

**SCIENCE AS A CULTURAL EVOLUTIONARY PROCESS:
BRIDGING EVOLUTIONARY EPISTEMOLOGY AND CULTURAL EVOLUTION**

by

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Abstract

This study provides an account of how a naturalistic orientation in epistemology known as evolutionary epistemology can benefit from integration with some more recent advancements in cultural evolutionary theory. In particular, this thesis explores the ways in which a Darwinian selective framework of culture provides new insights to rethink unsolved epistemological disputes and problems such as the growth of scientific ideas, the notion of scientific progress, the ‘context distinction’, the theory ladenness of science and a demarcation criterion between science and other cultural realms. All these issues are approached from an evolutionary and selective standpoint that assumes that biological and cultural selection are representative of a wider and more encompassing selective process. Thus, the thesis also explores theoretical disputes concerning the analogies and disanalogies between cultural and biological selection.

Five early evolutionary analogies of knowledge accretion are explored in the first chapter setting the context of the debate for the epistemological issues that are connected in the other chapters to cultural evolution. Here, a populationist account of scientific ideas is presented as a viable model to study the conceptual and ‘ideational’ dynamics of change in science. In the last part of this work, my populationist and selective model is applied to study a specific scientific innovation: a molecular technique for taxonomic identification and classification known as DNA barcoding. Finally, the last chapter explores the use of text analysis to investigate the network of concepts and ideas that relate to the ‘DNA barcoding’ concept’. Here, I evaluate the impact of this technique on the fields of taxonomy/systematics, particularly, for what concerns the so called ‘species problem’.

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Introduction

Science apparently grew out of biological and cultural development as its most superfluous offshoot. However, today we can hardly doubt that it has developed into the factor that is biologically and culturally the most beneficial. Science has taken over the task of replacing tentative and unconscious adaptation by a faster variety that is fully conscious and methodical.

Ernst Mach

Evolutionary epistemology is a naturalistic orientation that contests the traditional view that the subject matter of epistemology is the concept of ‘knowledge’.¹ On the contrary, evolutionary epistemology maintains that ‘knowledge’ must be studied as a process, as a certain kind of adaptive response to environmental conditions. Moreover, most if not all evolutionary epistemologists would agree that there is no clear cut distinction between common sense knowledge and scientific knowledge. Science is just a perfected and refined form of common sense knowledge.² Starting from this assumption, the ‘process of

¹ The traditional tripartite analysis of knowledge identifies three conditions (justification, truth, belief) as necessary and sufficient conditions for knowledge. Traditional epistemologists state that epistemological claims regarding necessary and jointly sufficient criteria for ‘knowledge’ are gained through ‘a priori’ reasoning and not by means of empirical investigation (see Crumley, 2009, Chapter 7). In this respect, their formulations are largely definitional and conceptual. On the contrary, evolutionary epistemology challenges the assumption that ‘knowledge’ is a matter of ‘a priori’ reasoning alone. Rather, ‘knowledge’ is a bio-cognitive state that can be empirically investigated and subjected to scientific description.

² The idea of a continuation between common sense knowledge and science is not exclusive to evolutionary epistemology. Popper admits that many philosophers and scientists such as “Kant, Whewell, Mill, Pierce, Duhem, Poincaré, Meyerson, Russell and Whitehead (...) would agree that scientific knowledge is the result of the growth of common-sense knowledge” (Popper, 1959, p. 22). However, evolutionary epistemologists add an evolutionary spin to this argument connecting scientific cognition to ‘common sense’ and from there to animal cognition.

knowledge’ is framed into a Darwinian perspective where ‘knowledge’ in general and ‘science’ in particular are seen as selective processes.

The main scope of this thesis is to explain how it is the case that the ‘process of knowledge’ and thus ultimately science are Darwinian selective processes. At this point, an important preliminary clarification must be made. The selective process investigated here is first and foremost a Darwinian cultural selective process. Thus, conspicuous parts of this work are dedicated to illustrating what Darwinian cultural evolution and selection is and how it relates to and differs from biological selection. Following the steps of Darwinian cultural evolutionists, I maintain that biological and cultural selection must be seen as two interrelated but also largely autonomous selective processes. The main idea of Darwinian cultural evolution is that culture became in our species a sort of parallel and largely independent (from the biological base) system of inheritance and transmission of information. Coevolutionary feedbacks that go in both directions (e.g., from our genetic base to culture and from culture to the genetic base) remain important drivers of both biological and cultural change and, of course, biological considerations underlie the cognitive mechanism that allows cultural accumulation in the first place (e.g., complex ‘social learning’ skills as an adaptation). However, the main idea is that the two selective processes are largely independent from each other. Or, to put it in another way, both evolutionary processes, notwithstanding their differences, can be subsumed under a general theory of selection.

Thus, to show the significance of cultural evolutionary theory for epistemology it is first necessary to introduce the reader to two main areas of research. On one side, evolutionary epistemology presented through the contributions given by the most influential protagonists of the field. On the other, Darwinian cultural evolutionary theory and particularly those theoretical debates that contribute to present science as a selective process. This thesis is intended to tentatively bridge evolutionary epistemology with more recent advancements in Darwinian cultural evolution. The chapters are intended to offer a sort of ‘pincer movement’, where a core subject (knowledge and science as selective adaptive processes) is attacked from two sides, two fields of research that converge to present a more comprehensive selective and adaptationist account of scientific growth. Here, possible flaws present in the early formulations of the evolutionary epistemologists are exposed and corrections are put forth in light of more recent advancements in Darwinian cultural evolutionary theory.

Having these goals in mind, the first chapter provides the philosophical context of the debate in evolutionary epistemology. Here, I discuss five of the most influential early evolutionary analogies of science proposed by Popper, Kuhn, Toulmin, Hull and Campbell. I begin by examining the influential “Popper-Kuhn debate” through the lens of their distinct evolutionary epistemologies. Then, I move to consider Toulmin and Hull’s evolutionary models that assign a preponderant role to the social dynamics of science. Concerning Hull, it is suggested that his model fails to recognize a ‘replicator’s eye view’ of selection (analogous to the ‘gene’s eye view’ of biological selection). This perspective assumes that scientific ideas are the relevant units of selection, not the scientists and their scientific circles as Hull seems to suggest. Finally, Campbell’s universal selectionism is presented as the most

successful and comprehensive evolutionary epistemology. As an in depth historical survey of each author was beyond the scope of the chapter, I have limited my analysis to the author's key theoretical stances that highlight their peculiar form of selectionism. Between several commonalities, it is shown that each of the five conceptions has its own distinctive account of the selective factors operating in science.

The second chapter introduces the reader to some debates in Darwinian cultural evolution that bear particular significance for evolutionary epistemology. This chapter focuses on three main programs of research in Darwinian human behavioural science: evolutionary psychology, dual inheritance theory and memetics. In the first section, Ruse's peculiar evolutionary epistemology is evaluated in relation to more recent advancements in the evolutionary psychology of reasoning. The second section presents the cultural selectionist model of dual inheritance theorists and discusses its significance for evolutionary epistemology. Here, the notion of 'population thinking' acquires particular importance for its application both to cultural evolution and evolutionary epistemology. I argue that "population thinking", among other things, offers a model to understand science as a populational process. Scientific ideas are populations of replicators divided in sub-populations that constitute scientific fields and branches of a field (disciplinary matrices). These populations of replicators are engaged in an evolutionary and selective process. Finally, the last section, starting from Dennett's discussion of the selective environment of a 'meme', evaluates the selective role played by the external reality in the selection of what can be described as 'scientific memes'.

The third chapter addresses the evolutionary theory of creativity and its significance for evolutionary epistemology. Here, I acknowledge a number of critiques that have validly

pointed out the disanalogies between the process of creativity and biological selection and I evaluate the significance of these objections in the context of an evolutionary selective epistemology. It becomes apparent that we need to distinguish between two phases: the phase of generation of variation and the phase of transmission/selection. The phase of generation might not be selective while the transmission phase seems suitable for a selective treatment. The rest of the chapter explores the significance of a selectionist theory of scientific creativity for epistemology. First, I explore the implications for the ‘context distinction’, where a selectionist approach clarifies that the context of discovery and that of justification are interconnected selective processes. Secondly, I suggest a new ‘realist’ perspective to account for the reiterative selective dynamics between scientists’ cognition and the external physical world. This perspective assumes that the scientist and Nature are engaged in a sort of evolutionary race, where at each step Nature presents new clues to the scientists. Here, I suggest that the physical world has an active selective role that has often been under-represented. Finally, in the last section, I put forth four methodological steps for the study of scientific advancements within an evolutionary, selective and populationist framework.

The final two chapters are intended to test the evolutionary framework delineated in the previous chapters as it applies to the study of a scientific innovation which can be taken as an example of a scientific replicator. This innovation is a molecular standard for taxonomic identifications called ‘DNA barcoding’. In chapter 4, I apply the four methodological steps delineated at the end of chapter 3, discussing the selective context and wider theoretical implications of the innovation. In particular, I explore the significance of DNA barcoding for the so called ‘species problem’. Chapter 5 investigates the use of text analysis as a quantitative tool that can assist with the elucidation of scientific conceptual

dynamics in order to establish a more robust ground for my evolutionary and selective framework and show how it can yield testable hypotheses.

Chapter 1

Five early evolutionary models of scientific change.

1.1 Premise

Evolutionary epistemology is a variegated research program that has at least two main distinct orientations within it: one is called ‘evolution of epistemological mechanism’ (EEM); the other ‘evolutionary epistemology of theories’ (EET) (Bradie, 1986). The former is more concerned with the evolution of the bio-cognitive architecture and mechanism that allows knowledge acquisition.³ The latter is concerned with a selective account of scientific ideas over historical time and can be more easily associated with Kuhn, Toulmin and Hull’s evolutionary analogies presented in this chapter. However, such a distinction cannot easily accommodate Campbell’s model. As I will show in the last section of this chapter, Campbell’s BVSR model of knowledge acquisition is both concerned with a selective account of the mechanisms and with the selection of ideas.

Nonetheless, with limitations, Bradie’s disciplinary labels capture an important polarity within evolutionary epistemology that mirrors more recent controversies within the human evolutionary behavioural sciences which are further explored in the next chapter. Thus, it is first necessary to provide the historical background about the early debate in evolutionary epistemology. This historical analysis prepares the ground for a more fully extended

³ Ruse (1986, 1995, 2012) has championed this version of evolutionary epistemology. His position is critically evaluated in the next chapter in connection with evolutionary psychology. They both seem to share a fundamental assumption that the study of the biological mechanism has priority over the evolution of ideas.

treatment of what a selectionist account of scientific change might look like today in light of more recent advancements in cultural evolution. This wider aim is attempted in chapter 2 and chapter 3 where I present an abstract characterization of a selection process and I defend the thesis that cultural selection offers (1) a viable model to study the evolution of scientific ideas and (2) sheds new light on some contested disputes in the philosophy of science (e.g., the context distinction, the relevant criteria for theory choice, etc.). Until a more refined notion of selection is introduced (chapter 3), the use of the term ‘selection’ must be taken in a fairly intuitive and ambiguous way. This is exactly because the ambiguity in the use of this term is what characterizes these early evolutionary accounts of knowledge accretion. In this chapter, whenever possible, this ambiguity is made explicit and comparatively evaluated. As such, it is worth emphasizing that I do not take a side with one or the other ‘selective’ account. To a closer look, they all present a number of important limitations that can only be reconciled within a more recent cultural evolutionary framework. Thus, the main goal of this chapter is to highlight points of friction (e.g., the Popper vs Kuhn debate), merits (e.g., Toulmin’s notion of a spectrum of selectors or Campbell’s trial and error view of human cognition) and limitations (e.g. Hull’s disanalogy pertaining to conceptual interactors). My stance on these debates will appear more clear at the end of chapter 3 once I have introduced my evolutionary and selective framework, which I claim, can reconcile a number of incongruences in these early evolutionary models. This first chapter must only be intended as a necessary historical presentation of the early debate that illuminates a number of problems that are still key in evolutionary epistemology.

1.2 The Popper-Kuhn debate in the light of their evolutionary epistemologies.

A quick look at the history of science shows that scientific theories surpass each other. Within the same explanatory domain and with sufficient time, newer theories replace previous theories with lower explanatory capacity. In this respect, the history of science seems to show a progressive character. Lavoisier's oxygen theory made Becher and Stahl's phlogiston theory obsolete, Einstein's theory of special relativity surpassed Newton's mechanics, the Darwinian theory of natural selection superseded Lamarck's theory of evolution based on the inheritance of acquired characteristics. These are but a few examples of many.⁴

Nonetheless, scientific progress, which seems almost an empty platitude, has been very hard to justify in philosophical terms. For example, if past rejected theories have been proven to be false, how can we say that false theories represent steps towards truth? This is the scenario of problems faced by Karl Popper and Thomas Kuhn, two main protagonists of the last century's philosophy of science.

Early in his career, Popper acknowledged that “almost all the problems of traditional epistemology are connected with the problem of the growth of knowledge” (Popper, 1959, p. 19). Kuhn, on his part, sought to understand “how development through revolutions can be compatible with the apparently unique character of scientific progress” (Kuhn, 1970a, p.8). The growth of knowledge as it relates to an epistemology of science urges Kuhn and Popper to justify how rejected (thus false) theories can lead to truer theories and to more accurate

⁴ It is well known that Darwin himself recognized the importance of Lamarckian mechanisms. This fact does not present an objection to the point made here that the theory of natural selection has a broader explanatory capacity for what concerns organic evolution.

representations of 'external reality'. To circumvent this sort of 'epistemic discontinuity' Popper introduces the notion of "truth approximation" (Niiniluoto, 1999, p. 65). Roughly speaking, this notion maintains that false hypotheses have different degrees of approximation to truth. In this perspective, truth is a sort of ultimate goal to which past rejected theories inevitably tend, even if they were, strictly speaking, false. In this way, the potential "fallibilism" of the present state of knowledge is happily reconciled with an epistemic optimism that claims that those theories currently validated must be at least closer to truth than their predecessors. This epistemological stance is coupled with Popper's "critical scientific realism" (Niiniluoto, 1999) that maintains that there is a semantic relation between theories (or language, beliefs, statements) and external reality. Hence, according to Popper, it is rational to believe that science progresses towards truth and, thus, it is rational to think that scientific methods warrant better cognitive representations of reality itself.⁵

On the other hand, Kuhn interprets the epistemic discontinuity of the growth of knowledge in a different way. First, he maintains a rather ambivalent position regarding the applicability of the triad "truth-progress-reality" (TPR) to scientific developments. For example, speaking about the notion of 'truth' he clearly states that "we may have to relinquish the notion, explicit or implicit, that changes of paradigm carry scientists and those who learn from them closer and closer to the truth" (Kuhn, 1970a, p.170). Famously, he advocated the notion of "incommensurability" to stress that any comparison between theories that belong to different times (e.g Aristotelian and Newtonian mechanics) lacks a common measure of

⁵ Niiniluoto (1999) describes his and Popper's *critical scientific realism* as a realist position that has six main theses: (1) Ontological independence (reality is at least partially independent of human minds); (2) Truth is a semantical relation between language and reality; (3) Truth and falsity are applicable to all linguistic products of scientific enquiry; (4) Truth is an essential aim of science; (5) It is possible to approach the truth, and to make a rational assessment of such cognitive progress; (6) It is rational to believe that the self-corrective methods of science are progressive in a cognitive sense (see Niiniluoto, 1999, p.10).

evaluation (Wray, 2011, p. 66).⁶ Kuhn strongly reacts against any attempt to discard out-of-date theories as “unscientific” for he maintains that at each historical step theories have their own “historical integrity” and their internal coherence with the observations, experiments and theoretical terms that can be derived from them. In other words, Kuhn seems to radically question any notion of 'progress towards truth' and with the underestimation of the role of “truth” comes a rejection of a realist position:

A scientific theory is usually felt to be better than its predecessors not only in the sense that it is a better instrument for discovering and solving puzzles but also because it is somehow a better representation of what nature is really like. One often hears that successive theories grow ever closer to, or approximate more and more closely to, the truth. Apparently generalizations like that refer not to the puzzle-solutions and the concrete predictions derived from a theory but rather to its ontology, to the match, that is, between the entities with which the theory populates nature and what is “really there”. Perhaps there is some other way of salvaging the notion of 'truth' for application to whole theories, but this one will not do. There is, I think, no theory-independent way to reconstruct phrases like 'really there': the notion of a match between the ontology of a theory and its “real” counterpart in nature now seems to me illusive in principle (Kuhn, 1970a, p. 206).

However, it will be premature to conclude that with a rejection of a stable criterion of 'truth approximation' and with a rejection of a 'realist' stance Kuhn totally renounces a notion of cognitive progress (see note 5 in this chapter). In fact, he continues:

I do not doubt, for example, that Newton's mechanics improves on Aristotle's and that Einstein's improves on Newton's as instruments for puzzle-solving. But I can see in their succession no coherent direction of ontological development (Kuhn, 1970a, p. 206).

Thus, Kuhn admits some form of cognitive appraisal that allows us to evaluate the improvements of Newton's over Aristotle's mechanics. Nonetheless, he is hesitant to apply

⁶ 'Incommensurability' is one of Kuhn's key notions and, as such, it has exerted many interpretations. For our purpose, the simplified characterization of the term as “lack of a common measure of evaluation” suffices. For a more detailed discussion of three forms of incommensurability in Kuhn's lexicon as “topic incommensurability, meaning incommensurability and dissociation” see Wray, 2011, Chap. 4.

the triad TPR to the growth of knowledge, at least until we have explained how new theories attach to reality more than their predecessors. In his words:

Scientific theories, it must be remembered, attach to nature only here and there. Are the interstices between those points of attachment perhaps now larger and more numerous than ever before? Until we can answer more questions like these, we shall not know quite what scientific progress is and cannot therefore quite hope to explain it. On the other hand, answers to those questions will very nearly provide the explanation sought... Already it should be clear that the explanation must, in the final analysis, be psychological or sociological. It must, that is, be a description of a value system, an ideology, together with an analysis of the institutions through which that system is transmitted and enforced (Kuhn, 1970b, pp. 20-21).

Thus far, I have traced a first schematic distinction between Popper and Kuhn concerning the role of the triad TPR. On Popper's side, the growth of knowledge fully corresponds with TPR. On Kuhn's side, the triad is fragmented by a partial concession of a form of cognitive progress devoid of “truth approximation” and, even more importantly, not directly linked to a realist 'ontological' stance. At this point, the question is: how do these two different interpretations of the role of TPR in science relate to Popper and Kuhn's evolutionary analogies of science?

To begin with, Popper's interest in the analogy between organic evolution and the evolution of scientific ideas spanned throughout his philosophical career. Early on, he suggested that scientific theories have their own “fitness” due to the “fiercest struggle for survival” with other competing theories (Popper, 1959, p. 42). Later, he formulated a more mature selectionist account wherein “the growth of our knowledge is the result of a process closely resembling what Darwin called 'natural selection'; that is, the natural selection of hypotheses” (Popper, 1979, p. 261). Popper persistently presented his philosophy of science as a selectionist model of knowledge accretion. Similarly, Kuhn has repeatedly made use of evolutionary and selective analogies. One of the most explicit invocations of selectionism is contained in the final chapter of his *Structure*, where he claims that changes of paradigm

during periods of revolutionary science are due to “the selection by conflict within the scientific community of the fittest way to practice future science” (Kuhn, 1970a, p. 172).

Hence, almost paradoxically, all their disagreements converge on a selectionist view of scientific growth. They both endorse the idea that those theories that succeed do so because they express a relatively higher “fitness” towards competitors or predecessors. However, when it comes to defining scientific beliefs’ selective factors’, Popper and Kuhn are again in sharp disagreement.

Famously, Popper has stressed the importance of logical and ‘rational’ criteria of validation, while Kuhn has emphasized the insufficiency of such criteria and the necessity to appeal to the psychology of research.⁷ Popper thinks that the logical analysis of scientific knowledge has a preeminent role with problems of justification and validation of scientific statements; thus, it must have a role in the weeding out of erroneous hypotheses. Thus, according to Popper theories are advanced as deductive conjectures which are later scrutinized and selected through a logical process that he calls ‘falsification’. Popper delineates a scenario where scientists are constantly engaged in an effort to find experimental and observational evidence that can undermine the foundations of such conjectures. What is important to highlight here is that Popper sees “falsification” as a logical selective device. Logic, and particularly deductive logic through *modus tollens*, is presented as a selector in

⁷ The “Popper-Kuhn debate”, as it is presented in *Criticism and the Growth of Knowledge*, is mostly polarized around the role of logical vs psycho-sociological factors. Emblematic here is their different interpretation of the role of the super-empirical virtues (e.g., accuracy, simplicity, comprehensiveness, consistency etc.). Popper and the Popperians see these criteria as rational selectors that operate in theory choice, while Kuhn notes that the applicability of these criteria is not unanimous and unambiguous between different scientists. Thus, it cannot be said that all scientists abide by the same decisions or selective criteria in the same way. I shall return to this issue in the next chapter. For a discussion of the “super-empirical virtues” in Kuhn see Kuhn, 1977, Ch. 13. For Popper’s discussion of ‘simplicity’ see Popper, 1959, Ch. 7.

the growth of knowledge. I shall return later to this issue.⁸

Kuhn, on the other hand, concedes that logic is a fundamental component of science and he is also sympathetic with Popper's deductivist emphasis.⁹ Nonetheless, Kuhn offers a very different account of what acts as a 'selector' in theory choice. Kuhn posits that the psychology and sociology of research also matter. He argues that "idiosyncratic factors dependent on individual biography and personality" (Kuhn, 1977, p. 329) play an important role in theory choice. These components largely surpass the logical analysis of knowledge. Epistemic warrant itself comes from "nurture and training in the psychological make-up of the licensed membership of a *scientific group*" (Kuhn, 1970b, p. 22, emphasis in original). A novice scientist becomes familiar with exemplars, learns to think and problematize within the theoretical framework that he/she has learned within their community. These factors act as "rhetorically induced and professionally shared imperatives" (Kuhn, 1970b, p.22) and are part of the selective factors of theory choice. In other words, their selective emphases are polarized, on one side, the internal criteria for rational validation (Popper's), on the other, the more 'opaque' and external factors pertaining to the psychology and sociology of research (Kuhn's).

Furthermore, Popper and Kuhn have different interpretations of Darwinism in its application to the progress of scientific ideas. While Popper is inclined to see science as a

⁸ Further in this chapter, I go back to discuss Popper's contribution in the light of Campbell's selectionist model. If we read Popper from the point of view of Campbell's model, it appears clear that falsificationism is part of a wider psychological scenario than the one assumed by a strict logical point of view. What is at stake in Campbell's recognition of the importance of falsificationism is a trial and error component of human reasoning. Logic and particularly falsificationism is thus subsumed, under a wider psychological scenario.

⁹ As Kuhn has stated, "...neither Sir Karl nor I is an inductivist. We do not believe that there are rules for inducing correct theories from facts, or even that theories, correct or incorrect, are induced at all. Instead we view them as imaginative posits, invented in one piece for application to nature" [emphasis added] (Kuhn, 1970b, p.22).

selective process that culminates in better representations of external reality and truer theories, Kuhn interprets the selection of science as a non-teleological process. These are two very different ways to interpret the applicability of Darwinian principles to the evolution of scientific ideas. Wray (2011) has recently noted that Kuhn's peculiar epistemology of science cannot be understood if not from his interpretation of the theory of natural selection as a non-teleological process. Biological adaptations are the product of selection but they are not the result of a preordained or clairvoyant process that establishes an aim towards which change must approximate. By the same token, Kuhn thinks that conceptual evolution does not have a fixed-in-advance goal to which the growth of knowledge is directed. Science, instead of being thought as moving towards truth, is rather seen as “pushed from behind” to explore new adaptive niches. Kuhn looks at scientific specialization as analogous to the process of speciation (see Wray, 2011, chap. 7). Kuhn uses the metaphor of the evolutionary tree to state that if we isolate two different theories in a phylogenesis of theories, we find that each theoretical-stage is perfectly 'adapted' to its environment (Kuhn, 1970a, p. 205). This is a peculiar form of 'progressionism' very different from Popper's 'directed' and 'teleological' evolutionary analogy of knowledge. Kuhn states that “later scientific theories are better than earlier ones for solving puzzles in the often quite different environments to which they are applied. That is not a relativist's position, and it displays the sense in which I am a convinced believer in scientific progress” (Kuhn, 1970a, p.206). The emphasis here is on the '*quite different environments*': it means that new theories are more similar to new species adapted to new niches. Older or extinct species are no less 'fit' to respond to their different adaptive pressures and so are theories.

To sum up, I presented the main elements of the “Popper-Kuhn” debate as they relate

to their rather vague evolutionary epistemologies (EE). Between the several points of disagreement, Popper and Kuhn share the faith that science presents some selective devices that operate in theory choice. An enumeration of these devices counts: logic, the super-empirical virtues, epistemic values and norms, such as truth approximation, and broader psychological and sociological aspects. The reference to evolutionary theory is a key element of both Popper and Kuhn's epistemology of science. However, since their stances towards TPR are different, their interpretations of Darwinism and the role of selection are also very different. While Popper sees the evolution and selection of scientific theories as directed to truth and a better representation of 'reality', Kuhn interprets the analogy between scientific and Darwinian evolution as a non-teleological process *par excellence*. Throughout this work I will constantly return to Popper and Kuhn's formulations and philosophical divergences trying to reconcile their points of disagreement within a more refined cultural evolutionary epistemology.

1.3 Toulmin and Hull's social evolutionary epistemologies.

In this section, I want to examine Toulmin and Hull's evolutionary epistemologies. I will particularly focus my attention on their selective accounts. I argue that these accounts lack epistemological focus because they put a greater emphasis on the social evolution of science, and thus largely overlook the role of selective components that belong to the conceptual variants (e.g., scientific theories, concepts and methods that, at some point in time, are preferentially adopted than others).

Starting with Toulmin, it must be emphasized that he is one of the most prominent figures of the 1960s' *historical turn* in the philosophy of science (Callebaut, 1993, p. 11). Toulmin fully absorbed Kuhn's lesson that the history of science needs to have a preeminent role in epistemology. Moreover, Toulmin sees Darwinian thinking as an historical paradigm itself. According to him, both conceptual evolution and biological evolution are subsumable under Darwinian theory as a 'general form of historical explanation' (Toulmin, 1972, p.135). Toulmin further elaborates and enriches Kuhn and Popper's germinal and rather vague evolutionary analogies by adding two main contributions : (1) a "populationist' notion of conceptual evolution and (2) a reorganization of Popper and Kuhn's selective accounts on a spectrum of selectors. These selectors range from more 'internal' to more 'external' components, from logic to sociology. In a certain sense, Toulmin's model unifies the fairly antithetical views of Popper and Kuhn positing that logical (or other 'internal' factors, such as the super-empirical virtues) and sociological factors must be integrated in a unified evolutionary framework.

Populationism (Mayr, 1959), as it applies to scientific ideas, sees science as a 'pool' of conceptual variants that exhibit fluctuations due to different selective pressures.¹⁰ When compared to each other, two historical times, like "successive time-slices" or "cross-sections", show the populational change that has occurred from one step to the other:

Moving from one historical cross-section to the next, the actual ideas transmitted display neither a complete breach at any point – the idea of absolute "scientific revolutions" involves an over-simplification – nor perfect replication, either. The change from one cross section to the next is an evolutionary one in this sense too: that later intellectual cross-sections of a tradition reproduce the content of their immediate predecessors, as modified by those particular intellectual novelties which

¹⁰ In the next chapter, I will return to the notion of "population thinking" as it relates to cultural evolution and science. For a more extended treatment of populationism in cultural evolution see (Claidière et al., 2014). It is also worth mentioning that Mayr did not personally endorsed the application of population thinking in the context of cultural evolution. Thanks to Liane Gabora for this point.

were selected out in the meanwhile – in the light of the professional standards of the science of the time (Toulmin, 1967, p. 466).

