Logic as the Outcome of an Evolutionary Process*

A Lógica como Resultado de um Processo Evolucionário

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Abstract: William Cooper's book, *The Evolution of Reason* (Cambridge University Press, 2001) advances the bold thesis that not just our powers of reasoning, but the logical standards by which we reason, and many of our conclusions, can be explained as the result of evolutionary pressures. Any other canons of rationality, he suggests, would be (in the long run) disadvantageous. The story that Cooper tells begins with 'life-history strategies', continues to what is usually called Bayesian decision theory, and then encompasses probability theory (here called 'inductive logic'), classical deductive logic, classical mathematics, and even some non-classical systems of deduction into the bargain.

As a critical rationalist who does not believe that there is such a discipline as inductive logic and, moreover, regards the directive to maximize expected utility as uncharacteristic of, even in conflict with, genuine human rationality, I am (to say the least) unenthusiastic about many of Cooper's startling conclusions. The aim of this paper is to identify some of the differences between us, and to determine whether either of us is right.

Key-words: Logic. Inductive logic. Critical rationalism.

Resumo: *O livro de William Cooper,* A evolução da razão *(Cambridge University Press, 2001) apresenta a tese corajosa de que não apenas nossos poderes de raciocínio, mas os padrões lógicos pelos quais raciocinamos, e muitas de nossas conclusões, podem ser explicados como resultado de pressões evolucionárias. Quaisquer outros cânones de racionalidade, sugere ele, não seriam (no longo prazo) vantajosos. A história que Cooper conta começa com "estratégias de histórias da vida", e continua aquilo que é geralmente chamado teoria de decisão bayesiana, e depois compreende a teoria da probabilidade (aqui chamada "lógica indutiva"), lógica dedutiva clássica, matemática clássica, e mesmo alguns sistemas não clássicos de dedução numa barganha.*

Como racionalista crítico que não acredita que haja uma disciplina como a lógica indutiva e, ainda mais, que considera a diretiva para maximizar a utilidade esperada como não característica de (mesmo em conflito com) a

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racionalidade humana genuína, sinto pouco entusiasmo (para dizer o mínimo) a respeito de muitas das espantosas conclusões de Cooper. O objetivo deste artigo é identificar algumas das diferenças entre nós, e determinar se algum de nós está certo.

Palavras-chave: *Lógica. Lógica indutiva. Racionalismo crítico.*

1. The evolution of reason

It is hardly open to question that the human capacity to bring reason to bear on thought is a characteristic that is open to an evolutionary explanation. This capacity depends on the more primitive, but (it seems) uniquely human, capacity to use language descriptively, as a medium for communicating statements that may be judged true or false. In his strikingly ambitious book *The Evolution of Reason*, William Cooper sets out to show that not only the ability to reason, but the standards by which we reason, can be explained as the result of evolutionary pressures: any other canons of rationality would be disadvantageous in the long run. His argument depends crucially on a perceived parallelism between Bernoulli's rule (often called Bayes's rule) to maximize expected utility and the maximization of fitness. As a critical rationalist for whom Bernoulli's rule is uncharacteristic of, and even in conflict with, genuine human rationality, I have little enthusiasm for Cooper's startling conclusions. I, for one, do not think that logic is, in the indicated sense, the outcome of an evolutionary process. The aim of this note is to investigate a little the principal differences between us. In a short paper I am not able to do justice to the wide range of ingenious ideas that Cooper marshals in advancing his thesis, but I hope that I can say enough to make it evident how and where we disagree.

There exists considerable convergence on the idea that there is some instructive similarity between evolutionary development and rational decision making. Skyrms (2000, p. 273), for example, writes: "The most striking fact about the relationship between evolutionary game theory and economic game theory is that, at the most basic level, a theory built of hyper-rational actors and a theory built of possibly non-rational actors are in fundamental agreement. This fact has been widely noticed and its importance can hardly be overestimated." To my mind, however, Skyrms's (and Cooper's) hyper-rational actors hardly qualify as rational at all, and the most evident similarity between their behaviour and the behaviour of primitive organisms is its mechanical unimaginativeness. I shall maintain, nonetheless, that there does exist a similarity between all evolutionary development and human rational action and decision making. What these activities have in common is that they are both instances of problem solving by the method of trial and error. Where they differ, as Karl Popper often remarked, is in the deliberate (rather than fortuitous) character of the human search for and elimination of error: "from the amoeba to Einstein there is just one step" (1972, p. 246; see also p. 24f., 70, 261, 265, and 347).

