DARWIN'S EVOLVING LEGACY

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METHODOLOGICAL ISSUES IN THE DUAL INHERITANCE ACCOUNT OF HUMAN EVOLUTION

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Dual inheritance theory is one of the main contemporary approaches to human evolution. It is actually an aggregate of various theories, and I have no intention of appraising them all here—my focus is, rather, methodological.[1]

Richerson and Boyd are stimulating in this regard, since they are they are willing to make explicit the methodological underpinnings of their work. Their second order discourse is intended to provide justification of the methods, conceptual tools, cognitive aims, etc., that are deployed in their properly *scientific* business.

Richerson and Boyd's methods and their methodological views (that is, their theory of method) will be illustrated with their account of the evolution of social learning by imitation.[2] I will highlight, especially, the way in which they use mathematical models, empirical data—of different kinds— and intuitions, responding to different inquiries.

Dual inheritance theory (DIT) is overarching: it deals with all the central issues about human evolution. How does one devise such a theory, aimed at explaining complex phenomena of various kinds? A straightforward answer would be: constructing realistic models in an attempt to mirror the complexity of the phenomena.

Richerson and Boyd argue that we should strive, instead, to build simple models of different aspects of the phenomena under investigation. The final theory, composed of those simple models, might be

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[1] For a critical overview of Richerson and Boyd's theory, see Abrantes and Portela, 2011.

[2] What I call "imitation" here refers to the same modality of social learning named by some authors "true imitation" or "observational learning". It will be clear in what follows that there are other modalities of social learning besides imitation, but those are not able to support a new inheritance system and, therefore, cultural accumulation.

complex after all. They are here advocating a methodology inspired by Leibenstein (1976).

Leibenstein conceives theories as a set of relations in which parameters are "sufficiently specified" so that they assert something "about the world of facts" (1976: 17). This is not the case with "analytical frameworks [which] may be looked upon as the mold out of which the specific types of theories are made." Hence, theories are falsifiable but not analytical frameworks that "may be looked upon as toolboxes from which we can fashion theories to explain events, but they are not themselves theories" (p. 18). Leibenstein calls these structures fashioned out of analytical frameworks "sample theories" or alternatively "sample models."[3]

Following quite closely Leibenstein's "theory of theories," Richerson and Boyd argue that instead of complex models, in certain areas the aim should be to grasp several purportedly similar processes through building sample theories:

> Students of complex and diverse subject matters develop a large body of models from which "samples" can be drawn for the purpose at hand. Useful sample theories result from attempts to satisfy two competing desiderata: they should be simple enough to be clearly and completely grasped, and at the same time they should reflect how real processes actually do work, at least to some approximation. A systematically constructed population of sample theories and combinations of them constitutes the theory of how the whole complex process works (Boyd and Richerson, 2005: 404).

This is not a *theory* as it has been conceived by several generations of philosophers of science. In particular, Richerson and Boyd's view is certainly not captured by the received view of the structure of scientific theories. But it seems they are also arguing for something quite different from a semantic view of theories:

[3] Leibenstein's 1976 book has a whole chapter on what one could call the *philosophy of economic science.* He conceives "sample models" as resulting from "analytical frameworks [and] should be seen as containing sample propositions. These propositions are essentially relationships that in themselves are not necessarily true. They are samples in the sense that they suggest the form that the theory should take.." (p. 22). Later on, some relationships in those sample models can be specified; but "in the absence of specific studies, or in the absence of specific policy situations, the sample theories are not to be looked upon as working theories but simply as illustrations of the kinds of working models that could be created" (p. 26).

The study of complex, diverse phenomena like organic evolution requires complex, multilevel theories but ... such theories are best built from toolkits made up of a *diverse* collection of simple models. Because individual models in the toolkit are designed to provide insight into only selected aspects of the more complex whole, they are necessarily incomplete. Nevertheless, students of complex phenomena aim for a reasonably complete theory by studying many *related* simple models (Boyd and Richerson, 2005: 397, my emphasis).

