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Gould, Hull, and the Individuation of Scientific Theories

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Abstract When is conceptual change so significant that we should talk about a new theory, not a new version of the same theory? We address this problem here, starting from Gould's discussion of the individuation of the Darwinian theory. He locates his position between two extremes: 'minimalist'—a theory should be individuated merely by its insertion in a historical lineage—and 'maximalist'—exhaustive lists of necessary and sufficient conditions are required for individuation. He imputes the minimalist position to Hull and attempts a *reductio*: this position leads us to give the same 'name' to contradictory theories. Gould's 'structuralist' position requires both 'conceptual continuity' and descent for individuation. Hull's attempt to assimilate into his general selectionist framework Kuhn's notion of 'exemplar' and the 'semantic' view of the structure of scientific theories can be used to counter Gould's *reductio*, and also to integrate structuralist and population thinking about conceptual change.

Keywords Theory Individuation · Selectionism · Structuralism

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1 Introduction

The problem of the structure and individuation of scientific theories has been a central one in contemporary philosophy of science. Logical empiricists reconstructed scientific theories as linguistic objects, individuated by their logical structure and the semantics of the languages that constitute those objects. It is well known that, as a result of this approach, the meaning of theoretical terms became one of the chief problems for their program.

Popper's critique of logical empiricism downplayed semantic issues and focused, instead, on methodology. Lakatos tried, in the 1960s, to assimilate the so-called historicist turn in the philosophy of science into the general framework of Popperianism. From a historicist perspective, it became pretty obvious that the conceptual units to be appraised should be more inclusive: series of theories, programs, or what have you. These units are understood as having an explicit dynamical dimension, being irreducible to isolated and static theories, as conceived by the 'received view'.

Given that new focus on the dynamics of scientific knowledge, it is not surprising that the characterization of the units of appraisal became, thereafter, one of the fundamental problems of 'post-positivist' philosophy of science. If we take into account the historical and social dimensions of scientific activity—instead of just reconstructing scientific knowledge in terms of inferential and semantic relations—this problem turns out to be a very complex one.

In this paper, we will focus our attention on the following question: Can an evolutionary approach make us see the problem of the units of appraisal under a new light?

We take as a starting point the more restricted problem of the individuation of the Darwinian theory, as formulated by Stephen Jay Gould in *The Structure of Evolutionary Theory* (2002). Gould faces this problem because of its relation to an important issue in current evolutionary biology. This science is undergoing a theoretical change and it raises the inevitable question of whether this is a change within the boundaries of Darwinism, or it may result in an entirely new evolutionary theory, a 'post-Darwinian', or even an 'anti-Darwinian' theory. Gould argues that the current versions of evolutionary theory cannot be regarded as 'anti-Darwinian', but, to do so, he understands he has to tackle the philosophical problem of the nature and individuation of scientific theories in general.

Since Gould faces this problem by engaging in polemics with David Hull, we will subsequently discuss the selectionist theory of science developed by this philosopher. Finally, we will compare their views, situating this discussion in the context of significant trends in contemporary philosophy of science. We will also suggest an avenue for further developments.

2 Gould on the Individuation of Theories

Making use of architectural metaphors, Gould argues that the "foundations" of Darwinism are preserved, despite changes in its "structure", particularly in the last two decades of the twentieth century. As a consequence, we would have now a theory that remains within the domain of the Darwinian "logic", but must be construed as basically different from the "canonical theory of natural selection" (Gould 2002, p. 3).

For Gould, it is possible to identify a "central core" that remained intact in Darwinism, despite the changes in the framework of the theory (Ibid., pp. 6, 166). To demonstrate this continuity, Gould thinks it is necessary "…to describe a construct like 'evolutionary theory' as a genuine 'thing'—an entity with discrete boundaries and a definable history" (Ibid., p. 6). For him, to conceive a theory as a thing means to ascribe to it a historical continuity, the nature of a lineage, but also to consider it as an entity with "defining properties of anatomical form" (Ibid. id.). From this perspective, Gould launches a critical appraisal of Hull's characterization of theories as just lineages. He emphasizes the role of structuralist thinking, arguably capable of identifying the central core of a theory, something he regards as necessary to understand whether a "structure" altered throughout time still could be characterized as the same theory or not (Ibid., p. 7).[1]

Notice, however, that to regard lineages as "things" means to treat them as individuals. This raises a difficult ontological problem. In the biological realm, for instance, organisms are usually regarded as individuals. But it does not seem straightforward to conceive single lineages, such as species, or bunches of lineages, such as clades, as individuals in the same sense. Species and clades are usually regarded as classes, not individuals. But recently evolutionary biologists have been arguing that the living world contains individuals at levels higher than the organismic. Gould (Ibid., pp. 71, 597–613), for example, argues that organisms are not the only biological entities showing the requisite properties of Darwinian individuality. These properties include both vernacular criteria (definite birth and death points, sufficient stability during lifetime, the nature of true entities with boundaries) and more specific Darwinian criteria (production of daughters, inheritance of traits). When we consider these properties, it becomes clear that species, for instance, can be construed not only as classes, but also qualify as individuals. Therefore, they can potentially undergo selection (Ibid., p. 72). Generally speaking, lineages can be conceived as entities endowed with historical continuity, i.e., as individuals, which are subject to selection at several hierarchical levels.

Now, if we take theories to be historical things, such as lineages, a structuralist viewpoint (such as Gould's) entails that the individuation of any theory should depend on the identification of a central core preserved throughout historical changes. Gould locates this proposal between two extremes: one, "minimalist"—in which a theory is individuated merely by its insertion in a line of historical descent, without regard to any "essence"—yes, he blatantly uses the term—or "shared content"; and another extreme, "maximalist"—aiming at an exhaustive list of necessary and sufficient conditions for a theory to be individuated (Gould, ibid., p. 7).