Copernicus replaced the stationary earth of Ptolemaic astronomy by a stationary sun, and demonstrated that it can explain the observed phenomena no less adequately. It is Cooper's view (p. 2) that nowadays

> *logic* is treated as though it were a central stillness […] as an immutable, universal, metascientific framework for the sciences as for personal knowledge. […]

> All organisms with cognitive capacity had better comply with the universal laws of logic on pain of being selected against! […] Comfortable as that mindset may be, I believe that I am not alone in suspecting that it has things backward. There is a different, more biocentric, perspective[, in which t]he principles of reasoning are neither fixed, absolute, independent, nor elemental. If anything it is the evolutionary dynamic itself that is elemental. Evolution is the law giver. The laws of logic are not independent of biology but implicit in the […] evolutionary processes that enforce them. The processes determine the laws.

The sense of this dependence of logic and rationality on evolution may become clearer from Cooper's contrast between reasoning and flying. To explain how birds, bats, and insects fly we should normally invoke two quite different kinds of theory: physical theories of aerodynamics and fluid mechanics, which explain how flying is possible, and evolutionary theories about the development of wings that explain how various species mastered the art. But in the case of reasoning, Cooper ventures, only theories of the second kind are needed (p. 5): "*There are no separable laws of logic.* […] the laws of logic emerge naturally as corollaries of the evolutionary laws. […] The laws of logic are redundant in the presence of the laws of evolution."

The bulk of Cooper's book is concerned to establish in outline a series of reductions, in the sense, say, of Nagel (1961) (a sense that will here be taken to be sufficiently well understood and uncontroversial): *mathematics* may be reduced to *deductive logic*; *deductive logic* may be reduced to *the theory of probability* (here called *inductive logic*); *the theory of probability* may be reduced to *the (Bayesian) logic of decision*; *the (Bayesian) logic of decision* may be reduced to *the theory of life-history strategies.* In sum, the whole of the content of the formal sciences may be reduced to and explained by evolutionary phenomena. It should perhaps be noted explicitly that there is one sense is which this conclusion is quite trite; for the theorems of logic are consequences of any theory whatever, and therefore of evolutionary theory in particular. Cooper's thesis is evidently stronger than this, and must be that logical manipulations, from the point of view of survival and reproduction, can be shown to have pragmatic significance.

My criticisms, mostly brief, will be presented in the reverse order in § 1-4 below, and may be summarized as follows. The reduction (1) of mathematics to deductive logic, and the reduction (2) of deductive logic to the theory of probability are not only disappointingly sketchy, as Cooper well appreciates, but seriously defective in respects that he seems not properly to appreciate. The reduction (3) of the theory of probability to Bayesian decision theory may be cautiously accepted, even if its appropriateness is much open to question. My main objection to the reduction (4) of decision theory to population biology is that it depends on a historical hypothesis that is no part of population biology. Once we get right the principal features of rational decision making (as opposed to Bayesian decision making), the gratuitousness of this historical hypothesis becomes obvious.

2. The reduction of mathematics to deductive logic

For the reduction of mathematics to deductive logic (Chapter 7), Cooper calls on the logicist programme of Frege and Whitehead & Russell (1910–1913), which is (to say the least) an involved system of higher-order logic. Despite the fact that axiomatic set theory is usually regarded as a branch of mathematics on its own account, Cooper suggests (p. 127) that it provides an alternative route, not very different from higher-order logic, in which we may make "[d]erivations of mathematics from logic". The obvious objection to this proposed reduction is that it does not work. In addition to what may be thought of as genuinely logical laws, such as those of sentential calculus, elementary predicate calculus, the theory of identity, and some rules of higher-order logic, *Principia Mathematica* is obliged to call on axioms – especially the axiom of reducibility and the axiom of infinity – that even their inventors could not easily regard as part of logic's domain. That is to say, traditional logicism completely fails to show that the "axioms and rules of inference [of *Principia Mathematica*] are purely logical in the sense of having no empirical content whatsoever" (p. 128).

