Uma breve história da teoria evolutiva

A short history of evolutionary theory

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SILVA, E. P. DA: 'A short history of evolutionary theory'.
História, Ciências, Saúde — Manguinhos, vol. VIII(3): 671-87, Sept.-Dec. 2001.

The history of the Theory of Evolution has been told a number of times by historians, philosophers, professors, writers, scientists and so on. However, many of these versions differ from or even contradict one another. In this article, the history of the Theory of Evolution is retold according to a dialectical-materialistic perspective. It analyzes the historical contradictions between Darwinian evolution theory and Mendel's model, the background that led to the synthetic theory of evolution, the debate carried out by classic schools and the result of synthesis, as well as the still current debate between Neutralism and Selectionism. Finally, it also discusses the interpretative model used ("an idiosyncratic dialectic materialism"), mainly in relation with Popper's and Kuhn's models.

KEYWORDS: Theory of Evolution, Heredity, Dialectic Materialism, Neutralism, Selectionism

SILVA, E. P. DA.: 'Uma breve história da teoria evolutiva'.
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A história da teoria evolutiva tem sito contada inúmeras vezes por historiadores, filósofos, professores, escritores, cientistas etc. Contudo, muitas destas versões diferem entre si ou mesmo se contradizem. Neste trabalho, a história da teoria evolutiva é recontada a partir de uma perspectiva materialista dialética. São analisadas as contradições históricas entre a teoria evolutiva darwiniana e o modelo mendeliano, o caminho para a teoria sintética da evolução, o debate entre as escolas clássica e do balanco que sucedeu a síntese, bem como o debate, ainda atual, entre neutralismo e selecionismo. Ao final, o modelo interpretativo utilizado ("um materialismo dialético idiossincrático") é discutido, principalmente, em relação aos modelos popperiano e kuhniano.

PALAVRAS-CHAVE: teoria evolutiva, herança genética, materialismo dialético, neutralismo, selecionismo.

Introduction

The history of evolutionary theory has been told and retold. However, no general agreement can be found among the different accounts of the facts. Disagreements are abundant about the relative importance of different fields (e.g. population genetics, experimental genetics, natural history, developmental biology) for the synthetic theory, as well as about the correct epistemological framework in which the history should be interpreted (e.g. falsificationism, relativism, dialectical materialism). Therefore, any attempt to tell again such history will need to make choices at all steps. The brief overview on evolutionary theory given here will try to make these choices as explicit as possible. The final aim is not that of reaching a specific solution, which seems a lost cause, but of reading through the published material to analyse the way in which science operates.

Evolution and Inheritance

There are many different views about what is the nature of the Darwinian revolution. Some of them center on the importance of natural selection as a mechanism for evolution (Dennett, 1995) or on the huge amount of data which was offered as scientific proof for Darwin's arguments (Mayr, 1991). However, Darwin's mechanism of natural selection was flawed without an adequate theory of inheritance, what by Popperian standards should have refuted Darwin in the beginning (Popper, 1972). Also, most of the time Darwin explains this mechanism in the "Origin", he uses the Lamarckian inheritance of acquired characters (Darwin, 1859). However, a huge amount of data, albeit secondhand, supporting the evolutionary view had already being gathered by his contemporary Chambers in his book *Vestiges of the natural history of creation* (Chambers, 1845).

Darwin's work brought two great insights to the confused evolutionary field of his time. First, the understanding that the variation among individuals within species was not an imperfection or noise which could be ignored, but that variation represents the reality in the organic world. The second insight was to realise that a mechanism which could transform variation within groups into variation between groups, would explain the origin of species (Lewontin, 1974). The envisaged mechanism was the differential survivorship of variants within populations. In other words, Darwin's revolution consists of an antiplatonic view of nature and a materialistic interpretation of the natural process of speciation (Gould, 1977; Levins e*t al.*, 1985). Thus, the revolution was not to solve the problem, which in fact Darwin was not able to do (Coyne, 1994), but to show where (variation) and how (mechanism) to look for the solutions.

The second great pillar of the modern evolutionary theory is the work of Mendel. His theory was a simple model for understanding heritability as a phenomenon of discrete genes moving across generations, in a series of independent segregations (Mendel, 1866; Fisher, 1966). The 'factors' described by Mendel as a way to explain inheritance, had no material existence outside the theory, and in this sense they were a beautiful example of the "applied rationalism" (Bachelard, 1983) or of the building up of a "concrete concept" (Marx, 1858). They were the starting point, the reasoning behind, the whole inheritance process. However, because Mendel's data were too close to expectation statistically, they have been an enigma to geneticists. Many explanations have been advanced to deal with the case. Wright's (1966) explanation was that Mendel unconsciously classified ambiguous cases in the direction of reaching a favourable ratio. A second explanation is that Mendel omitted crosses that were aberrant (Dobzhansky, 1967). However, the most ingenious alternative, offered by Fisher (1966), is that Mendel had an assistant who modified his counts to give ratios more agreeable to his master's theories. The real reasons behind such an exceptional agreement between expected and observable in Mendel's data will probably never be known.

Although the evolutionary theory as it is understood nowadays is based on the work of Darwin and Mendel, their ideas were in different fields. While Darwin's work was aiming to understand the laws of mutability, Mendel's work was concerned with an explanation for the stability of a world created by God. The contradictions between Darwinism and Mendelism were present even during the historic time of the foundation of classical genetics.