Gould imputes the "minimalist" position to Hull, for whom theories are merely tokens related by descent, while the "maximalist" position claims that theories should be types. He recognizes something in Hull's position he is inclined to accept, though, namely the treatment of theories as 'things' or coherent historical individuals (Gould, ibid., id.). He disagrees with Hull when the latter argues that, to count as the same theory, it is sufficient that two conceptual tokens be part of the same lineage (Hull 1988, p. 17). Gould accepts this as a *necessary* condition for individuating a scientific concept or theory, but not, as Hull intends, as a *sufficient* condition. Gould requires, furthermore, the identification of the "morphology" or "idea content" of theoretical lineages (Gould, ibid., p. 8). One should take into account "conceptual continuity", a shared content that constitutes the "essence" of a theory, "operationally definable as minimal sets of propositions so crucial to the basic function of a system that their falsification must undermine the entire structure" (ibid., p. 11).

Gould attempts, then, the following *reductio ad absurdum* of Hull's proposal:

> "…a pure criterion of continuity, imbued with no constraint of content, forces one to apply the same name to any conceptual lineage that has remained consciously intact

[1] Concerning the relationship between structure and function, a structuralist gives more emphasis to the structure of entities and, on the grounds of it, makes inferences about function. A functionalist, in turn, focuses on function, and, then, makes inferences about structure. Darwinian theories have been much aligned to functionalist thinking, with structuralist approaches usually appearing in anti-Darwinian perspectives. Recently, however, a new prospect for combining structuralist and functionalist thinking in Darwinian evolutionary thought seems to be emerging through efforts in fields such as evolutionary developmental biology.

> and genealogically unbroken through several generations (of passage from teachers to students, for example), even if the current 'morphology' of concepts directly inverts and contradicts the central arguments of the original theory" (ibid., p. 8).

And he offers an example:

> "Thus, on this account, if the living intellectual descendants of Darwin, as defined by an unbroken chain of teaching, now believed that each species had been independently created within 6 days of 24 h, this theory of biological order would legitimately bear the name of 'Darwinism' " (ibid., id.).

Hull seems to acknowledge this as a possibility: "A proposition can evolve into its contradictory" (1988, p. 18). We will come back to this point later.

Anyway, in Gould's view this is an unacceptable implication of the "minimalist" position:

> "…if I wish to call myself a Darwinian in any fair or generally accepted sense of such a claim, I do not qualify merely by documenting my residence within an unbroken lineage of teachers and students who have transmitted a set of changing ideas organized around a common core […]. I must also understand the content of this label myself, and I must agree with a set of basic precepts defining the broad ideas of [this] view of natural reality […]. In calling myself a Darwinian I accept these minimal obligations […]; but I do not become a Darwinian by the mere default of accidental location within a familial or educational lineage" (ibid., p. 9).

Gould also requires a commitment to the "essence" of Darwinism, which he operationally defines by means of three "central features": Agency, Efficacy, and Scope (ibid., pp. 12–15, Chap. 2).[2] They would constitute the "core" of any "Darwinian" theory, its "truly essential claims", to be distinguished from a "larger set of more peripheral assertions" (ibid., 124). These features play in the structure of the Darwinian theory the central role of explaining how the abstract mechanism of natural selection—which is surely also part of the "essence" of Darwinism—is made concrete in nature.

It is important to examine how Gould addresses the concept of "essence" in evolutionary biology, since his approach to the individuation of theories involves a metaphorical extension of this concept to epistemology. He proposes that the term "essence" should be reintroduced in biology in a manner that evades an interpretation in terms of a Platonic *eidos*. For Gould (ibid. p. 10.), "…a meaningful notion of essence in biology" should be grounded on the role played in evolution by constraints of structure and history, expressed as *Bauplans* of higher taxa. He argues that a notion of essence compatible with current biological thinking becomes more plausible when we consider the shifting from the idea of "…a disembodied and nonmaterial archetype employed by a creator" to that of a common ancestor. A *Bauplan* should be understood as "…an actual structure (or inherited developmental pathway) present in flesh and blood ancestor—a material basis for channeling, often in highly positive ways, the future history of diversity within particular phyletic lineages" (ibid., p. 11).

[2] The principle of agency refers to the level (or levels) in the biological hierarchy that is (or are) the locus (or loci) of action of natural selection. Darwin claimed that the individual organism is the level in which natural selection acts. The principle of efficacy explains what natural selection is able to do. Darwin, for instance, insisted that natural selection is not a minor and negative mechanism, capable only of eliminating the unfit, but plays a positive role in evolution, producing the fit. The principle of scope concerns the spectrum of evolutionary phenomena explainable by natural selection. Darwin argued that the theory of natural selection can be extrapolated to explain the whole range of evolutionary phenomena. For details on these principles and the debates about them throughout the history of evolutionary thought since Darwin, we refer the reader to Gould (2002).

Can a *Bauplan*, i.e., the fundamental body plan of organisms, have causal powers? Or would it be merely a concept reached by abstracting a set of common structural characteristics among organisms? Gould adopts the first stance, claiming that the set of particulars is united by these common characteristics into "meaningful relationships of common causal structure and genesis" (ibid. id). If *Bauplans* were only abstract concepts, it would be hard to understand in what sense they might "act" as "fundamental building blocks", and, in fact, contain "particular incarnations" (e.g., species) through this mode of action or functioning, as Gould proposes.

We are not sympathetic, however, towards the idea of bringing back the notion of "essence" to either biology or epistemology. Although biology needs, to be sure, some concept that grasps the structural plan of organisms in its universality—avoiding at the same time an appeal to an abstract Platonic *eidos*—to name this concept "essence" is not a good idea. And the same is true of theories. After all, this term carries over its shoulders the weight of meanings incompatible with population thinking.

Despite their disagreements, Gould and Hull concur, actually, in a kind of evolutionary historicism concerning the nature of theories: both take them to be historical entities, as biological species are.[3] Biological species are lineages for them, individuals that satisfy, as much as more familiar individuals (say, organisms), the criteria for individuality spelled out by Gould himself (see above).

If theories are historical entities as well and a general selectionist framework is applied to science, they can be described as evolving, on a par with biological species. This is, indeed, Hull's project. He attempts to articulate an account of selective processes sufficiently general and abstract to be able to accommodate a broad range of phenomena, including scientific dynamics. His theses about the nature and individuation of scientific theories have to be contextualized in his selectionist theory of science, which we address in the next section.

3 Hull's Selectionist Theory of Science

Gould mentions, in his 2002 book, the first chapter of Hull's *Science as a process* (1988), especially the section "Conceptual Lineages", in which the latter proposes his "general analysis of selection processes".

