARY BIOLOGY BETWEEN SCIENCE AND HISTORY.

INTRODUCTION

There are two interlinked perspectives that can be taken on living organisms. One may wonder about how these organisms work or, alternatively, about how these forms of life came to be. Biology, the study of forms of life, has thus often been described as being composed of present-centred and past-centred investigations. It is from the latter that biology can be described as a *historical* inquiry. This historical side of biology comes under the name of *evolutionary biology.* This dissertation proposes a study of the practice of evolutionary biology as a *historical science*.

Evolutionary biology is a multifaceted set of practices, and this work does not aim at exploring all of the richness this discipline has to offer. Some evolutionary biologists are dedicated to working out the general principles that drive the evolution of forms of life. Achieving this amounts to a formalization of evolution under a series of concepts, equations and models, and draws together theoretically-inclined biologists and philosophers of biology[1]. Other evolutionary biologists have tried to infer the series of events that marked the history of life, from its origin to the present. This dissertation is exclusively focused on this second type of practice[2]. This work studies evolutionary biology as a historical endeavour to reconstruct the history of life.

What types of theories are produced? How do these theories represent their target? What kind of evidence is mobilized? How does theory choice occur? These general questions are going to be addressed in this specific context within evolutionary biology in the following chapters. Before doing this, this first chapter starts off with some methodological considerations. Concretely, three things are on this chapter's program: (a) clearing off some of the tensions at the

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[1] Examples include Sober 1984; Okasha 2006, Pigliucci and Kaplan 2006; Godfrey-Smith 2009.

[2] These endeavours are not mutually exclusive. There are various examples of works that combine both theoretical and historical aspects (e.g. Maynard-Smith and Szathmáry 1997; Darwin 1859).

intersection between history and science; (b) specifying my aims and methodology and (c) breaking down the upcoming chapters.

Evolutionary biology, as a historical science, lies at the intersection between history and science. Qualifying something as historical *and* scientific generates tensions. Analysts and practitioners of history have on several occasions claimed the autonomy of their field from science. Conversely, some philosophers of science do not consider history to belong to the traditional catalogue of sciences and to provide scientific knowledge.

The first part of this chapter aims at dissipating these residual tensions by dispelling two arguments mobilized in favour of a separation between history and science. Firstly, I argue that some of the stated reasons for separating history from science are based on an outdated and narrow picture of what scientific knowledge is. Once released from the idea that science is a strictly nomological endeavour and emphasizing instead the methodological disunity of the sciences, there are no obstacles to the accommodation of history within the scientific picture. Another separatist strategy is to ground the autonomy of history on its object of inquiry: humans. The special nature of humans is argued to grant history a separate epistemic position, away from the scientific study of a "mindless" universe. I argue, on methodological grounds, that a strong separation is unwarranted and that the study of the history of humans bears continuities with the rest of historical inquiries, whether about animate or inanimate matter.

Undermining the arguments supporting the separation of history from science prepares the ground for two things. First, rejecting the separation between history and science legitimates a study of history as a scientific inquiry. In terms of resources, then, it makes relevant the use of concepts and notions derived both from the study of scientific and historical practices. Establishing methodological continuities between (human) history and historical sciences ensures that no artificial barrier exists between the studies of these practices.

After releasing existing tensions and enlarging the breadth of potentially relevant literature to my analysis, the second part of this chapter explores some of the available philosophical methodologies. This allows me to specify the types of questions, methods and results I'd like to draw inspiration from. To do that, I draw parallels between philosophical projects independently presented in the context of the study of science and of the study of history. In the context of science, I discuss Godfrey-Smith's distinction between "philosophy of nature" and "philosophy of science". In the context of history, I think Tucker's distinction between "philosophy of history" and "philosophy of historiography" goes on similar lines to Godfrey-Smith's. On one hand, philosophy of nature and philosophy of history propose studies of the *constituents and properties* of, respectively, the phenomena studied in scientific and historical inquiries. It attempts to extract the deeper meaning of our best scientific and historical theories, a philosophical method characterized as *naturalism*. On the other hand, philosophy of science and philosophy of historiography provide analyses of the scientific and historical *practices and methods*. It relies on historical, social and methodological studies to understand how scientists and historians acquire knowledge about the world.

I argue against a strong separation between the first and the second types of projects. Both types of projects, I think, can be run conjointly. For this, I draw inspiration from existing studies of science that study the relation between theories, practices and the world at the same time. I characterize this latter position further and outline some of its commitments. I argue these projects also commit to a form of methodological naturalism, which defends the use of the best available scientific and conceptual resources to tackle questions about science. This methodological stance provides an ideal that is only partially achieved in this present work. I conclude this chapter by presenting and motivating the successive chapters of this dissertation, and by a discussion of their scope and limits.

I. History away from Science? A rebuttal

A. Historical sciences and human history

I propose a study of evolutionary biology as a *historical science*. By doing this, I position my work within an emerging subfield in philosophy of science. Currie and Turner have recently provided a definition of "historical scientists":

> Historical scientists, from cosmologists to archaeologists, tackle important but difficult tasks: reconstructing the events and entities which populate the deep past, understanding their formation and development,

> and learning how to see our contemporary world in terms of its long history. (Currie and Turner 2016, 43)[3]

This definition integrates a wide set of activities revolving around the study of the deep past. Since it involves both cosmologists and archaeologists, it sets minimal boundaries on how deep the events of the past must be to belong to the historical sciences. The time-span dealt with by historical sciences only excludes the “shallow” past, a past too close yet to be worthy of historical study. What remains unclear, from this definition, is the relation between historical sciences and history, the latter traditionally understood as the study of the human past.

While Currie and Turner explicitly avoid “playing demarcation games” (Currie and Turner 2016, 43), I think that inquiring whether history belongs to historical sciences can help to unpack some tensions at the intersection between history and science. This relation is not always perceived as problematic. Tucker, in the introduction of his *Companion to the Philosophy of History and Historiography*, affirms that

> The scope of history is all of the past: societies have a history, but so do rocks, languages, species, and indeed the universe. Historiography in this broader sense attempts then to infer descriptions of the histories of everything (Tucker 2009a, 2–3).

In this view, historical sciences are uncontroversially part of history (and vice versa). This statement matches with the wide variety of subjects addressed in Tucker’s companion, which includes chapters on human history, but also on the history of non-human living as well as non-living entities. For others, however, the idea of continuity between history and historical sciences is perceived as problematic and faces some objections. I attempt here to deal with two such objections. The first argues that history does not provide scientific knowledge. The second one argues that the focus on humans singles out human history

[3] Tucker has also provided a definition of historical sciences, as “sciences that attempt to infer rigorously representations of past events, processes, and their causal relations from their information-preserving effects” (Tucker 2014, 365). I prefer the wider array of activities included in Currie and Turner’s definition.

(and the rest of the social and human sciences, for that matter) from the rest of the sciences.

B. Objection 1: "History is not a science"

On a recent defence of scientific pluralism, Dupré characterizes history as part of the "traditional non-sciences" (Dupré 2012, 38). While this brief mention was surely not a key component of his argumentation, I think that it still provides a good illustration of the tendency to keep history and science apart. What legitimates this separation?

Making this separation requires committing (implicitly or not) to a view about what science is. Often[4], science has been described as following a "nomological ideal". In this view, scientific investigations try to uncover the "laws of nature": a set of regularities couched in mathematical equations that enable to explain and predict the behaviour of the various entities composing this world. Historical investigations, which aim at explaining the causes driving the occurrence of single events, are not scientific in this sense. They are not interested "in the search for general laws which might govern these events" (Hempel 1942, 35). This interpretation leaves four possibilities to the notion of "historical science": (1) contrary to general beliefs, historical inquiries are scientific because they do establish laws of nature; (2) historical sciences are immature forms of science, as they fail to establish and apply laws of nature; (3) "historical science" is an oxymoron, since they do not aim to establish laws of nature and (4) historical sciences are scientific because the nomological ideal does not adequately capture what counts as scientific.

Possibilities (1), (2) and (3) stem from a belief in the nomological ideal of science. It hinges the scientific character of history on matters of goal and success. If they both do not aim to and fail to establish the laws of history, then (3) prevails and history is not a science. If they aim to, but fail, to establish laws, then (2) prevails and history is an immature science. If they aim, and manage, to establish the laws of history, then (1) prevails and history is a science.

An example of the defence of (1) in the context of human history dates from the 19th century and the works of the founder of sociology: Auguste Comte and his

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[4] Though I doubt this is Dupré's case.

“Law of Three Stages” (Comte 1830). While it is not articulated in mathematical terms, it argues that the evolution of human societies follows three stages: “theological”, “metaphysical” and “positive”. On Comte’s view, history is scientific (possibility (1)) because it satisfies a positivistic ideal by connecting observations about past societies into well-established regularities. In another context, the existence of such laws has also been used to claim the epistemological superiority of evolutionary biology over (human) history. Here, evolutionary biologists Maynard-Smith and Szathmáry spoke of the “great advantage” they possessed over historians:

> we have agreed theories both of chemistry and of the mechanism of evolutionary change. We can therefore insist that our explanations be plausible both chemically, and in terms of natural selection. This places a severe constraint on possible theories (Maynard-Smith and Szathmáry 1997, 3).

What the authors mean by the agreed theory of evolutionary change is a commitment to a gene-centric view of evolution. In their view, “the transitions must be explained in terms of immediate selective advantage to individual replicators” (Maynard-Smith and Szathmáry 1997, 8). In addition to this, the constraints on organisms imposed by the principles of chemistry, according to them, are sufficiently strong to limit greatly what can be claimed about the past. Historians, in their view, do not benefit from any such set of constraints to limit what they can say about human actions. In other words, Maynard-Smith and Szathmáry consider evolutionary biology as scientific since it establishes and applies nomological principles (possibility (1)). History, however, isn’t because it fails to do so (possibility (2) or (3)).

Comte’s positive law for the evolution of societies has been refuted several times since it arguably fits history in a very tight Procrustean bed. Maynard-Smith and Szathmáry can also be criticized for their strong commitment for and confidence in a gene-centric view of evolution that has also been criticized and considered as too monolithic. The existence and scope of general principles driving historical change, as mentioned in the opening lines of this chapter, are the subject of ongoing scientific and philosophical attention. In the case of biological evolution, a consensus on these questions does not seem to have been reached and does not always seem to be aimed for. Similarly, the strength of the constraints stemming from the principles of chemistry also needs to be relativized. On a discussion of existing theories about the origin of life, Malaterre observed that the space of possible chemical reactions was only “very loosely” constrained (Malaterre 2010, 45-46). This view has more broadly been formulated by Gould:

> Invariant laws of nature impact the general forms and functions of organisms; they set the channels in which organic design must evolve. But the channels are so broad relative to the details that fascinate us! […] When we set our focus on the level of detail that regulates most common questions about the history of life, contingency dominates and the predictability of general form recedes to an irrelevant background (Gould 1989, 289-290).

Without necessarily subscribing to how weak Gould argues these constraints to be (and the “strong contingency” thesis associated with it), his and Malaterre’s positions illustrate the open nature of this debate. The absence of agreed theories of evolution and of sufficiently strict constraints provided by the laws of nature seem, then, to invalidate the posture that would single out evolutionary inquiries from historical ones on the basis of their closeness to a nomological ideal. At best, this difference could be interpreted as a matter of degree: it is likely that some disciplines within historical sciences apply tighter, “quasi-nomological”, constraints on the past phenomena they try to explain.

Another possibility is to argue for the possibility (4), by claiming that the “nomological ideal” presents a dated view of what scientific knowledge is. This criticism has a long history. In the late 19th century, Windelband already complained about a form of “nomothetic imperialism” originating from mathematics and the natural sciences and providing normative guidelines on how other sciences should look (Windelband 1980). This complaint also underlines the literature defending scientific pluralism and the disunity of science (see Dupré 1993; Wylie 2002; Kellert, Longino and Waters 2006; Chang 2012) which highlights in several ways the unrealistic expectations and nefarious consequences of the reliance on a monistic nomological ideal for the sciences[5]. If one endorses this view, and I do, it then relaxes our criteria of scientificity by releasing the expectation that properly scientific endeavours aim at producing and establishing laws of nature[6]. The scientificity of history, then, does not depend on its ambition to produce and successfully apply laws of nature. Failure to do both things is not sufficient to refuse to grant scientific status to a practice. In other words, history's traditional focus on explaining unique events without merely invoking general principles is not in principle unscientific.

If there are no arguments against viewing history as a science, are there contrary, positive ones? Despite defending the disunity of science, Dupré identifies a set of virtues shared, in different degrees, by all scientific inquiries, namely

> sensitivity to empirical fact, plausible background assumptions, coherence with other things we know, exposure to criticism from the widest variety of sources and no doubt others (Dupré 1993, 243).

It would be surprising to think that these virtues are not possessed by all historical investigations. Sources of various origins provide empirical grounds and enable criticisms. The background assumptions invoked for the behaviour of the various entities and phenomena described must be plausible. Historical hypotheses are undoubtedly aiming to have some degree of coherence with existing knowledge on the relevant subjects. In addition to these virtues, achieving progress can be seen as a sign of flourishing scientific practices. While scientific progress has been a notoriously difficult notion to pin down with precision, historical investigations can be seen as progressive. This, for instance, has been documented by Rudwick concerning the history of the Earth (Rudwick 2014), Gould, Sapp and Archibald for our understanding of biological

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[5] The argument runs for other forms of monism in science.

[6] By defending this approach, I am not denying the importance of regularities and nomological principles in scientific investigations, including historical ones. I just refuse to evaluate the scientificity of an investigation solely on its capacity to provide and apply such general principles.

evolution (Gould 1989; Sapp 1994; Archibald 2014) and has been metaphorically summarized by Bloch in the context of human history:

> [In a bit more than a century,] gigantic patches of humanity emerged from the mists. Egypt and Chaldea shook off their shrouds. Dead cities of Central Asia revealed their languages that no-one could speak anymore, and their religions that were long extinct (Bloch 1949, 22, own translation).

This spectacular ability that history possesses to "bring to epistemic life" unobservable features of the world, I think, is similar to the ability of physics to discuss unobservable microparticles and faraway planets. No one doubts the scientific character of physics. By possessing or responding to all these epistemic virtues, I think that history belongs to the sciences. There is, therefore, nothing paradoxical in talking about "historical sciences".

C. Objection 2: "Humans make a difference"

If there are no good objections to considering history as a science, there are still arguments that would grant a special character to the study of the *human* past. These arguments point to a strong distinction between (human) history and the rest of historical sciences. The idea is that the subject-matter of history, humans, somehow separates this inquiry from the rest[7]. This section attempts to critically assess these arguments.

"The subject-matter of history is, by nature, Man. Better: Humans" (Bloch 1949, 4, own translation). This quote from Bloch can be interpreted in two ways. A weak interpretation would see in this quote the delineation of what is traditionally called "history" as a subfield of the existing historical sciences. This delineation can, alternatively, be seen in a stronger way: there is a something fundamentally distinct about the study of the human past. What is not about humans is not distinctively historical. The weaker interpretation does not conflict with a claim of methodological continuity between historical sciences and history. The stronger interpretation, however, does. It echoes some of the intuitions sometimes stated by analysts. Kosso, for instance, declares that

[7] It is likely that this discussion similarly applies to the rest of social and human sciences.

“persons, unlike electrons, are not all alike. People, unlike planets, are expressive and creative” (Kosso 2009, 10). As a consequence, “[u]nderstanding humans may be a fundamentally different process than understanding the mindless objects studied in natural science. [Human history] may be fundamentally distinct from science” (Kosso 2009, 24).

Tucker has, in another context, nicely summarized the tension between the two interpretations of Bloch’s claim. He affirms that

> some philosophical approaches to historiography consider it special for having a human subject matter. Forms of description, understanding, and explanation in historiography are allegedly different because of this special subject matter. From this perspective, *history* would refer then exclusively to the human past […]. Alternative philosophical approaches argue that there are some common and unique features to all the sciences of the past, sciences that are concerned with the inference of unobservable token events from their traces in the present (Tucker 2009a, 3)[8].

In order to critically assess the stronger separatist interpretation, I now turn to the works of Collingwood, who articulated a detailed argument for the separation of human history from the rest of the historical (and natural) sciences.

Collingwood, in strong terms, denies the possibility of a genuine historical knowledge about non-humans:

> [T]here is and can be no history of nature, whether as perceived or as thought by the scientist. No doubt nature contains, undergoes, or even consists of, processes; […]. But all this goes no way towards proving that the life of nature is an historical life or that our knowledge of it is historical knowledge. The only condition on which there could be a history of nature is that the events of nature are actions on the part of some

[8] A similar distinction has been made by Glennan, attributing the first view to a form of “anti-naturalism” and the second to a form of “naturalism” (Glennan 2010, 252). I discuss forms of naturalism later on this chapter.

> thinking being or beings, and that by studying these actions we could discover what were the thoughts which they expressed and think these thoughts for ourselves. This is a condition which probably no one will claim is fulfilled. Consequently, the processes of nature are not historical processes and our knowledge of nature, though it may resemble history in certain superficial ways, e.g. by being chronological, is not historical knowledge (Collingwood 1994, 302).

This is an extended quote, but it clearly albeit densely states the main lines of Collingwood's argument.

In this paragraph, Collingwood doesn't deny that disciplines such as geology or evolutionary biology generate scientific knowledge, but that these disciplines can generate *historical* knowledge. Why is human history so special? According to Collingwood, it is because it is driven by the actions of *thinking* beings: humans. Then, what difference does thinking make? Thoughts drive (but does not fully determine) individual acts (Collingwood 1994, 309). It would be foolish, otherwise, to assume that someone "acted with no idea whatever what would come of it, but did the first thing that came into his head and merely waited to see the consequences" (Collingwood 1994, 310).

Not merely conceived as a driver to history, thoughts are also argued to possess a *universal* character:

> The peculiarity of thought is that, in addition to occurring here and now in this context, it can sustain itself through a change of context and revive in a different one (Collingwood 1994, 297).

This way, acts of thought possess a certain independence from experience or others thoughts occurring in a specific context. Collingwood illustrates this by talking about mathematics:

> The self-identity of the act of thinking that these two angles are equal is not only independent of such matters as that a person performing it is hungry and cold, and feels his chair hard beneath him, and is bored with his lesson: it is also independent of further thoughts, such as the book says they are equal, or that the master believes them to be equal; or even thoughts more closely relevant to the subject in hand, as that their

> sum, plus the angle at the vertex, is 180 degrees (Collingwood 1994, 298).

These self-conscious, reflective thoughts, thoughts "performed in the consciousness that it is being performed", which lead us to "do something of which we have a conception before we do it" (Collingwood 1994, 308), are the "atoms" which historians aim at discovering in their work. In other words, these acts of thoughts are the *mental objects* remaining from the subjective actions of past people. This is what is epistemologically accessible to the historian, and what he aims to re-enact in his own thoughts.

Collingwood illustrates this with the way to gain historical knowledge about the Theodosian Code:

> Suppose, for example, [the historian] is reading the Theodosian Code, and has before him a certain edict of an emperor. Merely reading the words and being able to translate them does not amount to knowing their historical significance. In order to do that he must envisage the situation with which the emperor was trying to deal, and he must envisage it as that emperor envisaged it. Then he must see for himself, just as if the emperor's situation were his own, how such a situation might be dealt with; he must see the possible alternatives, and the reasons for choosing one rather than another; and thus *he must go through the process which the emperor went through in deciding on this particular course*. Thus he is re-enacting in his own mind the experience of the emperor; and *only in so far as he does this has he any historical knowledge*, as distinct from a merely philological knowledge, of the meaning of the edict (Collingwood 1994, 283).

The epistemic privilege granted by this unique connection between historians and their human subject-matter is what maintains history as an "autonomous form of thought with its own principles and its own methods" (Collingwood 1994, 140). To summarize, historical knowledge is the knowledge of human acts of thoughts, driving human actions and accessible by mental re-enactment by historians.

What, then, makes a historical re-enactment a good one? Historical evidence, according to Collingwood, is

> evidence of how such thinking has been done and that the historian should be able to interpret it, that is, should be able to re-enact in his own mind the thought he is studying, envisaging the problem from which it started and reconstructing the steps by which its solution was attempted (Collingwood 1994, 312–13).

This, then, requires a good knowledge of (a) the context in which these thoughts occurred as well as (b) a good knowledge of human behaviour and psychology. The former relies on knowledge of the numerous material aspects of societies (i.e. geography, agriculture, architecture). I argue that this type of knowledge does not single out human history from the rest of historical sciences. I argue that knowing about the former is methodologically continuous with the rest of the historical sciences. Of course, palaeontologists and cosmologists are not attempting to "reenact" in their own mind the experiences of extinct mammals and asteroids. However, these different inquiries share a commitment to epistemically recreate the "universe" in which the past event of interest purportedly occurred.

Knowing about human behaviour and psychology is, in some respects, continuous with knowing about the behaviour and psychology of other, non-human, historically relevant beings. However, this is also where human history and historical sciences might bear some methodological divergences. Contrary to Collingwood, I don't think that this comes from the ability to decontextualize and recontextualize acts of thoughts. Historians, as much as other social scientists, study what Hacking calls *interactive kinds* (Hacking 1999). In this view, humans interact with the classifications they describe themselves with. These classifications can shape the behaviour of the people thus classified and, consequently, eventually requires changing the description of the category via what Hacking terms a *looping effect*. In the above example, the expectations and norms associated with the status of Roman emperor undoubtedly shaped Theodosius' behaviour, and in turn, his reign as an emperor surely shaped the expectations and norms associated with this social category.

This phenomenon, however, does not occur with categories describing non-humans. In Hacking's terms,

> The classification "quark" is indifferent in the sense that calling a quark a quark makes no difference to the quark (Hacking 1999, 105)

The extent to which the interaction between the knowledge of past humans with the kinds they are described with makes human history methodologically distinct from historical sciences is a matter of interpretation. It surely constitutes a source of methodological difficulty. However, here, like in the rest of historical sciences, historians are tributaries of increases of knowledge in the relevant scientific domains.

Emphasizing the presence of methodological continuities between historical sciences and human history by no means eliminates the diversity of phenomena and methods found in these inquiries: from archaeology to social history, from post-colonialism to cosmology. In this view, however, they are rather subparts of the same family, a set of related inquiries interested in the past and producing different forms of historical knowledge. It is likely, then, that some of the analyses of the practice of human history are illuminating for our understanding of how knowledge in other historical sciences is produced. The reverse is also true. I argue that analysts of human history should not dismiss methodological investigations on the production of historical knowledge about non-human entities. To summarize this first part, I argued against conceiving human history and historical sciences as strongly separated. These inquiries are both defended as being scientific, and as possessing methodological continuities.

II. Methodological possibilities

If history and historical sciences are methodologically continuous, the scope of relevant resources to understand the practices of evolutionary biology is then extremely wide. In this second part I attempt to explore methodological possibilities, thus help to clarify the sort of projects I'd like to run in this dissertation. I start by pointing to the existence of parallel projects in both the study of human history and in the philosophy of science.

A. Metaphysical vs. practice-oriented philosophical projects

Tucker recently attempted to clarify and unify the terminology used in the various studies of historical practice. His conceptual arsenal is mainly articulated around a distinction between “history” and “historiography”. History is defined as the “past events and processes” (Tucker 2009b, xii). It designates the phenomena under historical investigation. Historiography is defined as “what historians write, about past events, about history” (Tucker 2009b, xii). It designates the products of historical investigation[9].

Following this line, Tucker makes a distinction between “philosophy of history” and “philosophy of historiography”. Philosophy of history is the “philosophical examination, study, and theorizing about the past” (Tucker 2009b, xii). It aims at uncovering the deeper dynamics of the past: its various components and the principles and processes that guide historical change. Philosophy of historiography is defined as the “philosophical examination, study, and theorizing about […] what historians write, and its relation to the evidence” (Tucker 2009b, xii). This one is not concerned with the structure of the past, but with how the past is studied. To summarize, Tucker’s distinction between philosophy of history and philosophy of historiography is between an examination of the deeper meaning of historical knowledge and a study of the production of historical knowledge. This distinction closely maps Dray’s earlier distinction between “speculative” and “critical” history[10].

Tucker highlights the parallels between his distinction between “philosophy of history” and “philosophy of historiography” respectively with the one between “philosophy of nature” and “philosophy of science” in the study of science (Tucker 2009a, 3-4). In the latter context, Godfrey-Smith proposed to

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[9] “Historiography” has a variety of possible meanings. In addition to Tucker’s definition, it can also designate the analysis of the writing of history (Salevouris and Furay 2015, 256), the analysis of historical practice (beyond writing) (Offenstadt 2011, 1) or the activity of writing history (Arnold 2000, 5).

[10] “The speculative seeks to discover in history, the course of events, a pattern of meaning which lies beyond the purview of the ordinary historian. The critical endeavours to make clear the nature of the historian’s own inquiry, in order to “locate” it, as it were, on the map of knowledge” (Dray 1964, 1).

> distinguish *philosophy of science*, in a narrower sense, from *philosophy of nature*. Philosophy of science in this narrower sense is an attempt to understand the activity and the products of science itself. When doing philosophy of nature, we are trying to understand the universe and our place in it (Godfrey-Smith 2014, 4).

The parallels between Godfrey-Smith and Tucker's distinctions are indeed clear. Philosophy of nature and philosophy of history share a common interest in being focused on scientific *products* and their metaphysical meaning. They are, in Tucker's words, "sub-fields of metaphysics that examine the ultimate constituent parts of everything" (Tucker 2009a, 4). Philosophy of science and philosophy of historiography concern the examination of scientific *practice* and its various constituents. It examines the conditions under which scientific knowledge is produced.

Despite differences in subject-matter, I argue that these two types of philosophical projects (henceforth *metaphysical* and *practice-oriented*) are underpinned by a similar methodological commitment, broadly named "naturalism". I first illustrate how naturalistic commitments are at play in philosophy of biology, an instance of a metaphysical project.

B. Naturalistic approaches in metaphysical and practice-oriented projects

If the metaphysical project is a study of the deeper meaning of scientific products, it is intuitive to guess the nature of the data on which this philosophical analysis is based. Godfrey-Smith describes this methodology as "working out what the raw science is really telling us, and using it to put together an overall picture of the world" (Godfrey-Smith 2014, 4). Applied to the context of biology,

> The science of biology becomes an instrument – a lens – through which we look at the natural world. Science is then a resource for philosophy rather than a subject matter (Godfrey-Smith 2014, 4).

This position is echoed by Griffiths' assessment of the methodology ideally employed by philosophers of biology:

> Ideally, philosophy of biology differs from biology itself not in its knowledge base, but only in the questions it asks. The philosopher aims to engage with the content of biology at a professional level, although typically with greater knowledge of its history than biologists themselves, and less hands-on skills (Griffiths 2014).

This way, philosophy and biology are perceived to be in a mutually beneficial relationship. As Sterelny and Griffiths describe,

> philosophy is important to biology because biology's exciting conclusions do not follow from the facts alone. Conversely, biology is important to philosophy because these exciting conclusions really do depend on the biological facts (Sterelny and Griffiths 1999, 5).

They illustrate their claim with the refutation of the doctrine of biological determinism, a philosophical position that has been shown false "because of the facts of evolutionary theory and genetics" (Sterelny and Griffiths 1999, 5-6). A proposed methodology for the metaphysical project, then, is to extract, by philosophical analysis, the deeper meanings from the facts provided by the biological sciences[11].

Griffiths considers these methodological commitments to belong to a form of *naturalism* in which there is "no profound discontinuity in either method or content between philosophy and science" (Griffiths 2014). It makes it difficult to tell apart theoretical biology from philosophy of biology. In a similar vein, Godfrey-Smith recognizes that tackling metaphysical questions "is not something that only philosophers can do" (Godfrey-Smith 2014, 4). Conceptually-minded biologists are also in the business of providing a deeper meaning to biological facts: they are also capable of "distilling the philosophical upshot of scientific work" (Godfrey-Smith 2014, 4).

[11] An earlier endorsement of a similar position has been made by Mayr, who describes biological sciences as "the most suitable […] starting point of analysis" for deeper questions about the nature of life and the place of humanity (Mayr 1969, 202).

By extrapolation from this case within biology, metaphysical projects, comprising philosophy of history and philosophy of nature, endorse a form of naturalism, defined as a commitment to ground philosophical analysis on scientific results. In the case of biology, this means grounding philosophical discussions about the nature of life on biological facts. In the case of historical sciences, it means discussing the nature of historical change on the basis of historical facts.

In practice-oriented philosophical projects, there are (at least) two domains that can be uncontroversially attributed to a naturalistic methodology. The first of them is "evolutionary epistemology" (EE). Evolutionary epistemology can refer to two distinct projects. The first one ("the evolution of epistemological mechanisms", EEM) is "the label for the program which attempts to provide an evolutionary account of the development of cognitive structures" (Bradie and Harms 2016). The production of scientific knowledge relies on individual and collective cognitive capacities that are products of evolution. How and why did these capacities come to evolve in humans? The second project ("the evolutionary epistemology of theories", EET) "attempts to account for the evolution of ideas, scientific theories, epistemic norms and culture in general by using models and metaphors drawn from evolutionary biology" (Bradie and Harms 2016). In this view, it is possible to account for the processes behind the genesis and development of scientific theories by using the conceptual apparatus provided by evolutionary theory[12]. Both EE programs, then, are openly naturalistic as, at least, it employs the concepts stemming from evolutionary biology to understand scientific practices.

Another example of a practice-oriented naturalistic project can be found in the study of scientific reasoning as defended by Bechtel. In this case,

> the *naturalized* approach to understanding the mind and brain involves seeing them as part of the natural world (rather than as miraculous or supernatural anomalies) and recognizing the biological, evolutionary, and

[12] For classical works in EET, see Popper 1972; Campbell 1974; Hull 1988. An instance of a more recent attempt is Rouse 2016. For a critical discussion, see Callebaut 1993, 286-337.

> environmental pressures which have helped to shape them (Bechtel *et al*. 2001, 7).

This naturalized approach is continuous with EEM, as it also studies reasoning in an evolutionary lens, albeit this project is less strictly committed to describing these processes in strictly evolutionary terms. The overarching methodological commitment emphasized by Bechtel is that it is particularly important to be "shaped" by science when facing these questions (Bechtel in Callebaut 1993, 352-353). By this, he means that philosophical questions and concepts must come *second*. They should not guide the analysis of scientific knowledge by framing the questions and problems the relevant scientific knowledge will help us to solve. Instead, in Bechtel's view, philosophical questions and problems must emerge and be shaped by the analysis of existing scientific debates and knowledge. For instance, anybody interested in questions about cognition should start by analysing what scientific studies of this process have to say.

These examples highlight how methodological commitments to naturalism can fuel a variety of philosophy of science projects. At the very least, naturalism represents an application of metaphors and concepts derived from scientific disciplines to feed practice-oriented analyses (as in the case of EET). At the other extreme, attempts at philosophical analysis of the outcomes of scientific practice must, as Bechtel's stronger view argues, primarily draw inspiration from, and be shaped by, scientific practices and problems. In this case, philosophy is entirely shaped by and subordinate to science. All these philosophical projects share a belief, albeit in various degrees, on a scientific basis to philosophical discussions and in the impossibility of a philosophical inquiry on science that is autonomous from scientific knowledge and practices.

This section argued that the metaphysical and practice-oriented projects are amenable to a naturalistic methodology. I have, however, not yet displayed instances of projects that explicitly bridged these two types of philosophical practice. The next section presents projects that are overtly both metaphysical *and* practice-oriented. They can be found in recent studies of scientific practice. I argue that these projects are also committed to a form of methodological naturalism.

C. Integrated Studies of Scientific Practice

These integrated projects, albeit not exclusively, can be found in works of members of the “Society for Philosophy of Science in Practice” (SPSP hereafter). This society’s manifesto contains an explicit statement of its aims and methods.

> If we are interested in exploring the assumptions and methods underlying the sciences, it is essential not only to explore the theories and results produced by scientists, but the processes by which they came to these conclusions. And what we learn from history of science is that scientific practices should be evaluated in their historical contexts reaching up to the present moment. Without excavating underneath the tidy surface of published papers or finalized theories, it is extremely difficult to identify these processes. SPSP is dedicated to fostering the pursuit of a philosophy of science that considers *theory, practice and the world simultaneously, and never in isolation from each other* (Ankeny *et al*. 2011, 304, own emphasis).

The first part of this quote primarily concerns practice-oriented projects. It emphasizes the methodological need to go beyond the analysis of scientific products to understand the processes shaping scientific practice. This need to “excavate underneath” what appears in scientific outcomes is a call, in philosophy of science, to integrate historical, sociological and anthropological studies of scientific practices. It corresponds to what Soler *et al*. characterized as a shift “from decontextualized, intellectual, explicit, individual and ‘purely cognitive’” understanding of scientific practice to a “contextualized, material, tacit, collective, and psycho-social” understanding of scientific practice (Soler *et al*. 2014, 20).

This kind of naturalistic practice-oriented projects is dedicated to using the best available scientific resources to deal with a given philosophical problem. By scientific, I do not only mean resources from the natural sciences as sometimes understood (and as approaches such as EEM primarily use), but also include historical, sociological or anthropological resources. As in naturalistic philosophy of biology, these resources provide the knowledge base on which philosophical analysis can develop.

One successful example of such a naturalistic study of scientific practice is Leonelli's study of the *Gene Ontology* database, which, she argues, requires "the integrated use of history, philosophy, and social studies of biology" to provide a "complex, multifaceted, and possibly more truthful analysis of what bioinformatics tools are and mean for contemporary biology" (Leonelli 2010, 121). Leonelli's method is, in her own terms, a form of "empirical philosophy of science"[13]. This type of approach, I think, is methodologically continuous with the naturalistic approaches outlined above, to the difference that it seems committed to using a broader knowledge base by drawing from a wider array of disciplines.

The last sentence of the quote from the SPSP manifesto contains a more radical philosophical statement. By arguing that "theory, practice and the world" must be studied simultaneously, they deny grounds to "merely metaphysical" and "merely practice-oriented" philosophical projects. On this quote, it is unwarranted to engage in metaphysical discussions that abstract from methodological considerations of a given practice. In this view, conceptual commitments and scientific results are embedded within a given scientific practice, and cannot be given proper meaning if the context in which these concepts are at play and these results are produced is backgrounded. In other words, scientific practice and scientific products are too entangled to be studied in isolation.

Existing work in philosophy of historical sciences provides cases of such an entangled methodology. All across her collection of essays on archaeology,

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[13] The goal of this way of proceeding "is to bring philosophical concerns and scholarship to bear on the daily practice of scientific research and everything that such practice entails, including processes of inquiry, material constraints, institutional settings, and social dynamics among participants. […] The methods used in this work range from argumentation grounded in relevant philosophical, historical, anthropological, and sociological literature to analyses of publications in natural science journals; consultation of archives documenting the functioning and development of biological databases; and multisided ethnographic explorations, on- and offline, of the lives and worlds that these databases create and inhabit" (Leonelli 2016, 6-7).

Wylie ties conceptual and methodological issues with the direct results they may have in practice (Wylie 2002). For instance, she discusses the influence of assumptions about gender roles on archaeological knowledge (Chapter 14, 185-199) or the ethical and epistemic risks associated with the interaction of archaeological practice with non-scientific commercial practices (Chapter 17, 229-246). In her latest book, in collaboration with Robert Chapman (Chapman and Wylie 2016), they cover issues on the constitution, use and re-use of evidence in archaeology. They also deal with the theoretical underpinnings behind the constitution of archaeological knowledge claims and how new evidence (from the world) is continually capable of reshuffling existing interpretations. In all of these cases, the studies of practice, theories and the world are intertwined.

The articles of a recent special issue in *Studies in History and Philosophy of Science* present a similar entanglement. For instance, Bromham looks at ways of testing hypothesis about macroevolution in evolutionary biology (Bromham 2016), O'Malley at the consequences of the increased methodological sophistication in evolutionary biology (O'Malley 2016) and Currie at the use of the "ethnographic analogy" for reconstructing the past of human societies (Currie 2016). In all three cases, the focus is not only on the methods of investigation but on the consequences that these methods have on the epistemic grip that historical scientists possess about the past and the concepts and theories that can be generated from them. In other terms, these instances do not present a parallel demarcation between philosophy of history and philosophy of historiography in the study of historical sciences. In this view, understanding scientific products and understanding how they are produced are two interrelated sides of the same inquiry.

In this dissertation I study evolutionary biology as a historical science by looking at its practices, its theories and their relation to the phenomena under study. It is an attempt to unpack the consequences of conceptual and methodological commitments for the way empirical evidence is handled and the type of scientific theories that are formulated. The methodology employed in this dissertation is an attempt to emulate the type of projects discussed above that are both metaphysical and practice-oriented. This commits me to a form of naturalism that addresses philosophical problems using the best possible resources at hand, and to let scientific *practice* (and not only its outcomes) shape as much as possible the philosophical questions and answers that I provide. At the same time, this work is a new contribution to the philosophy of historical sciences.

CONCLUSION

This introductory chapter aimed at doing three things. The first was to dissipate tensions at the intersection between history and science. Doing this, it clarifies the relation between historical sciences and history. I argued that the latter, sometimes defended as distinct because taking humans as its subject-matter, bears several methodological continuities with the former. Both these sets of investigation can, therefore, benefit from analyses of the concepts and practices of the other. Claiming methodological continuity between the study of the history of humans and study of the history of non-humans and non-living entities comes back to the genealogical relationship between these two practices. Our knowledge about the history of non-human entities, as Rudwick argued, originally stemmed from the extension of the methods to study the human past to the non-human past, including, for instance, the quest for "natural antiquities" in the 17th century (Rudwick 2014).

The second aim of this chapter is to devise an ideal philosophical methodology driving this dissertation. With philosophy of nature and philosophy of science in the context of science, and philosophy of history and philosophy of historiography in the context of history, analysts distinguished between two types of philosophical projects. The first is a metaphysical project ("nature" and "history) that aims at unravelling the deeper meaning of the outcome of scientific practice. The second is a practice-oriented project ("science" and "historiography") that attempts to understand the methods and processes of scientific knowledge production. I argued that both of these projects could be carried out with a commitment to a form of methodological naturalism. I retained this commitment to methodological naturalism, by endorsing a wide conception of what counts as a scientific resource (including historical and social sciences). However, I follow a third type of philosophical project, which attempts to consider metaphysical *and* practice-oriented questions at the same time.

That said; let's now briefly summarize how I plan to carry on this inquiry. This dissertation contains 6 additional chapters. The plan is to integrate more and more elements of scientific practice in the philosophical picture as the dissertation progresses. The project's initial driver is to understand how diametrically opposed accounts of the evolution of early forms of life on the planet, such as Tom Cavalier-Smith's and Bill Martin's, have been coexisting, and what were the benefits and the risks associated with such a situation.

Chapter 2 provides a description of Cavalier-Smith and Martin's hypotheses about the origin of eukaryotic cells. This chapter works as a basis for the case studies of the following chapters. Chapter 3 aims at describing the form of hypotheses in evolutionary biology. "Narrative explanations" and "ephemeral mechanisms" have been proposed as candidate epistemic tools. I adapt the concept of "lineage explanation", drawn from Calcott (Calcott 2009), to accommodate mechanisms and narratives as subtypes. This move allows me to characterize Cavalier-Smith and Martin's hypotheses as mixed lineage explanations, composed of both narrative and mechanistic elements. Chapter 4 is a study on the topic of representation: how do these evolutionary narratives represent the past? I propose reconciling Toon's direct and Frigg and Nguyen's indirect accounts of scientific representation by characterizing lineage explanations as *superficially* directly representing unique events. These explanations represent by displaying fictional truths about their targets and being capable of being used to generate further, implicit ones. After characterizing what counts as a representation of the past, I turn to more strictly methodological questions. Chapter 5 critically assesses the existing literature on the methodology of historical sciences. It provides a series of concepts and demands that can be put to use to understand specific cases in historical sciences. Chapter 6 draws inspiration from such concepts and demands to defend and apply a framework for evidential claims to the case of Archezoa, an initially supported and eventually rejected classification proposed by Cavalier-Smith. Chapter 7 looks back to the initial question by exploring the benefits and risks of the coexistence of contradicting hypotheses in historical sciences. It does so by focusing on questions of underdetermination and pluralism.

There are two things I want to address before closing this first chapter. The first is a matter of the scope of application of my analysis. As I mentioned in the opening words, the type of practice I am looking at does not represent the entirety of what's going on in evolutionary biology. There should, however, be convergences with any other practices that aim at reconstructing unique events from indirect traces. What I say, then, should be relevant to several other practices within historical sciences. It might also, as was noticed by Currie, bear commonalities with any other scientific practices that are in "epistemically precarious situations" (Currie 2018).