Cooper's response to this quite reasonable objection is that "[i]n the evolutionary reductionist scheme of things[,] 'logical' versus 'nonlogical' is not a distinction that carries philosophical weight", since this distinction depends on the idea, rejected by reductionism, that there is "a great divide between *a priori* and *a posterior*". "[W]ithin the reductive framework [...] all of logical theory is granted to be full of empirical content anyway", he writes (ibidem), flatly contradicting the assurance given on p. 107 that "in the evolutionary development [... d]eductive logic is [...] about patterns of inference in which, if the premises are known, the conclusion can be known without [...] further factual knowledge". But never mind. However profoundly mathematics and logic may be steeped in empirical or factual content, no clue is given as to how such controversial axioms as those of reducibility and infinity can be derived from the laws of logic purportedly obtained in the previous stage of the reduction (the reduction of logic to the theory of probability). The final step of Cooper's reduction, the derivation of mathematics from deductive logic, is by any lights a step in the dark.

It may be mentioned that, in the last twenty years or so, there has been a vigorous revival of the logicist programme, culminating in the work of Hale & Wright (2001). If higher-order logic can genuinely be assimilated to logic, this offers a much better prospect of a reduction of mathematics to logic than do the resources of *Principia Mathematica*.

3. The reduction of deductive logic to the theory of probability

The principal difficulty with Cooper's reduction (chapter 5) of the theory of deduction to the theory of probability (which has nothing obviously to do with induction) is that it has all the appearance of being uninterestingly circular. To be sure, any derivation is circular in the sense that the content of the conclusion is included within the joint content of the premises (MILLER, 1994, chapter 3.3., and 2006a, chapter 3.1), but the more overtly present the conclusion is in the premises, the less exciting is the derivation. In the present case, unless I have sadly misjudged something, the theory of probability to which Cooper wishes to reduce the theory of deduction explicitly assumes the whole of sentential calculus. There is therefore no real reduction. As for predicate logic, the situation is even less satisfactory, as noted below.

As we shall record in § 3, the axiomatic theory of probability is supposed to emerge, via a celebrated theorem of Savage (1954), from a preference ordering among options. Cooper never states the axioms that are salvaged from Savage's theorem, but it is clear that they are a variant of the standard axioms of Kolmogorov (1933) in which

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sentences, or equivalence classes of sentences, stand in for subsets of a sample space. The axioms assume that the probability function p is defined on what is known as a *field of sets*: that is, to say, a family of sets that is closed under the usual set-theoretical operations of union and complementation. When probability is applied to linguistic items, it is further assumed that interderivable sentences receive the same probability. It is accordingly evident that the laws of Boolean algebra, and even the laws of sentential logic, are effectively assumed in the theory of probability as it is usually presented.

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Following Adams (1975), Cooper defines logical consequence (and hence logical derivability) as follows (p. 216):

> B is a *logical consequence* of A_1, […], A_N (where $N \geq 0$) if and only if for every $\varepsilon > 0$ there exists $\delta > 0$ such that for all p, if $p(A_1)$, […], $p(A_N) > 1 - \delta$ then $p(B) > 1 - \varepsilon$.

To show, therefore, the validity of the classical law of double negation, that each of A and $\neg\neg A$ is a logical consequence of the other, it certainly suffices to show that $p(A) = p(\neg\neg A)$ for every probability function p. But this is a simple consequence of the laws of Boolean algebra assumed as part of the Kolmogorov axioms. And in general, all that is needed, in addition to the Boolean laws, is the the monotony condition that $p(A) \leq p(B)$ if and only if $A \subseteq B$, which is derivable from the axioms. It follows from the Boolean laws, for example, and therefore from the Kolmogorov axiomatization of probability, that $A \cap (A \rightarrow B) \subseteq B$. We may therefore conclude that a probabilistic version of the rule of modus ponens is valid: $p(A \wedge (A \rightarrow B) \leq p(B)$. A little more work is required if we are to prove that B is a logical consequence of A and $A \rightarrow B$ taken together (rather than conjoined), but it can be done. Indeed, something would be seriously amiss with Cooper's definition if it could not be shown that $A \wedge B$ is always a logical consequence of A and B together. It should be noted that this criticism does not apply immediately to Adams's programme, whose aim was to develop a non-classical logic of conditionals (discussed by COOPER, chapter 6) that is based on a probabilistic criterion of validity.

