Philosophical and Historical Dimensions of Charles S. Peirce's Self-Corrective Thesis (SCT)

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Contents

1. Introduction

The view that the growth of knowledge and the progress in science lie in its method – which is self-corrective – and that science in its development is moving closer to the approximate representation of reality, introduced by Charles S. Peirce as Self-Corrective Thesis (SCT), is controversial in the philosophy and history of science up to this day. Supporters claim that all the aspects of scientific inference (abduction, deduction, qualitative and quantitative induction) contribute to its self-correction, while critics maintain that the justification for the self-corrective character of scientific method is inadequate. Some critics argue that there is no justification for the self-corrective character of abduction; some claim that from all four methods only quantitative induction is proved to be self-corrective, therefore all scientific methods could be reduced to quantitative induction, and some reject scientific method as a means of approaching the truth. In this paper I explore Peirce's proposed scientific methodology and discuss it in comparison with all these objections, so as to defend the SCT and distinguish the context of its validity. I appeal to the historical case of the Chemical Revolution, i.e. the replacement of Stahl's phlogiston theory by Lavoisier's oxygen theory, in order to shed new light upon this well-known episode in the history of science, this time in the perspective of Peirce's methodological theory.

I begin in Chapter 1 with a few words about Peirce, the presentation of his SCT, its actual importance and its critique. In Chapter 2, in order to provide the background knowledge required for grasping Peirce's account of truth and the self-corrective nature of scientific inference, I explore Peirce's epistemological notions, his new Categories (Firstness, Secondness, and Thirdness), his pragmatic meaning of ideas, and his notion of inquiry based on belief and doubt, as well as their philosophical dimensions. Furthermore, I analyze Peirce's Logic of scientific method as a triadic unity (abduction, deduction and induction), his probabilism, fallibilism and his view on the contribution of scientific community to the process of scientific inquiry, so as to be able to explore his proposed scientific methodology.

I proceed to the justification of the SCT in Chapter 3 and explore through Peirce's works the development of his Self-Corrective Thesis **(SCT),** which followed his maturity of thought, as well as his arguments for its justification. Then I discuss this justification in comparison with the objections raised by the philosophers of science, in order to defend the SCT and distinguish the context of its validity.

In Chapter 4 I appeal to the historical case of the Chemical Revolution, in other words, the replacement of the phlogiston theory by Lavoisier's oxygen theory, so as my conclusions of the self-corrective character of scientific method from the first part to be tested, supported or even corrected. Here I examine the historical data and discuss its interpretations, given by different methodological views (positivists, conventionalists, falsificationists, Kuhn, Lakatos and social constructivists), in comparison with Peirce's scientific methodology. This discussion enables me: first, to explore whether Peirce's account is more plausible than the existing accounts of this well-known episode in the history of science, second, to appraise the different aspects of the SCT (scientists' skill, self-corrective sorts of inference, criteria of admissibility of the hypotheses, scientific community etc.), in order to justify it. And third, in case of discovering any weaknesses or omissions of Peirce's account, to integrate some new views into the Peircean conception of the SCT, which might secure the self-corrective mechanisms of the scientific method better.

I summarize all my conclusions in Chapter 5, so as to show that my own revised interpretation of the SCT can stand against its modern critics, it is within the Peircean framework of scientific inquiry and valid for single disciplines, as well as that it is consistent with the historical data of the Chemical Revolution.

1.1. Charles S. Peirce's Self-Corrective Thesis

Charles Sanders Peirce (1839-1914), who is best known as the founder of pragmatism and semiotics, was an American philosopher, natural scientist, mathematician and logician, born in Massachusetts. He was educated as a chemist and employed as a scientist for nearly 30 years, and he made major contribution to research methodology, formal logic, philosophy of science, epistemology, mathematics and theory of signs (semiotics). He foresaw that electrical switching circuits could carry out logical operations, and this idea was used decades later to produce digital computers. Although he was largely ignored during his lifetime, he was later called by Paul Weiss in his *Dictionary of American Biography* [1] '*the most original and versatile of American philosophers and America's*

[1] See Brent Joseph (1993), *Charles Sanders Peirce: A Life*, Introduction.

greatest logician', by Bertrand Russell[2], '*the greatest American thinker'* and by Karl Popper[3], *'one of the greatest philosophers of all times'.*

Peirce's Self-Corrective Thesis **(SCT)** is based on the idea that what permits us to make progress in science and allows us our knowledge to grow, is the fact that science uses methods that are self-correcting or error correcting. Scientific method according to him consists of **abduction** or retroduction that leads to the formation of hypotheses, **deduction** that draws conditional, experimental consequences and predictions of the hypotheses, and **induction** that tests them experimentally. It begins with some fundamental beliefs adopted by the individual, as a result of his experience and background knowledge, which serve as basis from which one can 'set out'[4]. It is distinguished from other methods of inquiry, because, although is fallible, it is self-corrective in nature, for it allows us, if it persisted in long enough (*in the long run*), to correct our errors by gradual modification of our hypotheses. Apart from that, it is sufficient for the temporal cessation of doubt and the establishment of new beliefs that cohere with experience, whereas when it is applied in the long run by a community of inquirers could lead to the establishment of true beliefs, or else beliefs that represent approximately the reality of natural laws, as regularities in nature.

Peirce in his earlier papers treated induction, deduction and abduction as independent forms of inference and held that only induction is self-corrective, if it persisted long enough[5]. However, in his later writings, after considering the role of observation in self-correction he

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[2] See Russell Bertrand (1945), *A History of Western Philosophy.*

[3] See Popper Karl (1972), *Objective Knowledge,* p. 212.

[4] See CP 5.416.1905*: 'The very state of mind in which you find yourself at the time you do set out, a state in which you are laden with an immense mass of cognition already formed, of which you cannot divest yourself if you would'* Those beliefs include also the whole corpus of opinions presented by the Scottish philosophy of Common-sense like Thomas Reid and Stewart. However, Peirce as Critical Common-sensist distinguishes himself from the philosophers of Common-sense and the scholastic realists *(CP 5.438-63, Monist* 15, October 1905, 481-99, *The Consequences of Pragmaticism),* as I am going to show, because he holds that some indubitable beliefs in the course of evolution have been changed and are no longer indubitable. Apart from that, those indubitable beliefs are subject to criticism and change, since in the process of inquiry these initial 'vague' premises will be replaced by scientific hypotheses, which in turn are subject to verification or falsification through observation and experiment. *(**CP*** indicates the *Collected Papers of Charles S. Peirce* followed by volume number, paragraph number and year of publication, e.g. *CP 2.281.1878,* indicates the 2nd volume, paragraph 281, year 1878. ***MS*** indicates unpublished manuscripts followed by number assigned in Richard Robin's *Annotated Catalogue of the Papers of Charles s. Peirce*).

[5] See (CP 2.281.1878): *'If (induction) duly persisted in, must in the very nature of things, leads to a result indefinitely approximating to the truth in the long run',* and (Studies in Logic, Probable inference 1883)*: 'Nor we must lose sight of the constant tendency of the inductive process to correct itself. This is of its essence. This is the marvel of it'.*

concluded that '*inquiry of every type has the vital power of self-correction and of growth' (CP 5.582. 1898; The first rule of Logic).*

As his thought was further developed, induction, deduction and abduction became closely interlinked and contributed to self-correction. In the year 1903 and afterwards, he clarifies his notion and holds that the three types of inference are complementary to one another, because it is only their use in concert that they can lead in the long run to the correction of our errors and to the true representation of reality (*the reals*)[6].

The definition of the 'reals' according to Peirce is the following: they exist independently of our opinions about them and affect our senses according to the laws of perception, while their true interpretation by the inquirers is the opinion about them defined as an ideal limit, which the scientific community will approximate gradually and inevitably, if the inquiry is carried out ad infinitum[7].

1.2. Actual importance of the Self-Corrective Thesis

Although SCT has been for more than 100 years a source of fascination and frustration for followers and critics respectively, it remains always current and modern due to its application not only to the most branches of scientific inquiry, but also to many aspects of human activity (computer technology, statistics, education, medicine, social economics, etc.). First, it is well known that during the last ten years self-corrective mechanisms and devices, based on Peirce's conception of the SCT, have been broadly and successfully

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[6] See (CP 7.327. 1903): *'Persistent and judicious use of abduction, deduction and induction in concert would lead from the arbitrary state of belief, however erroneous, to knowledge of the truth'*, and *(A Neglected Argument for the Reality of God, CP 2.769. 1905)*: *'The true guarantee of the validity of induction is that it is a method of reaching conclusions which, if it be persisted in long enough, will assuredly correct any error concerning future experience into which it may temporarily lead us. This it will do not by virtue of any deductive necessity (since it never uses all the facts of experience, even of the past), but because it is manifestly adequate, with the aid of retroduction and of deductions from retroductive suggestions'.*

[7] See more (CP 3.254. 1877): *'There are real things, whose character is entirely independent of our opinions about them; those reals affect our senses according to regular laws, and though our sensations are as different as our relations to the objects, yet by taking advantage of the laws of perception', (Some Consequences of Four Incapacities, CP 5.311-312. 1868): 'Thus, the very origin of the conception of reality shows that this conception essentially involves the notion of a community, without definite limits, and capable of a definite increase of knowledge'*, also *(How to Make Our Ideas Clear, CP 5.406-407. 1878): 'The opinion which is fated to be ultimately agreed to by all who investigate, is what we mean by the truth, and the object represented in this opinion is the real'*, and *(Truth and Falsity and Error, CP 5.565-566. 1902): 'Truth is that concordance of an abstract statement with the ideal limit towards which endless investigation would tend to bring scientific belief'.*

introduced in Computer programming and technology. As Phyllis Chiasson argues[8], Relational Thinking Styles (RTS), a model for identifying practical reasoning habits, inspired by Peirce's *metodeuthic* (abduction, deduction and induction), is amenable to computer modelling of the abductive like process, since it is capable of predicting future consequences (deductive predictions) and of empirical verification (inductive) by means of a reliable assessment tool; therefore it can contribute to the development of an abductive inference engine.

Second, as Deborah Mayo argues[9], 'Peirce's SCT provides something current statistical methodology lacks: an account of inductive inference and a philosophy that links the justification for statistical tests to a more general rationale for scientific induction'. Moreover, as Matthew Lipmann argues, modern curriculum should aim at developing more critical thinking, because it is self-corrective about student's thinking, as it corrects the subjective and partial character of the individual perspective[10]. As Kakas A. C., Kowalski R. A. and Toni F.[11] have shown, abductive models, based on Peirce's method of abduction can generate causal explanations for fault diagnosis and can be used for model-based diagnosis in medicine, where the candidate hypotheses are the possible causes (diseases in this case) and the observations are the symptoms to be explained.

Furthermore, institutionalists[12] in social economics, following the ideas of Dewey[13] (value judgments) and C. E. Ayres[14], have tried to apply Peirce's SCT to matters of social values and policies. In other words, values are seen as being determined, appraised, reappraised

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[8] See more Phyllis Chiasson, '*The Semiotic Structure of Practical Reasoning Habits'*, in Gudwin R. and Queroz J. (2007), *Semiotics and Intelligent Systems Development*, pp. 70-108.

[9] See Mayo Deborah G. (2005), *Peircean Induction and the Error-Correcting Thesis*, in *Transactions of the Charles S. Peirce Society*, Spring 2005, Vol. XLI, pp. 299-319.

[10] Lipman Matthew in (2003), *Thinking in Education*, pp. 205-241, shows how Peirce's SCT can be applied to modern curriculum.