Hull's selectionist theory of science presupposes a formulation of the Darwinian process so as to make it applicable not only to any possible life form but also to any structure, even beyond the biological domain, that might be seen as adapted (or able to increase its adaptability). For this purpose, he redescribes in more abstract terms the entities involved in the biological evolutionary process, calling them "replicators", "interactors" and "lineages" (henceforth 'RIL formulation').

Replicators and interactors are defined in terms of their functions or causal roles. This functionalist description makes it possible to pin down the concrete things playing those roles at a particular hierarchical level in a complex system. Since replicators and interactors are defined functionally, different things in different systems—or even at different hierarchical levels in one particular system—might play these roles. They are defined in terms of the abstract selectionist description that has, so to speak, a methodological priority, helping to find out the entities (or structures) that realize those functions. For lack of space, we will not expand on the RIL formulation as applied to the biological realm, but, instead, we will look at which entities, in science, might play the role of replicators, interactors, and lineages.

[3] Hull acknowledges his debt to Toulmin for using the notion of 'historical entity' (Hull 1985, pp. 780–1).

It is well known that Dawkins called memes the units in conceptual evolution. Hull regards a scientific concept a meme, playing the same role as a gene in biological systems—that of a replicator.[4] A conceptual system (in science, typically a theory) is taken to be a set of memes that plays the same role as the genotype in biology.

Hull requires, nonetheless, a material basis for memes. In the case of science, this material basis can be the brains of scientists, besides the media used to communicate scientific conceptual systems: papers, books, computers, etc.[5] Replication occurs by the communication of conceptual systems through those media that play the role of vehicles for these memes.

Hull (1988, pp. 12–3; cf. 2001a) claims that he is not proposing an evolutionary epistemology. Nonetheless, he still addresses the epistemological issue of the appraisal of scientific theories when he claims that what corresponds here to the biological phenotype are the consequences inferred from theoretical principles—a 'conceptual phenotype'. These consequences are compared to observations that play the role of a selective empirical environment. Therefore, conceptual lineages result from the interplay between replicators, interactors, and this kind of environment.

Hull argues that, in a similar way to the expression of the genotype into a biological phenotype, much information is lost in those derivations of the consequences of a theory. Although just a small subset of all the potential consequences of a theory is effectively derived, it is still the case that the theory is tested on the basis of such consequences: "A particular experiment or observation bears on only one small part of the meaning of the theoretical term" (Hull 2001b, p. 39). In other words, the success or failure in the replication of these conceptual systems depends on this slim "interface" between the conceptual phenotype and the empirical environment.[6]

One implication of Hull's selectionism is the underdetermination of theory by data and the Duhem-Quine thesis:

> "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 […]. There are no absolutely crucial experiments" (ibid. p. 40).

Hull rejects, however, the proposal of looking at the evolution of science as a simple change in meme frequencies, in the same way as he (and Gould) rejected a bookkeeping view of biological evolution as just a change in gene frequencies—a position known as "gene selectionism".

Gould's arguments against gene selectionism are based on the idea that replicators, at the genetic level, are basic units of recording, of 'bookkeeping', and not causal agents. Causality in the evolutionary process takes place, fundamentally, at the level of the interaction of biological systems with the environment, and, consequently, units of selection should be defined as interactors, not replicators (Gould 2002, p. 622). Gould rightly recognizes the great merit of Hull for introducing the distinction between interactors and replicators, particularly

[4] Actually, science has different kinds of replicators as "elements of the substantive content of science—beliefs about the goals of science, the proper ways to go about realizing these goals, problems and their possible solutions, modes of representation, accumulated data reports, and so on" (Hull 2001b, p. 116).

[5] Therefore, there is no place in Hull's theory for an autonomous Popperian "third world". Cf. (Hull 2001b, p. 33).

[6] Analogously, a biological genotype is 'tested' through a particular phenotype that expresses just part of its possible exemplifications. Genes and genomes "succeed or fail in replicating themselves, depending on a relatively small number of actual exemplifications of all possible exemplifications permitted by the information they contain" (Hull 2001b, p. 39).

because it avoids the conflation of recording ('bookkeeping') and causality, the fundamental logical error of gene selectionism.

Hull evades 'bookkeeping' in biological and scientific evolution alike. The latter is not seen just as a change in concepts frequencies. Causal interactions are taken to be as fundamental in conceptual evolution as they are in biological evolution: in the former, interactions involve the vehicles for the memes– social interactions and also interactions with the physical environment. Hull looks for selective mechanisms that might explain evolution in science (Hull 1988, p. 17; 1981). Therefore, he has to take into account causal relations, besides logical relations. A meme or conceptual phenotype cannot enter by itself into causal interaction with the physical world. We should recognize that theories are necessarily tested through scientists, especially through their experimental apparatuses. If the focus is on causation, one has to take into consideration the relevant interactors:

> "Omitting reference to the interaction in conceptual change leaves out not only the testing part of conceptual change but also the tester—in cases of science, the scientist" (2001b, p. 42).

Therefore, Hull is not just proposing a kind of memetics in the realm of scientific change.[7] In the next section we will analyze some implications of adopting a selectionist framework like Hull's:

(i) The test and replication of theories have a local and, to some degree, accidental character, depending on various historically contingent issues, like the lineages of scientists the theories are associated with. The "origin" of theories is, therefore, a chief concern;
(ii) The local processes relevant for explaining conceptual evolution in science comprise the particular methods, models, idealizations, instrumentations, etc. employed by scientists to bridge the gap between theories and the empirical world. We should look for actual inferences made by scientists, not for possible inferences, as philosophers of science usually do.

4 Theories as Historical Entities

Hull applies "population thinking" to the history of ideas, just as he does with regard to the individuation of biological species:

> "…if conceptual change is to be viewed from an evolutionary perspective, concepts must be treated in the same way. In order to count as the 'same concept', two term-tokens must be part of the same conceptual lineage. Population thinking must be applied to thinking itself" (Hull 1988, p. 17).