These knowledge structures and the associated methods are considered adequate to the biological and also the social sciences.[4] However, it is commonplace in the latter to dismiss the usefulness and applicability of a mathematical language (Boyd and Richerson, 2005: 377).

It is undeniable that we are much more used to verbal arguments than to mathematical-formal ones. Richerson and Boyd point out, however, the limitations of verbal descriptions: they usually have a qualitative character whereas reality has quantitative dimensions. Besides that, natural languages are plagued with ambiguity; the words we use often take different meanings in different contexts.[5]

In what follows I will present Richerson and Boyd's justifications for what I will call a "sample methodology": the use of simple and mathematical models to understand complex processes, such as human evolution.

COMPLEX OR SIMPLE MODELS?

The *drawbacks* of complex models are the following:

a] They are hard to understand:

[4] It is debatable whether these structures and methods are also typical in sciences, such as physics, which was the reference for most philosophers of science in the last century.

[5] However, Richerson and Boyd acknowledge that to give a verbal description (prose explanation) of the mathematical models can be helpful. It is a way of examining the acceptability of particular presuppositions, avoiding the risks of the "garbage in, garbage out" phenomenon, associated with mathematical-deductive reasoning (Boyd and Richerson, 1985: 30; Boyd and Richerson, 2005: 433).

> To substitute an ill-understood model of the world for the ill-understood world is not progress. In the end, the only way to understand how such a model works is to abstract pieces from it or study simplified cases where its behavior is more transparent. Even when complex models are useful, they are so because we understand how they work in terms of simple models abstracted from them (Boyd and Richerson, 2005: 402).

Richerson and Boyd argue that understanding is the chief epistemic virtue in scientific activity, but it might betray engrained habits of thought. The historical case of William Thomson complaining about his difficulties in intuitively grasping Maxwell's electromagnetic theory comes to my mind. W. Thompson required that every acceptable theory in physics should be constructed out of mechanical models, which could be more easily visualized (at least for him, used to working with this kind of models in agreement with a mechanistic image of nature).

Richerson and Boyd seemingly are not committed to any particular image of nature—the use of simple models is just "tactical." Nonetheless, there is a defense of this methodology that has to do with our cognitive limitations.

b] Complex models are not adequate to tackle processes which are diverse, besides being complex.

Evolutionary processes are of this kind (more on this below), as well as those that are studied by the human sciences.

c] Complex models are difficult to analyze.

To work out their implications is a hard task since they use a large number of variables and presuppose an even larger number of relations between them. Equations in those models usually do not have exact solutions and we have to rely on numerical methods. Even with the help of powerful computers this work is time-consuming and costly.

The arguments Richerson and Boyd offer *in favor of* a sample methodology are the following:

a] Simple models help "to school our intuitions" (Boyd and Richerson, 2005: 377; Richerson and Boyd, 2005: 98).

Intuitions are often error prone, especially when we deal with complex systems and processes. Given our cognitive limitations, we cannot avoid the use of simple models, after all (Boyd and Richerson, 1985: 30). They help to prevent the investigator from getting lost in the details of an intractable complexity, from losing sight of what is relevant in the phenomena under study: "In our own case, at least, the formal exercise of reducing intuitive notions to mathematical propositions and deriving results has often led to unexpected conclusions" (1985: 30).

In a more recent paper, they are even more emphatic about the advantages of this methodology:

> Bit by bit, models can be used to dissect the logic of complex systems. The sharp contrast between the difficulty of making good models and their manifest simplicity compared to the phenomena they seek to understand is a humbling, even spiritual, experience (Richerson and Boyd, 2005: 256).

b] Mathematical models help to check whether our descriptions are "deductively sound" (2005: 240).

They compensate for the limitations of our mental powers in deducing the implications of our assumptions (Boyd and Richerson, 1985: 30) and, therefore, they play a role as "prosthesis" to our minds (Richerson and Boyd, 2005: 240). Our intuitions and verbal descriptions are often inconsistent, but it is difficult to show this inconsistency.

c] Richerson and Boyd emphasize also the role played by sample theories in helping to "capture the generic properties of the processes of interest" (1985: 24-5).