[11] See Kakas A. C., Kowalski R. A. and Toni F., *The Role of Abduction in Logic Programming*, in Gabbay D. et al. (1998), *Handbook of Logic in Artificial Intelligence and Logic Programming*, pp. 235-324. For more detailed account and examples see also Peng Y., James A. Reggia (1990), *Abductive Inference Models for Diagnostic Problem-solving.*

[12] Gordon W. in (1980), *Institutional Economics*, pp. 43-45, argues: *'The value theory of Institutional economics, which may also be called instrumental value theory (and is in the general tradition of C. S. Peirce, William James and Dewey) views value determination as a process involving continuously testing a technique… But at the same time that the quality or the value of the technique is being tested, the value itself is subject to reappraisal in the light of the consequences of the effort to implement it…The value theory, then, is that values are created and identified in a process involving self-correcting value judgements'* .

[13] See Dewey John (1939), *Theory of Valuation.*

[14] See Ayres C. E. (1961), *Towards a Reasonable Society*, pp. 282-285.

and modified in a continual process of instrumental and experimental investigation, similar to Peirce's process of scientific inquiry, since it is self-corrective. But this view, as M. Rutherford[15] argues, misses the point that, according to Peirce, scientific method is self-corrective only in the indefinite long run and not in the short term of social experimentation. But, in my opinion, if one sees the development of the values throughout the history of mankind can infer that mankind can discover better values through that experimental process of values modification.

As Peirce argues: '*everybody uses the scientific method about a great many things, and ceases to use it when he does not know how to apply it' (CP 5.384),* because every one after taking into account the indications of experience instinctively tries to correct the errors of his conducts, so as to achieve better results. However, if one does not know the certain norms of scientific method, since apart from descriptive it is also normative; he abandons soon this initial attempt.

1.3. Critics of Peirce's SCT

Even though SCT has been broadly applied, it has been severely criticized, as N. Rescher argues[16]: '*No part of Peirce's philosophy of science has been more severely criticized, even by his most sympathetic commentators, than this attempted validation of inductive methodology on the basis of its purported self-correctiveness*'. Critics may be classified into different groups: 1. Those who reject the self-corrective character of scientific method, as proposed by Peirce's account, since they consider it false and optimistic, 2. Those who claim that the accounts of Peirce's proposed scientific method and fallibilism are inconsistent, and 3. Those who reject scientific method as a means of approaching the truth, because convergence upon truth, apart from self-correctiveness, presupposes other valid accounts. Under the first category fall: 1. Those who regard the self-corrective character of abduction, as an inarticulate faith without logical justification, and as a matter of course reject the self-correctiveness of the whole unity (abduction, deduction and induction), 2. Those who maintain that all three methods should be reduced to induction, because from all

[15] See Malcolm Rutherford, *Science, Self Correction and Values,* in Lutz Mark (1989), *Social Economics: Retrospect and Prospect*, pp. 391-406.
[16] See Rescher N. (1978), *Peirce's Philosophy of Science: Critical Studies in his Theory of Induction and Scientific Method*, p. 20.

three inferences only induction is proved to be self-corrective, and 3. Those who argue that there is no deductive justification for the self-corrective character of induction.

In order to defend the SCT in this paper, after exploring Peirce's scientific methodology, I am going to show, first, that each method involves a distinct leading principle that contributes to the self-correction, second, that the three forms of scientific inference are irreducible, and third, that the whole unity forms a dialectical and gradual process, which in the long run can lead to the correction of errors and the growth of knowledge. Furthermore, I am going to show that critics' mistakes lie in the dissociation of the three sorts of inference from one another, or even in the underestimation of one of them, while according to Peirce's conception of the SCT, it is only when they are closely interlinked and complementary to one another that they can contribute to self-correction.

On the other hand, although I consider that Peirce's proposed scientific methodology is progressive and self-corrective in nature, as it allows us to correct our errors gradually, I am going to show that this self-corrective character is independent of Peirce's notion of convergence upon truth, because it can work without that presupposition, and apart from that, this notion is based on problematic account. But since Peirce's SCT is based upon his general epistemological and philosophical conceptions I explore in the next Chapter this background.

2. Background of the SCT

Peirce is committed to the view that each form of scientific inference corresponds to his Categories, therefore I begin with his phenomenology and theory of knowledge, so as to exhibit the relation between his Categories, theory of signs, interpretation of signs and his pragmatic maxim, applied to all the types of scientific inference. I also explore here his conception of inquiry, his Logic of abduction, deduction and induction, his probabilism, fallibilism and his view on the contribution of scientific community to the process of scientific inquiry. This background knowledge will enable me to proceed afterwards to Peirce's scientific methodology, which is the foundation of his SCT.

2.1. Peirce's theory of knowledge (Categories, Pragmatic maxim)

Peirce's phenomenology concerns the observation and analysis of experience, which although is similar to Hegel's *Phänomenologie des Geistes*, as he defines it (CP 1.544), it is significantly different, because it is not only restricted to experience, but is also extended to the *universal characteristics* of whatever is experienced[17]. First, Peirce distinguishes his phenomenology from psychology, since the latter is concerned only with the 'inner world' of a man, while his phenomenology embraces both the 'inner' and the 'outer world'. Second, since the word '*phenomena*' suggests a contrast with its opposite '*noumena*' or 'intelligible reality', he introduces the term '*phaneron*', which refers to what is present to the mind regardless to whether it corresponds to a real thing or not (CP 1.284).

In other words, Peirce is committed to the monistic view of phenomenology, because, according to him, the *phaneron* exhibits an essential unity, not a structureless unity but of distinguishable parts, therefore both the material and mental aspects exist not separately from one another in the *phaneron*. Apart from that, his phenomenology is focused on the universal characteristics and not on the individual ones of the *phaneron*, since he regards the *universals* as existing in the individuals (particulars).

[17] See CP 5.37.1903: '*I will not restrict it (phenomenology) to the observation and analysis of experience, but extend it to describing all the features are in common to whatever is experienced or might conceivably be experienced or become an object of study in any way direct or indirect'*.

As Edward C. Moore argues[18], Peirce being influenced by the *moderate realists* (Avicenna, Thomas Aquinas and Duns Scotus) holds that the external objects have an essential nature (essence: *ousia*), which can also exist in a mind. Essence neither is universal nor particular in itself, it cannot exist in a separate realm by itself (realm of Ideas as Plato thought), and when exists in a particular object, it is perceived through abstraction as a *universal*[19] *(ens ratonis)* e.g. universal terms 'man', 'horse' etc. Since the mind experiences the particulars in a form of abstract concepts, these concepts correspond to something which is to be found in reality; therefore they are real, as they have a foundation in fact. Furthermore, as the *universals* (e.g. man, horse or other names of natural classes) correspond to something, which all members of these natural kinds really have in common, independent of our thought (real), then the ultimate idea of the concepts can be found in particular experiences of 'horse' or 'man' etc. This notion gave Peirce the orientation from which he grew the 'pragmatic meaning of ideas', as I am going to show.

The business of phenomenology, according to Peirce, is 'to bring out and make clear the *Categories* or 'fundamental modes' of the *phaneron* (CP 5.38), so to say the indecomposable elements of the phenomena of the first rank of generality, following the definition given by Aristotle, Kant and Hegel. While Aristotle presents ten Categories of the same order (*Substance, Quantity, Quality, Relation, Location, Time, Position, Habit, Acting* and *Being Acted Upon*), Kant and Hegel distinguish between two orders of Categories. Kant, for instance, subsumes his list of twelve Categories under four more inclusive headings: *Quantity, Quality, Relation,* and *Modality*. Hegel on the other hand presents the long array of his Categories in his *Enzyklopädie* but they are really subordinate to the three stages of thought: *Thesis, Antithesis* and *Synthesis*.

Peirce considers Kant and Hegel's second set of Categories – which he calls universals – more important, as, for him, phenomenology should deal with universal and not with particular Categories; therefore his investigations are limited to those universal Categories, as he presents them in his early paper *'On a New List of Categories'* (1867). The main argument presented there is that there are two absolutely basic Categories: *Substance* and *Being*. *Substance* is the universal conception prior to any judgment and expresses that

[18] See Moore Edward C., *American Pragmatism*, pp. 25-29, and Moore Edward C., *The Influence of Duns Scotus on Peirce,* in Moore Edward (1961). Also Robin Richard (1964), *Studies in the Philosophy of C. S. Peirce.*

[19] See Barnes J. (1998), Aristotle's *De Interpretatione* VII, the definition of the *universal* given by Aristotle is: *'That which is by its nature predicated of a number of things*'.

something is present, which in a proposition is represented by the subject. But in every proposition, to which each sensuous impression can be reduced, there is also a joining of predicate to subject, which is the universal conception of *Being*. For example, in the proposition '*the stove is black*', '*the stove*' is the subject (*Substance*) and the '*is*' (*Being*) joins the predicate *'black'* to the subject or in other words applies the 'blackness' to it.

However, these two Categories are not for Peirce the only basic ones, there are three others required completing the analysis of a proposition, namely: *Quality, Relation* and *Representation.* In the previous example of proposition, the 'black' is referring to the *Quality*, therefore Peirce regards it as *'the first conception in order in passing from being to substance' (CP 1.551)*. The conception 'stove' is the more immediate; whereas the conception 'black' is more mediate. Since the *Quality* can be known with reference to a correlate, therefore after the Category of Quality comes the Category of *Relation*. The third Category of the group arises from the necessary reference to an interpretant, which every proposition implies. It is the activity that the representant performs; therefore Peirce calls it *Representation*. To sum up, the new list of Categories proposed by Peirce has the following arrangement:

Being

Quality (reference to a ground)

Relation (reference to a correlate)

Representation (reference to an interpretant)

Substance **(CP 1.555)**

As Peirce's thought was further developed, he became convinced by his study of the relatives that there were only three modes of logical combination, therefore he finally settled on three Categories, which are applicable to every phenomenon of experience. These are the *Firstness, Secondness* and *Thirdness*.

2.1.1. Firstness

Every *phaneron* exhibits a certain unique and irreducible feature, which is sheer *quality undifferentiated and unspecified*, immediate and present - not abstract like Hegel thought, but concrete - which Peirce calls *Firstness*. With this notion he emphasizes the concreteness and qualitative immediacy of experience, which is the beginning of our knowledge. This feature (quality of feeling) cannot be described, since it has no definite spatial and temporal location, but it can be denoted or identified through abstraction. To give an idea of its denotation Peirce suggests:

> 'Go out under the blue dome of heaven and look at what is present as it appears to the artist's eye', (CP 5.44),

or some specific instances of it such as:

> 'The color of magenta, the odor of attar, the sound of a railway whistle, the taste of quinine' (CP 1.304),

some of its characteristics are:

> 'That is first, present, immediate, fresh, new, initiative, original, spontaneous, free, vivid, conscious and evanescent. Only remember that every description of it must be false to it' (CP 1.357).

As Thomas Goudge argues[20], the relevant terms to the *Firstness* are what we call *quality* in Phenomenology, *feeling* or *sensation* in Psychology, *cell excitation* in Physiology, *fortuitous variations* in Biology and *indeterminacy* in Physics.

Sometimes Peirce applies the term 'potentiality' or 'possibility' of an idea that can enter a mind, which is not an actual idea. Although *Firstness* cannot be described, Peirce following the notion of Duns Scotus tries to interpret the qualities as 'mere abstract potentialities', which, as universals, exist in the particulars in a form of 'possibilities' before they become 'actual'. This potentiality is a 'power' in Aristotelian sense[21] that determines the future manifestation of the quality, when it is actualized, and it remains possible even when it is not actualized. Peirce disagrees with the nominalists on this point, so to say that the quality exists only when it is actualized, for as he argues:

> 'the iron, when is not under pressure, does not loose its power of resisting pressure and the red body in the dark does not loose its power to absorb the long waves of the spectrum'[22].

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[20] See Goudge Thomas (1969), *The Thought of C. S. Peirce*, p. 109.

[21] For more about *potentiality* and *actuality*, see Aristotle's *Metaphysics*, Book Z, 12, in Barnes J. (1998).