[7] Notwithstanding, Hull rejects explicitly, in his 2001 book, a thesis he used to admit before—that scientists play a role analogous to biological interactors. He is here replying to critiques that pointed to some important disanalogies in this regard. Genes code for biological interactors, but we cannot say that ideas (or, more generally, memes) code for scientists. It is also true that, as Hull argues, the "differential extinction and proliferation" of scientists would not cause by itself the extinction and proliferation of memes, what is the case in actual interactors. It is also the case that some scientists just pass on memes without trying to test them, playing only the role of passive vehicles. In our view, it is difficult, however, to completely reject the idea that scientists play also the role of interactors, that is, of "agents that facilitate interaction" (ibid., p. 4). We should, in this context, mention a critique we find in many authors (e.g. Sterenly 1994; Grantham 2000), namely that Hull completely ignores, in his selectionist theory of science, the cognitive processes in individual scientists. In this sense, his theory is not as embodied as we would expect. We will propose, at the end of the paper, a way to graft this cognitive dimension into Hull's selectionist account of scientific change. It might be fruitful, on this issue, to compare Hull's theory with Popper's or Campbell's evolutionary epistemologies.

According to Hull, as far as conceptual systems such as theories are evolving entities, they have the ontological status of individuals, of tokens in conceptual lineages. Whenever a scientist comes up with a theory, she always expects it to be sufficiently general (to be taken as a type) and accepted across the board. Hull asserts that this purported generality is lost, though, whenever the theory is tested and transmitted to other scientists. What is actually replicated is not a type but just tokens:

"…Term-tokens are tested and transmitted locally but interpreted globally as types […]. Each generation of scientists intends for their conceptual systems to be generally applicable and universally accepted, but in each generation only a very small percentage of instances of these systems gets passed on, and the version of a particular conceptual system that eventually comes to prevail may well not be the one that early scientists intended" (Hull 2001b, p. 131; cf. 1988, p. 506).

Hence, one way of seeing the divergence between Hull and Gould concerning the topic of the nature and individuation of scientific theories is that the first gives priority to a 'phylogenetic framework', in which descent takes priority to similarity (Hull 1985, p. 778). Gould focuses, instead, on similarities anchored to a morphological type, reconceptualized as an inherited structure or developmental pathway. The relevance of structuralist thinking seems to be, indeed, a key point of disagreement between them:

"Instead of treating the historically unrestricted types as constituting the general framework in which conceptual change is investigated, as is usually done, an evolutionary analysis takes a phylogenetic framework as basic; then conceptual types are periodically fitted into the interstices of this tree" (Hull 2001b, p. 41).

Adopting this phylogenetic framework, in which theories are treated as conceptual lineages, it becomes conceivable that, in the long run, versions of the same evolving theory may contradict each other. This is an unacceptable consequence for Gould. While Hull goes on to articulate the far-reaching and sometimes counter-intuitive consequences of his selectionist approach, Gould attempts, actually, to save our common-sense intuitions regarding theories—according to which they are types, not just tokens—although this raises difficulties of its own, as we will point out in Sect. 6.

Hull admits that conceptual/linguistic tokens that contradict each other might pertain to the same conceptual lineage, given the necessary heterogeneity of conceptual lineages and the fact that they are "loose" and can change indefinitely through time:

"At any one time, they can contain contradictory elements, and a particular statement-token can give rise through successive replications to a statement-token that contradicts it" (ibid., p. 127).

Hull does not see in this, however, a *reductio* of his theory— as Gould does—but a necessary consequence of taking seriously the thesis that scientific dynamics is an evolutionary process. An analogous situation is observed in biological evolution: sometimes there is more heterogeneity within a particular lineage than between that lineage and its close relatives (ibid., p. 126)!

But it is undeniable that there are problems here: given this heterogeneity, how can we name or refer to a conceptual lineage in a 'non-ambiguous' way? How can we group different tokens, despite their differences, and claim that they are 'of the same type'?

5 Conceptual Lineages and the Type Specimen Method

To deal with the problem of naming a heterogeneous conceptual lineage, Hull proposes the application of the same method used by systematists to name a biological species. When a specimen of an unknown biological species is discovered by a systematist, she gives a name to it, and that name is subsequently used to name all individuals of the same species. This specimen now represents, in a certain way, the species. Even if the specimen is not really typical, it keeps being the reference for the name of the species. The systematists communicate to each other the specimen and the name associated with it. By using this *type specimen* method, a name refers *rigidly* to a species (or to a lineage), despite the modifications the organisms pertaining to it may undergo as a consequence of evolution.

Hull strongly suggests that the type-specimen method can be used to individuate any historical entity, including theories, in order to solve the problem of the heterogeneity within a lineage. For instance, to give a name to a particular version of a theory (say, to call it a "Darwinian theory"), we have to locate it in a certain lineage, by tracing back the causal links of transmission of conceptual-theoretical tokens. The naming is *not* based on any shared content (or meaning), abstractly characterized, as Gould proposes, but just on descent.

In this context, Hull criticizes how the problem of meaning has usually been addressed, i.e., in terms of just abstract relations between words and concepts. He tells apart two ways in which different instances of a term (two term-tokens) might be grouped:

(a) Grouping based on similarity of meaning (content), without taking into account historical links, i.e., descent. By adopting this approach, two term-tokens are subsumed under the same type if they have the same (or similar) meanings. Types in this case refer to meaning, to abstract concepts; historical genesis is considered irrelevant for the grouping.
(b) Grouping based on descent, on causal chains of replication and transmission. Two term-tokens are grouped if they pertain to the same lineage, to the same "sequence of replicators", notwithstanding possible (and expected) meaning change (for instance, changes in the way the terms are "connected to their referents"). In this case, we have "identity by descent" (Hull 2001b, pp.130–1).

As far as one is concerned with the way terms and concepts *evolve*, the second way of grouping term-tokens is the relevant one, according to Hull: "If one wants to treat conceptual change as a selection process, then term-tokens must be grouped into lineages and trees by means of transmission" (Ibid., p. 131).

He recognizes the relevance, in certain contexts, of the first way of grouping, but argues that people and the historical processes in which they are engaged have necessarily to be included into the picture, since words get meaning through them—real agents partaking in causal interactions.