Classical genetics had its major components identified and experimentally verified by the Drosophila Group composed of Thomas Morgan and his students, during the time between 1910 and 1915 (Carlson, 1975). Inside this group, a polarisation existed between Morgan, Sturtevant and Bridges on one side and Muller and Altenburg on the other. Morgan had no commitment to Darwinism. His search for mutations was actually based on a rejection of Darwinism and a fascination with de Vries' mutation theory. In the same way, Lamarckian interpretations could be used to interpret the variable phenotypic expression of dominant mutations, such as Truncate and Beaded, which defied Mendelian ratios (Carlson, 1975).

On the other hand, Muller was the figure who always tried to tie together the emerging science of genetics with Darwinism and also search for explanations for the variable phenotypic expression of Truncate and Beaded mutations as interactive effects of genes. For such commitment, Muller was criticised as a zealot (Carlson, 1975). Muller has a controversial role in the history of the foundation of classical genetics, not for absence of documentation, but because his explicit and sometimes rough positions brought him much trouble within the scientific establishment (Carlson, 1972). The synthesis of Darwinism and Mendelism was only possible in the thirties with the theoretical work of Fisher, Wright and Haldane. Their work showed that the evolutionary process can be understood as changes in gene frequencies within populations.

The Synthesis

The pathway from the works of Mendel and Darwin to the Evolutionary Synthesis still remains controversial. Mayr, among others, is recognised as the one who advocated that the evolutionary synthesis was not the result of the work of the 'bean bag' population geneticists, but it was a synthesis of natural history (systematics, palaeontology, experimental genetics) and evolutionism in the tradition of Charles Darwin (Mayr, 1993; Gould, 1994). Although the work of figures like Mayr, Dobzhansky, Simpson and Stebbins had a great impact in extending the synthesis as an unifying theory in biology (Simpson, 1953; Dobzhansky, 1970; Mayr, 1977), the core of the synthesis is still the understanding that Mendelism is not the theory of stability but it can be understood also as a science of movement and mutability. No doubts exist that population genetics is the discipline which best established this revolution, opening the way forward for deeper understanding of evolution (Lewontin, 1974; Crow, 1987).

The works of the 'three giants', Haldane, Fisher and Wright, so called by Crow (1987), were at the same time unified and different. Fisher (1930) and Wright (1931) built up distinctive systems. Fisher's system was centred on a general theory of natural selection of small mutations in large populations. On the other hand, Wright's system put strength on population structure and the way that random changes could operate in small populations helping the species to climb adaptative peaks (Maynard Smith, 1989; Leigh, 1990). Fisher's system is mathematically very robust and easily testable. However, it is extremely reductionist and ignores the complex way that genes interact. Wright's system on the other hand, pays attention to the net relationships of genes, especially in the form of epistasis, and to genetic drift and its power to cause rapid changes in the genetic composition of populations. However it is still a very much a verbal qualitative system, lacking Fisher's mathematical elegance and quantitative vigour, although very appealing.

Haldane refused to build up his own system but chose to work with four specific questions (Haldane, 1932; Leigh, 1990). First, are the differences between species of the same nature as differences between populations (the core of the Darwinian revolution)? Second, how does natural selection operate (the trial to establish the mechanism)? Third, how can apparent non-adaptative evolution exist (dealing with the contradictions)? Finally, how are the conflicting interests of gametes, individuals and populations reconciled in the evolutionary process (the dialectics of the whole and its parts)? Haldane's scientific agenda refused to be encapsulated within a system. He was probably aware that scientific systems open room and refuge for many extra-scientific ideas and ideals (Kuhn, 1970; Althusser, 1979; Bachelard, 1983).

After the establishment of the Evolutionary Synthesis, a great controversial discussion appeared on the population genetic scene: What was the amount of variation present within wild populations? Two different answers defining two schools of thought were the classical and the balance view of the problem.

The Debate between Classical and Balance Schools

The classical view held that the level of genetic variation in wild populations was low, because normalising selection kept wild type genes fixed. Variation came from mutation and was transient, going for fixation if it was good or for extinction if it was bad. On the other hand, the balance view held that natural populations had high levels of genetic variation maintained by the action of balancing selection, which could be diversifying, frequency-dependent or heterotic (overdominance or heterosis) (Dobzhansky, 1970; Lewontin, 1974; Mayr, 1977).

Besides the obvious scientific interest of the debate, the two schools had strong social connotations. The classical view would admit the existence of the wild (or normal) type, which was to some extent a resurrection of the Platonic ideal type, the reference, from which judgements of value could be made. The eugenics movement borrowed part of its justification from such a view (Allen, 1975; Paul *et al.*, 1995). For the balance school, no wild type was required, since variation was the reality of the biological world, and it had a much more tolerant social translation, in for example the acceptance of a pluralist society. At a first sight the balance school seems aligned with traditional Darwinism, however, Darwin's arguments about the adaptation process were much more in line with the classical view, with emphasis on what would later be called normalising selection (Darwin, 1859). Another impression that could be born in mind is that the proponents of the balance school would never have any relationship with the eugenic movement. But that is also not true. For instance Dobzhansky, one of the supporters of the balance school, was not without eugenical sympathies (Dunn *et al.*, 1952).