[22] See CP 1.422: *'A quality is a mere abstract potentiality; and the error of those nominalistic schools lies in holding that the potential or possible, is nothing but what the actual makes it to be... I ask the conceptualist, do you really mean to say that in the dark it is no longer true that red bodies are capable of transmitting the light at the lower end of the spectrum? Do you mean to say that a piece of iron not actually under pressure has lost its power of resisting pressure?'*

2.1.2. Secondness

The second Category of the *phaneron* Peirce calls *Secondness*, which is referring to the particular fact of experience, it has a definite spatial and temporal location and 'exists' in the strict philosophical sense[23]. To exist, according to Peirce, is to *'react with the environment' (CP 5.503)*; therefore *Secondness* contains an element of 'struggle' or 'reaction' *'due to brute fighting force or self assertion' (CP 1.434)*. It is a dyadic element of experience between two objects A and B, but it cannot be reduced to two ideas of *Firstness*, because it involves action between the two objects *A* and *B*, that which acts (*A*) and that which is acted upon (*B*)[24].

Each existent object effects upon other objects, therefore effects upon the senses of a human being, and when it is experienced by some mind, its 'potential' universal quality is actualized and produces the *percept*. *Percept* is a mental entity analogous to sense data or Locke's simple ideas, which are the fountains of knowledge, but it is distinguished from the notions of the subjective empiricists like Hume or Berkeley. This '*percept compels the perceiver to acknowledge it'*, so to say *'it acts upon us, it forces itself upon us without any reason or pretension of reason*' (CP 7.618 ff). It is a bare *happening*, irrational and accidental or *haecceitas*, to use the scholastic form. *Secondness* is the dominant characteristic of existence and experience: *'it is a brute existence and hence is the modality of actuality' (CP 1.175), 'a brute force existing, whether you opine it exists or not' (CP 2.138)*, therefore, for Peirce, it forces us to confess that it exists, and we cannot dismiss it and say 'I don't believe that it appears'. Hence, *Percept* is a part of the external real world, because: 1. It cannot be dismissed by an act of will, 2. Other people agree about its characteristics and 3. It can be used as a basis for prediction and experiment (CP 2.142). To show the contrast with idealism on this point Peirce describes the bruteness of external reality as follows:

> 'Still many (idealists) do deny (the reality of the external world) or think they do. Very well an idealist of that stamp is lounging down Regent Street… where some drunken fellow… unexpectedly let fly his fist and knocks him in the eye. What has become of his philosophical reflections now?' (CP 5.539).

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[23] See Wennerberg H. (1962), *The Pragmatism of C. S. Peirce*, pp. 35-38.

[24] Similar to the last two Categories of Aristotle: *poiein* (acting) and *paschein* (being acted upon).

From this point of view, he claims that all hypotheses must ultimately be brought to the test of experience (induction), since *'experience is the fountain of knowledge' 'All knowledge whatever comes from observation' (CP 1.238 also 5.611)* and *'experience is forced upon a man's recognition' (CP 5.613).*

As the objects around us act upon us, we also act upon them, and although this action is voluntary, it is in itself brute and unreasoning, as Peirce puts it: *'After I have determined how and when I will exert my strength, the mere action itself is in itself brute and unreasoning' (CP 1.431).* Since experience involves cognition in our mind, as a reaction to the experience, therefore *Secondness* involves the consciousness of the human *subject* in its existential aspect also, an element that refers to Kierkegaard's internal oppositional tension and the bruteness of human existence. As Richard J. Bernstein argues, Peirce with the use of his Categories shows a revealing way of linking up existentialism with empiricism[25].

To sum up, the relevant terms of the *Secondness* are: actuality in Aristotelian sense, fact, experience, action and reaction in Phenomenology, percept in Psychology, passage of nerve impulse in Physiology and force in Physics.

2.1.3. Thirdness

The final Category that Peirce calls *Thirdness* has more complex structure, because it signifies the triadic relation between three terms *A, B* and *C* (e.g. *A* gives *C* to *B*), which neither can be reduced to *A* plus *B* plus *C* nor to *A* and *B, B* and *C,* and *C* and *A* (CP 1.363). Peirce under *Thirdness* classifies many concepts, such as perceptual judgment, propositions, inference, thought, habit, laws, potentiality, intentions, meaning, conduct and signs.

In each observation, according to Peirce, after the *percept* arises the first spontaneous uncontrollable judgment (*perceptual judgment*), which involves *Thirdness. Perceptual judgments* provide the primitive units of our knowledge or else are *'the first premises of our*

[25] See Bernstein R. (1965), *Perspectives on Peirce*, pp 67-91. There is also an illuminating discussion about Firstness, Secondness, perceptual experience and Thirdness in Goudge Thomas (1969), *The Thought of C. S. Peirce*, pp. 85-110, Almeder Robert (1980), *The Philosophy of Charles S. Peirce: A Critical Introduction*, pp. 138-145. Also Helmut Pape, *The Logical Structure of Idealism,* and Carl Hausman, *Charles Peirce and the Origin of Interpretation*, both in Brunning Jacqueline and Forster Paul (1997), *The Rule of Reason: The Philosophy of Charles Sanders Peirce*.

reasoning' (CP 5.116). These are simple mental propositions like 'This is red', 'It is light', 'This building is large' etc. that we form in each observation. Each perceptual proposition consists of a subject, who is concrete and *particular* (this, that, he, it etc.), and of a predicate, which is a *general* (universal) term (red, light, large etc.) attached to the particular. Therefore, *'Perceptual judgments are the vehicles by which generality and universality enter into our knowledge' (CP 5.150)*. Peirce characterizes this process as the performance of the first instinctive abductions that generates hypotheses. He describes the whole process as follows:

> 'I see an azalea in full bloom. No, no! I do not see that; though that is the only way I can describe what I see. That is a proposition, a sentence, a fact; but what I perceive is not proposition, sentence, fact, but only an image, which I make intelligible in part by means of a statement of fact. The statement is abstract; but what I see is concrete. I perform an abduction when I so much as express in a sentence anything I see' (MS 692, 1901; underline mine).

Perceptual judgment provides the first point of contact between experience and abstract reasoning, since it is the first abstract interpretation of the *percept* or else the formation of the first proposition about it; therefore Peirce considers it the basis of our knowledge. The **self-corrective** nature of scientific inference is based upon this contact, because these spontaneous and instinctive perceptual judgments through abduction, as I am going to show later, generate the first hypotheses. Thus experience and instinct both contribute to the formation of the perceptual judgment. Perceptual judgment is infallible, as it asserts the nature of the *percept*, and it is not subject to criticism and control, since the process of forming a perceptual judgment is very swift; therefore Peirce characterizes it as a *'subconscious process' (CP 5.181)* or as *'belonging to the instinctive part of the mind' (CP 5.212)*. In other words, perceptual judgment is a limiting case of the more general procedure of abductive inference that also involves memory and hypothesis. Since perceptual judgment does not exist until is completely made and what remains is only its memory (CP 5.544) e.g. 'this is red', therefore only memory is fallible and subject to criticism and control. Furthermore, hypotheses generated by the memory of the perceptual judgments are subject to criticism and control, as they involve an act of conscious volition and self-controlled thought; therefore they can be brought to the test of experience for verification and further evaluation. The whole process of perception can be represented as follows:

Percept – perceptual judgment – memory – hypothesis

Peirce is committed to the view that all things both animate and inanimate have a disposition or property to behave in a certain way under certain conditions, or as he states: *'would behave in a certain way whenever a certain occasion should arise' (CP 8.380)*. These dispositions that Peirce calls *habits* are characterized by generality, therefore they are universals. They are manifested in the physical phenomena, in a form of statistical regularities, i.e. all particular things conform to certain *laws* or have certain dispositions (habits). Peirce's notion of universals is distinguished from Duns Scotus' notion[26], because he holds that these laws are *'general principles (universals) that are really operative in nature'* (CP 5.67). To make it clearer, *universals*, according to Peirce, are not simply *ens ratonis*, but exist *in re*, so to say, they correspond to something that really have in common the members of the same class (e.g. man, horse, hardness[27] etc.) and determines their disposition under certain conditions; therefore they are really operative laws in nature. While, for Duns Scotus, *universals* do not exist *in re*, but, rather, they are the outcome of a mental abstraction of what is in common in the individual members of the same class[28]. Peirce provides the following example to explain this notion: When we release a stone, we know that the stone will fall towards the earth, for its behavior is governed by law, which is real fact and not a mere figment of the mind, that is to say, independent of our thought. The proof that is real is the fact that we have no influence or control over the fall of the stone,

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[26] See MS 309, Fourth Harvard Lecture (1903), *The Seven Systems of Metaphysics*: *'I should call myself an Aristotelian of the scholastic wing, approaching Scotism, but going much further in the direction of scholastic realism'.*

[27] Certainly, this notion was not the initial view of Peirce, but followed the development of his thought. In 1878, *How to Make our Ideas Clear III*, he said that a diamond, which supposed to have been 'crystallized in the midst of a soft cushion of cotton and remained there until was finally burned up, we can say that is soft because it has never been scratched'. This implied the nominalistic view that universal qualities are not real dispositions (potentialities) but actuality makes them to be real. Therefore in his later writings, when he claims to be a scholastic realist, he returns to the same example and corrects his initial view: *'Was that diamond really hard? It is certain that no discernible actual fact determined it to be so… As for the pramgaticist, it is precisely his position that nothing else than this can be so much as meant by saying that an object possesses a character. He is therefore obliged to subscribe to the doctrine of a real modality, including real Necessity and real Possibility' (CP 5.438-63. 1905, Issues of Pragmaticism). 'I myself went too far in the direction of nominalism when I said that it was a mere question of the convenience of speech whether we say that a diamond is hard when it is not pressed upon, or whether we say it is soft until it is pressed upon. I now say that experiment will prove that the diamond is hard, as a positive fact. That is, that it is a real fact that it would resist pressure, which amounts to scholastic realism' (CP 8.208. 1905).*

[28] See for differences between Peirce and Duns Scotus on universals: Almeder, R. (1980), *The Philosophy of Charles S. Peirce: A Critical Introduction*, pp.160-182, Charles McKeon, *Peirce's Scotistic Realism*, in Wiener Philip P. (1952), *Studies in the Philosophy of Charles Sanders Peirce*, and Moore Edward C., *The Influence of Duns Scotus on Peirce,* in Moore Edward and Robin Richard (1964), *Studies in the Philosophy of C. S. Peirce.*

while if it were a construction of the mind we would have control over it by our will. Thus, for Peirce, *Thirdness* is operative but conditional principle in nature in a form of general laws, dispositions or habits, because, as in the example of the falling stone, the law of gravity is operating with the condition that nothing disturbs the free fall. Therefore Peirce defines *Thirdness* as follows:

> 'Everything in the universe is governed by exhibit laws, and these laws are to be understood in terms of conditional generality characteristic of Thirdness' (CP 4.157)
> 'What we call a thing is a cluster or habit of reactions' (Ibid).

By applying now the same doctrine to the human thought, since all things tend to 'take on habits', Peirce holds that someone's *beliefs* are also his *habits*. E.g. when someone believes in something he thinks and behaves in a certain way, which means he has a disposition to expect from the events such and such experiences (expectation of certain predictions). Hereupon he bases his notion of *doubt* and *inquiry*, as follows: When one's prediction is falsified, his belief-habit is shaken off, therefore he needs to replace it, and as a matter of course he begins his inquiry that searches for another belief more stable and firm. As Peirce describes it, the whole process begins with a surprising fact, which is directly perceived, and leads to the privation of habit-belief, which is doubt:

> 'Doubt is not a habit but a privation of habit' (CP 5.417)
> 'Inquiry begins with the observation of a surprising phenomenon' (CP 6.469)
> 'When a man is surprised, he knows that he is surprised…by direct perception, that is in a direct perceptual judgment' (CP 5.57, 5.58).

Furthermore, if the inquiry could be conducted indefinitely and scientifically (experimentally verification of the prediction of hypotheses), it would eliminate all unstable beliefs and leave the community with the common collection of beliefs, which would be perfectly stable (true beliefs)[29]. Here we see another aspect of the **self-corrective** nature of scientific inquiry, arising from doubt (change of habit-thoughts) and leading to the growth of knowledge and continuous correction of false beliefs, due to a genuine dissatisfaction with them. I shall return to this topic in the relevant Section in detail.