Going back to scientific dynamics, Hull acknowledges that "both ways of grouping term-tokens have their function in science" (ibid. id.). However, if one is concerned with the evolution of scientific theories, i.e., with a natural process, the second way should be preferred to group versions of theories.[8]

[8] It is pretty consensual to distinguish homoplasies from homologies, as far as biological cases are considered. To subsume particular tokens in a type due to an assumption of common descent is a serious mistake when one is dealing with homoplasies. Nonetheless, Hull certainly recognizes that the latter way of grouping has a function in biology, as elsewhere, depending on the question that is being asked. Furthermore, the RIL description is taken by Hull as a requirement for the proposal of laws in biology, precisely because "replicators" and "interactors" are conceptual *types* in contrast with other concepts that have been used in this science (like those of "gene", "organism", and "species"), which are not (Hull 2001b, p. 40). What about conceptual

It is also important for this way of grouping to keep track of the lineage of scientists who test, replicate, and transmit these theoretical versions. In effect, Hull insists that conceptual lineages and lineages of scientists should be "followed separately" (ibid., p. 127). Both lineages have much heterogeneity and are submitted to changes in time.

Hull's approach is able to deal with the historical possibility that different names be associated with different theoretical versions in the same lineage: these term-tokens can be grouped the same way we group what they refer to. Theoretical versions and their names are both seen as historical entities:

> "The type-specimen method works so well for historical entities because both the entity being named and the subsequent link-to-link transmission of its name form historical entities that can be traced independently of meaning change to see if, in the past, they intersect in the way claimed" (ibid., pp. 128–9).

Summing up: theoretical versions should be grouped by descent, by keeping track of the relevant replication and transmission events. One can name the same way any two versions, despite their conceptual differences, if they pertain to the same unbroken evolutionary lineage.

6 Gould, Hull and Historicism in the Philosophy of Science

As we saw, Gould adopts a structuralist perspective while addressing conceptual change. It is not clear, however, if this structuralist thinking is indeed compatible with historicism and, especially, with an evolutionary view of theoretical dynamics. As we said before, from a viewpoint like Gould's, we have to identify a central core of a theory in order to individuate it, despite the historical changes it might undergo. This takes us straight to the topic of the nature of scientific theories and their structure.

Hull's selectionist approach has evident metaphysical implications for this topic. He acknowledges that "most contemporary philosophers" conceive theories as sets of axioms (Hull 1995, p. 209). This, he argues, makes theories a-temporal entities. Therefore, he opposes two views of theories: (a) theories as sets of axioms; (b) and theories as historical entities (ibid., p. 210):

> "Conceptual systems as historical entities are the units of appraisal in most versions of the new philosophy of science. That a selection mechanism for conceptual change also requires conceptual systems to be treated as historical entities is yet another reason to treat them that way" (Hull 1982, p. 498).

Even historicist trends in the philosophy of science look, however, for something immutable in dynamic-theoretical entities. Hull's selectionist stance is an invitation to look at theories as historical entities in a more radical sense: they are "protean" and there can be a "total changeover in their elements" (Hull 1982, p. 479); still, they can pertain to the same

Footnote 8 continued
evolution? Hull emphasizes that homoplasies should be here distinguished from homologies as well. Is there also a place for *types* in this kind of process? Hull is inclined to answer affirmatively to this question: to explain this cultural process we attempt to instantiate a general selectionist theory of the same kind we successfully applied to explain biological processes (Hull 2001b, p. 41). Although Hull is not wholly explicit on this, the status of his general selectionist theory is, however, the same as that of any scientific theory. It is a historical entity, submitted to the very same processes of replication, selection and transmission it postulates for other theories. Therefore, we should expect that it will also evolve, like Darwinian theory, for that matter. We hope this paper will take part in this process!

lineage and, therefore, be named the same way. Therefore, Hull's selectionist historicism should be distinguished from more conventional forms of historicism in the philosophy of science, which presuppose immutable cores for individuating theories, or more inclusive units of knowledge, like scientific research programs.

Hull rejects, in fact, the idea that any scientific theory (actually any historical entity) has an essence. Gould tries, instead, to reconcile mutability and immutability in a way similar to more conventional historicist philosophies of science. Despite his indisputable evolutionary inclinations, Gould is not ready to accept that we can keep naming a conceptual system the same way if there is a "total turnover" of its elements, even if it is an element of an identifiable conceptual lineage. This indicates that he has a distinctive notion of the properties of historical entities, with an emphasis on the idea of *Bauplans*.

Gould does not articulate a consistent Darwinian conception about the nature of scientific theories, despite the original way he borrows the notion of channeling to apply it to the epistemological domain. If there is in Gould's approach an attempt to apply a Darwinist perspective to the problem at stake, it does not seem to be totally conscious. This prevents him from extracting the far-reaching consequences of the ideas he advances.

What, in the philosophical literature, might correspond to Gould's understanding of *Bauplans*, as applied to the dynamics of theories? We think of the notion of "analogy", proposed initially by Campbell (1920) and developed by Mary Hesse and other philosophers. In the 1950s and 1960s, they were looking for alternatives to the so-called "received view" about theories. A conceptual *Bauplan*, in Gould's sense, looks pretty much like Campbell's analogy, since its role is that of providing a "channeling" of scientific activity. Kuhn's notion of "paradigm" (understood as a set of exemplars) also plays this role and can be included, retrospectively, in the same lineage of philosophical ideas as Campbell's.[9]

Gould's approach looks also similar to Lakatos (1978) construal of scientific research programs, particularly with regard to the notions of "hard core", "negative heuristics", and "positive heuristics". Something akin to a negative heuristics and a hard core would follow from Gould's observation that theories present inherent "essences", operationally defined as sets of minimal propositions, the falsification of which would undermine the whole structure. Also, something like a positive heuristics is suggested by Gould's remark that this minimal set of propositions is "…so necessary as an ensemble of mutual implication that all essential components must work in concert to set the theory's mechanism in smooth operation as a generator and explanation of nature's order" (Gould 2002, p. 11).

We should not go too far in drawing parallels between Gould's considerations and Lakatos' theory of science. After all, their motivations for addressing the problems of the nature and individuation of theories are fundamentally different. Lakatos was concerned with scientific rationality, a philosophical problem not really central to Gould. The latter deals with these issues from a scientific perspective, as a way of addressing a circumscribed issue in evolutionary biology.