As I have previously discussed, Toulmin arranges these “professional standards” in two main categories: “internal” and “external” (Toulmin, 1967, p. 460). 'Internal' refers to “intrinsic considerations that are genuinely relevant to the current problems of a science”. As such, they include logical methods of analysis, the super-empirical virtues and questions concerning the objectivity of science. “External” refers to “extrinsic considerations (...) that can distort or frustrate the normal procedures of disciplinary judgement” (Toulmin, 1972, p.253). These include styles of thought, metaphysical taste, broader political and socio economic factors pertaining to the individual scientist and his social circles. According to Toulmin there is no “sharp contrast between the “internal” and “external” aspects of science. The philosophical analysis of scientific growth “should be replaced, rather, by a *spectrum* of questions – ranging from those which involve almost exclusively internal considerations, to those which are concerned predominantly with external (socio-historical, political, or economic) factors” (Toulmin, 1967, p. 460). The two components are dynamically related: to isolate one element as more relevant would bring an over-simplification of the evolutionary and selective process, it would be like studying “the ontogeny or morphogenesis of a science in isolation from its ecological environment” (Toulmin, 1967, p. 458). This means that, for Toulmin, both components are part of the selective process of conceptual variants in such a way that they cannot be separated. As he clearly stated:

In demonstrating the established place of any new concept in a scientific discipline, for instance, we must now pay attention to the selection-procedures actually used in evaluating the intellectual merits of each new concept, and these procedures must themselves be related to the activities of the men who form, for the time being, the authoritative 'reference-group' of the profession concerned (Toulmin, 1972, p.143).

For Toulmin, internal/intellectual epistemic standards, either exerted by logical analysis or by other standards of rational validation, cannot claim total autonomy from broader sociological factors. Rather, the two must be understood as complementary aspects of the same 'populational' process.

Science develops (we have said) as the outcome of a double process: at each stage, a pool of competing intellectual variants is in circulation, and in each generation a selection process is going on, by which certain of these variants are accepted and incorporated into the science concerned, to be passed on to the next generation of workers as integral elements of the tradition (Toulmin, 1967. p. 465).

Hull follows in Toulmin's steps, delineating what probably is, up to this day, the most comprehensive and detailed evolutionary account of science. Hull has three major achievements to his credit in this regard. First, he elaborates a refined theoretical evolutionary model (involving a distinction between 'interactors' and 'replicators'). Second, in his influential *Science as a Process* (1988) he combines a range of methodologies from the social sciences to empirically test his formal framework. This framework applies to the study of two communities of taxonomists and their competition to affirm their paradigms. A third accomplishment is an extended discussion of some key issues pertaining to the analogy and disanalogies between organic and conceptual evolution.¹¹ Given the many theoretical implications of his work, a comprehensive survey is not possible. Instead, I want to limit my analysis to his main theoretical contribution: his formal evolutionary model comprising the distinction between 'interactors' and 'replicators'. This crucial point, I claim, has received scarce attention in the critical literature devoted to cultural evolution, while it has been very

¹¹ An analysis of this important contribution goes beyond the scope of this work. For Hull's discussion of crucial topics such as (1) the Lamarckian vs Darwinian character of cultural evolution, (2) the apparent disanalogy between cultural and organic mutation (one random the other directional and intentional), (3) the notion of progress in organic and cultural change, See Hull, 1988, Ch. 12. In chapter 2 and 3, I will go back to discuss these issues defending a selectionist approach.

influential in the context of organic selection (see Brandon, 1990).

Hull identifies three main hierarchical levels that operate in the selective process: replicators, interactors and lineages. Particularly, he draws a distinction between 'replicators' and 'interactors', intending to clarify what he sees as an ongoing misunderstanding between two factions of evolutionary biologists divided around the problem of the “unit of selection”. The term ‘replicator’, introduced by Dawkins (1976), is “used to refer to any entities of which copies are made”; Hull’s interactors are “entities which interact, as cohesive wholes, directly with their environment in such a way that replication is differential” (Lloyd, 2012). For example, genes for swiftness of deer are replicators while the individual deer is an interactor, a herd of deer is another interactor. The differential extinction and/or proliferation of these interactors with their relation with a certain environment leads to a differential reproduction of the replicators.

In order to explain the meaning and usefulness of Hull’s replicator/interactor distinction, it is first necessary to introduce the main element of disagreement pertaining to the “unit of selection’ debate.¹² On one side, Dawkins (1976), who builds upon the work of Williams (1966), and especially Hamilton (1964), is seen as the defendant of a gene centered view of selection. This position has stressed that evolution can be better understood from the “gene's eye” perspective: the replicator's “egoistic” point of view. The “*Cui bono?*” question - which asks who benefits from a process of evolution by natural selection - is fully answered when we put the gene as the main target of selection (Dennett, 1995). This has a number of

¹² The ‘units of selection’ debate is a rather stratified dispute involving several key questions and levels of analysis. Here, the debate is simplified and reduced to its minimum terms. Dawkins’ “gene’s eye view’, especially when presented as the only possible selective perspective, has also received a number of important objections. More cautiously, philosophers such as Kim Sterelny and Philip Kitcher have advanced a “pluralistic” perspective that assumes that the selective analysis can be conceived from different and equally valid perspectives. For a more extended treatment on the ‘units of selection’ debate, see Lloyd, 2012.

important implications. As Lloyd very well explains:

Dawkins adds the requirement of agency to the notion of beneficiary (see Hampe and Morgan 1988). For Dawkins, a beneficiary, by definition, does not simply passively accrue credit in the long term; it must function as the initiator of a causal pathway. Under this definition, *the replicator is causally responsible for all of the various effects* [emphasis added] that arise further down the biochemical or phenotypic pathway, irrespective of which entities might reap the long-term rewards. (Lloyd, 2012)

Thus, according to this view, genes are the relevant units of selection, not individuals, nor groups and certainly not larger entities such as species. Genes are the units that compete in order to replicate more copies of themselves, thus selective forces must be seen as acting on genes. According to this perspective, individuals or groups are just “vehicles” and carriers of the relevant units of selection. Phenotypic adaptive properties, no matter how 'extended' must be understood as the result of selection acting upon genes (Dawkins, 1976; Dawkins, 1982; Dennett, 1995). This approach contrasts with the early group-selectionist view, first proposed by Wynne-Edwards (1962) and today mutated into a more refined paradigm called “multilevel selectionism” (D.S. Wilson and Wilson, 2007; Wilson, 2012). Roughly speaking, group selectionists maintain that group level phenotypic properties can be targeted by selective forces. Differential proliferation or extinction of groups might lead to adaptive change at the group's level and also at other interactive levels (e.g. individual organisms). Within this fierce debate that is still very much alive in evolutionary biology,¹³ Hull seeks to introduce his conceptual clarification pertaining to a distinction between units of replication (replicators) and units of selection (interactors). In his words:

When gene selectionists say that genes are the primary units of selection, they mean that genes are the primary units of replication. They do not mean to assert that they are the only or even primary units of interaction (Hull, 2001, pp. 23-24).

¹³ A clear and comprehensive summary of the more recent debate on multilevel selectionism is offered by Wilson, 2012, chapter 4.

Hull's maintains that while Dawkins is right that genes are the relevant units of replication, it is also true that genes build up 'interactors' intended as higher levels of functional organization (e.g., individual organisms, groups of individual organisms, species etc.) that bias the replication of their more fundamental units: the genetic replicators. In Hull's words:

Interactors are those entities that interact as cohesive wholes with their environments in such a way as to make replication differential. Thus, selection can be characterized generally as any process in which differential extinction and proliferation of interactors causes the differential perpetuation of the replicators that produced them (Hull, 2001, p. 22).

Since Hull characterizes interactors as being involved in a process of 'differential extinction and proliferation', it can be concluded that his solution is a form of 'multilevel' selectionism.

Thus, it is not clear whether the main target of the selective process are genes or rather higher functional entities.

However, and this is a crucial point, when the distinction between replicators and interactors is transposed from organic to conceptual evolution, Hull surprisingly marks a disanalogy between the two evolutionary processes: concepts are not allowed to function as interactors (only as replicators) while genes are both interactors and replicators.

In other words, he allows genes to function as replicators and interactors, while conceptual replicators are just replicators whose interactors are scientist's minds and their social circles.

According to him, genes, given their functional complexity, are both replicators and interactors; they are "entities that interact with their environments in such a way as to bias their own replication" (Hull, 2001, p. 27). On the contrary, concepts are passive entities that require scientists to function as interactors. The scientist's brain is the lower level interactor that Hull recognizes:

In biological evolution, variable chunks of genetic material are the primary replicators ... They also function as interactors in the production of more inclusive interactors. Conceptual replicators cannot interact directly with that portion of the natural world to which they ostensibly refer. Instead they interact only indirectly through scientists. ... Scientists are ... the primary interactors in the conceptual development of science (Hull, 1988, p. 434).

However, Hull's account presents an incongruence on this point. To begin with, he offers a rather vague characterization of what a scientific conceptual replicator is. He describes scientific replicators as “elements of the substantive content of science – beliefs about the goals of science, proper ways to go about realizing these goals, problems and their possible solutions, modes of representation, accumulated data, and so on” (Hull, 1988, p.434). Secondly, he does not offer an explanation of how these variegated elements relate to each other. In fact, it seems more plausible that concepts directly interact with each other to form higher level interactors, for example, when standards, methods, theories, data sets, observations and so on build up a scientific theory, or a wider scientific paradigm. It is better to see these variegated elements as forming a net of replicators that interact with each other to create higher and progressively inclusive entities. This perspective presents an objection to Hull’s idea that concepts cannot build up interactors. Obviously, concepts need scientists to exist and interact; but this doesn’t mean that the concepts cannot be analyzed as causally interacting with each other and other things. For example, when historical linguists study how languages change over time, the ontological dependence of language on speakers doesn’t stop them from doing a causal analysis of how linguistic components and replicators interact, without mentioning speakers. These linguistic ‘replicators’ are not taken in their mentalistic, cognitive or neuro-physical structure but rather as entities that interact in the ‘public’ sphere.¹⁴ The same applies to the scientific ‘conceptual replicators’ that are the main

¹⁴ Thanks to Dan Ryder who drew my attention to this point.

subject of interest of this work. I shall return to this point in the next chapter.

If this intuition is true, conceptual replicators, instead of being thought of as passive entities which “cannot interact *directly* with that portion of the natural world to which they ostensibly refer” (emphasis added, Hull, 1988, p. 434) can be taken as active replicators and interactors themselves. As I will discuss in the next chapter, this view seems to agree with the cultural evolutionists' emphasis on the existence of cultural units of replication that is the task of the cultural evolutionist to follow on their populational dynamics.

This view urges us to assume the point of view of the conceptual replicator. As the gene's eye perspective discussed above implies in biological selection an emphasis on the gene as the unit of selection, by the same token, cultural evolution should assume the point of view of the conceptual replicator as the relevant unit of selection.¹⁵ I call this perspective a “concept's eye view” of conceptual evolution and I argue that Hull's emphasis on the social selective dynamics of science seem to miss the importance of the replicator's point of view.¹⁶

This limitation is linked to Hull's assumption that the individual scientist is the lowest interactive and selective level of science in agreement with his peculiar form of multilevel selectionism. From this, he derives a sociological evolutionary analysis of science that is important and valuable to represent the ‘external’ sociological selective factors of science but it is rather weak to account for the ‘internal’ factors. He is correct in pointing out that scientists form “conceptual demes” that have higher levels of “intrademic positive citation” (Hull, 1988, p. 435); that scientists have “conceptual kin”: those scientists with whom they

¹⁵ Again, I want to express cautiousness in endorsing a replicators' eye view given the contested nature of the debate. I am not assuming that this view is the only view or even the best one for examining the units of selection debate. Rather, I endorse a ‘pluralist’ view (see note 11) where different levels of analysis can address the selective process, and I commit to the more limited claim that the replicator's point of view has a number of explanatory merits in the context of the evolutionary analysis of science.

¹⁶ For an interesting early memetic characterization, see Gabora (1996).

share ideas that are identical by descent and that this leads to cooperation according to a process very similar to 'kin selection' (Mesoudi, 2011).

Some have also praised Hull for correcting the rather sociologically naive selectionism endorsed by Popper (Blute, 2013) with the integration of population thinking in the evolutionary analysis of science and a multilevel selective scenario where social groups play a preponderant role in the evolutionary process. However, as sophisticated and valuable as his account might be, Hull's model suffers the same problems encountered with Kuhn and Toulmin. There is an underestimation of the role of some sort of intrinsic cogency or saliency that ultimately refers to the positively selected conceptual variants themselves; the higher 'adaptedness' of certain variants compared to others, such as Aristotelian and Newtonian mechanics, that makes some conceptual or theoretical variants more fit than others. Popper, with his notion of truth likeness and his critical realism was better able to make sense of these intrinsic selective components.

This is not to say that Hull does not recognize that 'internal' components that belong to the replicators have a role. In this respect, for example, Hull considers testing to be of primary importance in science and talks of descriptive statements as conceptual tokens that are binary-selected: they survive and they are incorporated or they fail and are selected out (Hull, 2001, p. 39). However, this selection seems more applicable to descriptive statements alone and not to scientific theories, theoretical statements or conceptual terms. Rather, Hull acknowledges that conceptual systems are nearly impossible to operationalize because they contain a great number of observational consequences, while, "a particular experiment or observation bears on only one small part of the meaning of the theoretical claim" (Hull, 2001, p. 39). Theories are complex conceptual entities and "only one small aspect of a

scientific theory can be tested in a particular experimental setup, and the results can always be accommodated in a host of ways” (Hull, 2001, p.40). However, as I will defend throughout this work, such scenarios can be easily accommodated within a concept's eye perspective that assumes that theories themselves are larger interactors comprising a number of different replicators.

1.4 Universal selectionism and selective realism in Campbell.

The vast majority of Campbell's commentators have stressed that his greatest contribution to an evolutionary account of knowledge lies in a 'generalization' of the principle of natural selection far beyond the realm of organic evolution. Thus, Toulmin praises Campbell's efforts as “the most methodical and fully-worked-out attack on the problem of generalizing the Darwinian notions of variation and selection” (Toulmin, 1972, p. 336, note 2). Durham (1991) goes so far as to talk of a “Campbell's rule” according to which the analogy between organic change and cultural change “is not from organic evolution *per se*, but rather from a general model of evolutionary change for which organic evolution is but one instance” (Blackmore, 1999, p.17). However, as Cziko (1998) notes, Campbell's work has often been misunderstood and the scope and applicability of his terminological and conceptual innovations neglected.

Three elements are key to understanding Campbell's selectionist theory and its importance for an epistemology of science: (1) the isomorphism between knowledge and selection, (2) the organization of knowledge processes on a hierarchy of 'vicarious' selectors

(3) the 'meaning', scope and applicability of the attribute 'blind' in his 'blind variation and selective retention' (BVSR) model.

Starting from point one, it must be noted that Campbell sees knowledge and selection as one and the same process. Knowledge, at every stage, is for Campbell a selective process and conversely, every selective process (organic or cultural) is knowledge. Following in Lorenz's (1962) steps, Campbell generalizes the notion of 'knowledge' to include every adaptive interaction of an organism with its environment. With this generalization comes the formulation of what he describes as a “dogma” containing three propositions:

- a. A blind-variation-and-selective retention process is fundamental to all inductive achievements, to all genuine increases in knowledge, to all increases in fit of system to environment.
- b. The many processes which shortcut a more full blind-variation-and-selective-retention process are in themselves inductive achievements, containing wisdom about the environment achieved originally by blind variation and selective retention.
- c. In addition, such shortcut processes contain in their own operation a blind-variation-and-selective-retention process at some level, substituting for overt locomotor exploration or the life-and-death winnowing of organic evolution (Campbell, 1960, p. 91).

This 'dogma' maintains that from genetic adaptation (the first step of his selectionist hierarchy of knowledge processes) up to our most sophisticated products of Science (see table 1.1) a consecutive series of BVSR trials are solely responsible for any organism's adaptive properties.

However, and this brings me to the notion of ‘vicariousness’, from genetic adaptation up to science, the mechanisms responsible for knowledge acquisition grow in hierarchical complexity. At each level, new features are acquired that enable the organism to 'shortcut' the lower levels adding an element of 'vicariousness'. For example, if we think of the locomotion of a bacterium we observe that it resorts to non-mnemonic problem solving. A bacterium seeking a food source in the environment would start a random change in locomotor direction (*blind variation* phase); if an impenetrable obstacle is found (*selective retention* phase) the

bacterium will resort again to blind variation choosing another random direction.¹⁷

Table 1.1. Campbell's hierarchy of selectors. Adapted from Cziko, 1995, p.144.

10	Science
9	Cultural cumulation
8	Language (overlapping 6 and 7)
7	Socially vicarious exploration; Observational learning and imitation
6	Mnemonically supported thought
5	Habit
4	Instinct
3	Vicarious locomotor devices
2	Non-mnemonic problem solving
1	Genetic adaptation

The interaction of the bacterium with its environment qualifies as 'knowledge' because the organism has "learned" that "the environment is discontinuous and there are penetrable and impenetrable regions" (Campbell, 1974, p.58). The bacterium uses collision to direct its locomotion but other organisms, in place of collision, have developed 'vicarious' systems. Vision is a vicarious locomotor too because it represents a sort of shortcut that substitutes for more basic 'locomotive epistemologies' such as those that require direct collision.¹⁸

However, when vision goes beyond the range for which it was designed (e.g., in the dark), it has no choice but to 'resort' to blind variation and selective retention again:

¹⁷ What is 'retained' here is the information concerning the surrounding environment. In other words, the system emits a 'blind' output and it receives some form of feedback from the environment to process and 'retain'.

¹⁸ Campbell offers the example of the echolocation devices of bats and porpoises and from them to 'vision' (See Campbell, 1974, p. 59).

Thus the increase of vicariousness in going up the hierarchical ladder means that the higher-level vicarious selectors presuppose, although presumptively, the validity of the already evolved wisdom at the higher, more fundamental level. This, in turn, means that, beyond the range of the problems for which the constrained processes are adequate, one has no choice but to 'blindly' explore the unknown portion of the world (Kim, 2001, p. 105).

Thus, (and this brings me to the other key notion of 'blind variation') at each vicarious step in the hierarchy of selectors the 'blind' trial component might appear less and less present. However, and this is crucial, Campbell thinks that a BV component is always present even at the most sophisticated levels of the hierarchy (no matter how counterintuitive this might seem). The blind component, present in step 1 'genetic adaptation' is simply shifted to higher levels and 'vicariously' incorporated within a new selector. This is probably why Campbell's notion of 'blind variation' has attracted the most criticism. The more we go up in the ladder of selectors the more 'sophisticated' the mechanisms appear. Memory, language, cultural accumulation and science exhibit a form of intentionality and guidance that seems to make the 'blind' component unnecessary or superseded at some point in the evolutionary time scale. For Campbell, this is just an illusion because the blind variation component is still very much present every time organisms venture in the unknown.

Campbell's critics have largely misunderstood the applicability of the notion of 'blind variation' as it applies in his BVS model. Critics point out that scientists' trials cannot be 'blind' because scientists seem to direct their efforts to solve precise scientific puzzles. Thus, their innovations and solutions must necessarily be correlated with the problems that they are trying to solve. This is why scientists' efforts are not 'blind' in any reasonable sense. On the contrary, science seems the place where "clairvoyance" and "guidance" are *par excellence* exercised. Moreover, critics argue, this manifest 'guidance' marks a disanalogy with organic

evolution, which is universally recognized as a not directed process.¹⁹

However, Campbell does not deny the existence of 'clairvoyance' or 'guidance' in cultural or scientific innovation. Rather, he remarks that whenever some processes of knowledge acquisition seem to express directedness we have to question how this prior guiding knowledge has been achieved. His solution to this adaptationist enigma loops back to point b of his dogma presented above: that every directionality or intentionality is itself an inductive achievement containing wisdom from the environment that has been gained through BVSr. In other words, even those cognitive faculties that allow 'directionality' or 'guidance' are the product of a sequence of selective acts without clear cut discontinuity. The "insightful in advance" cognitive operations, Campbell claims, might just give an impression that blindness is removed from the process once and for all. However, every time our cognition ventures into the unknown, it resorts (at least to a certain extent) to some form of blind trials. This happens, for example, when scientists are formulating new hypotheses (BV component) intended as predictions with a degree of probability of being correct. The degree of probability itself testifies to the degree of uncertainty. Eventually, the correctness of the hypotheses is estimated on some form of evidentiary ground that provides validation (SR component).

It has also been noted that the critique that scientific posits are not 'blind' largely rests on the ambiguity of this term (Simonton, 2012). First, 'blind' does not mean 'random'. Not even Campbell had ever thought that the generation of novelties was 'random'.²⁰ The term

¹⁹ In chapter 3 I will return to this disputed issue from the point of view of an evolutionary theory of scientific creativity.

²⁰ Campbell's trial and error view includes directionality. He stresses "the very frequent tactical advantage of a trial and error of general strategic principles over a trial and error involving no classificatory effort nor attempt to use clues". What he is after is the discovery of general heuristics that can be included in a hierarchized trial and error process that includes partial sightedness and partial directionality. Thus, he

‘blind’ as an attribute for variation simply means not-fully-sighted. In this regard, Simonton (2012) has argued that creative attempts vary on a scale with different degrees of blindness and sightedness (Simonton, 2012). For example, a totally sighted creative trial is one where the innovator is 100% confident (highest probability of solution) that the innovation he advances will be successful and the creative output is indeed 100% successful and useful (highest utility).²¹ In all other cases, we would always have limited sightedness. Inevitably, some trials will fail to meet the expectations and will be rejected. This might confirm Campbell’s fundamental intuition that an element of non-sightedness is often associated with creative and particularly, scientific efforts. If it is true that scientists aim to achieve results, that does not mean that they know in advance the results of their attempts. This would imply total clairvoyance on their part, a completely deterministic scenario where solutions are fully known in advance. Furthermore, a trial and error view is often associated with scientists’ self-descriptions of the nature of their work. As Agassi put it, “we must invent many hypotheses because only a few of them survive the test, and these are the ones that matter, the hard core around which research develops” (as cited by Campbell, 1974, p. 71).

Science is at the top of the hierarchy of selectors and its refined methods have increased the reliability of our beliefs and descriptions of the external world. However, according to Campbell, the fit that 'scientific' beliefs seem to exhibit must be investigated as

concludes that “we are in general justified in expecting solutions to be non-randomly distributed, and to show significant contingencies with prior clues” (Campbell, 1960).

²¹ Simonton (2012) has offered a formalized model of creativity according to which for any tentative creative solution K_n there are two parameters: 1) the 'probability of solution' p_i (where $0 \leq p_i \leq 1$) that defines the innovator’s subjective expectation that a given solution will solve the problem; and 2) the 'solution's utility' u_i which defines how well the solution really solves the problem once implemented. Thus, “a solution’s creativity is defined as the ‘improbability’ (i.e. originality) multiplied by its usefulness” (Simonton, 2012, p. 53). Rather than a ‘blind vs sighted’ dichotomy, Simonton talks of a blind-sighted continuum.

‘epistemologically puzzling’. How can the referent, thus the external reality, participate in the selection of our beliefs and representations of it? Campbell thinks that the only viable explanation is to admit some form of BVSR iterations that are present at many different levels. For example, Campbell (following Popper) thinks that another important BVSR device that operates in science is logic itself. Logical reasoning, either being of the inductive, abductive or deductive type, accommodates some sort of BVSR process within itself.²² In particular, Popper’s selectionist characterization of the process of falsification (based on deductive logic through *modus tollens*) represents, in Campbell’s view, another trial and error component of logical and scientific reasoning.²³

To begin with, Campbell remarks that “Popper is unusual among modern epistemologists in taking Hume’s criticism of induction seriously” (...). Campbell presents Hume’s problem of induction as follows:

The “scandal of induction” can be expressed by noting that science makes use of an invalid logical argument, making the error of the “undistributed middle term”, or of “affirming the consequent.” But while invalid the argument is not *useless*. The logical argument of science has this form: If Newton's theory A is true, then it should be observed that the tides have period B, the path of Mars shape C, the trajectory of a cannonball form D. Observation confirms B, C, & D (as judged by the scientific consensus of the day...). Therefore, Newton's theory A is “true.” The invalidity comes from the existence of (...) other possible explanations for B, C, and D being observed. (Campbell, 1990, pp. 4-5)

Thus, Campbell, following Popper, continues:

But the syllogism is not useless. If observations inconsistent with B, C, and D are found, these impugn the truth of Newton's theory A (logically, if not always in scientific practice). The argument is thus highly relevant to a winnowing process, in which predictions and observations serve to weed out the more inadequate theories. Furthermore, if the predictions

²² The trial and error character of inductive and abductive logic is so manifest that does not require further investigations here. Campbell’s (and Hume’s) issue was with what he calls the ‘inductive incompleteness’ of the generalizations that are contained in deductive arguments.

²³ It must be emphasized that Popper expresses enthusiastic support for Campbell's work professing “almost complete agreement, down even to minute details, between Campbell's views and my own' (Popper, 1974, p. 1059)

are confirmed, the theory remains one of the possibly-true explanations. There is an important asymmetry between logically valid rejection and logically inconclusive confirmation.²⁴ (Campbell, 1990, pp. 4-5)

It is this asymmetry between *modus tollens* and *modus ponens* that, according to Popper, solved Hume's problem of induction (see Popper, 1972, ch.1). In his words:

My proposal is based upon an *asymmetry* between verifiability and falsifiability; an asymmetry which results from the logical form of universal statements. For these are never derivable from singular statements, but can be contradicted by singular statements. Consequently, it is possible by means of purely deductive inferences (with the help of the *modus tollens* of classical logic) to argue from the truth of singular statements to the falsity of universal statements. (Popper, 1959, p. 41)

Here, it is important to understand how a falsificationist view accommodates a BVS mechanism. It seems true that Hume did not envision an asymmetry between *modus tollens* and *modus ponens* which is Popper's main contribution. It is also true that it is possible by means of purely deductive inferences to argue from the truth of a singular statement to the falsity of universal statements. Wherever this happens, this could be a paradigmatic example of a trial and error selective process. Thus, it must be acknowledged that falsificationism presents a way to consider whether a selectionist model applies not just for inductive or abductive inferences (where the trial and error component seems more evidently present) but also for the deductive ones. All that emerges from this selectionist interpretation of logical reasoning is that all forms of logical procedures could be connected to a trial and error component of human cognition.

²⁴ It must be emphasized that *modus ponens* does not provide a basis for 'confirmation theory' which defines plausible models of non-deductive reasoning (Crupi, 2016). This means that Popper's asymmetry is rather between *modus tollens*/ falsification and inductive evidential support. The former is conclusive as much as logical validity is concerned the second is constitutively inconclusive. Thanks to Holger Andreas who drew my attention to this point.

1.5 Summary

In this chapter, I have presented five early evolutionary accounts of knowledge accretion. Each model puts a different emphasis on the relevant selective factors of scientific ideas and interprets various ways to apply a Darwinian framework. One lesson that can be learned from the analysis of these early evolutionary accounts of science is that Darwinian analogies can potentially have a synthetic capacity to accommodate polarized disputes. The fact that Popper and Kuhn converged on a Darwinian analogy notwithstanding the great deal of disagreement requires further attention from those interested to understand not just the nature of this important debate (Wray, 2011) but also a way to reconcile it.

In the next chapters, a number of issues presented in this chapter will be re-evaluated in light of more recent advancements in cultural evolution. Some instances of the ‘Popper vs Kuhn’ debate will be tentatively accommodated within a spectrum of selective biases (content and context biases) such as the ones studied in cultural evolution. Hull’s model that includes replicators and interactors will be applied to study the way scientific replicators and disciplinary matrices interact.

Chapter 2

Three evolutionary approaches for the study of scientific change

2.1 Premise

At the beginning of chapter 1, I introduced Bradie's disciplinary labels that remark on the existence of two different programs in evolutionary epistemology: EEM (evolutionary epistemological mechanism) and EET (evolutionary epistemology of theories). I have also anticipated that these two evolutionary areas of research mirror a more recent controversy within the human evolutionary behavioural sciences. In this chapter, I present the main points of this controversy and I explore its significance in the context of epistemology. On one hand, evolutionary psychologists have emphasized the role of cognitive adaptations and downplayed a selectionist or adaptive account of cultural ideas. On the other hand, a variegated front of cultural evolutionists maintains that the selective forces (located as preferences in human cognition) directly affect the production and preferential imitation of cultural ideas. This dispute seems to closely mirror Bradie's distinction between the two main orientations in evolutionary epistemology. EP is as interested in the adaptiveness of cognitive mechanisms as EEM. In contrast, cultural evolutionists look at cultural ideas as entities of replication. This very well resonates with the program called EET, which is concerned with the evolution of scientific concepts, ideas and practices. The chapter also presents the case that both the theoretical perspectives of EP and that of cultural evolutionists can give important insights for evolutionary epistemologists.