Finally, this dissertation is not based on empirical work I've carried out myself on practitioners of evolutionary biology. If the tape of my PhD were to be "replayed", this is definitely something I would change and something that I will look to include in my future research projects. Instead of this, my work's primary source is an extensive study of Cavalier-Smith and Martin's published work and a careful study of their methodological, conceptual and evidential choices. I tried, as much as possible, to have informal exchanges with scientists involved with this practice, including Cavalier-Smith and Martin themselves, and to expose some of my insights to their first-hand experiences. I hope that this blending of resources allows me to make an account that is not too far removed from actual scientific practice and, at the very least, that the claims I make can help to shape empirically tractable questions, for researchers either working on this case study or on other similar ones.

CHAPTER 2: CAVALIER-SMITH AND MARTIN ON THE ORIGIN OF EUKARYOTES

INTRODUCTION

The works of Tom Cavalier-Smith and Bill Martin provide this dissertation's central scientific resource. Both scientists are interested in reconstructing the "deep past" of life on Earth. Their work is historical: they infer about the key events marking the evolution of unicellular forms of life on Earth, from the origin of life to the diversification of unicellular eukaryotes. The two scientists I am focusing on have differing hypotheses on the events filling up this time span and divergent assumptions driving such reconstructions. The task of this dissertation is to disentangle as much as possible the various components of this process of scientific knowledge production.

This case study is not entirely philosophical *terra incognita*. O'Malley has already provided an extended and thorough survey of the disagreement on the origin and early evolution of eukaryotes (O'Malley 2010). In her analysis, Cavalier-Smith and Martin are pictured as the main proponents of two different types of evolutionary explanations (respectively, "autogenous" and "exogenous"). Her substantive reconstruction of the disagreement is explicitly helpful in three of the following chapters. It helps with the task, undertaken in this chapter, of recapitulating each scientist's proposed explanations. Her account of the rejection of the Archezoa hypothesis (O'Malley 2010, 216) is a valuable help to build my own study of this case, though my philosophical angle is different from hers. Her article also provides a summary of the theoretical, evidential and methodological lines of disagreement between these scientists. On Chapter 7, my discussion of scientific pluralism is fuelled by a narrower study of only one component of this disagreement (namely, the necessity of phagocytosis for eukaryogenesis). The rest of the chapters use this case study to fuel philosophical discussions on issues that O'Malley only sometimes implicitly addressed (i.e., the type of explanation employed, how these theories represent their target).

Before proceeding to further philosophical analyses, this chapter introduces the theories defended by Cavalier-Smith and Martin, focusing in particular on their conflicting accounts of the origin of eukaryotes. It is targeted to the reader unfamiliar with the intricacies of evolutionary biology and avoids having to re-explain Cavalier-Smith and Martin's work from scratch in each subsequent chapter. In addition to this, words marked with an asterisk are defined in a glossary available at the end of the dissertation.

Please note that this chapter does not aim to provide an assessment of the quality of these two competing hypotheses, but instead is purely focused on theoretical content. At no later point does in this dissertation I attempt to adjudicate between Cavalier-Smith and Martin. Later chapters, however, dwell further on the evidence and assumptions mobilized in favour of both sides and other details of their practices.

Martin has worked on the issue for more than 20 years and Cavalier-Smith for more than 40 years. Unsurprisingly, then, a lot of alterations have been made to their hypotheses (this is particularly true of Cavalier-Smith). In this chapter, I try to stick as much as possible to the most "up-to-date" version of their claims. The contents provided here are therefore likely to be outdated (or severely incomplete) in the space of months or years. This usual caveat of studies of science in the making, I think, should not overly affect the credibility of my analysis of this case. This is because I believe that while theoretical *contents* might (and will) change, the practices underlying this knowledge production should remain more or less the same. The quality of the philosophical claims I make in the following chapters is, I argue, partly independent of the stability of the hypotheses I present in this chapter.

As said above, this chapter mainly focuses on the origin of eukaryotes. It also briefly discusses elements of hypotheses from Cavalier-Smith and Martin about other past events when deemed relevant. Please note that at no point does it aims to be an exhaustive summary of these scientists' work.

I. William Martin

A. In previous episodes

There are, I think, a few key things to know about Martin's views on events preceding the origin of eukaryotes. They help to illuminate aspects of his account of eukaryogenesis and differences with Cavalier-Smith's claims.

It is relevant to know that he views the origin of life as the origin of *both* archaea *and* bacteria, both stemming from proto-living self-replicating colonies formed in hydrothermal vents (Martin and Russell 2003; Koonin and Martin 2005). In this view, Earth was therefore already populated, *ca*. 3Bya, by the two main types of prokaryotes. Both of these pioneering lineages had a vast array of ecological niches to populate. This resulted, in these organisms, in the quick emergence of a vast metabolic diversity, leading Martin to affirm that “most of the biochemical pathways that drive modern prokaryotic carbon, sulphur and nitrogen cycles were in place by as early as 3.5 Gya [billion years ago], by 2.7 Gya at the latest” (Martin *et al*. 2003, 194).

Among these metabolic options, oxygen production by photosynthesis is argued to have emerged “by at least 2.7 Gya” (Martin *et al*. 2003, 194). Oxygen production by cyanobacteria, however, mostly affected the atmosphere and superficial oceanic layers. The deeper strata of the oceans, in this view, remained anoxic “until *ca*. 600 Mya [million years ago]” (Mentel and Martin 2008, 2724). The environmental backdrop to the origin of eukaryotes, which purportedly occurred *ca*. 1.5 Gya in the depth of the oceans, is therefore resolutely anoxic.

Before discussing the origin of eukaryotes, another important piece of information concerns Martin’s views on what drives prokaryotic evolution. On his view, these changes are driven by processes of lateral (or horizontal) gene transfer (LGT/HGT). They are conceptually opposed to traditional means of genetic inheritance, especially in multicellular organisms, which are going in a vertical direction from parent to offspring. These horizontal processes include “transformation”, the uptake and incorporation of exogenous genetic material found in the surrounding environment; “transduction”, a transfer of genetic material from a bacterium to another mediated by bacteriophages* and “conjugation”, direct transfer of small circular pieces of DNA, called plasmids, from a donor cell to an acceptor through direct cell-to-cell contact. These processes allow for transfers of genetic material across species boundaries. As a result of these processes, prokaryote evolution is marked by numerous genetic reshuffling episodes and cross-species genetic exchanges.

B. The origin of eukaryotes

The origin of eukaryotic cells occurred 1.5 Gya in the depth of an anoxic and sulphidic ocean. This event generated the last common ancestors of all cells with a nucleus, and all multicellular forms of life, including us. This dramatic episode of evolution is accounted for by the “Hydrogen hypothesis”, a scenario formulated in 1998 by William Martin, in collaboration with Miklós Müller (Martin and Müller 1998). To talk of the origin of eukaryotes is to talk of the emergence of a series of eukaryotic traits: the nucleus, mitochondria*, the cytoskeleton*, phagocytosis*, the endomembrane system* and large genomes charged with introns* to name a few. Debates around these issues often boil down to matters of timing, to the order of appearance (and more importantly, the reasons behind this order) of the aforementioned structures. In Martin's case, mitochondria are argued to come first and to underpin the origin of all of the other eukaryotic structures.

As the name suggests, hydrogen is the key to the emergence of eukaryotes, "[i]t is the bond that forges eukaryotes out of prokaryotes" (Martin and Müller 1998, 40). Hydrogen is what drove a set of methanogenic archaea to interact with free-living, metabolically versatile alphaproteobacteria. Methanogens "live by reacting hydrogen gas with carbon dioxide, and evanescing methane gas as a waste product" (Lane 2005, 52)[14] and are thus confined to anaerobic environments. There is, then, an advantage for methanogens that can live in the vicinity of facultatively anaerobic alphaproteobacteria which produce hydrogen as a waste product of their anaerobic metabolism.

This close proximity induces a change of shape of the methanogens. They surround hydrogen-producing alphaproteobacteria, in order to "reap the greatest benefit from them" (Martin and Müller 1998, 39). This tight embrace progressively turned into physical encapsulation. These alphaproteobacteria are now engulfed organisms found within the walls of methanogens-turned-hosts. The following steps in this narrative are successive problem-solving episodes to transform this initially ill-functioning collaboration into a fully-functioning host/endosymbiont association. As repeatedly stated by Martin to defend this scenario, its steps do not require major evolutionary inventions, but "merely genetically rearranging pre-existing components" (Martin and Müller 1998, 39). These genetic transfers require the intervention of various episodes of LGT.

[14] Note that Lane, who collaborated extensively with Martin, wrote books destined to lay audiences that formulate some of these ideas. I used these sources as an inspiration for Martin's account in this chapter.

Once inside the methanogen host, the engulfed alphaproteobacteria do not have access anymore to the requisite nutrients for survival found in the surrounding environment. Some of the intracellular alphaproteobacteria might even not have survived from it. The situation is equally problematic for methanogens, as this interrupts their vital hydrogen consumption. As alphaproteobacteria starve and die into their hosts' cytoplasms*, alphaproteobacterial genes are released in the environment. Some of these genes are then incorporated in the methanogen's chromosome by transformation. Luckily, among these genes are the ones coding for proteins that enable the host to ingest organic compounds from the environment. To solve this situation, then, all that is needed is for these genes to be expressed by the methanogens, so that "the host would in principle be able to feed its symbiont with organics and thus feed itself with H_2 and CO_2" (Martin and Müller 1998, 39).

However, another issue arises, in the form of a "tug-of-war" for imported goods. This conflict arises because of the polarized natures of the host and engulfed organism's metabolisms. "The trouble was that methanogens normally use glucose to build up complex organic molecules, whereas the alphaproteobacteria break it down for energy" (Lane 2005, 60). The solution was to find a way to fundamentally invert the metabolism of the methanogen, by turning this autotrophic* organism, mainly a "producer" of organic molecules, into a heterotrophic* one, a "consumer" of these same molecules. After acquiring the endosymbiont's carbohydrate metabolism via LGT, it is necessary for the host, to avoid possessing mutually negating ("futile") metabolisms, to abandon its original autotrophic metabolism. The host can now import and ferment glucose, and the symbiont could use some of these breakdown products to generate ATP*.

The resulting host/engulfed organism association leads, in the words of Martin and Müller, to a "curious situation.

> The selective pressure that associated the partners from the start and that drove the integration of eubacterial genes into archaeal chromosomes was the host's strict dependence upon hydrogen produced by the symbiont. But by transferring the symbiont's importers and

> glycolysis to the [cytoplasm] in order to satisfy that dependence, the host suddenly can meet both its carbon and energy needs from organic substrates. The functions of both methanogenesis and autotrophy* have been replaced, and there is no obvious selective pressure to retain either. The host has irreversibly become heterotrophic, and hydrogen is once again a waste product, but now of a compartmentalized metabolism (Martin and Müller 1998, 40).

The various conflicts solved and transfers of metabolic pathways indeed led to a complete reshuffling of the host's initial functioning and the motive behind their initial association has, at this stage, become irrelevant to the sustainability of the interaction. The last step on the way to a fully functioning association is to "plug" an ATP transfer protein in the endosymbiont's membrane so that the methanogen can import some of the ATP produced by the endosymbiont and compartmentalize the production of energy for the whole cell within the endosymbiont.

As a result, Martin and Müller conclude that

> That cell has time, energy and ample genetic starting material (two highly divergent and partially merged prokaryotic genomes) to evolve cytological and genetic traits that are specific to the eukaryotic lineage (Martin and Müller 1998, 41).

The endosymbionts subsequently lose superfluous genetic material and become specialized in the performance of a narrow set of functions. It becomes specialized, in particular, in energy production. These endosymbionts are the common ancestors of all of the forms of mitochondria: the "classic", aerobic, one known as mitochondria; the anaerobic form known as hydrogenosomes*; and the "reduced" version known as mitosomes*[15]. To make this possible, the newly made endosymbiont must not have lost its metabolic versatility in the process, especially its elements of aerobic metabolism, despite the fact that the emergence of eukaryotes occurred at a time of widespread anoxia and thus of disuse of this particular function. The sustained existence of facultatively

[15] Mitosomes were discovered after the publication of the hydrogen hypothesis (Tovar *et al.* 1999).

anaerobic, versatile, metabolisms is considered by Martin to be a rather common state of affairs in marine environments**.**

In parallel to metabolism, another, more silent, revolution occurred and requires explanation. The host at the origin of eukaryotes is archaeal, and therefore initially possesses typically archaeal lipids*. However, a eukaryotic lipid bilayer is made of typically eubacterial lipids. Somewhere in the process, eubacterial lipids must have replaced archaeal lipids on the plasma membrane. To do this, the lipid synthesis pathways that were operating in the endosymbiont must have been successfully transferred to the host.

The explanation favoured by Martin, elaborated in detail with his colleagues Gould and Garg (Gould *et al.* 2016), manages to cover both this membrane changeover and the emergence of the endomembrane system. The key to this scenario is that prokaryotes are known to secrete outer-membrane vesicles (OMVs), small lipidic vesicles involved in multiple roles such as inter-cellular communication and trafficking of molecules. Once inside the host, the endosymbiont's OMVs had two choices.

> They can fuse, either with themselves to generate larger vesicular compartments, or with the plasma membrane to export their contents to the cell exterior. The former generates a basic [endoplasmic reticulum* (ER)] topology. The latter constitutes, we propose, the ancestral outward state of eukaryotic membrane flux, and furthermore converts the chemical composition of the host's plasma membrane from isoprene ethers to bacterial fatty acid esters (Gould *et al.* 2016, 3).

Therefore, it is this "continuous flow of bacterial lipid OMVs to the archaeal plasma membrane" that "would have naturally transformed the lipid composition of the archaeal plasma membrane from ether-linked isoprenes to ester-linked fatty acids […]" (Gould *et al.* 2016, 5). The endomembrane system would have emerged by the fusion and specialization of such OMVs.

The secretion of OMVs is also what provided the primary material for the origin of the nucleus, even though the nucleus emerged out of quite distinct selection pressures (Gould *et al.* 2016, 2, Box 1). Within the acquired alphaproteobacterial genome were found mobile genetic elements, named “group II introns”, intercalating between other genetic elements and capable of autocatalytic* activity. These mobile elements hitchhiked with other alphaproteobacterial genes and invaded the archaeal host genome. From group-II introns evolved spliceosomes (Martin and Koonin 2006, 43), protein complexes involved in maturing mRNA* by removing (“splicing”) introns from it.

As compared with translation*, splicing is prohibitively slow, this runs the risk of genes being translated into proteins before splicing has been finished, something characterized as

> an extremely unhealthy situation because few functional proteins will ensue, and the prospects of any descendants emerging from this situation are bleak (Martin and Koonin 2006, 43).

The solution favoured by Martin and Koonin involves the emergence of an evolutionary innovation to

> physically separate splicing from translation, allowing the former (slow) process to occur to completion first, before the latter (fast) process sets in. Physical separation in cells usually entails membranes, so [this solution] would involve the invention of a membrane separating splicing from translation, with pores sufficiently large and selective enough to export matured ribosomal subunits, mRNA and tRNA* (Martin and Koonin 2006, 43).

Being contiguous and functionally tied to the ER, the nuclear envelope is argued to have emerged from the ER, as the ER itself emerged from OMVs (Gould *et al.* 2016, 7).

The appearance of mitochondria results in the compartmentalization of energy production and provides configurations allowing the emergence of the endomembrane system and nucleus. This is, according to Martin, a sufficient starting point for the emergence of the more complex eukaryotic features.

What is it that makes it possible for eukaryotes to have evolved more complex forms of life? According to Lane and Martin,

> [v]irtually every 'eukaryotic' trait is also found in prokaryotes, including nucleus-like structures, recombination, linear chromosomes, internal

> membranes, multiple replicons, giant size, extreme polyploidy, dynamic cytoskeleton, predation, parasitism, introns and exons, intercellular signalling (quorum sensing), endocytosis-like processes and even endosymbionts. Bacteria made a start up virtually every avenue of eukaryotic complexity, but then stopped short. Why? (Lane and Martin 2010, 929)

The bottom line of this inability is, in their opinion, that prokaryotes cannot afford the bigger genomes required to evolve complex traits. Large amounts of DNA, in themselves, are chemically inert and do not cost a lot to sustain, but the production of proteins mobilizes about 75% of the cell's energy budget. To have more complex traits, therefore, requires the expression of more genes, and expressing more genes requires more energy.

Energy synthesis, in prokaryotes, occurs at the plasma membrane. To synthesize more energy, therefore, would require increasing the surface dedicated to energy synthesis on this very membrane. The problem with this is that scaling-up also increases the volume of the cell, therefore the volume using the energy produced by membranes. Because the volume increases faster than the surface if bacteria increase their size, the energetic efficiency would actually decline as the size increases. In a competitive environment in which slight differences in efficiency determines who survives, bacteria face pressure to keep their size and their energetic expenses as low as possible, making it impossible for them to afford a massive gene expansion.

The acquisition of mitochondria is the key to the release of this selection pressure and allowed the emergence of eukaryotic complexity. Instead of having energy production located at the outside of the membrane, eukaryotes possess, with mitochondria, a compartmentalized way to synthesize energy. They are now liberated from the diktat of the surface-to-volume ratio,

> internalization releases the eukaryotic cell from the geometric constraints that oppress bacteria. Eukaryotes are on average 10 000 to 100 000 times the volume of bacteria, but as they become larger, their respiratory efficiency doesn't slope off in the same way. To increase energetic efficiency, all that eukaryotic cells need to do is to increase the surface

> area of mitochondrial membranes within the cell; and this can be done simply by having a few more mitochondria (Lane 2005, 125-126).

As a consequence, eukaryotes are much more energetically efficient. They have been calculated to afford a "roughly 200,000-fold rise in genome size" (Lane and Martin 2010, 929)[16]. This is all that is required for eukaryotes to "evolve, explore and express massive numbers of new proteins in combinations and at levels energetically unattainable for its prokaryotic contemporaries" (Lane and Martin 2010, 933). The emergence of eukaryotes is on its way, and this is where I stop.

II. THOMAS CAVALIER-SMITH

A. IN PREVIOUS EPISODES

A crucial difference between Cavalier-Smith and Martin lies in their views about the origin of life. According to Cavalier-Smith, the origin of life bears witness to the emergence of bacteria but *not* archaea. Archaea is argued, instead, to have appeared roughly simultaneously with eukaryotes, as another outcome (as seen below) of the same event.

Martin's hypothesis is primarily (but not exclusively) centred on questions of metabolism, biochemistry, and the relation of cells to their (changing) environments. By contrast, Cavalier-Smith pieces together hypotheses that (again, not exclusively) articulate a large array of knowledge in cellular biology. Particularly salient is his emphasis on the changes in membranes, as they are conceived as the primary drivers of major evolutionary events in prokaryote and early eukaryote evolution. As a result, Cavalier-Smith's account of bacterial evolution centres on the origins of single and double-membranes in bacteria.

B. THE NEOMURAN REVOLUTION AND THE ORIGIN OF EUKARYOTES

The origin of eukaryotes, in terms of timing, brings an end to the "boring billion", a relatively uneventful period of bacterial evolution. This relative boredom was however not without explanation: the next exciting evolutionary events were simply very hard to realize (Cavalier-Smith 2014, 41; 2006, 998). The set of events at the origin of the archaeal and eukaryotic lineages are called the

[16] As the "roughly" suggests, this is not meant by these authors to be a precise estimation.

“neomuran revolution”. As the name suggests (“neomura means “new walls” in Latin), it involves a radical change in membrane composition. This was about 1-1.5 billion years ago (Cavalier-Smith 2014, 66).

The bacterial cell wall is formed of a “covalently cross-linked bag” of peptidoglycan* murein, “completely surrounding the cytoplasmic membrane” (Cavalier-Smith 2014, 43). The neomuran revolution started with the accidental loss, by an actinobacterium, of its peptidoglycan cell wall “to become naked L-forms” (Cavalier-Smith 2014, 51). This destabilized these cells’ general functioning. From this situation emerged a new type of membrane coat (the “new wall”) based on *N*-linked glycoproteins*. The new *N*-linked glycoproteins are not cross-linked, but merely linked to residues of asparagine amino acids (*N*) attached to oligosaccharides*. This provides a more flexible configuration that both lineages at the origin of eukaryotes and archaea exploited differently. These lineages, independently, surrounded this new basic structure with different types of lipid membranes, at the origin of two radically novel, contrasting lifestyles.

Archaea, on one hand, evolved a new type of lipids (isoprenoid ethers) to colonize extremely hot environments and effectively becomes the first set of hyperthermophiles*. Becoming specialists to a given environment, archaea, in their evolution, “lost so many lipids and proteins that they could never have evolved directly into eukaryotes” any more (Cavalier-Smith 2014, 42-43, figure 1). Later in their evolution, they reverted to more mesophilic* lifestyles by acquiring genes by LGT from bacteria. Apart from their change of environment and membrane, the emergence of archaea did not come with the emergence of a new “cell structure, growth, division and genetics”, remaining on the whole “fundamentally bacterial or prokaryotic” (Cavalier-Smith 2014, 54). This justifies why Cavalier-Smith considers them as a relatively young form of bacteria rather than a wholly separate domain of life[17].

[17] Notice that archaea’s relative young age is inferentially independent from their phenotypic proximity to bacteria. Even if they were shown to originate farther in time, this would still not legitimate, in Cavalier-Smith’s view, a consideration of this group of organisms as a separate domain. This runs against views that contest the legitimacy of the notion of “prokaryotes” on the

Eukaryotes, on the other hand, “arose by exploiting the new flexible glycoprotein surface” of the cell to evolve phagotrophy, a metabolism based on the consumption of external foodstuff acquired via phagocytosis (Cavalier-Smith 2014, 48–49, figure 3). The evolution of phagotrophy depends on the development of a fully-fledged cytoskeleton, providing both the requisite structural integrity and flexibility. This new capacity to internalize food from the outside allowed the emergence of a system of functional compartments within the cell, the endomembrane system. The capacity to ingest other organisms is also what made it possible for this proto-eukaryote to ingest and fail to digest an aerobic alphaproteobacterium, which was progressively enslaved into the ancestor of mitochondria.

The primary payoff of the evolution of mitochondria is in the improvement it provides in the “aerobic utilization of intracellular digestion products” (Cavalier-Smith 2014, 51). This increased efficiency is not the result of the sole presence mitochondria. It rather stems from a new, multicomponent, and eukaryotic-specific division of energetic labour. The endoplasmic reticulum* (ER), mitochondria and peroxisomes* together constitute an “energy belt” argued to have originated more or less simultaneously. Peroxisomes generate breakdown products of the lipid metabolism that are sent to mitochondria which generate ATP from these products, and this ATP is in turn exploited by the ER to synthesize novel proteins and cellular components.

Another consequence of the formation of the cytoskeleton and the endomembrane system is the evolution of the nucleus. The cytoskeleton is, roughly speaking, composed of a network of actin* filaments and microtubules*. This structural network mediates all of the cellular movements, from vesicle transport to changes of shape, and does this through continuous assembly and disassembly as well as from the action of various cellular “motors” such as myosin, dynein and kinesin. While this greatly diversifies the possible cellular movements, this also exposes cellular structures to potential damage from the movements of these same motors. This risk particularly applies to the fragile

basis of the evolutionary and phenotypic distinctness of bacteria and archaea (Pace 2006).

genetic material and is at the origin of the nucleus that initially evolved to protect DNA “from shearing damage” (Cavalier-Smith 2014, 44).

These various innovations – the rise of a cytoskeleton, the different elements of the endomembrane system and the ability to phagocytise – are at the origin of an inversion in the selective pressures affecting genome size. These different components allowed for an increase in cell size, which “imposed upward coevolutionary pressures on genome size” and increased nuclear volume with it (Cavalier-Smith 2014, 15). This order of events runs contrary to Martin’s hypothesis. Here, changes in membrane structure allowed for the origin of phagocytosis, which allowed the ingestion of mitochondria and the emergence of the endomembrane system, ultimately increasing cellular volume, which released the selection pressures that kept genomes small, allowing for the evolution of the rest of the eukaryotic features. This is where I stop for Cavalier-Smith’s hypothesis.

CONCLUDING WORDS

After this presentation of Cavalier-Smith and Martin’s hypotheses for the origin of eukaryotes, I now have the scientific basis on which cases studies from next chapters are based. Chapter 3 starts this investigation by asking an apparently basic question: what kind of theories are these hypotheses?

CHAPTER 3: THEORIES ABOUT UNIQUE EVENTS

INTRODUCTION

In the previous chapter, I provided a summary of the hypotheses presented by Martin and Cavalier-Smith about the origin of eukaryotic cells. This chapter presents the first philosophical investigation inspired by this case. I propose an exploration of the *type* of hypotheses presented by Cavalier-Smith and Martin. To what category do they belong?

The hypotheses presented by Cavalier-Smith and Martin are attempts to explain *unique events* in the past. There are several ways to understand ”unique” here. There is a “trivial” understanding of uniqueness which considers things to be unique when there is nothing *exactly* like them, in the sense of sharing exactly the same components, properties and spatiotemporal location. This ontological understanding grants that everything that happens is unique. For instance, my present typing on the keyboard at 9:23 am on the 23rd of November 2017 in my flat in the 18th *arrondissement* of Paris will never be *exactly* replicated. At the epistemological level, however, the uniqueness of this action is much less obvious. This event can be described as belonging to many different types of activities: typing on a keyboard, writing a PhD dissertation, inhabiting Paris… Even though everything occurring is unique in a very literal sense, there are many ways, at the descriptive level[18], with which this uniqueness can be decreased.

This brings me to another understanding of uniqueness. Unique events are ones *described* as tokens that cannot be subsumed under a type. This epistemic understanding is the one I endorse in this chapter. Unique events are both *perceived* as having happened “once and only once” (Tucker 1998, 63) and *investigated as unique*, not as a type of events. Here, I am not focused on whether this granted explanatory uniqueness is legitimate or temporary. It might be the case that events such as the origin of eukaryotic cells are better studied as a type of event rather than as a token. In this latter case, future

[18] This discussion of uniqueness focuses on an *epistemological* understanding of uniqueness. I avoid, for lack of time and knowing, delving further into metaphysical understandings of uniqueness.

investigations will be able to develop a broader category of events in which the origin of eukaryotes belongs[19]. In all cases, this chapter deals with events that are currently explained as unique.

The explanation of unique events has sometimes been considered as a special scientific task. Windelband famously separated between ”nomological” and “idiographic” sciences, the former being concerned with “what is invariably the case” and the latter with “what was once the case” (Windelband 1980, 175). On a strong reading of the demarcation, there would be, on the one hand, explanations from the nomothetic sciences in the form of laws of nature. What, on the other hand, are the sorts of explanations generated by idiographic sciences? This chapter searches for answers to this question[20].

The first part of this chapter reviews existing positions on the subject and identifies current candidates for explanations of unique events. The best-known candidate is the notion of *narrative explanation*, a form of explanation tightly associated with historical investigations. An alternative to narrative explanations, *ephemeral mechanisms*, has been proposed by Glennan (Glennan 2010) as an outgrowth of the philosophical studies about mechanistic explanations. After discussing both candidates, I identify the points of convergences and of tensions between them. Despite both standing out as alternative strategies to law-based explanations, I argue that the main tensions between narrative and mechanistic explanations revolve around how much room is left for contingency and on how to identify explanatorily relevant components. Currie proposed a strategy for reconciling both types of explanations by making them relevant to a complementary range of events

[19] This is already partly the case with the origin of eukaryotes and other landmark events in the evolution of life on Earth, which are studied as “major evolutionary transitions”. Several studies (i.e., Maynard-Smith and Szathmáry 1997) provide a characterization of the common traits of all these transitions. The authors of these studies, to my knowledge, are not trying to replace the study of these events in their individuality.

[20] Note that the following arguments do not hinge on the degree of validity of Windelband’s distinction.

(Currie 2014). In this view, ephemeral mechanisms are *simple* narratives, unique events explained by an appeal to a general model. *Complex* narratives, equivalent to narrative explanations, are appealed to when no such model can be produced[21].

If both explanatory strategies can be applied complementarily in different cases, can they also be made complementary *within* a given explanation? In other words, can specific aspects of narratives and mechanisms be fruitfully incorporated into one type of explanation[22]? As a way to positively answer this latter question, I propose Calcott's concept of *lineage explanation* (Calcott 2009) as a potential candidate for this task. I first argue that ephemeral mechanisms and narrative explanations can successfully be described as subtypes of lineage explanations. I then propose a conciliatory strategy in which, in principle, elements of narrative explanations and of ephemeral mechanisms can be put to work within a lineage explanation.

The third part assesses if lineage explanations can be useful to describe Cavalier-Smith and Martin's hypotheses. This application bears two argumentative roles. I already mentioned the importance of showing the fruitfulness of lineage explanations to the explanation of unique events. The second is to assess whether, contrary to the initial opposition of these two epistemic tools, lineage explanations, in this case, combine mechanistic and narrative elements. I argue that lineage explanations provided by Martin and Cavalier-Smith are, for the most part, closer to narratives than mechanisms. Interestingly, they prove to be mixed in another way: they combine both tidily ordered sequences of events with more messy explanations, invoking events sometimes running parallel to each other, sometimes devised to branch at loosely determined moments of the explanation. The conclusion gathers this

[21] Note that Currie does not completely argue for mutual exclusivity. In his view, complex narratives are not used *only* in cases when it is not possible to generate simple narratives.

[22] In this view, ephemeral mechanisms and narrative explanations would not be full explanations, but considered as explanations of specific sub-parts of the event in question.

chapter's main insights and reflects more broadly on possible roles for mechanistic explanations in historical sciences.

I. Narrative explanations and ephemeral mechanisms

The first part of this chapter starts off with a brief characterization of the main features of the two candidate epistemological tools for the explanation of unique events, starting with narrative explanations.

A. Narrative explanations

Narratives are traditionally associated with the activity of telling stories. A narrative is often seen as an entertaining product of our imagination, or as an organized display of a collection of facts as someone in the course of a conversation recalls them. To distinguish this everyday and literary usage of narratives from its usage in scientific theories, the latter is described as *narrative explanations*. The formal requirements for narrative explanations are usually quite minimal. To provide a narrative explanation is to provide a "logically consistent representation of at least two asynchronous events" (Prince 2008, 19, cited in Beatty 2016, 33; see also Danto 1962, 146). Narrative explanations are also characterized by Currie and Sterelny as "an explanation which follows the causal trajectory of [something's] origin and subsequent history" (Currie and Sterelny 2017, 1). Three uncontroversial features emerge out of these early remarks. (1) Narrative explanations have a *subject* (more on the nature of the subject in an instant). (2) They track the development of this subject in a series of events that develop over time. (3) The series of events provided by narrative explanations are internally consistent.

According to Mink, narratives do not merely pull together a series of events and actions. Their primary role is to present a way in which these events *fit*, or are "configured", together (Mink 1970). On a similar line, Morgan insists that

> It is the ability and facility to order materials and weave them together to form explanations – regardless of whether the warp is a time thread, or a space thread, or a theoretical and conceptual thread – that characterises narratives (Morgan 2017, 87).

The subject of the narrative is what provides the ordering. It provides the thread that enables picking up, in the diversity of available things, the features relevant to the explanation. This has been conceptualized by Hull in terms of the notion of *central subject*. Central subjects are threads around which narrative explanations are built. An organizing principle, “the role of the central subject is to form the main strand around which the historical narrative is woven” (Hull 1975, 255). As Morgan and Hull emphasize, the nature of central subjects can be varied: Napoleon is an example, the extinction of the dinosaurs another, and the French revolution can also be a central subject.

What matters to central subjects, however, is that they display sufficient continuity in the span captured by narrative explanations. This condition, according to Hull, ensures the cohesiveness of these explanations. Continuity, here, is not to be confused with identity. Hull insists that “no degree of similarity between earlier and later stages in [the development of the central subject] is required, as long as the development is spatiotemporally continuous” (Hull 1975, 256). Central subjects, then, either “persist unchanged or develop continuously through time” (Hull 1975, 255). For instance, biological species are conceived as lineages of individuals sharing a common descent from a last common ancestor, populations of changing sizes, with its members both sharing a series of traits in common and gradually accumulating variations by evolution. They are, in paleobiology, central subjects of narrative explanations addressing the changes of morphology of their members, as well as their extinction. Narrative explanations, in this view, track the events and factors behind the stability or changes of such central subjects.

In attempts to legitimate narrative explanations as epistemological tools, a particular feature concerning the *nature* of events, best captured by narrative explanations, is often invoked. Narrative explanations are argued to be particularly suited to track *contingent* events. In Sterelny’s definition of contingency, “a change in a system is contingent if it could not be predicted from information about the prior state of that system” (Sterelny 2016, 522). The most famous defence of the pervasiveness of contingency in the study of the past is attributed to Gould’s “replaying life’s tape” argument, formulated in the context of palaeontology. Assuming you could “press the rewind button and, making sure you thoroughly erase everything that actually happened, go back to any time and place in the past” and then press play again, Gould affirms that “any replay of the tape would lead evolution down a pathway radically different from the road actually taken” (Gould 1989, 51). In other words, the history of life is contingent since its course could not be determined from a given point in the past. Scrutinizing Gould's argument, Beatty distinguished between two possible meanings for contingency in this context. The occurrence of an event is *contingent per se* if "it was not necessary, not bound to occur; it was possible, but there were other possibilities; it was a matter of chance". However, an event is *contingent upon* another when "it depended on that other event occurring; that other event was necessary for its occurrence" (Beatty 2016, 36).

Events that are contingent *per se* (henceforth contingent *tout court*) are considered by Beatty as turning points. They are events in the past that were "not bound to happen, but did" (Beatty and Carrera 2011, 495). They constitute junctions at which several courses of events were open and possible, and where only one of them was chosen. What is the relation between contingency and narrative explanations? Beatty argues that contingent events "are what make narratives *worth* telling. *Indeed, turning points make narrative essential*" (Beatty 2016, 37). On this reasoning, a series of causally related events where no alternative path was available is not worthy of a narrative explanation. Narrative explanations instead track series of events where "we need to be told what will happen next because we wouldn't know otherwise" (Beatty 2017, 35).

Narrative explanations, to summarize, are woven around a central subject, a thread that underlies the unity and continuity of the events described. This type of explanation is particularly suited to cases where the events described are contingent, when they could not have been anticipated from the information provided about the initial state. Here, I assume a weaker, epistemological, understanding of contingency, close to Sterelny's formulation above, rather than a strong version grounded on a defence of an ontologically indeterministic world. Focusing on contingency proves useful to compare narrative explanations with ephemeral mechanisms. The next section articulates further the latter concept.

B. Ephemeral mechanisms

This section begins with a characterization of the main tenets of philosophical investigations into mechanistic explanations. Of course, proponents of this approach are diverse and they disagree on several non-trivial aspects (see Nicholson 2012 for a review). This characterization is therefore far from exhaustive but is simply meant to introduce the conceptual framework behind ephemeral mechanisms.

Mechanistic explanations[23] account for the behaviour of *systems*. Systems are spatially and temporally delineated. Mechanistic explanations describe systems as composed of organized and interacting parts that together bring about behaviours of interest. Mechanistic explanations, thus, aim at an epistemic *decomposition* of the system of interest into its salient components and at identifying the relevant interactions between these components. A mechanistic explanation also unravels how a specific *organization*, spatial and temporal, of these entities and activities enables the occurrence of the behaviour of interest. Mechanistic explanations follow a sequential, continuous start-to-end causal sequence. Traditionally, this epistemological tool has been developed to explain *types* of behaviours. Once the components, interactions and organization of a system are successfully described, Machamer, Darden and Craver argue that "mechanisms are regular in that they work always or for the most part in the same way under the same conditions" (Machamer *et al.* 2000, 3). At first sight, mechanistic explanations are not the best-suited epistemological tool to deal with unique events.

Glennan, however, recently defended the relevance of mechanistic explanations for historical hypotheses, by bringing forward the concept of *ephemeral mechanism* (Glennan 2010). The notion of ephemeral mechanism retains the majority of the components of traditional mechanistic explanations. Ephemeral mechanisms still define their *explanandum* as a system that produces the behaviour of interest in virtue of being composed of a series of interacting parts. In Glennan's terminology, the behaviour of these parts follows *change-relating generalizations*. A change-relating generalization "describes a relationship between two or more variables in which an intervention that changes one variable will bring about a change in another variable" (Glennan

[23] In this chapter I take Nicholson's point that mechanistic explanations are best conceived *epistemologically* as "heuristic models which target specific causal relations and thereby facilitate the explanation of the particular phenomena scientists investigate" (Nicholson 2012, 154), and not *ontologically*.

2002, 345). Change-relating generalizations ground the regularity of mechanistic explanations.

Ephemeral mechanisms are invoked to explain unique events. This uniqueness, according to Glennan, is grounded on the rarity of the *initial configuration* of the parts. Putting it together, ephemeral mechanisms are "a collection of interacting parts where:

1. The interactions between parts can be characterized by direct, invariant, change-relating generalizations.
2. The configurations of the parts may be the product of chance or exogenous factors.
3. The configuration of parts is short-lived and non-stable, and is not an instance of a multiply-realized type (Glennan 2010, 260).

The initial conditions described for ephemeral mechanisms, then, cannot easily be replicated and therefore the event they bring about is to be considered unique. Glennan highlights that this set-up can be improbable, unstable, and also involving components that are themselves not instance of types. An example that covers these three elements is the one provided by Glennan: the death of French philosopher Roland Barthes. In Glennan's description, "Barthes had been invited to a luncheon with then [French] president François Mitterrand, and was struck by a laundry truck while crossing a Paris street on his way home" (Glennan 2010, 260). It is rather improbable that pedestrians cross streets unaware of a truck coming in. This configuration is short-lived: would Barthes be crossing a few seconds earlier or later this initial configuration would indeed have been lost. And the description involves Roland Barthes, an individual that is here not described as an instance of a kind. For these reasons, the initial configuration of the mechanism that explains Barthes' untimely encounter with a laundry truck was ephemeral, and this underlies this event's uniqueness.

However, Glennan argues that once the ephemeral configuration is described, the behaviour of the parts is subject to a set of change-relating generalizations. In Barthes' case, "we can describe the interaction between Barthes and the laundry truck as an instance of change-relating generalization involving persons and laundry trucks, or persons and large vehicles" (Glennan 2010, 261).

Ephemeral mechanisms, then, explain unique events by simultaneously identifying the unlikely initial circumstances at the origin of events of interest and by understanding the series of interactions between these components, driven by change-relating generalizations, which brought the outcome of interest. According to Glennan, the difference between traditional mechanisms and ephemeral mechanisms lies in the contingency of the initial configuration, not in the behaviour of the parts of the system, since "[t]he same sorts of generalizations which characterize the interactions between parts of ordinary mechanisms also characterize interactions between the parts of ephemeral mechanisms" (Glennan 2010, 261).

Even though I think ephemeral mechanisms and narrative explanations have interesting common features, it is not possible to characterize one as a subset of the other. The next section details the respective convergences and dissimilarities between the two.

C. Convergences and tensions

A shared feature of narrative explanations and traditional mechanisms, thus *a fortiori* of ephemeral mechanism, is that they stem from attempts to provide alternatives forms of scientific explanations. In particular, both were partly developed in reaction to the deductive-nomological model of explanation that relies exclusively on the use of laws of nature.

It is possible to trace the defence of narrative explanations as a legitimate form of scientific theories to a reaction against Hempel's argument downplaying the scientificity of history. Hempel defended a unified picture of the scientific method, in which scientific explanations follow a deductive structure. On this model, a sufficient knowledge of the initial conditions and of the application of the relevant laws of nature make it possible to deductively infer the occurrence of the phenomenon under study and hence to explain it. As discussed in Chapter 1, history does not possess a readily usable collection of laws which can be invoked to explain the events under study. Therefore, it would only be when history is equipped with such laws that this discipline would truly deserve a scientific status. Before reaching this state (if it is ever reached), explanations in history are, at best, what Hempel calls "explanation sketches" (Hempel 1942, 42). In reaction to this position, narrative explanations have been defended, notably by philosophers of history, as a legitimate form of explanation, even though one that departs from the traditional understanding of what counts as a scientific explanation (see, for instance, Dray 1957).