My objection to Cooper's reduction is not that it is incorrect, but that all the laws of sentential logic (which can, after all, all be expressed as identities) are already explicitly assumed in the Kolmogorov axiomatization of probability. There are, however, other axiomatizations of the theory of probability, most notably that of Popper (1959), appendices *iv and *v, that make no explicit assumptions concerning the Boolean operations. A survey of many such axiomatizations, and others, is to be found in Chapter 8 of Roeper & Leblanc (1999). Whether one or other of these axiomatizations can be fitted smoothly into Cooper's reductionist project remains to be seen. It would evidently be necessary to prove an analogue of the theorem of Savage to be reported in the next section.

It needs to be emphasized that there exists no satisfactory extension to predicate logic of any of these axiomatizations. It is hardly sufficient to treat "universally and existentially quantified expressions […] as abbreviations for conjunctive and disjunctive expressions involving all the individual constants" (COOPER, p. 104), since no ordinary language suitable for mathematics can contain constants for all the elements of the intended domain. Similar attempts (such as that of FIELD, 1977) to resort in some way to substitutional semantics seem equally unpromising. The reduction of the whole of elementary logic, let alone higher-order logic, to probability theory is far from having been achieved.

Cooper's thesis, if I understand it, is that such terms as 'logically valid' and 'logical consequence' have no genuine evaluative component, and are merely ways of describing forms of inference that, in some way or other, possess some pragmatic significance for those organisms that employ them. This renders decidedly puzzling his view, quoted above, that "[d]eductive logic is […] about patterns of inference in which, if the premises are known, the conclusion can be known" (p. 107), a characterization of validity that is quite inadequate as it stands. For most logicians, valid deductive inferences are identified with inferences that transmit truth (rather than knowledge) from the premises to the conclusion. As Czech people say: "*Pravda vítĩzí*". The rule of modus ponens, for example, permits the derivation from the premises A and $A \rightarrow B$ the conclusion B. It is valid, under the usual truth-table semantics, since B is true on every row on which both A and $A \rightarrow B$ are true. Since a tautology is a statement that is true on all rows, in all circumstances, tautologousness too is transmitted from premises to conclusion (and so, thanks to the completeness theorem, is theoremhood). Modus ponens, that is to say, licenses not only the derivation from the premises A and $A \rightarrow B$ of the conclusion B, but also the inference from "$\vdash A$ and "$\vdash A \rightarrow B$ of the conclusion "$\vdash B$. But, like all the other rules of inference, it goes beyond this, and it is only by going beyond the transmission of (near)-certainty that it appears to be able to play any useful role.

Cooper goes so far as to conjecture (without any biological reference) that (p. 98):

> there is selection for epigenetic rules for Bayesian behaviour based on subjective probabilities in an inferred space of […] propositions, […] structured in such a way that for evolutionarily stable individuals in which $A \rightarrow B$ and A are both sufficiently strongly believed, B must also be believed.

To me it is unclear what the practical advantage might be of making inferences from strongly believed propositions to other propositions, which themselves become, or perhaps already are, strongly believed. Why it should be of any advantage to an organism to perform such uninteresting and unrewarding deductive inferences? Outside mathematics, premises that are highly probable are (as Popper noted years ago) almost devoid of content, whether construed objectively or subjectively, and the conclusions that may be drawn from them are equally, or even more, unexciting. This is in stark contrast to what if offered by the theory that a valid argument is one that transmits truth, or (most importantly) retransmits falsity, namely that argumentation is a method for the identification and hence the elimination of covert error (MILLER, 2006a, Chapter 4). It might be thought that a similar device could be used to reveal the advantage of the type of inference that Cooper and Adams advocate: that if the conclusion of an inference turns out not to be strongly believed, then (strong) belief in the premises must be withdrawn or withheld. This suggestion does not sit comfortably with the idea that an individual's subjective probabilities are what determine his beliefs, rather than the other way round.