For example, studies in the psychology of reasoning which have more recently

received an evolutionary spin within EP, seem to testify for the importance of cultural evolution. An EEM program, such as the one advanced by Ruse, does not seem to be able to account for increased adaptability of reasoning routines that go beyond the bio-cognitive dispositions. Another important contribution comes from cultural evolution's selective biases that accommodate Toulmin's spectrum of scientific selectors within a coherent 'populationist' account.

It is also worth mentioning that in presenting these fields I do not claim any exhaustiveness nor I am subscribing to the entirety of their theoretical assumptions. For example, when I present the EP's emphasis on the domain specificity of human cognition that does not mean that I am endorsing a full-fledged *domain specific* characterization of human cognition. My discussion is instrumental to highlighting those aspects of their research that might have significance in understanding scientific practices and scientific reasoning.

2.2 Evolutionary Psychology: domain specificity and 'evoked' culture in science.

Evolutionary Psychology (EP hereafter) focuses its attention on the analyses and inventory of innate psychological mechanisms that developed in humans' minds due to the adaptive challenges faced by our species under the Pleistocene conditions (Tooby and Cosmides, 1989). According to EP, evolutionary biology "provides the crucial missing element that will allow psychologists to discover the design of the innate [cognitive] mechanism" (Barkow, Cosmides and Tooby, 1992, p. 30).

In order to understand EP's leap from our biological and ultimately genetic base to our cognition, it is useful to understand that EP organizes evolutionary explanations in a hierarchy. At the top of this hierarchy is the principle of evolution by natural selection; going down the hierarchy, there are middle level theories, such as parental investment theory, parent-offspring conflict theory (Trivers, 1972; 1974) and inclusive fitness theory (Hamilton, 1964). From these, EP derives 'specific evolutionary hypothesis' and 'specific evolutionary predictions'" (Buss, 2012) as shown in Figure 2.1.

As Buss noted, even if the middle level theories must be compatible with the general theory of evolution through natural selection, they have their own logical and explanatory autonomy:

There is nothing in the theory of natural selection that says anything about parental investment. Thus middle-level theories must be compatible with general evolutionary theory, but they must also stand or fall on their own merits (Buss, 2012, p. 43).

Middle levels theories provide a 'formal analysis of what behaviors would be favoured by selection in a variety of newly explored domains'" (Barkow, Cosmides and Tooby, 1992, p.98).

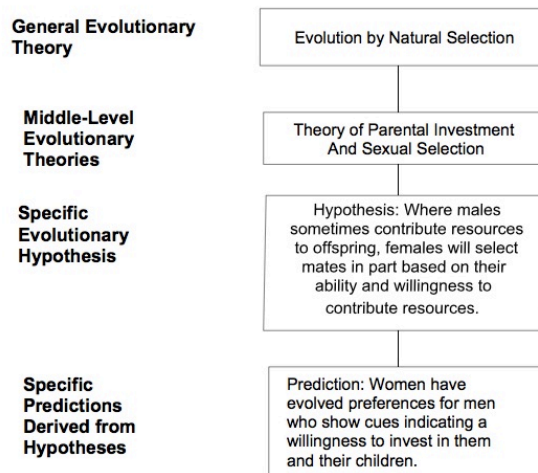


Figure 2.1. Hierarchy of evolutionary explanations in EP. *Source: Adapted from Buss, 2012,*

p.41.

Another important theoretical assumption of EP is that such hypotheses, derived from middle level theories, refer to cognitive modules that are *domain-specific*. EP suggests that *domain-specificity* was required on an evolutionary scale to solve the different adaptive challenges faced by our ancestors. EP rejects the idea that for a diverse range of human activities (such as dealing with language, sex, food, infants, siblings, friendship, etc.) the human mind has evolved as a *general-purpose* mechanism that modulates and controls such an array of different reasoning routines (Tooby and Cosmides, 1992, p. 94). Thus, the distinction between *domain-specific* and *domain-general mechanisms* is key in EP. According to them, cognitivist scientists interested in problem solving put an excessive emphasis on a general computational mechanism, but they overlook that such a system is costly and unnecessary. Rather, EP urges us to see human cognitive abilities as specifically adapted to circumscribed contexts. I shall return later on to the implications of such a perspective for an evolutionary account of science.

I want now to direct my attention to another fundamental theoretical debate that has divided EP from cultural evolutionists.²⁵ The dispute revolves around the role assigned to culture (and cultural evolution) in the wider evolutionary framework that addresses human behaviour. EP defines culture as “any mental, behavioural, or material commonalities shared across individuals, from those that are shared across the entire species down to the limiting case of those shared only by a dyad”. However, its proponents point out that “when the causes of the commonality can be identified, we will use a qualifier, such as “evoked.” (...) Within-group commonalities may have been evoked by common circumstances impacting

²⁵ In the following, I will use the general term “cultural evolutionists” to refer to researchers in memetics and dual inheritance theory (DIT). The divergences between these two evolutionary approaches are discussed in the next sections. Here, the contrast is between their commonalities and EP.

universal architectures” (Tooby and Cosmides, 1992, p. 117). This is a crucial passage to understanding the divide between EP on one side and cultural evolutionists on the other.

What does the term “evoked” mean? To explain this key notion of culture as an 'evoked' process it is useful to refer to EP’s juke-boxes metaphor:

Imagine that extraterrestrials replaced each human being on earth with a state-of-the-art compact disk juke box that has thousands of songs in its repertoire. Each juke box is identical. Moreover, each is equipped with a clock, an automated navigational device that measures its latitude and longitude, and a circuit that selects what song it will play on the basis of its location, the time, and the date. What our extraterrestrials would observe would be the same kind of pattern of within group similarities and between-group differences observable among humans: In Rio, every juke box would be playing the same song, which would be different from the song that every juke box was playing in Beijing, and so on, around the world. (...) Moreover, each juke box's behaviour would change over time, because the song it plays is a function of the date and time, as well as of its location. Juke boxes that were moved from location to location would appear to adopt the local songs, sequences, and "fashions." Yet the generation of this distinctive, culture-like pattern involves no social learning or transmission whatsoever. This pattern is brought about because, like humans, the juke boxes (1) share a universal, highly organized, architecture, that (2) is designed to respond to inputs from the local situation (e.g., date, time, and location). All humans share a universal, highly organized architecture that is richly endowed with contentful mechanisms, and these mechanisms are designed to respond to thousands of inputs from local situations. As a result, humans in groups can be expected to express, in response to local conditions, a variety of organized within group similarities that are not caused by social learning or transmission (Barkow, Cosmides and Tooby, 1992, pp. 115-116).

Hence, according to EP, the 'highly organized architecture' with its domain specific mechanisms interacts with the environment in such a way as to constrain cultural manifestations. The role of social learning, imitation and transmission of cultural information is downplayed: the emphasis is on an evolved *repertoire* of mechanisms.²⁶

On the contrary, cultural evolutionists have stressed the importance of the imitative capacities and social learning skills of our species. According to them, the refined social learning is an adaptation that has generated a parallel, second level, evolutionary process

²⁶ In this respect, EP is faithful to the original sociobiological program of Wilson (1975; 1978). Famously, Wilson stated that: “The genes hold culture on a leash. The leash is very long but inevitably values will be constrained in accordance with their effects on the human gene pool. The brain is a product of evolution. Human behaviour - like the deepest capacities for emotional response which drive and guide it - is the circuitous technique by which human genetic material has been and will be kept intact” (Wilson, 1978, p. 167). Of course, EP refers to the genetic base indirectly by means of hardwired cognitive modules of the mind.

largely released from biological constraints. They both agree that 1) humans' imitative skills have no close parallel in the animal world, and 2) these unique skills have brought our species to release Wilson's 'leash' to a far greater extent than sociobiology and EP admit (Richerson and Boyd, 2005; Mesoudi, 2011; Blackmore, 1999).

According to cultural evolutionists, culture is a form of cumulative adaptation that cannot be 'evoked' in any reasonable sense. Henrich and McElreath (2003) contrasted EP's 'jukeboxes' metaphor with the historical example of the *Burke and Wills Expedition*. In 1860, a group of Europeans tried for the first time to travel south to north across Australia. Due to the harshness of the environment, they did not succeed and most of them died. The few survivors, before being rescued by another expedition, turned themselves into improvised hunter gatherers. Having sporadic contact with aboriginal communities, they tried to imitate the Aborigines' refined cultural practices and technologies. One of these techniques was the so called 'bush bread' made of *nardoo* seeds, which, if not properly processed, are toxic. The Europeans were unable to grasp the technique in an accurate way (think to the role of a linguistic barrier here), further contributing to their starvation and death. The story is offered as an exemplary case of the cumulative adaptedness of cultural information that no 'evoked' process can retrieve from a bio-cognitive endowment:

This bit of history makes a simple point: Humans, unlike other animals, are heavily reliant on social learning to acquire large and important portions of their behavioral repertoire. No evolved cognitive modules, "evoked culture," or generalized cost-benefit calculations²⁷ delivered to these men the knowledge of how to detoxify *nardoo* spores or how to make and use rat traps, bird snares, or fishing nets from locally available materials. Unlike social learning in other animals, human cultural abilities generate adaptive strategies and bodies of knowledge that accumulate over generations (...) The mental representations that allow individuals to detoxify the fern spores or bring down large game with relatively light-weight bows and arrows do not come coded in their genes, nor are these continually relearned by each individual via trial-and-error experimentation or deduced solely by fitness oriented cost-benefit analysis. Instead, such adaptations result from and embody the cumulative effects of the efforts, experiments, errors, insight, and interactions of many individuals across generations (Henrich

²⁷ In this passage, it seems that Henrich and McElreath are marking a point of agreement with EP in rejecting the importance of domain general mechanisms.

and McElreath, 2003, pp. 123-124).

Hence, according to cultural evolutionists, cultural information is cumulative and it has a peculiar adaptive character that goes far beyond instinctual-genetically encoded information. According to a cultural Darwinian perspective, humans have made that leap forward that allows them to accumulate, maintain and perfect a cultural baggage of information. On the contrary, the vast majority of species are unable to have extended cultural accumulation. The limited knowledge that individual organisms accumulate during their life span, disappears with them, so that each generation of individuals have to rely on 'instinctual' heuristics or resort to trial and error. Hence, for all these reasons, dual inheritance theorists conclude that the complex cultural skills exhibited by our species cannot be simply 'evoked'. They build up as a consequence of a parallel cultural evolutionary and selective process that rests on sophisticated imitation and social learning and creative skills unique to humans. Moreover, as I will more fully discuss in the next section cultural evolutionists too assign a primary role to evolved cognitive predispositions as they constrain and bias the adoption of cultural variants. These constraints and biases are psychological preferences located in human minds (Richerson and Boyd, 2005; Dawkins, 1976; Dennett, 1995) which provide ground for an epidemiological and selective conception of cultural evolution. Transmission and imitation are largely downplayed by EP (Richerson and Boyd, 2005).

It is important to note that the dispute highlighted above seems to closely mirror Bradie's distinction between the two main orientations in EE. EP is as interested in the adaptiveness of cognitive mechanisms as EEM. On the other hand, cultural evolutionists look at cultural ideas as entities of replication, and this very well resonates with the program

called EET, which is concerned with the evolution of scientific concepts, ideas and practices. In the following, I argue that both the theoretical perspectives of EP and the cultural evolutionists have great significance for the evolutionary epistemologists.

The EEM program, akin to EP, has been championed in epistemology by Ruse (1986; 1995), who criticized what he calls “the cultural type of evolutionary epistemology,” which is concerned with the evolution and selection of scientific ideas (Ruse, 1995, p. 193). Ruse’s skepticism seems motivated by objections similar to those advanced by Thagard discussed in the previous chapter.

According to Ruse, the disanalogy between a randomly generated mutation on one side (organic evolution) and the non-randomness of cultural ideas poses a serious challenge to any cultural evolutionary epistemology of science. As Ruse put it:

Ultimately the trouble seems to be in the nature of variation (Ruse, 1986). The essence of Darwinism is that the new variations, the mutations in today’s language, are random, in the sense that they do not occur according to need. Even if they are just what is required, there was no design behind their appearance. And yet although it is certainly possible that the new variations of science be random (...) by and large they are the epitome of design. They are sought after and come to the best, prepared minds. (Ruse, 2012, p. 135)

Ruse continues noticing that:

Perhaps best of all is to think of this kind of evolutionary epistemology [EET] less as a full blown theory of evolutionary change, and more as a proposal that will have to wait for full success on the development of an adequate theory of cultural evolution. (Ruse, 2012, p. 136)²⁸

Thus, Ruse inclines to an EEM research program that studies what Lumsden and

²⁸ In the next sections and in chapter 3, I will advance the hypothesis that the theory of cultural evolution present today is adequate enough to answer to Ruse’s concerns. Particularly relevant is the notion of ‘population thinking’ (Mayr, 1959) advocated by DIT. This notion bypasses Thagard and Ruse’s criticism. The idea that an EEM program represents an alternative rather than a complement to the ‘cultural type of evolutionary epistemology’ is misplaced. Ruse himself admits that his critique “has not offered a definitive case-closing refutation” of the culturalist approaches (1995, p. 194).

Wilson (1981) have called 'epigenetic rules'.²⁹ Roughly, these are described as cognitive dispositions hardwired in our brains that constrain cultural evolution: “underlying channels, as it were, into which culture must flow” (Ruse, 1995, p. 158). According to Ruse, evolutionary epistemology must be concerned with an elucidation of bio-cognitive dispositions that govern scientific reasoning.

[W]hat I argue is that the principles of science (and I include here mathematics and logic) are reflections of the innate dispositions, or epigenetic rules, which are burned into the thinking processes of every mature normal human being. We believe that $2+2=4$, not because it is a reflection of absolute reality, or because some of our ancestors made a pact to believe in it, but because those proto-humans who believed in $2+2=4$, rather than $2+2=5$, survived and reproduced, and those who did not, did not. Today, it is these same selectively produced techniques and rules which govern the production of science (Ruse, 1995, p. 163).

Hence, Ruse urges us to adopt an 'adaptationist' perspective on the underlying bio-cognitive constraints that operate in science. This is the same theoretical move that EP advances towards the Darwinian models of cultural evolution. As such, Ruse's evolutionary epistemology suffers the same limitations that the cultural evolutionists impute to EP: the bio-cognitive dispositions alone cannot explain science's cumulative cultural complexity as we know it today. Rather, cultural accumulation seems to be exactly what has allowed humans to go beyond the hardwired cognitive dispositions, promoting the emergence of cumulative revisable knowledge. Evidence of a leap in adaptive cumulativity comes from evolutionary psychologists' research itself.

²⁹ Here, the term 'epigenetic' does not refer to the most recent notion of *epigenetic inheritance* intended as hereditary variation that does not involve changes in DNA structure. Rather, Wilson is talking of epigenetic development. Wilson defines epigenetic rules as the constraints that human genes put on the way the mind is formed – “which stimuli we perceive, how information is processed, the kinds of memories most easily stored and recalled, the emotions that are more likely to evoke, and so on”. Examples include, outbreeding, colour perception and vocabulary, odour and taste discrimination (with effects on evolution of language and cuisine), facial expressions, phobias, etc. (see Wilson, 1983). For an more historical treatment of developmental epigenesis and its contrasting notion of 'preformationism' see Maienschein, 2017.

For example, EP provided some evidence concerning the domain specific character of some reasoning routines that have significance in the context of scientific reasoning. The evidence gathered by EP, seems to testify in favour of the importance of cultural accumulation rather than against it. In fact, according to EP, humans seem to not 'naturally' possess a general purpose logical mechanism that produces correct deductive inferences in any context (Cosmides, 1985). Rather, they argue, our logical abilities are highly domain specific.

Previous psychological studies in the psychology of reasoning had already shown that humans do not fully master the generality of first-order predicate logic (Wason, 1968) and that surprisingly we “lack a procedure corresponding to *modus tollens* almost entirely” (Cosmides, 1985, p. 16). More recently, EP put an evolutionary spin on these early studies showing that humans reasoning performances vary according to the context in which they are presented. For example, the experimental evidence based on the Wason’s selection task is an illustrative example of the type of contributions that EP might give to the field of evolutionary epistemology (in particular, for the EEM program defended by Ruse).³⁰

The “Wason selection task” is a very simple logical puzzle that gives to the participants a conditional rule of the type “If P then Q” and four cards representing four situations (*P*- *not P*- *Q* – *not Q*). The experimenter asks the participants to indicate which cards need to be turned over in order to see if the conditional is violated (see figure 2). Experimental evidence has shown that people are very often unable to indicate which cards

³⁰ Again, I want to remark that here I am just presenting EP’s research as it is instrumental in clarifying the epistemological debate. I am not necessarily subscribing to EP’s general theoretical assumptions such as, for example, the idea that domain specificity is the privileged way to look at human cognition. Nor do I think that all evolutionary psychologists consider this point as a theoretical dogma of their discipline. Their findings are just instrumental to advance my objection that science requires cultural adaptations that go beyond the biological constraints of human cognition.

need to be turned over to see if the rule is violated. As I said, EP has given an evolutionary twist to these early studies in the psychology of reasoning showing that the logically correct answers are context sensitive: the performance of the subjects change if the context is changed.



Figure 1.2. The Wason selection task. The rule “If P then Q ” is offered to the participants with different contents. For example, it might be presented the rule “if a person is a biologist, then that person enjoys camping” .For this rule every card represents ‘biologist’ (P), “chemist” ($notP$), “enjoys camping” (Q), “does not enjoy camping” ($not-Q$). The test asks to identify the cards that violate the conditional (the checkmark indicates the logically correct card choice) (Cosmides, Barrett and Tooby, 2010).

For example, in those situations where the conditional expresses 'social contract exchanges' (e.g., “If you accept benefit B from me, then you must satisfy my requirement R”) subjects perform sensibly better than any other situation where the conditional does not express a contract in which a person is eligible for a benefit (in case she pays a price or meets a requirement) (Cosmides, 1985, p. 41). These empirical findings lead evolutionary psychologists to hypothesize that humans possess a 'cheater detection mechanism', as a sort of *domain-specific* module, which is responsible for better reasoning routines in those cases where conditionals involve social exchange and detection of violations (Cosmides, 1985; Cosmides, Barrett, and Tooby, 2010). This agrees with the aforementioned idea that the human mind has modules that are adapted to solve specific adaptive challenges. The implications for an evolutionary account of logic are immediately clear: the content-

independent generality of the conditional rules (a prototypical feature of logic itself) does not seem hardwired in our brains. EP's proponents conclude that “either our species did not evolve a full and unimpaired version of first order logic, or significant parts of it are not activated when people try to solve this simple information search task” (Cosmides, Barrett, and Tooby, 2010, p. 9009). Thus, even if Ruse noted that the evolutionary psychologists’ studies have great relevance for evolutionary epistemology because they help to clarify the adaptive origin of our reasoning faculties (Ruse, 1995, p. 111) he does not seem fully aware that the implications of these studies rather justify the importance of cultural accumulation and evolution.

In fact, it is reasonable to assume that a logician, through exemplars accumulated within his community and transmitted to him through some form of social learning, is able to master the generality of the conditional rule far better than a common person subjected to their 'innate' logic and its associated fallacies. Of course, the comparatively poor innate cognitive dispositions are a necessary precondition to mastering the more sophisticated logical abilities of modern logic. However, the latter manifest an increase in adaptedness that requires an explanation. Unless we want to resort to 'skyhooks,' providential solutions very well presented by Dennett (1995), this gain in adaptedness that surpasses the biologically evolved disposition is in need of an evolutionary explanation of some sort. Cultural evolution and accumulation seems the right candidate to account for the emergence of this second-level adaptedness because, as in the example of the ‘bush bread’ presented above, cultural adaptations go beyond the bio-cognitive dispositions. Cultural evolution can circumvent the biases created by evolution in our reasoning faculties and thus respond to new adaptive challenges. In other words, the refinement of entire bodies of knowledge, the refinement of

logical and mathematical skills, seems rather a product of cultural evolution, which has advanced beyond what is dictated by not very well specified “epigenetic rules”.

Nonetheless, potentially, Ruse’s EEM can be integrated with EP’s program to generate domain specific hypotheses that highlight important underlying cognitive aspects on which science ultimately rests upon. Such an approach could, for example, help to discriminate the role of biological evolution on one side and cultural evolution on the other. These two programs must not be seen as radical alternatives. Ruse’s and EP’s models can be reconciled with the ‘culturalist’ orientations that stress the importance of a parallel process of transmission and adaptive accumulation.

To summarize, the refined social learning, which allows for accumulation, reiterative corrections and refinements of cultural ideas and products, generates cultural adaptation that cannot be ‘evoked’ in any meaningful sense. By the same token, Ruse’s ‘epigenetic rules’ cannot account for the complexity of extant science. Nobody, would reasonably claim that the highly specialized scientific ‘knowledge’ accumulated today can be ‘evoked’ from a bio-cognitive background (nor can the sophisticated formal languages of logic and mathematics). If this accumulated ‘information’ cannot be evoked and if it exhibits increasing adaptedness (e.g., cognitive progress, Niiniluoto, 1999), it is clear that the merely bio-cognitive level of explanation is ruled out as insufficient.³¹ Unless we want to endorse the idea that genetic evolution is directly responsible for this increase in adaptedness of our scientific principles, ideas, beliefs, representations, we have to account for adaptedness in terms of cultural accumulation. This is not to deny the importance of bio-cognitive constraints, but rather, to

³¹ This point might appear obvious but some anthropologists sympathetic to the EP program have recently argued that “gradual biological changes in cognition and personality played a key role in the birth of the industrial and scientific revolutions” (see Cochran and Harpending, 2009, chapter 4). However, this account seems to largely undermine the role of cultural transmission.

take them as prerequisites that underline another evolutionary level. In other words, we have to hypothesize another process of transmission of information that is parallel to the genetic one, acted upon by peculiar types of selective forces located in the human mind as 'preferences'. These preferences are the subject of DIT's selective model discussed in the next section.

2.3 Dual Inheritance Theory: transmission biases in science.

In the previous section, I presented the cultural evolutionists' critique of EP's conception of culture as an 'evoked' behavioural response triggered by innate cognitive mechanisms as they interact with environmental factors. I have also remarked that cultural evolutionists see cultural accumulation as a largely autonomous (from a genetic base) evolutionary process. This observation immediately leads to an analysis of the evolutionary forces that operate on cultural ideas. DIT presents a comprehensive inventory of them. The first important distinction is between inertial and non-inertial forces. As Richerson and Boyd put it:

We divide the evolving system into two parts. One is the "inertial" part – the processes that tend to keep the population the same from one time period to the next. In this model cultural inertia comes from unbiased sampling and faithful copying of models. The other part consists of the forces – the processes that cause changes in the numbers of different types of cultural variants in the population. These processes overcome the inertia and generate evolutionary change (Richerson and Boyd, 2005, p. 68).

This distinction is also important in the context of scientific ideas. For example, Kuhn's periods of normal science can be described as dominated by inertial transmission. In both cases, there is a tendency to replicate the same pool of variants from one time period to

the next. Inertial forces do not lead to change but rather hinder it.³² On the other hand, non-inertial forces can be further divided in two types: 1) cultural transmission biases, which includes “forces that arise because people's psychology makes them more likely to adopt some [ideas] rather than others” and 2) other forces that alter the cultural pool of information such as cultural drift, natural selection and demic diffusion.³³ It is important to note that only the former are selective forces, while the latter are non-selective forces (Richerson and Boyd, 2005). In the following, I want to focus my attention on biased transmission, not because the second type of forces do not have any relevance in science, but because I am concerned here with the selective criteria of scientific ideas. This allows me to evaluate DIT's selective model as it applies in the context of science. My question is: Does the selective model of DIT contribute to clarifying epistemic selection? As I have discussed in the previous chapter, evolutionary epistemologists have been very divided on the selective factors operating on science, so it is worth investigating whether DIT's model clarifies the matter.

To begin with, DIT transmission biases are of three main types. *Content biases* refer to the preferential adoption of traits that are chosen for their intrinsic attractiveness. Whether through cost-benefit calculations or as a result of cognitive dispositions, they make the variant easier to learn or remember (Richerson and Boyd, 2005; Mesoudi, 2011). *Model-based biases* operate when individuals preferentially adopt traits based on the characteristic of the model. For example, a predisposition to imitate successful and prestigious individuals

³² To give an historical example of these inertial obstacles, we can look to the harsh early reaction of a number of prominent scientists to Darwin's *Origin*. For Agassiz, Darwin's theory was "a scientific mistake, untrue in its facts, unscientific in its method, and mischievous in its tendency"; Sedgwick talked of "a dish of rank materialism cleverly cooked and served up", while Flourens described natural selection as a “metaphysical jargon clumsily hurled into natural history” (sic!) (as cited by Simonton, 1999, p. 134).

³³ Cultural drift defines the effects caused by statistical anomalies in small populations. For a full list of the several evolutionary forces of cultural evolution, see Mesoudi, 2011, chapter 3.

would qualify as a prestige bias. Furthermore, *frequency-based biases* define the use of commonness or rarity as a ground for preference. For example, humans exhibit the inclination to adopt the most common trait (conformity bias) or, in other cases, the rarest (rarity bias) (See Fig. 2.3).

How can such a model be extrapolated from the general context of cultural ideas and be applied to the more specific context of scientific ideas? A *prima facie* evaluation in science immediately leads to the problem envisioned by Toulmin: how to make sense of a spectrum of selectors from epistemic norms to the sociology of scientific research?

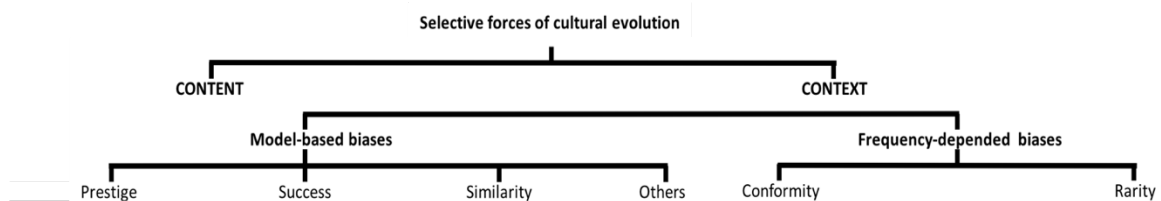


Figure 2.2. Selective biases in cultural evolution. Adapted from Henrich & McElreath, 2003, p.8.

I argue that DIT’s selective model, comprising transmission biases, helps to systematize this spectrum of diversified selective forces.³⁴ Moreover, such a model helps to clarify the polarized controversy on which selective factor has priority over the other. The populationist account defended by DIT predicts that there is a great deal of variability in the preferences. No two scientists preferentially adopt a conceptual variant for the same exact reasons as if they all execute a sort of algorithm of theory choice. I shall elaborate on this

³⁴ In the next chapter I also recognize the main limitation of DIT’s model. DIT does not provide an account of the process that introduces and generates variation. In other words, they undermine the importance of a creative phase of cultural evolution. As I discuss in the next chapter, this might require a non-selective account.

point later in this section. First, I want to clarify how DIT's selective model can accommodate the spectrum of selectors presented by Toulmin.

We noted in chapter 1 that Toulmin's spectrum of selectors ranges from 'internal' to 'external' components. It can be said that context biases (frequency and model biases) are on the "external" side of the spectrum because they highlight aspects of the sociology of knowledge (as it has been presented by Kuhn, Toulmin and Hull). Here, the emphasis is on social dynamics and standards that ultimately are social constructs. Such external selective factors include styles of thought, metaphysical taste, broader political and socio economic factors (Toulmin, 1972).