During the sixties, the controversy between the balance and classical view was resolved by the work of Lewontin and Hubby, that addressed the application of allozyme electrophoresis technique to the problem (Hubby *et al.*, 1966; Lewontin *et al.*, 1966). It became clear at that time that wild populations had high levels of genetic variation. However, following the resolution of the controversy between balance and classical views, another controversy began: What were the evolutionary forces maintaining the high levels of variation in wild populations? On one side we have the selectionists arguing that natural selection is maintaining the genetic variation. On the other side, the neutralists support the view that high levels of genetic variation in wild populations can be maintained by a balance between population size and mutation rate (Lewontin, 1974).

The Debate between Neutralism and Selectionism

After the Lewontin and Hubby (1966) work, a torrent of similar data followed, supporting the neutralist or the selectionist views. Two kinds of strategies were employed. First, following a blueprint set out by Clarke (1975), many studies tried to relate different allelemorphs with environmental factors, especially temperature and salinity. An example of these attempts are offered by the "Adh" polymorphisms in *Drosophila* (Day *et alii*., 1974a, b), "Lap" in mussels (Koehn, 1978, 1985; Hilbish *et al*., 1985a, b), "Ldh" in fishes (Mitton *et al*., 1975; Powers *et al*., 1978; Place *et al*., 1984; Beneden *et al*., 1989) and "Gpi" in copepods (Burton *et al*., 1983; Koehn, 1985). A second strategy was to study large databases on several taxonomic groups and compare the results with the predictions of the neutral theory (Fuerst *et alii*., 1977; Chakraborty *et alii*., 1978, 1980; Nevo, 1978, 1983; Nevo *et al*., 1988; Skibinski *et alii*., 1993; Woodwark *et alii*., 1993). Although the results of both strategies have been contributing to expand the knowledge of patterns of allozyme variation in natural populations, they were not able to solve the controversy.

More recently, Lewontin (Lewontin, 1991; Barbadilla *et alii*., 1996) has been in some way discrediting the power of allozymes to answer questions about patterns of variation in the biological world (Watt, 1994). Such a stance by Lewontin can be interpreted as contradictory or disillusioned, since his classical work (Lewontin *et al*., 1996) was the starting point of this scientific agenda. Lewontin and Hubby's study was an enlightened example of how to bring a dimension of scientific method to an otherwise simple protein separation technique (electrophoresis). A procedure able in some circumstances to create a whole new science, as in the case of the Freudian psychoanalysis with the free association and the construction of the unconscious theory (Althusser, 1985). Therefore, Lewontin's motives should probably be searched for anywhere else other than in scientific contradiction or personal disillusionment.

The controversy between selectionism and neutralism remains a burning issue in the DNA age (Brookfield *et al*., 1994; Kreitman, 1996; Ohta, 1996). The strictly neutral model was replaced to some extent by the nearly neutral model (Ohta, 1992; Gillespie, 1995; Ohta *et al*., 1996). Far from being trivial, such a shift at the same time both empowered and weakened the neutral theory. The main criticism for the selectionists models is that they are very difficult to test, because different scenarios can be created which will explain any pattern of molecular variation.

However, assuming the possibility of having slightly deleterious and advantageous mutations (Ohta, 1996), the neutralist model is refusing refutation and opening the windows for the construction of scenarios which can also explain any pattern of molecular variation as can the selectionists models (Ohta *et al.*, 1996).

The neutral theory first appeared explicitly in 1968 (Kimura, 1968). At first it was mainly a claim about patterns of molecular variation (allozymes and DNA polymorphism) (Kimura, 1982, 1986), and had little to do with for example phenotypic variation. However, further developments of the theory seem to have enhanced its proponents ambitions in relation to the whole of population and evolutionary biology, ranging from the origin of life to sociobiology (op. cit.). The theory is now referred to as a paradigm, and although it is not stated whether in a Kuhnian sense or not, it is obvious that it is at least as inclusive as such, since it is empowered in its claims and impoverished in its mathematical scope (op. cit.).

This short overview, centering on the historical aspects of the development of the evolutionary theory, was an attempt to illustrate points and underline events which are considered important for the opinions expressed in the sections which follow.

Epistemological Framework

The way in which history is told, has very much to do with the way in which history is interpreted (Chalmers, 1982; Losee, 1993). Much of the history telling about evolutionary theory follows a general trend of interpretation through, for example, falsificationism (Popper, 1969) or the relativism of paradigms shifts (Kuhn, 1970). An example of the first can be found in Mayr (1991) and of the latter in Olby (1975). But the influence of these philosophies goes much further, being present for instance, in reviews of the field by the scientists themselves (Crow, 1985). A dialectical approach has also been tried (Levins *et al.*, 1985), though this is much less influential. The overview employed here adopts the dialectical and the dialogue with the paradigm shift approaches, but rejects the falsificationist approach.