Mechanistic explanations have similarly been proposed as an alternative to law-based explanations. Despite their emphasis on regularity and the recourse to change-relating generalizations, proponents of mechanistic explanations aim at capturing practices in which such regularities do not have the deductive and necessary character possessed by those explanations invoking laws of nature. As Glennan summarizes, the generalizations invoked in these explanations are "mechanistically fragile":

> Just as the behaviour of the mechanism as a whole is regular but not exceptionless, so is the behaviour of the mechanism's parts (Glennan 2010, 257).

Narrative and mechanistic explanations, therefore, have been developed as alternatives to a deductive-nomological model of scientific explanation. The latter model is criticized as inappropriate to the domain of inquiry by proponents of narrative explanations, and as placing too rigid demands by proponents of mechanistic explanations. In addition to similarities in their origins, they share a common priority in identifying the factors and components that play a role in the occurrence of a phenomenon of interest. The type of factors and components they emphasize, however, are dissimilar.

Parts and interactions vs. central subject

Mechanistic explanations, including Glennan's ephemeral mechanisms, are *epistemologically* reductionist. They require specifying "the parts of a mechanism and the operations the parts perform" (Bechtel 2011, 538). This reductionism implies that the *explanandum*, the system whose behaviour is to be explained, is at a higher level than the *explanans,* the components and activities of the system under study. Explanatory relevance, here, is to be sought *within* the workings of the system under study. Narrative explanations, however, do not restrict the search for explanatorily relevant components within the system under study. As Currie argues, in narrative explanations "explanans are not 'components' but rather causal factors which influenced the particular pathway" the central subject took (Currie 2014, 1180). Causal factors can be of any sorts, be it meteors from outer space driving the extinction of lineages of dinosaurs or genetic mutations causing drastic changes in the fitness of a subset of insect species.

Moreover, mechanistic explanations do not make reference to the explanandum in the explanation. They instead attempt to unpack its inner workings. Narrative explanations, instead, do not create a formal separation between the explanandum and the explanans. The development of the central subject, here, is the central thread around which the narrative is woven, and is constantly referred to. As Currie summarizes, “the history of a central subject is explained in reference to its interaction with various causal factors” (Currie 2014, 1180). Changes can come both from within and from outside of the narrative’s central subject. Narrative explanations, thus, do not entail the mechanistic commitment to epistemological reductionism.

More tentatively, I think that the focus on components, activities and organization by mechanistic explanations implies that the nature and properties of the components of the system are, at the scale of the explanation, stable. Mechanistic explanations account for changes at the system level by unravelling the interactions of relatively stable components. Because narrative explanations revolve around the developments and events affecting central subjects, it does not seem to assume the same degree of stability in the nature of the explanans. As Hull insists, the central subject does not need to be stable throughout the narrative but needs to persist through changes. What matters is that there exists a form of continuity across the various forms the central subject can take across the narrative.

Contingency

Narrative and mechanistic explanations both appeal to some degree of contingency to account for the explanatory uniqueness of the event. As explained above, in ephemeral mechanisms contingency is essentially allowed in the description of the initial conditions. Glennan insists on the contingency of the configuration of parts: they can be the product of chance or exogenous factors and are short-lived and non-stable. In ephemeral mechanisms, once these contingent initial conditions are gathered, the outcome – the unique event to be explained – has some degree of necessity (Glennan 2010, 260). Unique events, using Beatty’s terminology introduced above, are *not* contingent *per se*. They *are* expected to occur if one possesses sufficient relevant information about prior states of the system. Instead, they are contingent *upon* these initial conditions for their realization. The "chance conspiracies of circumstances"[24] upon which unique events are contingent are the ones that are contingent *per se*. In Glennan's example of the death of Roland Barthes, it could not have been anticipated that Barthes and the laundry truck are simultaneously located on a dangerously close patch of road. However, once they find themselves in this situation, the death of Barthes was not a contingent outcome. Ephemeral mechanisms, therefore, propose a *restrictive* use of contingency, locating it only in the occurrence of the initial conditions.

Contingency, on the other hand, is claimed by defenders of narrative explanations to be their essential feature and the main source of legitimacy for their use. As presented above, narrative explanations are argued to be mostly concerned with turning points. To recall, history matters, according to Beatty and Carrera, when "a particular future depends on a particular past that was not bound to happen, but did" (Beatty and Carrera 2011, 495). Narrative explanations, in this view, are needed to account for the various stages that were needed to occur for the realization of a unique event. In Beatty's terminology, the outcome of narrative explanations is *contingent upon* the realization of a series of steps that were *contingent per se*. This view does not, at first sight, make ephemeral mechanisms and narrative explanations extremely different with regards to contingency. They both attempt to explain an event that was contingent upon circumstances that were contingent *per se*. The difference lies in how widespread this contingency can be. As Currie rightly explains, ephemeral mechanisms leave room for contingency only in the mechanism's formation, *not* in its behaviour (Currie 2014, 1179). Narrative explanations, instead, are required to account for a series of contingencies affecting the central subject. In this case, contingency is pervasive all across the unfolding of the narrative.

Explanatory load

These differences in emphasis between mechanistic and narrative explanations have an impact on the type of things that carry the explanatory load. Ephemeral mechanisms constitute explanations because they manage to link a contingent

[24] This seems close to the Aristotelian view on "co-incidence".

initial situation with a necessary outcome. This link is established by the use of change-relating generalizations. These change-relating generalizations track the set of causal interactions between the various components of the system. To explain with an ephemeral mechanism is to describe the situation with the right components and appeal to the relevant set of change-relating generalizations (Currie 2014, 1179). In narrative explanations, explanatory relevance is argued to be centred on the various contingent steps identified throughout the causal sequence. Beatty argues that narratives explain the occurrence of unique events “in the context of surrounding branches” by highlighting “counterfactual difference-making events” (Beatty 2017, 32). Here, the explanatory load is placed on these branching points which were not bound to occur but did and that together made the occurrence of the *explanandum* possible. In this view, it is because these branching points could have been different, that their occurrence was not entailed by the initial conditions, that they constitute relevant explanatory factors for the occurrence of unique events of interest.

To summarize this section, I have assessed the convergences and key differences between narrative explanations and ephemeral mechanisms. Narrative explanations and ephemeral mechanisms both provide alternatives to the deductive-nomological model of scientific explanation. They both insist on the need to identify key components and factors playing a causal role in the occurrence of the explanandum. They also both highlight the importance of some degree of contingency. However, ephemeral mechanisms have restrictions that narrative explanations do not share. Ephemeral mechanisms focus on strategies of decomposition that explains the occurrence of the phenomenon of interest with components found at a *lower level*. They also restrict contingency to what brings together the *initial conditions* to the ephemeral mechanism. Narrative explanations, instead, search for causal contributors of different nature from within *and* without the central subject. It also allows and emphasizes contingency at *every* step of the narrative, not merely in the set-up. Consequently, both strategies place the explanatory load on different things. Ephemeral mechanisms ground their explanations on the change-relating generalizations invoked to link between contingent initial conditions to a necessary outcome. Narrative explanations emphasize the contingency of each of the steps needed to reach the desired outcome.

From this survey of their points of convergence and divergence, another question follows: "are these explanatory strategies competing or complementary?" The following section considers the second option and presents an attempt to reconcile narrative explanations and ephemeral mechanisms by presenting them as relevant for different kinds of situations. This possibility relies on Currie's distinction between "simple" and "complex" narratives.

D. Currie's simple and complex narratives

This section details Currie's attempt to reconcile ephemeral mechanisms with narrative explanations. Ephemeral mechanisms are, in Currie's view, the equivalent of *simple narratives*. In simple narratives[25], "an event is explained by a general model, and minimal causal factors are referenced" (Currie 2014, 1167). Currie illustrates his claim with the explanation of a geological episode of planet-wide Ice Age that purportedly occurred around 650 million years ago. This geologically unique event is explained by invoking the "Snowball Earth" model. According to this theory, the planet-wide episode of glaciation was triggered by the clustering of landmasses around the tropics. These contingent initial conditions led to atmospheric changes that globally lowered the temperatures, notably at the poles, and triggered a feedback loop that strengthened the cold wave, which eventually led to a planet-wide ice envelope on the oceans. If such initial conditions were replicated, the Snowball Earth model states that a similar planet-wide glaciation would occur. This model fits with the notion of ephemeral mechanisms. The behaviour of a system (here, planet Earth) is explained by identifying lower level parts in a (geologically speaking) short-lived configuration and the change-relating generalizations driving the behaviour of these parts. As in ephemeral mechanisms, the emphasis is placed on decomposition, the identification of contingent initial conditions, and the use of the relevant change-relating generalizations that leads to the production of the desired outcome.

[25] Please note that there is no negative connotation associated with the qualification of ephemeral mechanisms as "simple".

In this chapter's meaning[26], narrative explanations are equivalent, in Currie's view, to *complex narratives*. In these cases, "there is no appeal to a general model in explanation, but rather a unique, detailed causal sequence is employed" (Currie 2014, 1167). Currie discusses the case of sauropod gigantism, an attempt at explaining the size of the biggest land animals that have ever existed. To do this, scientists combined sauropod's inferred primitive characteristics (i.e., oviparity, the lack of mastication, small-head-and-long-neck morphology) and new adaptations (i.e., increased basal metabolic rate, pneumatised skeleton). All of these factors are pieced together in a "unique, detailed causal sequence" (Currie 2014, 1167) that uses *explanans* at many levels of grain to account for the origin and viability of sauropods. The explanation of sauropod gigantism takes the form of a narrative explanation as it "proceeds by drawing together a plethora of diffuse, contingent explanans and telling a well-supported, coherent story about the sauropod lineage" (Currie 2014, 1169). The emphasis here is placed on the identification of a variety of contingent factors, acting at different scales, which enabled the occurrence of a unique outcome: the extreme size of a species of dinosaurs.

The complementarity between simple and complex narratives, according to Currie, is grounded on the fact that the recourse to one or the other is not dependent on merely subjective considerations. In other words, the best explanatory strategy is indicated by the "world" (Currie 2014, 1164). In some cases, the event of interest yields itself to be explained by recourse to a mechanistic explanation, invoking a general model to link between initial conditions and the outcome. In other cases, such a strategy fails and a particular, contingent course of events must be invoked to explain the outcome. Both tools are complementary in the sense that there would generally be no cases in which both tools would be simultaneously suited to explain an event of interest. This requires making a choice about the preferred mode of explanation.

Moreover, Currie ties simple and complex narratives with the notion of progress in historical sciences. It is possible that in the course of an investigation,

[26] By contrast, Glennan seems to equate narrative explanations with ephemeral mechanisms.

according to Currie, a complex narrative replaces a simple one, constituting a form of explanatory progress. Reverting from complex narratives to simple ones, however, seems quite unlikely and counterproductive. Any simplification of the causal sequence runs the risk to be arbitrary and to miss out on essential causal factors[27]. He discusses the details of the explanation of sauropod gigantism to illustrate his claim:

> An account which did not include oviparity would fail to explain how sauropods managed such gigantic sizes, as how they managed to have resilient populations is left as a mystery. One which doesn't mention pneumatisation is also dissatisfying, as how Sauropods oxygenated their blood is left mysterious (Currie 2014, 1171-1172).

The complementarity between narrative explanations and ephemeral mechanisms is therefore grounded on what investigations of unique events indicate over time. Different investigations privilege one type of explanation. And in some cases, complex narratives (narrative explanations) might end up replacing simple ones (ephemeral mechanisms).

The first part of this chapter outlined the two candidate epistemological tools for the explanation of unique events, narrative explanations and ephemeral mechanisms. After identifying the convergences and divergences between both explanatory strategies, I presented Currie's distinction between simple and complex narratives. Currie's distinction is used in a conciliatory strategy that claims narrative and mechanisms to be useful for the explanation of a different range of unique events. Can these two explanatory tools, on top of that, be complementary *within* a given explanation? In other words, can explanations of unique events integrate elements of *both* ephemeral mechanisms and narrative explanations? The next part defends lineage explanations as a potential candidate for this task.

II. Lineage explanations

A. Introduction

[27] This relies on a strong interpretation of Currie's argument. How far he thinks his case can be extrapolated is not explicitly stated.

Lineage explanations are, similar to ephemeral mechanisms, a recent outgrowth of the philosophical study of mechanistic explanations. They also share with ephemeral mechanisms the intended application to the domain of historical sciences, more specifically in evolutionary developmental biology (evo-devo). Lineage explanations are, according to Calcott, epistemological tools suited to explain phenotypic changes, here changes in patterns of development over evolutionary time. It attempts to deal with “the details of how [a] particular mechanism of interest worked, and how it changed over time” (Calcott 2009, 52). These explanations typically display causal trajectories between two states of the same entity. If applied to the professional career of a human being, a lineage explanation retraces the series of steps from early education to retirement, describing the state of the person at each of the stages and describing the changes required to proceed from one step to another (promotion, degree, etc.).

These explanations can be represented in the form of a cartoon strip:

> Each stage shows how some mechanism worked, and the differences between each adjacent stage demonstrate how one working mechanism, through minor modifications, could be changed into another working mechanism (Calcott 2009, 52).

To provide a simple illustration, Calcott describes a game in which one must go from one word to another. There are two rules to this game. First, one can only change one letter at a time, illustrating how changes must be gradual. Second, each of the stages must form an actual word, illustrating the need for “viable” transitions. For instance, it is possible to provide a lineage explanation that tracks the changes from “scale” to “plume” (Fig. 1).

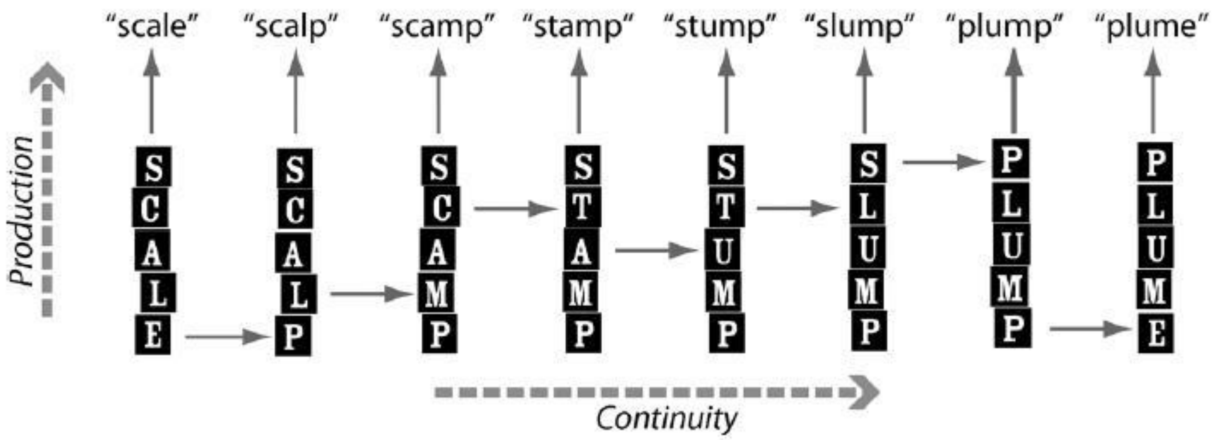

Figure 1 Representation of a lineage explanation (from Calcott 2009, 55). Reproduced with permission from Oxford University Press.

As the figure shows, lineage explanations run in two dimensions, following two types of requirements. The first requirement, which runs horizontally in Fig. 1, is termed the *continuity dimension*. The continuity dimension constrains lineage explanations by imposing limits on how much the subject of a lineage explanation is allowed to change. In the case presented, to respect the continuity dimension is to respect the rule that the words can only be modified by incremental change, one letter at a time. The second requirement is the *productive dimension*. The productive dimension constrains lineage explanations by checking that each step in the explanation is considered viable according to some principles. Here, each of the slightly modified sequences of letters must still be able to form a valid English word. These are the two generic types of constraints that lineage explanations must simultaneously respect.

Applied in the context of evo-devo, lineage explanations track changes in the phenotypic space. What were the various stages, for instance, in the evolution of eyes and feathers? In his discussion of these cases (Calcott 2009, 57-61), Calcott respects the continuity dimension by proposing only a series of gradual changes, respectively in the eye and the hair follicle structures. The lineage explanations he discusses also respect the productive dimension by proposing at each step structures that are considered viable. In other words, at each of the stages, the described organisms are (on paper) capable of living and surviving. Combining continuity and productivity, these two lineage explanations respectively account for the gradual improvement of visual acuity and the development of fully-fledged feathers.

Lineage explanations are a compilation of an interrelated set of two types of lower level explanations (Calcott 2009, 62). Respecting the productive dimension requires providing an explanation of how each of the developmental steps is a viable one. These explanations, according to Calcott, are mechanistic. They rely on the explanation of the viability of the entity postulated at each of the steps by unravelling the organization and interaction of its parts so that they fit together into a functional whole. In this view, a lineage explanation is composed of at least as many mechanistic explanations as there are steps postulated by it. The validity of each of these "vertical" mechanistic explanations are both independent of the others and of the series of explanations provided to account for the "horizontal", continuity, dimension.

Calcott is much less restrictive on the type of explanations that can be invoked to explain what caused the changes between each of the steps of the lineage explanation. In the cases he is describing,

> [t]he same lineage explanation for some biological change could be given, whether the process driving that change was natural selection, artificial selection or genetic engineering (Calcott 2009, 75).

In my interpretation, Calcott here leaves open the possibility of filling in the causal gaps between each of the stages with the variety of types of explanations that evolutionary biology possesses: selectionist and non-selectionist, population versus individual-based… The types of explanations filling the continuity dimension, in his opinion, are not as easily determined as for the productive dimension.

In the example illustrated in Fig. 1, the same type of change connects the various steps, namely a single letter change. As will be visible in my application of the concept to the explanation of the origin of eukaryotes, explaining continuity by the same type of change is, I think, not required in lineage explanations, especially when the trajectory studied is complex. To summarize, lineage explanations are composed of two sub-types of explanations of (a) the viability of each step and (b) the transitions between each step, respectively accounting for (a) the productive and (b) the continuity dimensions. If the viability of each step is argued by Calcott to be explained mechanistically, the transitions are however not explanatorily constrained in this way.

B. Narrative explanations and ephemeral mechanisms as types of lineage explanations

As the somewhat long title of this section suggests, and before presenting the possibility of combining both of them *within* a lineage explanation, I argue that narrative explanations and ephemeral mechanisms can be successfully described as sub-types of lineage explanations. Doing so requires making slight amendments to the initial characterization of lineage explanations. These changes, I think, extend the scope of applicability of Calcott's concept. At the same time, it enables me to use a conceptual framework to compare both narrative explanations and ephemeral mechanisms and to eventually devise another conciliatory strategy between the two.

Ephemeral mechanisms as lineage explanations

As a reminder, ephemeral mechanisms employ a traditional strategy used in mechanistic explanations. The *explanandum* is a behaviour produced by a system. Explaining this behaviour is a matter of an adequate decomposition of the system into its relevant parts, describing how they are organized and identifying the interactions that occur between them. The difference between traditional and ephemeral mechanisms lies in how contingent and frequent the initial configurations of the system of interest occur. Ephemeral mechanisms, concerned with explaining unique events, deal with purportedly rare or unique initial configurations. The initial step of lineage explanations corresponds to the description of the initial configuration of ephemeral mechanisms. Ephemeral mechanisms explain the connection between unique initial configurations and the outcome of interest by appealing to change-relating generalizations. In lineage explanations, change-relating generalizations fill the gap between each of the steps. They ensure the continuity of the system of interest by explaining the series of changes that affect it. As in mechanistic explanations, the outcome of the unique event in need of explanation stands at the end of the lineage explanation.

Having found equivalents for the initial and end stages, as well as for how the continuity requirement is fulfilled, there remains the need to account for the production requirement in ephemeral-mechanisms-as-lineage-explanations. Because the vertical explanation of each step's viability is done in mechanistic terms in Calcott's formulation, I argue that the adaptation of ephemeral mechanisms to this part of the framework is unproblematic. This question, however, highlights a difference of focus between lineage explanations and ephemeral mechanisms. In the context of evo-devo, from which the concept originates, each of the steps of lineage explanations corresponds to the phenotypes of different species. These phenotypes are stable and viable, corresponding or akin to the ones of extant species. The transitions between steps described by Calcott is considered as what is needed, at a *population* level and over a range of generations, for species bearing a phenotype to change into another. In ephemeral mechanisms, instead, each of the steps is meant to be a reconstruction of the changes *an individual* subject went through. In this view, the steps described are transitional snapshots of what resulted from a series of interactions between the parts of the system before another series of interactions occur again. It is, however, still of primary importance that none of the intermediate steps postulated in ephemeral mechanisms is non-viable. None of the assessments of the viability of the steps, in ephemeral mechanisms, are usually done by providing self-standing mechanistic explanations of their viability. It is instead assumed that a viable (albeit rare) initial configuration undergoing the relevant change-relating generalizations is going to be viable. Vertical explanations are therefore usually kept implicit in ephemeral mechanisms. A representation of ephemeral mechanisms as lineage explanations is provided in Fig. 2.

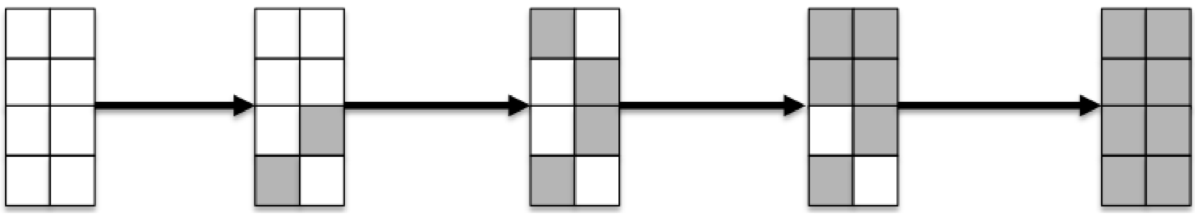

Figure 2 A representation of an ephemeral mechanism as a lineage explanation. Fragmented rectangles represent the system of interest, decomposable into entities represented as squares. Modifications in the components are represented by changes in colours. Black arrows represent change-relating generalizations. At each extreme are represented rare initial conditions (left) and the outcome to be explained (right). This representation assumes that the initial configuration of 8 white squares is a rare one.

Narrative explanations as lineage explanations

Because of the "mechanistic origins" of lineage explanations, the analogy between the latter and narrative explanations seems less obvious. With a few adjustments to the concept of lineage explanations, I argue that this remains an analogy worth drawing. The accommodation of the central subject of narrative explanations in lineage explanations seems quite natural. Lineage explanations are threaded around the changes in a subject over time. Contrary to systems in ephemeral mechanisms, the subjects of lineage explanations share with central subjects a form of relaxed space-time continuity. They possess some form of cohesiveness without (contrary to ephemeral mechanisms) necessarily tracking one and the same system over the course of the explanation. This is why narrative and lineage explanations can track the development of a wider variety of historical individuals. Lineage explanations can follow the phenotypes of evolutionarily related species; narrative explanations can track biological lineages (species) or intellectual lineages in the development of ideas. In this relaxed vision of continuity, central subjects of narrative explanations and subjects of lineage explanations are analogous[28].

It is worth reminding that Calcott did not put restrictions on the type of explanation to be employed to fulfil the continuity dimension of lineage explanations. In the case of narrative explanations, two things are emphasized about the cause of these horizontal changes. Chiefly, changes in the central subject of narrative explanations are mediated by causal factors that bring about *contingent* intermediates. These causal factors affecting the central subjects can be contingent (but need not be), but the result of their action is itself not *bound* to occur. In this view, narrative explanations contrast sharply with explanations in terms of ephemeral mechanisms. In the latter, the intervention of change-relating generalizations brings an element of necessity to the intermediate steps proposed. Such a necessity is not present in narrative explanations.

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[28] Here, I characterize narrative explanations and ephemeral mechanisms as subtypes of lineage explanations. I am aware that it is possible to reverse that claim, and argue that lineage explanations (and, possibly, ephemeral mechanisms) are a subtype of narrative explanations. This would mean that my characterization of narrative explanations is a narrow one, one that captures only one of the possible forms of narrative explanations.

In order to fit narrative explanations, however, slight amendments need to be made to the concept of lineage explanations with respects to the production requirement. Lineage explanations, Calcott argues, mechanistically explain the viability of each of the intermediate steps. Similar to ephemeral mechanisms, intermediate steps in narrative explanations possess a certain degree of transience. While they must be plausible states of affairs, there is no in principle need to restrict the explanation of their viability to mechanistic ones. This is in part a consequence of the differing nature of central subjects and systems, respectively in narrative explanations and ephemeral mechanisms. If systems in ephemeral mechanisms are decomposable, I think that it is much more difficult to decompose central subjects in this way, hence to explain its viability in mechanistic terms. In this sense, narrative explanations have looser production requirements than lineage explanations have.

In short, it is not wholly implausible to characterize narrative explanations as a form of lineage explanations, provided that some slight alterations are made to the original production requirements. In terms of focus, narrative explanations are more directly concerned with horizontal explanations of changes in the subject than Calcott's account was. Calcott's notion seems primarily suited to provide representations of the trajectory of the subject that underwent changes, rather than representing the causes of the changes that underpins this trajectory. The two requirements that Calcott identifies, however, strongly echo Hull's two types of linkages identified in narrative explanations. These linkages are "the cause-effect relation connecting the events associated with the historical entity", corresponding to the continuity requirement, and "the part-whole relation integrating the central subject into a single historical entity" (Hull 1975, 260), equivalent to the production requirement. A representation of narrative explanations as lineage explanations is provided in Fig. 3**.**

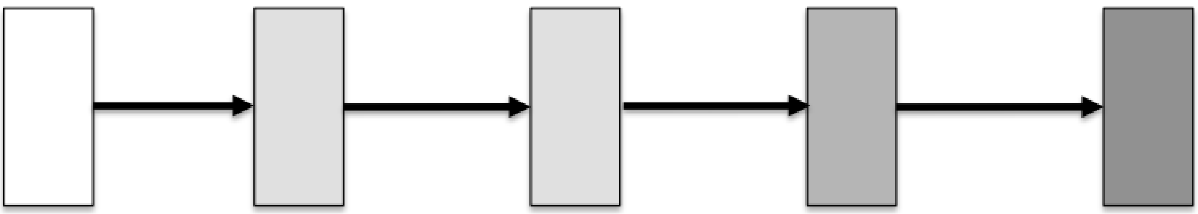

Figure 3 Representation of narrative explanations as lineage explanations. Rectangles represent the central subject of the explanation. Changes (or lack thereof), marked by

changes in colours, here concern the central subject as a whole. Black arrows represent the intervention of causal factors.

Mixed explanations

Conceiving ephemeral mechanisms and narrative explanations as subtypes of lineage explanations opens up, in addition to Currie's, another strategy to conciliate these two. A *mixed* lineage explanation presents a sequence of steps and transitions that contain *both* mechanistic and narrative elements. Ephemeral mechanisms and narrative explanations, in these cases, would not cover the whole trajectory of the unique event. Certain episodes within the explanandum, instead, would be distinctly explained by an appeal to a narrative or to a mechanism. Ephemeral mechanisms, for instance, could be particularly useful to explain parts of the trajectory that do not exhibit the degree of contingency emphasized by narrative explanations, and where the changes in the central subject can be explained by invoking change-relating generalizations between some of its parts. Other parts of lineage explanations could uncover a series of contingent outcomes, stemming from the action of a variety of factors, from within and without the central subject. Narrative explanations seem better-suited to deal with these kinds of episodes.

A representation of such a *mixed* lineage explanation can be seen in Fig. 4. Please bear in mind that this conciliatory strategy is presented here as an *in principle* solution. It might well be possible that it does not fit with any real-life case of explanations of unique events (and as shown below, it does not fit so neatly with the hypotheses of Cavalier-Smith and Martin). It merely stems from an attempt to provide an alternative conciliatory strategy between ephemeral mechanisms and narrative explanations. Contrary to Currie's distinction between simple and complex narratives, mixed lineage explanations avoid seeing narrative explanations and ephemeral mechanisms as mutually exclusive forms of explanations that cannot coexist as part of a bigger explanation[29]. This section, by using a slightly amended version of lineage explanations, provides such an alternative.

[29] Even though it is not explicitly stated, please note that nothing however seems to contradict this possibility in Currie's paper.

The following paragraph assesses the relevance of the ideas presented so far to an analysis of the hypotheses of Cavalier-Smith and Martin. My analysis identified ephemeral mechanisms and narrative explanations, the two candidate epistemological tools to deal with unique events, as subtypes of lineage explanations. Therefore, there are four possibilities with regards to the type of theory presented by Cavalier-Smith and Martin. They can be (a) narrative explanations, (b) ephemeral mechanisms, (c) mixed lineage explanations or (d) something different. In what form do Cavalier-Smith and Martin tell us a story about the origin of eukaryotes?

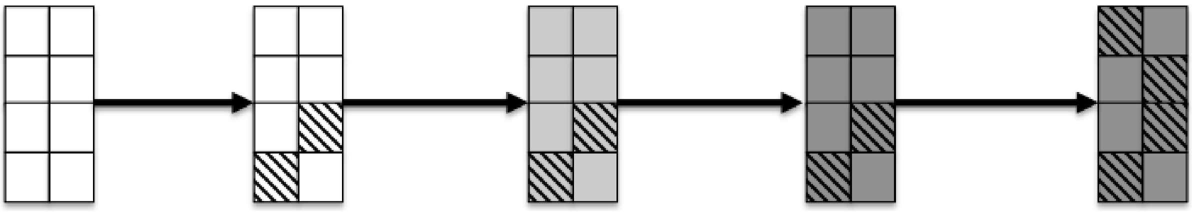

Figure 4 Representation of mixed lineage explanations. Mechanistic-like parts are represented by changes in a set of components (squares) within the subject of interest. Narrative-like parts are represented by changes in the colour of the whole subject.

III. CASE STUDY

A. MARTIN'S HYPOTHESIS AS A LINEAGE EXPLANATION

Martin's hypothesis for the origin of eukaryotes has kept the majority of its main steps unchanged since its initial formulation, in collaboration with Müller (Martin and Müller 1998). This section proposes a characterization of Martin's hypothesis as a lineage explanation, retracing in a series of steps the explanation of this unique event.

This hypothesis is composed of an initial step, an outcome and a central subject. It starts with two populations of cells interacting in the depth of anoxic oceans and ends with the emergence of a fully-fledged eukaryote bearing the ancestors to all mitochondria-related organelles. The central subject is the following evolutionary transition: the origin of eukaryotic cells from prokaryotic ones. All of the steps explain how the outcome came to be. Therefore, the targeted conclusion is what establishes what counts as relevant in the explanation. The continuity of the central subject through changes is ensured by a genealogical link of descent.

Step 1: The encounter

The explanation starts with two types of cells: archaea[30] and alphaproteobacteria. In an anoxic environment where there is competition for resources, organisms would benefit from being in close proximity to food supplies. The encounter between hydrogen-consuming archaea and hydrogen-producing alphaproteobacteria is thus driven both by the internal specificities of their respective metabolisms and the external pressures of a competitive environment. This step in Martin's hypothesis can be captured by a mechanistic explanation. Let's consider the deep oceans of the Palaeozoic as a system and our two organisms of interest as its parts. A change-relating generalization driving these parts could run like this:

> In a system in which there are limited available resources, organisms tend to cluster around available food supplies.

The first step of Martin's explanation can thus be explained as an ephemeral mechanism. In the initial conditions described below (widespread anoxia in deep oceans and competition for resources), applying the above change-relating generalizations explains the outcome: the physical proximity of hydrogen-consuming archaea and hydrogen-producing alphaproteobacteria.

Step 2: The entry

As the physical proximity increases, both organisms eventually lose their physical distinctness, and an alphaproteobacterium is engulfed by an archaeon. This part of the scenario describes an instance of a prokaryote harbouring another prokaryote within it. Despite Martin's repeated attempts to decrease the exceptionality of such arrangement (for instance, in Martin *et al*. 2017, 23-26), this situation is still not consensually considered to be widespread. In addition to this, in the few cases describing a prokaryote-prokaryote engulfment, little is known about how this situation came to be. This situation is therefore contingent upon an interaction that, to the best of our knowledge, is contingent and needs to be specified: a prokaryote finds itself within another prokaryote as a result of an unknown process of encapsulation. No mechanistic explanation of

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[30] The nature of the host is here explicitly vaguer than the nature of the symbiont. This is because there is less scientific consensus on the precise nature of the former than on the latter.

how physical proximity turns into encapsulation has been provided in prokaryotes. This step in the lineage explanation, I think, is aptly conceived as a narrative explanation. Note, however, that this judgement can be temporary. If future investigations provide change-relating generalizations relevant to this event, this step would instead be explained by an ephemeral mechanism.

Step 3a: The rearrangements

This step compiles a series of steps which share common features. In Martin's hypothesis, it corresponds to the series of "problem-solving" steps that helped to turn an oddly-functional host/symbiont association into a fully-functional cell with the precursor of mitochondria-related organelles in it. All of the steps described are contingent *per se*. For many of the lineages descending from the initial engulfment of the host by the symbiont, many of the required steps to stabilize the interaction did not occur the way they were described in Martin's hypothesis. This resulted in the death of the vast majority of these descendants[31]. Martin's hypothesis only foregrounds the very few that survived.

Martin describes a series of genetic transfers that were required both for the survival of the cell and for some of the features of the purported first eukaryotic cell to emerge. The right series of genes must have been transferred or imported at the right time. After each of these required gene transfers, Martin describes the temporarily stabilized situation that resulted from some of these genetic reshufflings. This step is, therefore, a lineage explanation of a narrative subtype. The changes in the central subject are contingent and not captured by change-relating generalizations. This explanation is constrained by continuity constraints, the slight changes in the composition of the entity of interest must be tracked, and by production constraints, an account of the survivability and the challenges faced by the newly-emerging transitional organisms is done at each stage.

Step 3b: Membrane changeover

In parallel to the genetic rearrangements runs another crucial step in Martin's hypothesis. It is stated to explain the changes undergone by the plasma

[31] Martin is not explicit about how many lineages he thinks have managed to follow the steps he describes. Knowing that he considers this event as unique and therefore extremely unlikely, a probable answer is "very few".

membrane lipids. During the whole interaction, it changes from being archaea-like to bacteria-like. This membrane changeover is explained by invoking a purportedly typical behaviour from the engulfed alphaproteobacteria: the secretion of lipid vesicles. The secreted vesicles are also at the origin of several cellular compartments and cellular trafficking processes. As in step *3a*, the series of events is dependent on normal behaviour from the central subjects, be it horizontal gene transfer or vesicle secretion. What prevents these steps from being explainable by appeal to ephemeral mechanisms, however, is that the outcomes that are to be explained are too specific to be made a necessary result of such regular behaviours. A narrative explanation is needed here to specify the contingent outcomes that resulted from these processes.

Step 4: Eukaryotic expansion

Once the ill-functioning prokaryotic association has been turned into a fully-fledged eukaryotic cell, the origin of the rest of the eukaryotic features, according to Martin, has been made possible. Even though the details of the steps on the way to the rest of the eukaryotic innovations have not been provided in the previous chapter, I argue that these lineage explanations will require at least some narrative elements. It is unclear how the described increase in the affordability of bigger genomes can be translated into mechanistic explanations. It seems that, as in step *3a* and *3b*, some degree of contingency is needed to account for how these processes (which, contrary to step *2*, are not unusual cellular behaviours) are shaped into outcomes as precise as new eukaryotic structures.

To summarize, Martin's hypothesis can be described as a mixed lineage explanation with a majority of narrative elements. Interestingly, some of its steps are running in parallel to each other. They are not following a neatly ordered temporal sequence.

B. CAVALIER-SMITH'S HYPOTHESIS

Can a similar assessment be made with Cavalier-Smith's hypothesis? This lineage explanation is woven around different threads than Martin's. It starts not with two but one type of organism, an actinobacterium, and finishes not with one but two types of organisms: the first eukaryotes and the first archaea. It results in a subtly different central subject. Here, it is the transition from bacteria to the first eukaryotes *and* archaea, not merely the former. The lineage explanation tracks the changes in this lineage of bacteria at the origin of both eukaryotes and archaea. In the below description, I focus mostly on the origin of eukaryotes.

Step 1: Lost and found

Cavalier-Smith starts with the loss of the cell wall by an actinobacterium. This usually untimely loss is, in this case, followed by the evolution, in descendants of this cell, of two new types of membrane structures at the origin of both eukaryotes and archaea. The whole sequence is named the “neomuran revolution”. This initial step does not correspond to a series of events that are known to regularly occur in nature. It is similar, in some respects, to step *2* from Martin’s hypothesis. The emergence of two new types of membrane arrangements from an uncommon event is a radically contingent event, which could not be captured by invoking an ephemeral mechanism. No change-relating generalizations can be invoked to make the link between the recovered wall-less bacteria, an initially rare but mechanistically explainable configuration, and the cells with the new types of membrane. This step is, at this stage, definitely of a narrative type.

Step 2: Intracellular coevolution and secondary acquisitions

Cavalier-Smith actually uses representations of lineage explanations to describe the later steps in the evolution of eukaryotes (see, in particular, Cavalier-Smith 2014, 46-47, fig. 2). The series of cellular changes described after the neomuran revolution are depicted as events of “intracellular coevolution”. It proceeds, echoing the focus on decomposition by mechanistic explanations, by articulating the organization of existing and newly evolved cellular components and the type of functions this enables. The transition between each of these stages is however not mechanistic. Instead, the order of emergence of some of the cellular structures is only loosely constrained. Mitochondria, for instance, emerge somewhere within this series of change as a secondary consequence of the acquisition of phagocytosis*. It is only stated that it comes simultaneously (evolutionary speaking) with endoplasmic reticulum* and peroxisomes*, and that this trio of cellular structures has boosted the energetic efficiency of the cell. The same goes for the nucleus, which became a necessary protective measure for the genetic material after the emergence of the cytoskeleton*, but of which its moment of appearance isn't known with precision.

The origin of mitochondria, in Cavalier-Smith's hypothesis, is contingent *upon* a series of higher level intracellular changes, which reshuffled the organization, and physiological possibilities of the lineage at the origin of eukaryotes. These wider intracellular changes were themselves contingent *upon* the recovery from the loss of cell wall by a lineage of bacteria, which is at the origin of eukaryotes and archaea.

Most of the intracellular changes at the origin of eukaryotes in Cavalier-Smith's lineage explanation are aptly described and represented as narrative-like lineage explanations. They gather together a series of steps that keep track of the changing components of the central subject. Contrary to ephemeral mechanisms, the transition between these steps cannot be explained by recourse to change-relating generalizations. At the side of this series of cellular changes, Cavalier-Smith postulates the origin of some cellular structures, such as mitochondria, as secondary outcomes of the main evolutionary changes. The origin of mitochondria, therefore, is explained with a *loose* narrative: a sequence of events that develops somewhere on the side of a bigger, more constrained, narrative.

C. Analysis

It is clear that Cavalier-Smith and Martin provide lineage explanations for the origin of eukaryotes that are, at best, slightly mixed. Only Martin's initial step of bringing two types of bacteria physically closer has been successfully explained as an ephemeral mechanism. His second step might also become mechanistic if change-relating generalizations explaining the integration were uncovered. The rest of the explanations possess numerous narrative-like features by postulating a series of contingent steps: steps that were not bound to occur as a result of the initial conditions. However, and it is very visible in Cavalier-Smith's hypothesis, both scientists sometimes drift away from providing a linear sequence of events. Instead of having a tidy series of contingent events, Cavalier-Smith and Martin's hypotheses sometimes describes events running parallel or secondary events that did not occur at a determinate point and in a determinate order in the lineage explanations provided. Their hypotheses are thus mixed lineage explanations in a second sense: they combine tidily ordered elements with more loosely constrained, messy bits.

CONCLUSION

The present chapter inquired into the nature of the explanations of unique events. For this task, two epistemological tools have been put forward: narrative explanations and ephemeral mechanisms. I summarized the main features of these epistemological tools and described Currie's distinction between simple and complex narratives as a conciliatory strategy that makes narratives and mechanisms efficient for a complementary range of events. The second part of the chapter puts forward and argues for the notion of lineage explanation as a fruitful epistemological notion for explaining unique events. Calcott's notion can be tailored to accommodate both ephemeral mechanisms and narrative explanations as subtypes of lineage explanations. This suggested another conciliatory strategy between the two epistemological tools, by presenting the notion of *mixed* lineage explanations. Mixed lineage explanations, in principle, are able to contain elements of both ephemeral mechanisms and narrative explanations at different stages of the explanation.