For example, a *conformity bias* might be at play in what Kuhn has described as the pedagogy of science, where scientists are conditioned through exemplars presented in textbooks to conform to the expertise of the majority (Kuhn's 'textbook science'). An 'opposite' *rarity bias*, expressing the tendency to acquire the rarest trait (instead of the most common), might also play a role in the emerging phase of a new scientific paradigm. For example, Darwin had a small number of supporters who formed an *entourage* of fierce defendants of his ideas. One of the most notable was T. H. Huxley. Frank Sulloway has suggested that the differential responses in the scientific community to *Origin* are due to individual differences in openness to the acceptance of new ideas. This seems particularly true, he noted, when a critical review of the early debates shows that rational considerations, such as data and logic, were largely downplayed by a conflict of personalities (as cited in Simonont, 1999, pp. 134-35). In this respect, Darwin's entourage's tendency to embrace theoretical novelties or to adopt the rarest trait in the population of variants can be interpreted as a *frequency bias*.

A second type of context biases, *model based biases*, assume that humans have predispositions to imitate successful or prestigious individuals (*prestige bias*), to imitate individuals similar to oneself (*similarity bias*) or preferentially copy older models (*age bias*) (Mesoudi, 2011, p. 73). Mesoudi gives a number of convincing examples that range from social psychology experiments, where individuals shift their preferences following prestigious individuals, to sociolinguistic evidence that shows that linguistic changes are often driven by high-status speakers in a community (see Mesoudi, 2011, pp. 73-76). No doubt, these biases are important in many cultural domains including science.

On the other hand, the 'internal' components of Toulmin's spectrum of selectors, what makes a theory better than another, is something that belongs to intrinsic properties of the variants themselves. The greater explanatory power of Lavoisier's oxygen theory compared to Stahl's phlogiston theory seems a matter of intrinsic properties. *Content biases*, which express the intrinsic attractiveness of an idea, seem the right candidates for the role of intrinsic selective forces. It is reasonable to hypothesize that these content biases are preponderantly important in science when compared to other types of transmission biases. This is why Popper did not want to concede to Kuhn that other types of context biases might also have a selective relevance. However, Kuhn was not denying the importance of content biases, but he was rather noting that context biases participate as well in the selective process. Popper and Kuhn were missing a populationist framework that can easily accommodate their polarized positions and find a reconciliation between them. Figure 2.2 shows how Toulmin's spectrum of selectors can be organized within DIT's selective model.

However, even if we assume that the most relevant selective biases of scientific ideas are *content biases* and that only these are epistemically significant for philosophers, we are

still burdened with the elucidation and enumeration of these preferences. How many they are? Do the scientist apply them in the same unanimous way?

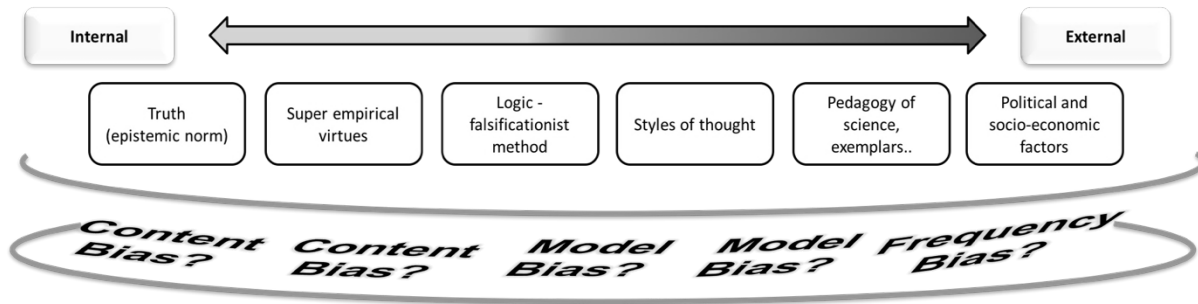


Figure 2.3. Toulmin's spectrum of epistemic selectors. (Here, Toulmin's spectrum is compared to the transmission biases in cultural selection.)

This problem seems very close to early attempts made by philosophers of science (also in Popper and Kuhn) to compile a list of 'preferences' that are unanimously used as selective criteria in theory choice. This became a very controversial issue between Kuhn and the Popperians. Kuhn (1977) was critical toward the philosophical attempt to build an entirely well-articulated list of criteria as a sort of algorithm for theory choice that dictates rational, unanimous preferences. He noted:

When scientists must choose between competing theories, two men fully committed to the same list of criteria for choice may nevertheless reach different conclusions. Perhaps they interpret simplicity differently or have different convictions about the range of fields within which the consistency criterion must be met. Or perhaps they agree about these matters but differ about the relative weights to be accorded to these or to other criteria when several are deployed together (Kuhn, 1977, p. 324).

Thus, even two scientists committed to the same list of criteria may nevertheless make different evaluations. Kuhn concluded that the search for an algorithm that is unambiguously used by all scientists is "a not quite attainable ideal" (Kuhn, 1977, p. 324).

This perspective agrees with the cultural evolutionists' selective model that does not require a unique algorithm of preferences and biases. Scientists are expected to present a great deal of internal variability in terms of motivations and preferences and this agrees with a populational framework of cultural evolution. Famously, Mayr (1959) argued that Darwin's greatest revolution in biology was a shift from typological to population thinking. In his words:

The assumptions of population thinking are diametrically opposed to those of the typologist. The populationists stresses the uniqueness of everything in the organic world. What is true for the human species, - that no two individuals are alike, - is equally true for all other species of animals and plants. Indeed, even the same individual changes continuously throughout his lifetime and when placed into different environments. All organisms and organic phenomena are composed of unique features and can be described collectively on in statistical terms. Individuals, or any kind of organic entities, form populations of which we can determine the arithmetic mean and the statistics of variation. Averages are merely statistical abstractions, only the individuals of which the populations are composed have reality. The ultimate conclusions of the population thinker and of the typologist are precisely the opposite. For the typologist, the type (*eidos*) is real and the variation an illusion, while for the populationist the type (average) is an abstraction and only the variation is real" (Mayr, 1959, p.2)

DIT transposes population thinking from biological to cultural evolution (Richerson and Boyd, 2005).³⁵ In the realm of culture, individuals are not expected to execute the same exact preferential algorithms in adopting the same cultural variant. By the same token, scientists are not expected to have an unambiguous algorithm for theory choice. This is not to deny that these diversified set of preferences might converge on a set of shared criteria. Kuhn concedes to his critics that all individual scientists have an algorithm and that all their algorithms have much in common. However, he thinks that each scientist, especially in the early stages of development and acceptance of a theory, presents a great deal of variability

³⁵ As previously stated on note 10, Mayr was not sympathetic with the application of evolutionary analogies from the biological to the cultural context. Thus, I am referring to cultural evolutionists' interpretation of Mayr's legacy and notions rather than directly to Mayr's work.

within its selective criteria. At this stage, ‘subjective’ differences pertaining biographies, personalities, environmental experiences (e.g., the peculiar learning environment where their expertise was gained) all contribute to add variability in the selective criteria.

A populationist selective approach can clarify this dispute between Kuhn and his critics. Following Mayr’s definition that states that for the populationist ‘only variation is real’ we can look at scientist’s algorithms for theory choice as sets of criteria that present individual variability. We could think these algorithms as analogous to genomes. In this respect, the ideal of the philosophers to reach an unambiguous unique algorithm is closer to the typologist’s strategy that tries to find a type an *eidos*. However, this *eidos* is for the populationist just a statistical abstraction, an ‘average’ that, strictly speaking, is not ‘real’.³⁶ For this reason, population thinking seems the right candidate to reconcile the many selective emphases posed by different evolutionary epistemologists. In admitting variation within the preferences, DIT offers a more plausible psychological scenario for the adoption and transmission of scientific ideas, reconciling Popper and the Popperians with Kuhn’s concerns.³⁷

Such a view, very well resonates with more recent attempts made by dual inheritance theorists to present science as an evolutionary and selective cultural process (Mesoudi et al., 2013; McCauley, 2013). As Mesoudi et al. noted:

The evolution of scientific knowledge itself exhibits all the dynamics characteristic of an evolutionary process, here referred to as “epistemic evolution” (...) The exploration of the inherent potential of the means for gaining knowledge in a society gives rise to a variety of conceptual alternatives within a knowledge system, corresponding to mutation in biological evolution. As these alternatives are elaborated and pursued, they lead to internal tensions and contradictions, resulting in the transformation or the branching of a new knowledge system;

³⁶ In chapter 4, I shall return to the importance of the disciplinary matrix and its associated epistemic aims for understanding the source of such variability in the selective criteria of scientific change.

³⁷ For Kuhn’s persuasive argumentation against an unambiguous algorithmic view of theory choice, see Kuhn, 1977, Chapter 13.

this can be seen as analogous to speciation. For example, in the early modern period a broad variety of proposals for a new theory of motion was advanced by Galileo, Descartes, Harriot, and others which eventually led to convergence on a new understanding of motion (...)
Various selective pressures may act on scientific knowledge systems and theories, such as compatibility with existing knowledge, internal coherence, compliance with methodological and institutional constraints, as well as societal expectations, prestige, fashions, and ideologies (Mesoudi et al., 2013, p. 212-213).

Here, the variety of selective pressures represents a realistic view of the complex selective factors of scientific ideas. However, an objection can be raised. This model seems to be equally applicable to science as to any other cultural realm, such as religious or political ideas. Campbell moved this objection to Toulmin and Hull's models noting that they were equally applicable to the evolution of pottery's styles, religious ideas and science, but they failed to capture the distinctive evolutionary epistemic puzzle offered by science: how scientific ideas, over time, lead to better representations of external reality. In the next chapter, I argue that 'reality' plays a different role in the selection of scientific beliefs on one side and aesthetic, religious or political beliefs on the other. While religious, political, artistic ideas etc. do not necessarily improve the fit between beliefs and the external 'reality', science does.³⁸ Thus, the external reality must also be included as a selective factor of science. This 'fitness' of representations and beliefs is the puzzle that an evolutionary epistemology must address in order to trace a demarcation line between science and other cultural realms.

To conclude, DIT's selective model offers a meta-framework to rethink Toulmin's (1967, 1972) spectrum of selectors and to pacify the harsh debate between Popper and Kuhn. A populationist perspective on the preferences that act in theory choice (or more generally in

³⁸ This demarcation criterion is rather superficial and presents some counterexamples. Think of technological innovations applied for special effects in movies. Arguably, they improve the fit between cultural technological replicators (ideas) and 'reality'. However, it is not immediately clear how these improvements relate to 'belief states'. Thanks to Liane Gabora who drew my attention to this point.

the selection of scientific ideas) highlights that variation of preferences is to be expected.

This should motivate the epistemologist of a naturalistic turn to engage in empirical research to highlight scientists' motivations and transmission biases. I shall return to this point in the next chapters.

2.4 Memes and memplexes as conceptual replicators in science.

A 'meme' is defined as a unit of cultural information that is transmitted between individuals through a process that can broadly be referred to as 'imitation' (Dawkins, 1976, p. 192).

Memetics maintains that “the theory of evolution by natural selection is neutral (...) regarding the differences between memes and genes; these are just different kinds of replicators evolving in different media at different rates” (Dennett, 1995, p. 345). As Dawkins put it:

... for an understanding of the evolution of modern man, we must begin by throwing out the gene as the sole basis of our ideas on evolution. I am an enthusiastic Darwinian, but I think Darwinism is too big of a theory to be confined to the narrow context of the gene. The gene will enter my thesis as an analogy, nothing more. What, after all, is so special about genes? The answer is that they are replicators ... But do we have to go to distant worlds to find other kinds of replication and other, consequent, kinds of evolution? I think that a new kind of replicator has recently emerged on this very planet. It is staring us in the face. It is still in its infancy, still drifting clumsily about in its primeval soup, but already it is achieving evolutionary change at a rate which leaves the old gene panting far behind (Dawkins, 1976, p. 206).

To begin with, it is important to note that between memetics and DIT there has been some disagreement regarding the 'units' of cultural replication. According to memeticists, the cultural replicators are ideas that “form themselves into *distinct memorable units*” or “the smallest elements that replicate themselves with reliability and fecundity” (Dennett, 1995, p.

344, emphasis on text). On the other hand, DIT contested the idea that we need a 'Mendelian' characterization of the cultural units as bits of information that are faithfully replicated and transmitted. DIT stresses that such a Mendelian view is unnecessary and rather misleading: a “populationist’ Darwinian method is equally applicable to the 'smallest bits of culture' (such as a phonological rule) or to large integrated systems of cultural elements (such as an entire language or a religious ideology). Particularly, the latter contains internal variety across the population and cannot be thought of as faithfully replicating units (Ramsey & De Block, 2015). As Richerson and Boyd put it:

At the formal level, Darwinian methods will apply equally well in either case. We keep track of the different variants, independent little bits or big complexes as the case may be, present in a population, and try to understand what processes cause some variants to increase and others to decline. The same logic applies whether the variants are individual phonological rules or entire grammars (Richerson and Boyd, 2005, p. 91).

In this respect, DIT's rejects a too straightforward analogy between genes and memes. This issue bears significance for an evolutionary model of scientific change. In fact, a memetic Mendelian characterization seems insufficient to explain the vast and complex systems of integrated ideas, beliefs and practices that form what Ankeny and Leonelli (2016) describe as “scientific repertoires”.³⁹ To highlight the “smallest replicator” in science seems indeed a very difficult task, given the interrelation between these variegated elements. In this scenario, theories are more similar to integrated entities, comprising a range of different elements, which stand together to build up larger units. These larger units can be thought of as 'memeplexes' (Blackmore, 1999) rather than particulate “memes”. Given this complexity, we have to admit that a clear description of what might constitute a scientific meme has not

³⁹ A scientific repertoire is a “well-aligned assemblages of skills, behaviors, and material, social, and epistemic components that groups may use to practice certain kind of science” (Ankeny & Leonelli, 2016, p. 20).

yet been advanced. Dennett himself does not say much about this crucial point. How do the different components such as observations, data, classifications, theoretical concepts (but also more ‘external’ components of a scientific repertoire), qualify as memes or memplexes?

It must also be emphasized that when cultural evolutionists talk of discrete cultural units of transmission they are not necessarily referring to the neuronal level of said ideational units. At this level, cautiousness is suggested. As Mesoudi (2011) put it:

"... artifacts, speech sounds, and stated beliefs are the outward behavioral expression of information stored in the brain and as such are the cultural equivalents of phenotypic traits such as height or skin color. As we saw for the biological case, even though phenotypic traits such as height may vary continuously and appear to blend in offspring, they are nevertheless determined by discrete underlying units of inheritance (genes). In the same way, it may be that continuous, blending cultural traits such as speech sounds and expressed political values are determined at the neural level by discrete cultural units of transmission. This is ultimately an issue for neuroscientists studying how information is represented in the brain and how it is transmitted from one brain to another. Given our current lack of understanding of such issues, it is impossible to say with certainty whether cultural transmission, at the neural level, is particulate or non-particulate. Without this evidence, a cautious working assumption should be that cultural variation may, in some cases, be continuous, and that cultural transmission may, in some cases, be blending" (Mesoudi, 2011, p. 42).

Thus, for the purpose of this work any dispute concerning the neuronal level is largely irrelevant because the replicators I am talking about are not taken at this level of analysis.⁴⁰ Here, I assume that cultural replicators are ideational contentful items individuated at a much coarser level than the neuronal one. For example, the scientific technique called “DNA barcoding”, which I introduce in chapter 5, counts as a scientific replicator because (1) we can trace a populational dynamic over time in the pool of variants⁴¹ and (2) DNA barcoding

⁴⁰ For a critique of the discreteness of ideational replicators from a neuro-cognitive perspective, see Gabora, 2011a. Gabora emphasizes that recent neuroscientific research suggests that ideas are distributed across neurons and not discrete and particulate.

⁴¹ In chapters 4 and 5, I provide empirical evidence of this dynamic using text analysis.

is taken in its ‘coarse grained’ and public characterization. DNA barcoding, as an ideational replicator, counts as the “same thing” through a number of different fine-grained understandings of it. My perspective allows me to circumvent the disputed nature of the units of replication presented above. My notion of replication is general enough to encompass both a Mendelian gene-like characterization and wider ideational aggregates studied by DIT. In other words, the possibility to trace a lineage of scientific ideas requires a notion of ‘replicator’ of some sort. A scientific concept (e.g., the concept ‘gene’) can be the same concept despite different conceptualizations in individual minds and despite different neuronal encodings of those ideas.⁴² What is relevant for a Darwinian framework is to be able to reconstruct a possible phylogeny and to assess the cultural fitness of this replicator over time.⁴³

From the point of view of the evolutionary approach defended here in its application to the evolution of scientific ideas, two assumptions undermine a whole sale rejection of ‘replicators’. First, the information transmitted between time t and $t+1$ must have a distinctive nucleus of information that allows us to trace continuity of transmission of a given variant. Secondly, this nucleus must be maintained no matter how ‘noisy’ the process of transmission might be (otherwise the entire enterprise of tracing the epidemiological dynamics of a cultural trait and its cultural phylogeny is compromised). These two conditions apply even when the cultural units of transmission are complex sets of ideas which are the

⁴² I shall return to this point in the next chapter where I introduce an abstract characterization of a selection process where the range of the variant property is specified in the simplest possible binary way: a ‘replicator’ does or does not have the property P .

⁴³ For a recent defence of the explanatory value of a notion of ‘cultural fitness’, see Ramsey and De Block, 2015. For Henrich (2004, p.21) cultural fitness measures “the degree to which a particular value of ϕ , representing stuff stored in the head of individual i in group j at time t , affects its proportional representation in the population at time $t+1$.”

object of interest of DIT. Thus, it seems that the notion of 'replicator' is more useful than not. As dual inheritance theorists admit, "sometimes cultural variants are somewhat genelike, while other times they are decidedly not. But – and this is a big but- in either case, the Darwinian approach remains useful" (Richerson and Boyd, 2005, p. 80).

To look at scientific ideas as 'replicators' seems to have another important consequence for the study of scientific change. The gene's "point of view" defended in organic evolution by Williams (1966) and Dawkins, (1976) sees the organism as a "vehicle", a "sort of survival machine created to enhance the gene's chances of continued replication" (Dennett, 1995, p. 325). Analogously, memetics sees the cultural or 'ideational' (Durham, 1991) 'replicators' as involved in a 'struggle' to enhance their own continued replication. The "*cui bono?*" (see p. 14) question of evolution and selection has a clear answer: the replicator. It is for the replicator's advantage that all phenotypic characteristics are created and maintained (Dennett, 1995; Dawkins, 1976). Based on the usefulness of the "gene's eye" perspective in biological evolution,⁴⁴ we should consider adopting a "concept's eye" view in cultural evolution and science. According to this view, observations, theoretical terms, data, etc. (components that are part of the philosopher of science's lexicon) build up wider theories and wider scientific repertoires in order to foster their own replication. The replication of a repertoire adds to the replication of a theory which adds to the replication of the sub units of a theory themselves. Moreover, in this perspective, the social circles and even the institutions that promote scientific knowledge are the indirect product of the replicators: they are part of their extended phenotype (Dawkins, 1982). The replicators aggregate scientists in scientific

⁴⁴ Again, I must emphasize the contested nature of this debate. Here, I endorse a pluralist conception that assumes the possibility to switch perspective according to the level of analysis from the replicator to the interactor 'point of view', see note 12. My more limited and cautious claim is that the 'replicators eye view' seems to better fit my evolutionary model.

communities not the opposite.

Finally, another important contribution that comes from the adoption of a concept's eye point of view, is the clarification of the selective environment of a replicator. Dennett noted that memes have “a phenotypic effect that tends to disable the selective forces arrayed against them”. For example, the meme of *faith* “discourages the exercise of the sort of critical judgement that might decide that the idea of faith was, all things considered, a dangerous idea” (Dennett, 1995, p. 349). When this insight is transferred in the context of science, it seems to be promisingly explanatory. For instance, this account seems to be applicable to the Kuhnian characterization of 'normal science' as an established set of practices that “often suppresses fundamental novelties because they are necessarily subversive of its basic commitments” (Kuhn, 1972a, p. 5). Thus, a Kuhnian paradigm can be thought of as a set of replicators organized in such a way as to have a 'phenotypic property' that 'disables the selective forces' that might attempt to invalidate it. However, according to Dennett, the selective forces of memes are 'minds and other memes'. In his words:

Like a mindless virus, a meme's prospects depend on its design – not its “internal” design, whatever that might be, but the design it shows the world, its phenotype, the way it affects things in its environment. *The things in its environment are minds and other memes*. For instance, whatever virtues (from our perspective) the following memes have, they have in common the property of having phenotypic expressions that tend to make their own replication more likely by disabling or pre-empting the environmental forces that would tend to extinguish them (Dennett, 1995, p. 349, emphasis added).

Dennett is right in characterizing the selective 'environment' of a meme as “minds and other memes”. Our internal cognitive predispositions and the landscape of previously accumulated ideas clinging to our neurons both act as selective forces on newly introduced cultural variants. However, in the case of science, there is another environmental selective factor that is 'reality' itself. If we take seriously Campbell's *monitum*, as evolutionary

epistemologists we are burdened with a selective explanation of how the external referent participates in the selection of our scientific ideas. Dennett does not seem to fully acknowledge the selective role of the physical world in this passage. Part of the selective environment of a scientific replicator is the external 'reality' itself. In fact, the phenotypic expression of a meme (or groups of them organized as a memplex) also preempts 'reality' to extinguish certain established scientific ideas. This occurs, for example, during periods of Kuhnian normal science, when observational and experimental evidences are shelved because they threaten the stability of the dominant paradigm.

Take the Michelson-Morely's experiment, which had broad theoretical consequences for the theory of Galilean relativity and for the hypothesis of the aether as a medium for the propagation of light. Until a better theory that could account for such an observational puzzle came (Einstein's theory of special relativity), observation and so to speak, 'reality', were temporarily shelved as observational puzzles. Thus, particularly for what concerns scientific replicators, it would be erroneous not to include the selective role of external reality as a crucial 'environmental factor' of selection. As I will further explain in the next chapter, 'reality' and pre-theoretical observation have an active role in promoting the emergence of new theoretical ideas.

Another example is Democritean 'physics'. According to Aristotle, Democritus formulated the hypothesis of 'atoms' as indivisible bodies in order to respond to Parmenides' metaphysical puzzle on the possibility of change and multiplicity (Berryman, 2011). Thus, the concept of the 'atom', was generated on merely speculative grounds. In this case, Dennett is right in noting the selective environment of the meme 'atom' was composed by 'minds and other memes'. However, today, this meme is positively selected in the pool of variants on

totally different evidentiary grounds. The accumulation of technological advancements, from x rays crystallography to atomic force microscopy, offers a radically new selective environment for the replication of such a meme. Thus, it can certainly be said that the selective environment for the meme 'atom' has drastically changed. Through these technological improvements (a product of adaptive cultural accumulation) 'reality' indirectly participates in the selection of a conceptual replicator. To conclude, the selective environment of a scientific replicator cannot be restricted just to 'minds and other memes' but it needs to include the 'external reality' as an active counterpart in the selection of our scientific posits. I shall return to this important point in the next chapter where I discuss a realist selective perspective that I call 'dynamic selective realism'.

2.5 Summary

In this chapter, I have discussed three main evolutionary approaches that have significance for a cultural evolutionary analysis of scientific change. Against EP's attempt to underplay the role of a second level - cultural evolutionary process, I have defended the DIT and memetics' perspectives according to which cultural evolution is the product of an evolutionary and selective process that produces its own adaptations. This gives ground to an evolutionary and selective treatment of scientific cultural ideas as a peculiar type of cultural adaptations.

Scientific ideas can be seen as ideational replicators that are subjected to populational dynamics of transmission. Overall, cultural evolutionists' selective model offers a meta-

framework to organize scientist's selective criteria on a spectrum of epistemic and non-epistemic biases and preferences. A populationist and selective framework seems able to accommodate some aspects of scientific research that have puzzled philosophers of science such as Kuhn and Popper. For example, a populationist framework establishes that variation in the selective criteria (think to the criteria for theory choice) is to be expected both at the level of individual scientists and their wider circles. In other words, consensus becomes a matter of a populational dynamic that at some point in time, eventually, crystallizes. In this respect, populationism offers a way to reconcile Popper with Kuhn.

However, an evolutionary and selectionist approach to scientific ideas has also to respond to another important criticism. This criticism, briefly introduced in chapter 1, claims that there is a fundamental disanalogy between cultural and biological selection. The former is directed and intentional while the latter is a non-guided process *par excellence*. In the next chapter, I direct my attention to this objection from the point of view of an evolutionary theory of creativity. As it will become apparent, creativity poses a stimulating challenge to a selectionist account of cultural evolution and scientific change.

Chapter 3

Toward a dynamic selective model of epistemic creativity and epistemic transmission.

3.1 Premise

Creativity is studied from a plethora of theoretical approaches that have emphasized different aspects or facets of the creative process. The so called “four P’s” of creativity alternatively study creativity as a process, a product, a person (meaning the person’s personality) or a place (meaning the context that promotes creativity). An exhaustive treatment of this variegated scenario of theories is beyond the scope of this chapter (for a general introduction see Kozbelt et al., 2010). In the first section, my more limited focus is on those approaches to creativity that have a declared evolutionary perspective. Here, I present a more refined selective scenario that I will apply in the context of science throughout the remaining chapters.

The first section clarifies why creativity is puzzling from an evolutionary and selective perspective. Here, I argue that the proper creative phase might not be selective but rather analyzable within a non-selectionist framework. However, I distinguish between a proper creative phase that is the generation phase of variation and a phase of cultural transmission. It will become apparent that only the latter can be described as an entirely selective process (coinciding with biased transmission in cultural evolution, see Chapter 2). On the contrary, the proper generation phase is hard to characterize as a selective process and might even be seen as entirely non-selective. Nonetheless, I suggest that a more

encompassing evolutionary framework is needed, one that presents the two phases (generative and selective) as both involved in the production of cultural adaptations through different non-selective and selective mechanisms.

In the other two sections of this chapter I show the relevance of these disputes in the context of ‘epistemic selection’ (Mesoudi, 2011) and I defend the applicability of a selectionist model that accounts for population-level change in science. Towards the end of the chapter I first introduce the four methodological questions that, according to my framework, should direct the evolutionary and selective study of scientific ideas.

3.2 Creativity: a selective puzzle for evolutionary epistemology.

To begin with, to understand why creativity is important in cultural evolution, it is useful to analyze the DIT’s selective model limitations. Recall that, in such a model, ‘content biases’ operate on cultural innovations to determine the populational dynamics of change. However, it is immediately clear that such a scenario does not say much about what is causing the emergence of the novelties in the first place (Gabora, 2013). What needs to be specified is if the biases operative in transmission, or some analog of these, also operate at the creative stage (when the innovator is seeking to introduce a cultural novelty) or if they intervene in a later phase, once the cultural novelty is already introduced.⁴⁵ These two phases seem

⁴⁵ This issue is also relevant in the so called “context distinction” in the philosophy of science. Are the epistemic criteria used in the validation of scientific ideas present at the generative stage of innovation or do they rather intervene *post facto* when the novelty is introduced? Popper, for example, thought that the latter was the case and that scientific creativity implied “leaps of reason”. I shall return to this point in the next section.

legitimately different. For example, we can think to the blueprint for the Watt's steam engine. What leads to the generation of this creative idea? The mechanisms and processes through which this novelty was generated and eventually implemented as a new artifact or technology. These mechanisms are responsible for what I call the "generative phase".

However, the generative phase with its underlining mechanisms, does not say anything about the mechanisms and processes that make that innovation (once it is generated) spread in the population of alternative variants within a culture. In fact, for example, the generation phase does not account for the process that made the Watt's engine a successful alternative to its predecessor, the Newcomen steam engine. To account for this second type of change, that I call populational-change, we need to consider a different phase, with different causal factors and mechanisms involved. I call this second phase the transmission phase which is the one responsible for the populational success of an idea once generated. Only this latter phase can be easily accommodated within a selectionist model similar to that presented by cultural evolutionists. For example, human preferences that target certain characteristics of the Watt's engine (e.g., its greater efficiency), made this technological idea⁴⁶ spread and be preferentially copied and adopted by other individuals.