The rejection of the falsicationist approach is centred on five reasons, most of them already discussed throughout the work of Kuhn (1970), Lakatos *et al.* (1974) and Feyerabend (1975), and some of them extracted by the acceptance of alternative views (Marx, 1858; Althusser, 1979, 1985; Bachelard, 1983, 1984). First, falsicationism is a prescriptive philosophy, trying to force a norm of practice on scientific activity. Second, it fails as a norm of practice, since most of the scientific theories are not refuted but reformed; what is in fact very reasonable, otherwise scientific activity would be left without any working theory most of the time. This is because refutation procedures do not grant any alternative theory, which are usually produced with long gestation times. Third, falsicationism does not offer a realistic account of the history of science itself, since scientific practice throughout history is very conservative, trying to save established theories by means of several *ad hoc* explanations. Fourth, it is built up only as an internalist interpretation of the scientific activity, ignoring that science, as a human activity, is open to all sorts of influences from human life, from history to passions. Finally, the argument that falsicationism represents the ideal, the way in which science should be done and that as such should be pursued in day to day scientific activity is flawed, since an epistemology which fails to understand the structure of scientific production is unable to reform it.

The relativism of the paradigm shift is a very seductive framework to work with. It depicts science as an open activity, strongly influenced by social factors, explains very well the establishment of some of the more important systems of thought, like Darwinism for example, as well as some important characteristics of the development and shift of such systems (accumulations of inconsistencies, crises, competition of new alternative systems and shifts) (Kuhn, 1970). However, Kuhn's epistemology presents some problems. First, the definition of paradigm is something very uncertain and confused. Should it be defined widely, so that Darwinism is the paradigm in which the research program on evolution has been developing or should it be defined more narrowly, so that we can have the selectionist and neutralist paradigms competing within evolutionary theory? Another confusing aspect of the paradigm framework is the demarcation criterion. If different paradigms have the property of incommensurability, and the shift is done on a basis other than through objectivity, how could it be decided if science does advance in some way? The characterisation of normal science is also something problematical. Although, it is true that most of the science done day by day does not have a revolutionary character, it is also true that this normal science expands the boundaries of knowledge and the shifts are helped in some way by this expansion (Lakatos *et al*., 1974; Feyerabend, 1975; Chalmers, 1982).

Different from the falsicationist and the paradigm shift frameworks, the dialectical method as an epistemology is less clear. Some clues can be found in Marx's *Grundrisse* (1858), Engels' *Dialectics of nature* (1954) and in the works of Althusser (1979, 1985). The work of Levins and Lewontin (1974, 1985) present a clear cut Marxist interpretation of some aspects of their specific fields but, as stated by the authors, they are much more advocating the dialectical character of nature, following the Engels tradition, than an epistemology. Beside these, the works of Gaston Bachelard (1983, 1984) also represents what is very much a non-Marxist dialectical epistemology. What follows is an attempt to make explicit the dialectical epistemological framework which was used for this overview on evolutionary theory and how it dialogues with the epistemology of Thomas Kuhn.

An Idiosyncratic Materialism Dialectics

From a dialectical point of view, there are two most important aspects of science. The first aspect has an ontological character, which is the material relationships established in the production of knowledge between elements, such as the present day advance of techniques, the established scientific tradition and the specific interests and questions present in society. The second aspect has a epistemological character, which is the nature of the relationship between knowledge and reality, in the specific case of biology, between nature and the knowledge of nature.

The first aspect is the key to understanding the nature of the debates inside the paradigms (Mendelism x Darwinism, balance x classical, selectionism x neutralism) and the importance of normal science beyond the simple role of problem solving. As seen before, scientific theories start with specific problems, how is flower colour inherited? What is the amount of variation present in the biological world? Is selection or genetic drift responsible for the patterns of variation observed at the molecular level? and so on. However, the fact that science activity is done in the real world by real people, has the result that scientific questions mirror many of societies own questions. This is the reason why the contradiction between Mendelism and Darwinism can be viewed as a contradiction between a world which wanted to change and a world which wanted stasis. The contradiction between the balance view and the classical view was also a contradiction between a society which accepts pluralism and that adopting xenophobia. The contradiction between selectionism and neutralism might be viewed as a contradiction between the once absolute acceptance of the progress of science (derived from the optimism after the industrial revolution) and of society (with the welfare state or the socialism ideal) and the more bitter reality of the pollution problems, the threat of extinction by atomic power, the reality of Stalinism, which made society uneasy. A well documented recent example, although not interpreted from a dialectical point of view, of this link between scientific and social questions is the work of Nelkin and Lindee (1995) about DNA, showing that beside the scientific agenda, a whole social agenda is linked to scientific research on DNA.

The Kuhn epistemology assumes that the only creativity in science occurs with the revolution of paradigm shifts, otherwise the flat and boring normal science dominates. However, normal science also has its revolutionary character. Science sometimes advances by technological innovations which make possible the answer to old problems (Solé-Cava, 1986). At other times, what is necessary is a different theoretical approach. In both cases, advances are made, expanding the boundaries of knowledge, although still inside the same paradigm but causing small scale revolutions. An example of the former case is the beautiful use of the electrophoresis of proteins by Lewontin and Hubby (1966) to solve the controversy between the balance and the classical school. An example of the latter is the Oparin (1956) approach to the problem of the origin of life. What is being advocated is that, in contrast to Kuhn's description, shifts are occurring all the time in scientific activity and that the paradigm shift is a qualitative change after small scale quantitative changes inside the paradigm, to use Engels' (1954) terminology in *Dialectics of nature*.