The diversity of evolutionary processes, for instance, should not compromise the quest for generality. Natural selection can be seen as a taxonomic construct that discloses the similarities between tokens (or sub-types) of various types of processes (e.g. variation, differential reproduction, inheritance):

> Basic theoretical constructs like natural selection are not universal laws like gravitation; rather they are taxonomic entities, general classes of similar processes which nonetheless have a good deal of diversity within the class (Boyd and Richerson, 1985: 25).

Despite the advantages of this methodology, Richerson and Boyd (2005: 377) acknowledge that simple models are limited in their predictive power since they are idealized and unrealistic. So the construction of complex models is justified in technological and other practical tasks where prediction is at stake (meteorology is a good example). However, predictive power is not guaranteed by the use of complex models. Besides the problem of computational power availability, for a complex model to make predictions it requires data for all the parameters it assumes. These data are, in general, lacking (Boyd and Richerson, 1985: 26). If this is the case, in using complex models "it is easy to make large sacrifices of understanding for small gains in predictive power" (Boyd and Richerson, 2005: 403).

In any case, prediction should not be privileged in place of explanation and understanding. The latter are "scientifically far more fundamental" (Boyd and Richerson, 2005: 403; *cf.* Leibenstein, 1976: 12-13). However, the aim of getting understanding through simplicity and idealization cannot avoid a minimum commitment to realism:

> The sample models are caricatures. If they are well designed, they are like good caricatures, capturing a few essential features of the problem in a recognizable but stylized manner and with no attempt to represent features not of immediate interest (Boyd and Richerson, 2005: 404).

Some of Richerson and Boyd's claims disclose an epistemological modesty, even a moderate skepticism concerning the possibility of obtaining knowledge—at least in areas like evolutionary biology. In the context of their attempts to come up with a theory of cultural dynamics, they ask themselves: "Are the models a true depiction of the adaptive properties of culture? Unfortunately, we don't know" (Richerson and Boyd, 2005: 119).

They appeal, again, to our cognitive limitations: "Simple models, simple experiments and simple observational programs are the best the human mind can do in the face of the awesome complexity of nature" (Boyd and Richerson, 2005: 377).

The fact that science has a pragmatic dimension is also explicitly recognized. Models which have shown their fruitfulness in biology can be exported to solve problems in other areas.[6] When Richerson and Boyd argue for the application of evolutionary models—taken mostly from population genetics, but also from evolutionary game theory and decision theory—in explaining phenomena in the domain of the human sciences, they plead for a methodological and not an ontological stance: Darwinism is just a "tool kit" and not an "uni-

[6] For an account of analogical reasoning as a problem-solving strategy, see Abrantes, 1999.

versal acid" as Dennett claims (Richerson and Boyd, 2005: 119; Boyd and Richerson, 2005: 434; *cf.* Dennett, 1995). To recognize the heuristic role of Darwinian models does not imply, either, a commitment to the *universal Darwinism* program (Dawkins, 1984; Cziko, 1995, 2001).

Models help to explore the causal intricacy of complex processes, separating relevant factors from those that are (shown to be) secondary or plainly irrelevant: "The reductionism of evolutionary science is purely tactical" (Richerson and Boyd, 2005: 98). Reductionism has a "heuristic use" (Boyd and Richerson, 2005: 408).

Furthermore, model building does not aim at finding out laws:

> ...explanatory models are not laws but tools to be taken up or not as the situation warrants. Good models are like good tools: they are known to do a certain job reasonably well. Simple models that work well for a wide variety of jobs are an especially valuable part of the biologist's tool kit (Boyd and Richerson, 2005: 397).

The heuristic role of model failure is also congenial to that sample methodology. Wimsatt is a main reference here, explicitly acknowledged by Richerson and Boyd in several of their writings (see Wimsatt's 2007 collection of his papers on this topic).