On the contrary, the generative phase is harder to characterize as a fully selective phase and a combination of selective and non-selective processes (or even a fully non-selectionist process) might better represent it. This issue bears significance for a general selective model of cultural evolution and scientific change because if scientific creativity is

⁴⁶ The concept of technology is variously defined. Lipsey et al., have defended an "ideational" characterization. As they put it: "Technology is not just the tools themselves, nor is it simply their use; it is the ideas or specifications of which the tools are embodiment" (...). Technological knowledge, technology for short, is the set of ideas specifying all activities that create economic value. (Lipsey, Carlaw, & Bekar, 2012, pp. 57-58)

non selective that might legitimately question the foundations of the entire selective account of science defended here. However, I argue that a selectionist phase of cultural evolution is present no matter how we characterize the generative phase. Furthermore, as I will discuss in the next section, my distinction between these two phases gives new insights to understand the so called ‘context distinction’ in epistemology and the philosophy of science.

Philosophers have long debated how the epistemic standards that are present at the level of creative discovery of a scientific innovation are relevant in the latter phase of its ‘justification’. A clarification of this epistemological issue might derive from a better understanding of how the generative and the transmission phase of cultural evolution relate to each other.

Going back to the generative phase of cultural innovation, it is illustrative to consider the objections that researchers in creativity have moved to a selectionist account of this phase (Gabora & Kaufman, 2010; Gabora, 2013; 2017). In fact, there seems to be a number of important disanalogies between the mechanism that introduces variation in biological evolution on one side and creativity (which analogically, is taken as the process that introduces variation in cultural evolution) on the other. To start with, when the biases are thought to operate in the generative phase, this departs from the biological model, because biological mutations (analogous to creative innovations) are not biased or directed in any meaningful sense. They are usually considered to be “random”.⁴⁷ These researchers argue that if the production of creative outputs (analogous to biological mutations) is biased *ab initio* by innovators that are trying to innovate in certain predicted directions, that means that

⁴⁷ I shall return later to the concept of ‘randomness’ in the generation of cultural novelties. The application of this term is far more complicated than a vague characterization of ‘directedness’.

cultural creative outputs depart from randomness, a key requirement not only of natural selection, but selection more generally. As Gabora (2011) explains:

Natural selection *assumes* randomly generated variation; that is, if Darwin's theory is to be applicable to a phenomenon, it is required that agents of change at the generation phase, i.e. *prior to* selection, be *negligible*. Deviations from randomly generated variation render selectionism inapplicable as an explanatory framework because evolutionary change in the distribution of variants over time is then attributed to the *nature* of those biases, *not* to differential selection amongst variants. (Gabora, 2011, p. 156)

There is undoubtedly some truth to this criticism. Strictly speaking, the creative phase of cultural evolution (that is, the way new novelties are introduced in the pool of variants) is not analogical to natural selection. However, the crucial question becomes: is this element of disanalogy (with respect to the generative phase) enough to undermine the applicability of a selective phase to the evolution of culture and, in particular, scientific ideas? Here, I argue that non-randomness in the generative phase is consistent with there still being an explanatory contribution by selection in the transmission phase. The degree to which each phase explains the final adaptive result is an empirical matter and not a theoretical one.

Thus, the framework advanced here suggests that cultural adaptations result from the interplay of two phases: the generative phase (P_g) and the selective phase (P_s). To each phase a number can be assigned on a scale from 0 to 1 ($0 > P_n > 1$) that represents the explanatory contribution to the final adaptive result (i.e., the distribution of variants). The adaptive result α becomes the sum of each contribution ($\alpha = P_g + P_s$). There might be cases where the contribution of P_g to the production of the adaptive result α is very low or non-existent and cases where it is paramount ($P_g = 1$) as Gabora suggests. I argue that this evaluation of the explanatory contributions of generation and selection as a matter of degree better captures the interrelation between a generative phase and a selectionist phase solving a number of

theoretical disputes in cultural evolutionary theory.⁴⁸

A key notion that we must examine carefully is that of randomness as it applies in biological mutation and the generative phase (P_g) of cultural evolution. As I have stated above, researchers in creativity that object to the applicability of a selective framework to cultural evolution contend that mutation in biological evolution is ‘random’ while it is ‘non-random’ in cultural evolution. The term ‘random’ is often used to express independence from the effect. According to this meaning, mutations in biological evolution are “random with respect to (i.e., independent of) the effects that they produce” (Hull, 2001, p. 55). What does this mean more precisely? “Randomness” seems to imply the two related notions of (1) non-predictability and/or (2) equal probability. Let me first scrutinize the applicability of these notions for what concerns random variation in biological evolution.

Non-predictability implies the idea that the occurrence of mutation is totally unpredictable and equal probability implies that it has the same chances to occur when compared to another mutation. However, this is often not the case in biological evolution. For example, there are biological mutations involved in the production of certain traits (e.g., melanic forms) that appear more frequently in certain groups of organisms. Furthermore, some “hot spots” in the genome have higher rates of mutation when compared to others (Hull, 2001, p. 54). Thus, violations of “pure randomness” do exist also in biological evolution. This shows that “pure randomness” is not necessary for selection to play an explanatory role in a process of evolution. What then is necessary? I would suggest that

⁴⁸ Moreover, this model allows me to (1) better locate the specificity of science within an evolutionary and selective framework for evolutionary epistemology and (2) to solve some important controversies in the philosophy of science (e.g., the context distinction debate). I shall return to this issue in the next section.

what is required is a significant element of chance. For example, while mutations may be biased in favour of particular locations on the genome, *within* that location it is a matter of chance which particular mutation will result. This can be expressed as a determination of a range of possibilities (the biasing part), plus a probability distribution on those possibilities (which may be an equal-probability distribution).⁴⁹ Nonetheless, we are usually eager to admit with approximation that non-predictability and equal probability apply to most mutational events in the biological domain. As Gabora put it:

Natural selection works by generating numerous possibilities through a process that can be approximated by a random distribution, such that often enough at least one variant is bound by chance to be fitter than what came before, and likely to be amplified in subsequent generations. (Gabora and Kauffman, 2016, p. 635)⁵⁰

Going from the biological to the cultural domain, how can we make sense of the notion of randomness in cultural evolution? First, it must be emphasized that cultural innovations (analogous to biological mutations) can also be fortuitous and random. The generation of new trials is not always biased *ab initio* but sometimes occurs also by chance, for example, when accidental copying errors or misunderstandings occur (Gabora, 1998).⁵¹ I have already mentioned a clear example of random innovation in the context of science: Fleming's discovery of penicillin (see note 27). Thus, the critique that cultural selection does not occur because the generation of variance is not random is unjustified on the ground that random generation of variants is indeed sometimes produced.

However, to a closer look, it is not immediately clear what is the relationship between

⁴⁹ Thanks to Dan Ryder who help me to clarify this point.

⁵⁰ In another passage, Gabora clearly acknowledges that "Biological variation is not genuinely random; for example, we can trace the source of some mutations to certain causal agents. However, the assumption of randomness generally holds well enough to serve as a useful approximation" (Gabora, 2013, p. 124).

⁵¹ For other examples of random mutations in cultural evolution, see Mesoudi et al., 2013 p.198.

randomness and purposefulness/ directedness. Recall that Ruse (2012) states that the main disanalogy between biological evolution and selection and cultural evolution is represented by the notion of randomness intended as the absence of purposeful design.⁵² However, as Kendall and Babington Smith (1938) noted “randomness must be sought elsewhere than in the negation of purpose”. In their words:

It may be desirable to emphasize at the outset that statistical randomness cannot be defined in terms of absence of design. For instance, it is not legitimate to define a method of sampling as random if it follows no law of choice. So far as is known, the irrational number π gives a series of digits which are random in a certain sense. Nevertheless, this series is produced by a purposive process, and is, in fact, given by a comparatively simple law of formation. The number is not random, but it may generate a random series of digits. The suggestion has been made that to require a series of objects to be chosen without following any law is itself to lay down a law of choice. This, we think, is a logical confusion, but the fundamental thought behind the remark is sound—namely, that a definition of randomness must be sought elsewhere than in the negation of purpose. (Kendall and Babington Smith, 1938)

Given the rather complex meaning of this term, it is not very well characterized in the literature on creativity and cultural evolution.⁵³ I suggest the characterization given above might work quite well. According to my definition, the biasing factor (whether purpose or something else) determines a range of possibilities, and the random element is characterized by a probability distribution over those possibilities.

According to this view, randomness seems to require comparisons. A creative innovation is random (or not-random) only when it is compared to a framework of reference either being the general domain of the innovation (range of possibilities) or other innovations

⁵² As Ruse put it: “Darwinism [requires that] mutations (...) are random, in the sense that they do not occur according to need. Even if they are just what is required, there was no design behind their appearance. And yet although it is certainly possible that the new variations of science be random (...) by and large they are the epitome of design” (Ruse, 2012, p.135).

⁵³ I am not able to find a fully developed definition either statistical, mathematical or philosophical of the term “random” in the literature examined for this section. When present, the term is used in a rather intuitive and loose way, similar to Ruse’s definition reported in note 48.

(probability distribution of possibilities). For example, the generation of advertising jingles when compared to the range of all possible jingles might preferentially (thus, non-randomly) cluster on specific pitches and BPM (e.g., upbeat rhythms). In this sense, the generation of jingles with certain adaptive features is biased *ab initio* and non-random. Very likely, advertising jingles have higher probabilities to cluster in specific areas of a chart representing pitch in the *Y* and BPM in the *X* axes. Furthermore, we can grant that the generation is purposeful and directed because the innovator is trying to solve a specific creative task.

However, whenever we compare the probabilities of generation of each jingle the non-randomness might change in this new frame of reference. In the case where there are two or more jingles with equal probabilities of being generated, the generation of one creative solution will be random compared to the alternatives. In this sense, each alternative has been randomly generated. Thus, directedness explains the range of possibilities, it is more similar to the constraints or restrictions that bias the space of possibilities, but this does not preclude the presence of randomness (see Figure 3.1).

Note that in this scenario, a fully biased, directed and clairvoyant process would require an assessment of the relative probabilities of each solution and a choice that produces the bias. If two solutions, all other things being equal, have the same probabilities of being generated, but the creative agent is not aware of the equal probability of the alternative, in that case, almost by definition, he resorts to a not fully sighted and random choice.

Notwithstanding the non-randomness accountable for the general domain. In this case, the choice is rather “blind” respect to equally probable alternatives.⁵⁴

⁵⁴ This better explains Campbell’s notion of ‘blindness’ presented in chapter 1 which is not a synonym of pure randomness as it has often been misunderstood.

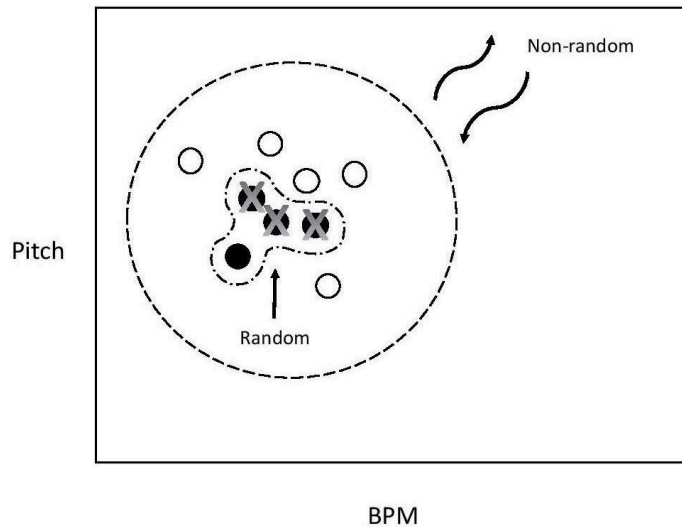


Figure 3.1. Random and non-random domains of variation in the generative phase. The black dots represent equally probable creative attempts.

The relative probability of each solution cannot be taken as a given on the mere ground that creativity exhibits “directedness”. Directedness or purposefulness does not grant that other solutions might or might not have had equal probability of being generated. Thus, to advocate for “directedness” as an indication of non-randomness does not seem justified. The property of directedness (which presents problems of its own as Kendall and Babington Smith explain) does not confer maximum likelihood of a given creative solution at the expense of other trials. On the contrary, the directed solution (non-random respect to the domain of innovation) might still be randomly generated with respect to alternative solutions with equal probabilities in the same creative domain. For all these reasons, it is plausible to admit that both randomness and non-randomness seem to have a place in the generative phase of cultural evolution.

At this point, the relevant question becomes: how can selection then operate on a variety of solutions that are randomly created in this sense? Here, a selective model might

easily apply. Preferences located in the creative individual's mind would create a selective pressure to choose between the alternatives (see Figure 3.1). However, it must be noted that other scenarios do not support a fully selectionist account, for example, when a single creative solution is the only possible creative outcome within a domain (Figure 3.3). In such a case, when we lack the requirement of variation, we have to conclude that selection does not occur. However, the degree to which the solution is arrived at through a fully sighted process depends on how we define "sightedness". If we use Simonton's blind-sighted continuum (see note 21), full sightedness is granted only when the innovator is fully confident ($P = 1$) of the successfulness of his/her innovation and the innovation is indeed successful.⁵⁵

So far, I have talked of selection at the generative level. Now our attention needs to go to another level and another type of change which is the transmission/population change that cannot be explained by what happens in the generative phase. Here, a clear cut distinction is necessary. Generative change is imminently different from transmission change. We saw how the generative phase might contain random and non-random elements but transmission change does not require an account of the generation at all. All that is required is that variation is introduced within a population and that some variants differentially reproduce over time. What accounts for the success of those creative attempts once they are introduced in the pool of variants? To answer this question, we need first to address the concept of cultural fitness.

⁵⁵ Most researchers on creativity include an "utility function" of some sort in their analysis. However, there are exceptions. In particular, those researchers that have emphasized the so called small C creativity (the daily or even solipsistic creative processes), think that the study of creativity does not necessarily require an utility or success function. It must be emphasized that, Gabora's model does not present an utility function. From there, she dismisses a preponderant role played by evaluation criteria located in human cognition that are the main selective mechanism in cultural evolutionary theory.

From a replicator' eye view, cultural fitness is based on the change in the replicator's frequency between t and $t+ 1$. Going back to my example, the fitness of a jingle is determined by the ability to be copied, stored and reproduced more frequently than other jingles (Blackmore, 1999). Here, I argue that a better way to calculate the cultural fitness of a replicator is to count just one variant per biological organism as proposed by Ramsey and De Block (2015), who distinguish between cultural growth (quantitative increase of replicators) and cultural reproduction (transmission between individuals).⁵⁶ As they explain:

[T]he currency of cultural fitness is individual organism's adoption of cultural variants. This does not mean that the number of copies of the T-shirts, or number of images of the shirt posted on Facebook, are irrelevant—more T-shirts or T-shirt images can increase the probability of their adoption—it is that these copies are not what is to be counted in fitness determinations. The focus is on the number of organisms adopting the variants, not the number of variants. Put another way, while meme copy can serve as evidence for cultural fitness, it is not definitive of cultural fitness. (Ramsey and De Block, 2015, p. 10).

What is important here is that the generative phase cannot account for the cultural fitness of an ideas thus conceived. The generation of a creative attempt does not automatically grant populational success and a great deal of creative attempts, for a number of reasons, are not retained in humans' minds and are selected out. Cultural evolutionists defend the idea that once the cultural novelty is introduced in the population of variants (no matter how we characterize the generative phase) this is subjected to a selective dynamic according to human preferences. As we saw in chapter 2, human preferences represent the selective forces that cultural evolutionists describe as context and content biases.

The total obliteration of this phase for what concerns a general cultural evolutionary

⁵⁶ To adopt the perspective of the individual organism to account for cultural fitness has a number of advantages also for the clarification of the notion of "generations". I shall return later on this point.

model is not justified. In fact, even in those cases where the generation of novelty is totally biased, directed and non-random at the generative stage, this clearly does not account for what makes the novelty spread in the pool of variants. In other words, there is a difference between generative-change and population-level change. The latter seems to better fit a selective model while the former does not necessarily have to do so. For example, as we have seen above, the generation of new variants might not be selective when operating in the mind of the creative individual. However, once the creative output is generated, its faith as a successful or non-successful innovation (that is incorporated and transmitted as an adaptive feature) depends on the interaction with other individuals and other minds. These individuals and other minds would exert a selective pressure that will eventually result in an population-level dynamic of change. Thus, it is crucial to distinguish the generation phase from the transmission/replication phase. Only the second, can be treated as a manifest selective process.

At this point, I find it useful to offer an abstract selective model that can be, potentially, instantiated in every selective process and particularly, in the transmission phase of cultural evolution. Following Darden & Cain (1989), I suggest that a typical selection process occurs if (1) a set of preconditions and (2) an environmental critical factor obtain together as to generate (3) differential reproduction of variants *Y*. Their abstract characterization of a selection process goes as follows:

A. Preconditions

- i. A set of *Y*'s exists and
- ii. *Y*'s vary as to whether they have property *P* and
- iii. *Y*'s are in an environment *E* with critical factor *F*.

B. Interaction

- iv. *Y*'s, in virtue of possessing or not possessing *P*, interact differently with environment *E* and
- v. critical factor *F* affects the interaction such that

C. Effect

vi. the possession of P causes Y's with P to benefit and those without P to suffer.
(Darden & Cain, 1989, p. 116).

I want to emphasize that, even upon a quick examination of this abstract formulation, the importance of the 'environment' as a crucial factor of selection stands out. The key selective role of the environment has often been downplayed in many causal accounts of the selective process. In particular, Hull stressed that several accounts of selection put an undue emphasis on replication even though replication alone is not sufficient to characterize a selective process.⁵⁷ In an effort to counteract this limitation, Hull describes selection as formed by two strictly interconnected processes. In his words:

On my account, selection is not one process but two intricately connected processes – replication and environmental interaction. Replication is necessary for selection; it is not sufficient. Environmental interaction is also necessary for selection; it too is not sufficient. Any account of selection must make reference to *both* of these processes (Hull, 2001, p. 2).

Thus, in accordance with this view, Hull defines selection as “repeated cycles of replication, variation, and *environmental interaction* so structured that *environmental interaction causes* [emphasis added] replication to be differential” (Hull, 2001, p.53). The crucial selective role of the environment is also very well represented in Darden and Cain's formulation. When we transfer this model from the biological to the cultural context, the selective role of the critical environmental factor *F* remains crucial. The analogous of the environmental selective pressure in cultural evolution is the pressure exercised by preferences located in human cognition. These content and context biases determine the

⁵⁷ The notion of 'environment' itself has been judged as the most difficult and overlooked notion in selective analyses (Hull, 2001, p.65). For a more extended discussion of the causal role of the environment in selective processes, See Hull, 2001 Chapter 3.

fitness or success of a creative innovation at the population level. This characterization of the selective forces as evaluative criteria located at different levels (e.g., individual, group level) has already been recognized by some creativity researchers. For example, Csikszentmihalyi's (2014) Dynamic Model of the Creative Process incorporates various 'selective' processes: individual, social, institutional. This is why Csikszentmihalyi stressed that:

We cannot study creativity by isolating individuals and their works from the social and historical milieu in which their actions are carried out. This is because what we call creative is never the result of individual action alone; it is the product of three main shaping forces: a set of social institutions, or field, that *selects from the variations produced by individuals those that are worth preserving*; a stable cultural domain that will *preserve and transmit the selected new ideas* [emphasis added] or forms to the following generations; and finally the individual, who brings about some change in the domain, a change that the field, will consider to be creative (Csikszentmihalyi, 2014, p.47).

Csikszentmihalyi's "shaping forces" can be easily accommodated within Hull's account of interactors (see chapter 1) where different levels of environmental interaction (e.g., individual, social circles etc.) cause the preferential replication of a creative innovation. For example, even in the case where a given painting style, say 'impressionism', has been generated through a non selectionist process, this does not tell much about the factors that lead to its affirmation as an influential painting style in the 19th century. To account for this transmission phase you have to involve different levels of interaction *al la* Hull. The wider community is responsible for 'selecting' the specific styles that are deemed as worth preserving.

When we apply the selective abstraction presented above to the phase of cultural transmission, it is clear that such an abstract model applies with no difficulties. In fact, to take a closer look, the abstract model does not require a specification of the process by which the new variants are produced; all that is required is that a creative innovation has already been introduced in a pool of cultural variants. As Darden and Cain put it, "as long as pre-

existing diversity exists, the details of the mechanism of its production can be omitted from a selection type theory” (Darden and Cain, 1989, p.111). This intuition is undoubtedly correct, because when variation is present the selective model works. That is not to imply that selection will account for the entire pattern of change taking place in the system. A non-selectionist mechanism might have been responsible for the generation of the novelty as we have discussed above. However, this does not affect the applicability of a selective model to understand how novelty, once it is generated, spreads in the population of variants interacting with human preferences. Again, there are two distinct ways to look at evolutionary change in the cultural domain. One is ‘change’ in the generative phase that can be potentially non-selective. Another type of ‘change’ is the populational change that measures the success of an idea in its selective interaction with human preferences and biases. Insofar as a transmission/selective phase is responsible for this second pattern of change, the Darden & Cain model will apply. Thus, it appears clear that an abstract selection type theory has an explanatory value for the evolutionary phase that occurs after the introduction of the creative variant.

However, at this point a reasonable objection might be that, as a matter of fact, this selective account leaves out the creative process itself intended as the process through which the variants are generated. It rather concentrates on what happens next, once the creative output is already present. I argue that a selectionist type theory does not demand *explanatory absolutism*. A selectionist model will only apply where the criteria of its general abstraction are met.⁵⁸ If change at the level of the generative phase does not fit a selectionist model the

⁵⁸ Darden and Cain’s model presented above is one possible formulation. I do not claim that it is the only possible abstraction of a selective process. It suffices as a good approximation.

generation phase requires a non selectionist evolutionary explanation. However, population-level change might still occur to a later stage and this seems tractable within a selectionist framework. As such an evolutionary and non-selective framework for the generative phase is still a viable model.

In fact, non-selective accounts of creativity do exist and they have a number of merits. For example, Gabora has defended the idea that the creative/generative phase is governed by an evolutionary process akin to the one present in the early stages of life development, called ‘collective evolution’ (Vetsigian, Woese, & Goldenfeld, 2006). Gabora (2013; 2017) describes an analogical non-selective framework for culture that she has dubbed “communal exchange”. I argue that this framework can be validly applied to the generative phase to understand the biases that operate in the generation of new creative outputs. However, I reject the idea that a non-selectionist framework for the generative phase obliterates the applicability of a selective mechanism in the context of cultural evolution as a whole. In particular, the transmission phase seems genuinely selective.

Carl Woese’s theory of ‘collective evolution’ was first presented in three main papers around the turn of the millennium and was heralded by Woese himself as a new theory of evolution (Woese, 1998). Woese’s theory concerns a framework for reconstructing cellular evolution. He focused on the evolutionary process that predated the formation of the stable and static features of the genetic code that allow the early cells to cross the so called “Darwinian threshold”. This requires cohesion of cellular lineages, something that was not yet developed at earlier stages of evolution. As Woese put it , at this earlier stage “the universal ancestor is not an entity, not a thing. It is a process characteristic of a particular evolutionary stage...” (as cited by Koonin, 2014). Aboriginal cell prototypes present a great

deal of horizontal gene transfer (HGT) which is the primary driving force of cellular evolution at this stage (Woese, 2002). Within this evolutionary scenario the key factor is “innovation sharing, an evolutionary protocol whereby descent with variation from one ‘generation’ to the next is not genealogically traceable but is a descent of a cellular community as a whole” (Vestigian, Woese, & Goldenfeld, 2006, pp. 10697-10698).

Furthermore, and this is key for Gabora’s cultural analogous model, Woese remarks that universality and optimality can be explained by a process that does not involve vertical evolution which is key in natural selection. On the contrary, HGT enhances optimality thus offering an alternative evolutionary model to natural selection. At this stage, cooperation and innovation-sharing are crucial to understand how the entire distributed community benefits from the evolutionary process. Gabora (2013) advocates Woese’s model emphasis on the exchange at the level of the ‘community’ in the context of cultural evolution. She suggests a network-based approach to cultural evolution that models the way larger units, like individual *worldviews*, interact together exchanging their constitutive elements in order to bring up new adaptations. She also suggests to model these dynamics with autocatalytic interactions such as the ones studied by Kauffman for polymers. In Kauffman’s model, polymers’ interactions increase diversity that, in turn, increases the probability to reach a threshold that initiates a catalytic pathway to every member producing ‘autocatalytic closure’ (see Gabora, 2013; 2017). Accordingly, Gabora claims that this model can be viable to explain the restructuring of worldviews in their constant exchange of components. This implies self-organization: *worldviews* self-organize to achieve a state of equilibrium and minimize entropy. Gabora (2013) sees a possible integration of Kauffman and Woese’s frameworks. In particular, she praises the fact that they represent non-Darwinian and non selectionist evolutionary accounts

that do not involve competition and differential survival. As she puts it:

If it took time for natural selection to emerge as the mechanism by which life evolves, it seems reasonable that culture too would evolve by way of this more primitive mechanism. Thus it is proposed that, like these earliest life forms, culture evolves through a non-Darwinian process of communal exchange. What evolves through culture is *worldviews*, the integrated webs of ideas beliefs and so forth, that constitute our internal models of the world, and they evolve, as did early life, not through competition and survival of the *fittest* but through transformation of *all* [italics in original]. (Gabora, 2013, p. 132)

It must be emphasized that both Kauffman and Woese's models apply to molecular dynamics as they generate the emergence of macro-molecular properties. We certainly are in a different analogical domain than that of genes and memes. Accordingly, Gabora argues that the main units of cultural evolution are human 'worldviews' and not 'memes' or 'ideas'. Worldviews are created not just through social learning and individual learning (processes that would more easily fit within a selectionist account) but also through restructuring of existing representations due to an associative mode of thought that can explore a vast array of cultural permutations (Gabora, 2017).

An analogical cultural evolutionary model based on molecular interactions might contribute to solve a number of limitations of cultural evolutionist's selective scenario which has largely downplayed the creative/generative phase of cultural evolution. From the perspective of Gabora's analogy, we can also understand the urgent need to provide an account of how new ideas emerge through the interaction of previously existent assemblages of ideas.⁵⁹ In particular, Gabora's framework of 'communal exchange' has inspired the formulation of the evolutionary framework presented in the next chapters. Here, I talk of

⁵⁹ Gabora's work is also relevant for a psychological analysis of the creative process as operates in several creative domains including science. For a discussion of various psychological traits of creativity see Gabora, 2017.

interdisciplinary matrices (which could be thought of as disciplinary *worldviews*) that interact together in the generation of new scientific posits.

However, even if I think that a ‘communal exchange’ framework has an important explanatory significance in explaining the generation of cultural adaptations, I do not see a radical incompatibility between communal exchange and selectionism. In particular, the incompatibility cannot be granted if we assume a ‘replicator’s point of view’, which I have endorsed throughout this work. In fact, the extent to which selective mechanisms apply in the context of Woese’s theory of ‘collective evolution’ is still a matter of dispute and it has been suggested that his formulation lacks clarity respect to the agency of selection at this early evolutionary stage of molecular interaction. As Koonin puts it:

Woese aigned the Darwinian threshold, the origin of species, with the divergence of the three cellular types (Fig.1). There seems to be some lack of clarity here with respect to the agency of selection at early stages of evolution antedating LUCA [Last Universal Common Ancestor] . The divergence of the cellular domains (...) might correspond to the origin of cellular species. However, competition between and selection of distinct replicating entities is essential for evolution from the outset, thus, a different type of “organisms” (molecular species) subject to selection must have preceded the cellular life forms. Conceivably, such primitive units of evolution could have been represented by small, virus-like replicons that populated abiogenic lipid vesicles or inorganic compartments and were subject to selection for replication efficiency. In the course of evolution, such small replicons would accrete to form larger genomes, those that carried favorable combinations of genes attaining selective advantage. Once such growing replicons reached the level of complexity sufficient for the formation of cells, the major evolution transition to cellular life forms (reproducers sensu Maynard Smith and Szathmary) associated with the viral parasites would occur – Woese’s Darwinian threshold would be crossed. (Koonin, 2014, p. 202)

Thus, if Koonin’s intuition is right, collective evolution might require a notion of replicator or ‘replicon’ intended as molecular species. As such, if we take Gabora’s analogy seriously, it is premature to conclude that ‘communal exchange’ totally undermines the applicability of selective mechanisms especially when thought from the replicators’ point of view. An emphasis on the replicators seems viable both in the context of classical Darwinian

evolution and in the context of “collective evolution”. As such it still needs to be clarified what is analogous of ‘replicons’ or molecular species in Gabora’s “communal exchange” model. An emphasis on wider entities such as *worldviews* analogous to molecular communities does not suffice. Nonetheless, this is not to deny that collective evolution might give important insights to model and understand the interactions between *worldviews* as Gabora suggests. My explanatory cautiousness derives from the fact that until the role of ‘replicons’ is analogously explained in ‘communal exchange’ the judgment about a radical incompatibility of selectionism and communal exchange should be suspended.