Finally, the epistemological aspect of dialectics is important to understand why and how a paradigm shift does represent an advance in science and despite a certain level of incommensurability it is still possible to see this advance. Popper for example assumes that science is progressing towards the truth, the content of truth of scientific theories are always growing, and although the truth probably will never be reached, scientific progress is linear. Kuhn on the other hand, does not deny that science progress, but he is unable to say how, since the incommensurability of the paradigms prevent any kind of comparison. Here it is advocated that despite the fact that paradigm shifts do produce change in the way the world is viewed and in the language in which it is told, this is a reflection of two aspects of the nature of knowledge. First, scientific knowledge is not representative, although it does carry some information about the world, it is not the world itself. This is what was called a "concrete concept" by Marx (1858), an "unrepresentative realism" by Chalmers (1982), the "scientific reality" by Bachelard (1984) and "objects of thought" by Althusser (1985). Thus, paradigm shifts always create a qualitative jump, which is the cause of loosing of the power of comparison but this jump must be understood as the result of the accumulation of quantitative changes inside the old paradigm. The second aspect is the nature of the relation between this scientific reality and the reality itself, which is a dialectic nature. In other words, scientific knowledge does not have a linear growth or vector which approximates it to the truth as in Popper's conception, but has an interaction with reality. The result of this kind of relation is that besides the fact that reality creates knowledge, knowledge also creates reality. An example of this is the concept of the gene, which cannot be defined simply as a stretch of DNA because part of its definition is dependent on the character which the gene represents, and the path from a stretch of DNA to one character is far from trivial, depending on interactions with other genes, environment and splicing to mention just the most obvious complications.

Brothers in arms but in different camps

After much debate in the sixties about the positions defended by Karl Popper and Thomas Kuhn, there followed a period of expositions and comparisons. Among the new standpoints, Imre Lakatos' and Paul Feyerabend's are some of the most influential.

Lakatos developed the idea of *Scientific research programmes* as an alternative to the failure of the Popperian *Conjectures and* *refutations* normative epistemology. His ideas are very much in line with Popper, however, he acknowledges with Kuhn that refutation is not invariably followed by rejection, and that theories should be allowed to flourish even when "anomalies" are present. Lakatos' alternative is to substitute the theories by research programmes as basic units for appraisal. These research programmes would be able to tell which paths of research to avoid (negative heuristics) and which ones to pursue (positive heuristics). Negative heuristics isolates a "hard core" of propositions which are not exposed to falsification and, therefore, are accepted by convention (Lakatos, 1974). The main aim of such ideas is to maintain a rational reconstruction of theory replacement, improving the Popperian view of doing science.

Although a very interesting alternative, Lakatos' conception is still a very much reform of the Popperian internalist view of science, and as such is liable to the same objections. For example, the only contradictions which are taken into account by Lakatos are those which are reputed as logical and formal disputes. Social and political pressures do exist but make science fail to conform to the standards of scientific rationality. Moreover, while the relative merits of competing hypotheses within a research programme can be determined by rational standards, the comparison of rival research programmes can be decided only by the extent to which they are progressing or degenerating. However, it is not forbidden for a scientist to choose to pursue a degenerating research programme, even if his decision is based only on social and political preferences, which are not a matter of interest in Lakatos' epistemology. Because of Lakato's failure to tackle problems such as those described above, Feyerabend said of his methodology that it is "a verbal ornament, as a memorial to happier times when it was still thought possible to run a complex and often catastrophic business like science by following a few simple rational rules" (Feyerabend, 1974).

Feyerabend is very concerned with a "return to the sources", meaning that epistemology should abandon ideal logical reconstruction and immerse itself in the history of science. His position is very radical and was called by himself an "anarchistic theory of knowledge". Feyerabend argues that methodologies of science have failed to provide adequate rules for guiding the activities of scientists. Furthermore, he suggests that, given the complexity of history, it is implausible to expect that scientific activity be understood on the basis of a few simple normative epistemological rules. Against any method, Feyerabend concludes that the assumption that there is a universal scientific method to which all scientific activities should conform is false and, therefore, (almost) anything goes. And should go, because "science both is, and should be, more irrational than Lakatos and Feyerabend (the Popperian author of the preceding sections of this paper and of 'Problems of empiricism') are prepared to admit" (Feyerabend, 1974).

All Feyerabend's attacks on orthodoxy are very beneficial and important for building up a new philosophy of science, less normative and more opened to the fact that science is a real activity, carried out by real men in a real world. However, much of the thesis present in Feyerabend's anarchistic theory of knowledge is negative: incommensurabilty, irrationalism, against method. His positions and criticism go so far as to say that there is no future for philosophy of science. Although Feyerabend's positions represent an intransigent defence of freedom, democracy and individuality, his objectives cannot be fully achieved by such an iconoclastic stance. Arguing that everyone should follow their individual inclinations and do their own thing can obscure the fact that there are real constraints operating in society and science should try to tackle them in some way. Therefore, it could be that Feyerabend's "anything goes... means in practice, everything stays" (Krige, 1980).