In the next section I will illustrate Richerson and Boyd's modeling heuristics, as well as some of their methodological theses, in the way they conceive a "why-maybe account" (Richerson and Boyd, 2005: 127) of the origins of culture.[7]

THE ORIGINS OF CULTURE

What follows is a reconstruction, in the shape of an informal argument, of Richerson and Boyd's model building approach to come up with a plausible account of the origins of a new modality of inheritance system in the hominid lineage.[8] I will present the chief results

[7] Richerson and Boyd's "why-maybe accounts" are akin to Brandon's "how possibly explanations" (Brandon, 1995; Richerson and Boyd, 2005: 127, 305).

[8] It is important not to conflate the question about the origins of culture with a totally different one, about cultural evolution. I mentioned before that Richerson and Boyd proposed also several models for making the forces responsible for cultural dy-

of a series of nine models they built to face the conceptual and empirical challenges of an adaptationist stance in dealing with this topic (Boyd and Richerson, 2005, caps. 1-3).

In these models, several parameters are manipulated (often independently) in idealized settings:[9]

1] The rate of change of the environment;
2] The number of environmental states and the modality of environmental change—either discrete or continuous;
3] Genotypes for individual learners, imitators or both thought to exist in the population;
4] Rules employed in social learning—e.g. stipulating whom to copy and when;
5] The costs of individual and social learning.

I will start with their definition of culture:[10]

Culture is information capable of affecting individuals' behavior that they acquire from other members of their species through teaching, imitation, and other forms of social transmission (Richerson and Boyd, 2005: 5).

This definition grants the existence of culture in many species. There are, effectively, other modalities of social transmission besides imitation. I will show that the crucial issue in this account is *not* the existence of culture *per se*, but the possibility of cultural *accumulation.*

1] Mathematical models built by Richerson and Boyd in 1989 (Boyd and Richerson, 2005, cap. 1) showed that, given certain environmental conditions, observational learning (imitation) does enhance the fitness of individuals in a population since it affords an economy of

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namics explicit. This task is, however, a different one from that of devising a model of how culture, as a new kind of inheritance system, evolved in the first place on top of the genetic system!

[9] "Tinkering" might be a good word (not employed by Richerson and Boyd, though) to describe this kind of experimentation with selected parameters in models which picture idealized evolutionary scenarios. Cf. Wimsatt, 2007.

[10] "Culture" should be considered a theoretical term, that is, its meaning depends on the role this term plays in the DIT. Many notions of culture have been proposed, and they should be understood in the context of different research programs, with their particular commitments, problems being addressed, aim, an so on.

the costs associated with individual learning. Effectively, individual learning is error prone; information might not be available for the individual by its own; the costs of obtaining information in this way might be too high, etc.

Their first model is an exercise on what they call "strategic modeling," often used for behavioral traits (e.g. in behavioral ecology):

In strategic modeling, we begin with the tasks that the environment sets for an organism and attempt to deduce how natural selection should have shaped the species' adaptation to its niche [...] This is just the sort of modeling we have undertaken in our studies of social learning and culture. We ask: how should organisms cope with different kinds of spatially and temporally variable environments? (Boyd and Richerson, 2005: 75).

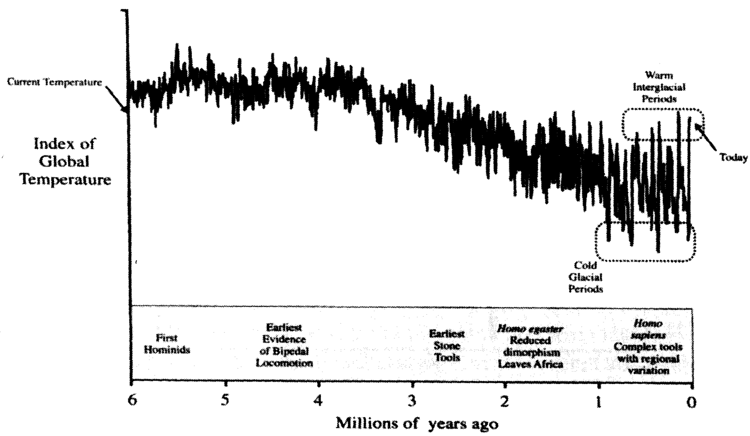

Figure 1. The world climate in the last six million years plotted against some crucial milestones in hominid evolution; from Richerson and Boyd, 2005: 133.