Even though the similarities between Gould's and Lakatos' approaches might be seen as merely superficial, we want to highlight that Hull explicitly rejects a Lakatosian hard core as a solution to the problem of conciliating immutability and conceptual change. He insists that hard cores and essences are the result of retrospective philosophical reconstructions (a point Lakatos himself admitted, by the way). Still more fundamentally, hard cores cannot *explain* the actual dynamics of historical entities like scientific theories. A selectionist approach like Hull's brings forth the intricacies of causal relations involving replicators, interactors and environments.

9 For a development of this argument, see Abrantes (1998, 2004).

7 Population Thinking, Variation, and Scientific Change

If theories are historical entities, it is expected that they exhibit significant changes as time goes by. Furthermore, as Hull asserts, many variations can also be detected synchronically: the versions of a theory adopted by the relevant scientific community at a given point in time constitute a population with great heterogeneity. This variation is, actually, a necessary condition for a genuine evolutionary process, be it in science or elsewhere.

Hull takes seriously population thinking in his understanding of scientific change. Replying to a remark by Dawson on the "protean" character of a philosophy called "Darwinism", Hull says:

> "I […] insist that Darwinism is not the only philosophy that is "protean". Every conceptual system, to the extent that it is successful, is just as protean. The only research programs that can possibly have an essence, a set of tenets that *all* and *only* the advocates of that program hold, are those that fall stillborn from the press or degenerate into ideologies. No matter what strategy one uses to pin down conceptual systems, they always succeed in slithering off the point before one's very eyes" (Hull 1985, p. 776).

From a population thinking perspective, one expects great variability in the theoretical versions adopted by the scientific community at any given point in time. This heterogeneity typically increases with time (Hull 2001b, p. 127; 1995, p. 196), what raises the troublesome problems we started to address in Sect. 4 and 5: if theories are "protean" and there is no immutable core in a conceptual lineage, how can they be individuated and named? Can Hull meet Gould's critique of the "metaphysics"[10] of scientific theories which derives from this population perspective?

Hull suggests that Kuhn's notion of 'exemplar' might help in this regard, if conveniently reinterpreted. A set of solved problems in a scientific research tradition can be taken as a type-specimen and used, as exemplars, to name a whole theoretical lineage, very much in the same way as a particular organism can be used to name a biological species:

> "One possible interpretation of Kuhn's notion of an exemplar is that it is designed to function as a type specimen. Even though scientific change is extremely complicated and at times diffuse, one still might be able to designate particular theories by reference to 'concrete problem-solutions', as long as one realizes that these exemplars have a temporal index and need not be in any sense typical…" (Hull 1995, p. 210; cf. 1988, pp. 113–4).

The function intended to be played by exemplars in Hull's type specimen method is not the same as in Kuhn's theory, though. In particular, exemplars do not have to be "exemplary" and they do not have to be "similar to their exemplifications", as Kuhn required (Hull 1982, p. 491).

Furthermore, to be a lineal descendent, a representation should have, in Hull's approach, the required causal linkages to the chosen exemplar. We already emphasized the central role that causal relations play in the individuation of scientific theories, according to an approach like Hull's: two versions of a theory are said to be versions of the same theory only if they descend from one another. Hull emphasizes that "descent is, after all, a causal relation" (1988, p. 448). Just similarity of structure is neither a sufficient condition to make this individuation, nor even necessary, as it is for Gould! We can conceive of theories (or sets of

[10] This term is Hull's. The title of Chap. 4 of his 2001 book is: "A mechanism and its metaphysics". Hull intends that chapter to be a summary of his 1988 book.

problem-solutions, for that matter) which are similar but cannot be individuated as the same theoretical lineage, if we fail to retrace the relevant causal linkages. If a bunch of theories satisfy these linkages by descending from each other, they should be named, nonetheless, the same way, even if they might be contradictory.

In Hull's scheme, to individuate a scientific theory, we have not only to keep track of a "conceptual historical entity" but also of a "social historical entity" (in this case, a lineage of scientists) and the interconnections between them.[11] Differently, the social dimension of science is not taken into consideration in Gould's account of the individuation of scientific theories. He is not concerned, for that matter, with the role played by scientists in an evolutionary scientific change.

A striking difference between Hull and Gould is that the latter is not interested in explaining the evolutionary dynamics of scientific theories, taken as historical entities. As a consequence, he does not address explicitly the relevant causal processes.

8 The Structure of Theories and Evolutionary Change: A Semantic Approach

It is a good old principle of philosophical methodology that philosophers have ultimately to come to grips with commonsense intuitions. We are, more often than not, wrong about our intuitions, but Hull apparently accepts that principle when he compares the view of the nature of theories championed by logical empiricists with his own proposal:

> "Viewing theories as sets (or clusters) of axioms does considerable damage to our intuitions about scientific theories. On this interpretation, most examples of scientific theories degenerate into unrelated formulations. Viewing scientific theories as historical entities also results in significant departures from our usual modes of conception. Perhaps scientific theories really cannot be interpreted as historical entities. If so, then this is just one more way in which conceptual evolution differs from biological evolution. The more these disanalogies accumulate, the more doubtful the entire analogy becomes" (Hull 1995, p. 210).

The received view (also known as the "syntactic view") has a counter-intuitive consequence: the outcome of a theoretical change is, more often than not, a "formulation" that cannot be logically related to the previous one. Hull acknowledges, nonetheless, that his own view also departs significantly "from our usual modes of conception". Maybe in a confrontation with our intuitions it fares no better than viewing theories as sets of axioms. Does Hull have the resources to make us revise our intuitions about the nature of scientific theories?

He does not systematically articulate a position about the structure of scientific theories that might be compatible with his selectionism. Hull is, nonetheless, far more explicit than Gould concerning the implications of his evolutionary approach to this topic, which is central to contemporary debates in the philosophy of science. We think further work is required for integrating Hull's type-specimen proposal for naming theories as well as the mechanisms put forth to explain the evolution of these historical entities, on the one hand, and a consistent view of their structure, on the other.

[11] Several commentators pointed out difficulties, however, in putting together the two "pillars" (Grantham 2000) of Hull's selective theory of science: the social and the conceptual.

As we pointed out in the last section, Hull appropriates Kuhn's notion of 'exemplar' in an attempt to deal with some of the troublesome implications of his general selectionist approach to conceptual change. In what follows, we want to work out his insights concerning the structure of scientific theories.