The idiosyncratic materialism dialectics developed here tried to interpret history of science as the material relationships established between techniques, scientific tradition and the specific interests present in society. In this manner, such interpretation did not separate internal from external history of science, achieving a better understanding of science as a human activity. Furthermore, the dichotomy rationalism versus irrationalism, based mainly on the problem of incommensurability, could be supplanted by the dialectical view of jumping from quantity to quality. The problems of the logical reconstruction and advancement of science could also be better understood under the materialistic dialectic interpretation of history, which reveals that the criteria for judging progress can only be historically reconstructed. Once ontology and epistemology are joined in the praxis of this idiosyncratic materialistic dialectics, this praxis could probably also constitute a better methodology to understand and transform scientific activity.

Although not developing his work in this way, Feyerabend was very sympathetic to dialectical materialism. He was to say in his article of 1974 that "little of this (materialism dialectical) is known to the 'analytic' or 'empiricist' philosophers of today who are still very much under the influence of the Vienna Circle". Hopefully the ideas developed here will help to tear down this curtain.

Conclusion: Muller, Haldane and Lewontin

At this point it is important a short last consideration of the work of Muller, Haldane and Lewontin. These three scientists have roles in the history of the evolutionary theory which were singularly important. Muller with his commitment to Darwinism and Mendelism and his perception of the importance of gene interaction (Carlson, 1972, 1975), was non-compromising with a more loose position compared to the other members of the Drosophila group. Haldane has his refusal to build up a system and concentration on specific questions (Leigh, 1990), when system building was the trend. Lewontin had an apparently iconoclastic position in relation to the allozyme scientific agenda initiated by himself (Watt, 1994). Beside these singular roles played in the history of evolutionary science, another singularity of these three scientists is the commitment to the dialectical method. Thus, their stances in science probably represent a conscious search for the leaping point, the revolution, the qualitative jump, hidden in the quantitative accumulation which characterises normal science.

REFERENCES

Allen, Garland E. 1975 — Genetics, eugenics and class struggle. *Genetics,* 79, pp. 29-45.

Althusser, Louis 1985 — *Freud e Lacan, Marx e Freud.* Edições Graal, Rio de Janeiro.

Althusser, Louis 1979 — *Filosofia e filosofia espontânea dos cientistas.* Editoral Presença, Lisboa.

Bachelard, Gaston 1984 — *A filosofia do não.* Editorial Presença, Lisboa.

Bachelard, Gaston 1983 — *Epistemologia.* Zahar Editores, Rio de Janeiro.

Barbadilla, Antonio; King, Lynn M. & Lewontin, Richard C. 1996 — What does electrophoretic variation tell us about protein variation? *Molecular Biology and Evolution,* 13:2, pp. 427-32.

Beneden, Rebecca J. V. & Powers, Dennis A. 1989 — Structural and functional differentiation of two clinally distributed Glucosephosphate Isomerase allelic isozymes from the Teleost *Fundulus heteroclitus. Molecular Biology and Evolution,* 6:2, pp. 155-70.

Brookfield, John F. Y. & Sharp, Paul M. 1994 — Neutralism and selectionism face up to DNA data. *Trends in Genetics,* 10:4, pp. 109-111.

Burton, Ronald S. & Feldman, Marcus W. 1983 — Physiological effects of an allozyme polymorphism: Glutamate-pyruvate transaminase and response to hyperosmotic stress in the copepod *Tigriopus californicus. Biochemical Genetics,* 21, pp. 238-251.

Carlson, Elof A. 1975 — The Drosophila group. *Genetics,* 79, pp. 15-27.

Carlson, Elof A. H. J. Muller (1890-1967) 1972 — *Genetics,* 70, pp. 1-30.

Chakraborty, Ranajit; Fuerst, Paul A. & Nei, Masatoshi. 1980 — Statistical studies on protein polymorphism in natural populations. III. Distribution of allele frequencies and the number of alleles per locus. *Genetics,* 91, pp. 1039-63.

Chakraborty, Ranajit; Fuerst, Paul A. & Nei, Masatoshi. 1978 — Statistical studies on protein polymorphism in natural populations. II. Gene differentiation between populations. *Genetics,* 88, pp. 367-90.

Chalmers, Alan F. 1982 — *What is this thing called science?* Second edition. Open University Press, Milton Keynes.

Chambers, Robert 1845 — *Vestiges of the natural history of creation and other evolutionary writtings.* The University of Chicago Press, London.

Clarke, Bryan 1975 — The contribution of ecological genetics to evolutionary theory: Detecting the direct effects of natural selection on particular polymorphic loci. *Genetics,* 79, pp. 101-13.

Coyne, Jerry A. 1994 — Ernest Mayr and the origin of species. *Evolution,* 48:1, pp. 19-30.

Crow, James F. 1985 — The neutrality-selection controversy in the history of evolution and population genetics. In: Ohta, T. & Aoki, K. (eds). *Population genetics and molecular evolution.* Japan Sci. Soc. Press, Tokyo. Springer-Verlag, Berlim, pp. 1-18.

Crow, James F. 1987 — Population genetics history: A personal view. *Annual Review of Genetics,* 21, pp. 1-22.

Darwin, Charles R. 1859 — *The origin of species by means of natural selection.* John Murray/1859, Penguin Books/1985, London.

Day, T. H.; Hillier, P. C. & Clarke, Bryan 1974a — The properties of genetically polymorphic isozymes of alcohol dehydrogenase in *Drosophila melanogaster. Biochemical Genetics,* 11, pp. 141-53.