I brought these conceptual considerations to my case study, which analysed Cavalier-Smith and Martin's hypotheses for the origin of eukaryotes as lineage explanations. The widespread contingency pervading both Cavalier-Smith and Martin's hypotheses makes them more readily analysable as narrative explanations than as ephemeral mechanisms. However, the case study highlighted another way in which lineage explanations can be mixed. Both Martin and Cavalier-Smith's hypotheses defy a strictly linear understanding of lineage explanations by incorporating a parallel series of events and loosely timed and ordered elements alongside more tidy and sequential arrangements of events.

The presence of such messiness in explanations of unique events is a potentially interesting locus of inquiry. Does it show the insufficient development of currently available explanations? In this case, one would expect progress in explaining unique events to be a matter of "tidying up" hypotheses by providing more strictly ordered lineage explanations. This runs contrary to Currie's emphasis on the complexification of narratives as a sign of progress (Currie 2014). Is it, instead, a sign of maturity of these hypotheses? Loosely ordered lineage explanations would be needed, in this view, to accurately reflect the complex array of factors that operate simultaneously for the realization of an outcome. These factors might affect the central subject in different places and their effect might develop over different time-scales. For these reasons, their actions cannot be neatly packaged in a linear and tidy lineage explanation that integrates them in a linear sequence of steps. In the case of loosely ordered events, it might be the case that the near-simultaneous occurrence of such events (like, in Cavalier-Smith's hypothesis, the acquisition of mitochondria, endoplasmic reticulum, and peroxisomes) makes it epistemologically unrealistic to access the details of which one came first.

This emphasis on the non-linearity and complexity of the *explanans* echoes other existing analyses. Bechtel, for instance, recently criticized "basic mechanistic explanations" (such as the ones presented by Glennan) as being incapable of dealing with non-linear series of events (Bechtel 2011). He presented an updated notion of mechanistic explanation, which attempts to integrate "nonsequential organization" in order, for instance, to provide satisfying mechanistic explanations of oscillatory behaviours. Whether or not this updated conception of mechanistic explanations can, in turn, be expanded to the explanations of unique events is an interesting question. However, this chapter showed that Bechtel's critique of mechanistic explanations for their overemphasis on linearity can equally be applied to narrative explanations. To explain unique events, it seems that one must sometimes give up, in some respect, an overemphasis on providing linear explanations.

The position developed in this chapter also echoes Fehr's defence of a form of explanatory pluralism for the evolution of sex (Fehr 2006). Fehr presented three types of models to explain the existence of sex in a population, namely the "Red Queen hypothesis", "Muller's Ratchet", and the "DNA Repair explanation". These three explanations work at different degrees of abstraction, both spatially and temporally. Therefore, Fehr argues that the "[e]xplanation of the evolution of sexual reproduction requires multiple accounts, which cannot be integrated with one another without loss of content or explanatory information" (Fehr 2006, 168). This suggests another reason grounding the impossibility of integrating the explanation of some unique events in a neatly ordered sequence[32].

What would a mechanistic explanation of the evolution of eukaryotes look like? Cavalier-Smith and Martin's explanations follow a similar pattern. An initial unlikely major disruption is, equally improbably, stabilized. A series of events then bring about (among other things) the origin of eukaryotic cells. The initial major disruption, in both cases, stems from abnormal or dysfunctional behaviours from cells. Both Cavalier-Smith and Martin use this explanatory resource as adequate to their *explanandum*. In this view, a change of this magnitude and rarity requires events that are deemed improbable enough. They would have otherwise happened several times, something that the degree of contingency of each scientist's explanation does not seem to allow. This type of improbable initial event can be compared to the combination of the meteorite impact and the extended volcanic activity that triggered the Cretaceous-Tertiary extinction.

It is hard to see how the initial disruptions postulated in Cavalier-Smith and Martin's explanations could be mechanistically explained. However, they seem to be great candidates for bringing about the contingent initial conditions of an ephemeral mechanism. In this case, a mechanistic explanation would be capable of deriving the end state, the origin of eukaryotes, by the application of a series of change-relating interactions to the newly-made symbiotic interaction. Here, in Cavalier-Smith's case, a purported mechanistic explanation would work out the probability for the newly formed cellular organization to ingest (and then fail to digest) foreign elements. In Martin's explanation, it would amount to systematize the probability to transfer genetic elements of various size and nature (i.e., specific genes, whole metabolic pathways) and from this, to devise the probability of the initial organisms to successfully undertake the transformations presented in Martin's scenario over several generations.

[32] Currie and Sterelny (2017) also discuss the difficulty of providing integrated narratives for the explanation of unique events, highlighting the latter as a significant epistemic achievement. I come back to this part of their account in Chapter 5.

The current state of the explanations is far from this potential mechanistic state. It is currently insufficient, in Cavalier-Smith and Martin's explanations, to merely identify contingent initial conditions and change-relating generalizations to explain the outcome. This line of thinking runs parallel with Skipper and Millstein's argument (2005) against the possibility of conceiving natural selection as a mechanism. Their criticisms mainly concerned the impossibility of explaining how the conjoined action of parts and interactions could provide the desired outcome as a result of natural selection.

All in all, whether or not the explanations of the origin of eukaryotes will grow messier or more mechanistic is an open question. This case study does not enable me to say anything definite with regards to the usefulness of mechanistic explanations for unique evolutionary events. Independently of how "mechanistic" explanations of unique evolutionary events turn out to be, this type of explanation can play alternative roles in this type of research. One such role that mechanistic explanation can play is an *evidential role*. In the next chapter, I present the production of implicit "fictional truths" derived from lineage explanations as a way to provide empirically tractable claims which validity are directly assessed. Some of these claims can be mechanistic.

Before discussing that, the next chapter starts with a study of the relation between these lineage explanations as theories and the past they are supposed to *represent*.

CHAPTER 4: HYPOTHESES REPRESENTING PAST EVENTS

INTRODUCTION

In Chapter 3, I defended hypotheses about unique events to take the form of lineage explanations. A lineage explanation retraces the development of its subject by retracing its trajectory via a series of steps. They are composed of two types of lower level explanations. One subset explains the transitory state of the subject at each of the identified stages, complying with what Calcott (2009) describes as the “production requirement”. The other subset explains the transitions between each of the stages, something that Calcott calls the “continuity requirement”. After having characterized the nature of these lineage explanations, and argued narrative explanations and ephemeral mechanisms to be subtypes of this category, this chapter now explores the relation between the provided explanation and the target event it aims at explaining. In other words, this chapter deals with questions of *scientific representation*.

How are Cavalier-Smith’s and Martin’s lineage explanations of the origin of eukaryotes representations of their target event? The reader might be wondering what motivates the opening of such an investigation in the first place. How would it fit in the broader argument of this dissertation?

I am ultimately interested in the production of knowledge in historical sciences. The next chapter dwells further on how evidence is generated in historical sciences. By exploring questions of representation, this chapter explores how lineages explanations are turned into claims to be assessed using these various evidential strategies.

Several criteria to distinguish scientific representations from other, non-scientific, representations have been proposed. One of them is to make the status of scientific representation hinge on direct, here understood as user-independent, relations between a scientific representation and its specific target. The nature of these relations is described with different degrees of precision. Nonetheless, in this approach, the focus rests on how scientific representations possess some form of similarity to their targets (Suppes 1962; van Fraassen 1980; Lloyd 1988; Giere 1988; da Costa and French 2003). Because of its restricted emphasis on the representation/target relation, these accounts have been qualified as “dyadic” (Knuuttila 2009) and have been the object of vehement criticisms (Suárez 2003). .

Alternatives to dyadic accounts integrate a third element in the representational picture, namely the user(s) of scientific representations. In this view, “[p]ragmatic approaches make representation less a feature of models and their target systems than an accomplishment of its users […]” (Knuuttila 2011, 265). A weak version of such user-centric accounts make scientific representation hinge on an act of “stipulative fiat” and does not provide explicit norms to assess the quality of the chosen representation (Callender and Cohen 2006). A stronger version, which is adopted in this chapter, is presented in Suárez’s inferential account (2004). Here, in Knuuttila’s words, scientific representations represent their target in virtue of their ability to “enable the informed and competent user to draw valid inferences regarding the target” (2011, 266).

Subscribing to the inferential account of scientific representation is a way to retain normative guidelines to evaluate the status and quality of scientific representations while giving up on excessively strict constraints (hence its characterization as a “deflationary” account). In my case study, it leaves room for the possibility of characterizing *false* scientific representations as *good* scientific representations. One example of this being the “Archezoa” hypothesis, discussed in Chapter 6. This hypothesis, albeit ultimately proven wrong, was used as the stepping stone for the formulation of several useful inferences and lines of investigation. This line of thinking is followed by several authors who urge to decouple the questions of whether something is a scientific representation and what makes this representation an accurate one (Frigg and Nguyen 2016a; Toon 2012, 23). This position also resonates with authors who emphasized the importance of using “false models” in scientific knowledge production (Levins 1966; Wimsatt 1987). In this chapter, my interest lies in how the lineage explanations provided by Cavalier-Smith and Martin, as representations of unique past events, are turned into empirically tractable claims.

The philosophical literature about scientific representation generally deals with how scientific *models* represent target systems. Scientific models are not always representing a particular real-world target. They generate what Morgan describes as a "world in the model" (Morgan 2012) which can be an *autonomous* object of investigations. On a related point, this autonomy grants the applicability of models to a wide range of epistemic situations. To illustrate this, consider the "Lotka-Volterra" model. This model was initially devised to capture the dynamics of predator/prey populations with a set of differential equations. Exploring the mathematical properties of the model is an object of investigations in itself. The Lotka-Volterra model can also be applied to a variety of systems, most notably to biological populations, but it has also been applied to describe economic oscillations (see Knuuttila and Loettgers 2016 for an in-depth analysis).

The construction of scientific models involves deliberate abstractions, distortions and idealizations which underpin the applicability and transportability of models. As summarized by Weisberg, scientific models "are abstract structures or physical structures that can potentially represent real-world phenomena" (Weisberg 2007, 216). Therefore, they are often said to represent their targets *indirectly*, enabling its users to gain "understanding of a complex real-world system *via* an understanding of a simpler hypothetical system that resembles it in relevant respects" (Godfrey-Smith 2006, 726).

The lineage explanations proposed by Cavalier-Smith and Martin are *not* scientific models. The "world in the explanation" they contain is not *epistemically* autonomous from the target system they represent, since these hypotheses constitute the sole epistemic access to their event of interest. Such theories are not built to be transported to other contexts outside of their scope of origin. They are specifically tailored to the explanation of the origin of eukaryotes. Additionally, these explanations do not deliberately contain idealizations and distortions. Instead, they constitute attempts to provide *direct* representations of their unique event of interest. In this view, they are close to what Weisberg and Godfrey-Smith describe as *abstract direct representations* (henceforth, ADR). In this view, Cavalier-Smith and Martin "seek to directly represent the workings of the real-world system [they] are trying to understand" (Godfrey-Smith 2006, 730).

This vision is noticeably similar to the dyadic account sketched above, but differs in two key respects. Firstly, the main focus of this approach remains on how scientists learn from the use of such epistemological tools, and this is partially independent from the extent to which a given ADR accurately represents its target. Secondly, dyadic accounts of scientific representation are mainly conceived with mathematical models in mind, and are thus made to account for mathematical similarities between the model and the target. Here, my case study involves linguistic descriptions of events, not mathematized models of it.

To summarize, this chapter deals with how lineage explanations about the origin of eukaryotes, conceived as abstract direct representations, represent their target. It builds on Toon's *make-believe* approach, who proposes a *direct* view of how models represent. The purported directness of his account, despite its focus on scientific models, is what drew me to build my argument from it. Part 1 introduces the main concepts of Toon's view and applies it to my case study. Part 2 presents Frigg and Nguyen's *DEKI* account, devised as an *indirect* view of scientific representation and presented as an improvement on some of the shortcomings of Toon's account. I argue in part 3 that the claimed difference between both views is exaggerated and provide a way to consider Toon's view as a particular configuration (a case of *DEK(I)*) in Frigg and Nguyen's framework. In part 4, I argue that Cavalier-Smith and Martin's lineage explanations constitute an even more specific special case (a case of *D(E)K(I)*) in this same framework. This chapter provides a way to reconcile Toon with Frigg and Nguyen's views and to retain some of the insights Toon's account provides (notably, his insistence on directness).

I. TOON'S "MAKE-BELIEVE" VIEW

This inquiry starts with a characterization of the "make-believe" view, proposed by Toon (Toon 2012). This view is developed in the context of the use of models in science and proposes an analogy between the latter and the imaginative games of children. Extending this analogy, Toon develops a terminology derived from the context of these games.

A. PROPS

A game of make-believe starts with a group of children that decide to use their imagination to pretend an object to be something else. In games of make-believe, branches of wood can become magic wands, plastic constructions of space-ships can become real ones, and old wardrobes can become entry doors to parallel worlds. The objects which are imagined to be something else are termed *props*. Props are used by the players to imagine things. They are constitutive elements of the game.

Extrapolated to scientific contexts, Toon argues that models are akin to props. They are considered, by an act of pretence, to be representations of something else. This leads Toon to provide the following definition:

> *M* is a model-representation if and only if *M* functions as a prop in a game of make-believe (Toon 2012, 62).

An example of model-representation for Toon is the ball-and-stick physical model of molecules, used in chemistry. These physical models are used as props in a game of make-believe in the sense that scientists pretend, in manipulating them, that they are manipulating genuine molecules and chemical bonds (Toon 2012, 122).

This pretence-based view of scientific models outlines what Toon considers to be a *direct* view of scientific representation. Toon suggests that the relation of representation is one between the prop and the target of the representation. The former corresponding, in the example discussed above, to balls and sticks, and the latter to atoms and chemical bonds. There is not, in this view, any intermediate abstract entity between the model and the target. Representation, then, occurs when "a model *M* […] prescribes imaginings about (a target) *T* within a game of make-believe" (Toon 2012, 62).

B. Principles of generation

The presence of props is however insufficient to prescribe imaginings about a target. A set of rules is needed to direct and constrain the imagination. These rules are, in the make-believe approach, called *principles of generation* (Toon 2012, 34–35). They are defined as "convention[s] that the children establish by […] agreement" in the game they play. In a game of make-believe, then, the imaginings produced by players through the usage of props depend on and are constrained by the application of principles of generation. This concept is fairly straightforwardly transferred to a scientific context. In this case, principles of generation are the set of theoretical principles that both enable and constrain scientists' imagination in their use of models. In the case of ball-and-stick models of molecules, principles of generation dictate that balls are atoms and sticks are covalent bonds, that red balls stand for oxygen atoms, or that each carbon atom can make a maximum of four covalent bonds. As Toon states, some of these principles are shared implicitly (or are assumed to be) across a scientific community (Toon 2012, 36). Principles of generation, also, do not need to be fully deterministic: there is often some leeway in what can be imagined while using models. In all cases, they enable the users of scientific models to turn props into representations of targets.

C. Fictional truths

Games of make-believe involve the interaction of players with props endowed with imaginary meanings provided by principles of generation. The resulting utterances that are made by children in such games, such as "I put a spell on you with my magic stick!", are what Toon calls *fictional truths*. It is fictionally true to make such utterances because (a) it conforms to the presence of props conjoined with the application of principles of generation and (b) it is only in virtue of these principles of generation that such utterance can be considered true. In a scientific context, fictional truths are similarly uttered when using models. In manipulating ball-and-stick models of molecules, it becomes fictionally true to assert that someone holds a carbon dioxide molecule if she holds a black ball linked on each side by two sticks to two red balls.

D. The benefits of exploration

Toon's make-believe view is stated to be an account of how it is possible to learn with scientific models. By using props to prescribe imaginings about a target, Toon suggests that the process of learning occurs through the exploration of the fictional truths thus generated. In his view,

> we are quite aware of the state of our props, and of many of the fictional truths these props generate. What we don't know are many other fictional truths that these primary fictional truths imply. Learning about our models is a matter of discovering these implied fictional truths (Toon 2012, 47).

Consistent with his emphasis on directness, to learn about the fictional truths generated by the use of models, in short learning about the model, is akin to learn about the target these models are supposed to represent. To summarize, Toon defends a view where models, as props, and principles of generation are combined to prescribe imaginings, or fictional truths, about a given target. The directness of the view suggests that the fictional world thus generated via the model is pretended to be equivalent to the actual world of the target of the representation. For instance, holding a black ball linked on each side by two sticks to two red balls is therefore *like* holding a carbon dioxide molecule. The make-believe view is represented in Fig. 5.

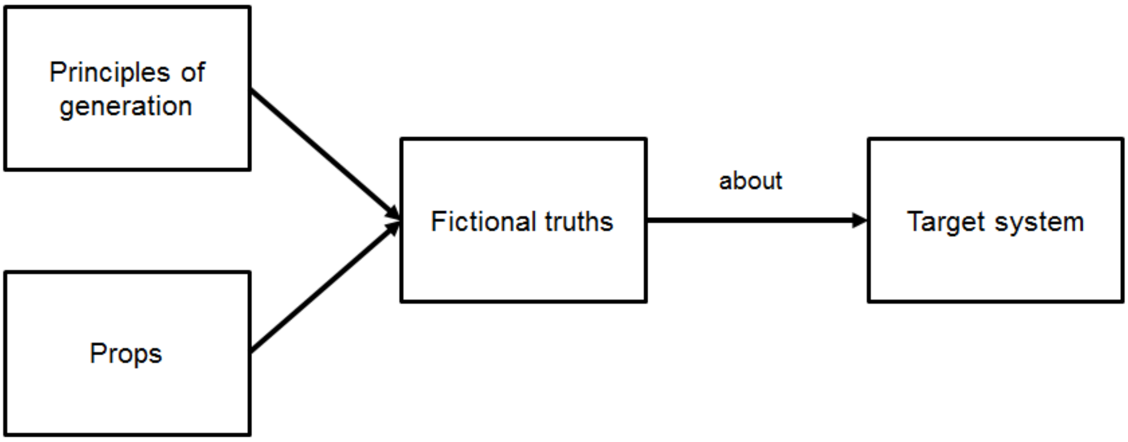

Figure 5 Representation of Toon's make-believe direct view of representation.

This emphasis on directness is at the heart of Frigg and Nguyen's criticism of Toon's account. Their "DEKI" approach to scientific representation attempts to go on similar lines to those of Toon but, according to them, it avoids some of the identified pitfalls of the make-believe approach.

II. Frigg and Nguyen's DEKI account

Frigg and Nguyen, also discussing this issue in the context of the use of models in science, propose to break down scientific representation into four components, at the origin of the *DEKI* acronym. These are *denotation*, *exemplification*, *keying-up* and *imputation*. According to Frigg and Nguyen, representation is a relation between a vehicle *M* and a target system *T*[33]. While there is supposedly no further mediator in Toon's account (hence a *direct* view), the relation between *M* and *T* is here mediated by *R*, the latter being a representation of *T* generated with *M.* This together constitutes an *indirect* account of scientific representation. The DEKI account proposes to disentangle the processes at play between these three entities.

A. Denotation

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[33] They would have respectively used *X* and *Z* here, but for the sake of clarity I continue with the letters used in Toon's account.

Denotation, quite straightforwardly, is the relation of attribution that links the vehicle *M* and its target *T*. It is the relation, for instance, that links the balls and sticks of the molecular model with molecules and covalent bonds. A black ball connected to two red balls, in this case, denotes carbon dioxide.

B. Exemplification

In many cases, not all of the features instantiated by *M* are relevant to its role as a representation of *T*. Out of the properties of *M*, the relevant ones to this relation are said to be *exemplified* by *M*. This notion used by Frigg and Nguyen is inspired by Elgin's discussion of how caricatures represent. For instance, consider a caricature denoting Winston Churchill representing him as a bulldog (Elgin 2009, 79). Out of the features of the drawings, some of them (such as hair colour and paw shape) are irrelevant to the representation relation while some of them (menacing eyes, ample cheeks, defensive posture) are. The caricature therefore exemplifies certain features that purportedly represent some physical and personality traits owned by its target. In ball-and-stick molecular models, the colours, shapes, and relative arrangements of the components (not, say, the texture or smell of the balls and sticks) are the features exemplified by the vehicle that are relevant to the representation of the target.

C. Keying-up

It is not sufficient for *M* to exemplify some features to make it denote *T*. The exemplified properties need to be interpreted in order for them to signify something about the target. This process of interpretation is mediated by what Frigg and Nguyen describe as a "key"

> which explicitly associates the exemplified properties with properties to be imputed onto the target (Frigg and Nguyen 2016b, 228).

The key does a similar job as "principles of generation" in the make-believe account. It is what states, in ball-and-stick models, that red balls correspond to oxygen atoms and that sticks count as covalent bonds. In Frigg and Nguyen's terminology, keying-up turns properties exemplified by the vehicle *M* in properties "*I*-exemplified" – properties that are exemplified by the vehicle after the application of the key. The ensemble of *I*-properties obtained in the use of a model is what constitutes *R*, the "world in the model".

D. IMPUTATION

Properties that are *I*-exemplified are then imputed to the target by an act of stipulation, similar to Callender and Cohen's position described above (Frigg and Nguyen 2016b, 228). The need for imputation is justified by the fact that, according to Frigg and Nguyen, not all the *I*-exemplified properties need to be imputed to the target. This is especially relevant to cases of idealized models where some of the assumptions (i.e. frictionless planes, infinite populations) are not carried over to the target. Imputation therefore connects the world in the model *R* with the target *T.*

E. SUMMARY

The various activities constituting representation described in the DEKI account are represented in Fig. 6. Frigg and Nguyen defend an *indirect* account in which models *M* do not represent a target *T* by directly prescribing imaginings about the latter (as Toon argues). Instead, the imaginings prescribed by the model *M* constitute a world in the model *R* through the processes of exemplification and keying-up. Some elements of *R* are then imputed to the target *T* denoted by *M*. I think that Frigg and Nguyen's account emphasizes the presence of two *selection processes*. First, between *M* and *R* as only some features of *M* are relevant and keyed-up into the world constituted by *R*. Second, between *R* and *T* as only some features of *R* are imputed to *T*.

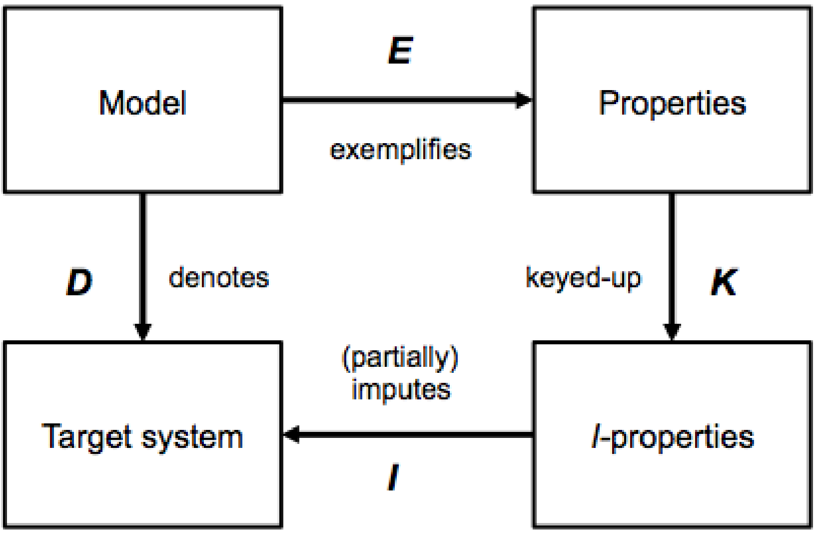

Figure 6 Representation of the Frigg and Nguyen's DEKI indirect account of representation

III. DIFFERENCES AND RECONCILIATION

A. THE MAKE-BELIEVE APPROACH AS A CASE OF DEK(I)

What, exactly, are the differences between the DEKI and the make-believe views? By defending an account which makes representation an indirect relation between *M* and *T,* mediated by *R*, Frigg and Nguyen consider that they compensate for some of the shortcomings of Toon's direct view of representation. By arguing for an unmediated link between the vehicle and the target, Frigg and Nguyen consider that Toon bypasses denotation, exemplification, and keys to focus merely on imputation (Frigg and Nguyen 2016b, 233). I agree that Toon's account blackboxes a lot of what's going between the use of a model *M* and the fictional truths generated from it. In particular, it lacks an explicit emphasis on selective processes, described as exemplification and imputation in DEKI, which filter the irrelevant features of *M*. I don't think, however, that Toon's view takes as extreme a shortcut as Frigg and Nguyen imply. I argue instead that Toon's account is a particular case in the DEKI framework, one in which imputation is rendered invisible (hence a DEK(I) variant) and hence can be perceived as "direct".

In the make-believe approach, models *M* denote a target *T*. These are the atoms and molecules denoted by ball-and-stick models. In Toon's words, what is denoted is the entity, concrete or abstract, about which imaginings are prescribed and fictional truths generated. As for exemplification and keying-up, in both cases principles of generation play a key role. As Frigg and Nguyen notice (Frigg and Nguyen 2016b, 233), Toon argues that

> principles of generation often link properties of models to properties of the system they represent in rather direct way. If the model has a certain property then we are to imagine that the system does too (Toon 2012, 68-69).

In this view, principles of generation indicate which of the features are meant to be turned into imaginings about the target. It therefore indicates how to single out the exemplified properties of a model. Principles of generation also constitute the key with which to turn the exemplified properties into what Frigg and Nguyen described as *I*-properties, and what Toon calls fictional truths. I think it is an uncharitable assessment of Toon's view to state that it neglects denotation, exemplification and keying-up. Doing so would leave unexplained the transformations that occur, in the make-believe view, between something like a ball-and-stick model and the imaginings about atoms and covalent bonds that this model licenses.

The paragraph above, instead, displays how Toon's make-believe view contains the recipe for the creation of a world in the model *R*, similar to the one present in Frigg and Nguyen's indirect view. The existence of this intermediate *R* is precisely what underscores their defence of indirectness in scientific representations. If my interpretation of Toon is correct, how to conciliate the generation of *R* and his outspoken defence of directness? The answer lies, I argue, in the fact that Toon presents scientists using models as *pretending R and T to be one and the same thing*. At first sight (and in Toon's formulation), it gives the impression that there is no additional abstract entity between the model and the target. In my interpretation, however, an abstract entity *R* is indeed created but it is simply not *epistemically* singled out from *T*. In some respects, it turns the DEKI framework into DEK(I). The make-believe view here describes cases in which imputation is made transparent because the *I*-properties (or fictional truths) generated by the association of the model *M* and the principles of generation are *all* imputed to the target *T*. In this interpretation, Toon proposes a superficially direct but implicitly indirect view. Toon's make-believe view presents a special case of the DEKI framework, represented in Fig. 7.

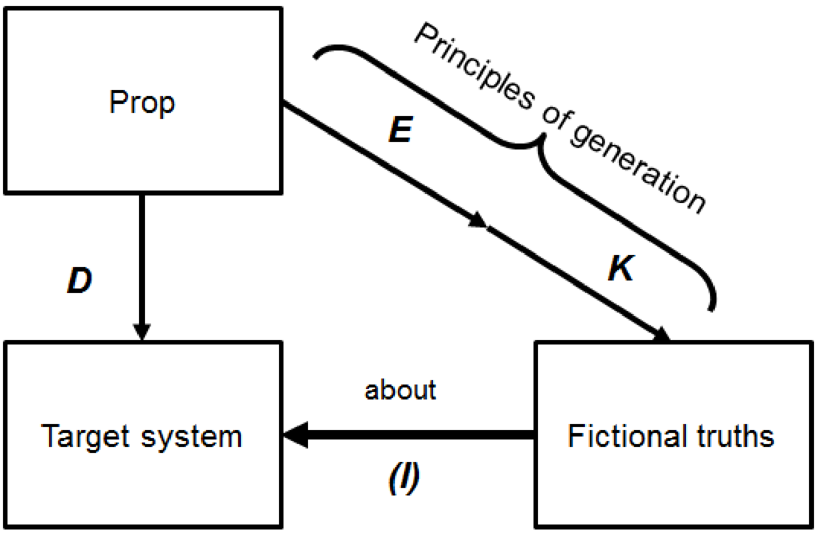

Figure 7 Representation of Toon's make-believe view as a case of DEK(I)

After having integrated Toon's account as a special case of Frigg and Nguyen's framework, I now argue that lineage explanations about unique past events, such as Cavalier-Smith and Martin's, are not only aptly described as one of Toon's special case of a DEK(I) representation but correspond to a case in which exemplification is also made transparent.

B.. LINEAGE EXPLANATIONS AS A CASE OF D(E)K(I)

I first argue that Toon's account of make-believe is directly applicable to representations of unique past events such as Cavalier-Smith and Martin's lineage explanations. These scientists' hypotheses, here a set of sentences, are epistemic entry points to worlds in which the events described in the hypotheses occur. In the make-believe terminology, these hypotheses undoubtedly play the role of props which denote their target, here the origin of eukaryotes. These props, because of their content, prescribe imaginings about a target system. It compels us to believe, in Martin's case, in the engulfment of an alphaproteobacterium by a hydrogen-consuming archaeon and, in Cavalier-Smith's case, in the origin of the flexibility of eukaryotic membranes from the recovery of a loss of the bacterial cell wall. The two propositions enunciated in the last sentence are to be taken *literally* in the context of each scientist's hypotheses. They are akin to what Toon calls fictional truths.

The worlds thus epistemically generated by these lineage explanations are not strictly limited to what is described in each hypothesis. Part of the attempts at consolidating or undermining the validity of each hypothesis revolves around elements that were not explicitly contained in the hypothesis' initial formulation. Examples include the criticisms from de Duve based on the lack of explanation of implicit aspects of Martin's hypothesis. According to him, if Martin's hypothesis proposes an explanation of the origin of mitochondria, this lineage explanation is unable, however, to "explain the development of other complex features of eukaryotic cells, or how that development could have been triggered by the assumed interaction between two prokaryotes". Further, it lacks any "credible mechanism" to account for the replacement of the host cell's own membrane (De Duve 2007, 401). The two gaps, or absence of convincing fictional truths, identified at the time by de Duve have later become, interestingly, the object of attention from Martin and his co-workers. In later papers, they both addressed the link between the acquisition of mitochondria and the origin of eukaryotic innovations (Lane and Martin 2010) and more recently proposed a detailed explanation of the "membrane changeover" critically pointed out by de Duve[34] (Gould *et al*. 2016). A similar strategy is present in Cavalier-Smith's lineage explanation, which has also been criticized, for instance, on its implicit physiological impossibility (Martin *et al*. 2017).

In Cavalier-Smith's and Martin's cases, then, the contents of their lineage explanations go beyond what is explicitly described in each scientist's hypotheses. Instead, the exploration of implicit elements is here deemed important. This is reminiscent of Toon's emphasis on the exploratory dimension in the use of models, and the importance to seek for the implicit fictional truths that their use generates.

In coherence with the introduction of this chapter, I am here agnostic about how accurate to their target are the fictional truths generated from such lineage explanations. What matters, however, is that fictional truths generated from lineage explanations are, in the cases of Cavalier-Smith and Martin, taken *literally*, *as if* they were true. It matters epistemically, since the generation of fictional truths constitute, as the previous paragraphs illustrate, claims to be assessed (the next chapters address how such claims are assessed).

To gain access to the implicit elements of the world contained in Cavalier-Smith's and Martin's hypotheses, it is important to supplement the latter two with the relevant background principles. Similarly, as with Toon, the generation of fictional truths from the use of props is enabled and constrained by the equivalent of principles of generation. Here, these principles are all the relevant background information about the physiology and structures of the microorganisms postulated in these hypotheses. A lot of these principles of generations are kept implicit, and it is quite likely that different scientists can apply different, sometimes contradictory, principles of generation to the hypotheses presented. Such heterogeneity possibly stems in the generation of contradictory fictional truths from the same hypothesis. This issue is given greater attention in Chapter 7.

[34] I do not mean here to establish a direct causal link between de Duve's criticisms and Martin's further work, but rather underline the importance of the exploration of implicit fictional truths for the sake of reinforcing or undermining these hypotheses.

The message, here, is that Toon's make-believe approach to models is particularly compatible with how Cavalier-Smith and Martin's hypotheses, here being ADRs, represent their targets. They do so by acting as props in a game of make-believe. When coupled with principles of generation, it is possible to use these props to prescribe imaginings about the target of interest. Lineage explanations about unique past events, with the two types of explanations they possess, are already quite explicit and describe a substantial portion of the fictional truths to be imagined about their target. However, as Toon rightly emphasizes, the use of these lineage explanations with principles of generation also make it possible to uncover implicit fictional truths which can play an important role in the consolidation and undermining of such explanations.

As I hope the above paragraphs made clear, a major reason for the compatibility between Toon's make-believe approach and Cavalier-Smith's and Martin's lineage explanations is based on the apparent directness of how the latter two represent their target. Keeping with my interpretation of Toon's approach, there is, in this case, no epistemic distinction made between the world in the hypothesis and the "actual" world of the target. The world in the hypothesis generated by the lineage explanations is taken to be a *literal* representation of the target. It is in this sense that I argue lineage explanations of unique past events, such as Cavalier-Smith's and Martin's, to be fictional truths. Such hypotheses provide access to a fictional world that is to be taken as a literal representation of a target of interest, here a unique event in the past.

The way these lineage explanations represent their target is therefore an instance of a case of DEK(I), in Nguyen and Frigg's terminology. Imputation is indeed made invisible by the literal interpretation of the fictional truths generated with these hypotheses. I argue, moreover, that they constitute a case of D(E)K(I). It is easy in these cases to identify denotation. It corresponds to the specification of the target event, the origin of eukaryotic cells in Cavalier-Smith and Martin's case. The action of principles of generation, described in the *keying-up* phase, is also made clear in the generation of implicit fictional truths (or "*I*-properties") described above. However, I argue that the process identified by Frigg and Nguyen as exemplification is invisible. By this, I mean that there is no selection process which sorts out the relevant aspects in lineage explanations about unique past events from the irrelevant ones. All of the elements of Cavalier-Smith and Martin's lineage explanations *are* fictional truths. Further exploration, with the help of the principles of generation, helps generating further, implicit, fictional truths. This absence of exemplification is represented in Fig. 8. This view echoes Weisberg's view on ADRs, which I argue Cavalier-Smith and Martin's explanations to be examples of. I agree with him that

> [b]ecause the theorist is analysing a representation that is directly related to a real phenomenon, anything she discovers in her analysis of the representation is a discovery about the phenomenon itself, assuming that it was represented properly. *There is no extra stage where the theorist must coordinate the model to a real phenomenon* (Weisberg 2007, 226-227, emphasis added).

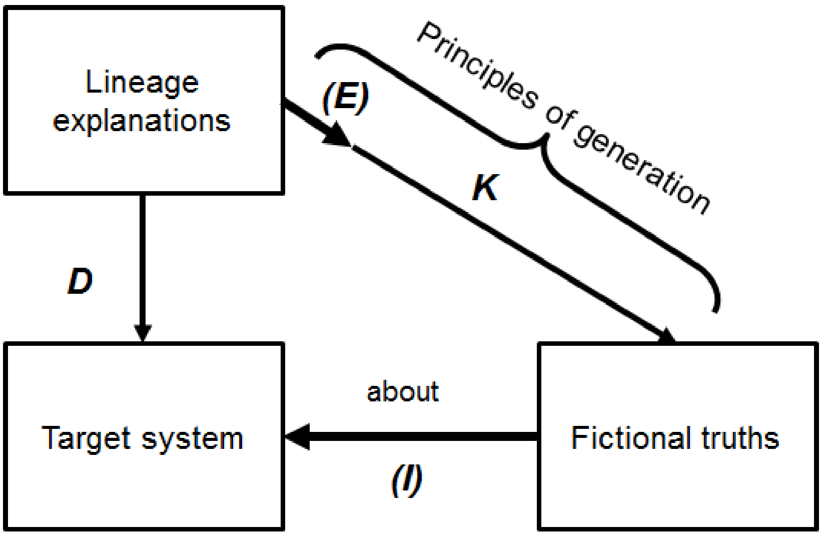

Figure 8 Representation of Cavalier-Smith and Martin's Lineage explanations as a case of D(E)K(I).

CONCLUSION

This chapter was motivated by puzzlement over questions of representation: in what respect could lineage explanations of unique past events, such as the ones of Cavalier-Smith and Martin, be considered as representations of their target? This initial puzzlement has driven me to two main results.

The first is a reconciliation of two conflicting accounts of scientific representation: Toon's make-believe approach and Frigg and Nguyen's DEKI. Authors of the latter presented this account as an improved version of the former. This is done by, according to them, avoiding some of the key mistakes from Toon's account, notably his defence of direct representation. Proposing a different interpretation of Toon's account as a superficially direct but implicitly indirect account of representation allowed me to fit Toon's approach as a special case of Frigg and Nguyen's DEKI framework. I have described the models in Toon's approach as cases of DEK(I), cases in which imputation is made invisible, thus explaining the apparent directness.

This conciliation enabled me to benefit from the fruitful framework laid out by Frigg and Nguyen while keeping some of the appeals of the apparent directness argued by Toon. In a second movement, I've extended their approach to representation (which essentially stems from discussions of scientific models) to my own case study of lineage explanations of unique past events, which are better characterized as abstract direct representations of their target. The characterization of these hypotheses as representations strongly resonates with the way Toon describes representation to occur in his approach. I argued that lineage explanations of unique past events could be adequately described, in Frigg and Nguyen's framework, as cases of D(E)K(I). These are cases where not only imputation is made invisible because of the literal interpretation of the fictional truths these hypotheses generate, but also where exemplification is made invisible because there is no selection of the relevant features in the hypotheses that are turned into fictional truths. In this view, lineage explanations essentially represent "without filters": they directly constitute fictional truths.

This picture of representation, combined with the characterization of Cavalier-Smith and Martin's hypotheses as lineage explanations, provides an interesting entry point to the topic of the next chapter. After having discussed the type of theories these hypotheses were and the way they represent their target, the next chapter critically reviews existing positions on the method of historical sciences. Lineage explanations, composed of a subset of two types of explanations, explicitly articulate a series of fictional truths about the target as well as provide, as I have argued above, an epistemic entry point to the world contained in the hypothesis. Exploring this world is a second source of fictional truths, which this time are implicit, as they were not at first explicitly contained in the lineage explanations. This overall set of fictional truths, both generated explicitly and implicitly by the assessment of lineage explanations, are the claims that are empirically assessed. The "business" of historical sciences can, in this light, be seen as a matter of finding ways to probe the validity of the fictional truths contained in lineage explanations. As the next two chapters will make clear, there is a multiplicity of ways with which such probing can be achieved.

CHAPTER 5: A SURVEY INTO HISTORICAL METHODOLOGY

INTRODUCTION

Chapter 3 and 4 presented philosophical analyses centred on the theories presented by Cavalier-Smith and Martin. What kind of theories are they? To this question, I have provided an answer drawing on the notion of lineage explanations. Lineage explanations track the trajectory of a subject and can accommodate aspects of narrative and mechanistic explanations within them. How do these lineage explanations represent their target events? I argued that they did so by being constituted of and capable of generating further fictional truths. These fictional truths mediate the epistemic access to an imaginary world: the world in the hypothesis. What can be said of this world, the fictional truths that can be drawn from it, is to be interpreted literally, as attempting to provide accurate representations of the events it describes. To assess the validity of these lineage explanations is therefore to evaluate the validity of the explicit and implicit fictional truths that can be drawn from them. The next chapters dive into methodological considerations. I start with a critical review of existing positions on the methodology of historical sciences. It is particularly concerned with (a) the nature and role of lines of evidence, (b) the way they are generated and (c) the bi-directional relation between lines of evidence and the theories they support/reject.

This inquiry does not touch on the specifics of my case study but reaches for a higher level of abstraction. At this level, I do not aim to provide a tidily ordered account of the methodology of historical sciences, one that would apply across disciplines and cases. I doubt of the existence of such a recipe. Instead, I try to identify key generic *components* of these investigations and useful *conceptual resources* that can be mobilized to account for them. The first inquiry places demands that help in the evaluation of the latter. The first part of this chapter uses quotes from analyses of the methodology of human history to identify features shared across historical sciences that need to be accounted for by philosophical accounts of its method. The second part is a critical analysis of the strengths and limits of Cleland's oft-discussed account of the methodology of historical sciences, an account notably centred on the notion of *smoking guns*. The third part presents a portion of the conceptual toolbox defended by Currie which, I argue, complements Cleland's in covering some of the gaps identified in her account. The last, fourth part, applies Wylie's discussion of "security" to characterize the varying strengths of evidence, completing the conceptual methodological picture sketched in Part 2 and 3. I conclude by summarizing how the concepts discussed in part 3 and 4 respond to the demands of Part 1.