Figure 1 shows how the world climate became more and more variable in the last 6 million years until the Holocene—the starting point of a period of stability in which we still live. Their hypothesis is that social learning by imitation was an adaptation to cope with those extraordinary conditions.

2] Richerson and Boyd point, however, to a puzzle arising from this kind of adaptationist story: "The apparent rarity or at least lack of sophistication, of social learning in species besides humans is a considerable puzzle given our results" (Boyd and Richerson, 2005: 33).

We will see that this "adaptationist puzzle" (Richerson and Boyd, 2005: 100) is hard to solve, with an unexpected potential for jeopardizing their whole explanatory endeavor.

3] Mathematical modeling built by Rogers (1988) showed, surprisingly, that at the evolutionary stable situation, imitation actually does *not* increase the fitness of the population as a whole (mean fitness), composed of imitators and individual learners (contrary to the intuition grounding Richerson and Boyd's 1989 models).

The way Rogers himself interprets the implications of his models is instructive: "These results [...] contradict the widely held belief that, if the capacity for culture evolved through natural selection, then culture must, on average, enhance fitness" (1988: 6). This idea is certainly a possibility: the fact that a trait is selected for does not guarantee that it is adaptive!

Boyd and Richerson (2005, cap. 2) showed with further modeling—which handled other parameters in a number of different circumstances— that Rogers's results are robust despite the idealizations he introduced. Figure 2 captures the main result.

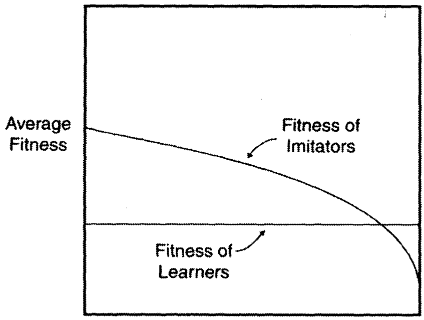

Figure 2. The fitness of the population as a whole doesn't increase if there are imitators in the population. As the number of imitators increases, their fitness decreases gradually and, at the equilibrium point, the average fitness is the same as if the population had only individual learners. This model presupposes that the fitness of the individual learners is constant, not being affected by the existence of imitators in the population. From Boyd and Richerson, 2005, p. 37.

Richerson and Boyd looked at this unexpected result as an opportunity for a deeper understanding of what is at stake:

Alan Rogers's very simple model in which social learning evolved without being adaptive led to some real insights into exactly what properties are needed *for culture to be adaptive.* Good models produce diamond-clear deductive insights into the logic of evolutionary processes (Richerson and Boyd, 2005: 256; emphasis mine).

Notice that Richerson and Boyd are here effectively making room for our intuition that culture *is* adaptive! It can be formulated as follows: humans present exceptional plasticity in their behavior compared to other primates. This trait made possible an extraordinary geographical spread. Culture comes up immediately as a proximate cause of this adaptability (other explanations would appeal to the genes, the environment or both).

4] If we start with the intuition that culture is *in fact* adaptive, Rogers's results are intriguing indeed. They can be evaded, however, if one can conceive ways in which individual learners enhance their fitness when there are imitators in the population. In further modeling (Boyd and Richerson, 2005, cap. 2), they explore two ways in which this might happen.

a] Imitation enhances the fitness of individual learners by making them more selective: they rely on imitation when they cannot tell by themselves which behavior is best (according to a particular learning rule), given the environmental conditions. Therefore, imitation lowers the costs of error in those conditions in which it is difficult to learn individually the adaptive behavior.

b] Imitation makes possible a cumulative culture, that is, a set of behaviors that cannot be learned by individual learning in a lifespan.

Figure 3 shows in what ways Rogers's previous results are evaded when conditions *a* or *b* are satisfied. I will focus on the scenario *b*, in which we do have complex behaviors available for imitation.