Other problems, with a full application of Gabora’s evolutionary but non-selective account remain. In fact, it must be emphasized that Gabora’s model does not only present a rejection of selection in the generative phase but a total rejection of a transmission/selective phase as a whole. On the contrary, my framework advocates a pluralistic view that tries to accommodate selective and non-selective mechanisms both in the generative and transmission phase.⁶⁰ Whether one of the two phases is selective or non-selective becomes an empirical matter and not a theoretical one. More generally, when understood as a totally non-selectionist process, cultural evolution leads to puzzles that are difficult to explain from a radical non-selectionist perspective.

Bio-engineering is a case in point. As Hull (2001, pp.122-123) noted the ‘intentional’, ‘non-random’ biased generation of variation occurs also in other processes that we consider genuinely selective such as artificial selection due to genetic engineering. Bio-engineering seems a genuine case of directed, non-random generation of variation but it is even hard to

⁶⁰ It must be emphasized that the cultural evolutionist’s model also includes non-selective evolutionary forces. Inertial forces and non-inertial but non selective forces (e.g., drift, migration, see p. 42) are part of their evolutionary model.

imagine in what way it is not a case of selection. Even if we concede that the generation of a bioengineered tomato is non randomly directed, how does that affect the spreading of that variant in the market and its competition with other variants according to the economic agents' preferences (e.g., companies, consumers)? In my account, what has led to the generation of a bioengineered tomato (generative change) might not be due to a selective process. However, what leads the tomato's bioengineered blueprint to be preferentially replicated (population-level change) involves a selective interaction with a critical environmental factor F (e.g., human preferences) that biases the diffusion of that variant with given phenotypic properties P (say "durability" that provides better profits for companies). Again, the selective role of the critical environmental factor becomes key here. On the contrary, a non-selectionist account of creativity is burdened with a justification of how the creative, non-random, but non-selective process accounts for population-level change, not just for the generative-change. Thus, for all the reasons presented above, even if we admit that biological mutation and cultural mutation are occasionally different (the former is usually random while the latter is often directed and correlated with the solution that the cultural innovator is seeking),⁶¹ this argument does not rule out 'selection' from cultural evolution as a whole.

Other theoretical problems remain. Gabora points out a number of disanalogies between organic and cultural selection which cannot be fully addressed here. I will discuss two main interrelated objections and show how they do not provide conclusive evidence for the rejection of an evolutionary and selective model of cultural evolution.

⁶¹ Again, these are only approximations in both domains because, as I have discussed above, counterexamples remain. Biological mutation is often non-random, conversely, cultural mutation can be random.

The first objection is that natural selection requires a distinction between inherited and acquired characteristics, where acquired characteristics are (mostly) not transmitted. Culture lacks this distinction thus a selectionist model is ruled out as explanatory (Gabora, 2011). In fact, Gabora claims that “acquired change can accumulate orders of magnitude faster than inherited change, [thus], if [acquired change] ... is not getting regularly discarded, it quickly overwhelms the population-level mechanism of change identified by Darwin; it ‘swamps’ the phylogenetic signal” (Gabora, 2011). The second related objection is that biological generations do not have an equivalent which is analogous in cultural evolution. As Gabora argues:

In biological terms, the fitness of an organism is the number of offspring it has in the next generation. The term ‘generation’ is applicable when individuals are irretrievably lost from a population and replaced by new ones. But with respect to culture, an outdated (or seemingly ‘dead’) idea or artifact can come back into use (or seemingly ‘come back to life’) when styles change or circumstances become right. Because there is no hard and fast distinction between a living entity and a dead one, there is no basis for determining what constitutes a generation. Thus the term generation does not apply to culture. (Gabora, 2011a, p. 74)

To answer to the objection of the absence of generations indirectly responds to the first objection that acquired characteristics must not swamp the phylogenetic signal. To begin with, I argue that generations do indeed exist in cultural evolution. In fact, Hull has argued that the appropriate time frame for replication in conceptual evolution is generational. In his words:

Each time a meme is replicated, that is a generation. Thus, in the course of his biological lifetime, a geometry teacher may replicate the Pythagorean theorem hundreds of times. From the perspective of physical time, conceptual generations are much shorter than certain biological generations and longer than others; but from the perspective of generations *per se*, biological and conceptual evolution take place at the same speed – by definition (Hull, 2001, p. 36).

Hull's point makes clear that this debate boils down to a definitional problem. To state that "generations" require "distinction between a living entity and a dead one" (as Gabora put it) might be a way to describe generations, replication could be another one. However, even if we assume that the applicability of the concept of generations is challenging in cultural evolution or even if the problem of defining generations in cultural evolution would reveal to be intractable this does not undermine the concept of cultural fitness. In fact, Ramsey and De Block have responded to Gabora's concerns on the lack of a cultural analogous of "generations", with the objection that, very often, time-indexing replaces generation-indexing in biological evolution. Thus, the same can be done for cultural evolution (Ramsey and De Block, 2015).

In biology, fitness is often time-indexed: genetic fitness has been characterized as a 'measure of the rate at which allele or linkage-group frequencies change within a gene pool through time' (Wittenberger [1981], p. 614). Time-indexing can be advantageous for biological fitness, and the use of non-generational fitness measures has been defended over and over again (Metz et al. [1992]; Coulson et al. [2006]). (...) Since time-indexing works in such biological cases (and frequently works much better than generation-indexing), then it should work for culture in the absence of (well-defined) cultural generations. We therefore hold that the generation problem for CET is solved by time-indexing. (Ramsey and De Block, 2015, p. 8)

Another possibility that I want to mention here that needs further exploration would be to treat generations in reference to the individual organism. Recall that Ramsey and De Block (2015) suggested to calculate the cultural fitness of an idea referring to the individual organism that holds that idea and not to the replication. It is worth exploring if the concept of ideational "generations" can also be referred to the individual organism's span of life. In fact, memes and ideas die with their carriers no less than the genes do. With the organism's death, both types of replicators, genetic and ideational cease to exist. Copies of them survive elsewhere (other organisms and other minds). A striking case of death of memes due to death

of their carriers with disastrous cultural consequences is illustrated by Diamond's (1978) discussion of the Tasmanian aborigines.⁶² To consider cultural generations in reference to the individual organism would allow (1) to operationalize the common sense notion that ideas are passed from one generation to the other and (2) to satisfy Gabora's criterion that generations require a distinction "between a living entity and a dead one".

Furthermore, a third way to characterize generations that respects Gabora's criteria could be to try to establish a measure of "ideational decay". Even at the molecular level considered by Kauffman and Woese, the criterion of "life and death" might be attributed to the process of atomic decay. Analogically, cultural ideas have a 'retention potential', which can be seen as the probability of the variant being retained over time or alternatively, as the variant's resistance to decay. (Ramsey and De Block, 2015). Thus, I conclude that the generation problem is not a conclusive argument against the dismissal of a selectionist model, particularly when this is understood from a pluralistic "replicator's eye view" (Loyd, 2012).

Now, how this issue of 'generations' bears significance to Gabora's first objection that states that in cultural evolution (due to the lack of a distinction between inherited and acquired change) acquired change accumulates to such a fast pace that 'swamps the phylogenetic signal'? It seems that when generations are understood as time-indexed replications *a la* Hull, replication is seen as occurring at a faster rate than the acquisition of

⁶² Upon their arrival on the island, the first Tasmanians possessed a number of technologies such as the bow and canoes that they subsequently lost due to accidental factors such as the death of those individuals who carried the memes. In such a small population even the death of a single individual can have a dramatic impact in terms of loss of cultural diversity. This reduction of cultural variability represents the analogous of a "bottle neck" genetic effect that is the reduction of genetic variability due to shrinking populations. When the first Europeans reached the island's shores they found the Tasmanians with a very primitive lithic technology, with no bows and canoe; only posterior archeological evidence will show that these technologies were part of the original repertoire of the early settlers (Diamond, 1978).

acquired traits. Hull himself is ready to admit that cases where acquired change swamps the phylogenetic signal do in fact occur (Hull, 2001, p. 35). If this does or does not happen is an empirical issue and not a theoretical one.

On the contrary, Gabora's model seems to start from the theoretical assumption that the rate of acquired change is always greater than the rate of replication.⁶³ From there the inevitable deduction is that the phylogenetic signal is always swamped in cultural evolution. This theoretical assumption is unjustified precisely because this is not a theoretical but an empirical matter. In fact, when replication occurs faster than acquired change (a plausibly empirical assumption not contemplated in Gabora's schemes), the phylogenetic signal is indeed maintained. In other words, if the phylogenetic signal is maintained through replication or not depends on the relative rates of replication and acquired change.

Moreover, I argue that the distinction between inherited and acquired characteristics might be of value also in cultural evolution. Inherited characteristics are the ideational replicators (in the coarse-grained sense described in chapter 2). Acquired characteristics are the interactions between the replicator and other ideas within a discipline, a disciplinary matrix, or for a particular researcher.⁶⁴ In particular, this distinction fits the representation of scientific replicators that adapt to satisfy the epistemic aims of given disciplinary matrices as presented in the next sections and chapters.

Finally, I want to observe that when it comes to selectionist analogies the

⁶³ This can be seen on the schematic depiction of four evolutionary processes as presented in Gabora and Kauffman, 2016, p. 634. All four schematic illustrations of the different evolutionary processes assume the initial parameter that acquired change occurs faster than replication. The lack of a concept of 'generation' intended as replication leads to this theoretical assumption. However, again, the issue if replication happens at a higher pace than acquisition of new traits is empirical and varies case by case.

⁶⁴ Some people understand the term 'inherited' in a restricted sense that requires a self-assembly code. For a definition of "inheritance" based on the von Neumann algorithmic notion of *self-replicating automaton* (SRA) see Gabora, 2013. I am not using the term "inherited" in this technical sense.

disagreement is also definitional. Selectionists and non-selectionists do not agree on the same definition of a ‘Darwinian selective process’. Disagreement is present also among the selectionists themselves. As Turner (2006) noted, everyone has their own formula to try to capture the essence of a Darwinian evolutionary process, which is as follows: “All and only Darwinian evolutionary processes have features F_1, F_2, \dots, F_n ” (Turner, 2006, p. 110).

Thus, for example, cultural evolutionists uses Mayr’s (1959) notion of population thinking to define a Darwinian process. Remarkably, their interpretation of population thinking and Darwinian theory also accommodates Lamarckian processes, non-random variation and other processes that might not have a strict analogous equivalent in cultural evolution.⁶⁵ As Mesoudi (2011) put it:

Whereas cultural evolution does not appear to resemble neo-Darwinian evolution, with its strict assumptions of blind mutation and particulate, non-Lamarckian inheritance, cultural evolution can still be described as Darwinian, given the evidence reviewed above that it exhibits the basic Darwinian properties of variation, competition, and inheritance. Variation that is non-random is still variation, and inheritance that is Lamarckian and non-particulate is still inheritance. (...) What is needed is a theory of Darwinian cultural evolution that explicitly incorporates non-neo-Darwinian microevolutionary processes such as blending inheritance, Lamarckian inheritance of acquired characteristics, and non-random variation, as well as other processes that may have no parallel whatsoever in biological microevolution (Mesoudi, 2011, pp. 46-47).

The cultural evolutionists’ view well resonates with Dennett’s algorithmic view which is comprised of variation, heredity and differential reproduction (Dennett, 1995). Other selectionist accounts have offered other formulas. Hull (2001), as we have seen above, talked

⁶⁵ However, as Hull noted, the objection that cultural evolution is Lamarckian is largely metaphorical and inaccurate. In fact, in Lamarckian evolution, acquired phenotypic traits are passed on into the genotype and from there inherited by the new generations. The genotype /phenotype distinction is key in Lamarckian evolution: the acquired phenotypic characteristics must be coopted into the genotype and inherited and not just transmitted. However, no cultural evolutionist is really claiming that culture is Lamarckian in this sense as if ‘memes’ are passed into the genotype and from there inherited. Moreover, ‘memes’ are not analogous to the phenotypic acquired characteristics but to ‘genes’. Thus, it will be more correct to talk of “inheritance of acquired memes” (Hull, 2001, p. 34).

of replication, interaction and selection. For him, the selectionist formula becomes “repeated cycles of replication, variation, and environmental interaction so structured that environmental interaction causes replication to be differential” (Hull, 2001, p. 53).

Others restrict the concept of a ‘Darwinian process’ to the principle of natural selection, comprising random mutation and differential reproduction (Thagard, 1980; Gabora, 2013; 2017). Thus, in a certain sense, their rejection of selectionism in scientific and cultural evolution seems much more a rejection of neo-Darwinism rather than a wholesale rejection of Darwinian selection.

To conclude, for all the reasons presented above and, last but not least, for the apparent disagreement pertaining to the notion of Darwinian selection, it is prudent to admit that a rebuke of a selectionist framework for cultural and conceptual evolution in science is still premature. Researchers in creativity are right in pointing out a number of disanalogies between cultural and biological selection. However, Darwin’s pluralistic conception gives ground to Mesoudi’s point that this criticism does not represent a case-closing refutation of selectionism as a whole.⁶⁶

Nonetheless, the focus on the creative phase as contrasted with the transmission phase opens up a pluralism of evolutionary perspectives both selectionists and non-selectionists which demand greater attention from the philosophers and epistemologists. In particular, relationship between the generative/creative phase (that operates on the emergence of the creative novelty) and the transmission/selective phase (that govern the diffusion of the novelty) mirrors a contentious dichotomy in the philosophy of science and epistemology

⁶⁶ It must be emphasized that Darwin uses selective breeding as an exemplification of natural selection itself. Moreover, he wrote *The Descent of Man and Selection in Relation to Sex* to introduce another selective theory that must be understood as a middle level selective theory, largely independent from the theory of natural selection. Last but not least, Darwin admitted Lamarckian elements in his selective account.

known as ‘context distinction’. In the next section I turn my attention to this dichotomy to see how it can be accommodated within an evolutionary and selective framework for science.

3.2 The ‘context distinction’ in light of a selectionist account of scientific change.

In this section, I want to show how the evolutionary and selective account defended above has important implications for the so called ‘context distinction’ debate. The ‘context distinction’, which is present in the philosophical tradition at least from Kant (Hoyningen-Hune, 2006), assumes that there is a clear cut divide between the process where scientific ideas are generated (context of discovery) and the process pertaining to their validation (context of justification). Traditionally, only the latter is a philosophical object of study. As Schickore sums up:

[Advocates of the context distinction argued that] the act of conceiving a new idea is a non-rational process, a leap of insight that cannot be regulated. Justification, by contrast, is a systematic process of applying evaluative criteria to knowledge claims ... philosophy of science is exclusively concerned with the context of justification... neither is it possible to prescribe a logical method that produces new ideas nor is it possible to reconstruct logically the process of discovery. Only the process of testing is amenable to logical investigation (Schickore, 2014).

Both the logical empiricists and Popper made use of this dichotomy. Popper, for example, explicitly stated that what leads scientist to discover a new theory is not the business of the philosophers but rather that of the empirical psychologist. From his logical and particularly deductive understanding of the way science progresses, discovery is logically intractable.⁶⁷ In

⁶⁷ Ironically enough, Popper’s famous book *Logik der Forshung* which translates as ‘logic of research’, has been mistranslated as *Logic of Discovery*, even though Popper’s intent is not to offer a logical treatment of

his words:

The initial state, the act of conceiving or inventing a theory, seems to me neither to call for logical analysis nor to be susceptible of it. The question how it happens that a new idea occurs to a man—whether it is a musical theme, a dramatic conflict, or a scientific theory—may be of great interest to empirical psychology; but it is irrelevant to the logical analysis of scientific knowledge. This latter is concerned not with *questions of fact* (Kant's *quid facti?*), but only with questions of *justification or validity* (Kant's *quid juris?*). Its questions are of the following kind. Can a statement be justified? And if so, how? Is it testable? Is it logically dependent on certain other statements? Or does it perhaps contradict them? [...] Accordingly I shall distinguish sharply between the process of conceiving a new idea, and the methods and results of examining it logically (Popper, 1959, p. 31).

On the other hand, Kuhn did not believe in the existence of a sharp contrast between these two moments. For him, considerations relevant to the context of discovery are relevant to the context of justification as well. He pointed out that “scientists who share the concerns and sensibilities of the individual who discovers a new theory are ipso facto likely to appear disproportionately frequently among that theory’s first supporters” (Kuhn, 1977, p. 328). In other words, a set of communal cognitive values both present at the level of discovery and justification concur to determine the outcome of the theory choice situation (Hoyningen-Heune, 2006, p. 127).

I argue that the debate delineated above concerning a generative and a transmission phase in cultural evolution, helps to clarify this matter. Upon a closer look, the ‘context distinction’ mirrors the distinction between the generative phase (governed by the generative biases) and the transmission phase (transmission biases) as it has been studied in cultural evolution. Roughly, the context of discovery pertains to preferences that bias the emergence of novelties while the context of justification more closely corresponds with the transmission biases that govern the transmission and imitation of an idea at the population level (once the

discovery. This would rather be of the inductive or abductive type.

variant has already been introduced in the pool of competing alternatives). These two pairs of dichotomous phases are substantially equivalent (see table 3.1).

Table 3.1. The ‘context distinction’ and the creative / transmission phases in cultural evolution.

	Generative Biases (Origin)	Transmission Biases (Validation)
Context of Discovery	✓	✗
Creativity	✓	✗
Context of Justification	✗	✓
Transmission Biases	✗	✓

As I argued in the previous section, only the transmission phase seems to fully and unequivocally satisfy a standard selective model. For the generative phase, a more complex selective scenario or a non-selective one might be needed. However, in the more specific context of scientific creativity, philosophical analysis has suggested that scientific discovery is intimately tied to confirmation and explanation (Brigandt, 2012; Weber, 2005). In other words, there seem to be common biases that operate in the context of discovery and that of justification. Thus, as evolutionary epistemologists, we are burdened with the elucidation of an evolutionary scenario for (1) the emergence of given scientific ideas and (2) the subsequent preferential transmission of certain scientific replicators over others.

A first step in this direction is to emphasize the role of what Brigandt (2012) has called 'epistemic aims'. Epistemic aims are “assumptions about what issues are currently in need of scientific study and explanation” (Ibidem). Epistemic aims are strictly correlated with the disciplinary matrix (Kuhn, 1962) of a given field or sub-field of science. A

disciplinary matrix is a population of scientific replicators that generates specific epistemic aims in those scientists that belong to a disciplinary community. As such, epistemic aims are the right candidates for the role of biases that operate on the generative/creative phase. For example, Brigant (2003; 2012) studies how the concept of ‘homology’ has been moulded to satisfy specific epistemic aims of different branches of biology in a process that resembles conceptual adaptive radiation. This concept that first emerged in the field of comparative anatomy had as general criteria topological similarity, similarity in structural detail, histology and correspondence of developmental origin. From this disciplinary matrix, the concept ‘radiated’ to different branches of biology, acquiring different characterizations according to the peculiar epistemic aims of each biological discipline. In the context of evolutionary biology, homology became a precondition for explaining adaptations; in systematics it is a diagnostic feature to classify taxa, and in molecular biology, molecular homology seeks to explain how genes and proteins behave (Brigandt, 2003).

It is important to stress that epistemic aims cannot be easily accommodated within the logical positivist's account of science which has put an emphasis on the context of justification alone based on beliefs and revision of beliefs through logical relations. Epistemic aims “operate on a different dimension than scientific beliefs”. They are not representations of the world but epistemic values prompted by given disciplinary matrices. A populational and selective framework is perfectly suited to accommodate the role of epistemic aims. Variation is to be expected at every level, even at the level of individual disciplinary matrices. No two disciplinary matrices are exactly the same in the scientists’ brain. As Mayr’s populationist *monitum* prescribes, to talk of a disciplinary matrix is to refer to a statistical average which is not ‘real’: only variation is real. Nonetheless, when we

compare the disciplinary matrices of particular scientific professions, we expect a greater similarity. For the same reasons, we expect that members of a given branch of science have a greater propensity to share the epistemic aims of their field. Moreover, it must be noted that disciplinary matrices can also offer a resistance to innovations coming from epistemic aims that collide with pre-assumptions of the disciplinary matrix itself. In the next chapter, I will provide a clear example of this type when I discuss the emergence of DNA barcoding within peculiar disciplinary matrices and epistemic aims.

What is important to remark here is that a populationist view allows us to bypass the distinction between a context of discovery and a context of justification. The context of discovery and the context of justification are treated as interconnected phases of a partially selective process.

3.3 Toward a ‘dynamic selective realism’.

In this section, I want to address a crucial topic already encountered in the previous chapters. This topic is scientific realism. Particularly, I want to limit my analysis to the significance of a realist stance for an evolutionary and selectionist perspective of scientific ideas.

The main obstacle for an evolutionary and selective understanding of science comes from the fact that selection is the product of a reiterative and recursive process between two main elements - human cognition and the external reality. To disentangle the recursive selective dynamics that link the two sides has been revealed to be a very hard task. The evolutionary epistemologies presented in chapter one have just highlighted partial aspects of

these selective dynamics but they have not provided a comprehensive scenario that fully clarifies the selective role acted by both parts: cognition and the external reality. In fact, it turns out that both human cognition and external reality play a selective role. Campbell (1974) came closer to a more comprehensive formulation of this relationship. He showed that 1) cognition is organized with many nested selectors and accumulated knowledge that shapes the selective landscape of solutions and 2) that the ‘referent’, the external reality, is another crucial factor of selection.

Concerning point one, I have already defended Campbell’s fundamental intuition that human cognition is organized by a number of preselected processes (vicarious selectors in Campbell’s terminology) that generate guidance. The primary selective environment of a scientific (or cultural) idea is the pre-existing net of ideas with which a new replicator interfaces. This goes back to Dennett’s formulation of the selective environment of a meme as “minds and other memes” (see chapter 2, p. 48).

Moreover, the scientific replicator is also selected by the external reality and the elucidation of this second selective relationship is mandatory for every evolutionary epistemology (Campbell, 1990).⁶⁸ Here, I propose to see the relationship between the external referent and our cognitive posits as a reiterative and recursive selective ‘game’ between Nature and the scientist. On one side, we have the scientists’ trials with a certain degree of probability of being correct. On the other side, Nature, the physical world, selects some trials, when, for example, experimental results confirm a certain hypothesis (justification/transmission phase). This is a more passive selective role where the external

⁶⁸ Campbell noted the similarities of his selective realism with Goldman’s reliabilist epistemology, which posits a causal chain between the external referent and justified beliefs (See Campbell, 1990, pp. 14-15).

reality acts as a 'sieve'. However, the physical world has also a more active role in selection by providing new clues for the generation of new solutions (creativity/discovery phase) (Martin and Osherson, 1998). Aspects of the external reality that are elucidated by pursuing certain epistemic aims prompt new theoretical and conceptual explorations that were previously uncharted. New observational or experimental evidence that does not fit with the pre-existent theoretical background generate a state of cognitive dissonance in the scientists' minds that pushes them to find new theoretical solutions.

To give a concrete example of the way reality motivates new trials on the scientist's side, we can look to the ground-breaking discovery of proteins' structure in early molecular biology. In the late 50s, Francis Crick predicted the development of a discipline called 'protein taxonomy' as "the study of the amino acid sequences of the proteins of an organism and the comparison of them between species" (as cited in Strasser, 2011, p. 323). However, the term 'protein taxonomy' was ill chosen because Crick's epistemic aim (which he shared with the other molecular biologists of his time) was not to answer evolutionary or phylogenetic questions using the structure of proteins. On the contrary, as Strasser noted, the "major intellectual agenda among biochemists and early molecular biologists" was "to assess the relations between structure and function in proteins" (Strasser, 2011, p. 323). Thus, early molecular biologists were trying to explain how the structure of certain proteins made them function in certain ways and not others.

Nonetheless, and this is a crucial point, once the 'structure of protein' was elucidated it could legitimately serve other epistemic aims of other scientists. In the early 60's, Emanuel Margoliash "introduced the revolutionary idea that organismal relationships could be deduced from such macromolecular data" (Avisé, 2014, p.67), creating the first phylogenetic

tree from genetic material (Awise, 2004, p. 52). In other words, once the structure of proteins was elucidated, there was an observational base for the proteins' structure to interact with wider theoretical replicators (or complexes of replicators) that were already present in the scientists' mind (e.g., phylogenetic trees or cladograms). The subsequent interaction led to the merging of these replicators in a new interactor that was eventually subjected to a selective process (e.g., validation, acceptance, cooption within a stable repertoire).

This example gives a clear understanding of the more active role that external reality plays in promoting the emergence of new theoretical trials on the scientists' side. Taxonomic classification was not the main agenda of early molecular research but once the structure of proteins was on the table, new opportunistic applications could be explored. Here, the physical world appears as an 'active' counterpart in the extension of new theoretical possibilities.

Thus, the selective relationship between scientist's cognition and the physical world can be understood only by means of what I describe as a *dynamic selective realism* (SDR) where selective pressures are exercised in multiple directions: from theory to the physical world and from the physical world to theory. The evolutionary dynamic is not unidirectional from theory to experiments and observations as the "theory laden" conception of science has suggested. The idea that elements of theory are present in any set of observations and that the theoretical phase has priority over observation is an overgeneralization.⁶⁹ On the contrary, each moment the theoretical, observational or experimental can be the starting point of a possible dynamic of scientific exploration (See table 3.2). Thus, an SDR can have important

⁶⁹ This is not to deny that some scientific fields might be more theoretically inclined. I am just arguing that all possible directions can be explored from theory to observations and from observation to theories. Again, theories are ideational posits that entertain a selective relationship with the external reality.

implications for the philosophy of science. It helps to bypass polarized controversies over which phase has priority. It also maintains that all paths are open to exploration according to the fundamentally trial and error, strategic and opportunistic nature of science (Campbell, 1974).

Table 3.2. The dynamics of scientific activity. *Source:* Adapted from Wilkins and Ebach, 2014, p. 14.

	Experiment	Observation	Theorizing
Experiment	Revision and correction	Experiment can restrict or guide “naive” observations	Experiment can restrict theoretical range, or disconfirm theory Experiment can suggest theoretical category-concept
Observation	Naive observation can influence the data used in experiment	Revision and correction	Theoretical predictions can fail to be borne out in observation Theory can be based on naive observation made pre-theoretically Observation can suggest theoretical category-concept
Theorizing	Theory can specify legitimate experimental protocols and approaches	Observation can depend upon the ontology and methodology of a theory	Revision and correction

This means that scientists are not constrained in what they can observe merely by theoretical commitments, nor they are unable to observe phenomena because theory does not permit it (Wilkins and Ebach, 2014).⁷⁰

⁷⁰ Wilkins and Ebach give the example of early taxonomic classification that emerged from the medieval herbalist tradition without a background scientific theory. Sometime, the species that these herbalists classified without any phylogenetic notion were revealed to be quite accurate, even today. One might argue that classification is a peculiar epistemic enterprise when compared to other scientific enterprises. However,

3.4 Four methodological steps to study the evolutionary dynamics of science.

In this section, I advance a four step methodology to investigate scientific innovations from a selectionist and populationist point of view. In the next chapter, I apply this method to a circumscribed scientific innovation called DNA barcoding. The study of DNA barcoding is presented as a sort of ‘evolutionary case study’ and is aimed at offering a procedural model of research for the evolutionary epistemologist. These methodological steps synthesize most of the topics discussed in the previous chapters. Particularly, the method closely resembles the populational method defended by the cultural evolutionists.