I. QUOTES AND INSIGHTS

What are the main features of the methodology of historical sciences? This part makes a first attempt at answering this question by drawing insights from four different quotes. These quotes are taken from analyses of the methodology of human history[35]. They provide concise and sharp illustrations of key aspects of this scientific method which, in line with Chapter 1, I assume to apply across historical sciences. These key points provide guidelines upon which the conceptual resources discussed in the next parts are critically assessed.

The first of these quotes comes from Bloch. In an attempt, already quoted in Chapter 1, to illustrate how much progress has been achieved in historical knowledge, he describes how

> [in a bit more than a century,] gigantic patches of humanity emerged from the mists. Egypt and Chaldea shook off their shrouds. Dead cities of Central Asia revealed their languages that no-one could speak anymore, and their religions that were long extinct (Bloch 1949, 22, own translation).

Bloch's assertion is, of course, metaphorical. He does not describe the zombie-like revival of extinct civilizations. What he means, instead, is that historical scientists have the capacity to revive the *epistemic* existence of civilizations that were once collectively forgotten. This point is even more striking when applied to the past *before* the appearance of humans. Before the historical sciences started dealing with these topics, there was comparatively very little, close to

[35] The choice of using cases from human history, not from the history of other historical sciences such as geology, is done to further emphasize on the methodological continuities (argued in Chapter 1) between human history and historical sciences.

nothing, that could ever be known about events in the pre-human past (i.e. the origin of life). Historical sciences, therefore, are capable of epistemically populating the past in ways that are impossible otherwise.

This quote aims to illustrate the *epistemic optimism* that seems granted by this ability to achieve such progress. Despite the inaccessibility of the object under study, Bloch's quote underlines how historical sciences are characterized by a continuous and spectacular capacity to overcome epistemic limitations posed by the nature of their subject of study. In this view, historical sciences are capable of devising a variety of ways to obtain epistemic accesses to such subjects. By this, I do not mean that all of the puzzles of the past will invariably be solved if given enough time. What I mean is that, generally, there are good chances that some epistemic progress will be achieved about them.

The second quote comes from Arnold, which proposes a subtle description of the relation between our knowledge of the past and our knowledge of the present:

> It has been suggested (by the writer L.P. Hartley) that 'the past is a foreign country; they do things differently there'. Douglas Adams, the science-fiction author, posits an opposite case: the past is truly a foreign country, they do things just *like* us (Arnold 2000, 6–7).

Putting side by side Hartley and Adams' points of view allows Arnold to exploit our conflicting intuitions about the familiarity and the foreignness of the past[36]. I interpret Hartley's side as a warning against the risk of transposing the present into the past. Understanding the behaviour of the entities observed in the present is not sufficient to understand the behaviour of the entities of the past. This is because entities existing in the past have characteristics that are not known or unobserved in present entities. This "foreign country", therefore, requires a study of its own. Adams' point softens without fully contradicting Hartley. When asserting that the entities of the past "do things just *like* us", I interpret Adams as emphasizing the *partial* similarity of these entities to the

[36] It has kindly been suggested to me that Arnold here misquotes Adams' work. Luckily, whether or not it is the case is independent of my interpretation of Arnold's point and its relevance to my argumentation.

entities populating the present. A study of the past, then, is a study of entities that are both familiar in many respects and at the same time differ in several ways from what can be observed in the present.

From this quote, it is possible to infer the fundamental importance and usefulness of our knowledge of the contemporary to understand the past. In this view, historical knowledge benefits from and is shaped by the progress in the understanding of contemporary entities. Better understanding the behaviour of the latter is, in this view, a gateway to a better understanding of their past counterparts. It is clear, for instance, that progress in the knowledge about the origin and evolution of bacterial cells would have been impossible before biologists gained a deeper understanding of the behaviour of contemporary bacteria. The foreignness of the past, however, reminds us that knowledge of the contemporary is important but insufficient: it is important for historical scientists to capture the multiple “foreign” specificities of the entities populating the past.

The third quote comes back to Bloch who, this time, discusses the epistemic value of medieval hagiography:

> From the lives of saints in high middle ages, at least three-quarters of this are incapable of telling us anything solid about the pious characters about which they pretend to retrace the destiny. But if questioned, on the contrary, on the particular ways of living and of thinking of the epochs in which they were written, all sorts of things the hagiographer had no intention of exhibiting, they turn out to be priceless sources. In our unavoidable subordination to the past we have, therefore, at least, freed ourselves from the fact that, forever condemned to exclusively know it through its traces, we nonetheless manage to know a lot more about it than it itself saw fit to let us know (Bloch 1949, 25, own translation).

This quote substantiates the initial argument about epistemic optimism. I take this passage to be of interest on three related points. Firstly, it highlights the *creativity* displayed by historical scientists to extract information about a variety of topics in ways that were not intuitively visible. Here, a purported biography of the lives of medieval saints is turned into an epistemic window on various aspects of medieval culture. The second aspect is the *contextual flexibility* possessed by traces of the past. The same piece of information can be meaningful in a variety of ways, used as evidence in a variety of contexts. Lastly, this quote reminds how inescapably *theoretically mediated* interpretation of traces of the past is. In historical sciences as in other scientific domains, lines of evidence do not “speak by themselves”. The interpretation of traces of the past depends on the bringing together, by historical scientists, of a variety of methodological and conceptual backgrounds that underpin these interpretations. As a consequence, the assessment of claims about the past can focus on several loci. It can focus not only on the nature of the evidence mobilized but also on these conceptual and methodological backgrounds that are brought in for the generation of these claims.

The last of these quotes once again comes from Bloch’s work. It deals with ways to verify the accuracy of the exploits narrated by Marbot, a Napoleonic general, in his autobiography:

> How to verify the anecdote? By calling to the rescue other testimonies. We possess the orders, the notebooks, the reports from the armed forces. They testify that, during the famous night, the Austrian corps, which Marbot pretends to have found the bivouacs on the left bank, were still occupying the opposite bank. From Napoleon’s correspondence it furthermore emerges that, on the 8th May, high waters had not started yet. We eventually retrieved a request for promotion established on the 30th June 1809, by Marbot in person. Among the honours he invokes, he does not say a word about his so-called feats from the month before (Bloch 1949, 52–53, own translation).

In the last paragraph, I highlighted the theoretically mediated dimension of evidence about the past. The above quote highlights that traces of the past indeed do not speak by themselves, but instead that the strength of their interpretation also depends on how they fit with other, related, traces. Here, the credibility of Marbot’s testimony as a factual source about its author’s exploits is undermined by a variety of credible traces. With regards to the nature of lines of evidence in historical sciences, it means two things. First, the information extracted from sources, as well as the latter’s credibility, are dependent on the existing *evidential network* within which they are embedded. Because of that, these interpretations can possess different degrees of *strength*, which can evolve over time with the discovery of new evidence or changes in concepts, methods and theory. Lastly, I also extract from Bloch's quote the possibility for claims about the past to be *conclusively evaluated*. In the example mobilized, Marbot's autobiographical claims are true or false, not *comparatively* true or false. They do not fare worse compared to other hypotheses about what happened on the night of the anecdote. These claims are, here, simply shown to be false.

This short survey stemming from the analysis of four quotes allows me to extract a series of components belonging to the methodology of historical sciences. They are summarized here:

(a) *Epistemic optimism* is generally warranted about the progress of our knowledge of the past.
(b) Knowledge about *contemporary* entities is instrumental to learn about the past.
(c) Historical scientists display *creativity* in the extraction of information from traces.
(d) The same trace of the past possesses *contextual flexibility:* it can be employed as evidence in a variety of contexts.
(e) Interpreting traces of the past is inherently *theoretically mediated*. These traces never speak by themselves.
(f) The existing *evidential network* matters to the meaning and strength of individual lines of evidence.
(g) The interpretation of a trace of the past has varying degrees of *reliability*.
(h) The evaluation of claims about the past is not necessarily conditional or relative, it can also be *conclusive*.

This series of insights provides evaluative guidelines for the critical assessment of existing positions articulated in the philosophy of historical sciences. I focus in particular on the claims these accounts make about the *nature and role of the evidence*, the *way they are generated,* and their *relation with the hypotheses* they are related to. I start by making a critical assessment Cleland's "smoking gun" view of the historical method. I argue that her account is rightly optimistic and forward-looking: it highlights the changes that new trace evidence can bring to an existing epistemic situation. However, I think that it is incomplete in the sense that it emphasizes too much on the search for traces, insists on the necessity of a comparative assessment of hypotheses and disregards the role of theories on the search for evidence.

II. Strengths and limits of the smoking gun view of the historical method

A. Smoking guns - definition

The view detailed here has been defended in the works of Cleland over the years (2001; 2002; 2009; 2011). It describes her vision of the methodology of the historical sciences, contrasted with the methodology possessed by experimental sciences, in order to establish the epistemic legitimacy of the former despite being distinct from the latter. Cleland's account revolves around the notion of *smoking gun*, of which she provides the following definition:

> A smoking gun discriminates among rival hypotheses about long-past, token events by showing that one or more provides a better explanation for the total body of evidence available than the others (Cleland 2011, 554).

This notion can be seen as an attempt to explain how knowledge of the past is constantly reshaped. According to her, historical scientists constantly search for new traces to be added to what has already been collected from the past. This is done with the aim of establishing the *comparative* superiority of a hypothesis over others by the finding of a smoking gun, a trace that destabilizes the current evidential picture by upsetting the evidential equivalence of competing hypotheses. One could, as she does, see this process as ideally converging "upon a single hypothesis" (Cleland 2011, 554), or, if it doesn't, argue for a continuous renewal of competing hypotheses that accounts equally well for the ever-changing total body of evidence[37]. Forber also describes the fundamental importance given to the finding of new lines of evidence and their capacity to break ties between hypotheses. According to him,

[37] See Chapter 7 for a criticism of this particular view about how competing theories are generated.

> Evidence is contrastive. For some data to count as evidence they must favour one hypothesis over some set of alternatives (Forber 2009, 249).

The absence of contrastive evidence corresponds to what he describes as cases of "contrast failure" (Forber 2009, 249).

B. Advantages

Cleland's account is a fundamentally forward-looking one. It successfully emphasizes the continuous need, for historical sciences, to keep the evidential picture moving, hence to enable the continuous progress of the knowledge of the past. Her account thus undoubtedly displays epistemic optimism. She firmly believes that

> our ability to extract information about the past from contemporary phenomena is rapidly increasing, so much so that I suspect the 21st century may become the age of historical science (Cleland 2011, 579)!

Wylie also defends a forward-looking and optimistic line, in the context of archaeology:

> [archaeological data] do (sometimes) have a capacity to challenge and constrain what we claim about the past: they routinely turn out differently than expected; they generate puzzles, pose challenges, force revisions, and canalize reconstructive and explanatory thinking, sometimes raising doubts about even the most well-entrenched presuppositions (Wylie 2002, 191).

In Wylie's view, new lines of evidence (or, in some of the cases she describes, the re-interpretation of old one) have the capacity to keep things epistemically moving and to upset and improve on potentially all of the components of our interpretations of the past.

In his own assessment of the epistemic legitimacy of the historical sciences, Turner highlights a series of factors that warrants a form of pessimism. He first emphasizes the pervasiveness and importance of *information-destroying processes* in historical sciences (Turner 2007, 3)[38]. This concept describes the

[38] The notion of "information-destroying processes" is drawn from Sober 1991.

processes at play that led to the progressive permanent erasure of remains from the past. For instance, fossils are progressively disappearing through the combined actions of erosion and of various destructive human processes (i.e., construction, mining). In this view, this ever-decreasing quantity of traces proportionally decreases what can be potentially known about the past.

The epistemic optimism discussed in part 1 encourages me to side with Cleland and Wylie. While it is true that information-destroying processes are continuously erasing traces from the past, historical scientists have also continuously improved their ability to extract and to interpret these traces. These methodological improvements are combined with the creativity of historical scientists as well as their capacity to exploit the contextual flexibility displayed by these lines of evidence. In addition to this, improvements in knowledge of the contemporary also benefit knowledge of the past. I think that the combination of these positive factors, as a whole, generally overcomes the epistemic hurdles continuously formed through the action of information-destroying processes. It does not mean that historical scientists always manage to overcome these detrimental effects. There are probably areas of the past that are epistemically forever out of reach. However, I think, like Cleland, Wylie and Bloch that it is legitimate to be optimistic with regards to the progress of our knowledge of the past[39].

On another positive note, Cleland's characterization of the effects of smoking guns entails a view in which the evidence's epistemic significance is relative to the already available traces of the past. The effect of new lines of evidence is indeed to selectively support a hypothesis above others *in combination with* what has already been found out, not by itself. This agrees with one of the insights described in part 1.

C. Limits

While the emphasis on the search for smoking guns and the underlying optimism are, I think, strong points of Cleland's account, it is less clear to see

[39] This cautious optimism with regards to how information about the past can be lost, preserved or generated is also captured by Currie's "ripple model" of trace-based evidence in historical sciences (Currie 2018, 111-136).

why the evaluation of hypotheses must be comparative, hence to see why the research of new lines of evidence must necessarily be placed in a competitive context. In part 1, the case of the evaluation of the credibility of Marbot's anecdote illustrated how a claim about the past's truth-value can be evaluated conclusively, not necessarily relatively to another. In this view, Cleland's notion of "smoking gun" and Forber's notion of "contrast failure" overly emphasize the competitive evaluation of hypotheses. It is true that the constant reshaping of our knowledge of the past by the finding of smoking guns *can* establish a competitive advantage of a given hypothesis over others (as Cleland and Forber insist), but I don't believe this is to be an *essential* component of hypotheses evaluation. In the next chapter I illustrate this claim with the case of Archezoa, a classification with evolutionary underpinnings that has also been shown to be wrong, *not* relatively incorrect.

By centralizing her account on the discovery of a smoking gun, Cleland's picture of historical sciences primarily places emphasis on the capacity of new pieces of evidence to *dramatically* reshape the evidential picture of a given claim. This is confirmed in another definition she provides of the term, in which a smoking gun is defined as

> a trace(s) that *unambiguously* discriminates one hypothesis from among a set of currently available hypotheses as providing "the best explanation" of the traces thus far observed (Cleland 2002, 481, my emphasis).

By emphasizing on the "unambiguous discrimination" smoking guns are capable of bringing, I argue that Cleland conveys a slightly distorted picture of historical methodology. This is for two reasons. Firstly, it creates inflated standards for what the effects of evidence should be. Rather than seismic changes in the evidential picture, I think instead that a lot of new pieces of evidence have only minute, uncertain effects. This reflects a criticism already formulated by O'Malley in her study of molecular phylogenies:

> What is happening for the most part in phylogeny […] is not the 'unambiguous discriminat[ion]' of one hypothesis as the best, but tentative arguments for the plausibility of certain broad historical patterns

> and processes given phylogenetic outputs (O'Malley 2016, 12, references removed).

In other words, it is possible that some lines of evidence dramatically change the epistemic situation at play, but most of the time they don't. Instead, they have a much more modest, sometimes yet not precisely determined, sometimes changeable, but non-negligible effect.

In part 1, I mentioned the inescapable interpretative dimension behind the claims and evidence mobilized in historical sciences. I think that the idea of a "smoking gun" confers a rather minimal role to this theoretical mediation. In a literal interpretation of the metaphor, it is indeed not theoretically sophisticated to infer from the smoke stemming from a gun that a shot has been fired from this gun in the near past and to identify the murderer as the person still holding the smoking gun. In her analysis of molecular phylogenetics, O'Malley contests this minimal role ascribed to the investigator by presenting how the evidence is generated through

> model-driven selection and processing of some molecular data and not others – in a manner akin to 'systematic variation' (O'Malley 2016, 12).

The idea behind this criticism is that the trick for historical scientists is not to find smoking guns dispersed in nature (contra Cleland 2002, 490). On the contrary, the generation of evidence is also a matter of questioning new and existing traces in an appropriate way.

Beyond this need for theoretical mediation in the generation and interpretation of evidence, I also think that Cleland's account puts excessive weight on the finding of new traces as a source of epistemic movement. In her view, it seems that the key source of changes for the support of claims about the past is the finding of traces by historical scientists. It gives comparatively little importance, I think, to other ways to alter the support for hypotheses.

This tendency can be illustrated by Cleland's response to Jeffares' defence of the importance of *middle-range theories* in archaeological sciences. Middle-range theories (MRTs) are, in Jeffares words, attempts to find regularities in the way that archaeological sites came about, and to find regularities between observable remains and the behaviours of past people (Jeffares 2008, 472).

In other words, to establish these theories is to provide a theoretical basis that mediates the interpretation of traces of the past as evidence. Examples of MRTs include a framework, established experimentally, that can be used to demarcate, on bones found on archaeological sites, marks left by the result of Hominin tool use from marks left by scavenging canines (Jeffares 2008, 473). It is clear, in this example, that without the existence of such MRT, it would be impossible to determine which of the marks are evidence for Hominin tool use. This is why it is unclear, in my mind, how the establishment of theoretical tools such as MRTs can be "epistemically secondary" to the finding of traces of the past, as Cleland (Cleland 2011, 565-66) and O'Malley (O'Malley 2016, 80) have argued. If by this they mean to affirm the overarching importance of the discovery of traces to support claims about the past, I agree. But I argue that what theoretically mediates the interpretation of these traces, such as MRTs, is equally central and cannot be considered secondary. This asymmetrical emphasis is, I think, problematically embedded in the notion of "smoking gun" that suggests a form of trace-centrism.

The last critical point I bring about Cleland's account ties this chapter with some of the points raised in the previous two chapters. As a reminder, I argued that hypotheses about past events take the shape of lineage explanations, which track the becoming of a subject through a series of steps. These lineage explanations are constituted of and can be used to draw *fictional truths* about the target event. *Explicit* fictional truths are already present in the formulation of lineage explanations and *implicit* fictional truths can be derived from the exploration of the fictional worlds these explanations contribute to creating. The evaluation of hypotheses about unique past events depends on the evaluation of these implicit and explicit fictional truths generated from lineage explanations.

Cleland's account, in my interpretation, only explicitly acknowledges the influence of the evidence on hypotheses. The influence from hypotheses to evidence seems, however, neglected. While it is uncontroversially important that the finding of new evidence is crucial to support or reject hypotheses about the past, I think that more should be said about how the reverse influence occurs. Her discussion of narrative explanations and predictions, I think, illustrate this relative disregard from Cleland.

Cleland conceives narrative explanations as telling “a coherent causal story about how a puzzling contemporary phenomenon, a trace, was produced” (Cleland 2009, 53-54). In this view, narrative explanations link the past with the present by explaining *the contemporary presence of the trace*. Such narratives are argued by Cleland not to be of primary methodological importance. In her view, they both require the invention of “many of the events in the narrative sequence” and consequently relegate justification “to a minor role” (Cleland 2009, 54). Because of that, Cleland considered it important to place as little emphasis as possible on the construction of narratives. Otherwise, this would divert attention from the primary task of historical sciences, namely

> explaining observable phenomena in terms of unobservable causes […] (Cleland 2001, 987).

In other words, the relation of influence is here only conceived to go from the available evidence to the inferred common cause.

This unidirectionality is also illustrated by her treatment of predictions in historical sciences. A typical instrument of the experimental sciences, the activity of prediction is a typical case of a movement from hypotheses to evidence, hypotheses producing expectations with regards to the evidence to be obtained. In historical sciences, Cleland argues, these predictions “are typically too vague for their success or failure to play central roles in the evaluation of the hypotheses with which they are associated” (Cleland 2011, 553). They only serve as

> tentative guides – educated guesses, based informally upon both theoretical and empirical background knowledge – about where additional evidence (ideally, a smoking gun!) *might* be found for a hypothesis and perhaps even what form it *might* take (Cleland 2011, 563).

In the roles ascribed to narrative explanations and predictions, I essentially agree with Cleland's observations. I disagree, however, with the interpretation she draws from it.

First, I agree that the postulation of narrative explanations that thoroughly explaining the genesis of traces of the past with causally uninterrupted sequences of events is not very interesting. However, this is not the role I assign to lineage explanations (of which I argued, in chapter 3, narrative explanations were a subtype). These explanations, instead, describe a causal sequence between two events located *in the past*. It aims at explaining the occurrence of a past event, not to explain the evidence available in the present. Traces of the past, such as smoking guns, are evidence in support of such types of explanations[40].

Second, I also agree that predictions from hypotheses in historical science can be tentative guides and that they do not play as much of a central role in the confirmation and rejection of hypotheses as they play in "paradigmatic cases" of experimental sciences. However, I think that the role she attributes to predictions, namely shaping the search for evidence by playing the role of a tentative guide, is already quite important! Disregarding this importance is equivalent to distinguishing strongly between the so-called "context of discovery" and "context of justification" and giving epistemic importance only to what belongs to the latter. More specifically, if historical sciences are, as typically characterized, operating under a scarcity of evidence, then any element that could help to find evidence is playing a non-negligible role.

D. *Summary*

An analysis of Cleland's account in light of the components identified in part 1 results in a mixed assessment. She describes a methodology primarily dedicated to the search for remains of the past, "smoking guns", that are capable of shifting the evidential picture by comparatively supporting a hypothesis over several others for the explanation of a phenomenon. I think that

[40] To my knowledge, Cleland doesn't explicit discuss the type of hypotheses that common-cause explanations were. It is possible, then, that they can be compatible with lineage explanations.

this account has the advantage of being forward-looking and optimistic. It provides a mechanism explaining the gradual improvement of existing hypotheses as historical scientists' ability to extract new lines of evidence improves. It also rightly highlights that the meaning and importance of new lines of evidence is dependent on the existing evidential network available in a given investigation. Smoking guns, in her view, dramatically shift the epistemic status of existing hypotheses in combination with the already available evidence, not without it.

Further assessment of Cleland's account, however, reveals some limits of her account. Some of them are straightforward disagreements on what is part of this method. Others are simply disagreements on matters of emphasis and priorities. I disagree with her view that the evaluation of hypotheses in historical sciences is necessarily comparative. I provided an example of a conclusive assessment in part 1, and provide another one in the next chapter. Seeing historical sciences as merely capable of granting a relative advantage of a hypothesis over another is, in my opinion, underestimating how much can be done in this set of disciplines. I also disagree with the relative disregard her account displays of how hypotheses influence the search for evidence. Her account focuses exclusively on how new evidence influences existing hypotheses and, I think, lacks of an articulation of how the relation runs in the other direction. Her view on prediction suggests that she considers the influence of hypotheses to be fortuitous and not crucial to the finding of new evidence.

I also argued that Cleland's account placed too much emphasis on a restricted set of things. By presenting the discovery of smoking guns as the main aspect of this method, I argued that it illegitimately backgrounds other lines of evidence that cause a less dramatic impact on the evidential picture. I also argued that this view focuses too intensely on the traces of the past by themselves and does not say enough about the conceptual and methodological framework that mediates their interpretation. In this case, the "smoking gun" metaphor is misleading since it does not bring attention to what enables the interpretation of a piece of evidence. Backgrounding the interpretative dimension similarly restricts the wide variety of things that can have an impact on the degree of support of existing hypotheses. By focusing too much on traces of the past, Cleland backgrounds the wide range of effects that changes in the conceptual and methodological interpretative framework can have on the solidity of historical hypotheses. On the whole, I argued that a description of the methodology of historical sciences has to focus more equally on what enables the finding and interpretation of the traces of the past, the latter about which Cleland's brings excessive emphasis. It is not so much that I think Cleland's emphasis on smoking guns is invalid, rather than that it is incomplete and too narrow in its scope.

Part 3 of this chapter focuses on Currie's vision of the methodology of historical sciences. It provides a critical analysis and a refinement of some of the concepts and insights that he and like-minded scholars propose. I argue that his concepts are filling some of the shortcomings of Cleland's account and are more in line with the insights identified in part 1.

III. Omnivories, scaffolds and virtues

A. Methodological omnivory and investigative scaffolds

A central notion in Currie's account of the methodology of historical sciences is *methodological omnivory*. In this section, I analyse its meaning and the methodological components it takes into account. In particular, I argue that there are two distinct interpretations of Currie's notion, namely *evidential omnivory* and *methodological autotrophy*.

In the animal world, the notion of omnivory captures the capacity of organisms to consume as food any type of organic matter. In the scientific study of the past, investigators are methodologically omnivorous in the sense that

> [r]ather than specializing in a certain kind of method, a certain array of tests, or a certain set of epistemic practices, historical scientists are opportunistic: drawing on whatever resources they can, at many levels of grain, to triangulate their way to plausibility (Currie 2015, 188).

This first understanding of methodological omnivory is what I call *evidential omnivory*. Evidential omnivory designates the ability of historical scientists to draw on any kind of resource as long as it is capable of making a difference in the current evidential picture. This point echoes the emphasis on creativity made in part 1. It is, in some ways, an antidote to what Chapman and Wylie describe as a risk to assign a "foundational status to any one line of evidence"

(Chapman and Wylie 2016, 181). I argue that evidential omnivory avoids some pitfalls of the "smoking gun" view. In this view, traces of the past are resources among others in the search to support or discredit hypotheses of the past: no *a priori* restriction is placed on what counts as evidence[41]. This first interpretation of Currie's notion of methodological omnivory is thus about the *nature* of the evidence mobilized in assessing claims in historical sciences.

The second interpretation of Currie's concept concerns the *processes* by which this evidence is *generated*. In this sense, methodological omnivory is understood as the capacity possessed by historical scientists to generate "new evidence by designing new techniques and tools" (Currie 2015, 196). This emphasis on the innovative capacities of investigators is what I call *methodological autotrophy*. Autotrophy is a biological term. It designates the capacity by organisms (autotrophs) to produce complex organic foodstuff from simple molecules. I privilege autotrophy over omnivory. It captures better the creative dimension possessed by scientists, in a context that requires them to achieve so much with so little (*cf.* Currie 2015, 198). This generative capacity is absent in the notion of omnivory, which merely emphasizes the diversity of things that can count as food for the organisms concerned. Methodological autotrophy, in other words, designates the construction of

> purpose-built epistemic tools tailored to generate evidence about highly specific targets (Currie 2015, 187).

Methodological autotrophy describes the development of methods that extend our epistemic reach by bringing a new perspective to existing traces or generating evidential relevance to previously unexploited elements. One example is the application of radiocarbon dating to archaeological sites (see Manning 2015 for a historical overview). The application of this technique enabled an increase in the precision of the dating of archaeological objects. Another example (taken from Currie 2015) is the devising of a model, the "Bite

[41] Currie provides a detailed and systematic discussion of the varieties of "non-trace evidence" in historical sciences (Currie 2018). Here, I am rather focused on the methodological richness of historical sciences, in order to complement Cleland's narrow methodological focus on the search for smoking guns.

Force Quotient", from anatomical data from extant and extinct animals. This model was used to provide evidence for the type of hunting behaviour of an extinct specimen of interest, here *T. carnifex*. The construction of such tools can combine both trace evidence as well as non-trace evidence coming from relevantly analogous elements. As such, they are also illustrations of evidential omnivory in action[42].

This characterization of methodological autotrophy paves the way for another important conceptual element discussed in Currie's framework, *middle-range theory*, which has already been mentioned above. In Currie and Sterelny's words, an MRT is the interpretative theoretical package that "tells us how an event's footprint at a time is made and then transformed" (Currie and Sterelny 2017, 19). MRTs are usually tightly adapted to the type of thing from which evidence is extracted and the type of contexts in which evidence is used. These rather specific theoretical packages, according to Currie and Sterelny, are attempts to capture the effects of "the processes which shape history":

> Fossilization, political revolutions, mineralization, mass-extinctions, economic pressures, and so forth, [these processes having] more-or-less regular effects. Moreover, the signs of those effects change over time in reasonably recognizable and well-understood ways. Reconstruction of the past is possible in virtue of these processes and our understanding of them (Currie and Sterelny 2017, 19).

Without MRTs, it is impossible to turn a trace of the past into lines of evidence. In the study of the human past, it is this need for MRTs that is at the origin of what is called the "auxiliary sciences of history", reflecting the vast diversity of expertise (thus the evidential omnivory) needed to investigate the past. It includes, in this context, the capacity to

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[42] Currie also mentions the capacity of simulations as "surrogate experiments" as another strategy to make use of non-trace based evidence in historical sciences (Currie 2018, 249-274). The epistemic value of simulations is still under debate (see Morgan 2003 ; Morrison 2015 ; Winsberg 2003 ; Barberousse et al. 2009). Since neither Cavalier-Smith nor Martin appear to employ these strategies, I choose to leave this topic aside here.

> read critically a medieval charter, to explain correctly the etymology of place-names, to date unerringly the ruins of dwellings of the prehistoric, Celtic, or Gallo-Roman periods, and to analyse the plant life proper to a pasture, a field, or a moor (Bloch 1949, 68; cited in Chapman and Wylie 2016, 135).

One example of MRT was provided in part 2 with Jeffares' discussions of the epistemic tools produced to distinguish between bone and tool marks on bones in archaeological setups. Another illustration from archaeology has been provided by Chapman and Wylie in their discussion of the increasingly sophisticated methods that allowed radiocarbon techniques to provide increasingly reliable evidence to archaeological investigations (Chapman and Wylie 2016). Wylie has also discussed the various methodological strategies employed for the re-use of "legacy data" in archaeology (Wylie 2017), bringing new interpretations to previously used elements of the material record. O'Malley discusses a similar requirement for adequate MRTs in the case of molecular phylogenies. This time, the attention is placed on how models underlying selection of genomic data, combined with the increased sophistication of the "molecular clock" model[43], constitute an increasingly effective method to "extract signal from tremendously noisy molecules" (O'Malley 2016, 69).

To summarize, methodological autotrophy – the methodological creativity of historical scientists – highlights the ability of historical scientists to construct a variety of epistemic tools. This variety of tools, which include MRTs (theories that link traces of the past with their contexts of origin), is what underpins the evidential omnivory observed in historical sciences. In this picture, both aspects of methodological omnivory are distinct but closely tied. Together they echo many of the insights evoked in part 1. Methodological autotrophy emphasizes historical scientists' creativity and licenses epistemic optimism: methodological innovations will increase our epistemic reach into the past. MRTs are often dependent, in their creation, on a lot of knowledge of the contemporary (and thus are often built using non-trace evidence). Eventually, it provides a conceptually precise way to characterize how the interpretation of traces of the

[43] The molecular clock is a model of the varying mutation rates of genetic material.

past is theoretically mediated. This emphasis on the creation of MRTs is here rightly considered as an epistemically central aspect of historical sciences.

MRTs present background theories that are at play in historical sciences as enablers. This positive vision contrasts with the main arguments mobilized by Turner to ground his epistemic pessimism concerning historical sciences. In a book-length treatment, Turner compares our knowledge of the past with our knowledge of microphysical particles. He argues for “historical hypo-realism”, a position that asserts that

> the standard arguments for realism (if they are any good at all) give less support to minimal epistemic realism about the past than to minimal epistemic realism about the tiny (Turner 2007, 61).

This relative pessimism is grounded on the identification of two asymmetries between investigations of the past and investigations of the tiny. The first is the “asymmetry of manipulability”, which states that

> [o]ur ability to manipulate tiny things and events helps us a great deal in our endeavours to acquire knowledge of the microphysical structure of the universe. But if we seek knowledge of the past, we will have to do without this help (Turner 2007, 25).

In other words, compared to the study of microparticles that benefit from our ability to physically access and manipulate them, the physical inaccessibility of objects from the past puts us in a disadvantageous epistemic posture. The second asymmetry is the “asymmetry of background theories”, which states that

> In historical science, background theories all too often tell us how historical processes destroy evidence over time, almost like a criminal removing potential clues from a crime scene (Turner 2007, 3).

This asymmetry relies on an argument already discussed above, which is the asserted pervasiveness, in historical investigations, of information-destroying processes. In the comparison with the study of microphysical particles, instead, Turner states that

> background theories about the microphysical world frequently do tell us how to create new evidence by which to test claims and theories (Turner 2007, 25).

An optimistic outlook on historical sciences, which focuses on the successful generation of MRTs that further our epistemic reach, goes in a completely opposite direction. It denies grounds to the asymmetry of background theories as it emphasizes how historical scientists continuously shift their current evidential limits (and the effects of information-destroying processes) by devising methodological innovations. In this view, new background theories *do* tell us how to create new evidence with which to test claims and theories[44]. These MRTs rely on regularities sometimes obtained via experimental investigations. This counteracts the asymmetry of manipulability. The ability to manipulate contemporary entities not only benefits knowledge about contemporary entities but also can directly increase knowledge of the past. A focus on methodological autotrophy, therefore, provides ways to undermine Turner's argument for the comparative epistemic advantage of particle physics over historical sciences.

The notions of methodological autotrophy and the construction of MRTs are also reminiscent of Ginzburg's discussion of an "indicial paradigm" shared across a multitude of practices, scientific or not. A shared assumption of these practices, Ginzburg argues, is that

> [t]hough reality may seem to be opaque, there are privileged zones – signs, clues – which allow us to penetrate it (Ginzburg 2013, 123).

An illustration of this paradigm is the work of art connoisseur Giovanni Morelli. In the 19th century, Morelli dealt with the problem of the attribution of paintings, such as "how to ensure the authenticity of a Botticelli?" Facing this difficult task, he proposed to shift our attention away from the

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[44] Of course, I do not deny that, in parallel to this, our understanding of the information-destroying processes also gets improved. I wanted to highlight, however, that Turner's sole emphasis on these processes in historical sciences was unwarranted.

> most conspicuous characteristics of a painting, which are the easiest to imitate: eyes raised towards the heavens in the figures of Perugino, Leonardo's smiles, and so on (Ginzburg 2013, 97).

Instead, he hinges his paintings' attributions on the examination of the

> most trivial details that would have been influenced least by the mannerisms of the artist's school: earlobes, fingernails, shapes of fingers and of toes (Ginzburg 2013, 97).

By doing this, Morelli made the assumption, in Wind's analysis, that "personality should be found where personal effort is weakest" (Wind 1985, 38; cited in Ginzburg 2013, 98). Because the least noticeable details of paintings are those made with the least conscious effort, Morelli presumed that they provided privileged loci of attributions. This assumption provides to Morelli an MRT that turns specific elements of paintings into important evidence.

Similar to Currie, Ginzburg places emphasis on the creativity of investigators to devise methodological resources to extract evidence from a variety of sources. Ginzburg insists, however, that these practices are mostly *tacit* forms of knowledge, which are

> richer than any written codification; it was learned not from books but from the living voice, from gestures and glances; it was based on subtleties impossible to formalize, which often could not even be translated into words (Ginzburg 2013, 114–15).

Contrary to Ginzburg, I do not have a definite opinion on how tacit the MRTs created by historical scientists are. A bias in my dissertation's analysis is that my resources are only textual. Therefore, I only have an access to how Cavalier-Smith and Martin's methods and theories are *propositionally* articulated. Having said this, it is clear that some of their epistemic tools are, at least, partially explicitly articulated. It is likely, however, that tacit skills and assumptions are also brought in in these processes.

B. Investigative scaffolds and productive speculation

I have argued earlier that Cleland's account does not say enough about the influence of the formulated hypotheses on the search for evidence. I argued that an account built around the notion of "smoking guns" is too unidirectional in its emphasis, as it accounts solely for how new (trace) evidence impacts existing hypotheses. This conceptual incompleteness is, I think, compensated in Currie's framework with the notions of "investigative scaffolds" and "productive speculation".

The notion of *investigative scaffolds* is devised to capture some of the concrete consequences of the progress in historical knowledge. It designates cases in which

> a set of claims must already be on the table for new evidence to be relevant. Investigation is piecemeal and comes in stages: both the plausibility and richness of hypotheses is built step-by-step. As scaffolds are reached, new data gains evidential relevance (Currie 2015, 188).

In other words, scaffolds are bits of *consolidated* knowledge which further empirical investigations by supporting new lines of inquiries and new searches for evidence.

The notion of productive speculation stems from a discussion of the importance of narrative explanations in the investigation of unique past events (Currie and Sterelny 2017). It is very similar to investigative scaffolds. The difference being that, in the case of productive speculation, the elements of explanations postulated by scientists are *speculative*, they are not yet (and might never be) considered as consolidated bits of knowledge. Prost provides an instance of such speculation in the context of human history:

> The atrocities committed in Italy by French troops after the Battle of Monte Cassino have probably been allowed by General Juin, but no document signed by his own hand certifies it (Prost 2010, 855, own translation).

Such speculations, Currie and Sterelny argued, are "central to successful historical reconstruction" (Currie and Sterelny 2017, 14). It is necessary because, as the case discussed by Prost highlights, the building of a narrative

> typically involves the reconstruction of causal intermediaries that have left no unambiguous trace in the present; positing rather than finding links in a causal chain (Currie and Sterelny 2017, 15).

While this practice has sometimes been viewed with hostility (see Cleland's discussion in part 2) and used to underline the inferior epistemic status of historical sciences (see, for instance, Gee 2000), Currie and Sterelny aim at separating the wheat from the chaff by distinguishing between *productive* and *idle* speculation.

To engage in productive speculation is to fill in the gaps of a narrative in a way that "serves to increase the empirical constraints on historical reconstruction" (Currie and Sterelny 2017, 16). This type of speculation "reveals avenues for testing" (Currie and Sterelny 2017, 19) by enabling historical scientists to recruit new lines of evidence. It can make relevant previously unsuspected elements, which then helps the generation of new MRTs. Alternatively, some of the speculative elements can turn out to be confirmed and provide investigative scaffolds for further empirical research. An instance of productive speculation is discussed in the next chapter, as Martin and Müller's hydrogen hypothesis for the origin of eukaryotes (Martin and Müller 1998) brought to the forefront issues of anaerobic metabolism (notably through the attention brought to hydrogenosomes*) which drove investigations in directions that were not previously considered relevant. In addition to this, the hydrogen hypothesis – when considered valid – forms an investigative scaffold which fuels further research to work out the details of the initial explanation. All this is opposed to idle speculation, argued to be "mercifully rare in science" (Currie and Sterelny 2017, 15). This form of speculation is incapable of keeping the evidential picture moving, the additional claims it brings do not carry with it signs of empirical tractability and therefore should be avoided.

The building of lineage explanations, when integrating elements of productive speculation, can generate theoretical elements that can act, to borrow Turner's terminology, both as unifiers and tools. They are unifiers because they can "give a more or less unified or coherent account of the observable evidence" (Turner 2007, 70) and tools because they are instrumental to the generation of new lines of evidence. On the whole, productive speculation and investigative scaffolds are notions that help to clarify how hypotheses about unique events can influence the production of evidence. It occurs by facilitating the generation of evidential relevance and epistemic tools, including MRTs, that underlie the constitution and interpretation of new lines of evidence (or the reinterpretation of existing ones).

C. *Coherence and consilience*

This last section in the discussion of Currie's conceptual framework deals with aspects that are not usually much expanded upon. It is absent from part 1's methodological points but, I think, matters in the evaluation of claims about unique past events. This last ingredient concerns the notion of coherence. It stems again from Currie and Sterelny's defence of narrative explanations and, as with Currie's notion of methodological omnivory, can be decomposed into two distinct aspects.

As Currie and Sterelny emphasize,

> [c]oherence is a much under-rated epistemic virtue. Achieving it involves much more than establishing mere logical consistency between what is said about one stage of a trajectory and what is said about the other stages (Currie and Sterelny 2017, 17).

According to them, producing coherent lineage explanations[45] is a "considerable epistemic achievement" (Currie and Sterelny 2017, 19). This comes from the fact that

> [a]s our information about the causal background is enriched, coherence becomes an increasingly important, increasingly demanding constraint. So, for example, a theory of the stability conditions of human cooperation has to fit a larger number of empirical and theoretical constraints (Currie and Sterelny 2017, 19).

I argue that two distinct notions are at play in this appeal to the virtues of coherence. In the last quote, what Currie and Sterelny describe concerns the relation between hypotheses and evidence. They emphasize how difficult it is to successfully take the available evidence into consideration. I think, rather, that this dimension of coherence is better characterized as "consilience". Consilience, in Whewell's terminology, occurs when "an Induction, obtained from one class of facts, coincides with an Induction, obtained from another

[45] In their article, Currie and Sterelny speak of narrative explanations.

different class" (Whewell 1840, 23). Understood in the present context of evidential omnivory, consilience is defined as the coherence of an inferred claim with a diversity of evidence[46]. This understanding of coherence as consilience supplements the more traditional understanding of coherence as mere logical consistency. This latter view concerns the relation between elements *within* hypotheses and designates the absence of inconsistencies within them. Because of this, I hereafter use the word *consistency* to point to this other dimension of coherence.