5] When the results of Rogers's models are eluded, we are, however, back to the initial puzzle: there are kinds of culture in other species but nothing comparable to a complex culture. In other words, if the availability of a new inheritance system (besides the genetic one) en-

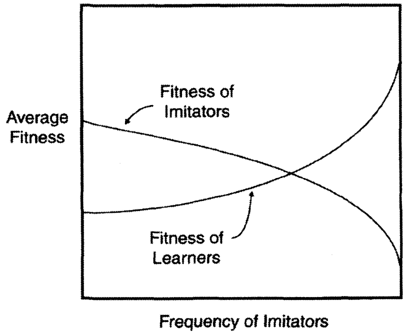

Figure 3. If the existence of imitators in the population reduces the cost of individual learning, or increases its possibilities and precision/correctedness, the fitness of the population as a whole increases. This is the case if imitators makes possible cultural accumulation: individual learning doesn't have to start, as if were, from scratch. From Boyd and Richerson, 2005, p. 39."

hances the average fitness of a population, why did it not evolve, as far as we presently know, in the great apes lineages, for example, which had to cope with similar environmental pressures as our own lineage?

6] The empirical evidence just referred to suggests that the models built hitherto did not capture something essential to the process: "models do not usually just fail; they fail for particular reasons that are often very informative" (*ibid.*; Richerson and Boyd, 2005: 98).

Model failure has, therefore, a heuristic value, pointing to factors that were opaque to unaided intuition:

> We have suggested that the ability of culture to couple individual learning to a transmission mechanism, thus to generate a system for the inheritance of acquired variation, could cause capacities for culture to evolve. However, this analysis also fails because it suggests that the advantages of culture are quite general, and hence that many organisms ought to have "Lamarckian" systems of inheritance. This failure in turn suggests that there are other costs to the inheritance of acquired variation that must be accounted for (Boyd and Richerson, 2005: 409).

In their first models, Richerson and Boyd assumed that learning by imitation had no costs. The failure of these models led them to propose the following hypothesis: imitation requires a *special-purpose* psychological mechanism. Other modalities of social learning, such as *local enhancement* or *stimulus intensification* require just the very same *general*-purpose psychological mechanisms as individual learning. Empirical evidence and theoretical results point to the limitations of the latter mechanisms to support social learning by imitation, or observational learning, which would require dedicated cognitive processing of some kind.[11]

7] The introduction, in the new models, of the costs of the capacity for culture on top of the costs of individual learning, illuminate Richerson and Boyd's strategy of manipulating simple models to find out what the relevant parameters are.

In the following steps I will take for granted that a cumulative culture, based on a new system of inheritance, requires a special-purpose psychological capacity for observational learning: mindreading (or a theory of mind).

8] When the costs of this psychological machinery for observational learning are taken into account in the 1996 models (Boyd and Richerson, 2005, cap. 3), however, we obtain as a result an *adaptive valley*!

This result can be grasped qualitatively as follows: if there are no imitators in the population, a complex culture cannot arise since it requires a faithful mechanism for the transmission of information. If there is no previous accumulation of culture (and, therefore, no complex behaviors available), the first individual who happens to be able to imitate (e.g. by some kind of weird mutation·) would not be fitter than the others: on the contrary, she pays the costs of the machinery but does not benefit from using it. This argument assumes that individual learning, or less costly social learning modalities, is sufficient for acquiring the behaviors that are adaptive. In these conditions, imitation cannot evolve at all.

Is the adaptive valley that follows from the modeling a robust result, despite the simplifications and idealizations that have been made?

[11] Even though Richerson and Boyd are not committed to the massive-modularity view of the mind that evolutionary psychologists argue for, the properties usually attributed to a cognitive module—they are innate, encapsulated and domain-specific—help to capture the idea of a special-purpose mechanism.