To begin with, I find it useful to talk of four methodological steps that guide the evolutionary analysis of science. These four steps can also be framed as four methodological questions - *What? Who? Why? Where?* - that guide the analysis.

The first step addresses the “*What ?*” question. We start isolating the scientific replicator to study its dynamics of emergence and diffusion. Recall that the populational method of cultural evolutionists starts isolating a cultural variant in order to track its populational dynamics (See chapter 2). Once the replicator is isolated, we try to characterize its ‘nature’. We try to define whether the replicator is an observation, an experiment, a technique, a theoretical concept, a larger theory and so forth. This stage requires some classificatory work. Here, the traditional lexicon of the philosopher of science is

other examples that do not conform to the ‘theory laden’ account abound in the history of science. For example, science sees also accidental discoveries, such as the often cited case of Alexander Fleming’s discovery of penicillin, who accidentally noted that the *Staphylococcus*’ colonies present in some abandoned petri dishes were inhibited by the growth of mould. Other examples are experiments that unexpectedly do not conform to the dominant theory and in the long run motivate a theoretical revision. For example, the Michelson-Morley experiment contributed to the falsification of the hypothesis of a “luminiferous aether”, positing, in turn, an experimental and observational cogency that will motivate the revision of an entire theory (Galilean relativity).

provisionally useful, even if the evolutionary epistemologist has to be aware of the limitations of the lexicon used by philosophers. In fact, as Churchland (1989) noted, the traditional approach in the philosophy of science has mostly endorsed a ‘sentential epistemology’.⁷¹ In short, a sentential epistemology looks at scientific representations as ‘beliefs’ or ‘sentences’. On the contrary, I am arguing here that the object of study of an evolutionary approach are scientific ideas and not merely sentences. Scientific ideas or ‘memes’ are more complex representational units than sentences. This is not to deny that ideas cannot be expressed in sentences or that logical or linguistic tractability is not crucial in science. All that is argued here is that ‘ideas’ do not reduce to sentences and that a populationist approach is not limited to highlighting logical relationships with sentences.

At this early stage of investigation, we also try to sketch the possible path of discovery as presented in the dynamic selective model discussed in the previous section. It can be a new theoretical concept suggested by new observations and data, or the other way around, new observations or experiments suggested by new theoretical hypotheses. In other words, we try to disentangle the reiterative dynamic between scientists’ theoretical posits and the physical world. We try to isolate a dynamic of emergence, keeping in mind that different paths of exploration are possible from theory to observation and vice versa. This respects the opportunistic character of science advocated throughout this work.

The second step answers the “*Who?*” question. Who is the innovator? To which sub-field of science does the innovator belong? This is a crucial point because it connects the

⁷¹ “In urging the poverty of ‘sentential epistemologies’ for over a decade now (...), I have been motivated primarily by the pattern of the failures displayed by that approach. Those failures suggest to me that what is defective in the classical approach is its fundamental assumption that language like structures of some kind constitute the basic or most important form of representation in cognitive creatures, and the correlative assumption that cognition consists in the manipulation of those representations by means of structure sensitive rules” (Churchland, 1989, p. 60).

discovery to the innovator's disciplinary matrix (Kuhn, 1962). As I have discussed above, the disciplinary matrix is a sort of disciplinary 'world-view', a collection of replicators arranged in certain ways that the scientists learn through exemplars in the pedagogic environment of their discipline. Recall that according to the populationist framework, each scientist's disciplinary matrix presents variation. No two scientists are expected to host the exact same disciplinary matrix or to execute algorithms of theory choice in the exact same way (See chapter 2). Nonetheless, we expect that disciplinary matrices that belong to the same sub-discipline are, on average, more similar. For example, the ecologists' disciplinary matrices are expected to be on average more similar when compared with the disciplinary matrices of geneticists. This is because the two communities share common standards, methods, concepts and so on; in other words, they share a population of replicators, a disciplinary matrix.

Thus, the isolation of the innovator's specific disciplinary matrix allows the evolutionary epistemologist to link the discovery to the wider context of ideas that the individual scientist shares with his peers. At this stage, we try to investigate how the advanced innovation fits with wider general assumptions of the field. However, and this is a key point, the innovation can also be suggested by what is lacking in the disciplinary matrix not just by what is present. For example, new data or observations can motivate new theoretical posits in those disciplines that lack theoretical commitments that would be an obstacle to innovation.⁷²

The third step, which is arguably the most important, investigates the 'Why?' question

⁷² Note how this agrees, with Dennett's formula of ideas having 'phenotypes' that disable or pre-empt the environmental forces that would tend to question them (See chapter 2, p....). Thus, previous commitments can hamper innovation.

- the explanatory intent of a scientist or a group of scientists in advancing a certain innovation. It is clear that this issue is strictly related to the second step, pertaining to the elucidation of the disciplinary matrix. In fact, previous commitments are expected to differently orient a sub-community of scientists. At this stage, some form of potential conflict is expected between different disciplinary matrices of different branches of science. Sometimes a given innovation and its epistemic aims collide with theoretical, methodological, evidential etc. standards and assumptions of other neighboring fields that share those same epistemic aims. Conflict arises because the replicator threatens or questions some stable elements of their disciplinary matrix.

Finally, the ‘*Where?*’ question addresses the populational dynamics. How the replicator spreads and it is evaluated by different disciplines according to their different epistemic aims. This is the stage of analysis where we evaluate the role of transmission biases that might guide the populational dynamics of diffusion of a scientific idea. Even at this level, conflict or resistance is to be expected, particularly in the early phases of diffusion, when a replicator is submitted to the evaluation of the larger community of scientists where different disciplinary matrices are present.

3.6 Summary

In this chapter, I have further explored the implications of an evolutionary theory of creativity as it bears significance for an evolutionary and selective cultural framework of scientific development. Here, I claimed that an evolutionary and selective model helps to

rethink traditional epistemological disputes such as the ‘context distinction’, the theory-ladenness of science and a notion of ‘selective realism’. In the final section, I have introduced the four methodological questions that I will apply in the next chapter to a specific scientific innovation called DNA barcoding. This molecular technique and the debate that followed its introduction can be very well accommodated in the selective and dynamic model delineated in this chapter. Thus, the next chapter is intended to show the explanatory validity of my wider evolutionary and selective framework delineated so far.

Chapter 4

DNA barcoding: a preliminary case study in the evolution of science

4.1 Premise

In this chapter I apply the four methodological questions to the analysis of a standard for taxonomic identification and classification known as DNA barcoding. First, I explore the nature of this innovation in relation to the disciplinary matrix where it was advanced (that of geneticists). Secondly, I connect this innovation to the peculiar epistemic aims of those biological disciplines where the technique had a greater impact and acceptance. Opposition to the technique, coming mainly from taxonomists, is also evaluated in an effort to highlight the theoretical and methodological presuppositions that generate conflict once a novelty is introduced within a field. The origin of these controversies is related to the different disciplinary matrices and the different reciprocal views of the nature of the taxonomic work. For example, taxonomists see their discipline as theoretically driven not less than other biological fields while other biologists tend to see taxonomy a “descriptive” discipline.

In this chapter, I also evaluate the degree to which DNA barcoding has impacted more theoretical issues in taxonomy known as the “species problem”. Here I claim that my evolutionary model allows to present a coherent historical and evolutionary scenario for the dynamic interaction of DNA barcoding (as a replicator) with wider theoretical assumptions. Even if DNA barcoding does not contain a theory of species, as an operational criteria used to arrive at a new species hypothesis, it has impacted the “species problem” exactly because

the species problem is not merely a theoretical issue. The species problem is a dynamic aggregate of theoretical and operational criteria in search of a composition, a stability of referential relations between scientists' ideas and the external reality. DNA barcoding participates within this dynamic and it is pushed forward from a peculiar disciplinary matrix with peculiar epistemic aims.

4.2 Applying the four methodological questions to the DNA barcoding case.

Step 1: The 'What?' question.

It is often remarked that since the early 1960s the use of molecular data has largely reshaped the field of taxonomy, which was previously dominated by comparative methods based on morphology and behaviour alone. During the last forty years (and increasingly since the introduction of the revolutionary polymerase chain reaction (PCR) in the early 80s), the use of molecular data has become widespread and increasingly important for taxonomists and systematists (Avise, 2004).⁷³ Dna barcoding makes use of molecular markers to infer taxonomic identifications and classifications, but its element of novelty lies elsewhere.

The idea behind DNA barcoding was that a relatively short (648 base-pair) fragment of a mitochondrial gene (known as cytochrome c oxidase 1 (CO1)) could be used as a

⁷³ There is not widespread agreement on the precise meaning and denotation of the terms 'systematics' and 'taxonomy'. A rather vague but prevalent characterization maintains that taxonomy is more concerned with the naming process, while systematics is concerned with phylogenetic classification. For the purpose of my analysis it is not important to trace a clear cut distinction between the two. Thus, I will generally use the term taxonomy to refer to both scientific enterprises. For a general account of this terminological debate, see Wilkins and Ebach, 2014, chapter 2.

standardized region of the genome, as a sort of barcode, to assess phylogenetic relationships (Hebert et al. 2003). The use of molecular markers for taxonomic identifications was already established when DNA barcoding was first advanced. Thus, the isolation of standardized loci itself marks the innovative character of DNA barcoding. As Moritz and Cicero noted:

Given the long history of the use of molecular markers (e.g., Allozymes, rDNA, and mtDNA) for these purposes (Avice, 2004), there is nothing fundamentally new in the DNA barcoding concept, except increased scale and proposed standardization (Moritz and Cicero, 2004, p. 1529).

Step 2: The “Who?” question.

DNA barcoding was first introduced in 2003 by Canadian geneticist Paul H. Hebert and colleagues at the University of Guelph in a paper titled *Biological identification through DNA barcodes*. Thus, we should investigate if there are particular circumstances that favoured the emergence of this technique within the geneticist’s disciplinary matrix. Significantly, in the same years, another group of geneticists was independently working on the very same idea. A similar technique under the name of Universal Primer Technology (UPT) (Verma and Singh, 2001) was advanced. The two groups were not mutually aware of their work so that DNA barcoding and UPT can be seen as a case of convergent innovation.⁷⁴ Nonetheless, for some reason, only DNA barcoding sparked a greater interest, while UPT remained largely unknown to the wider scientific community. It has been suggested that the attractiveness of the term ‘barcoding’ itself might have played a positive selective role:

⁷⁴ The difference between the two techniques, apart from the name of course, is mostly due to the proposed use of different mitochondrial standardized areas (Cyt-b in UPT instead of Cox-1). However, given that DNA barcoding has expanded to include different areas, the paternity of the idea is legitimately contested.

It is likely that the mere use of the word barcode is responsible for much of the appeal surrounding DNA barcoding, after all DNA-based identification methods (...) used prior to Hebert et al.'s (2002) proposal of the term failed to ignite significant attention from the scientific community and none whatsoever from the general public (Cameron et. al., 2006).

It is possible that an ‘irrational’ factor, such as the attractiveness of a name, has contributed to divert the attention of the scientific community to DNA barcoding over UPT. However, what matters for us here, is the fact that a very similar technique was simultaneously advanced by two distinct groups of geneticists. This gives ground to the hypothesis that such a technique is, in one form or the other, creatively linked to the geneticists’ disciplinary matrix. Thus, it is the task of the evolutionary epistemologist to study the selective and non-selective conditions that might lead to the emergence within the field.

In particular, it is important to focus on the explanatory urgencies that the wider external community of biologists was facing when the innovation was first introduced. Such urgencies are always a source of cognitive dissonance of some sort and, as such, exert a pressure to innovate in specific directions.

In this regard, it must be noted that the years that precede DNA barcoding, are marked by a widespread dissatisfaction with taxonomy. The wider community of biologists lamented that the slow pace of taxonomic classifications and naming of new species was negatively impacting all biological research. This perceived situation of impasse also received the name of “taxonomic impediment” (for an overview see Rodman and Cody, 2003). Since the larger community of life scientists unanimously agree that taxonomic work underpins all biological research, there was a strong imperative to speed up the taxonomic output.

In this context of dissatisfaction, the early two thousands are marked with a series of efforts to assign a more central role to the use of DNA sequences. ‘DNA taxonomy’ was advanced as a new taxonomic program with a strong genomic character (Tautz et al., 2003).

DNA barcoding can certainly be included within these wider efforts. The standardization that it offered was initially presented as a way to accelerate identifications and, at least in part, overcome the 'taxonomic impediment' (Janzen, 2004).

Step 3: The “Why?” question.

The “Why?” question addresses the ‘epistemic aims’ of the innovators. As I remarked in the previous chapter, this is arguably the most important methodological step because it tells us what explanatory goals the innovators are pursuing in the background of their disciplinary matrix.

DNA barcoders presented their technique as having two major epistemic aims: 1) molecular diagnostic of individuals relative to described taxa and 2) the identification of new taxa (Kress, et al. 2015). In particular, this last epistemic aim was received with a fierce resistance and opposition from some members of the taxonomic community. The resistance must be understood as a product of partially divergent disciplinary matrices that host different epistemic standards and aims. Criticism spanned from more technical objections⁷⁵ to wider methodological criticism that involved a deeper discussion of the standards and aims of the taxonomic discipline itself. Some taxonomists objected that DNA barcoding could not

⁷⁵ For example, it appeared immediately clear that plants that have lower rates of nucleotide substitution in mtDNA could not use the same COI mitochondrial gene advanced by Hebert to study north American bird species. Other processes that represent an obstacle for a single locus mtDNA barcoding are male-biased gene flow, selection on the identified locus, introgression due to hybridization and several other processes. Nonetheless, as I have already noted, DNA barcoding has refined its method using, for example, multiple loci or making efforts to identify more and more reliable areas of the genomes to use as barcodes. Today, the impression seems that even if DNA barcodes “are not uniformly successful for unambiguous identifications across the Tree of Life” their application in the different branches of biology is expanding “to answer both applied and basic biological questions” (see Kress et al., 2015).

have been used as a method to discover and describe new species because DNA data alone are not *per se* taxonomically explanatory. Taxonomists tend to think that taxonomic hypotheses are significant only in the context of multiple lines of evidence. Dna, morphology, behaviour, ecology etc. all provide valuable lines of evidence. Only when these variegated sources are combined together, do taxonomists have a greater corroboration for their species hypotheses (De Quieroz, 2007). On the contrary, the fact that DNA barcoding was also presented as a diagnostic tool to reveal new species was seen as an attempt to give a privileged status to DNA data.⁷⁶

Similar problems apply to other genomic criteria. Indeed, to give a privileged status to molecular data alone has been interpreted as a reductionist move within some taxonomic circles - a potential threat to an “integrated taxonomy” that sees both molecular and organismal data as complementary lines of evidence (Dayrat, 2005; Will et al. 2005; Pante et al. 2015). According to this view, any attempt to prioritize one source of data over the other (say molecular over organismal data), would undermine the theoretical richness of taxonomy.

As Lipscomb et al., put it:

There is no credible reason to give DNA characters greater stature than any other character type. When species descriptions are based on a broad range of data, they become interesting scientific hypotheses making explicit predictions about the distribution of attributes among organisms. We reconstruct phylogenies to explain patterns of organismic diversity. Molecular data certainly contribute, but when nothing is known about organisms except their DNA, there are no evolutionarily interesting patterns to explain – just a tedious pattern of sequence similarity (Lipscomb et al., 2003).

As Lipscomb continues:

The advocates of DNA taxonomy seem not to understand the intellectual content of taxonomy based on all available information, or the hypothesis-driven basis of modern revisionary work. The many levels of hypothesis testing in taxonomy, from characters to species to clades, are essential for all evolutionary biology. To relegate taxonomy, rich in theory and knowledge, to a high-tech service

⁷⁶ When the barcodes are used for ‘species delimitation’ this means that they are associated with a measure of genetic variability to test the consistency of species definitions.

industry would be a decided step backward for science (Lipscomb et al., 2003).

Moreover, it has been noted that the wider community of biologists is, so to speak, a 'second hand' recipient of taxonomic classifications and, as such, it has a tendency to see taxonomy as a merely descriptive science - a mere “provider” of classifications for other more theoretically driven branches of biology (Barberousse and Samadi, 2013). On the other hand, from an “internal” perspective, taxonomists share a far more complex vision of their discipline. They contest the pejorative use of the term “descriptive” and they see their discipline as a theoretically driven science no less so than any other biological branch. As Wheeler put it:

So-called descriptive taxonomy deals with hypothesis testing on multiple levels. Initial claims that two structures are the same (homologous) is a hypothesis. The conclusion that the distribution of a homologous attribute qualifies it as a character of a species or a synapomorphy of a higher taxon is a hypothesis. A species is a hypothesis. Every clade *at every* Linnaean rank is a hypothesis. Each and every one of these hypotheses represents a generalization from which specific future observations are predicted (...). A species name is an effective shorthand notation for an explicit hypothesis about the distribution of attributes among populations of organisms (Wheeler, 2004).

Thus, given this scenario, it should not be surprising if a new method, such as DNA barcoding, with epistemic aims relevant for the taxonomic field enters into conflict with previous assumptions of the field. Different interpretations of the epistemic standards and aims of the taxonomic discipline have certainly played a role in the early criticism directed at DNA barcoding. Taxonomists' interpretations of the role, status, standards, aims, methods of their discipline are inevitably different from those of the neighbouring biological disciplines.

Step 4: The “Where?” question.

In the last step, we investigate the way a scientific replicator, such as “DNA barcoding”,

spreads and is differentially evaluated, discussed and eventually accepted and coopted in different branches of science. According to what we have said in the previous chapter, we expect that different branches of science (in this case the life sciences) manipulate the technique in different ways to satisfy their peculiar epistemic aims. At this stage, we also evaluate which are the candidates for the role of preponderant transmission biases that guide the acceptance of the replicator. This methodological step will be explored in greater detail in the next chapter where I use text analysis as a new tool to investigate the populational dynamics of scientific replicators within and between different disciplinary matrices. In the next section I analyze the implications of DNA barcoding in the context of a theoretical and methodological dispute known as the ‘species problem’.

4.3 DNA barcoding and the ‘species problem’.

It has been noted that there are no less than 25 conceptions of 'species' in biology (Wilkins and Ebach, 2014).⁷⁷ Moreover, it has also been noted that different conceptions pick out different types of taxa in the world (Ereshefsky, 2009). Thus, it can be concluded that our species conceptions are not universals and are often inconsistent among taxonomists to the point that for some taxa, the numbers of species recognized by different taxonomists may vary by up to 50% (Kress et al., 2015). As a whole, this complicated and ‘turbulent’ theoretical and methodological scenario, characterizes the so called ‘species problem’.

⁷⁷ The term 'species' conceptions' is more appropriate than 'species concepts' because, as Wilkins and Ebach (2014, p. 126) noted, the concept of species is one, while the 'conceptions' are many. It is a single concept that is defined or described in different ways.

As Ereshefsky (2009) noted, the ‘species problem’ presents at least three different issues. First, there is a reflection on the 'ontological' status of the 'species' category: are species natural kinds, sets, or individuals? Secondly, there is the problem of the biological nature of species: are they groups of interbreeding organisms, phylogenetic branches, lineages of organisms that share the same ecological niche, etc.? And third, there is a discussion on whether the species concept exists at all: is it or is not a ‘real category’ in nature? (Ereshefsky, 2009).

Another influential characterization of the so called ‘species problem’ argues that the ‘species problem’ conflates two distinct issues. On one side, ‘species conceptualization’ is an effort to provide a conceptual definition of what ‘species’ are. On this issues, De Quieroz seems confident that an agreement can be reached between biologists on the definition of species as “separately evolving metapopulation lineages” (De Quieroz, 2007, p.880). On the other hand, there is the rather different problem of ‘species delimitation’ where different lines of evidence, different ‘operational criteria’, are used to assess the separation of lineages (De Quieroz, 2007). De Quieroz insists that the two stages must be kept separate and that there is no theoretical conflict between the two. The operational criteria do not bear significance for ‘species conceptualization’ but only for ‘species delimitation’, where multiple lines of evidence are associated with a higher corroboration of species hypotheses (Ibidem).

I argue that the evolutionary and selective model defended throughout this work can help to clarify this interaction. Both Ereshefsky and De Quieroz’s formulations of the species problem boil down to an evolutionary selective dynamic between our cognition (and ultimately our linguistic and semantic category of ‘species’) and the referent, the external physical world. Thus, the species problem offers a paradigmatic example of the evolutionary

recursive and opportunistic interaction between our theoretical and conceptual posits and the clues presented by the external world. In other words, it is a perfect example of that interactive game between Nature and the scientists that I have described as dynamic selective realism.

With regard to scientists' theoretical efforts, a stable definition of 'species' seeks to appease the inevitable cognitive dissonance. With regard to the referent, the physical world presents different lines of evidence to describe species. In an effort to arrive at stable theoretical formulations, scientists are engaged in all sorts of opportunistic trials and heuristics. Opportunistic explorations of the 'species problem' are tackled from multiple directions: from theory to classification based on pre-theoretical observations, from new lines of evidence revealed by new techniques, and so forth. Some biological branches start from a more theoretical standpoint in agreement with their disciplinary matrices and produce different species conceptions. Thus, the conception of 'species' is defined by naturalists as 'interbreeding organisms', by systematists as 'phylogenetic branches' and by ecologists as lineages of organisms that share the same ecological niche, etc. Others scientists, resort to other types of operational criteria such as DNA clustering and genomic criteria.

In the absence of a stable theoretical formulation, different lines of evidence are instrumental to arriving at species hypotheses, circumventing more theoretically driven formulations. That is why, even if DNA barcoding does not offer a 'theory of species', it seems premature to conclude that DNA barcoding is not relevant for the species problem.

Ereshefsky reached this conclusion when he rhetorically asks:

Does DNA barcoding provide a reductionist, perhaps essentialist, account of the species category? The answer is no, because DNA barcoding *is not relevant to the species problem* [emphasis added]. Biologists developing DNA barcoding are clear that they are not offering a

theory of species (Hebert and Gregory, 2005, p. 853) (Ereshefsky, 2009, p. 419).⁷⁸

Ereshefsky's point is true only if we concede that the species problem is an exclusively theoretical problem. I claim that the species problem must be interpreted not only as a theoretical issue, but also as a dynamic evolutionary process between theoretical and operational solutions. If we assume this perspective, the conclusion that DNA barcoding is irrelevant for the species problem appears as a hasty conclusion.

Rather, DNA barcoding can be seen as an 'operational criteria' (De Quieroz, 2007) between others, that is used in opportunistic ways to arrive at more grounded species hypotheses. DNA barcodes offer a practical circumstantial framework for taxonomic hypotheses that are eventually corroborated with other sources of data. Moreover, even a quick look at the taxonomic literature of the last decade shows the great contribution of DNA barcoding in 1) advancing new species hypotheses, 2) redescribing pre-existent and often incorrect classifications made with other criteria, and 3) revealing cryptic species that were not distinguishable by morphology alone (Kress et al., 2015). Not only is DNA barcoding is relevant for the "species problem" but it proved to be a very successful operational criterion to arrive at new species hypotheses.

For all these reasons, I conclude that DNA barcoding, as it is used in the actual taxonomic practice today, cannot be judged as 'irrelevant' for the so called species problem. Rather, the evolutionary and selective framework advanced throughout this work allows us to put this technique (its disciplinary matrix and epistemic aims of emergence) in a dynamic

⁷⁸ Ereshefsky goes on, stating that 1) "DNA barcoding is not even used for constructing classifications" and that 2) "CO1 is merely used as a marker to match a specimen to a voucher specimen that has already been classified" (Ereshefsky, 2009). Given the present state and use of DNA barcoding these statements are, at best, hasty generalizations.

selective context where theoretical and conceptual efforts collide with operational solutions in search of a temporary composition and stability of ideas. DNA barcoding participates in this dynamic where scientists' opportunistic explorations try to attack the 'species problem' from different angles. This wider evolutionary context is what we call "species problem" and DNA barcoding cannot be irrelevant for that.

Avise (2004) describes very well how new molecular techniques introduced in the mid-1960s in molecular biology can be seen as opportunistic and selective explorations.

A novel assay technique for proteins or DNA is introduced, and a flood of evaluative activity follows. Methods that fail to live up to advance billing (...) are abandoned. Approaches that survive the initial evaluations are then employed to address broad conceptual topics in the research paradigms of molecular evolution (...). Discussions also ensue about how best to analyze and interpret the new classes of molecular data. In the meantime, genetic markers provided by each new method are applied to interesting problems in natural history or evolution where their use appears appropriate. Success in such endeavours stimulates further interest, and *the more utilitarian molecular approaches eventually gain wide popularity* [emphasis added]. Usually, after a period of several years, the enthusiasm crests, and a new wave of interest may focus on another newly introduced assay procedure. Typically, the earlier methods are not abandoned, but merely become incorporated into the growing pool of molecular techniques that find continued application in studies of organismal biology, natural history and evolution (Avise, 2004, pp. 49-50).

Mutatis mutandis, the same can be said about DNA barcoding. DNA barcoding is a technique that offers an operational criterion to arrive at revisable and provisional species hypotheses. The fact that it does not contain a 'species' theory' does not mean that the technique is 'irrelevant' for the so called 'species problem'. This last must be interpreted as a wider process that involves all possible opportunistic paths of scientific exploration as presented in Chapter 3.

4.4 Summary

In this chapter, I have located a specific scientific innovation within the evolutionary and

selective framework delineated throughout this work. DNA barcoding has been connected to a given disciplinary matrix with given epistemic aims. Criticism is also understood from the point of view of different and partially conflicting disciplinary matrices. The evolutionary and selective meta-framework presented in this work allowed me to place DNA barcoding within a wider selective dynamic involving different communities of scientists that opportunistically explore new paths to characterize what a ‘species’ is and how it can be identified. Different sources of data, different theoretical and methodological presuppositions create conflict between disciplinary worldviews. There is not a clear formula to arrive at species identifications but rather there are several criteria whose primacy is a matter of contrast and debate between different disciplinary matrices and different scientists. Variation is to be expected between these preferences, even at the level of individual scientists that belongs to the same discipline.

In the next and final chapter, I explore the application of text analysis as a new tool that can complement the ‘close reading’ traditional methods of historians and philosophers of science. It is worth restating Toulmin’s passage concerning the study of intellectual cross-sections to understand the application of text analysis in the evolutionary context of scientific ideas.

The change from one cross section to the next is an evolutionary one in this sense too: that later intellectual cross-sections of a tradition reproduce the content of their immediate predecessors, as modified by those particular intellectual novelties which were selected out in the meanwhile – in the light of the professional standards of the science of the time (Toulmin, 1967, p. 466).

Potentially, text analysis offers a way to keep track of the epidemiological dynamics of scientific concepts and ideas providing new ways to investigate change that occurs between Toulmin’s ‘cross sections’. As it will become apparent, text analysis can also help to

clarify specific philosophical questions pertaining to the scientific innovations being studied.

Chapter 5

Text analysis as a quantitative tool to investigate science's conceptual dynamics

5.1 Premise

The digitalization of the scientific literature paired with the use of textual analysis tools opens up new ways to study science. With the use of textual processing engines, we can analyze large corpuses of texts and extract data that corroborate specific historical hypotheses and observations. These methods very well complement the 'close reading' traditional methodologies of the historians and philosophers of science (Ramsey and Pence, 2016; Maienschein and Laubichler, 2012).

In this section, I present two preliminary studies that make use of text analysis to investigate DNA barcoding. In particular, study 1 investigates the relationship between DNA barcoding and the so called 'species problem'. Here, I employed a freeware corpus analysis toolkit called AntConc (Anthony, 2012) to extract frequencies of words that correlate with the term 'species' in the taxonomic literature.⁷⁹ As it will become apparent, changes in the mean frequencies of clusters over a decade are tentatively correlated with the advent of DNA barcoding as it impacted the debate in the journal.

Study 2 is aimed at quantifying the reactions of different disciplinary matrices as they engage with DNA barcoding. For this second study, I used a text analysis application called

⁷⁹ These are pairs of words that have the form 'species ___'; from here on, I will refer to them as 'clusters'

Linguistic Inquiry and Word Count (Tausczik and Pennebaker, 2010). LIWC is able to measure various emotional, cognitive and motivational components present in individuals' verbal and written speech samples. It has been applied in mental health diagnostic, marketing, sociological research and also literary criticism (see Nichols et al, 2014). Here, I evaluate its extension to the analysis of scientific communities' reactions to newly introduced scientific innovations. In particular, LIWC is applied to investigate the different 'attitudes' expressed by taxonomists and ecologists toward DNA barcoding.