An objection, which may arise here with Peirce's concept of *Thirdness,* is that, this *C,* as a principle of relation between two entities *A* and *B,* it is very vague and general one, and it

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[29] See more Chisholm R., *Fallibilism and Belief,* in Wiener Philip P. (1952), *Studies in the Philosophy of Charles Sanders Peirce.*

might include different and contrasting cases of relations. Because *C* can be perceptual judgment, proposition, inference, habit, law, universal quality etc. If we take into consideration Hume's notion that the relation between two entities can be by *resemblance, contiguity (nextness in time or place) or cause and effect*[30], we may infer that we cannot classify all principles of connections under one group, since Peirce does not clarify this relation. On the other hand, by taking into account all the above definitions of *Thirdness* given by Peirce concerning i.e. perceptual judgment, proposition, inference, law and habit, we can infer that the only plausible alternative interpretation for the definition of *Thirdness*, as a principle of relation, is in the context of causal relation (cause and effect) between two entities under certain conditions, therefore it allows us to make predictions. In this sense it can be justified the consequent-antecedent relation, which exists in abduction, as I am going to show. This interpretation, in my opinion, may include partly the definition of universal qualities, e.g. hardness, where its attachment to the particular describes its disposition to behave in a certain way under certain conditions - i.e. causally and relatively to our actions - as explained with the example of the proposition 'diamond is hard'. But since universal qualities express dispositions to behave in a certain way under certain conditions (Peircean *'would be's')*, though conditional and relative to our action, they presuppose that pre-exists in the particulars an inherent principle, which determines this disposition. Therefore we have to distinguish the Peircean causal principle of *Thirdness* from the universal dispositions *'would be's'*.

Another controversial aspect of *Thirdness* concerns the direct or indirect perception[31] of *universals* (generality), as well as their relation to the perceptual judgments and laws. This aspect is very significant for the justification of the self-corrective nature of abduction, as I am going to exhibit later. As I said at the beginning of this Section, Peirce holds that the *universals* exist in the *particulars* 'potentially', and that in each perception they are 'actualized' in our mind, but whether wholly or partially actualized, directly or indirectly, it is not clear; because, Peirce in his earlier works characterizes Aristotle's doctrine, namely 'the general is directly perceived in the particular', as *'an extraordinary crude opinion' (CP 2.26)*. While in his later works he states:

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[30] See David Hume (1993), *Enquiry Concerning Human Understanding, Of the association of ideas*, p. 14: '*To me, there appear to be only three principles of connexion among ideas, namely, Resemblance, Contiguity in time or place, and Cause or Effect*'.

[31] For more about direct or indirect perception of Thirdness, see Goudge Thomas (1969), *The Thought of C. S. Peirce*, *Thirdness,* pp. 91-95, and Almeder, R. (1980), *The Philosophy of Charles S. Peirce: A Critical Introduction,* pp. 136-147.

> 'Thirdness is directly perceived' (CP 5.20), 'Generality pours upon us through every avenue of sense' (CP 5.157),
> 'Generality cannot be given otherwise than in perceptual judgment' (CP 5.186).

First, in both cases, as we can see, the *universal* is real, since it exists really and potentially (even when not actualized) in the particular. Therefore in his later works he states: *'Pragmaticism could scarcely have entered a head that was not already convinced that there are real generals' (CP 5.503*). Otherwise if one accepts the nominalist notion that potentiality is merely an idea in our mind, then there is no reason why objects should act in accordance with our idea, in other words there is no basis for prediction (as with the example of the stone), which, as Peirce holds, has been proved that it successfully works.

Second, in my opinion, Peirce in his later work meets the Aristotelian view and holds that 'we perceive the general feature with the formation of the first spontaneous perceptual judgment'. This conclusion arises from the comparison of 'spontaneous perceptual judgment' with 'directly perceived' from the above mentioned citations, because perceptual judgment is characterized by Peirce also as perception, whereas observation is more related to the *percept*[32]. Furthermore, since many spontaneous perceptual judgments are consisted in subconscious thought, they are false, as he argues: *'the error is one of judgment not of perception'*[33] *(CP 5.568)*. For instance, when one observes a stick that is partly in the water (*percept*), his first perceptual judgment that 'the stick is bent' is in one sense infallible, since it ascertains faithfully the nature of the *percept*, but in another sense, it is false, since further inquiry reveals that it is not the stick but 'the light rays that are bent'. We can infer, then, that, for the later Peirce, Thirdness is directly given in perception in a form of universal predicate, therefore it is justified the abductive inference in a form of abstract interpretation of the percept, namely a premise that contains a general predicate, which is attached to the particular. This aspect, in my view, reveals Peirce's commitment to direct realism.

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[32] See: '*Observation is a process of attentive experience, involving some often great effort'* (CP 2.605), and: '*the most ordinary fact of perception, such as 'it is light', involves precisive abstraction, or precision'* (CP 4.235).

[33] See O' Connor Eugene (1993), Epicurus' *Letter to Herodotus,* 51, and compare with my analysis in Bakalis (2005), pp. 193-7: *'There is always falsehood and error involved in importing into judgement an element additional to sense perceptions, either to confirm or deny'.* Peirce had a thorough knowledge of Greek philosophy; therefore the citations of the Greek philosophers.

Furthermore, according to Peirce's notion of general potentialities, universals are inexhaustible *(Perceptual judgment represents one or more feature of the known object without exhausting the meaning of the object,* CP 7.198), and the meaning of universals *'cannot be exhausted by any multitude of existent things' (particulars) (CP 5.103),* either in the past, in present or in the future, as they can be applied to infinite instantly actualizations[34]. For example, the natural laws, like habits, embody the characteristic of potentiality, in a sense of possible dispositions or behaviors, although are not actualized wholly, they embrace all the phenomena in its future manifestations.

Apart from that, the laws meditate between possibility (potentiality in a sense of *Firstness*) and manifestation (actuality in a sense of *Secondness*), so to say, the laws or habits make the potential actual; therefore they can explain the phenomena. Our theories about natural phenomena, e.g. Newtonian laws of mechanics embrace only a part of bodies relation manifestations, while relativity theory is more universal, because includes the mechanics. But, perhaps, in the future some phenomena of mass manifestations (actualizations) cannot be explained with relativity theory; therefore will follow a new theory.

In order to solve the problem of inexhaustibility Peirce introduces his fallibilism, as I am going to show later, and he argues that we need to correct through experience continuously the hypotheses generated by perceptual judgments[35]; therefore he proposes the indefinite inquiry carried out by the scientific community. I can proceed now to Peirce's theory of signs and examine how it is connected with his three Categories.

2.1.4. Signs

Peirce's theory of ***signs*** presented in his *Speculative Grammar* rests also upon the generality of perception. *Signs,* as symbols, are, for Peirce, the medium through which the rationality in the universe can be expressed and communicated[36]. Following the triadic logic in every symbolic state, Peirce distinguishes three divisions of signs, which he also

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[34] See Sfendoni-Mentzou Demetra, *Reality of Thirdness,* in Cohen Robert S. (1996), *Realism and Anti-Realism in the Philosophy of Science.*

[35] See CP 2.141: *'The only way we can correct perceptual judgements … is to collect new perceptual facts relating to new percepts'.*

[36] Peirce in his semiotics (theory of signs) reformulates and develops further the triadic Stoic theory of signs (*σημεία*: semeia), which according to Diogenes Laertius (Adv. Math. II, 245), held that '*the sign is an antecedent proposition in a valid hypothetical major premise, which serves to reveal the consequent'.* See more Winfried Nöth (1995), pp. 15-16.

calls *representaments*, as follows: **(a) the sign in itself, (b) the sign in relation to its object and (c) the sign in relation to its *interpretant***[37] (effect of the sign on an interpreter, idea in a person's mind). Each one of them is subject to three Categories, therefore we obtain the following trichotomies: Under (a) fall: *Qualsign, Sinsign and Legisign,* under (b): *Icon, Indice* and *Symbol,* and under (c): *Rheme, Dicisign* and *Argument.* I quote the following table indicating the classification of the signs:

Categorical Aspect	**(a) Sign in itself**	**(b) Sign in relation to its object**	**(c) Sign in relation to its interpretant**
Matter or Potency (*Firstness*)	*Qualsign* or *Tone*	*Icon*	*Rheme*
Existence, Compulsion (*Secondness*)	*Sinsign* or *Token*	*Indice or Index*	*Dicisign, Dicent Sign*
General rule or Law (*Thirdness*)	*Legisign* or *Type*	*Symbol*	*Argument*

Signs in themselves are: **1. *Qualsign*** or ***Tone,*** a sign that refers to its material aspect (represents sheer quality or appearance e.g. color, form, size etc.), **2. *Sinsign*** or ***Token*** refers to its existential aspect (presupposes a group of qualsigns), and it represents a specific object or event e.g. traffic sign and **3. *Legisign*** or ***Type*** refers to its rural aspect (general rule or law established by men). For instance, the word 'the' is repeated many times on this page (the generality, the universe, the logic etc.), and every time it has a different meaning, since every time it is referring to different specific object or instance (sinsign), established by grammar laws of the English language.

Signs in relation to its object are as follows: **1. *Icon*** is a sign, which is similar to the object that represents. When it resembles in respect of qualities, it is called an *image* (e.g. photograph, picture etc.), when in respect of form and analogous relations, it is called a *diagram* (e.g. map, blueprint of a building etc.), while when it shows a general 'parallelism' we have a *metaphor*. To sum up, *Icons* can help us to get knowledge about the formal and the structural features of the world (aspect of Firstness).

[37] See Goudge Thomas (1969), *The Thought of C. S. Peirce,* and Pape Helmut (2004), *Charles S. Peirce zur Einführung.*

2. *Indice* or ***Index*** bears no resemblance to its object, and it is always referring to single unit or collection of units (singular, existential aspect). Its compulsive aspect of *Secondness* lies in directing attention to the object or event that represents. For example, a weather-cock is an index of the direction of the wind, a cry 'Hi' uttered by a driver of a vehicle is a warning to a careless pedestrian (compulsive), a demonstrative pronoun such as 'this' or 'that' call attention to some item present to speaker or listener. In other words, Indices enable us to know particular existents by compelling us to perceive it: *'They (indices) call upon the hearer (or reader, observer) to use his power of observation, and so establish a real connection between his mind and the object' (CP 2.287*; parenthesis mine*)*. Perceptual judgment is an *index* of the *percept*, as Peirce argues[38], for the *'external world cannot be described as it really is; it can only be denoted by indices (CP 4.530).*

3. Symbol is always indicating a kind of things (*universal*), not a *particular* existent, since its meaning is associated with general idea or law (Thirdness). The words 'watermelon', 'star', 'man', 'gravity' etc. do not specify particular existents, but kinds, classes and laws; therefore *Symbols* provide us the means of knowing universals (kinds, classes and laws), because as general descriptions can only represent what is general. They enable us to imagine the things that represent and create abstractions, without which we should lack a great engine of discovery. Peirce gives the following example to exhibit the connection between Index and Symbol: *'That foot print that Robinson Crusoe found in the sand (compulsive fact), and which has stamped in the granite of fame, was an Index to him that some creature was on his island, and at the same time, as a Symbol called up the idea of a man (abstraction)' (CP 4.543*; parentheses mine). This third kind of *signs* is the most important, for Peirce, for their meaning is associated with his *pragmatic maxim*, as I am going to show.