Day, T.H.; Hillier, P. C. & Clarke, Bryan. 1974b — The relative quantities and catalytic activities of enzymes produced by alleles at the alcohol dehydrogenase locus in *Drosophila melanogaster. Biochemical Genetics,* 11, pp. 155-65.

Dennett, Daniel C. 1995 — *Darwin's dangerous idea.* Allen Lane, The Penguin Press.

Dobzhansky, Theodozius. 1970 — *Genetics of the evolutionary process.* Columbia University Press, London.

Dobzhansky, Theodozius. 1967 — Looking back at Mendel's discovery. *Science,* 156, pp. 1588-9**.**

Dunn, L.C. & Dobzhansky, 1952 — Theodozius. *Heredity, race and society.* A Mentor Book, Published by The New American Library, New York.

Engels, Friedrich 1954 — *Dialectics of nature.* Lawrence & Wishart.

Feyerabend, Paul K. 1975 — *Against method: Outline of an anarchistic theory of knowledge.* New Left Books, London.

Feyerabend, Paul K. 1974 — Consolations for the specialist. In: Lakatos, Imre & Musgrave, Alan (eds). *Criticism and the growth of knowledge.* Cambridge University Press, Cambridge, pp. 197-230.

Fisher, Ronald A. 1966 — 'Has Mendel's work been rediscovered?' In: Stern, C. & Sherwood, E.R. (eds). *The origin of genetics: A Mendel source book.* W.H. Freeman & Company, S. Francisco, pp. 139-72.

Fisher, Ronald A. 1930 — *The genetical theory of natural selection.* Oxford University Press, Oxford.

Fuerst, Paul A.; Chakraborty, Ranajit & Nei, Masatoshi 1977 — Statistical studies on protein polymorphism in natural populations. I. Distribution of single locus heterozygosity. *Genetics,* 86, pp. 455-83.

Gillespie, John H.	On Ohta's hypothesis: Most amino acid substitutions are deleterious. 1995 *Journal of Molecular Evolution,* 40, pp. 64-9.
Gould, Stephen Jay. 1994	Ernest Mayr and the centrality of species. *Evolution,* 48:1, pp. 31-35.
Gould, Stephen Jay. 1977	*Ever since Darwin.* Penguin Books, London.
Haldane, John B. S. 1932	*The causes of evolution.* Longmans, Green & Co./1932. Princeton University Press/1990, New Jersey.
Hilbish, Thomas J. Koehn, Richard K. 1985a	The physiological basis of natural selection at the LAP locus. & *Evolution,* 39, pp. 1302-17.
Hilbish, Thomas J. & Koehn, Richard K. 1985b	Dominance in physiological phenotypes and fitness at an enzyme locus. *Science,* 229, pp. 52-4.
Hubby, J.L. & Lewontin, Richard C. 1966	A molecular approach to the study of genetic heterozygosity in natural populations. I- The number of alleles at different locos in *Drosophila pseudoobscura. Genetics,* 54, pp. 577-94.
Kuhn, Thomas S. 1970	*The structure of scientific revolutions.* University of Chicago Press, Chicago.
Kimura, Motoo 1990	Some models of neutral evolution, compensatory evolution, and the shifting balance process. *Theoretical Population Biology,* 37, pp. 150-8.
Kimura, Motoo 1986	DNA and the neutral theory. *Philosophical Transactions of the Royal Society of London B,* 312, pp. 343-54.
Kimura, Motoo 1982	The neutral theory as a basis for understanding the mechanism of evolution and variation at the molecular level. In: Kimura, M. (ed.). *Molecular evolution, protein polymorphism and the neutral theory.* Japan Sci. Soc. Press, Tokyo. Springer-Verlag, Berlim, pp. 3-56.
Kimura, Motoo 1968	Evolutionary rate at the molecular level. *Nature,* 217, pp. 624-6.
Koehn, Richard K. 1985	Adaptative aspects of biochemical and physiological variability. In: Gibbs, P. E. (ed.). *Proc. 19th European marine biology symposium.* Cambridge University Press, London, pp. 425-41.
Koehn, Richard K. 1978	Biochemical aspects of genetic variation at the LAP locus in *Mytilus edulis.* In: Battaglia, B. & Beardmore, J.A. (eds). *Marine organisms: Genetics, ecology and evolution.* Plenum Press, London, pp. 211-27.
Kreitman, Martin 1996	The neutral theory is dead. Long live to the neutral theory. *BioEssays,* 18:8, pp. 678-83.
Krige, John 1980	*Science, Revolution and Discontinuity.* Harvester, Brighton, Sussex.
Lakatos, Imre 1974	Falsification and the methodology of scientific research programmes. In: Lakatos, Imre & Musgrave, Alan (eds). *Criticism and the growth of knowledge.* Cambridge University Press, Cambridge, pp. 91-196.
Lakatos, Imre & Musgrave, Alan (eds). 1974	*Criticism and the growth of knowledge.* Cambridge University Press, Cambridge.
Leigh, Jr., Egbert G. 1990	Introduction by Egbert G. Leigh Jr. In: Haldane, J.B.S. *The causes of evolution.* Princeton University Press, New Jersey, pp. ix-xxvi.

Levins, Richard & Lewontin, Richard C. 1985 — *The dialectical biologist.* Harvard University Press, London.