Currie and Sterelny have illustrated the difficulty of keeping both virtues together in the context of the study of the origin of human cooperation. Researchers have continuously improved their understanding of the factors (components of the so-called "cooperation stew") that have to be taken into account by hypotheses about the origin of this human trait. This increased pressure on the consilience side of things has so far resulted in the failure to create a coherent "step by step account of the transition to human ultra-cooperation" (Currie and Sterelny 2017, 20). A similar situation, according to Malaterre, exists in discussions about the origin of life. These investigators have so far been successful at increasing the pressures on consilience through an increased fragmentation of the problem and hence of the number of factors that have to be taken into account. The resulting difficulty in creating a coherent explanation for the origin of life has thus pushed researchers to decrease their ambitions. Instead of trying to "formulate a 'theory' about the origins of life", they rather aim "to identify links susceptible to intervene in such a theory" (Malaterre 2010, 53, own translation).

Despite these examples, the importance of virtues of consistency and consilience has, as Currie and Sterelny argue, sometimes been underrated.

[46] Currie recently discussed the importance of consilience in historical sciences. By defining consilience as "the exploitation of independent evidence streams" (Currie 2018, 138), he grounds it on the diversity of *methodologies* with which evidence are obtained. His use of consilience therefore differs from mine, which here focuses on the *nature* of the evidence. Currie's use is equivalent to Wylie's "horizontal independence" (see Currie 2018, 163) discussed below.

Cleland's account[47] of the methodology of historical sciences is a comparative one. It considers as unproblematic the existence, and thus the generation, of a multitude of competing hypotheses that successfully account for existing empirical evidence (see, for instance, Cleland 2001, 988). The emphasis on smoking guns is precisely designed for situations where an epistemic tie between competing hypotheses needs to be broken. An account starting with the existence of multiple empirical equivalent hypotheses as the routine situation in historical sciences presents, I think, consilience as trivially maintained. Cleland similarly downplays consistency as part of her rhetoric against the importance of narratives which was already evoked above. As a reminder, retaining consistency in a narrative is criticized as necessitating the invention of events in the narrative sequence linking the multiple traces of the past with the common cause of their existence. I already argued against this view since I believe lineage explanations have a different explanatory target (they explain past events, not available evidence). Emphasizing (contra Cleland) the difficulty of achieving consilience provides further arguments against her view. As the amount and variety of evidence pile up, accounting for the entire evidence and retaining consistency becomes both more difficult and more valuable[48]. In this view, I am sceptical that situations of epistemic equivalence are as pervasive as Cleland describes.

In addition to Currie and Sterelny, the difficulty of resisting the interlinked pressures from considerations of consilience and consistency has already been emphasized elsewhere. Kosso, for instance, argues for a "weighted coherence" view that simultaneously points to the importance of hypotheses to be "free of contradiction" (Kosso 2001, 75), but also to the fact that

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[47] As will be clear in Chapter 7's discussion of underdetermination, Turner also makes similar commitments.

[48] This discussion here is independent of whether the evidence mobilized is trace-based or not. To be sure, my emphasis on consilience is devised to counterbalance Cleland's unidirectional emphasis on the influence of evidence on hypotheses. Consilience, here, is not seen in itself as an alternative, non-trace-based, source of evidence.

> some claims in the network are epistemically weightier than others and are less likely to be challenged or abandoned (Kosso 2001, 92).

This formulation provides a possible entry point to a sophisticated understanding of the relation between consilience and the evaluation of hypotheses. Foregrounding consilience and consistency is indeed not the same thing as placing all of the elements of lineage explanations on the same plane and considering all of them to be evidentially undifferentiated. In Kosso's view, it is more important for lineage explanations to be consistent with some well-supported claims than others which, at a given stage, are more akin to productive speculation. Wylie has also previously highlighted the epistemic achievement constituted by keeping consilience and consistency in the context of archaeology:

> Most often the problem in archaeology is not to adjudicate between a number of equally plausible, well-supported, explanatory alternatives but to find one account, one reconstructive or explanatory hypothesis, that is consistent with all the lines of evidence that are constructed under diverse resources (Wylie 2002, 197).

There is a general message lying behind all these claims about the importance of consilience and consistency (as well as productive speculation). They all convey the idea that "hypotheses matter" and belong as much at the centre of the attention of accounts of the methods in historical sciences as does the impact of evidence on hypotheses. Far from being mere postulations of a common cause behind accepted evidence, the concept of productive speculation insists on the hypothesis' influence on further inquiries. The emphasis on consilience and consistency underlines the epistemic achievements lying behind their successful formulations.

D. Summary

This section presented and critically assessed a series of concepts taken from (but not exclusive to) Currie. I argue that they helped build a more complete set of conceptual resources to understand the methodology of historical sciences. I first argued that Currie's notion of methodological omnivory could be broken down into two distinct notions: evidential omnivory and methodological autotrophy. The former concerns the nature of the evidence mobilized in historical sciences and leaves room for more variety (actually, it does not place formal restriction) for the type of things that counts as evidence in historical sciences. Evidential omnivory underlines historical scientist's opportunism by characterizing them as using as evidence anything that can help them constrain their claims[49]. These lines of evidence can be trace-based, but they can also come from what Currie calls "analogues", defined as "naturally occurring surrogates of past entities" (Currie 2018, 135). The notion of methodological autotrophy emphasizes the ability of historical scientists to generate a multitude of methodological constructs, including middle-range theories, to help generate and interpret evidence. Both these aspects of methodological omnivory underlie an optimistic account of historical sciences. Contrary to Cleland, this account of methodology pays more attention to the virtues of the hypotheses and how they help drive the generation of evidence. The notions of productive speculation and investigative scaffolds, in particular, denotes the virtue from these hypotheses to help keep the investigation running by establishing evidential and conceptual relevance and facilitating the generation of MRTs. In addition to this, I identified in Currie and Sterelny's understanding of coherence the conjoined virtues of consilience and consistency – respectively the ability to account for a variety of evidence and the lack of internal inconsistencies. The difficulty for hypotheses to display both these virtues together is another reminder of the philosophical attention needed on the constitution and role of hypotheses.

On the whole, Currie's conceptual arsenal helps build a richer picture of the methodology of historical sciences, which pays attention to a wider variety of aspects. As it is, it covers a majority of the points summarized at the end of Section 1. These concepts, however, do not constitute helpful resources to articulate an understanding of the *strength* of the evidence mobilized. The next section fills in this gap with Wylie's threefold articulation of the notion of "security".

IV. The strength of evidence: Wylie on security

[49] This view is particularly compatible with the relational account of evidence (Leonelli 2016).

This last section aims at completing the methodological picture for historical sciences by reviewing the various meanings of the concept of security as discussed by Wylie. This articulation, I think, describes in a precise way the various loci that can be strengthened or weakened in the defence of hypotheses. These three understandings of security concern the investigative scaffold mediating the interpretation of a trace of a past as evidence, the nature of the information extracted from this trace, and the complexity of the link between the evidence and the claim it supports (or rejects).

In the first understanding, security designates the

> credibility of the source field and the degree to which the appropriated theory is uncontested within the contexts in which this theory was originally developed and applied (Wylie 2002, 175).

In this first meaning, an interpretation is secure if the discipline from which an MRT is developed and the theories within these fields are considered credible. Credibility decreases in cases in which MRTs are built out of contested theories or, worse, from fields with contested credentials. The use of radiocarbon decays in archaeology, or of genomic sequences in molecular phylogenetics, are examples of MRTs that have been considered as increasingly credible over time as their theories and fields of origins matured.

The second understanding of security concerns the *nature* of the imputed link:

> whether, or to what degree, the background knowledge in question establishes an exclusive and determinate connection between archaeological remains and the antecedent conditions or processes thought to have produced them (Wylie 2002, 175).

This second notion of security is located a step further into the process of argumentation: granted that the MRT underlying the interpretation of a trace is credible, this notion deals with how *determinate* the connection generated between a set of traces and the phenomenon of interest is. It might well be possible that a credible MRT only generates weak interpretations. Re-using the previous examples, it is possible that radiocarbon analysis dates samples with a large margin of uncertainty, or that phylogenetic analyses on a series of gene sequences generate weakly supported phylogenetic trees. A line of evidence is here secure if it supports the generation of determinate claims from it.

Finally, security can also be understood as telling something about

> the number and complexity of the linkages required to connect a body of archaeological material to those dimensions of the cultural past that are of particular interpretive or explanatory interest (Wylie 2002, 175).

This sense of security aims to capture the *directness* of the link between a given line of evidence and the claim it purportedly supports. The more complex this link is, the more fragile is the interpretation since this link then possesses more potential loci of fragility.

To summarize, these three notions of security as credibility, determinacy and directness cover a broad range of aspects of the evaluation of hypotheses about the past. They concern the evaluation of the background knowledge mediating the interpretation (credibility), the strength of the interpretation from a line of evidence (determinacy) and the complexity of the link between evidence and hypothesis (directness).

Conclusion

This chapter presents a critical survey of the existing literature on the methodology of the historical sciences. The first part extracted a series of key components of such accounts from the analysis of quotes from cases of human history. These key components served as guidelines and standards to critically assess, in part 2, Cleland's account of the method of historical sciences. In part 3 and 4 I presented a series of concepts, respectively stemming from Currie and Wylie's work, that together provide an arsenal of conceptual resources for the study of historical sciences. I conclude now by mapping the components identified in part 1 with the concepts discussed in part 2, 3 and 4.

Epistemic optimism towards historical sciences seems to pervade most of the concepts discussed from Section 2 to 4. Cleland's account revolving around smoking guns, albeit argued as incomplete, is resolutely optimistic with regards to historical scientist's ability to find new evidence from the past that will shift our evaluation of existing hypotheses. The notion of evidential omnivory gives a more varied idea of what counts as evidence and, at the same time, further enlarges the resources that can be exploited by historical scientists. This omnivory is complemented by the notion of methodological autotrophy, which designates the continuous creation of epistemic tools, including middle-range theories, that extend the historical scientists' epistemic reach.

The importance of embedding the evidence within its specific context and existing evidential network is highlighted by Cleland's account, which places the significance of smoking guns in relation to the existing available evidence. However, the importance of the evidential network is made more specific through the notion of evidential omnivory, which leaves room also for situations in which a large array of evidence of varied strength still generates substantial support by their combined effects. The importance placed on the conjoined virtues of coherence and consilience also foregrounds the achievement that constitutes successfully accounting for the available lines of evidence.

As much as evidence is embedded in an existing network, its interpretation is also thoroughly theoretically mediated. This is captured by the notions of middle-range theory, and the three dimensions of security discussed above. These concepts, in particular, highlight the necessity to bring conceptual and methodological resources to turn a given trace or available information into evidence for a claim.

The first section also identified the pervasiveness of creativity in the work of historical scientists. This creativity is present in the notion of methodological autotrophy, productive speculation and investigative scaffolds, which particularly emphasize ways in which historical scientists manage to keep the evidential picture moving and continuously generate new lines of evidence and constraints to their hypotheses.

Related to creativity is the idea that the same information or trace of the past can be used as evidence in a wide variety of contexts. Notions such as evidential omnivory and methodological autotrophy, which foreground historical scientists' opportunism and ability to squeeze as much information as they can from limited resources, account for this contextual flexibility.

Knowledge of contemporary entities centrally matters to historical sciences. This slightly counter-intuitive-point, to which Cleland seems to give only secondary importance, is accounted for with the concepts of methodological autotrophy and security as credibility. The generation of middle-range theories sometimes depends on knowledge acquired via experimental practices and is underlain by background theoretical knowledge of contemporary entities. The credibility of interpretations of evidence is also partly mediated by theoretical knowledge about contemporary entities: are the claims made about past entities plausible with regards to what is known about contemporary forms of life? Instead of conceiving the production of knowledge about past entities and present-day entities as requiring mutually exclusive methodologies, respectively historical sciences and experimental science, this chapter highlights how the former feeds from the latter.

The varying degrees of reliability of lines of evidence have been, unsurprisingly, mainly discussed in the tripartite analysis of the notion of security by Wylie. The notions of security as credibility, determinacy and directness enabled to clarify how strongly supporting a line of evidence can be.

Finally, the importance for hypotheses to be conclusively, not comparatively, evaluated has not been at the forefront of the conceptual discussions sketched above. The inherently comparative picture brought by Cleland’s account has been criticized on these grounds. I think that the conjoined virtues of coherence and consilience, as well as the dimensions of security as determinacy and directness, fare well with the idea that hypotheses are evaluated on their own. This dimension is further explored in the next chapter.

On the whole, this arsenal of concepts does not provide a neat and tidy account of the methodology of historical sciences. Instead, it provides resources to understand the essential components of this practice. It aims at finding ways to account for the nature and role of lines of evidence, the way they are generated, the relation between evidence and hypotheses and the virtues that matter in the evaluation of hypotheses. These resources can be used to build discipline-specific frameworks or topic-specific case studies. The insights derived from this series of concepts are used in the next chapter, which provides a framework that aims to successfully account for a particular aspect of this methodology, the making of evidential claims. The effectiveness of this framework is illustrated by a case study in evolutionary biology. Hopefully, the next chapter's discussion clearly resonates with some of the insights garnered in this chapter.

CHAPTER 6: A FRAMEWORK FOR EVIDENTIAL CLAIMS AND THE CASE OF ARCHEZOA

INTRODUCTION

The last chapter was devised as an attempt to do two things. First, it provided an identification of key components of the methodology of historical sciences. It then critically reviewed a series of concepts, assessing their validity in light of the components identified in the first part. Of these concepts, the ones that were positively evaluated were argued to successfully capture aspects of the methodology of historical sciences. They are specifically fruitful to help characterize the nature, role and generation of evidence, the virtues of hypotheses and the degree of security that lines of evidence can confer to the latter. Despite not providing a neat and linear account of the methodology of historical sciences, this previous chapter aimed at identifying useful tools that can be used in discipline-specific methodological accounts and case studies.

In this chapter, I propose to restrict my attention to a specific aspect of historical sciences, by focusing on *evidential claims*. This topic is concerned with the ways various lines of evidence are used in order to provide support or to undermine a given claim. This restriction of scope is by no means an implicit vindication of a strong distinction between the "contexts of discovery and justification". As defined by Sober:

> The context of discovery would involve questions about the psychological influences that lead a scientist or a creative thinker to come up with an idea ("*how you get there*"). The context of justification would involve questions of logic and methodology having to do with the justifiability and defensibility of that idea ("*once you are there, how do you evaluate the product of this free creation of your imagination?*") (Sober, in Callebaut 1993, 98).

In this view, this chapter then focuses on the justification side of things. I hope this chapter makes clear that focusing on justification provides a great way to generate numerous related questions about discovery, and that a full understanding of concrete case studies of evidential claims cannot be obtained without the (mutually benefiting) conjoined investigations of both aspects.

The central thread of this chapter is the defence and illustration of a framework that formalizes evidential claims with the help of six different conceptual elements. I argue that this framework is particularly useful to capture the embedded nature of evidential argument and the successive shifts in support for claims. It is composed of *facts*, *claims*, *warrants*, *qualifiers*, *backings* and *rebuttals*. The framework is derived from the works of Toulmin (2003) and recently applied in the context of archaeology by Chapman and Wylie (2016). Part 1 draws inspiration from the last chapter to sketch a few desiderata that an account of evidential claims must fulfil. Part 2 is a presentation of the framework and explains how it fulfils the demands identified in part 1. After these rather abstract considerations, part 3 illustrates the effectiveness of the framework by applying it to a case derived from the works of Cavalier-Smith and Martin. Within their works, I picked the unfortunate fate of the "Archezoa" hypothesis as a case study. Archezoa is a taxonomic unit, a grouping of evolutionarily neighbouring species, which first enjoyed popularity and support before a subsequent accumulation of counter-evidence led to its demise in 1998. Because of its eventual fate and of the shifts that happened in its evidential support, I think it constitutes a privileged locus to study evidential claims in historical sciences. The chapter concludes by discussing what this framework allows to represent, what it keeps in the background, and the lines of investigation it opens but is incapable of addressing.

I. PHILOSOPHICAL DEMANDS ON EVIDENTIAL CLAIMS

Similar to what I did in the previous chapter, this discussion begins with the analysis of a quote about human history. Here, Prost insists that

> facts never appear outside of an argument. It is within this argument, and in virtue of it, that a historian, to support the claims made, constructs facts. Others will construct these facts in different ways, in virtue of a different argument on a different problem (Prost, in Delacroix *et al*. 2010, 854, own translation).

This quote serves as a reminder of some of the key insights discussed in the previous chapter. It sheds light on the necessary *theoretical mediation* present in the construction of facts. Constructing facts require the bringing of specific background conceptual and methodological knowledge, something like the middle-range theories (MRTs) discussed in the previous chapter. Prost also underlines the *contextual flexibility* possessed by lines of evidence and their interpretation. The construction of facts depends on the epistemic context at play. In this view, lines of evidence and claims can be brought to play different roles in different argumentations. In addition to the necessary theoretical mediation behind the construction of facts, this contextual flexibility is also the result of the embeddedness of lines of evidence within an existing *evidential network*. It is only possible to make sense of the role of a line of evidence once the epistemic context in which it is mobilized and the surrounding lines of evidence are appropriately characterized. The notion of *evidential omnivory*, discussed in the previous chapter, is compatible with the vision developed here since context-dependence in principle allows for a vast variety of elements to count as evidence. It is therefore important, in an account of evidential claims, to make clear that the effects of adding new lines of evidence are individual *and* distributed: the difference they can make comes both from their own specific features and from how they are embedded in the existing context.

This contextual specificity and flexibility has also been formulated by Collingwood, who affirmed that

> Evidence is only evidence in so far as it is used as evidence, that is to say, interpreted on critical principles; and principles are principles so far as they are put into practice in the work of interpreting evidence (Collingwood 1994, 203).

The conception of evidence I endorse, and which I argue Toulmin's framework can successfully account for, is unambiguously *relational*. It is then unsurprising that this account bears a strong affinity with Leonelli's "relational account of evidence" (Leonelli 2016, 69-92). Adapting her definition of "data" to my definition of "evidence" what counts as evidence hinges on

> the *evidential value* ascribed to them at specific moments of inquiry – that is, the range of claims for which data can be considered as evidence (Leonelli 2016, 70).

To ascribe the status of evidence to anything that, in some epistemic context, bears evidential value can seem hopelessly tautological. This circularity, however, is a consequence of the characteristics discussed above. The contextual specificity, theoretical mediation and embeddedness in an existing evidential network, I think, make it impossible to provide a non-circular and essentialist notion of evidence that ascribes evidential status in a context-independent fashion.

Emphasizing contextual flexibility in terms of what counts as evidence and underlining the pervasive interpretative dimension of evidential claims also entails taking into account the varying degrees of strength of the components of an epistemic context. The last chapter's discussion on the threefold notions of security as credibility, determinacy and directness sheds light on three types of components. Respectively, it directs attention to the MRT mediating the interpretation of evidence, to the nature of the effect of individual lines of evidence and to the complexity of the link from evidence to claim. As the case study on Archezoa illustrates below, shifts in all three of these dimensions of security can occur in a given epistemic context. They can result from the addition of new lines of evidence or from various shifts in our conceptual and methodological understandings that can alter the security of the interpretation of lines of evidence in many ways.

In the next part, I introduce "Toulmin schemas" (TS) and argue that this framework can successfully account for the specificity and flexibility of evidential claims in historical sciences.

II. TOULMIN SCHEMAS

TS are constituted of a set of six components: facts, warrants, claims, backings, rebuttals and qualifiers. Together, they enable the articulation of a formal representation of the evidential picture for a given epistemic context. This layout is represented in Fig. 9, and the meaning for each component, as well as how my understanding relates to Toulmin's original intention, are specified in this part.

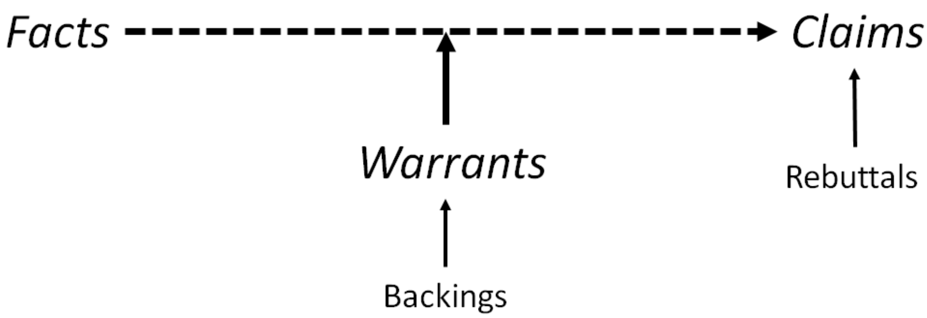

Figure 9 Representation of a Toulmin layout. Qualifiers are visualized by the dotting of the line between "facts" and "claims".

A. DATA, CLAIMS AND WARRANTS AS THE BASIS OF EPISTEMIC CONTEXTS

Epistemic contexts open when there is a jump from a set of premises to a conclusion, and the validity of this jump can be supported or undermined by bringing in a variety of lines of evidence. In the case of historical sciences, this inferential leap is traditionally conceived as going from a set of contemporary traces to a hypothesis about the past. In this framework, the traces are termed *facts* and the hypotheses about the past *claims*. Facts are the equivalent of "data" in the original formulation. Toulmin defines the latter as "the facts we appeal to as a foundation for the claim" (Toulmin 2003, 90). My proposed terminological shift is motivated by my endorsement of Leonelli's relational accounts of data. In this view, data are defined as "any product of research activities […] that is collected, stored, and disseminated in order to be used as evidence for knowledge claims" (Leonelli 2016, 77). In other words, "data" are (1) *potential* evidence built to (2) travel through *several* epistemic contexts. Here, facts are considered as (1) *actual* evidence which (2) grounds claims in a *specific* context. They are the comparatively more robust grounds upon which a more tentative claim can be formulated.

It is now well known that facts "do not speak by themselves". In addition to data, *warrants* are what theoretically mediate the interpretation from data to claim. Starting from a set of facts, warrants are "general, hypothetical statements, which act as bridges and authorize the sort of step to which our particular argument commits us" (Toulmin 2003, 91). The differences between facts and warrants are gradual rather than clear-cut. Following Toulmin, the first difference concerns the warrants' higher degree of generality. Secondly, facts are always explicitly stated in the defence of a claim, whereas most of the warrants are usually kept implicit (2003, 92-93). As said above, an epistemic context is opened with the generation of, and the investigation into, a tripartite relationship between data, claims and warrants. Conversely, epistemic contexts close when this investigation ceases, either because the link from data to claim is considered sufficiently solid or indefinitely broken.

How does the notion of warrant relate to the notion of MRT discussed in the previous chapter? MRTs designate the whole conceptual and methodological apparatus necessary to the generation and interpretation of lines of evidence. Warrants intervene at a moment where facts have *already* been constituted. They are the principles in virtue of which the facts, often constituted with the help of MRTs, are turned into key supports for a given claim. Chapman and Wylie characterize warrants as

> the gap-crossing assumptions, auxiliary hypotheses, background knowledge that constitute middle-range theory in an archaeological context (Chapman and Wylie 2016, 35).

This present characterization, on the contrary, considers that most of the work done by MRTs is not captured by warrants and, therefore, is *not visible* in TS. This restriction helps map what part of the methodology of historical sciences is backgrounded by this framework. My usage of TS leaves out the constitution of lines of evidence to solely represent how these lines of evidence support or reject claims.

B. Qualifiers

Toulmin recognized that warrants are, on the basis of a set of facts, sometimes only providing equivocal support to given claims:

> [some] warrants entitle us in suitable cases to qualify our conclusion with the adverb 'necessarily'; other authorize us to make the step from data to conclusion either tentatively, or else subject to conditions, exceptions, or qualifications […]. It may not be sufficient, therefore, simply to specify our data, warrant and claim: we may need to add some explicit reference to the degree of force which our data confer on our claim in virtue of our warrant (Toulmin 2003, 93).

This, for Toulmin, justifies the addition of *qualifiers* in his framework. Qualifiers, in this view, are used to specify the security of the inference to the claim.

Visually, the presence of qualifiers is marked by how dotted links in the epistemic context are. The spacing of the dots in an arrow is inversely proportional to the security of the link. It is possible to specify, if deemed relevant, the reasons behind a qualification on the representation. Spacing, however, is not to be interpreted in a *strictly quantitative* manner. It gives instead a rough and relative idea of the strengths of the links in an epistemic context. Whether or not increased quantitative precision is afforded by cases of historical sciences is an open question, discussed further in the conclusion. At face value, such an endeavour would face the difficulty of making commensurable the degrees of support and rejection brought by the wide variety of evidence at play.

C. Backings and rebuttals

According to Chapman and Wylie,

> Toulmin's central point is that the inferential work of warrants should be recognized as critical to the appraisal of substantial arguments […] (Chapman and Wylie 2016, 35).

Saying this, they underline how TS successfully capture the necessary theoretical mediation from facts to claims. This part of the framework, however, does not bring very sharply the specificity and flexibility I was emphasizing as the main demands for this framework in part 1. This task, for Chapman and Wylie, is particularly fulfilled by "(t)he secondary elements – qualifiers, backing and rebuttals" which "signal the pragmatic, dynamic nature of arguments-in-use" (Chapman and Wylie 2016, 35). What are backings and rebuttals? How do they enable to illustrate, in addition to qualifiers, the dynamicity of evidential claims in historical sciences?

Rebuttals are components that give the potential fragility identified by qualifiers a more specific meaning. They are used to indicate "circumstances in which the general authority of the warrant would have to be set aside" (Toulmin 2003, 94). In other words, rebuttals indicate the specific circumstances in which the claim made would turn out to be invalid.

As a consequence, further facts termed *backings* can be brought to ensure the applicability of the warrants by specifying that the circumstances in which the warrants are applied are the right ones. Backings are therefore secondary facts used in support of warrants. Backings are distinguished from facts functionally. The former is not necessary to the formulation and the sustained existence of a given claim, contrary to the latter. In Toulmin's view, in cases the applicability of warrants is "conceded without challenge" the backings remain implicit (2003, 98).

D. Summary and philosophical upshots

As argued above, TS are not built to capture the entirety of scientific practice. It explicitly focuses on evidential claims, TS effectively backgrounds what occurs at the level of the constitution of facts. Facts, however, can also be seen as,

> claims about surviving material traces that are identified as primary data, or about the context and relationships of these traces that constitute various kinds of secondary data (Chapman and Wylie 2016, 93).

Similarly, what counts as warrants in a given epistemic context

> are themselves claims that depend on further substantive arguments; they are not purely formal inference rules, nor are they 'self-authenticating', as Toulmin puts it (Toulmin 2003, 91) […] (Chapman and Wylie 2016, 34).

This observation indicates two things.

First, it shows that facts and warrants mobilized in support of a claim are themselves *fragile constructs*. It has already been noted by Chapman and Wylie that Toulmin's proposal emphasizes warrants as the potential source of fragility to the whole argumentative construction (Chapman and Wylie 2016, 35). The incapacity of warrants to license foolproof inferences justifies the addition of qualifiers, the identification of rebuttals and the need to provide backings. I think, however, that more emphasis should be given to the important repercussions of potential changes to the factual grounds for the solidity of claims. This necessity to recognize the dynamic nature of factual (and theoretical) grounds motivates my *snapshot approach* to the construction of TS. In their application of Toulmin's framework, Chapman and Wylie provide *synchronic* reconstructions of evidential claims, summarizing evidential arguments in one static diagram (for instance, Chapman and Wylie 2016, 35: Figure 1.2 and 70: Figure 2.3). By contrast, I build TS at different key stages (“snapshots”) of the evidential assessment of a claim. This way, I aim to extend their approach to capture a more *dynamic* picture of shifts in the security and relevance of claims, facts and warrants.

The constructed nature of facts and warrants also opens the door to the possibility of *conflicting* construction and usage of these two components. Divergent methodological and conceptual commitments can stem in the constitution of diverging facts and warrants, or in interpreting similar facts in different ways. This epistemic pluralism underlain by the heterogeneity of methodological and conceptual commitments within a discipline is discussed further, in relation to the problem of underdetermination, in the next chapter.

Toulmin’s intention behind this framework was to display the field-dependence of strategies of argumentation. Doing this, he was denying grounds to the existence of shared, cross-disciplinary, forms of argumentation (Toulmin 2003, 235). My interpretation simply allows for this heterogeneity to reach further. The specificity of each epistemic context does not rest on the type of components present in argumentations. It rather stems from the actual nature of each component (which actual facts and warrants, for instance, are employed) and how they are specifically arranged. Of course, points of convergence will be found within and across disciplines.

On the whole, I think one of the appeals of TS is that, from the interplay of simple components, this framework can depict complex and specific evidential structures in historical sciences. In particular, the existence of “secondary elements” (backings, rebuttals, qualifiers) enables increased precision in the roles assigned to lines of evidence and their effects. This framework allows substituting sentences such as “hypothesis *H* has been rejected by evidence *E*” with much richer, and visual, depictions of evidential claims.

Before turning to the case study, I also wanted to ensure that TS are not meant to make competition between theories irrelevant to the way hypotheses are assessed and evidence is generated. I insisted in the previous chapter on the importance of conclusively evaluating hypotheses about the past. This assertion is a way to contradict accounts of the methodology of historical sciences that, to my mind, overemphasize the idea that the strength of a particular hypothesis is only relative to others. This conclusiveness is encapsulated by the context-specificity embedded in this framework. Individual claims generate their own epistemic context and can be considered separately. This, however, leaves room for competition. The case study of the next chapter brings further illustration to this point, but the same lines of evidence can indeed have varying effects and relevance on different contexts. It is possible, also, to compare how strengthened or weakened competing hypotheses are and, more importantly, *what* strengthens or weakens them, in order to evaluate which one of them is to be favoured. The idea behind this use of TS is also to bring a more accurate picture of the grounds for disagreements between hypotheses, and, as stated in this chapter's conclusion, to open up investigations on the divergent methodological and conceptual commitments lying behind them.

After this relatively abstract presentation and defence of the framework. I now provide an illustration of its relevance with a case study of an evidential claim in historical sciences.

III. The case of Archezoa

A. What is Archezoa?

Archezoa, or the "Archezoa hypothesis", associated with the work of Cavalier-Smith, is a hypothesis in evolutionary biology with two interrelated components. First, it is a taxonomic hypothesis about the classification of contemporary eukaryotes. Archezoa, in its initial formulation, is the grouping of four phyla: *Archamoebae*, *Metamonada*, *Microsporidia* and *Parabasalia* (Cavalier-Smith 1987a, 56). All these four phyla are protists (unicellular eukaryotes). The basis for this classification is the shared possession (or rather absence) of several morphological traits, the most important one being "to completely lack any trace of mitochondria*" (Cavalier-Smith 1987b, 17). This claim does not simply mean a *current* absence of mitochondria, but an absence of *traces* of mitochondria that could indicate a *past* presence of this organelle.

This taxonomic inference has important evolutionary overtones. Protists, as unicellular eukaryotes, constitute privileged loci of investigation into several evolutionary questions. They are of interest for investigations into the origin of multicellularity – a trait exclusive to eukaryotes – since the first multicellular lineages stemmed from these organisms. More in line with this chapter's case, protists are also studied in relation to the origin of eukaryotic cells. This is because it is inferred from their unicellularity that they contain the descendants of some of the earliest emerging eukaryotic lineages. It is among these organisms, in this view, that can be found the closest relatives to “primitive” eukaryotes.

In investigations into the origin of eukaryotes, two traits, the possession of mitochondria and of a nucleus, occupy most of the scientific community’s attention. These two cellular structures are indeed considered as defining traits of eukaryotic cells and are observed in nearly all of the representatives of eukaryotes. This is why archezoans, defined as eukaryotes that never possessed mitochondria, were considered as “living representatives of the earliest phases of eukaryote evolution” (Cavalier-Smith 1987b, 17). In other words, members of Archezoa are hypothesized to be contemporary remains from one of the oldest eukaryotic lineage: one that formed *after* the emergence of the nucleus, but *before* the acquisition via endosymbiosis of mitochondria (see Chapter 2 for a reminder of current hypotheses about endosymbiosis). By these means, Cavalier-Smith considers the existence of Archezoa as a fulfilled prediction of his explanation of the origin of eukaryotic cells (defended at the time in Cavalier-Smith 1987a; 1987b).

The Archezoa hypothesis has largely shaped the discussions about the origin of eukaryotes from the late 1980s into the 1990s. It enjoyed initial support, especially until 1993, until a series of counter-evidence progressively weakened the classification (through the exclusion of specific phyla, or subparts of phyla) until the rejection (including from the author) of the classification in 1998 and the abandonment of the term in 2003. TS are here constructed to represent the evidential basis behind these shifts of support. Before building this framework, I start by a catalogue of the “omnivorous diet” of evidence used by evolutionary biologists. This sheds light on the creativity and variety of MRTs at play here. As stated above, the MRTs mediating the constitution of these lines of evidence are not visible in TS, since they concern the constitution of evidence, not the data to claim link. I also try to make clear the potential strengths and limits of these lines of evidence, accounting for what can affect their relevance in this particular context as well as the security of the links they are associated with.

B. Lines of evidence

rRNA morphology

The study of the structure of the ribosomal RNA (rRNA) is not to be confused with the construction of molecular phylogenies from rRNA gene sequences, which I discuss later. The latter is interested in the comparison of aligned nucleotides of rRNA *sequences* in order to establish genealogical relationships between species. The former, instead, is about comparing rRNA *morphologies* between species, aiming at establishing phylogenetic relations based on their resemblances. Ribosomes* are composed of two subunits: a large and a small one. Prokaryotes and eukaryotes tend to differ with respect to the size of each of them. For eukaryotes, the overall size is usually 80S[50]. The 60S large subunit is composed (in part) of three rRNA components of size 5S, 5.8S and 28S and the 40S small subunit possesses an 18S rRNA component. For prokaryotes, the overall size is 70S, with a 50S large subunit constituted with 5S and 23S rRNAs, and a 30S small subunit composed of 16S rRNA.

The study of rRNA morphologies, because they differ between eukaryotes and prokaryotes, is an interesting locus for generating evidence about "primitive eukaryotes". The possession of a "prokaryotic" ribosome by a group of eukaryotes can be interpreted as a sign of having evolved earlier than most of their eukaryotic counterparts, before the changes in ribosome type occurred. The reliability of this line of evidence, however, has been increasingly questioned over time. The possession of smaller, prokaryote-like ribosomal component could also be the result of *secondary simplifications*. Evolution, in this case, would not have proceeded linearly from prokaryotic to eukaryotic rRNA, but would have instead added an extra step from eukaryotic rRNA to prokaryote-looking rRNA. In this view, then, the possession of prokaryote-like rRNA by eukaryotes would not be an uncontrovertibly reliable indicator of an early origin.

Golgi Dictyosomes

The Golgi apparatus is a specific feature of eukaryotes responsible, among other things, for protein maturation and intracellular vesicle trafficking. Structurally speaking, it is constituted of a functionally differentiated network of

[50] *S* standing for "Svedberg units", a non-metric unit for sedimentation rate. Higher numbers correspond to quicker sedimentation, corresponding to bigger particles.

stacked compartments called “dictyosomes” (also called “cisternae”). The complexity, even the presence, of dictyosomes in eukaryotes can be taken as an indicator of the timing of the evolution of a given eukaryotic taxon. Eukaryote taxa without dictyosomes, or without a well-developed one, could be, in this reasoning, considered as having emerged early in eukaryote evolution. However, similar to rRNA morphology, inferring the primitiveness from this trait is risky since it could also derive from secondary simplifications.

Spliceosomal introns

Introns are sequences of RNA which, in the process of RNA maturation, are diced out and hence are not translated into proteins. Amongst the variety of ways with which these sequences can be recognized and removed, *spliceosomal* introns possess specific signal sequence recognized by *spliceosomal* RNA that then mediates their removal. This type of sequence is specific to eukaryotes and therefore possesses an interesting potential for generating evolutionary evidence. The absence or presence of these introns can, with the now usual proviso of secondary loss if they are absent, be taken as an indicator of the relative primitiveness of a given eukaryotic lineage.

Hsp60

“Heat shock proteins” (Hsp) are proteins responsible for assisting in the correct folding of other proteins, this function usually being described as “chaperoning”. Their name comes from the fact that they are usually generated in conditions of cellular stress, induced, for instance, by heat or wounds. Hsp60 is a mitochondria-specific protein responsible for protein import and macromolecular assembly. The presence of Hsp60 in a given eukaryote lineage is, therefore, a strong indicator for the contemporary or past presence of mitochondria, unambiguously arguing against the emergence of the possessor of such proteins before the appearance of mitochondria in eukaryotes.

Hydrogenosomes

Hydrogenosomes are cellular organelles responsible for energy production. Unlike mitochondria, they operate in anaerobic conditions and their functioning generates hydrogen, the latter giving the name to the structure. Another exclusive trait of eukaryotes, the evolutionary status of hydrogenosomes became a central point of interest for understanding the origin of this type of cell. The main question about them, at the time of the Archezoa hypothesis, was to know whether the evolution of hydrogenosomes was in any sense related, or instead entirely independent, from the evolution of mitochondria. If hydrogenosomes and mitochondria were showed to be evolutionarily related, this would mean that they possess a common ancestor. In this case, any eukaryote in possession of hydrogenosomes *cannot* have emerged before the origin of mitochondria (or, more precisely, of mitochondria-related organelles).

rRNA and other protein-specific phylogenies

The nucleotide sequence of genes coding for ribosomal RNA has often been considered as one of the best historical documents of the living world. Firstly, rRNA is a ubiquitous cellular component. Secondly, rRNA is crucial to the maintenance of living systems in virtue of being one of the main components of the protein synthesis apparatus. It is argued that this function has prevented rRNA from having undergone changes too quickly (else it could have directly threatened the viability of the concerned organism) and, therefore, makes it one of the best-conserved gene sequences across the living world. Because of these two properties, ubiquity and stability, the sequence of the gene coding for rRNA has been used from the late 1970s in *longue durée* phylogenetic reconstructions that would cover the entirety of existing species (for instance, Woese and Fox 1977), from bacteria to eukaryotes, generating phylogenetic trees usually called “trees of life”. The trees generated with rRNA sequences have been used as evidence for assessing the relative position of each Archezoa lineage and verifying their purported primitiveness.

In addition to rRNA sequences, other proteins, more specific and relevant to our understanding of the evolution of members of Archezoa, were also used to construct phylogenies. Taking genes coding for proteins that are less widespread for the construction of phylogenies enables to gain a more local understanding of the genealogical relationship between members of Archezoa and the rest of eukaryotes.

Parasitism and secondary evolution

Some members of Archezoa are never found in a free-living state. This means that their life cycle is integrally dependent on parasitic relationships they develop with their host, usually a multicellular eukaryote. This has two potential evidential consequences. The first one is that, by extrapolation, it makes it implausible for parasitic lineages to have evolved too early in the evolution of eukaryotes, as their lifestyles depend on the existence of later-emerging eukaryotes for their survival. The second related point is that the development of parasitic life cycles, by depending on resources brought by a host, makes redundant (hence dispensable) the possession of certain components. Lineages of parasitic organisms, therefore, are more likely to display superficial simplicity as an outcome of secondary simplifications.

It is noticeable that these lines of evidence are all fundamentally trace-based. They all have evidential value because they are seen as interpretable contemporary remain of past events and processes. However, what mediates their interpretation as remains is built from a variety of knowledge, including knowledge grounded on non-trace evidence from contemporary organisms. This description further illustrates the large scope of relevant knowledge (the evidential omnivory) that plays a role in the constitution and interpretation of evidence. I now describe how these various lines were mobilized to account for the shifting support of the Archezoa hypothesis as a whole. This also requires to provide lineage-specific analyses for each of the four members of the clade.

C. THE SHIFTING STATUS OF ARCHEZOA

Initial support for Archezoa

Why were Archezoa initially singled out from the rest of eukaryotes? On one hand, they were argued to share with the rest of protists a number of "superficially similar" characteristics, as they consist of

> unicellular phagotrophic or micropinocytotic, nonphotosynthetic eukaryotes which lack a cell wall in the trophic phase (Cavalier-Smith 1993, 962).

On the other, they were singled out as possessing a 70S ribosome (instead of eukaryotic-specific 80S, as described above), as lacking well-developed Golgi dictyosomes and mitochondria[51]. This trio of data, in combination with the warrants that these characteristics are a testament of a primitive state of eukaryote evolution and that they are uniquely present in Archezoa, support this

[51] The components of the cilia and the absence of peroxisomes were also often initially mentioned (Cavalier-Smith 1987b, 17), but do not play a major role in later discussions.

initial classification. The integration of this classification into a wider evolutionary explanation (which is close, but not identical, to what is described for Cavalier-Smith in Chapter 2) further backs this classification (conversely, the support of Archezoa also constituted a backing to the underlying evolutionary narrative it was embedded in. Relations of support were here mutual).