Signs represented by their *interpretants* concern propositions or their parts, as follows: **1. *Rheme*** is a part of proposition and provides the framework for a variety of propositions, e.g. in the formula '--------- *is a philosopher'* the *rheme* is the blank form (CP 4.560). This formula can become a proposition when the blank is filled with a proper name or subject. The aspect of *Firstness* (structural, potential) is clearly entailed here, because each time the blank is filled with different name (e.g. Socrates, Aristotle, Descartes, he etc.) another

[38] See Collected Papers (CP 7.628).

proposition will arise. Hence, *rheme* entails potentiality, since it makes many (indefinite) propositions possible. See: *'Non relative rhema: --------- is mortal, is nothing but a proposition with its indices or subjects left blank or indefinite' (CP 3.440).*

2. *Dicisign* is a singular proposition, e.g. 'this is red', which conveys some factual information to the interpretant. Peirce describes it as follows: *'Dicisign is equivalent to grammatical sentence, whether it be Interrogative, Imperative or Assertory' (CP 4.538).* Its compulsive aspect of *Secondness* lies, first, in the fact that compels the 'listener' to acknowledge the proposition, even when it is in a form of a question, e.g. the proposition 'what time is it'. And second, it lies in the assertion of a certain belief, because each proposition when is expressed asserts a belief and compels the 'listener' to accept it. When the 'speaker' and the 'listener' are the same person (ourselves), just like in case of personal judgment, as Plato argues in the *Sophist*[39], there is a process of compelling ourselves to accept a belief.

3. *Argument* is a complex symbol, which is a sign of a law for its interpretant. Peirce calls it *'a triple or rationally persuasive sign' (CP 2.309),* and it is composed of at least three *dicisigns* (propositions or premises) just like a traditional syllogism, which is regarded as a formal regularity or law of inference. For example, the deductive *argument*: 'Socrates is mortal' derives from its premises (*dicisigns*): 'All men are mortal' and 'Socrates is a man'. In sum, *arguments* are the fundamental ways by which signs are represented, are composed of *dicisigns* (propositions), while propositions are composed of *rhemes,* whose subjects are left blank.

The effect that a sign has on the interpreter (subject) is what Peirce calls *interpretant,* which he divides into *immediate, dynamical and final interpretant.* Immediate interpretant consists in the effect that the sign is naturally fitted to produce before it actually gets interpreted (CP 4.536), in other words the 'potential' effect of the sign.

The dynamical interpretant is *'the actual effect which the sign determines' (CP 4.536)* and is produced by a sign upon the interpreter. It is further divided into *emotional, energetic* and *logical interpretant.* Emotional interpretant is principally the feeling produced by the signs upon the interpreter e.g. a piece of concerted music produce a series of feelings. Energetic

[39] See Cooper M. John (1997), Plato's *Sophist,* 263e: *'Aren't thought and speech the same, except that what we call thought is speech that occurs without the voice inside the soul in conversation with itself?'.*

interpretant causes an effort on the part of interpreter e.g. muscular effort due to the command to ground arms. Logical interpretant is of general nature and is the effect produced by intellectual concepts (which is also a general), in other words, another proposition, which signifies, what is signified by the original sign. In that case it can be applied the pragmatic maxim, as I am going to show.

The final interpretant is *'that which would be finally decided to be the true interpretation if consideration of the matter were carried so far that an ultimate opinion were reached' (CP 8.184)*. This interpretant refers to the effect that the sign would have on the mind of scientific community, if it were allowed to investigate successfully and indefinitely, as we are going to see in detail in the relevant Chapter. What remains now to be shown is how the three Categories and the interpretation of the signs can lead to the pragmatic meaning of ideas.

2.1.5. Pragmatic meaning of ideas-Pragmatic maxim

Peirce's inquiry into the meaning of ideas or 'abstract terms' (which he classifies under the intellectual *signs*) begins with the rejection of the two traditional methods of clarifying the meaning of ideas, so to say: (i) familiarity and (ii) definition. The first method (familiarity) fails to clarify the meaning of ideas, because it is not precise and leads to infinite regress. The second method (definition), which consists in giving a definition of a universal term, can lead to enumerating all the universal predicates of a term, each of which is more abstracted and general than the term defined. Besides, this process can give rise to skepticism, as it can go endlessly (infinite regress), unless we stop at ideas such as Pure Being, Substance, Agency etc. For instance, if we attempt to define the term *'man'*, we can first define it as *'a rational animal'*, but then we have to give the definition of 'rational animal', let's say *'a part of the class of sensitive living things'*, which in turn can be defined as *'a part of the class of animate bodies'*, and then we can further define it as *'a part of the class of corporeal substances'*, and so on. As Peirce characteristically puts it:

> '(One word) will be defined by other words, and they by still others, without any real conception ever reached' (CP 5.423).

To put an end to this regress, Peirce introduces the ***pragmatic meaning*** of ideas, which is referring to their ***practical consideration***. This means that since the essence of anything is the sum of habits that involves (*Thirdness*) and it is immediately perceived in our experience, then the meaning of the ideas can be translated in terms of concrete experience and action. In other words, the meaning of a *sign* lies in considering its practical consequences, or else, if one exerts a certain action of volition or activity will undergo in return certain inevitable perceptions. For example, from the proposition 'honey is sweet' through the form of inference, which Peirce calls *hypostatic abstraction*[40]; we can deduce the conclusion 'honey possesses sweetness'. But as 'sweetness' can be applied to many things (sweet fruit, sweet chocolate etc.), it is a *universal* concept of quality; therefore it is real, as all the sweet things share it in common. The proper way to discover what the abstract concept (*symbol*) 'sweetness' means, is to translate it into a certain mode of action, e.g. 'if I put some honey on my tongue, I will experience that is sweet'. Or another example, 'diamond is hard', means 'diamond possesses hardness', and the practical consideration of the *symbol* 'hardness' is: 'if I press a knife-edge against it I will experience resistance'. To use Peirce's terminology, the *dynamical, logical interpretant* of the *sign* is the *pragmatic meaning* of the idea that the sign represents. In sum, the ***practical consideration*** of a general quality (heaviness, hardness etc.) consists in the premise:

'If I conduct myself in a manner x, I will have experience y'

Practical consideration represents a relation between volition (action with deliberation) and perception; therefore the meaning of the conception of a quality or its *rational purport* (CP 5.412) lies in its conceived effects. Peirce defines the meaning of ideas and the practical consideration, which he calls ***pragmatic maxim***, as follows:

> 'In order to ascertain the meaning of an intellectual conception, one should consider what practical consequences might conceivably result by necessity from the truth of that conception; and the sum of this consequences will constitute the entire **meaning of the conception**'[41] (CP 5.9. 1905).

> 'All reasoning turn upon the idea that if one exerts certain kind of volition, one will undergo certain compulsory perceptions. Now this sort of consideration, namely that

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[40] See CP 4.549: '*They (universals) are not discovered but rather produced by the abstractive process (which is called hypostatic abstraction in 4.463) - that wonderful operation … by which we seem to create entia ratonis that are nevertheless sometimes real*' (parentheses mine).

[41] See also *What Pragmatism Is*, (CP 5.430. 1905)*: 'Consider what effects that might conceivably have practical bearings you conceive the object of your conception to have. Then your conception of those effects is the whole of your conception of the object'.*

> certain lines of conduct will entail certain kinds of inevitable experiences, is what is called **practical consideration**' (Ibid; bold letters mine).

This process of discovering the meaning of abstract concepts through their practical consideration can be applied only to the certain kind of *signs*, or else to *symbols*, but not to *icons* that involve qualities of feelings (*Firstness*) or *indices* that involve *Secondness*. Because only *symbols* (intellectual concepts) involve *Thirdness*, which means disposition or behavior under certain conditions, therefore they are pragmatically interpretable. This is the crucial point that distinguishes Charles Peirce from another pragmatist William James[42], who held that the meaning of *all* ideas can be pragmatically interpreted[43]. This difference was one among the many reasons that prompted Peirce later in 1905 to change the name of his philosophical school from '*pragmatism*' to '*pragmaticism*' (CP 5.414), because, as he said, this word '*is ugly enough to be safe from kidnappers'*.

Furthermore, since *universals* are real, exist in things as *habits* or dispositions or behaviors (potentialities), therefore their meaning can be translated pragmatically into terms, which refer to concrete perceptual occasions. For the critical scholastic realist Peirce, as we have seen, the *universals*, though conditional, are real, as dispositions, existing potentially in the particulars, and they are not mental constructions, as nominalists argue.

> 'The property, the character, the predicate, hardness, is not invented by men, as the word is, but is really and truly in the hard things, and is one in them all, as a description of habit, disposition or behavior' (CP 1.27. 1909).

However, for Peirce, are not all generals (*universals)* real, as the scholastics used to believe, but only the ones which in the course of inquiry will be experimentally verified and in the

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[42] Peirce cites for his disagreement the following paragraph written by William James in the entry '*Pragmatic and Pragmatism*' in (1902) *Baldwin's Dictionary*: *'The whole meaning of a concept expresses itself either in the shape of conduct to be recommended or of experience to be expected'*.

[43] Peirce's comments on that in 1907: *'I understand pragmatism to be a method of ascertaining the meaning not of all ideas, but only of what I call intellectual concepts',* and he further declares that pragmatism has nothing to do with qualities of feeling and existential facts, but rather with intellectual concepts: *'Intellectual concepts, however – the only sign-burdens that are properly denominated concepts – essentially carry some implication concerning the general behavior either of some conscious being or of some inanimate object, and so convey more , not merely than any feeling, but more too, than any existential facts, namely the 'would-acts' of habitual behavior' (MS 318 Pragmatism).* For Peirce, pragmatism aims at ascertaining the meaning of ideas and not determining the truth of the things, since no collection of events can ever completely fill up the meaning of a 'would be'. But what can only be affirmed is that under all conceivable circumstances the subject of the predicate 'would behave' in a certain way. Therefore he says: '*The whole meaning of an intellectual predicate is that certain kind of events would happen under certain kinds of existential circumstances' (Ibid).*

long run[44]. Since the meaning of the *universals* cannot be totally exhausted by any multitude, therefore Peirce introduces the fallibilistic aspect of scientific inquiry and definition, which is cumulative, progressive and self-corrective, because always something new is added on the definition of universals. For example, where Peirce gives the practical definition of the symbol Lithium, which consists of all its physical and chemical properties[45], nowadays we can add some more features due to the development of Chemistry i.e. it consists of 3 electrons and each electron has such quantum number, when it is bombarded by high speed particles, will be given the emission in such frequency etc. We may infer, then, that the definition of Lithium will include some more features in the future, which are now unknown to us.

In order to establish the inferential relation between action and perceived experience Peirce uses the analogue of relation between antecedent and consequent. As he argues, in each syllogism there is an antecedent premise, a consequent premise and the logical consequence, which is the assertion that the consequent follows from the antecedent, in other words the consequence is the relation between them (CP 4.45). By analogue *practical consideration* is a relation between action and perceived experience:

> *'if I conduct myself in manner x, then I will experience y'* (1)

This *practical consideration,* as I am going to show, Peirce proposes in the deductive phase of scientific inquiry by drawing the practical consequences of a hypothesis, as well as in the inductive verification of a hypothesis. Because from the inversion of the proposition (1) one can easily infer the following:

> '*if I want to have experience y, then I will conduct myself in manner x*'

This means that after abduction and the generation of a hypothesis, one can guess what sort of experiments should conduct, in order to discover whether his hypothesis is true.

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[44] See (CP 5.430. 1905), *What Pragmatism Is*, where Peirce discusses the difference between Pragmatism and scholastic realism and argues: *'Of course, nobody ever thought that all generals were real; but the scholastics used to assume that generals were real when they had hardly any, or quite no, experiential evidence to support their assumption; and their fault lay just there, and not in holding that generals could be real*'.

[45] See CP 2.330, where Peirce gives the detailed practical definition of the Lithium.

Finally, apart from the cognitive aspect of the pragmatic meaning of ideas, which consists in their practical consideration, there is also a purposive aspect involved, as one has in mind the purpose of acting in a certain way and attaining a certain experience. This purposive aspect that involves volition (action with deliberation) Peirce calls ***intellectual purport***, and it is inseparable element of the pragmatic meaning of ideas. According to Peirce's definition of his new theory, Pragmatism recognizes the inseparable connection between rational cognition and rational purpose (CP 5.412). Now I can proceed to explain Peirce's method of inquiry, which is based upon his explained epistemological and phenomenological notions.