Lewontin, Richard C. 1991 — Twenty-five years ago in genetics: Electrophoresis in the development of evolutionary genetics: Milestone or millstone? *Genetics,* 128, pp. 657-62.

Lewontin, Richard C. 1974 — *The genetic basis of evolutionary change.* Columbia University Press, New York.

Lewontin, Richard C. & Hubby, J. L. 1966 — A molecular approach to the study of genetic heterozygosity in natural populations. II- Amount of variation and degree of heterozygosity in natural populations of *Drosophila pseudoobscura. Genetics,* 54, pp. 595-609.

Losee, John 1993 — *A historical introduction to the philosophy of science.* Third edition. Oxford University Press, New York.

Marx, Karl. 1858 — *Grundrisse.* Pelican Books, London.

Maynard Smith, John 1989 — *Evolutionary genetics.* Oxford University Press, New York.

Mayr, Ernest 1993 — What was the Evolutionary Syntesis? *Trends in Ecology and Evolution,* 8:1, pp. 31-4.

Mayr, Ernest 1991 — *One long argument.* Penguin Books, London.

Mayr, Ernest 1977 — *Populaçöes, espécies e evoluçäo.* Companhia Editora Nacional, Editora da Universidade de São Paulo.

Mendel, Gregor 1866 — Experiments on plant hybrids. In: Stern, C. & Sherwood, E.R. (eds). *The origin of genetics: A Mendel source book.* W.H. Freeman & Company, S. Francisco, pp.1-48.

Mitton, Jeffry B. & Koehn, Richard K. 1975 — Genetic organisation and adaptative response of allozymes to ecological variables in *Fundulus heteroclitus. Genetics,* 79, pp. 97-111.

Nelkin, Dorothy & Lindee, M. Susan 1995 — *The DNA mystique: The gene as a cultural icon.* W. H. Freeman & Company, New York.

Nevo, Eviatar 1983 — Adaptative significance of protein variation. In: Oxford, G.S. & Rollinson, D. (eds). *Protein polymorphism: Adaptative and taxonomic significance.* Systematics Association by Academic Press, pp. 239-82.

Nevo, Eviatar 1978 — Genetic variation in natural populations: Patterns and theory. *Theoretical Population Biology,* 13, pp. 121-77.

Nevo, Eviatar & Beiles, A. 1988 — Genetic parallelism of protein polymorphism in nature: Ecological test of the neutral theory of molecular evolution. *Biological Journal of the Linnean Society,* 35, pp. 229-45.

Ohta, Tomoko 1996 — The current significance and standing of neutral and nearly neutral theories. *BioEssays,* 18:8, pp. 673-7.

Ohta, Tomoko 1992 — The nearly neutral theory of molecular evolution. *Annual Review of Ecology and Systematics,* 23, pp. 263-86.

Ohta, Tomoko & Gillespie, John H. 1996 — Development of neutral and nearly neutral theories. *Theoretical Population Biology,* 49, pp. 128-42.

Olby, Robert. 1975 — The protein version of the central dogma: A crisis for biologists. *Genetics,* 79, pp. 3-14.

Oparin, A. 1956 — *A origem da vida.* Editorial Vitoria Limitada, Rio de Janeiro.

Paul, Diane B. & Spencer, Hamish G. 1995 — The hidden science of eugenics. *Nature,* 374, pp. 302-4.

Place, Allan R. & Powers, Dennis A. 1984 — The LDH-B allozymes of *Fundulus heteroclitus*: II. Kinetic analysis. *Journal of Biological Chemistry,* 259, pp. 1309-18.

Popper, Karl R. 1972 — *Objective knowledge.* Oxford University Press, Oxford.

Popper, Karl R. 1969 — *Conjectures and refutations.* Routledge & Kegan Paul, London.

Powers, Dennis A. & Place, Alan R. 1978 — Biochemical genetics of *Fundulus heteroclitus.* I- Temporal and spatial variation in gene frequencies of LDH-B, MDH-A, GPI-B and PGM-A. *Biochemical Genetics,* 16, pp. 593-607.

Simpson, George G. 1953 — *The major features of evolution.* Columbia University Press, New York.

Skibinski, David O. F; Woodwark, Mathew & Ward, Robert D. 1993 — A quantitative test of the neutral theory using pooled allozymes data. *Genetics,* 135:1, pp. 233-48.

Solé-Cava, Antonio M. 1986 — *Studies of biochemical genetics and taxonomy in coelenterates and sponges.* PhD thesis. Department of Marine Biology, University of Liverpool, Port Erin, Isle of Man.

Watt, Ward B. 1994 — Allozymes in evolutionary genetics: Self-imposed burden or extraordinary tool? *Genetics,* 136, pp. 11-6.

Woodwark, Mathew; Skibinski, David O. F. & Ward, Robert D. 1993 — Analysis of distribution of single-locus heterozygosity as a test of neutral theory. *Genetical Research,* 62:3, pp. 223-30.

Wright, Sewall. 1966 — Mendel's ratios. In: Stern, C. & Sherwood, E.R. (eds). *The origin of genetics: A Mendel source book.* W.H. Freeman & Company, S. Francisco, pp. 173-5.

Wright, Sewall. 1931 — Evolution in Mendelian populations. *Genetics,* 16, pp. 97-159.

Recebido para publicação em setembro de 2000.
Aprovado para publicação em novembro de 2001.