First, we would like to emphasize that exemplars are just one of the elements of a disciplinary matrix in Kuhn. He distinguishes the roles played by models and exemplars, even if this is not as clear-cut as we might expect (Kuhn 1970; Abrantes 1998). In a nutshell, exemplars are defined by Kuhn as concrete problem solutions. Models provide, instead, preferred analogies to the scientific community. We will bring to focus the latter element—models—and indicate how it might be integrated into Hull's selectionist framework.

In his more recent papers, Hull suggests that a semantic view of the structure of scientific theories is able to deal better with the selective nature of conceptual change than the received view. We will give here just some indications of how this might be fruitfully developed by adopting the semantic view as articulated by Giere (1988, 2001), in which theories are reconstructed as a hierarchy of models.

If a theory is reconstructed this way, instead of as a set of axioms, a particular version of a theory can be conceived as a bunch of models. Models are usually conceived as more concrete things than theories and, therefore, are more apt to play a local role in selection and in replication sequences (in the sense of being tokens, and not types) than more abstract entities:

> "Global systems are tested only in the form of 'versions'. What makes something a 'version' is not just similarity in structure. Descent is also required. Theories are best interpreted as families of models but these families have a necessary genealogical dimension" (Hull 2001b, p. 40).

The semantic view might be able to further clarify how different scientists solve different problems using different models and still be part of the same social lineage and contribute to the same conceptual lineage. Instead of a population of theories, we would have an evolving population of models connected through causal relations of descent.

In Sect. 4 we first remarked that scientists usually interpret scientific terms as being genuinely general—i.e., as types that gather similar tokens. One implication of looking at theories as historical entities in Hull's radical sense is that, since "only a few tokens actually get transmitted" (ibid., p. 41), the alleged generality of theories is lost in this process. By applying the semantic conception of theories, we are able to look at those tokens as models—what scientists communicate and replicate are bunches of models.

9 Hull, Gould, and a Philosophical *Conundrum*

The semantic view of theories, if allied with Hull's selectionism, might also eventually contribute to explain away a problem much discussed by philosophers: that of the incommensurability of theories.

Hull's diagnosis is that philosophical reconstructions of the products of scientific practice in terms of deductive inferences led to intractable (and spurious) problems, such as that of the purported insurmountable gaps in communication between scientists with different theoretical commitments. In reality, these commitments are usually too messy and complex for this kind of philosophical reconstruction to have any bearing on scientific activity.

Hull argues that scientists who disagree over fundamentals can still understand each other and collaborate:

> "…incommensurability is one of those rarefied, in-principle problems that seem important to those working on general semantic theories but seldom cause any problems in actual practice" (Hull 1988, p. 495).

The received view of scientific theories is one of those philosophical constructs that generate those "in-principle" problems:

> "According to the logical empiricist analysis of science, scientific theories are totally explicit, perfectly precise inferential systems. Either a statement is derivable from a particular theory or it is not. The conflict between two different theories must be needle sharp or nothing. Given the high level of precision assumed in this conception, slight differences in meaning can make a difference. But scientific theories as they function in science are much cruder…" (ibid., pp. 494–5).

Kuhn certainly contributed to dismantle the logical empiricists' view of scientific knowledge. Among the important points he made, we find the claim that scientific practice does not require one to follow explicit rules, and that it has a tacit dimension (an idea he took from Polanyi). It is surprising that, despite these ideas, he fell into the trap of incommensurability and, actually, helped bringing to the forefront those rather artificial and unusual situations in which it might arise.

Hull criticizes the monolithic and uniform character of a paradigm as portrayed by Kuhn. If a scientific community shows, in a tradition of normal science, this kind of uniformity, the passage from one paradigm to another, not surprisingly, looks entirely "arrational" (Hull 2001b, p. 43). Hull insists that intra- and inter-community variability is not only necessary for conceptual evolution, but a way out of this philosophical *conundrum*:

> "…once one acknowledges that considerable differences of opinion can exist within any socially defined community, the radical differences in kind between intragroup and intergroup communication disappear" (ibid. id.).

Conceptual heterogeneity is not an obstacle for communication and collaboration between scientists. In particular, collaboration can take place even if scientists do not mean "the same things by the terms they use" (ibid., p. 42).

Kuhn is an acknowledged chief influence on Hull's work (1988, p. 522). But he appropriates ideas like that of "exemplar" in a highly idiosyncratic way:

> "The role of concrete problem solutions is to allow scientists to move from one conceptual scheme to another in the absence of any explicitly formulated deductive connections. After all, if two systems are genuinely different, one cannot be a deductive consequence of another" (ibid., p. 112; cf. p. 289).

Kuhn never claimed that concrete problem solutions allow scientists "to move from one conceptual scheme to another". Exemplars play a role in normal science only. Kuhn overemphasized incommensurability, blocking the way to the possibility, imagined by Hull, that exemplars might facilitate navigation through different conceptual systems.

Can a semantic view of the structure of scientific theories help in understanding the grounds for collaboration between scientists and, also, in circumventing incommensurability? Applying this view, theoretical variability inside a community or between different communities would correspond to different families of models being used by different scientists to solve their problems. A particular version of a theory can "overlap" another version by sharing models (i.e., by sharing conceptual tokens).

Effectively, Hull emphasizes that continuity (besides cohesion) is a necessary property of lineages:

> "Lineages are also individuals but of a special sort. Lineages are peculiar in that the organization which they exhibit is sufficiently loose so that they can change indefinitely through time but sufficiently tight that the effects of selection are not lost" (Hull 2001b, p. 112).

A question that remains to be answered is the following: Do a family of models have the kind of cohesion and continuity required for them to be treated as historical entities, lineages, in Hull's sense? If we accept more inclusive conceptual units, like Kuhn's disciplinary matrices, two conceptual units might share elements of these matrices: symbolic generalizations, models, exemplars. If this is the case, we may cash out, after all, Gould's requirement of shared content, even though not at a global level.

10 Gould's *Reductio* Ultimately Fails, but a Nuanced Structuralism should be Grafted onto Hull's Minimalism

We are now better equipped to deal with the question we posed in Sect. 2, concerning Gould's alleged *reductio* of Hull's approach to the nature and individuation of scientific theories.