2.2. Fixation of Belief: Methods of inquiry

Peirce's account of scientific inquiry is based upon his notion of belief and doubt, presented in his works *'The Fixation of Belief' (CP 5.358-87.1877)* and *'How to make our Ideas Clear' (CP 5.388-410.1878)*, as well as on his definition of scientific method, which was developed in his later works and completed with his Harvard Lectures on Pragmatism (1903), and presented in the *Monist* in 1905 (*What Pragmatism Is, CP 5.411-37*)**.**

His inquiry about belief and doubt begins with the similarities and differences between them, and it is based mainly on psychological and biological accounts. By the use of the historical example of the Assassins[46] he argues that belief after continuous repetition becomes a habit and determines positively our action under certain conditions, therefore it is not passive modes of behavior but a dynamical force that is manifested in behavior. From this point of view, he compares belief to a certain form of behavior produced by repeated responses of an organism to stimuli of a determinate kind or to the habit of producing mouth water by the nerves, e.g. when smelling a peach, and this law depends on the properties of protoplasm, which reacts in such a way to remove the stimulus (CP 5.563). He also adopts the definition of belief given by Alexander Bain[47] ('*that upon which a man is prepared to act*'); therefore he regards him as the grandfather of pragmatism[48].

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[46] See *'The Fixation of Belief' (CP 5.371.1877), III: 'The Assassins or the followers of the Old Man of the Mountain (Sheik al Jebal), used to rush into death at his least command because they believed that obedience to him would insure everlasting felicity'.*

[47] Alexander Bain, (1818-1903), Scottish philosopher and psychologist, author of the work *The Emotions and the Will*, Parker and Son, London, 1859.

[48] See CP 5.11-13. 1907, *Pragmatism*, where Peirce refers with approval to the same definition of belief given in 1877, therefore I consider this definition permanent part of his doctrine.

> 'A genuine belief or opinion is something on which a man is prepared to act, and is therefore in a general sense a habit' (CP 2.148).

However, this definition of belief in behavioral terms, in my view, cannot include our beliefs about the past or the future, or even beliefs that are very distant from us in space, which may never give rise to action. On the other hand, if we consider the case of different persons that share a common belief, e.g. in the imminence of war, it does not follow that they will react in the same way, unless we associate those reactions with the whole corpus of one's beliefs. Peirce probably being aware of these difficulties, in his later works he admitted that his psychological account of belief was not sufficient to explain its nature, and he associated belief more with the act of judgment, therefore he stated: *'I do not think is satisfactory to reduce such fundamental things to facts of psychology' (The Maxim of Pragmatism, CP 5.28.1903), 'The question of the nature of belief or in other words the question of what the true logical analysis of the act of judgment is ...' (Ibid).* This means that, for the later Peirce, belief is an active attitude of mind towards a proposition and includes the association with the whole corpus of one's beliefs. In other words, when someone believes in something he does not only behave, but he also thinks in a certain way, that is, he has a disposition to expect from the events such and such experiences (expectation of certain predictions). In this sense, active belief is not only actual but also potential towards a certain imaginative event. It is based on logical analysis, involves volition and leads to the disposition to act (even if it is not actualized) both physically and mentally towards proposition in a certain way and under certain conditions[49]. This disposition according to Peirce belongs ontologically to the general principle of all things to conform to certain natural *laws* or *habits*, as explained in the Chapter of *Thirdness.*

Concerning these dispositions or habits, what distinguishes men from other beings, is men's ability of becoming conscious of their habits. This conscious habit is what Peirce calls *belief*[50]. To be more precise, when taking a proposition true or false, we are motivated and prepared to act in a certain manner according to it, and we repeat this action until it becomes a habit. This implies that belief involves an intentional aspect and it is

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[49] With my interpretation agrees also Peirce's definition of belief in his later work '*A Neglected Argument for the Reality of Good*', where he clarifies its meaning more, as follows: '*Now to be deliberately and thoroughly prepared to shape one's conduct into conformity with a proposition is neither more nor less than the state of mind called Believing that proposition' (CP. 6.467.1908).*

[50] See CP 4.53:*'Belief is a habit of which we are conscious'.*

distinguished from other internal representations that control the reflexes. E.g. there is a difference between closing our eyes as a reflexive response to sudden movement and closing our eyes purposively, because we decide to sleep. Hence, belief in the Peircean sense (conscious) involves reason for action or certain expectations from the external world (e.g. we expect that the sun will rise every morning). This notion is applied also to the pragmatic meaning of ideas, because if we have a belief *B*, then we habitually expect the consequences or the predictions we derive from *B* to come about, when the appropriate occasion arise. Psychologically belief provides us with a feeling of calmness and satisfaction, therefore it is a desirable state of mind; for example we believe in certain moral laws and we feel calm when we act in accordance with them.

Doubt on the contrary, as Peirce argues, is an uneasy and dissatisfied state, from which we struggle to free ourselves and pass to state of belief, therefore it stimulates us to action until is destroyed. Sometimes due to some surprising facts from the external world one is forced to block his habit, therefore also his belief, and this blocking is the state of *doubt*. Experience in this context can conflict with certain beliefs or with some part of one's whole corpus of beliefs. This does not imply that only experience justifies beliefs, since some beliefs can justify other beliefs, but simply that some beliefs resign in the face of experience, as Peirce puts it:

> 'Doubt, then, is a privation of habit, arising from the surprise of or shock which comes with a novel environment' (CP 5.417, 5.512).

This definition of doubt given by Peirce refers only to the interruption of habit, but not to the state of not having a habit or belief about something. Moreover, neither concerns it with the skeptic attitude of suspending judgment about a proposition nor with the consciousness of being ignorant ('calm ignorance', as it is marked in MS 334). Doubt in the Peircean sense arises when the expectations or predictions of a belief are not fulfilled by experience[51]. This involves the discovery of unexpected fact, contrary opinions of others, who had a relevant experience, some novel way of reasoning about the observations and potential experiences we can imagine noticing the consequences of a certain belief[52]. Doubt is accompanied with dissatisfaction that forces us to struggle for re-establishing and fixing

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[51] We can compare this notion with the Stoic principle of serenity (*απάθεια*: apatheia), which aims at the state that one does not fail in his expectations, namely in his desires, and he does not fall into what he wants to avoid. See Gill (1995), Epictetus, Discourses B, 8, 27-29.

[52] See CP 4.77, 5.373, 7.58.

new beliefs, which will secure us against future surprises and disappointments (CP 2.173). This struggle whose aim is the cessation of doubt and the settlement of stable opinion (cohered with experience) Peirce calls *inquiry*. In other words, we struggle to get an answer to a particular question with which our particular inquiry is busied.

Since the account of the state of 'dissatisfaction' implies psychological and hedonistic aspects, Peirce was criticized for this notion by his contemporaries. His response to that is: '*the aim or the result of action (inquiry) is considered to be satisfactory when is congruous to the aim of that action*' (CP 5.560.1906), which means that satisfaction is related to the aim of inquiry. The aim of inquiry for settled beliefs is to get beliefs that respond to and cohere with experience, for experience is compulsive and brute, as I explained in the Section of *Secondness*, and it constrains beliefs, while belief that conflicts with recalcitrant experience resigns. Therefore this notion has nothing to do with '*cognitive hedonism*', because the aim of inquiry is the permanently settled beliefs, which are fixed by *'an external permanency'* and cohere with experience. In this sense inquiry is satisfactory, as long as it provides us with these beliefs[53]. To return to our discussion, inquiry begins with surprising fact under the influence of the forceful element of experience, as Peirce says:

> 'As for this experience, under the influence of which beliefs are formed, what is that? It is nothing but the forceful element in the course of life. Whatever it is … in our history that wears out our attempts to resist it, that is experience' (MS 408, p. 147).

From this point of view, human inquiry is an infinite process between momentary states of doubt and belief, for every belief that is inconsistent with experience is a starting point for a new doubt and the doubt the starting place for a new thought, which leads to the fixation of a new belief, as follows:

Belief – surprise – doubt – inquiry – new belief – new surprise – doubt – inquiry **-** *...*

According to Peirce's definition, each surprise involves a new discovery arising both from a new observational resource and from some novel way of reasoning about the observations (CP 1.109). Peirce describes this process as follows:

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[53] Hilary Putnam in Conant and Żegleń (2002), p. 60, also argues: *'the interest that drives scientific inquiry is identified with the interest in having one's beliefs fixed by 'an external permanency', by 'nothing human'. In short, it is the aims of pure science (which are sui generis, in referring to the indefinitely long run) that Peirce has in mind'.*

> 'As it appeases the irritation of doubt, which is a motive for thinking, thought relaxes, and comes to rest for a moment when belief is reached. But since belief is a rule for action, the application of which involves further doubt and further thought, at the same time that it is the stopping place, it is also a starting place for thought' (CP 5.397).

Generally there are four ways of establishing beliefs, according to Peirce: 1. *Method of tenacity*, 2. *Method of authority*, 3. *Metaphysical or a priori method,* and 4. *Scientific method*. Peirce characterizes the *method of tenacity* as 'a means of escaping the annoyance of doubt, due to its consolatory character, since those who follow it, cling tenaciously to doctrines taught them at their mother's knee and turn with contempt and hatred from anything that might disturb them'. In other words, this method involves traditional and emotional aspects, since many traditional beliefs in some isolated communities have successfully worked (cohered with experience), even though they were false beliefs. Certainly, in the long run this method does not work, as doubt sparks when one notices that the opinion of others differs from his own, in other words, the social impulse is against it (CP 5.378).

The *method of authority* produced and imposed by an authority (religious institution or totalitarian state) is characterized by universal and catholic character. Although it involves a social aspect (imposed by the community), it is subject to doubt, i.e. when one notices that those in other states, other centuries or religions have believed different things. Because no institution can undertake to regulate opinions on every subject, and even in the most tyrannical of states individuals will arise, who are emancipated from official dogma.

More sophisticated and respectable is the *a priori method* adopted by the metaphysicians, which consists in the establishment of a set of propositions that are *'agreeable to reason'*. Peirce provides as an example for that Descartes' method of doubt, and he argues that this method of doubt for the sake of attaining knowledge by virtue of logical propositions is not a legitimate and genuine method of inquiry. Because it lacks the frustrating quality of uneasiness, it does not involve the aspect of a disposition to act in a certain way, and it does not have as criterion the coherence with experience. The weakness of the *a priori* method, for Peirce, are: 1. It is infected with subjectivity, as it involves no appeal to an external reality (experience), therefore from the earliest times to the latest the pendulum has swung back and forth between the extremes of monism and pluralism, idealism and realism, depending on the predilection of the particular experts of metaphysics, and 2. Instead of becoming steadily progressive and cooperative enterprise, it has been from the outset a series of controversies among rival metaphysicians (CP 5.358-87.1877).

The use of Descartes' method of doubt as a representative of the metaphysical *a priori method* is, in my view, unsuccessful, because it fails to exhibit the weakness of this method and, apart from that, it misinterprets Descartes' method of doubt. First, Descartes does not neglect the importance of sensual experience, as we can read in the end of his *Meditations on First Philosophy:*

> 'And I ought not to have even the slightest doubt of their reality if after calling upon all the senses as well as my memory and my intellect in order to check them'[54].

Therefore, one cannot regard Descartes as a typical representative philosopher of the metaphysical school that neglects the reality of the external experience, which is the crucial aspect of the *a priori method* that Peirce wants to emphasize. Second, as Susan Haack argues[55], Peirce misrepresents Descartes' method of doubt, since his method does not involve deliberate doubt, but deliberate suspension of beliefs found to be objectively dubitable. And the latter view is the one that Peirce also adopts, when claiming that his Critical Common-sensism requires a policy of submitting beliefs to critical scrutiny and severe test.