Richerson and Boyd are convinced that there is an obstacle for the evolution of individuals capable to learn by observation:

The exhaustive analysis of many sample models in various combinations is also the main means of seeking robust results [...] One way to gain confidence in simple models is to construct several models embodying different characterizations of the problem of interest and different simplifying assumptions. If the results of a model are robust, the same qualitative results ought to obtain for a whole family of related models in which the supposedly extraneous details differ (Boyd and Richerson, 2005: 410).

9] Supposing the existence of an adaptive valley, in which ways could it be crossed, by humans, at least? Richerson and Boyd propose a "roundabout path" along the lines of the so-called "social intelligence hypothesis":

If such an impediment to the evolution of complex traditions existed, evolution must have traveled a roundabout path to get the theory of mind module (or whatever) past the threshold necessary for bringing it under positive selection for the cumulative cultural adaptation. Some have suggested that primate intelligence was originally an adaptation to manage a complex social life... [social problems favored] a sophisticated ability to take the perspective of others. Such a capacity might incidentally make imitation possible, launching the evolution of the most elementary form of complex cultural traditions. Once elementary complex cultural traditions exist, the threshold is crossed (Boyd and Richerson, 2005: 139).

According with this hypothesis, mindreading abilities would be, actually, an exaptation of a *Machiavellian intelligence*, which evolved to solve adaptive problems in a social environment (and *not* to make cultural accumulation possible). In figure 4, I try to capture this idea.

A presupposition of this account is, therefore, that Machiavellian intelligence and imitation require the *very same ability* to adopt the perspective of the other (that is, to make imputations of mental states).

Is the adaptationist puzzle solved after all? At first sight it is *not*: this *roundabout path* could have been traveled by other species, such as the great apes, that also lived in complex physical and social envi-

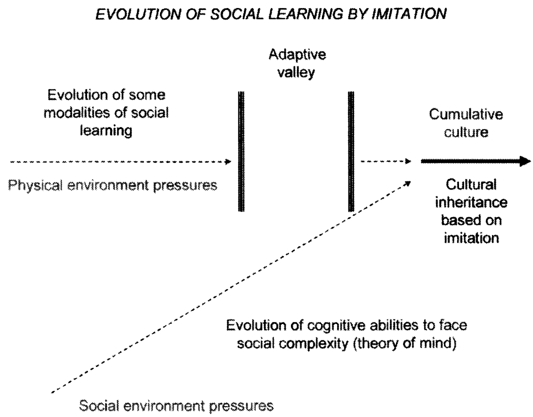

Figure 4. The *roundabout path* that made possible the evolution of the cognitive abilities required for cultural accumulation. Is it a plausible account?

ronments! All Richerson and Boyd have to offer, in face of the persistence of the puzzle, is the proposal that humans held possession first of the cultural niche. This niche can be occupied by those species which have a new inter-generational mechanism for the transmission of information:

> This argument provides one explanation for the rarity of cumulative cultural tradition: humans were the first species to chance on some devious path around this constraint, and then we have preempted most of the niches requiring culture, inhibiting the evolution of any competitors (Boyd and Richerson, 2005: 16).

This proposal is not very convincing though and looks like an *ad hoc* way out of the puzzle. Can we conceive other plausible trajectories across the adaptive valley? Abrantes (2006) and Abrantes (2010) explores compatibilist and niche construction scenarios for human evolution which can be explored in order to devise alternative hypotheses. These scenarios avoid also nativism concerning mindreading abilities, which is congenial to Richerson and Boyd's account.

With these remarks, I conclude this reconstruction. I hope it illustrates the *methods* used by Richerson and Boyd in their attempts to understand a particular dimension of human evolution, and also some of their *metascientific* theses.[12]

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[12] The conversations I had with Robert Boyd at UCLA in 2010 were very helpful to highlight some of his methodological theses. The comments of Fernando Leal and Sergio Martínez spotted errors and clarified some passages of this paper. André Bertran helped me understand some of the formal underpinnings of Richerson and Boyd's models. They are not responsible for any perduring mistakes and do not necessarily agree with the theses and interpretations here defended. I am grateful to the Brazilian Agency for Research (CNPq) for a scholarship that made this research possible.

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