5.2 Study 1. Measuring the impact of DNA barcoding on the 'species problem'.

Methods, dataset and hypothesis.

Study 1 applies text analysis to study DNA barcoding's impact on the controversial 'species problem'. I began by identifying the journal *Systematic Biology* as the target of my research because the journal publishes articles on a broad spectrum of biological topics but with a particular focus on taxonomic and systematics' issues.⁸⁰

Once I extracted a corpus of articles, I organized them by criteria that allowed me to observe the way DNA barcoding might have impacted the 'species problem'. In order to do this, I determined the starting point where DNA barcoding first appeared in the journal, and from there, I divided the articles in two groups: a 'PRE' and a 'POST' DNA barcoding's

⁸⁰ As it can be read in the journal's website *Systemtic Biology* publishes "original contributions to the theory, principles and methods of systematics as well as phylogeny, evolution, morphology, biogeography, paleontology, genetics, and the classification of all living things".

impact.⁸¹ Table 5.1 shows the dataset characteristics.

Table 5.1. Dataset 1 characteristics.

Source	N	Year of Publication	Partition Criteria
<i>Systematic Biology</i>	316	2000-2004	Pre DNA barcoding
<i>Systematic Biology</i>	340	2005-2009	Post DNA barcoding

Once the dataset was organized, I had to tackle the ‘species problem’. Thus, I gathered data on the ten most frequent words that are associated with the term ‘species’ (“species ___” pair of words that I call ‘clusters’).⁸² The rationale behind my method was that patterns of change in the ten most frequent clusters between the Pre and Post DNA barcoding spans of time would highlight changes on the underlying debate pertaining to the ‘species problem’.

However, even if important changes in the frequencies of clusters would have been revealed at this stage, this method alone would not have established that DNA barcoding was causally responsible for that change. Thus, to further characterize the possible contribution of DNA barcoding, I collected (from the same journal) the ten most frequent clusters for those articles that deal with DNA barcoding alone.⁸³ Table 5.2 illustrates this dataset’s characteristics.

⁸¹ DNA barcoding first appears in the journal in 2005 in a series of ‘Points of View’ articles aimed at evaluating the technique for the field of systematics and taxonomy. It is no surprise that such evaluation comes after two years from Hebert and colleagues’ introduction of DNA barcoding. Reasonably, a scientific innovation requires some time to reach the wider audience of scientists or to exert an evaluation according to the epistemic aims of a field.

⁸² When the term ‘species’ formed a cluster with adverbs, verbs, prepositions etc. (e.g., “species in”, “species were” etc.) that were manifestly not significant for the scope of my analysis, the clusters have been excluded even when they ranked high in the list.

⁸³ These articles were singled out using the keyword “DNA barcoding” in title, abstract and text in the journal’s issues indexed on line.

Table 5.2. Dataset 2 characteristics.

Source	N	Year of Publication	Partition Criteria
<i>Systematic Biology</i>	28	2005-2009	Topic - DNA barcoding

My guiding principle was to compare those clusters that ranked high in the articles pertaining to DNA barcoding with the clusters that showed a statistically significant increase in frequency between the Pre and Post DNA barcoding period. Clusters that satisfied both criteria would offer a better characterization of what can be attributed to DNA barcoding. On the contrary, if the clusters did not show any statistically significant increase between the two spans of time, this would suggest that no major change had occurred in the debate. Moreover, there was a qualitative implication of my text analysis. In fact, which particular clusters increased or decreased over time also matters. For example, my hypothesis was that in the Post DNA barcoding period there was a significant increase in those clusters that refer, broadly speaking, to the identification of new species (e.g., ‘species identification’, ‘species delimitation’).

However, before comparing the Pre and Post DNA barcoding groups, I decided to first assess the internal variation present within each group. Thus, I randomly ordered the articles present in each group, and I further created two sub-groups from which I extracted the ten most frequent clusters and their mean frequencies (cluster per article). T-tests were performed to assess if statistically significant variance occurred within the sub-groups. Here, my hypothesis was that there was no statistically significant variance between the sub-groups. Statistically significant variance had to be expected only when the Pre and Post DNA barcoding groups were compared.

Finally, the same method was applied to the Pre and Post DNA barcoding groups. The

t-tests assessed if the frequency of each cluster significantly changed between the two spans of time. A two-way ANOVA was also conducted to test if there was statistically significant variance associated with the Pre and Post DNA barcoding periods.

Results and discussion

The clusters do not present statistically significant variation when the internal variation of each group (the Pre and Post DNA barcoding) is assessed (See table 5.3 and table 5.6). However, statistically significant change occurred between the Pre and the Post DNA barcoding periods as predicted (See table 5.7). In other words, the dividing criteria of a Pre and Post DNA barcoding impact is correlated with a significant change in the frequency of species' clusters terms.

In particular, seven clusters show statistically significant change (p -value < 0.05). These are tree, phylogeny, delimitation, complex, relationship, divergence, identification (See Table 5.7 and Fig. 5.2 for a word cloud representing the change in frequencies). A comparison with the list of the ten most frequent clusters present in the articles dealing with DNA barcoding alone gives a better characterization of the possible contribution of DNA barcoding. The clusters “identification, delimitation, divergence and complex” which rank prominently in this list (See Table 5.8) are also associated with an increase in frequency in the Post DNA barcoding period. Thus, it is reasonable to suppose that DNA barcoding is associated with procedures for species identification and delimitation. Moreover, there is an increase in frequency per article for almost all clusters, which suggests that from 2005 on the term ‘species’ is more frequent in the journal. If we extract the frequencies of the term

‘species’ year by year, we see an increase in correspondence in 2005, when DNA barcoding was first discussed in the journal (see Fig. 5.1). Even if a causal link with DNA barcoding is not established, this offers evidence for the hypothesis that starting from 2005, discussions around the so called ‘species problem’ (in both its conceptual and operational form) became more prevalent in the journal.

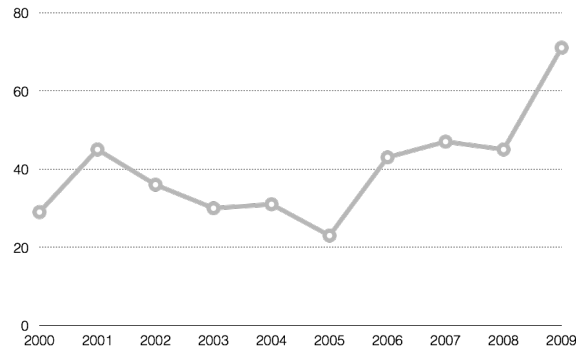


Figure 4.1. Frequency of the term ‘species’ per article in *Systematic Biology*

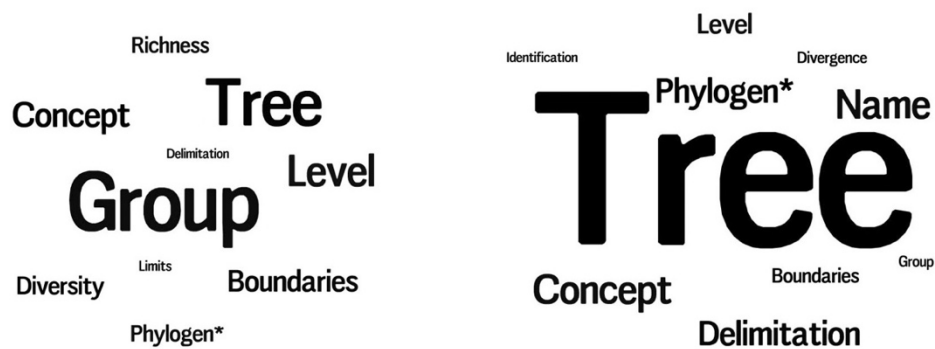


Figure 5.2. Word cloud representing clusters’ change in frequencies. The relative font size represents the frequencies of the ten most common clusters in the Pre (left) and Post (right) DNA barcoding span of time.

Table 5.3. The ten most frequent clusters in the Pre DNA barcoding period. “Species ____” clusters in *Systematic Biology* in the Pre DNA barcoding period from 2000 to 2004. Articles were randomly ordered and divided in two groups to assess internal variation.

Pre DNA Barcoding					
<i>1st group</i>			<i>2nd group</i>		
Rank	Clusters	N	Rank	Clusters	N
1	Group	146	1	Group	98
2	Tree	124	2	Level	76
3	Concept	73	3	Tree	66
4	Level	50	4	Boundary	56
5	Richness	46	5	Limit	36
6	Phylogeny	45	6	Delimitation	34
7	Diversity	40	7	Diversity	32
8	Boundary	31	8	Richness	20
9	Distribution	21	9	Concept	27
10	Composition	18	10	Phylogeny	23

Table 5.4. Mean frequencies of each cluster in the Pre DNA barcoding group. The *p*- values assess the variation between the two sub-groups and they are obtained with *t*-tests.

Pre DNA Barcoding						
<i>1st group</i>			<i>2nd group</i>		<i>p</i> -value	
Clusters	M	(SD)	M	(SD)		
Group	0.95	(4.7)	0.63	(2.83)	0.47	
Tree	0.78	(3.77)	0.42	(1.27)	0.24	
Concept	0.46	(3.18)	0.17	(0.93)	0.27	
Phylogeny	0.32	(0.93)	0.17	(0.45)	0.06	
Level	0.32	(1.28)	0.48	(1.97)	0.37	
Richness	0.29	(2.09)	0.15	(0.69)	0.4	
Diversity	0.26	(1.69)	0.20	(1.17)	0.72	
Boundary	0.21	(2.23)	0.40	(3.05)	0.52	
Distribution	0.13	(0.87)	0.03	(0.19)	0.13	
Composition	0.11	(0.83)	0.03	(0.19)	0.19	
Limit	0.04	(0.22)	0.23	(1.88)	0.19	
Delimitation	0.03	(0.21)	0.22	(2.47)	0.12	

Table 5.5. The ten most frequent clusters in the Post DNA barcoding period. Articles were randomly ordered and divided in two groups to assess internal variation.

Post DNA Barcoding					
<i>1st group</i>			<i>2nd group</i>		
Rank	Clusters	N	Rank	Clusters	N
1	Tree	1030	1	Tree	1365
2	Name	160	2	Concept	156
3	Concept	154	3	Name	145
4	Delimitation	128	4	Phylogeny	138
5	Phylogeny	114	5	Delimitation	134
6	Level	101	6	Boundary	108
7	Group	84	7	Level	106
8	Identification	66	8	Divergence	78
9	Relationships	54	9	Complex	64
10	Divergence	47	10	Richness	63

Table 5.6. Mean frequencies of each cluster in the Post DNA barcoding group. The *p*-values assess the variation between the two sub-groups and they are obtained with *t*-tests.

Post DNA Barcoding						
<i>1st group</i>			<i>2nd group</i>		<i>p</i> -value	
Clusters	M	(<i>SD</i>)	M	(<i>SD</i>)		
Tree	6.84	(27.07)	8.08	(27.46)	0.67	
Name	0.97	(10.13)	0.88	(7.16)	0.92	
Concept	0.91	(7.97)	0.92	(4.59)	0.98	
Delimitation	0.75	(4.64)	0.81	(4.46)	0.77	
Phylogeny	0.71	(2.61)	0.84	(2.94)	0.66	
Group	0.52	(2.02)	0.28	(0.75)	0.14	
Relationship	0.32	(1.95)	0.19	(0.68)	0.43	
Richness	0.24	(0.87)	0.34	(1.77)	0.51	
Boundary	0.22	(0.89)	0.64	(3.06)	0.09	
Complex	0.15	(0.53)	0.37	(1.78)	0.12	
Level	0.6	(1.81)	0.62	(1.56)	0.92	
Identification	0.4	(2.97)	0.3	(8.75)	0.7	
Divergence	0.3	(1.53)	0.46	(2.09)	0.4	

Table 5.7. Mean frequencies of all the clusters (Pre and Post DNA barcoding periods). These are compared individually with *t*- tests and collectively with ANOVA.

Cluster	Pre DNA Barcoding		Post DNA Barcoding		p-value
	<i>M</i>	(<i>SD</i>)	<i>M</i>	(<i>SD</i>)	
Group	0.79	(3.86)	0.40	(1.52)	0.093
Tree	0.60	(2.82)	7.46	(27.23)	< 0.001 ***
Level	0.40	(1.66)	0.61	(1.69)	0.104
Concept	0.32	(2.35)	0.93	(6.53)	0.112
Boundary	0.30	(2.67)	0.43	(2.26)	0.517
Phylogeny	0.25	(0.73)	0.78	(2.78)	< 0.001 ***
Diversity	0.23	(1.45)	0.12	(0.52)	0.214
Richness	0.22	(1.56)	0.29	(1.39)	0.545
Limit	0.14	(1.34)	0.11	(0.66)	0.743
Delimitation	0.13	(1.75)	0.78	(4.54)	0.014 *
Name	0.10	(0.44)	0.92	(8.76)	0.085
Distribution	0.08	(0.63)	0.10	(1.11)	0.733
Complex	0.08	(0.36)	0.26	(1.32)	0.014 *
Relationship	0.07	(0.38)	0.26	(1.46)	0.023 *
Composition	0.07	(0.60)	0.01	(0.11)	0.093
Divergence	0.04	(0.25)	0.38	(1.83)	< 0.001 ***
Identification	0	-	0.35	(2.45)	0.008 **
ANOVA					
		DF	F		Pr > F
		1	34.659		0.000 ***

* $p \leq 0.05$, ** $p \leq 0.01$, *** $p \leq 0.001$

Table 5.8. The ten most frequent clusters in the articles dealing with DNA barcoding. Asterisks indicate those clusters that also increase in the Post DNA barcoding period.

Rank	DNA barcoding clusters	N
1	Identification *	118
2	Concept	106
3	Delimitation *	102
4	Boundaries	79
5	Level	64
6	Tree	60
7	Discovery	43
8	Divergence *	26
9	Complex *	24
10	Limits	21

I argue that the text analysis applied to the DNA barcoding case offers a way to better understand what is at stake with DNA barcoding as it relates to the term ‘species’. “Species identification”, “species concept”, “species delimitation”, “species divergence” and “species complex” show a significant increase in the Post DNA barcoding period. This lends credence to the hypothesis that the introduction of DNA barcoding has significantly shifted the debate around the process by which species boundaries are determined and new species are discovered, bringing back the issue of ‘species delimitation’ to the front of the taxonomic debate. In other words, the replication of DNA barcoding in the population of variants is associated with the differential replication of larger units. Between the two spans of time a selective process has occurred and DNA barcoding seems to be the relevant replicator that has promoted this change in its interaction with larger ideational and theoretical entities.

5.3 Study 2. A text analysis tool to quantify scientific disputes.

Methods, dataset and hypotheses.

This second text analysis study was aimed at offering a quantitative characterization of the different reactions of two branches of biologists to the introduction of DNA barcoding. The general historical inquiry presented above had already showed that the technique drew a rather fierce criticism within a part of the taxonomic community. Thus, I decided to investigate if text analysis can help to quantify these different attitudes that might come from different disciplinary matrices with their peculiar epistemic aims. For this purpose, I employed a text analysis engine called Linguistic Inquiry and Word Count (LIWC). This text analysis' tool seems particularly appropriate for this task because it is able to count words from texts and assign them to psychologically meaningful categories (Tausczik and Pennebaker, 2010). As explained by its developers, LIWC contains dictionaries that are the heart of the program. Each word present in a corpus of texts is classified according to these dictionaries that contain more than 80 classificatory categories. For example, if the word 'clear' is found in the text it is associated with five word categories: adjectives, cognitive processes (cogproc), certainty, percept and see. When the word 'clear' is found, each of these five categories' scores is incremented.

To begin with, I collected two datasets of articles representative of two branches of biology (taxonomy/systematics on one side and ecology on the other) to measure if LIWC revealed a different reaction toward DNA barcoding. Thus, for both communities, I collected

articles that deal with DNA barcoding alone which were published in authoritative journals of the two fields.⁸⁴ See Table 5.9 for the dataset characteristics.

Table 5.9. Dataset 3 characteristics. It shows the journals and the number of articles that deal with DNA barcoding representative of the two communities.

Taxonomy / Systematics's journals	N	Ecology journals	N
Systematic Biology	67	Trends in Ecology and Evolution	6
Cladistics	9	Molecular Ecology Resources	127
Zookeys	23	Molecular Ecology	13

The next step was to find a way to operationalize my hypotheses within the categories provided by LIWC. My hypothesis was that three of the LIWC categories would be able to reveal a contrast between taxonomists (or at list a part of them) and ecologists in dealing with DNA barcoding. For instance, the ecologists would be revealed to be more favourable to DNA barcoding than the taxonomists/systematists. I operationalized these hypotheses with three categories present in LIWC. These are:

- Achievement – higher numbers indicate success; lower numbers indicate failure;
- Reward focus – expressing rewards, incentives, positive goals, approach;
- Risk – references to dangers, concerns, things to avoid.

To sum up, ecologists should show a greater ACHIEVEMENT, a greater REWARD and less RISK when compared with taxonomists and systematists (see table 5.10).

⁸⁴ First I identified the journals *Systematic Biology* and *Trends in Ecology and Evolution* as the two main journals of each field. Secondly, using Web of Science's Journal Relationship option, I identified the other journals. Then, I decided to use the span of time 2003 (the year when DNA barcoding was first introduced) to present, (March 2016). And finally, using the keywords "DNA barcoding*" and "Barcode*" in TITLE OR ABSTRACT OR TEXT I gathered the set of articles.

Table 5.10. LIWC categories. Dictionary examples and hypotheses' direction.

LIWC variable	Dictionary examples	Hypotheses
ACHIEVEMENT	obtain, improved, advancements	$E > S$
REWARD	optimal, better, scores, confidence	$E > S$
RISK	lack, problems, failing,	$E < S$

E = Ecology; S = Systematics

Moreover, another control experiment was set up in the same way but with a different keyword criterion for the articles. I chose the term “rodent*” in TITLE OR ABSTRACT because I did not suspect any difference in the way the two communities reacted to issues pertaining “rodents*”. If the data showed statistically significant differences between “DNA barcoding” but not with ‘rodents’ there was a ground for the hypothesis that LIWC was able to pick out methodological and theoretical divergences between branches of science.

The mean values of each category (percentage of words per article that falls in the category) were extracted for the taxonomists and the ecologists. *T*-tests were performed to assess the validity of my hypotheses. A two-way unequal Anova was also conducted to assess if there was statistically significant variation between the journals grouped in the two communities.

Results and discussion

As it is shown in Table 5.11 there is no statistically significant difference between the two groups in any category as they relate to DNA barcoding. Thus, my hypotheses were not supported.

Table 5.11. Word frequencies for LIWC’s categories (articles that deal with 'DNA barcoding').
M is expressed as a percentage of words for a given category in each article.

LIWC categories	Taxonomy			Ecology			p-value
	N	M	(SD)	N	M	(SD)	
1 ACHIEVEMENT	99	0.85	(0.43)	146	0.87	(0.43)	0.346
2 REWARD	99	0.59	(0.29)	146	0.60	(0.30)	0.436
3 RISK	99	0.26	(0.23)	146	0.24	(0.15)	0.261

Note: *p*- values indicate the results of *t*- tests.

ANOVA			
	DF	F	Pr > F
Taxonomy-Systematics / Ecology	1	0.002	0.878

Moreover, Table 5.12 shows that the control experiment using the term “rodent*” gave results contrary to my hypotheses. There is a statistical difference between the articles belonging to the two communities in two categories achievement and risk. This falsifies the hypothesis that the term “rodent*” is not associated with significant differences between the two communities.

However, even if LIWC did not pick out any scientific dispute or divergence between the two communities, it is premature to conclude that no theoretical or methodological divergences on DNA barcoding exists between the two. Further empirical evidence is needed to establish if LIWC is able to pick out scientific disputes even when they are clearly present in the literature.

Thus, further text analysis should be conducted to evaluate LIWC’s applicability to the study of scientists’ drives and opinions toward scientific innovations. For example, with a better tailored dataset that takes only articles that are manifestly critical and articles that have an enthusiastic response towards a scientific innovation.

Table 5.12. Word frequencies for LIWC's categories (articles that deal with 'rodent*').

LIWC categories	Taxonomy			Ecology			p-value
	N	M	(SD)	N	M	(SD)	
1 ACHIEVEMENT	27	0.55	(0.26)	64	0.90	(0.45)	<0.001 ***
2 REWARD	27	0.36	(0.24)	64	0.37	(0.18)	0.475
3 RISK	27	0.12	(0.08)	64	0.21	(0.20)	0.001 **

Note: *p*- values indicate the results of *t*- tests.

ANOVA			
	DF	F	Pr > F
Taxonomy-Systematics / Ecology	1	15.516	0.000 ***

* $p \leq 0.05$, ** $p \leq 0.01$, *** $p \leq 0.001$

My experiment simply establishes a starting point for further empirical research in this direction. Either LIWC is able to quantify the critical reaction of opponents in a scientific debate or it is not. If further research will show that the answer is negative another option could be to develop a dictionary within LIWC that is specifically tailored to highlight scientific disputes. The program is structured in such a way that new dictionaries can easily be compiled and added to the search engine.

5.4 Summary

In this final chapter the ‘close reading’ historical investigation presented in chapter 4 was complemented with two text analysis experiments. Text analysis was presented as a tool with a great potential to analyze the dynamics of scientific replicators over time, in what Toulmin (1967, 1972) describes as “successive time-slices” or “cross sections”. Study 1 was inspired

by such an evolutionary view. First, the appearance of a replicator in the literature was identified. Then, two cross sections were created and data was gathered to observe its impact on the pool of pre-existent conceptual variants. In this case, the focus was on the term ‘species’ alone. Variations in the frequencies of correlates and clusters were tentatively linked to the influence of DNA barcoding. These patterns of change over slices of time can offer a quick glimpse of populational dynamics of ideas within scientific communities. Of course, the evidence gathered with text analysis should not be intended as conclusive. On the contrary, text analysis offers a first programmatic inspection that is a starting point for further empirical research conducted directly on scientists’ preferences, ideas and opinions.

The text analysis conducted in the first study has shown to be instrumentally valid to assess two main hypotheses: (1) DNA barcoding is a replicator that has shifted the taxonomic debate in the journal towards the issue of species delimitation and (2) the replicator’s growth is correlated with the growth of the term “species” in the population between time T_1 and time T_2 . Again, these observations are only provisional and are meant to offer a ground to conduct further observations and collect new data (e.g, through surveys directly administered to the scientists).

However, the growth of the replicator over time gives a first insight on the correlated cultural fitness of the variant. Either if we use cultural evolutionists’ formula for cultural fitness that looks at increased numbers of replicators at $t + 1$ or if we use Ramsey and De Block’s (2015) account of cultural fitness intended as reproduction of a replicator per individual organism, text analysis can be a valuable first aid to get a first impression of the populational trend of an idea. For example, if we adopt Ramsey and De Block’s (2015) conception of fitness, then the scientist becomes the individual organism upon which the

presence of the replicator and its general fitness must be calculated. In such a scenario, text analysis rather than revealing direct fitness reveals a pattern of what Ramsey and De Block call ‘cultural growth’ that is the mere replication of the cultural variant.

The reason to distinguish cultural growth from cultural reproduction can be intuitively understood if we think to a replicator that indeed increases in the population (by it being copied or stored more frequently) even when a single scientist is producing all the copies (on scientific papers, conferences, university lectures, etc.) (see Ramsey and De Block, 2015). This would better represent growth, but this type of growth does not automatically mean acceptance, validation or borrowing and copying from others. On the contrary, the scientists’s acceptance of these replicators would better represent a measure of fitness. That’s why Ramsey and De block (2015) suggest to calculate fitness upon the individual organism acceptance of the ideational replicator. This seems an important distinction to make.

However, simple growth might still give a rough estimate of a trend in the population of variants and might still be correlated with acceptance by an increasing number of individual scientists. As such, text analysis offers a quick exploration of cultural growth that is a necessary first empirical step to conduct further research on the diffusion of a scientific idea within a given scientific community. This second step involves the use of a plethora of different methodologies which are routinely employed in psychology and the social sciences. Questionnaires and surveys can help to collect more quantitative data to corroborate specific hypotheses. Qualitative methods like participant observation of scientific communities, already pioneered by Hull (1988), is another valuable option. All these methods would greatly benefit from an evolutionary and selective meta-framework which provides an

integration of different explanatory instances coming from the psychology, sociology and epistemology of science.

The second study was aimed at investigating two communities of scientists as they react differently to the introduction of a new replicator. The main hypothesis that LIWC would have revealed a different attitude between taxonomists and ecologists towards DNA barcoding was falsified. The intent of this study was to assess if LIWC could validly highlight the different epistemic aims that come from different disciplinary matrices. However, this study did not provide conclusive results on the inapplicability of LIWC for this purpose. Further research should be conducted to first assess if LIWC recognizes different attitudes when they are manifestly present. A data set should be tailored for this purpose, for example, selecting a group of papers that are manifestly critical and another group that is favourable towards DNA barcoding. This can be done looking at the titles and abstracts that immediately show the critical content of the paper. Thus, this second experiment has traced possible paths of exploration for further research and the applicability of LIWC for this purpose is still possible.

Overall, these studies suggest that text analysis can be a valid tool to better study and model the replicators' populational dynamics, their context of emergence, and their trajectories of diffusion within and between different branches of science. In a few words, text analysis is suited to the implementation of the evolutionary, selective and populationist framework defended throughout this work. The use and future extension of these new analytic tools paves the way to a more robust selectionist model of science as it was first pioneered by Hull (1988), Toulmin (1972) and Campbell (1974).

Conclusion

This work offers a preliminary response to Mesoudi's (2011) recent plea for a rediscovery of the early evolutionary epistemologist's contributions to an evolutionary cultural understanding of scientific ideas and practices in what he calls "epistemic evolution". Here, I argued that Darwinian evolutionary theory bears great significance for understanding human cognitive faculties that instantiate 'knowledge' processes. In particular, I argued that, beside the epistemological relevance concerning the natural origin of cognitive faculties (bio-epistemology), Darwinian theory offers a model for the evolution of cultural and scientific ideas. I claimed that the theory of Darwinian cultural evolution present today is sufficiently developed to offer a viable model to study conceptual and 'ideational' change in science.

Thus, the primary contribution of this work is an elucidation of the adaptive puzzle presented by cultural adaptations and its significance for a notion of 'scientific change'. Scientific ideas show an increase in 'adaptedness' over time. Biological or genetic selection cannot fully explain the emergence and maintenance of such cultural improvements. Thus, unless we want to resort to what Dennett calls "skyhooks", i.e. providential solutions, we are in need of an explanation that tackles the adaptiveness of cultural ideas. Darwinian cultural evolution offers a way out of this adaptationist dilemma. The sophisticated social learning of our species (an adaptation of its own) has created a second level evolutionary process that allows adaptive cultural accumulation. Cultural adaptations are shaped by biases and preferences located in our cognition that is, in turn, shaped over evolutionary time. Within the realm of cultural evolution, science is that human enterprise that, as Mach's quote in the introduction reminds us, has replaced tentative and unconscious adaptation by a faster, fully

conscious and methodical variation to arrive at increasingly accurate representations of the external reality. This development can be understood from the perspective of an evolutionary and selective theory of cultural evolution. Science develops as the outcome of a selective process that contains several ‘nested’ selective mechanisms. Evolutionary epistemology’s naturalistic approach and its ‘universal selectionism’ give an important contribution to the epistemological debate. In this work, I have discussed how a selectionist and populationist model of cultural evolution sheds light on epistemological issues such as the context distinction, the criteria for theory choice, the theory ladenness of science and a notion of scientific realism. I argued that when these issues are presented through the lens of a cultural evolutionary meta-framework new insights are gained and old disputes can be reconciled. Finally, in chapter 4 and chapter 5 explore the use of text analysis as a valuable tool to investigate the populational dynamics of scientific ideas.

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