Against the previous mentioned three methods Peirce proposes the *scientific method*, which *'is found by beliefs determined by nothing human, but some external reality, by something upon which our thinking has no effect… But which on the other hand unceasingly tends to influence human thought' (CP 5.384).* In this previous sentence, Peirce gives the realistic definition of scientific method, which is based on evidence of experience. The definition presupposes the hypothesis that there exists prior and apart from the investigating scientist an objective order of nature (*reals*) that affects our senses and stimulates our thought, as I have shown in the Section of *Thirdness*:

> 'There are real things, whose character is entirely independent of our opinions about them; those reals affect our senses according to regular laws, and though our sensations

[54] See also Descartes, *Discourse on the Method,* Part 6, in Gottingham et al. (1985): '*For as experience makes most of these effects quite certain, the causes from which I deduce them serve not so much to prove them as to explain them ; indeed, quite to the contrary, it is the causes which are proved by the effects'.*

[55] See Susan Haack, *The First Rule of Reason*, in Brunning Jacqueline and Forster Paul (1997), *The Rule of Reason; The Philosophy of Charles Sanders Peirce*, pp 241-262.

> are as different as our relations to the objects, yet by taking advantage of the laws of perception, we can ascertain by reasoning how things really and truly are, and any man, if he have sufficient experience and reason enough about it, it will be led to the one true conclusion' (CP 3.254. 1877).

Since this definition of 'real things' might refer to particulars, and not to universals, therefore Peirce returned to the same definition in his later works in order to clarify it; because his notion about the real world was also developed together with his semiotics. While in his early works uses the expression 'real things' for the external reality, in his later writings he is always referring to the 'reals' in terms of universals, that is, conditional principles in nature in a form of general laws, dispositions or habits of things of the same class to behave in a certain way under certain conditions. We can compare the above mentioned definition with the followings, where it is clarified that the 'real' is referring to the predicate. When in different times of his later writings Peirce claims to be a scholastic realist, he states:

> 'Anybody may happen to opine that "the" is a real English world; but that will not constitute him a realist. But if he thinks that the word "hard" itself be real or not, <u>the property, the character, the predicate "hardness"</u> is not invented by men, as the word is, but is really and truly in the hard things, and is in one in them are, as a description of habit, disposition or behavior, then he is a realist' (CP 1.27. 1901; underline mine). 'What is meant by calling anything real? ... Any objects whose attributes, i.e. <u>all that may truly predicated or asserted of it</u>, will and always would, remain exactly what they are, unchanged, though you or I or any man or men should think or should have thought as variously as you please, I term external, in contradistinction to mental' (CP 6.327. 1908; underline mine).

Scientific method, for Peirce, as being the outcome of the 'intellectual development of mankind from the most primitive (represented by the method of tenacity), through the method of authority to the a priori method' (CP 5.564), it involves all the positive aspects entailed in the previous three methods of inquiry, plus the *pragmatic maxim* and *fallibilism.* Since it is based on evidence of experience, therefore it provides the means for pursuing the truth. This conception of truth is considered to be here as 'forced upon the mind in experience as the effect of an independent reality'. In other words, scientific method entails the aspect of satisfaction of the first method, the social aspect of the second one and the aspect of reasoning of the third one. Scientific method is a persistent pursuit of truth, for as Peirce states: *'scientific method consists in diligent inquiry into truth for truth sake's without any sort of axe to grind' (CP 1.44*). By truth Peirce means 'the unassailable by doubt belief' or *'opinion which <u>will</u> be ultimately agreed by all who investigate' (CP 5.388-*

410.1878), because the belief is temporarily settled and it will be always motivated to further inquiry. However, in his later work Peirce gave more cautious and complete definition of truth[56], as follows: truth may be described as *'that concordance of an abstract statement with the ideal limit towards which endless investigation would tend to bring scientific belief' (CP 5.565).* We can notice the change of Peirce's notion from *'which … will be ultimately agreed'* to *'which endless investigation would tend to bring*, which indicates that truth, for the late Peirce, is rather the potential product of an idealized inquiry.

With regard to the starting point of scientific inquiry, Peirce holds that it begins with some fundamental and indubitable beliefs adopted by the individual as a result of his experience, which are not simple sense impressions, since there is no clear distinction between sense impressions and their interpretations in the perceptual judgments[57], as we have seen. They serve as basis from which one can 'set out', as Peirce states:

> 'The very state of mind in which you find yourself at the time you do set out, a state in which you are laden with an immense mass of cognition already formed, of which you cannot divest yourself if you would' (CP 5.416).

Those indubitable beliefs e.g. propositions such as 'the fire burns', 'there is an element of order in the universe' etc., represent the accumulated wisdom of the race, therefore they can be also called 'instinctive beliefs', since they are the result of biological and social adjustments. *'That is to say they rest on … the total everyday experience of many generations of multitudinous populations' (CP 5.522).*

Those beliefs include the whole corpus of opinions about physical nature, human society and man presented by the Scottish philosophy of Common-sense, like Thomas Reid and Stewart. *'A man may say 'I will content myself with common sense' I, for one, am with him there in the main' (CP 1.129).* However, as Peirce analyzes in his *Issues of Pragmatism*[58], what distinguishes a Critical Common-sensist like him from the old Scottish philosophers of Common-sense is the recognition of evolution, which implies that some indubitable

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[56] John Dewey in (1938), *Logic: The Theory of Inquiry,* p. 345, characterizes this definition as *'one of the best definitions of truth from the logical point of view'.*

[57] Peirce also in *What Pragmatism Is* (CP 5.416.1905), points out: *'Another proposes that we should begin by observing the 'first impressions of sense', forgetting that our very percepts are the results of cognitive elaboration'.*

[58] See CP 5.438-63 or *The Monist* 15, October 1905, 481-99, *The Consequences of Pragmaticism,* where Peirce argues that the consequences of his pragmatism (pragmaticism) are the critical common-sensism, which is distinguished from the old Scottish common-sensism and the scholastic realism.

beliefs in the course of evolution have been changed and they are no longer indubitable[59]. The second difference is the notion that those indubitable beliefs are subject to criticism and change, since in the process of inquiry these vague premises will be replaced by scientific hypotheses, which in turn are subject to verification or falsification by means of observation and experiment. Therefore in the process of inquiry indubitable beliefs serve as initial 'vague premises, which need to be constantly subjected to scrutiny' (CP 5.515-7).

Peirce rejects Descartes' method of doubting all one's beliefs at once and accepting just those, which are certified by rational inquiry, as well as the Aristotelian view that the inquiry begins with some first principles; it is true that inquiry begins with some initial beliefs, he says, but these are subject to further criticism. As for Descartes' method, he argues that it is impossible to pursue the method of universal doubt, for no *'genuine doubt can be created by a mere effort of will, but must be compassed through experience' (CP 5.498)*, therefore *all* our beliefs can never be challenged *at once* by any experience. Apart from that, as he says, the Cartesian sort of doubt is formal, but not living and real, therefore leads to self-deception. Therefore he concludes: *'Let us not doubt in philosophy what we do not doubt in our hearts' (CP 5.265)*, because there is no genuine doubt if there is no surprise.

With regard to the Aristotelian view that the inquiry begins with some first principles, which are 'self-evident truths'[60], Peirce argues that there has never been in the history any agreement on these truths. Moreover, such cognitions if existed would have to be known either immediately by a kind of intuition, or mediately by inference (CP 5.213). In the first case, since each cognition requires time in which to take place, no cognition can arise immediately. While in the second case, since no cognition not determined by a previous cognition can be known, therefore there are no cognitions inconceivable.

Except for these aspects, scientific inquiry involves the *pragmatic maxim,* which presupposes that the meaning of any proposition lies in its conceivable practical effects, which its assertion would imply. *'Certain lines of conduct will entail certain kinds of inevitable experiences' (CP 5.9. 1907).* The consequence of that notion is that the conceivable effects should involve *perceptual facts and reasoning,* therefore scientific inquiry is based on the facts of experience, as the observation data are the source of our

[59] Peirce quotes as an example here the belief in Adam and Eve in comparison with Darwin's theory.
[60] See more Aristotle's *Posterior Analytics*, Book I, in Barnes J. (1998).

knowledge: *'The machinery of the mind can only transform knowledge, but never originate unless is fed with facts of observation (CP 5.392).* As I explained in the Section of *Secondness,* all of our knowledge comes from observation through the *percept*, which compels the perceiver to acknowledge it, therefore as Peirce states: *'we have excellent grounds for believing that the percept belongs to the real world' (CP 2.142).* Since the percept serves as a basis prediction and experiment in scientific inquiry, observation should not be passive but should rather involve great effort (CP 2.605). Apart from that, as I showed in the Section of *Thirdness*, the other component of perception is the *perceptual judgment*, which involves the interpretation of the percept in a form of abstract proposition and contributes to the generation of the hypothesis. The latter is subject to criticism and control through an act of conscious volition and self-controlled thought. But as experiment is the only self-controlled conduct in a future time[61]; therefore the hypothesis can be brought to the test of experiment for verification and further evaluation.

As for the inferential aspect of scientific inquiry, i.e. reasoning, is considered to be by Peirce the heart of inquiry and consists of three types: abduction, deduction and induction. I am going to discuss in detail the criteria and the methods of inference in the next Chapter, but here I simply sketch the general features of reasoning. First, according to Peirce, reasoning is a self-controlled and voluntary act; therefore it can be criticized and evaluated as valid or invalid: *'Reasoning is essentially a voluntary act over which we exercise control' (CP 2.44).* Second, the validity of all the forms of reasoning depends on the certain norms of logic, or else valid arguments of the three propositions of a syllogism (rule, case and conclusion). Third, all reasoning, as Peirce states, is conducted in signs; therefore *'Logic is the science of the general necessary laws of Signs and especially of Symbols' (CP 2.93).* Finally, since scientific inquiry embodies the three forms of reasoning, each one of them with its own features, therefore they are applied to different phases of inquiry. Abduction consists in studying facts and devising a hypothesis to explain them, deduction draws the consequences of a hypothesis, and induction tests it experimentally in order to measure the concordance of a hypothesis with the fact (CP 5.143. 1903).

[61] See *What Pragmatism Is,* (CP 5.427.1905): *'It is, according to the pragmaticist, that form in which the proposition becomes applicable to human conduct, not in these or those special circumstances, nor when one entertains this or that special design, but that form which is most directly applicable to self-control under every situation, and to every purpose. This is why he locates the meaning in future time; for future conduct is the only conduct that is subject to self-control …For an experimental phenomenon is the fact asserted by the proposition that action of a certain description will have a certain kind of experimental result; and experimental results are the only results that can affect human conduct'.*

The consequence of this third form of inference (induction) is the experimental test of a hypothesis that involves observation and experiment. After the removal of belief and the establishment of a new belief in a form of hypothesis, the verification process should determine the degree of probable truth of this initial hypothesis. Since the belief is of nature of habit, then it should be the result of a rule of action, according to the pragmatic maxim, which means an experiment that tests the fulfillment of the predicted consequences of the hypothesis (CP 5.170). In this inductive phase the inquirer sets up his experiment in such a way (action) so as to force nature to answer his question, while nature reacts in a determinate way and produces the verification (affirmation or denial) of the hypothesis through the force of facts. Peirce describes as follows the essential ingredients of an experiment:

> 'First, of course, an experimenter of flesh and blood. Second, a verifiable hypothesis. This is a proposition relating to the universe environing the experimenter, or to some well-known part of it and affirming or denying of this only some experimental possibility or impossibility… Passing over several ingredients on which we need not dwell, the plan and the resolve, we come to the act of choice by which the experimenter singles out certain identifiable objects to be operated upon. The next is the external (or quasi external) Act by which he modifies those objects. Next comes the subsequent Reaction of the world upon the experimenter in a perception; and finally his recognition of the teaching of the experiment' (What Pragmatism Is, CP 5.424. 1905).