Let us consider the situation Gould describes: a group of scientists, connected by a genealogically unbroken series of generations with original Darwinians, develops a version of Darwinism that contradicts central tenets of the original version. The *reductio* would follow from the idea that we could not consider this group as being composed by Darwinians, but Hull's position would force us to do so.

From the arguments we elaborated throughout this paper, we can draw the conclusion that Gould is not successful in his *reductio* of Hull's stance. What Gould's argument really does is to take a snapshot of the moment, in Hull's account, in which a new theoretical lineage would be generated out of a previous one. By adopting the semantic approach, the picture we have is of a population of models which gradually changes through processes described in the RIL formulation. Eventually, this process of change in a conceptual lineage may result in a version of a theory which contradicts elements of an earlier theoretical version.

Hull characterizes historical entities as "…spatio-temporally localized particulars that develop continuously through time while staying internally cohesive. Historical entities may undergo total turnover of their constituent elements just so long as they do so gradually and remain sufficiently cohesive in the process" (1986, p. 17).

Hull's theory of science requires similarity relations which hold only at the local level, and, therefore, through a long series of events of communication of models, the local similarity between theoretical versions can be ultimately lost at the global level. Thus, rather different, even contradictory theoretical tokens can still be part of the same lineage. But, then, heterogeneity within a lineage of scientists comes to the forefront, and, consequently, the problem of communication of models between different groups of scientists. If these groups are yet communicating their models with each other, the causal linkages are preserved, and they should be treated as pertaining to the same lineage. If they do not exchange their models to a significant extent anymore—as the contradiction between the models makes it likely to happen—what we will have before our eyes is an analog of a speciation event in biological evolution: an ancestor family of models will give rise to descendant families. We believe that these ideas give us the means to avoid Gould's *reductio*.

If we take a snapshot of this process of change, as we believe Gould does, then we will have an apparent *reductio* of Hull's position. Nevertheless, we would have simply grasped a moment of separation of one family of models into two descendant families, i.e., of one conceptual lineage into two descendant lineages, due to a break in the communication of models between groups of scientists. As in biological evolution, the old family of models would be regarded as a common ancestor, from that point on giving rise to two fairly distinct families. Obviously, one of these families may go extinct, and in the end it may seem as if the ancestor theory/lineage simply turned into a new position which contradicts itself. But to conceive the nature of theories as being historical entities should not entitle us to construe their evolution as a single, unidirectional line in time. On the contrary, if we subsume an account of scientific evolution under a general selectionist framework, we should have dichotomies in our tree of theories. We expect in the long run to have "speciation" and the generation of new theories. It is true that from an evolutionary point of view all these boundaries are fuzzy, even if one requires, like Hull, that lineages should be "sufficiently tight".

What Gould takes as a *reductio* of Hull's position is precisely the sort of event the latter describes as a "bottleneck". Bottlenecks in conceptual evolution are related to the importance of small research groups (2001b, pp. 131–2; 1988, p. 156). As in the case of the founder effect in biological evolution, a small research group that develops a theory which contradicts most models of the lineage to which it was causally linked, will give rise to a new family of models. This new lineage will not have the same characteristics of the ancestor, since its founders will bear only a fraction, if any, of the variation present in the original family of models. As founder groups of new biological populations, founders of new scientific theories represent a statistically biased population of conceptual versions. Here, causal relations involving scientists as vehicles of theoretical versions (or bunches of models) are crucial for individuating lineages (of scientists and conceptual units), besides inferential relations (broadly conceived).

Therefore, a change from a previous family of models to a new one will look, retrospectively, like a leap in the evolutionary process (just as it can happen in biological evolution). In sum, it seems to us that the observation that a given scientific theory evolved into its contradictory can be explained in the same manner as we explain the lack of transitional links between two species by means of the founder effect. It is not a case of linear evolution, in which we might talk about theories evolving into contradictory propositions, but rather a branching evolution, in which we can picture ancestor theories giving rise to new families of models—replicated by new groups of scientists—some of which will not share substantial content with the original theory, but will be, nonetheless, historical descendants of the latter lineage.

A selectionist approach to science cannot dispense with similarity relations, as Hull claimed it ought to, but these relations will hold only at the local level, i.e., at each event of communication of models, while at the global level, rather different versions of models can be part of the same lineage. These hints show that a problem Hull pointed out a long time ago is still with us:

> "Specifying the relations that integrate distinct replication sequences into conceptual systems and distinguish different conceptual systems from each other remains the chief piece of unfinished business of the 'new philosophy of science' " (Hull 1982).

A semantic conception of the structure of scientific theories might, after all, bring to focus not only Gould's and Hull's divergences but, eventually, show a way to combine their approaches to conceptual change! A version of the semantic view called "structuralism" can be likened to some of Gould's theses, especially those related to a possible analog, in the case of theories, of the notion of *Bauplan*, as well as his essentialism (Díez and Moulines 1997; Lorenzano and Díez 2002).

If structuralism is consistently applied to the conceptual realm, it should point to a material substrate and to concrete entities that are able to bear causal powers and, consequently, to generate explanations in this realm. It is not obvious, however, if Gould is a consistent structuralist in this aspect, as far as conceptual dynamics is concerned.

We can think of a reformulation of Hull's selectionist theory of science that keeps its naturalistic emphasis on causality and also assimilates a developmental perspective inspired by Gould—in which conceptual *Bauplans* play negative (constraints) and positive (modeling/exemplary problem solving) roles in theory construction. In this new theory of science, Kuhn's exemplars might also be exemplary, providing a channeling, a constrained generation of new versions in a theoretical lineage.

Anyhow, we cannot overemphasize that Hull's selectionist account of scientific activity brings causal relations to the forefront besides the acknowledged role played by inferences in traditional philosophy of science. We have to recognize, however, that Hull's theory does not take into consideration the cognitive processes in individual scientists, an element that cannot be missing in any causal account of conceptual change. Addressing scientists' cognitive processes may be a way of integrating Hull's naturalistic emphasis on causality and a role for conceptual *Bauplans* as constraining factors in conceptual development.

The details of this selectionist–structuralist account of scientific dynamics are yet to be filled out. It opens up a whole avenue for research, interestingly in a time in which evolutionary biologists are themselves engaged in building a similar sort of integration, stimulated by findings concerning the evolution of development, genomic architecture, and so on.

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