The security of this inference, however, was not foolproof. The claim for the existence of the taxon is threatened by three rebuttals. Further research can result in the finding of traces of mitochondrial presence in purported Archezoa members, such as mitochondria-specific genes, proteins and structures. It would also be possible to interpret the facts in the light of alternative warrants which contradict the initial claim. While the traits shared by Archezoa members are considered as marks of primitivity, it is also possible that they come from secondary simplifications. In such cases, these traits would indicate a rather late origin to these organisms, stemming from lineages that have lost some of their more complex traits. The exclusivity of the primitive traits shared by Archezoa members can also be undermined by showing that later eukaryotes also possess such traits. The initial support to the Archezoa hypothesis is pictured in Fig. 10.

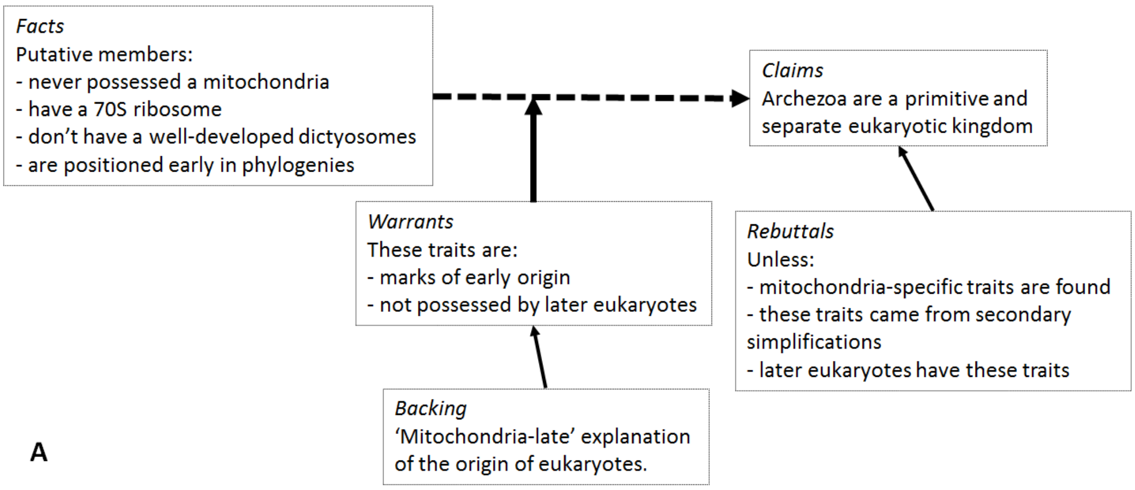

Figure 10 Toulmin schema of the initial support of Archezoa.

Initial support for Archamoebae

Archamoebae is a specific taxonomic creation to accommodate "amitochondrial amoebae that could not be placed" in the three other initial Archezoan phyla (Cavalier-Smith 1991, 27). In that respect, it was an initially under-explored taxon that did not possess distinguishing traits apart from the ones discussed above. In this respect, they were considered as rather prototypical Archezoa, and “present day *Mastigamoebae*” were considered to be the closest thing possible to “the most primitive eukaryote” (Cavalier-Smith 1987b, 17). In the early formulations of the hypothesis, therefore, the strength of the inclusion of Archamoebae matches the overall support for Archezoa. The only qualifier comes from their nature as a taxonomic gap-filler, which makes it run the risk that Archamoebae

> might turn out to be polyphyletic*, since they could well include both primitively and secondarily amitochondrial amoebae […] (Cavalier-Smith 1991, 27).

More work was therefore needed to further distinguish which member of archamoebae should belong to Archezoa.

Initial support for Metamonada

In addition to the initial general support to the Archezoa hypothesis, the presence of Metamonada in Archezoa was backed by specific rRNA phylogenies. In particular, a study ran by Sogin’s group (Sogin *et al*. 1989) sequenced the rRNA of metamonad *G. lamblia* and the results were interpreted as “very strongly” supporting an early branching of this group (Cavalier-Smith 1991, 27).

Initial support for Microsporidia

Similar to Metamonada, the inclusion of Microsporidia in Archezoa was also initially strongly supported by rRNA phylogenies. This time, the study focused on sequences of small subunit rRNA of microspora *V. necatrix* (Vossbrinck *et al*. 1987) and placed them at the time “as the deepest in the eukaryotic lineage” (Roger 1996, 10). In addition to rRNA-based phylogenies, the other main source of evidence stems from studies of the large subunit rRNA (Vossbrinck and Woese 1986) that showed that *V. necatrix*

> contains sequences corresponding to 5.8S rRNA, as in bacteria, rather than having separate 5.8S and 28S rRNA molecules as in metakaryotes […] (Cavalier-Smith 1991, 27).

The only initial confounding point stemmed from the parasitic nature of microsporidian lineages which, as stated above, left open the possibility that they could

> have suffered extreme parasitic reduction, including the loss not only of mitochondria and peroxisomes* but also of lysosomes*, cilia*, and centrioles* (the latter three organelles are present in all other Archezoa but absence from microsporidia) […] (Cavalier-Smith 1993, 964).

In other words, the claim that Microsporidia were archezoans was initially supported, but their exaggerated simplicity combined with their parasitic lifestyle left a lurking doubt with respects to their actual primitiveness.

Initial support for Parabasalia

The taxon Parabasalia was, in the beginning, the least-supported member of Archezoa. Contrary to other members of Archezoa, it possesses "a permanent dictyosome" (Cavalier-Smith 1987b, 43) and a "double-envelope […] hydrogenosome" (Cavalier-Smith 1987b, 23). The evolutionary status of hydrogenosomes was at the time unsettled, but Cavalier-Smith strongly qualified the inclusion of Parabasalia by assuming that hydrogenosomes

> might in principle have originated symbiotically […] or alternatively by reduction from mitochondria with the loss of mitochondrial DNA. If the later were proven, parabasalia should be transferred to the Mitozoa (branch Miozoa) from the Archezoa (Cavalier-Smith 1987b, 23).

Contrary to other Archezoa members, rRNA-based phylogenies provided only mixed support to their deep branching: sometimes supporting it (Sogin 1989), sometimes supporting a secondarily derived status (Qu *et al*. 1988; Perasso *et al*. 1989).

To summarize, the Archezoa hypothesis, as a classificatory hypothesis, was initially supported, with some members more than others. To account for the downfall of Archezoa, this time I proceed with each specific member in a chronological order of disappearance.

The exclusion of Parabasalia and the shifts in evidential meanings

The least initially supported of the members is unsurprisingly the first to be excluded from Archezoa. rRNA phylogenies were not crucial in this picture as they remained incapable of unambiguously resolving the positioning of Parabasalia in the eukaryotic tree (Cavalier-Smith 1993, 964)[52]. The main argument for leaving out the phylum Parabasalia rather comes from the changes in the understanding of the evolution of hydrogenosomes. Understanding precisely the reasons behind this shift would deserve a separate study, as there is currently no comprehensive survey on the matter. Müller, one of the pioneering researchers on the organelle, defended a separate symbiotic origin for hydrogenosomes until 1992, but, at that point,

> convincing data started appearing that showed my hypothesis to be way off the mark (Müller 2007, 9).

In this chapter's framework, the situation could be described by saying that the claim that hydrogenosomes were evolutionarily related to mitochondria was discussed in a separate epistemic context from the Archezoa hypothesis. Over time, support for this claim became sufficiently strong to convince the scientific community. This shift had repercussions in the case of Archezoa, as it added a warrant stating that "hydrogenosomes and mitochondria shared a common descent". It does not mean that the evolutionary relation between hydrogenosomes and mitochondria was established without doubts (it still needed further research) but it was taken to be sufficiently consolidated to decisively license rejection of the claim that Parabasalia is a member of Archezoa. The rejection of this latter claim is represented in Fig. 11.

[52] This does not mean that phylogenies constructed later were not capable of indicating that. It just doesn't seem to have been a significant factor at the time of the exclusion of Parabasalia from Archezoa.

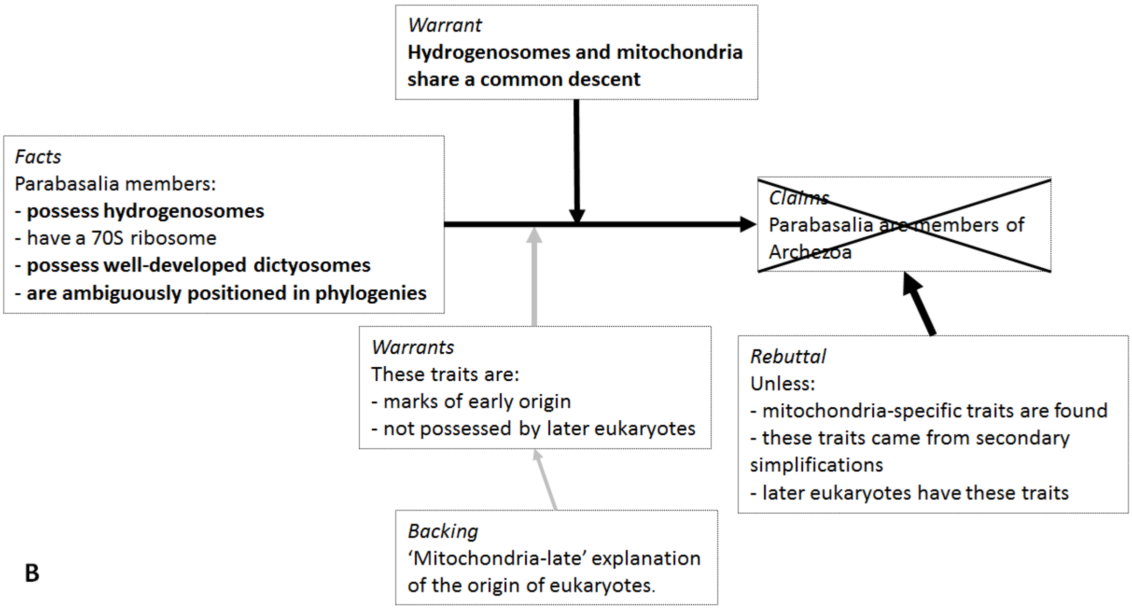

Figure 11 Toulmin schema of the rejection of Parabasalia from Archezoa.

This rejection has interesting repercussions on the status of the Archezoa hypothesis as a whole. Because Parabasalia, now excluded from Archezoa, possesses 70S ribosome, the possession of 70S ribosome as a defining character of Archezoa is now unequivocally denied, impacting on the credibility of one of the warrants in this context. This also impacts on the support for the other memberships to the kingdom, especially for the initially rather weakly-supported Microsporidia (Cavalier-Smith 1993, 965). The now decisive role played by hydrogenosomes also generates a new line of evidence in this context, as its absence is now invoked in support of the grouping. This shift is visible in Cavalier-Smith's characterization of Archezoa as possessing

> 70S ribosomes, like bacteria, rather than 80S ribosomes as in most other eukaryotes, and in never having mitochondria, peroxisomes, hydrogenosomes, or well-developed Golgi dictyosomes (Cavalier-Smith 1993, 962).

In addition to this, the strength of the lack of dictyosomes as evidence for Archezoa has been relativized after the observation of secondary losses of dictyosomes in non-Archezoan protists (Cavalier-Smith 1993, 965).

Archamoebae as "advanced" eukaryotes

The credibility of the archamoebae taxon initially suffered from being mainly negatively defined. Doubts were raised on the status of some of its members, notably *E. histolytica*, in the face of conflicting rRNA evidence (Cavalier-Smith

1993, 964). This process progressively led to the eventual exclusion of the totality of Archamoebae from Archezoa, coming from rRNA phylogenies (Morin and Mignot 1995) and the isolation of genes of mitochondrial origin in *E. histolytica* (Clark and Roger 1995). In light of these lines of evidence, Archamoebae were in fact considered to be "relatively advanced eukaryotes that have almost certainly evolved by the secondary loss of mitochondria" (Cavalier-Smith and Chao 1996, 557). Fig. 12 summarizes the evidential claims leading to the exclusion of Archamoebae.

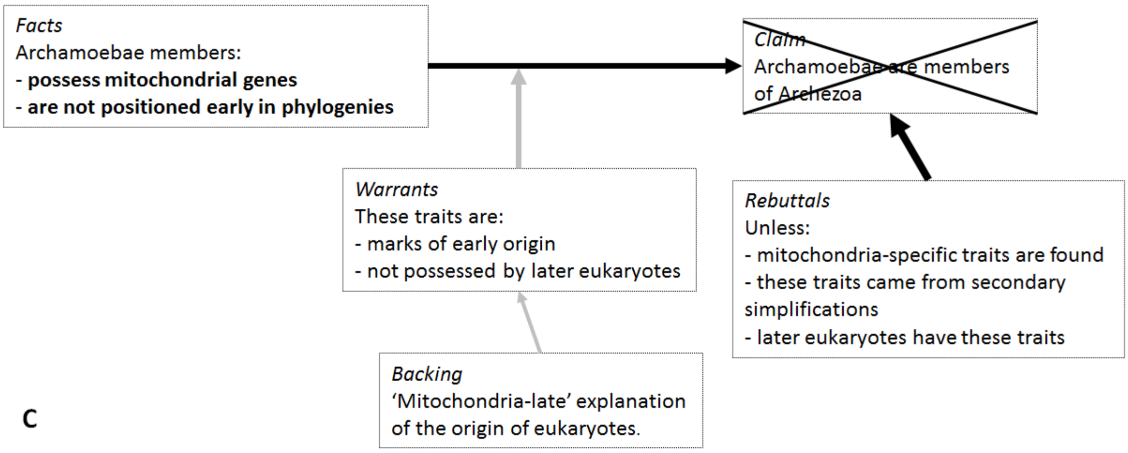

Figure 12 Toulmin schema of the exclusion of Archamoebae from Archezoa.

Microsporidia as "degenerate fungi"

The support for the inclusion of Microsporidia, initially weak, further degraded after the weakening of 70S ribosome as evidence for eukaryotic primitiveness. The main driver of the evolution from suspicion to rejection is the discovery of spliceosomal RNA in microsporidia (DiMaria *et al*. 1996; cited in Cavalier-Smith 1998, 227). The presence of spliceosomal RNA being evolutionary associated with the acquisition of mitochondria, Microsporidia could therefore not have emerged before its acquisition. In addition to that, protein-specific phylogenies, in particular from hsp70 (Germot *et al*. 1997) and proteins of the tubulin* family (Li *et al*. 1996; Keeling and Doolittle 1996; Roger 1996) presented Microsporidia members as secondarily amitochondrial and heavily degenerate fungi (Cavalier-Smith 1998, 227). A summary of the evidential picture leading to the rejection of Microsporidia is given in Fig. 13.

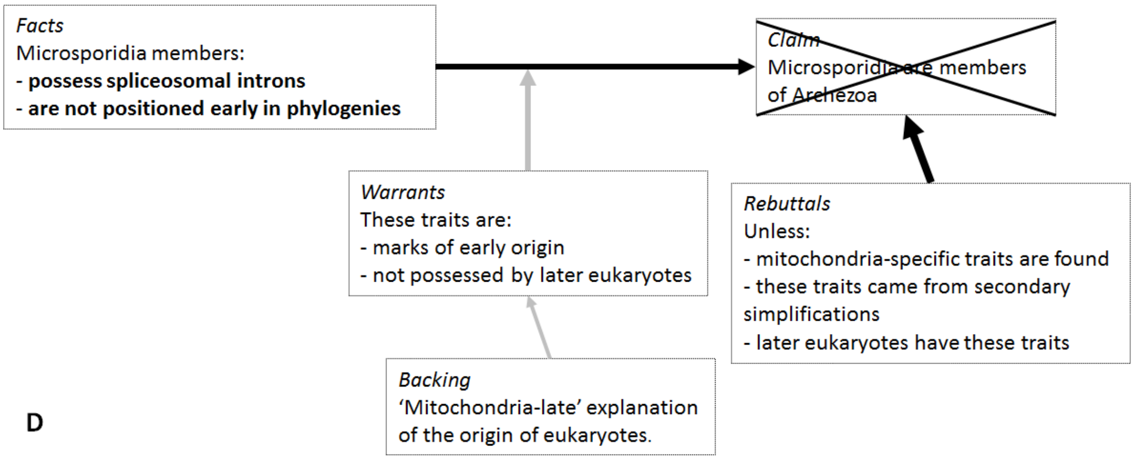

Figure 13 Toulmin schema of the exclusion of Microsporidia from Archezoa.

How about Metamonada?

Metamonada was one of the best-supported Archezoa members. Since other groups have been progressively removed from Archezoa, the latter's existence was increasingly questioned. It took more time, however, for researchers to generate decisive evidence to reject Metamonada from Archezoa. Protein-specific phylogenies, notably of hsp60 (Soltys and Gupta 1994), were judged generally unconvincing by Cavalier-Smith (Cavalier-Smith 1998, 229). What constituted "firmer sequence evidence for a secondarily amitochondrial character of the group" (Cavalier-Smith 1998, 229) is a series of phylogenies of the cpn60 gene, identified in *G. lamblia*, that "grouped solidly [the latter] with the mitochondrial and α-proteobacterial sequences" (Roger 1999, 152) (the latter which, as a reminder, is the lineage of bacteria from which mitochondria-related organelles stem) and of a gene involved in protein synthesis "specifically related to the homologue from *Trichomonas vaginalis* (Hashimoto *et al*. 1998)" (Roger 1999, 152), *T. vaginalis* being a member of the rejected parasabalian lineage. With this combined evidence, the last of the claims for the existence of a primarily amitochondrial lineage is now rejected. Fig. 14 summarizes the evidential picture for Metamonada.

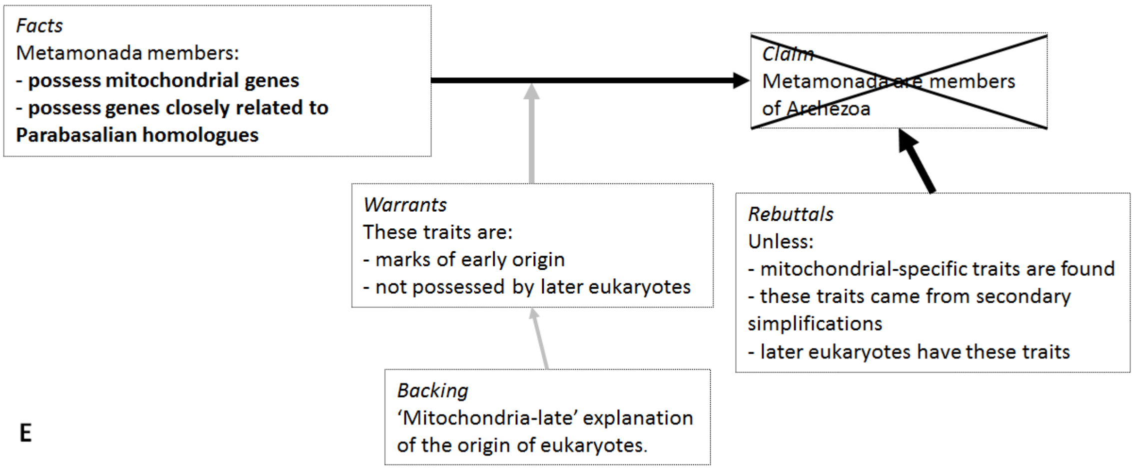

Figure 14 Toulmin layout of the exclusion of Metamonada from Archezoa.

D. Epilogue – what remains of Archezoa?

If taken as a classificatory hypothesis underpinned by an evolutionary motivation, Archezoa was unambiguously rejected by 1998. The name, however, remained as a phylum within protists that included Metamonada and Parabasalia and were still postulated to be the earliest (albeit post-mitochondrial) eukaryotes (Cavalier-Smith 1998, 206). Such rooting of the earliest eukaryote within Archezoa stopped in 2002 (Cavalier-Smith 2002, 311, 341), the new derived nature of the taxa leading Cavalier-Smith to drop the name altogether in 2003 (Cavalier-Smith 2003, 1745). If the classification is gone, what remains of the evolutionary underpinnings?

Cavalier-Smith defended, at the dawn of Archezoa, the logical independence existing between the classificatory and the evolutionary sides of his work. The bigger evolutionary explanation in which the Archezoa hypothesis integrated itself and partially supported, the "phagotrophic hypothesis", is argued to "apply to any potential host" (Cavalier-Smith 2002, 318). What was proven false, then, is that the Archezoa kingdom represented remnants of an intermediary stage between the emergence of eukaryotes and the acquisition of mitochondria, as well as a classificatory hypothesis grouping four distinct clades together. It might be perfectly legitimate for Cavalier-Smith to feel that the amalgam some scientists have made by regrouping distinct aspects of his work under the heading "Archezoa hypothesis" led to an unfair downgrading of Archezoa-independent aspects of his work (Cavalier-Smith 2002, 318). However, what is also a probable, and more legitimate, cause of the decrease in popularity of the phagotrophic theory of the origin of eukaryotes is also the flourishing of alternative hypotheses, such as the Martin and Müller's hydrogen hypothesis (Martin and Müller 1998, see Chapter 2).

Alternative accounts, such as Martin and Müller's, generated different research programs that came to grips with and shed light on alternative sources of evidence. They were, in other words, stimulating researchers to exercise their "methodological autotrophy". This was probably done by attempting to evaluate some of the fictional truths generated from the hydrogen hypothesis, this representing a case of productive speculation (as discussed in Chapter 5). Martin and Müller's hypothesis also came with different conceptual and methodological commitments. Since both Cavalier-Smith's and Martin's hypotheses for the origin of eukaryotes are still around, the benefits and risks of the existence of such divergent explanations are explored further in the next chapter.

Overall, the fall of Archezoa represented a failure of a classificatory hypothesis and of an evolutionary hypothesis about contemporary organisms, undermining but not totally invalidating the general framework about the origin of eukaryotes in which it was inserted. To illustrate this, elements of the reasoning behind the phagotrophic theory for the origin of eukaryotes, a subtype of "autogenous theories" (O'Malley 2010), remain present, as illustrated by the continued debate between "mito-early", "mito-late" and "mito-intermediate" scenarios for the origin of eukaryotes (Ettema 2016).

CONCLUSION

Chapter 5 dealt with the methodology of historical sciences "as a whole", and this allowed me to identify some of its key components and critically assess a series of concepts that can help to describe it. This chapter aimed to put into practice these insights by doing two things. Part 1 and 2 illustrated how conceptual demands stemming from the previous chapter can help evaluate a framework from a theoretical standpoint. Toulmin schemas (TS) were put to use in a diachronic approach in order to display the way lines of evidence are articulated and used in the context of historical sciences. Part 3 consists of an application of TS to a case study derived from my dissertation's focus on the works of Cavalier-Smith and Martin. It focused on the rise and fall of the Archezoa hypothesis, aiming to capture the evidential dynamics behind the initial support and subsequent fall of this taxonomic hypothesis.

As stated above, TS only enable us to visually display and reconstruct the modes of justification of hypotheses. I was keen, in the introduction, to remind that this exclusivity is by no means underpinned by a belief in the legitimacy to strongly distinguish between the context of discovery and the context of justification. On one hand, I indeed believe that the construction of TS enables a sharper analysis of the dynamics of justification. It provides a precise vocabulary and is flexible enough to build up sophisticated reconstitutions from the interactions of these simple elements.

On the other hand, I argue that TS are not only useful to refine our understanding of evidential claims in historical sciences. While they do not provide explicit insights into the context of discovery and about the constitution of facts, I think that the construction of TS opens up several lines of investigation into them. Used for historical reconstructions, TS identify the various security shifts stemming from the introduction of new lines of evidence (and reinterpretation of former ones), thereby indicating the presence of implicit conceptual and methodological innovations behind these changes. They open up questions about how these various changes and shifts occurred. For instance, in this chapter's case study, how did the change in the perceived evolutionary status of hydrogenosomes occur? What are the factors behind the sufficient consensus that seems to have emerged on the subject? Delving into these questions requires dealing with the articulation of lines of evidence, for sure, but also understanding the crossing of social, historical and conceptual dynamics that TS alone cannot capture. The descriptions provided by the use of TS and these complementary investigations can help, for instance, to probe the epistemic soundness of some of the theory choices that have been made (akin to Chang's account of the phlogiston hypothesis in his 2012 book). To reiterate, I clearly do not think that the focus on justification provided by TS comes with a neglect of discovery.

Among the conceptual resources devised in Chapter 5, I have so far mostly discussed how they explicitly account for the various degrees of security, illustrate evidential omnivory and implicitly display the pervasive theoretical mediation behind the constitution and interpretation of facts. I think, in addition to that, that the case study particularly highlights the benefits of productive speculation. The Archezoa hypotheses explicitly contained elements of speculation. It required a definite “leap of faith” to assert some weakly or mildly supported claims. This speculation however structured the debate and oriented research by directing attention to particular aspects of the organisms under investigation. It is unlikely that so much attention would have been devoted to the ways to identify marks of mitochondrial secondary loss in the members of Archezoa if the hypothesis weren’t formulated. This speculative hypothesis, despite being eventually rejected, contributed to an improved understanding of the evolution of early eukaryotes and, I assume, benefited from and contributed to parallel lines of investigation.

I have also, in Chapter 5, highlighted the importance of the knowledge of contemporary entities in our understanding of the past. This argument can also be perceived as a contestation of accounts that would place a strong methodological demarcation between historical sciences and experimental sciences (for instance, Cleland 2002). This argument being built on an emphasis the primary importance, for the former, of the discovery of the relevant traces of the past. I believe the case of Archezoa makes it clear that the study of the functioning and morphology of purported members of Archezoa (which are contemporary organisms) and the study of their evolution were tightly linked. Here, historical and contemporary knowledge are better seen as standing in a mutually beneficial relationship. In addition to that, the various types of evidence mobilized confirmed the importance of several experimental practices, from the extraction and isolation of proteins to the sequencing of genomes to the culture of organisms. Contemporary and experimental knowledge are, evidentially speaking, far from secondary in this chapter’s case study, and I believe that my case was not exceptional in these regards.

I conclude this chapter by identifying a few of the limits and open questions related both to the conceptual aspects of the framework and to the way I designed this case study. I already evoked above the non-quantitative character of this framework. Due to the evidential variety displayed and also the fact that scientists did not evaluate the claims, in the case studied, in this quantitative manner, I am doubtful of the possibility of changing this particular set of TS into something more quantitative. It would be interesting, however, to try and apply TS to cases, within historical sciences, where quantifications pervade the evaluation of lines of evidence and whole claims. Would there be the same variety of roles? How would that play out? I think that the possibility of adding qualifiers and of modulating the strength of the fact-to-claim link open up the possibility for more quantitative representations. In similar respects, TS here provided a visually tractable representation of the evidential structure around Archezoa. It might be the case that for more complex and nested arguments this tractable display becomes a visually cumbersome one. However, I assume that such complex arguments would then make any attempt at rational reconstructions difficult, be it summarized into words or into schemas.

Concerning the case study in itself, I have provided a reconstruction of how lines of evidence were used mostly derived from the vantage point of Archezoa's father and main defender, Cavalier-Smith. This choice is partly due to my own time and epistemic limitations. It would be difficult, for the time being, to handle a variety of points of view because of the additional material to digest and additional intellectual sophistication it would require. The dynamics captured by TS in the case of Archezoa are, therefore, an incomplete and limited picture that would welcome further study to improve and sophisticate it. I think, however, that this added complexity wouldn't undermine the main reason for building TS to capture evidential claims, because it wouldn't contradict the main philosophical upshots of the schema in the first place. As discussed a few paragraphs above, I mostly hope for such reconstructions to open up complementary lines of investigations that can refute, complement, or supplement this particular analysis.

After dealing with evidential claims and aiming to understand the evidential dynamics behind the evaluation of hypotheses considered on their own, the next and final chapter discusses the coexistence and competition between hypotheses. It does so by considering questions of underdetermination and how they relate to the notion of epistemic pluralism. It also proposes a different usage for TS.

Chapter 7: From Underdetermination to Pluralism: Benefits and risks of the coexistence of rival hypotheses in historical sciences

Introduction

This dissertation in general, and this chapter in particular, are motivated by two interrelated considerations. I wondered, first, how it was possible for two radically contrasting hypotheses, those of Cavalier-Smith and Martin, to have been coexisting for a substantial amount of time without one taking irreversible precedence over the other. It is a rather descriptive question, from which stems some normative ones. Is this coexistence a good thing? What are the benefits and risk associated with that? These two questions are at the basis of this last chapter. They provide an entry point for discussions about underdetermination and pluralism in historical sciences.

The problem of underdetermination is conceived here as a problem of evidential *security*. Applied to historical sciences, it asserts that the traces left from the past are often incapable of providing sufficiently strong and unambiguous evidential constraints to allow for clear choices between competing hypotheses. This lack of evidential security as determinacy, as discussed in Chapter 5, if considered as pervasive, leads to frequent cases of what have been described as *modest pluralism* (Kellert *et al*. 2006). This form of pluralism accepts the coexistence of contradictory hypotheses when the lack of better evidential grounds prevents from a decision between them.

Despite agreeing on the importance of the issue and the pervasiveness of underdetermination in historical sciences, I argue that the connection from underdetermination to epistemic pluralism needs to be revised. I do so by identifying a problematic assumption underlying traditional discussions around underdetermination. In short, philosophers engaged in this discussion have assumed the existence of a common pool of evidence which competing hypotheses must invariably take into account. This existing pool of evidence underlies the conceptual possibility of the empirical equivalence of hypotheses. Other philosophical accounts contest this vision. With them, I argue disagreements over what counts as evidence and what this evidence means are sufficiently prevalent in historical sciences to undermine the necessary existence of shared evidential grounds across a given scientific community. Underdetermination, then, does not only concern the move from evidence to hypotheses: it runs deeper. It is already there at the evidential level. Revising this assumption about underdetermination has consequences for the form of pluralism that stems from it. It no longer is the unhappy consequence of evidential shortcomings. Instead, it is the unsurprising outcome of the conceptual and methodological heterogeneity in a given area of inquiry, coming closer to situations described in Chang's (2012) defence of *active realism*.

The second part of the chapter proposes to illustrate and defend this form of epistemic pluralism. It puts to work Toulmin Schemas (TS), discussed in the previous chapter, to a perennial case of disagreement in evolutionary biology. The framework is used to deconstruct the evidential claims for and against the necessity of phagocytosis for the origin of mitochondria. This detailed analysis provides a clear illustration of the soundness of the proposed rearticulation of the problem of underdetermination. At the heart of the debate around the need for phagocytosis lies a disagreement about the relevance and solidity of some lines of evidence and principles, not the mere puzzlement over a variety of hypotheses that equally satisfy the existing evidence.

From this case, the last part identifies some of the benefits and risks associated with methodological pluralism. On the plus side, contradictory coexisting hypotheses allow for the exploration of a broader array of relevant aspects to a given problem. Endorsing methodological pluralism also pushes us away from seeing the reconciliation of conflicting hypotheses into a unified theory as the sole measure of epistemic success. Reaping these benefits is not automatic and effortless. It comes with potential pitfalls and risks. The main one being the difficulty associated with the necessity for a scientific community to use a wide variety of knowledge to tackle the problem they are facing. Evolutionary explanations, for instance, are often inherently multifaceted and the relevant expertise cannot be equally distributed across the community. Another problem is one of the funding choices: if the coexistence of hypotheses is legitimate, then which of the research programs defending one of these hypotheses should be pursued?

From these discussions, it is clear that finding ways of reaping the benefits of this form of pluralism and avoiding some of its pitfalls takes us outside of the dynamics of justification, which Toulmin schemas are only capable of representing. This discussion of methodological pluralism, instead, highlights the necessity to attend to the conditions under which fruitful evidential discussions can occur. The chapter closes by considering potentially interesting sources to tackle this issue.

I. From Contrastive Underdetermination to Modest Pluralism

A. Holist and Contrastive underdetermination

The discussion starts with a characterization of the problem of underdetermination. More precisely, the problem is often framed as the problem of "underdetermination of scientific theory by evidence" (Stanford 2016). It is often associated with the names of Willard Quine and Pierre Duhem and chiefly concerns the move from evidence to theories. At the heart of the problem, according to Stanford, lies the "simple idea that the evidence available to us at a given time may be insufficient to determine what beliefs we should hold in response to it" (Stanford 2016).

There are, according to Stanford, two ways of understanding the problem of underdetermination. The first of these is named *holist* underdetermination. This type of underdetermination emphasizes our inability to determine clear theoretical consequences in the face of new evidence. Let's imagine that we are faced with unexpected experimental results. They do not conform to the hypothesis that was under test. Underdetermination occurs when the effects of such unexpected result can't be decided with certitude. Does it refute the hypothesis under test? Are the results, instead, artefacts coming from the experimental setup? Does it refute some more or less explicitly articulated auxiliary assumptions? Holist underdetermination deals with cases like these. This is not the form of underdetermination I discuss in this chapter.

The other form of underdetermination is qualified as *contrastive*. Contrastive underdetermination occurs when a variety of hypotheses can account for the existing evidence. In other words, the existing lines of evidence about an investigated phenomenon underdetermine the theories that can be drawn from them. In these cases, the equally supported theories that can be formulated are said to be *empirically equivalent*. A definition of empirical equivalence has been provided by Turner:

> Two incompatible theories or hypotheses, *H* and *H**, are empirically equivalent just in case they have the same empirical consequence class, which is to say that they have exactly the same consequences with respect to the observations that scientists could ever make (Turner 2007, 46).

This formulation contains a strong temporal dimension. It claims that the notion of empirical equivalence does not only concern the *current* state of an investigation but asserts equivalence as *in principle* irrevocable. In this view, cases of empirical equivalence are cases in which, in principle, no evidence could ever help distinguish between two hypotheses *H* and *H**. My interpretation of empirical equivalence, here, is weaker. It concerns only the *current* state of an investigation and posits that two theories *H* and *H** are equally responsive to the currently available evidence.

B. Underdetermination in historical sciences

Turner discussed the notion of underdetermination and empirical equivalence in the context of historical sciences. Together with Currie, they share the belief that this type of science "provides excellent source material for enquiring after the nature of underdetermination and how scientists respond to it" (Currie and Turner 2016, 43-44). Why would historical sciences provide such a locus[53]?

Currie and Turner attribute the pervasiveness of underdetermination to the nature of the evidence. "Historical science", they affirm, "is often marked by degrading signals, and thus incomplete data" (Currie and Turner 2016, 43). Turner, in another publication, makes the general point that "in historical science, the background theories tell us *how nature has destroyed the evidence*" (Turner 2007, 60, his emphasis). This is contrasted with particle physics, where, in his view, background theories can be used to know where to *find* new evidence. In other words, historical scientists, when lucky, can gather well-preserved or slightly altered traces of a past phenomenon. When unlucky,

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[53] I don't think this is Currie and Turner's intention, and neither is it mine, to argue that historical sciences constitute *the best* locus to study underdetermination. In these sciences, however, the problem of underdetermination is often brought to the fore.

these traces are, at best, severely damaged and barely interpretable or, in most cases, these traces are simply gone, erased by the passage of time. Bloch personifies the past as a “tyrant” who has the capacity to decide which information can be known and which is forever out of reach (Bloch 1949, 22). If traces of past events are sparse, one would hope to find relief in the use of well-established background knowledge to help to extract a maximum of knowledge from minimal evidence. These “lucky circumstances” can occur. Currie, for instance, describes how a single platypus tooth can be used to provide a “rich picture of *O. tharalkooschild*’s morphology, ancestry and ecology” (Currie 2018, 2). However, there are cases in which well-established background theories are of little help. Malaterre, discussing the research on the origin of life, emphasizes how principles of physics and chemistry are only weakly constraining the space of possibilities, making it hard to evaluate the epistemic worth of competing hypotheses (Malaterre 2010).

Following this argument, the lack of evidential constraints, constituted by faint traces and weak background theories, results in cases of contrastive underdetermination. Cleland, for instance, makes the pervasiveness of contrastive underdetermination a cornerstone of her account of the method of historical sciences (see Chapter 5 for a thorough discussion of her account). The first stage of this method, according to her, is the “proliferation of multiple competing hypotheses to explain a puzzling body of traces encountered in fieldwork […]” (Cleland 2011, 554). Only the discovery of a “smoking gun” can break cases of contrastive underdetermination. Forber speaks of cases of “contrast failure”, similarly ascribing to evidence the role to “favour one hypothesis over some set of alternatives” (Forber 2009, 249). Turner has a more pessimistic take than Cleland and Forber. He describes *local underdetermination problems* on which there is no point to dwell because information-destroying processes have erased the evidence that could have broken these contrastive underdeterminations (Turner 2007). He uses the example of the colour of the dinosaurs as a typically intractable epistemic task[54]. Similarly, Malaterre emphasizes the variety of possible and equally satisfying explanations that can be generated from traces of the early history of life. He argues that the deeper one goes in time, the more the problem of underdetermination tends to be salient and insurmountable (Malaterre 2010). On the whole, these philosophers agree on the widespread presence of problems of contrastive underdetermination in historical sciences.

Associated with this pervasiveness of contrastive underdetermination is a form of epistemic pluralism[55] in historical sciences. If the available evidence is not sufficient to determine the superiority of a hypothesis over others, then one has to leave room for a plurality of empirically equivalent hypotheses. The resulting pluralism[56] is the necessary evil that comes when scientists face epistemically difficult problems. In a perfect world, signals about the past would not have degraded this badly and would be sufficiently clearly interpretable to generate evidence that helps distinguish between competing theories. Instead, in this view, historical sciences often find themselves in the disappointing situation where pluralism has to happen because of the lack of (strong) evidence. Ideally

[54] Even though it turns out that this particular pessimistic bet turned out to be lost, it is now possible to "draw inferences about dinosaur coloration based on the microstructure of fossil feathers" (Turner 2016, 60).

[55] The forms of pluralism discussed here are, of course, not representing the whole spectrum of "pluralist" positions in philosophy of science. Other accounts include Dupré 1993; Kitcher 2001; Longino 1990; Mitchell 2003; Ruphy 2016.

[56] I use Kellert, Longino and Waters' distinction between "plurality" and "pluralism". The former being "a feature of the present state of inquiry in a number of areas of scientific research (…) characterized by multiple approaches, each revealing different facets of a phenomenon. There can be a plurality of representational and conceptual schemas, of explanatory strategies, of models and theories, and of investigative questions and the strategies appropriate for answering them" (Kellert *et al*. 2006, ix). Pluralism, on the other hand, is "a view about the state of affair: that plurality in science possibly represents an ineliminable character of scientific inquiry and knowledge (…)" (Kellert *et al*. 2006, ix-x).

(in optimistic outlooks), this pluralism is transient: new lines of evidence intervene to relieve this case of underdetermination and dissolve this plurality.

This form of pluralism is a subset of what Fehr, drawing from Kellert, Longino and Waters, defines as "modest pluralism":

> a state of affairs in which multiple explanations of a phenomenon are tolerated because it is expected that they will eventually resolve into monism (Fehr 2006, 168).

In modest pluralism, the plurality of hypotheses is a marker of the immaturity of a given investigation. Cleland endorses modest pluralism when she states that the two-step process of the proliferation of competing underdetermined hypotheses and the finding of smoking guns eventually "converges upon a single hypothesis" (Cleland 2011, 554). Sustained states of pluralism are, on the contrary, the disappointing result of "the absence of the requisite technology" that could make potential smoking guns emerge. In these cases, "historical scientists have little recourse but to resign themselves to a collection of equally viable, rival hypotheses" (Cleland 2011, 555). In cases of local underdetermination, Turner would simply encourage historical scientists to "simply move on to more tractable research questions" (Turner 2007, 47). To summarize:

(a) Underdetermination in historical sciences causes modest pluralism.
(b) Modest pluralism is something to be defeated and needs further evidence to do so.
(c) If further evidence is not found. Modest pluralism is sustained and is considered a disappointment.

Kellert, Longino and Waters argue that modest pluralism and the underlying ideal of unification goes against the grain of a more radical *pluralist stance*:

> Scientific pluralism […] holds that […] the multiplicity of approaches that presently characterizes many areas of scientific investigation does not necessarily constitute a deficiency. As pluralists, we do not assume that the natural world cannot, in principle, be completely explained by a single tidy account; rather, we believe that whether it can be so explained is an open, empirical question (Kellert *et al*. 2006, x).