4. The reduction of probability theory to decision theory

On this reduction, which is addressed in Chapter 4 and (more technically) in the appendix of Cooper's book, I intend to say little. As already remarked, it depends on a famous representation theorem of Savage (anticipated at least in part by RAMSEY, 1926), which states that each suitably restricted qualitative preference ordering ▶ over the elements of a field of options (acts) may be represented by the combination of a probability measure p on those options, and a utility function u on the possible outcomes of the acts in the sense that f ▶ g holds if and only if $\mathbf{E}(f) \geq \mathbf{E}(g)$\$, where $\mathbf{E}(y)$ is the expected utility of the option y with respect to the probability measure p and the utility function u. The measure p is unique, and the utility function (like most utility functions) is unique up to a positive linear transformation. The restrictions imposed on the qualitative ordering ▶ are supposed to be for the most part intuitively natural, though some have a more formal character, and are required for mathematical reasons. There can be no doubt that some theorem of this kind is demonstrable.

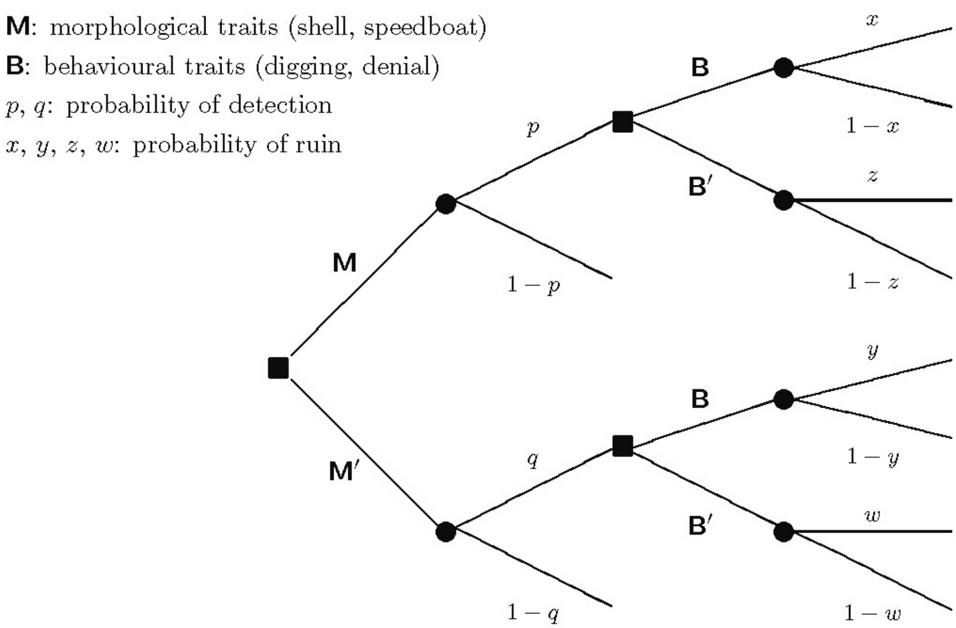
What is its import in the context of Cooper's planned reduction? It is that if the preferences of an organism or individual are sufficiently widely and precisely defined, then there exist (whether or not the organism is aware of them) a probability measure p and a utility function u such that the claim that *f is preferred to g* may be identified with the claim that the expected utility $\mathbf{E}(f)$ is greater than the expected utility $\mathbf{E}(g)$. Once preferences, or decisions between options, are given, probability too is given, provided that those preferences satisfy the required axioms. Probability assignments may be reduced to decision making strategies.

Despite its seemingly universal character, this result has decided limitations, and must be interpreted with care. Any agent may, on the evidence (if it deserves to be called that) of a suitable range of preferences over all possible options, be understood to be implicitly adopting a Bernoullian strategy of maximizing expected utility. Yet he may be working consciously to a quite different strategy, for example the strategy, which seems to me to be much more intelligent, of *attempting to maximize actual utility* (see § 5 below). Strategies are, at least for human agents, intentional entities, and should be understood accordingly. It is part of Cooper's thesis, of course, that the good sense of Bernoullian-Bayesian decision making emerges from more primitive biological considerations. To this final step in the reduction we now turn.

5. The reduction of decision theory to population biology

The final, most fundamental, and most important, part of Cooper's reduction of the formal sciences to evolutionary biology, occupying Chapters 2 and 3 of his book, consists in his attempt to draw a significant conclusion from the parallel, seen by others and mentioned above in the quotation from Skyrms (2000), between some elementary evolutionary processes and some equally elementary processes of decision making under uncertainty. Like Cooper's own discussion, however, the discussion here will steer clear of the game theoretical complications alluded to by Skyrms, and content itself with what happens at the most elementary level of organisms acting in a non-interactive environment.

Figure 0, which is a combination, with modifications, of Cooper's Figures 2.3 and 3.1, is designed to illustrate simultaneously a snapshot of the evolutionary development of a population of organisms and the decision tree of an agent caught in a situation of risk or uncertainty. In each case circular nodes indicate the operation of chance (or other probabilistic process), whereas the square nodes indicate inbuilt variations. The twigs at the top (that is, right-hand end) of the tree that are marked x, y, z, w, lead to ruin. The remaining twigs, including those marked p and q situated part of the way up the trunk, lead to success.



In its evolutionary guise the tree represents the career of one reproductive season of a polymorphic population of semelparously and asexually reproducing organisms. Among shelled M members a proportion p are detected by predators, while among exposed M' members the proportion is q. The two polymorphs have a repertoire of two behavioural responses to the predatory threat, digging B or running away B'. The proportion of shelled diggers MB who suffer capture and extinction (ruin) is x, and the corresponding the proportion of shelled runaways MB' is z. The proportions of exposed members M'B and M'B' who are extinguished are y, w respectively.

In its prohairetic (decision-theoretic) guise the tree represents the options open to a smuggler who is considering buying a speedboat M to replace his fishing dinghy. The probability is p that a speedboat will raise suspicion in the coastguards, and q that a dinghy M' will do so. There are only two things to do if detected: to play innocent and bluff one's way out of trouble B, and to escape B'. There is a probability x that a speedboat whose owner plays innocent MB will suffer capture and confiscation (ruin), and a probability z that the speedboat MB' cannot outdistance the coastguards. For a fishing dinghy M'B or M'B' the probabilities of capture and confiscation are y and w respectively.

In the evolutionary picture, it is straightforward to calculate which combination of morphology and behaviour exhibits the greatest proportional population increase in the next generation. After many generations under the same conditions this combination (say, MB) can be expected to swamp the population, and other combinations will be selected against. It is commonly held that the combination of nautical craft and diplomatic craft that the smuggler should opt for is calculated in the same way, since it is that combination that yields the greatest *expected utility* **E**: that is, the greatest average utility ascribed to each outcome of each combination, weighted by the probabilities of those outcomes.

There is one asymmetry, easily taken care of, in the two readings of Figure 0. In the nautical example, what corresponds to ruin is the capture and confiscation of the boat, whose disutility may vary with circumstances in a way that biological extinction does not; for the confiscation of an expensive speedboat may be judged even more disadvantageous than the confiscation of a dinghy. But this slight asymmetry can be taken care of by treating not mere survival but fitness (COOPER, 2001, p. 37–40) as what corresponds in the biological reading to utility in the decision-theoretic reading. Given this proviso, "*a classical decision tree is interpretable as a branch of a life-history tree*" (COOPER, 2001, p. 48).

In reality a life-history tree will be not only extended into another generation (for those individuals that survive the season depicted), but also much bushier. The natural environment is full of incidents that may necessitate some reaction. There may be a very large, even continuous, set of possible reactions to each threat. The reactions too may not be automatically induced, but conditional on further environmental variables: for example, there may be conditional behavioural responses such as *run away on rock, but dig on sand* and *run away at night, play innocent during the day*. The seemingly endless variety of possible *strategies* (as they are usually called) means that the relative frequency of survivors of even the best strategies may be very small. In addition, the environment is always changing to some extent, and a permanent background cannot be countenanced. It seems impossible that natural selection could ever work in such conditions to eliminate unfit strategies.

Cooper suggests that, to understand what has happened between the evolutionary beginnings and the human present, we must first replace the idea of the *fitness of* [the members of a population who follow] *a strategy* with the idea of *expected fitness*; not the actual number (and quality) of offspring, but their expected number. This is defined as an expectation in the usual manner, and is called a *propensity*, even when the relevant probability measure is interpreted as a frequency. The main point, however it is realized, is that we have "a way of talking about the fitness of a single individual" (COOPER, 2001, p. 52).