Their commitment to pluralism can be interpreted as one avoiding to perceive the coexistence of hypotheses as mere negative outcomes of contrastive underdetermination from evidential shortcomings. It does not perceive pluralism as an ideally temporary phenomenon that the scientific community should aim to eliminate. It considers, instead, that obtaining consensus is not the sole measure of scientific success. They are other epistemic benefits to be reaped, notably from situations of pluralism. While I do not deny the existence of empirically equivalent hypotheses in historical sciences – some set of traces *do* have equally good explanations – I, however, think that the question of underdetermination has been illegitimately narrowed on the move from evidence to theories. I now argue that underdetermination runs deeper than that, down to the constitution of evidence. This deeper form of underdetermination and the causes underlying it allow for the defence of a more positive form of epistemic pluralism.

C. Extending the problem of underdetermination

The views on contrastive underdetermination presented above, whether or not they are applied to the case of historical sciences, all share a common assumption. At the heart of the reasoning is the idea that there exists a *shared pool of available evidence* that scientific hypotheses about a given phenomenon need to account for. The existence of this pool of evidence underpins the notion of empirical equivalence of hypotheses: they are empirically equivalent *with regards to* the pool of available evidence. This whole discussion, therefore, hinges on a continuous agreement across a scientific community concerned with a given phenomenon. This agreement concerns the *nature* of the available evidence – which, in line with the relational account of evidence sketched in Chapter 5, concerns what *counts* as evidence and what doesn't – as well as the *interpretation* of such evidence.

While this expectation is probably fulfilled in some cases, I think that it is an unrealistic premise in others. Instead, I think that methodological and conceptual commitments are often sufficiently divergent *within* some scientific communities to prevent a continuous agreement on the nature and meaning of available evidence. In other words, two (or more) scientists working on the same phenomenon, but having different methodological and conceptual commitments, will *not* produce theories that aim to account for the same pool of available evidence.

This state of affair has already been described in several different ways. Wylie, for instance, uses Kuhn's discussion of incommensurability (Kuhn 1962) to argue that

> Kuhn's analysis of revolutionary theory change in the natural sciences makes it clear that the assessment of competing theories depends on considerations that are not just diverse but also internally complex and unstable: the strands that make up a cable of comparative, evaluative argument may conflict with one another; even when researchers share criteria of adequacy they may apply them differently, yielding incompatible judgements about the relative strength of alternative theories; and the criteria are themselves open to revision as research tradition evolve […] (Wylie 2002, 163).

Wylie describes situations in which the internal disciplinary dynamics results in a heterogeneity of methods and concepts that prevents the evaluation of hypotheses on shared conceptual and evidential grounds. Chang similarly describes situations of methodological incommensurability in which "there are no shared, objective methodological standards of scientific theory appraisal" as well as "no external or neutral standards which may be employed in the comparative evaluation of competing theories" (Chang 2012, 59). Such situations prevent us from seeing the evaluation of evidential support as a "straightforward matter of logical or probabilistic connections between theory and observation" (Chang 2012, 29). An illustration of such situations is provided later with the case study I propose.

These considerations push the question of underdetermination, as well as the resulting form of pluralism, to a deeper level. Underdetermination does not only occur at the interpretational level but also concern the constitution of lines of evidence. Because the pool of evidence to be accounted for varies across a given scientific community, it is therefore quite unsurprising that a plurality of hypotheses coexists. Here, they do not coexist because of a purported empirical equivalence in which they would all account for the same set of evidence. Instead, they coexist because they account for different sets of evidence underlined by divergent methodological and conceptual commitments[57].

Rather than being a disappointing consequence of our lack of evidential access, this form of pluralism can be perceived rather positively. It results from the dynamism and continuous heterogeneity present within scientific communities. Coexisting hypotheses provide a map of the various conceptual and methodological commitments existing at a time, and further investigations allow for their exploration and refinement. This form of pluralism comes close to Chang's "active realism", which

> recommends that we should pursue *all* systems of knowledge that can provide us an informative contact with reality; if there are mutually incommensurable paradigms, we should retain all of them at once (Chang 2012, xix).

Applying this view to historical sciences, conceptually and methodologically divergent investigations should be pursued simultaneously in order to maximize progress. Consistent with the argument sketched in the previous section, cultivating this plurality of inquiries should not be expected to converge into a single hypothesis, and the eventual achievement of a consensus should not be taken to be the sole aim to be pursued. To summarize:

(a) Methodological and conceptual divergences underdetermine the pool of evidence to be accounted for;
(b) This causes what counts as evidence (and how to interpret it) to vary across historical scientists interested in the same phenomenon;
(c) A plurality of hypotheses results from the plurality of pools of evidence scientists aim to account for;
(d) These cases of epistemic pluralism are acceptable and might (or might not) be temporary.

[57] The constitution of evidence can also be underdetermined as a result of the processes of preparation of evidence. For instance, fossils in paleobiology are underdetermined since they result from choices from preparators that contain an ineliminable idiosyncratic dimension (Wylie 2014).

This argument can seem a little strong. It might look like an invitation to indiscriminately develop good and bad methodological and conceptual frameworks and to artificially maintain disagreements. As it seems with many defences of pluralism, a fear of relativism lurks in the background. In some cases, the coexistence of contrasted hypotheses can be deemed acceptable since each hypothesis provides a partial understanding of the phenomenon under study. Multiple partial perspectives can then be seen as incommensurable and legitimately coexisting. However, the investigations I am interested in are attempts to provide explanations of unique events. Because these events are treated as unique, is it possible to be satisfied with a sustained coexistence of contradictory hypotheses? Is it possible even if, as Edwards puts it about another historical context, "there is no heterogeneity in this demographic history, because the history has happened only once" (Edwards 2009, 2)? The benefits and risks associated with this thesis are further discussed in part 3. Before that, the next part illustrates the proposed rearticulation of the problem of underdetermination with a case study from the works of Cavalier-Smith and Martin. It constructs a TS to characterize disagreement over the following claim: "phagocytosis is required for the origin of mitochondria".

II. CASE STUDY: PHAGOCYTOSIS IS REQUIRED FOR THE ORIGIN OF MITOCHONDRIA

A. MOTIVATION AND PRECEDENT ANALYSES

In the previous chapter, I provided an extended study of the rise and fall of the Archezoa hypothesis. As a reminder, this hypothesis, now rejected, postulated the primitiveness of a group of amitochondriate eukaryotes. This chapter's case is concerned with the disagreement about the requirement of phagocytosis for the origin of mitochondria. As stated in Chapter 2, this debate has the advantage of not being philosophical *terra incognita* and this chapter directly benefits from O'Malley existing survey of the disagreement about the origin and early evolution of eukaryotes (O'Malley 2010). Because I do not fundamentally disagree with the points made in O'Malley's paper, I need to expand on the reasons for re-opening this case.

In her paper, O'Malley provides a historical survey of how the endosymbiotic theory for the origin of mitochondria came to be consensually accepted and a presentation of contemporary competing explanations for how this endosymbiosis happened. Presently, she argues, there are two main types of hypotheses for the origin of mitochondria, demarcating them into “phagotrophic” and “syntrophic” models. Phagotrophic hypotheses, chiefly defended by Cavalier-Smith, postulate the acquisition of phagocytosis as the main event in the process of acquiring the ancestor of mitochondria. Syntrophic accounts, defended by Martin, affirm that the endosymbiotic origin of mitochondria came from a metabolic partnership that turned into an obligate association (both hypotheses are summarized in Chapter 2). The two hypotheses can be said to fundamentally diverge on matters of timing: phagotrophic hypotheses make the acquisition of mitochondria an outcome of the evolution of phagocytosis, while syntrophic hypotheses argue that the origin of phagocytosis depended on the prior acquisition of mitochondria.

O’Malley carefully describes the evidential and conceptual divergent foundations of each camp. Her analysis confirms the extension of the problem of underdetermination that I propose above. About the debate around the “first eukaryotic cell”[58], she observes that both camps

> make their evolutionary case for particular candidate organisms by marshalling different combinations of genetic, biochemical, cell-biological, phylogenetic, and geochemical data (O’Malley 2010, 213).

Divergent views, then, are not the result of a “classical” contrastive underdetermination in which the same evidence equally supports competing hypotheses. In her analysis, which echoes my own, coexisting competing hypotheses are generated from a divergent set of evidence. This stems, according to her, from the multifacetedness of the problem and from the deeper conceptual and methodological disagreements underpinning the field. This debate is, in her view, “about justifying certain models, methodological choices, disciplinary commitments, epistemic strategies and ontological assumptions” (O’Malley 2010, 219). Since O’Malley did not explicitly frame her article in terms

[58] As a reminder, the origin of mitochondria and the origin of eukaryotes are supposed to be sufficiently interlinked causally and temporally to be considered as one event.

of questions of underdetermination, one of the interests of reopening this case is to extend her analysis in this direction.

Related to this, O'Malley also did not explicitly articulate a discussion of scientific pluralism from this case study. However, these questions are twice hinted to when motivating her analysis:

> By working our way through these evaluative strategies we may understand more about […] how scientific disagreements persist despite the conviction that increasing body of evidence will bring about reconciliation (O'Malley 2010, 213).
>
> Other angles of inquiry involve how a field of inquiry copes with decades of epistemological tension between contested and unresolved explanations, and whether, in fact, such tension is a problem or an asset for explanatory puzzle-solving (O'Malley 2010, 214).

These two quotes provide a concise and well-formulated summary of some of the motivations underlying this present chapter. The first quote interrogates one of the expectations associated with modest pluralism, namely "bringing in more evidence is going to dissipate the plurality of hypotheses". The second quote opens a reflection on the possible benefits and risks of sustained pluralism. Framing explicitly this case study as a discussion of underdetermination and pluralism allows me, I hope, to provide more extensive answers to some of the questions she raised.

Despite the several common points there are between my chapter and O'Malley's article, there remains a methodological difference. The case she studies is framed as a debate about the origin of eukaryotes. I choose to restrict this interest to the debate around the necessity of phagocytosis for this origin. The origin of eukaryotes, as it is generally understood (and described in chapter 2), is an interlinked series of problems, comprising:

(1) the origin and nature of the future host;
(2) the origin and nature of the future symbiont;
(3) the initial interaction between the future hosts and symbionts;
(4) the integration of the symbiont within the host;
(5) the required adjustments to turn this integration into a functional one;

(6) the changes from the last common ancestor to all mitochondria-related organelles to various forms of mitochondria-related organelles;
(7) the origin of the various eukaryotic innovations (nucleus, cytoskeleton, peroxisomes…).

Cavalier-Smith and Martin integrate all of these aspects into lineage explanations (see Chapter 3). This nevertheless does not mean that each of these aspects must be treated as epistemically indivisible. Restricting my attention on the necessity of phagocytosis, which concerns the integration of the symbiont within the host, allows me to remove some of the complexity of this epistemic context. For instance, lines of evidence supporting or undermining the various mechanisms proposed to explain the functional integration of mitochondria do not necessarily bear on the debates around the necessity of phagocytosis.

Restricting the scope to the question of phagocytosis is also a relevant entry point to discussions about pluralism. Competing claims about phagocytosis are one of the cornerstones of the controversy on the origin of eukaryotes. Dealing with this specific question zooms in on some of the most fundamental sustained disagreements between Cavalier-Smith and Martin.

B. Endosymbiosis as phagocytosis: pros and cons

To provide a reconstruction of the evidence defending and contesting the necessity of phagocytosis for the origin of mitochondria, I now use the framework for evidential claims presented in the previous chapter. As a reminder, Toulmin schemas (TS) decompose epistemic contexts into six kinds of components: *warrants* that license an inferential jump from *facts* to *claim; backings* and *rebuttals*, respectively supporting or undermining a given component of the context; and *qualifiers* which modulate the strength of the links between components. In the previous chapter, this framework was used to provide historical time-slices of the varying support for the Archezoa hypothesis. In this chapter, I use this framework in a comparative and static way to highlight the differences between two contrasting evidential claims.

I start by outlining the various lines of evidence for the argument defending the necessity of phagocytosis (the “phagocytosis first” argument henceforth). An important and intuitive fact for this claim is the result of a consensus. The now accepted endosymbiotic theory for the origin of mitochondria, which postulates the integration then enslavement of a free-living bacterium that became the last common ancestor of all mitochondria-related organelles, requires the engulfment of an organism by another. Combining this fact with the principle that the integration of foreign organisms requires phagocytic capabilities supports the claim that the origin of mitochondria requires phagocytosis.

Another, more sophisticated fact, involves reasoning in terms of energetics. It is argued by Cavalier-Smith that the phagocytotic feeding mode allows for a release of the selective pressures applied to cell size. Phagocytosis thus enabled marked increases in cell size, these increases in cell size “imposed upward coevolutionary pressures on genome size” and nuclear volume (Cavalier-Smith 2014, 15). By allowing for larger genomes, the acquisition of phagocytosis then allowed eukaryotic cells to develop the various innovations related to eukaryotic cells. Because of that, the engulfment of a free-living cell at the origin of mitochondria-related organelles is considered here as a secondary consequence of the acquisition of phagocytosis, it accelerated but was not the main trigger for the origin of the main eukaryotic features.

These two data are backed from two sources. The first one is the evolutionary assumption of the greater importance and priority given to autogenous evolution – internally-driven changes in cellular constraints and potential (O’Malley 2010, 212). The second backing comes from the coherent integration of the “phagocytosis first” claim into a bigger evolutionary explanation about the origin of eukaryotes and early evolution of life. A representation of the evidential claim in favour of “phagocytosis first” is represented in Fig. 15.

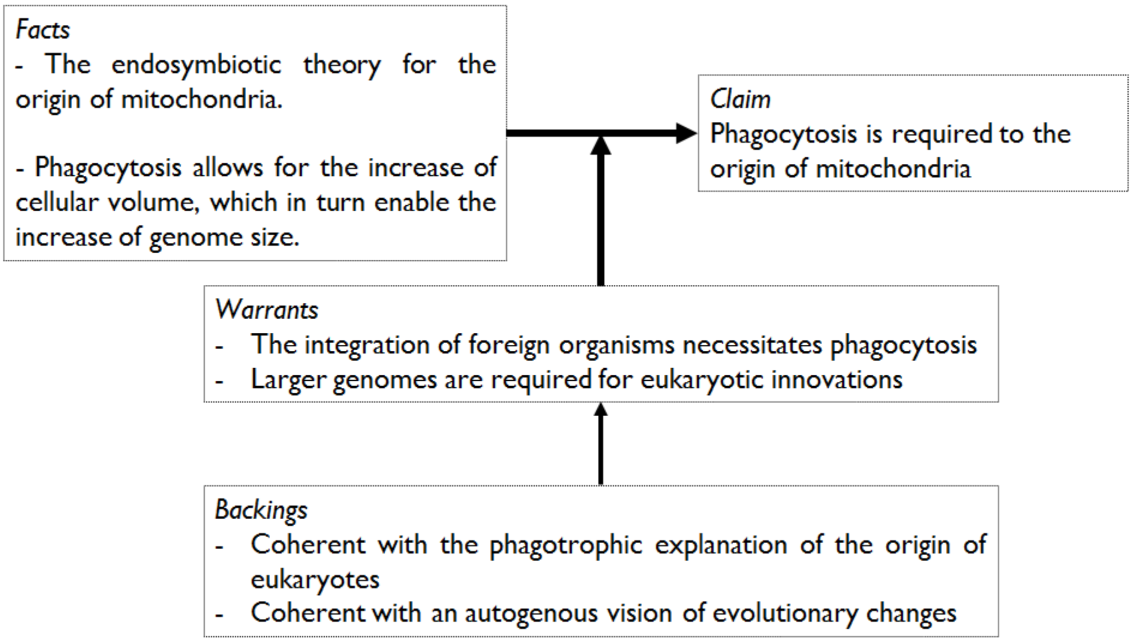

Figure 15 Representation of the various lines of evidence in favour of a "phagocytosis first" hypothesis.

The opponents to the "phagocytosis first" claim, claiming instead that the acquisition of mitochondria did not require the evolution of phagocytosis (the "mitochondria first" argument henceforth), ground their argument on diametrically different facts. They affirm, on the contrary, that it is only *via* the acquisition of mitochondria that the rest of the eukaryotic innovations, including phagocytosis, could evolve (Lane and Martin 2010). The acquisition of mitochondria would have allowed these cells to be energetically sufficiently efficient to afford bigger genomes, while mitochondria-less prokaryotes simply cannot afford them. These bigger genomes, so the argument goes, are the prerequisites for supporting the various eukaryotic innovations. The acquisition of mitochondria, then, is the exogenous event that was necessary to trigger the origin of eukaryotes. This openly contests one of the backing of the "phagocytosis first" claim, which postulates that the most important evolutionary changes come from the inside, as well as the validity of the bioenergetic line of reasoning behind one of the facts.

It is one thing to argue that only the acquisition of mitochondria, and not phagocytosis, could allow for the evolution of eukaryotes, it is another to affirm that phagocytosis is just too costly to evolve at such an early stage of eukaryote evolution. This second line of reasoning affirms that phagocytosis is not a trait that can be treated as an "early" one. It is rather affirmed that phagocytosis is a “sophisticated endocytic process” (Martin *et al*. 2017, 15) which “demands a fully functional endomembrane system” (Gould *et al*. 2016, 7). Arguments from Martin and his colleagues are based on a breakdown of the needed components for this cellular function, and the energetic demands that are associated with it.

Another type of facts mobilized in favour of the “mitochondria first” claim comes from the observation of extant organisms. It is argued that contemporary prokaryotes are capable of displaying Russian dolls-like arrangements. In these cases (reviewed in Martin *et al*. 2017, 23-26), some prokaryotes harbour other prokaryotes within them. The interesting thing about these host prokaryotes is that they are not capable of phagocytosis. While the actual mechanisms underlying the possibility of this integration are still debated, these cases, according to Martin, clearly show that the integration of foreign organisms does not necessitate phagocytosis, in sharp contradiction with the warrant mobilized in favour of the “phagocytosis first” claim.

Backings for the “mitochondria first” claim comes from the commitment of their defenders to alternative accounts for the evolution of eukaryotes (for instance, the “hydrogen hypothesis” by Martin and Müller described in Chapter 2). These explanations do not postulate the necessity of phagocytosis for the endosymbiosis of mitochondria. At the same time, they deny the autogenous idea that the main evolutionary events primarily come from the inside, by making the acquisition of mitochondria play the central role in the origin of eukaryotes.

An overall summary of the “mitochondria first” evidential claim is provided in Fig. 16. The community of evolutionary biologists still clearly sided on this particular questions. A substantial number of evolutionary biologists still support the necessity of phagocytosis, and there are similarly many scientists defending the opposite.

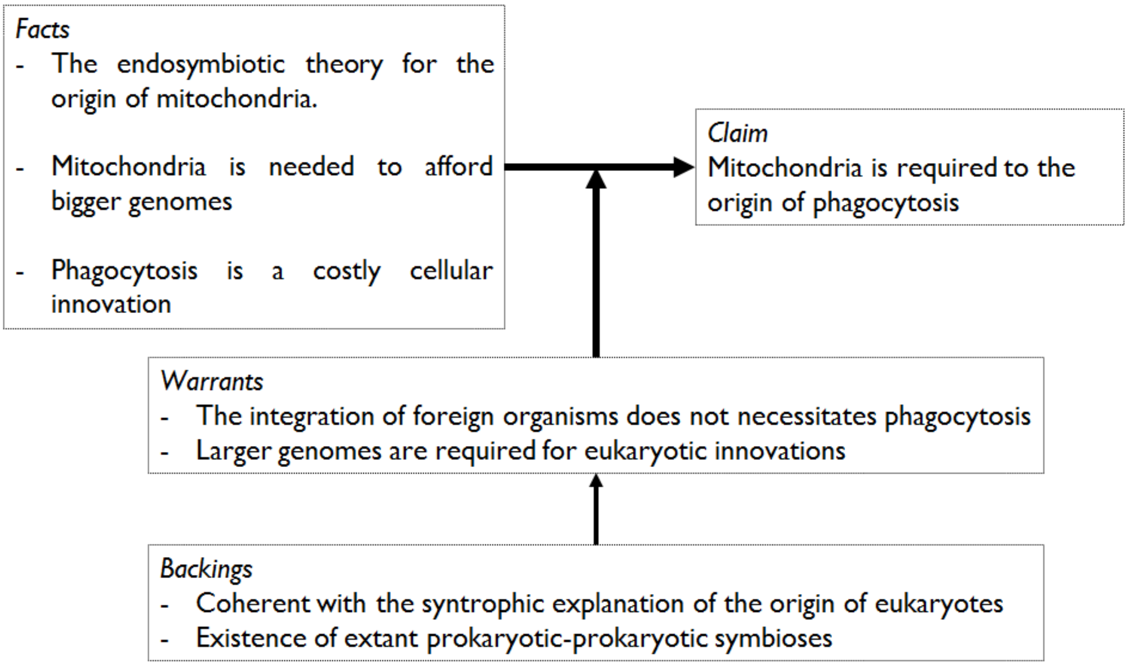

Figure 16 Representation of the "mitochondria first" evidential claim.

C. Analysis

The application of TS to this case confirms O'Malley's analysis as well as the reconfiguration of the topic of underdetermination I proposed above. The requirement of phagocytosis for the origin of mitochondria is underdetermined. The nature of this underdetermination is not classically contrastive. The case study displays that the disagreement between "phagocytosis first" and "mitochondria first" positions does not revolve around a conflicting use of the same evidence. Both sides mobilize conflicting facts, warrants and backings. They disagree on bioenergetics arguments, for instance, as they conflict over the validity of a fact ("phagotrophy-based metabolism enables the increase of cellular volume") and on the bioenergetic reasoning mobilized in support of it. This chapter only scratches the surface of this particular kind of disagreement, as the arguments based on energetic considerations applied to biological entities are worthy of an extended study. Another illustration of a factual disagreement is the "prokaryote Russian-doll" system used in support of "mitochondria first claims". While it is a powerful backing for some, it is dismissed as irrelevant to the problem to defenders of "phagocytosis first" claims (Cavalier-Smith 2002, 307; also de Duve 2007, 400)[59]. Integrating these

[59] Cavalier-Smith: "I consider that such large bacteria with relatively flexible surfaces able to take up other bacteria could have evolved only within the

positions within wider explanations about the origin of eukaryotes also provide variable support, depending on the scientist's take on the subject. On the whole, the disagreement runs over several levels and therefore cannot, again, be strictly narrowed to a matter of empirical equivalence. I further argue that it is not a case of a conflict of the hierarchy of the available evidence. These scientists are not disagreeing about which of the facts matters the most, but rather about what *counts* as facts as well as on the background knowledge underlying their constitution.

Secondly, this case study highlights the variety of knowledge mobilized on the discussions around this claim. The debate on phagocytosis is another illustration of historical scientists' evidential omnivory, discussed in previous chapters. Formulating evolutionary explanations necessitates taking into account an astounding breadth of cellular biological details, and phagocytosis is no exception. Phagocytosis as a cellular phenomenon has been subject to a careful breakdown by all the participants. Some emphasized, for instance, its relation to the cytoskeleton, others the acidification of food vacuoles or aspects of the membrane trafficking system. Understanding the evolution of each structure and process involved is potentially evidentially relevant. Phagocytosis as an energetic process has also been more and more brought to careful scrutiny, and it might constitute the main focus of future discussions on the issue. These discussions require the mobilization of notions of bioenergetics and thermodynamics to help estimate, for instance, the energetic cost of cellular innovations. Recent claims about the discovery of potentially phagocytic archaea (Spang *et al*. 2015) also spark debates around the genetic basis of phagocytosis.

Will this disagreement be settled at any point in the near future? An epistemic bet on this question is a matter of attitude. An optimist considers that new lines of evidence and improved theory and methodology will help to provide stronger arguments that will give a decisive advantage to "mitochondria first" or "phagocytosis first" claims. The acceptance of the endosymbiotic theory for the origin of mitochondria discussed above, and the rejection of the Archezoa

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protective cytoplasm of pre-existing eukaryotic cell and are therefore irrelevant to the mechanical problem of the origin of mitochondria."

hypothesis, discussed in the previous chapter, showed that this is not impossible to achieve something near certitude in evolutionary biology. There are, on the contrary, two reasons that can give grounds for pessimism. The first is to deny the epistemic tractability of the problem: the depth and variety of relevant knowledge are just too big to be integrated into a coherent and definite answer. The second is to consider that the damage made by information-destroying processes are too deep and that too much evidentially relevant information has been erased. This would prevent historical scientists from ever finding a "smoking gun": a sufficiently clear set of traces that would constitute sufficiently strong evidence in favour of one claim over another. As made clear by this analysis, I don't feel sufficiently confident to escape agnosticism on this question. Keeping with a pluralist stance, the fate of this controversy is an open, empirical question.

III. METHODOLOGICAL PLURALISM: PAYOFFS AND RISKS

A. BENEFITS OF METHODOLOGICAL PLURALISM

In all cases, a commitment to this pluralist stance, as outlined in the first section, reminds us not to perceive perennial disagreements as a failure of scientific research. There are many potential drawbacks associated with cases of forced consensus and unwarranted abandonment of competing hypotheses. Chang provides a particularly eloquent discussion of such a case in $18^{th}/19^{th}$-century chemistry (Chang 2012, 1-70). He argues that the "phlogiston theory" of combustion has been prematurely dismissed in favour of the "oxygenic theory". This unwarranted pursuit of a single system of knowledge then resulted in closing off "certain theoretical and experimental avenues for future scientific work" which eventually "retarded scientific progress in quite tangible ways" (Chang 2012, 47). Actively maintaining multiple competing claims underlined by divergent conceptual and methodological commitments, as recommended by Chang's active realism outlined above, allows instead maximizing epistemic entry points on various aspects of reality.

In this case, this form of pluralism pushes us to view the coexistence of the "phagocytosis first" and "mitochondria first" claims as a positive outcome of the existing heterogeneity of conceptual and methodological commitments in evolutionary biology. This plurality of approaches allows for the continuous redefinition and expansion of the problem. An illustration from the case study is the recent rise at the centre stage of energetic considerations. Increased attention on the evolution of membranes (see, for instance, Lombard *et al*. 2012) might also help the development of unforeseen lines of evidence. The development of this multiplicity of approaches would surely be hampered if the current plurality of opinions were not present.

More generally, promoting methodological pluralism diverts us from considering the achievement of a consensus to be the sole epistemic good to be pursued. Currie distinguished between direct and indirect epistemic goods (Currie 2018, 293-307). The former could be conceived as theoretical advances directly related to the investigation in question. In this case, direct epistemic goods range from theory choice to methodological and conceptual changes that bear directly with the investigation about the role of phagocytosis in the evolution of eukaryotes. Indirect epistemic goods are goods generated by this particular investigation that turn out to benefit other ones that are more or less related. The achievement of a consensus, in this view, is *one direct epistemic good among others*. Similarly, by encouraging a shift from the study of scientific theory to the study of scientific practice, Chang encourages us to stop solely being preoccupied with “propositions and their truth” (Chang 2012, 17). Here, while the main question does not get a definite answer, the debate around phagocytosis yield a variety of direct and indirect epistemic goods, including:

- knowledge about phagocytosis and its anterior evolutionary forms;
- knowledge about the physiology of extant and extinct microorganisms;
- continuous improvement of methods of phylogenomic analysis, the study of fossils, the study *in vitro* and *in vivo* of extant organisms…

Again, there are many ways with which one could see this sustained disagreement as positively impacting our knowledge (see O’Malley 2013; 2016, for similar points on molecular phylogenetics). It can even be the case, as Chang outlined in the phlogiston/oxygen controversy, that competing hypotheses accidentally mutually support each other by providing knowledge that will eventually benefit its competitor (Chang 2012, 49). Will such benefits be lost if this disagreement is eventually solved and only one claim survives? I think that resolving the disagreement on phagocytosis is likely to open up new questions and, hence, new areas of disagreements. The benefits of methodological pluralism, even though they are not always direct, would, therefore, be continuously generated in this domain of investigation.

Embracing this form of pluralism and reaping its benefits is, however, not a risk-free activity. There are inherent difficulties and challenges associated with the coexistence of competing claims.

B. Methodological Pluralism's challenges

What are the risks associated with the coexistence of incompatible hypotheses? In his defence of pluralism, Chang identifies three types of challenges a sceptic might pose. Because my view builds upon his, these challenges are, at face-value, similarly relevant to my argument:

(1) "Wouldn't the co-existence of different systems cause confusion and prevent effective research?"
(2) "Doesn't the maintenance of too many competing scientific systems dissipate valuable resources?"
(3) "Won't arguments about fundamentals divert scientists' energy and attention, preventing them from launching into specialist research?" (Chang 2012, 48-49)

The third worry does not affect this case study: this chapter's case of disagreement, if anything is, is part of specialist research. The first question is a matter of potential epistemological obstacles to scientific progress associated with the coexistence of competing hypotheses. The second question is a matter of fair repartition of available research funding.

What confusion could be caused by a sustained disagreement about the necessity of phagocytosis? The key problem, here, is the variety of knowledge involved in the case. Because the problem is multifaceted, a wide range of expertise is called upon. It seems impossible to require from each person involved in the discussion to have the whole range of relevant expertise. This poses a challenge in terms of trust and contestation *vis-à-vis* the various types of expertise in the community. How to facilitate the exchange of knowledge between different actors? How to avoid, on one side, authoritative impositions of insufficiently scrutinized claims and, on the other, illegitimate dismissal of claims because it was made with unfamiliar but relevant bits of knowledge? The heterogeneity of conceptual and methodological commitments makes these risks particularly salient.

If hypotheses coexist, how to make a decision about which research program should be funded? Chang brushes off this legitimate concern in his specific case by invoking the inexpensiveness of the phlogiston research of the time (Chang 2012, 49). This argument cannot be invoked here. The epistemic pluralism adopted in this chapter emphasized a change of focus from theories aiming solely at generating true descriptions of the world to a wider vision of the direct and indirect epistemic goods that can be fruitfully generated. Therefore, future resources should not be allocated on the promise that the funded project will overrule its opponent. Assessments of the potential payoffs, instead, can be made by estimating potential methodological improvements, potential relevance and opportunities for other areas of knowledge or for some of our conceptual understandings. This is a definitely not a "foolproof guide to a fair distribution of research funds", just some suggestion for criteria of evaluation of future projects.

C. Concluding thoughts

I have argued and illustrated in this chapter that the problem of underdetermination in historical sciences runs deeper than has usually been characterized. This is not a worrying sign. It instead provides, I think, a more realistic picture of underdetermination as a problem not restricted to the interpretation of evidence. It also can affect the choice of what counts as evidence and the methodological and conceptual commitments underpinning the creation and interpretation of this evidence. This also creates a more positive form of pluralism: from a necessary evil of cases of contrastive underdetermination, it becomes an indicator of the variety of (sometimes conflicting) approaches employed in the investigation of a phenomenon. This rearticulation of the relation between underdetermination and pluralism is illustrated by a study of the disagreement over the necessity of phagocytosis for the origin of eukaryotes.

This chapter closes on the benefits and risks associated with methodological pluralism. Cultivating several competing approaches is a double-edged sword: on one side an expansion and enrichment of the problem, on the other a risk of confusion and cross-talking between non-overlapping types of expertise. The problem of the unavoidably unequal distribution of expertise seems akin to the one faced in all cases in which various scientific actors facing a multifaceted problem have non-overlapping expertise. For this reason, it would be worth delving on the literature dedicated to “trading zones”, “inter-field theories” and “interdisciplinarity”. These are cases in which, as Chapman and Wylie describe, “it takes a community to mobilize the critical scrutiny necessary to hold evidential claims accountable” (Chapman and Wylie 2016, 84). Within historical studies of evolutionary biology, the importation of molecular techniques to the construction of phylogenetic trees has also been the object of extensive studies and could be a good place to look at to understand the forces driving the extension and the changing importance of types of evidence.

CONCLUSION

The present dissertation proposed an exploration of several aspects of a particular scientific endeavour, namely, the study of unique events in the past. This was done on the basis of a case study, focusing on the contrasted hypotheses of Bill Martin and Tom Cavalier-Smith on the origin of eukaryotic cells. The philosophical analyses proposed in the chapters initially derived from a general sense of puzzlement about the case. How was it possible for these radically different theories about the same event to coexist? This question, addressed in Chapter 7, drew with it a set of additional inquiries about the practice of these scientists. From this, the present work aims to sketch a philosophical picture of these scientists' practice.

Chapter 1 served as an introduction that interrogated the scope and meaning of the philosophy of historical sciences. I particularly interrogated the relation between historical sciences and human history, arguing against a strong methodological separation between both. This makes legitimate the use of analyses of the practice of human history as relevant resources for the case study at hand. I then drew parallels between 'metaphysical' and 'practice-oriented' projects in philosophy of history and philosophy of science. 'Hybrid works', looking at metaphysical and practice-oriented questions simultaneously, were then described as a source of inspiration to this dissertation.

Chapter 2 summarized the (current) content of the hypotheses of Tom Cavalier-Smith and Bill Martin about the origin of eukaryotes. This provided a scientific basis from which the philosophical analyses in the following chapters could draw and expand.

Chapter 3 started this analysis by characterizing the *type* of theories that such hypotheses were. After reviewing the two main candidate epistemological tools for this task, *ephemeral mechanisms* and *narrative explanations*, I presented these two as subtypes of *lineage explanations*. This opened the possibility of having mixed lineage explanations that combined mechanistic and narrative elements within it. Are Cavalier-Smith and Martin's hypotheses mixed lineage explanations? The application of this concept to my case study pointed to the rather narrative character of these scientist's theories. Moreover, this investigation revealed the presence, within these explanations, of non-linear events, running in parallel, as well as “messy” elements, events occurring in an imprecise order. I left it open to further investigations what these apparently messy elements tell about the maturity of such explanations, as well as how mechanistic such explanations will turn out to become.

Chapter 4 related these lineage explanations to the question of scientific representations. How are these explanations representing their target? I proposed an analysis focused on Toon’s “make-believe” and Frigg and Nguyen’s “DEKI” accounts. The latter argues to improve on the former’s main shortcomings. I argued to the contrary by defending an interpretation of the make-believe account as a special case of “DEK(I)”, one in which the process of “imputation” (“I”) is superficially invisible because it is maximized. I then characterized Cavalier-Smith and Martin’s lineage explanations as cases of “D(E)K(I)”, cases similar to Toon’s but where “exemplification” (“E”) was also made invisible. This created a picture in which lineage explanations directly represent their target by containing fictional truths about it. It is possible, with further exploration, to uncover other, implicit, fictional truths. This set of explicit and implicit fictional truths constitute direct representations of the event in question and are the set of propositions that are to be assessed empirically.

How are these fictional truths assessed? In Chapter 5, I proposed a critical survey of existing accounts of the methodology of historical sciences. From a set of quotes stemming from the context of human history, I first identified a series of components pertaining to investigations in historical sciences. This served as a set of guidelines with which to evaluate existing concepts put forward by philosophers of historical sciences. Despite its optimistic outlook and emphasis on mutual interactions of lines of evidence, I criticized Cleland’s account as placing too much emphasis on the discovery of trace evidence, and as neglecting the importance of theories in directing inquiries and generating evidential relevance. I then presented a series of concept adapted from Currie and Wylie that, I think, adequately capture key aspects of the methodologies of these sciences. This analysis did not enable me to construct a step-by-step account describing how these sciences work. I think that the inherent diversity of practices within historical sciences prevent such thing to happen. It instead provided a series of concept that, I think, can be useful to analyse local practices within these sciences.

Chapter 6 narrowed down its attention on the topic of evidential claims. How is evidence used in the justification of hypotheses about the past? I used some of the insights garnered from the previous chapter to articulate some desiderata for such an account. I then proposed to adapt Toulmin's framework for argumentation, "Toulmin schemas", to capture the dynamics of evidential claims in historical sciences. I illustrated the efficiency of this framework by using it to capture the shifting evidential dynamics of the Archezoa hypothesis, a taxonomic and evolutionary hypothesis from Cavalier-Smith that was initially supported then progressively rejected.

In the final chapter, I proposed another application of Toulmin schemas to help to address different philosophical concerns. This time, they revolved around questions of theory underdetermination and pluralism. I first proposed a critique of the notion of "contrastive underdetermination" as stemming in a form of "modest", and ideally temporary, pluralism. I argued that because of the heterogeneity of conceptual and methodological commitments within a given discipline, underdetermination was not mainly concerning the data to hypothesis link: it runs deeper, at the level of the constitution of evidence. Conceptual and methodological heterogeneity indeed undercuts disagreements on what counts as evidence, ultimately leading to formulations of hypotheses based on non-overlapping sets of evidence. Pluralism, here, stems from the inherent heterogeneity within a discipline. Such defence of a deeper form of underdetermination was illustrated by the disagreement on the necessity of phagocytosis between Cavalier-Smith and Martin. This disagreement is based on what counts as valid evidence and on divergent conceptual and methodological commitments. I concluded this chapter by outlining the potential benefits (the flourishing of many perspectives) and risks (the absence of sufficient common grounds for a fruitful discussion) which this pluralism entails.

GLOSSARY

actin: Protein composing the cytoskeleton involved in cellular movements and muscular contraction.

adenosine triphosphate (ATP): An organic molecule that serves as the cellular currency of molecular energy.

autocatalytic: To be said of chemical processes that are capable of increasing their own rate without the intervention of an external substance (a catalyst).

autotrophy: Designates the capacity by organisms (autotrophs) to produce complex organic compounds from simple ones (to contrast with ”heretrophy”).

bacteriophage: Viruses that specifically infect and replicate within a bacterium.

centrosome: In the cytoskeleton, centrosomes are responsible for the organization of microtubules.

cytoplasm: Space delineated within the plasma membrane of a cell.

cytoskeleton: Literally meaning “cellular skeleton”. The cytoskeleton is a cellular network responsible for many of the cellular movements within and outside of the cell.

endomembrane system: System of membranes that are found within the cytoplasm of a cell, typical of the eukaryotes.

endoplasmic reticulum: An interconnected network of compartments found in the cytoplasm of eukaryotes. This organelle plays the main role in protein synthesis and key functions in the synthesis and maturation of lipids.

glycoproteins: Proteins with chains of sugars (oligosaccharides) attached to them.

heterotrophy: designates organisms (heterotrophs) which are incapable of producing their own complex organic compounds and instead import these compounds from the external environment (to contrast with “autotrophy”).

hydrogenosomes: Organelle mainly responsible for energy production in the cell. Only works in anaerobic environments. Hydrogenosomes can also possess their own DNA, and are evolutionarily related to mitochondria and mitosomes.

hyperthermophiles: Designates a particular affinity of organisms for a type of environment. Hyperthermophiles live in extremely hot environments (from 60 degrees Celsius).

introns: Portion of a gene that will be spliced out after the transcription process (at the pre-mRNA stage). To be contrasted with “exons”, the part of a gene that will ultimately be translated into a protein.

lipids: Lipids are molecules made of a hydrophilic head and of a hydrophobic tail. Among specific characteristics, the link between the head and tail, in archaeal lipids, is made of an ether bond and the long hydrophobic tail is made with isoprene chains.

lysosome: Cellular structure responsible for the digestion of intracellular material.

mesophiles: Designates a particular affinity of organisms for a type of environment. Mesophiles live in environments with mild temperature (between 20 and 45°C).

microtubules: Network of cellular fibres, part of the cytoskeleton, involved in the maintenance of cellular structure and cellular transport.

mitochondria: Organelles mainly responsible for energy production in the cell. They do so mainly in aerobic environments. Mitochondria possess their own DNA and are evolutionarily related to hydrogenosomes and mitosomes.

mitosomes: organelle evolutionarily related to hydrogenosomes and mitosomes of yet undefined function.

mRNA: see “transcription”.

oligosaccharides: small polymers (a few residues) composed of sugars.

peptidoglycan: bacterial membrane component that maintains the membrane’s structural integrity.

peroxisome: Eukaryotic organelle responsible for the degradation and synthesis of various cellular components (lipids, amino acids, sugars)

phagocytosis: Process of engulfment and digestion of foreign material by a cell.

polyphyletic: A polyphyletic group is a taxonomic group that has been grouped together but turns out not to share an immediate common ancestor.

ribosome: see “translation”.

transcription: Process that transforms a type of molecule of biological information into another. Transcription processes transform DNA into corresponding messenger RNA (mRNA) by the use of a protein named polymerase.

translation: Process that transforms a type of molecule of biological information into another. Translation processes transform RNA into a sequence of amino acids usually called proteins using a multi-protein complex called “ribosome” and with transfer RNA (tRNA) carrying amino acids to the correspondent RNA triplet.

tRNA: see “translation”.

tubulin: constituent proteins of microtubules, fibres composing the cytoskeleton (see above)

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