For the reasons already adumbrated, piecemeal strategies, however unfit, will not be selected out of the population. According to Cooper, as the life-history trees became more and more involved, the time became ripe for the emergence of *meta-strategies* that attempt, at each point of the tree, to maximize the expected fitness of the individual. Cooper asks (p. 57):

> What will happen next? There will be selective pressure in the direction of a more sophisticated information processing capacity that enables each individual to construct a cognitive life-history tree branch appropriate to whatever decision situation it currently finds itself in.

It is not easy, at least for me, to see such meta-strategies (which incorporate judgements of probability and of utility) as components of a genuine reduction of decision theory to population biology. But the more important question is whether such strategies have indeed evolved. Cooper thinks that they must have evolved, since this is how we make decisions (ibidem):

> What is involved in such an adaptation is so impressive that it would hardly seem a serious evolutionary possibility if it hadn't already occurred in some species, notably humans. [… T]he individual [… in] some sense has to *analyze* the decision situation as a whole. It must identify the available acts and the events that might ensue from each act […]. It must make assumptions about the probabilities of the various events, […] and attach fitness estimates to them.

The view presented is that in this development subjective probabilities (which are crucial components of standard subjectivistic or personalistic Bayesian decision theory) have evolved as estimates of the objective probabilities that exist unknown behind the scenes. An evident problem is that in an environment in which genuine decisions are taken, there exist few stable objective probabilities, if any; certainly not long-run frequencies, and hardly any stable propensities either, since propensities generally depend sensitively on everything that occurs (including the outcomes of decisions). But my principal objection to the story that Cooper here tells is that it seems to be sheer make-believe. Not all incredible ideas are wrong, but this one – that what has evolved is a meta-strategy of decision making based on subjective probabilities – seems to me to be profoundly mistaken. It is indeed nothing but wishful thinking. Because Cooper, like so may others, takes it for granted that rational decision making in humans proceeds by Bernoulli's rule of maximizing expected utility, he is led to postulate a development that 'hardly seem a serious evolutionary possibility'. This may not be the weakest point in his reduction of the formal sciences to evolutionary biology, but it is a breaking point. In truth no evolutionary explanation has been given for the involvement of subjective probabilities in decision making. And the plain reason for this is that subjective probabilities are not an important component in either animal or human (rational) decision making.

6. Rational decision making

I cannot do more here than to summarize a theory of rational decision making (rather than a theory of the making of rational decisions) that seems not only to be much more in accord with the way that most intelligent decisions are made, but also to be open to an uncontroversial evolutionary explanation. The simple idea (suggested embryonically at the close of § 3) is contained not in the mechanical rule of maximizing expected utility, but in the speculative and fallible rule of maximizing actual utility. To this end, the wise decision maker will try to discover which of the courses of action open to him will yield the maximum utility, and follow that course of action. Of course, under conditions of uncertainty or risk he cannot know that he has chosen well. If it he discovers later, by chance or through active scrutiny and review, that he has acted inappropriately then he will correct his decision, and guess again at what is the best course of action to follow; and will continue to correct later decisions, as far as he is able, until, with luck, he attains his objective. As noted at the end of in § 2 above, this is why logic is of such fundamental importance. In short, decision making is, like all activity that is not purely mechanical, a process of trial and error. In this respect it resembles much biological behaviour, and all behaviour that is at the root of evolutionary change (MILLER, 2006b, § 3).

Rational decision making as it is here understood is an evolutionary development out of pre-rational thinking, but here the necessary steps in the development do not need to be hypothesized ad hoc. They are there for everyone to see: first, the emergence of a descriptive language in which thoughts and plans may be objectified, and second, the emergence of the critical (or rational) attitude, which uses logic as a tool for identifying serious mistakes and of bypassing them without disaster. For this reason, I am inclined to say, contrary to Popper, that there are two steps from the amoeba to Einstein, not just one. But two steps or one, they provide a fertile environment for decision making undertaken in a genuinely rational (that is, critical and argumentative) manner. It is a mockery of human rationality to suppose that it needs to remain at the level of decisions made in accordance with fixed, but objectively suspect, rules.

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