The *pragmatic maxim*, however, is not a narrow form of verificationism identified later with the Vienna Positivists, who by interpreting extremely Comte's positivist notion[62] were to argue that factual assertions about abstract entities are meaningless. On the other hand, as R. Almeder argues[63], for Peirce, there are a host of propositions that are meaningful, since they involve *emotional* and *energetic interpretants* (imperatives, interrogatives, explicatives). Furthermore, pragmatic maxim is rather a criterion necessary for the purpose of theoretical science, since the aim of scientific inquiry is to predict in theoretical and diagrammatic context, as well as to explain why its predictions are fulfilled. More about the verification process will be discussed in the next Chapter and in the inductive phase of scientific inquiry.

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[62] See (CP 5.178-79. 1903), *The Nature of Meaning*: '*The true maxim of abduction is that which August Comte endeavoured to formulate, when he said that any hypothesis might be admissible if and only if it was verifiable. Whatever Comte himself meant by verifiable, which is not very clear, it certainly ought not to be understood to mean verifiable by direct observation, since that would cut off all history as an inadmissible hypothesis'*.

[63] See Almeder, R. (1980), pp. 13-44.

As I said, the pragmatic maxim can be applied to the *logical interpretant* of a sign, because it expresses what the original proposition of the sign expresses, therefore what is expressed, is the properties or the effects of the proposition, as the object is governed by generality (law or habit). Thus our conception of the object that the sign represents is nothing but the effects (properties) of its proposition, which is general and indeterminate (universal); therefore it cannot be fully exhausted, and it is open to further specification, as explained in the previous Chapter. According to Peirce's doctrine of ***synechism***[64] (continuity), there is no existence of absolute individuals and what permanently exists is only general, for all things swim in the continuum of space and time[65], which is infinitely divisible; therefore individuals exist short and we cannot fully specify all their properties. If one claims that there are things that he has ascertained, he is committed to discontinuity, since according to continuity the exact ascertainment of real qualities is impossible. Even propositions of mathematics, which are supposed to be absolutely certain (analytic), strictly speaking, for Peirce, they are synthetic e.g. the sum of angles of a triangle is not equal two right angles in non Euclidean geometry (CP 7.568).

> 'The absolute individual cannot only be realized in sense or thought, but it cannot exist properly speaking. For whatever lasts for any time, however short, is capable of logical division, because in that time it will undergo some change in its relations. But what does not exist at any time, however short, does not exist at all. All therefore, that we perceive or think, or that exists is general' (CP 3.93).

Another aspect that establishes the doctrine of *synechism* is that, according to Peirce's evolutionary cosmology, the objects of perception that the sign supposes to represent are also subject to evolution, which means to continuous change; therefore we cannot fully specify their properties. Peirce's fallibilism of scientific inquiry based on the doctrine of synechism says that the conclusions of scientific method are provisional and susceptible of further refinement or correction, since inquiry is continuous. Fallibilism, for Peirce, affirms that *'every proposition, which we can be entitled to make about the real world must be an approximate one" (CP 1.404)*, because as we have seen, all signs are characterized by generality *'No cognition and no sign is absolutely precise, not even a percept' (CP 5.543*). On the other hand, the number of potentially confirming instances of a proposition being

[64] See CP. 7.565 -78. The word comes from the Greek word for continuity, *συνέχεια: synecheia*.

[65] Peirce's doctrine of synechism here meets Heraclitus' notion of the eternal change: *'All things are in flux'* See Kirk et al. (1983), pp.181-212, Bakalis (2005), pp. 26-31.

true is infinite. Therefore, we can never be certain that a proposition is in fact true, but the most we can say is that the probability of the proposition's being true approximates the probability value of 1, as a mathematical limit with each confirming instance[66] (CP 2.729, 1.720).

Granted fallibilism, scientific method, as method of fixing beliefs, should involve a community of scientists, who observe the same phenomenon by using different instruments and methods of observation tend to arrive at the same conclusion. In this context the aim of scientific inquiry is not only to get 'firm' beliefs coherent with experience, but also beliefs that would be agreed upon by the community of inquirers, if inquiry were to be pursued sufficiently far. This last definition of 'true beliefs' is the one that Peirce adopts. In other words, the necessary condition of a hypothesis being true is that it would be believed at the end of inquiry, if inquiry were to be pursued 'sufficiently far'. Certainly this 'sufficiently far' does not mean here the infinite long run inquiry, but only the step of inquiry until the fixation of the next stable belief. On the other hand the infinite long run inquiry concerns the final idealized opinion, which would be agreed upon by the scientific community.

Objections by some philosophers to this account suggest that there is no guarantee that the final opinion will ever be reached (Royce[67]) or as Bertrand Russell argues, following Peirce's account, the beliefs of the last people on earth will be true beliefs. Peirce argues against Royce that the 'would be' in his construal of truth is readily resolved into hope for a 'will be' (CP 8.113), and by the use of the analogue of the diamond he argues: *'Just as a diamond that sits on the ocean floor destined never to be touched is hard, a belief which would have been in the final opinion, despite the final opinion never coming to pass, is true'*. In other words, a belief, which is 'potentially' agreed upon by the community of inquirers, though never being 'actualized', is a *true belief*, that is to say the *final interpretant*. With regard to Russell's argument my objection is that in case of imaginative nuclear holocaust there would have been no final true belief.

As explained, according to Peirce's ontological definition of the 'reals', they exist independently of our opinions about them and affect our senses according to the laws of perception. While their true interpretation by the inquirer (epistemological definition, in

[66] More about probabilities will be explained later.

[67] Royce in his work: *The World and the Individua*l, does not mention Peirce by name, but he argues against Peirce's account of the end of inquiry.

relation to its interpretant), granted fallibilism, is the opinion about them defined as an ideal limit, which the scientific community will approximate gradually and inevitably, if the inquiry is carried out ad infinitum. In this sense, the road of inquiry is never blocked, and the definition of the true belief is approximate one close to the mathematical limit of the probability value 1, with each confirming instance, and where the 'real' is. Because, on the one hand, if that opinion is final, the road of inquiry will be blocked[68], this is inconsistent with synechism. On the other hand, since the questions answerable by scientific method concerns universals and not particulars, if the definition of those questions (universals) is fixed, then the definition of the universals will be exhausted, which implies the falsity of infinite potentiality. However, in my view, the two definitions of the 'real' are trivial and contradictory, therefore all these aspects of the real, fallibilism and scientific community I will discuss later in detail in the relevant Chapters.

In sum, scientific method of fixing belief[69], according to Peirce, has the following features: 1. It is a persistent pursuit of truth, 2. It begins with some common basic beliefs, which are subject to further correction and are replaced by hypotheses, 3. All its data must be obtained by some form of observation, 4. The method dealing with these data is that of reasoning, 5. The conclusions of scientific method must be verifiable by observation and experiment 6. It is cooperative and social (public) venture, and 7. The conclusions of scientific method are fallible and open to further correction. What follows now is to examine the three modes of scientific inference, which are proposed by Peirce and can ensure its self-corrective character.

[68] Peirce characterizes in *The First Rule of Logic* (CP 5.574-89. 1898) this rule as the *First Rule of Reason* (FRR), namely: *'Do not block the road of inquiry'*, for *'we can be sure of nothing in science'* (Ibid); this ancient truth, as he says, was also taught in the Academy of Plato. The consequence of that fallibilistic view is both that there is no final opinion and that there are no things uknowable.

[69] As Peirce in his later works states, the aim of scientific inquiry is not to discover the metaphysical truth but to get beliefs unassailable by doubt: *'Your problems would be greatly simplified, if instead of saying that you want to know the Truth, you were simply to say that you want to attain a state of belief unassailable by doubt' (CP 5.416.1905, What Pragmatism Is*).

2.3. Logic (abduction, deduction and induction)

Peirce divides his logic into three branches: Theory of signs, classification of inference types and theory of method[70]. In the present Chapter I focus mainly on the theory of method, which Peirce calls ‘the theory of deliberate self-controlled search for truth’.

The ‘method of science’, as we have seen, consists of two parts: observation and reasoning (CP 8.41). Brute experience alone does not suffice for guiding us to the fixation of belief; therefore, although doubts are sparked by experience through observation, we need to make inference with regard to these experiences, so as to achieve the fixation of belief. Following Peirce’s account of truth and inquiry, the aim of inquiry is to get beliefs, which result in a ‘maximum of expectation’ and a ‘minimum of surprise’, which means secure beliefs. Peirce distinguishes three types of scientific inference: abduction, deduction and induction, as we are going to see in detail. While abduction generates beliefs, deduction and induction make those beliefs secure. Thus the aim of reasoning in scientific inquiry is to fix beliefs that cohere with experience, as we can see:

> ‘It is those facts I want to know, so that I may avoid disappointments and disasters. This … is my whole motive of reasoning. Plainly, then, I wish to reason in such a way that the facts shall not and cannot disappoint the promise of my reasoning’ (CP 2.172.1902).

Reasoning is the means to achieve the goal of inquiry, therefore it is deliberately and self-controlled, since if a rational inference is unconsciously, there is a great danger of error. It presupposes both a voluntary act - for approval or disapproval requires an act of volition - and self-controlled thought, so as to be able to give an account how one came to certain conclusion. It is obvious that there is a great analogy with the moral conduct, which also requires deliberation and self-controlled thought[71].

From this point of view, Peirce neglects the notion of ‘logical fatalism’, because the scientific inquiry is not destined to reach the truth, no matter how carefully or carelessly one observes. The self-corrective aspect of scientific inquiry, so to say, to reach a final opinion which represents approximately the regularities in nature, presupposes careful use of reliable scientific methods. Each one carries in his mind patterns of good and bad

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[70] In this classification he calls them respectively ‘Logical Syntax’ or ‘Speculative Grammar’, ‘Critic’ and ‘Methodeutic’. See CP 1.191.1903.

[71] See CP 7.458 undated and CP 5.108.1903.

reasoning, which are called *norms* or *habits*, as I have shown, therefore, taking into account that all inferences are governed by habit, we ought to seek good habits. As Ian Hacking says[72], Hume was right in thinking that inductive conclusions are a matter of habit and not susceptible to criticism; however Peirce holds that we can assess our habits, adopt good habits and recognize the leading principle of an inference. Our instinctive logic (perceptual judgment), although is better than a flipping coin concerning the probabilities, it cannot provide us with a true conclusion. In order to increase the probability of reaching a true conclusion, we have to reason according to the more developed logic. Given the doctrine of fallibilism, we cannot know when we have true premises and conclusions, but we can know when we have premises and conclusions, which cohere with the data and therefore they are reliable. Those methods, which are reliable and increase the possibility of reaching a true conclusion, are the methods of abduction, deduction and induction.

Deduction is ‘explicative’ (analytic) inference, whereas abduction and induction are ‘ampliative’ (synthetic) inferences. As Peirce argues, ampliative inference is the only kind that can introduce new ideas into our body of beliefs[73], but in his later works he concludes that abduction is the only inference that can introduce new ideas, ‘for induction does nothing but determines a value’ (CP 5. 171.1903).

Peirce explains that the method of **abduction** (which he also called retroduction or hypothesis) is an inversion of a deductive syllogism B**A**RB**A**R**A**[74]**,** (AAA, A: affirmative proposition), which is as follows:

Deduction

1. All the beans in this bag are white (rule or major premise) - All M are P
2. These beans are from this bag (case or minor premise)- S is M
3. These beans are white (result as conclusion)- S is P

While deduction goes the way from nr.1 to nr.3 and induction from nr.2, 3 to nr.1, abduction uses a hypothesis in the form of a question to explain observed facts by supposing it to be a case of a general rule, as follows:

[72] See Hacking I., *The Theory of Probable Inference*, in H. Mellor (1980), *Science Belief and Behavior*.
[73] See CP 6.475. 1908.
[74] See CP 2.619-2.623. 1878.

Abduction

All the beans in this bag are white (rule or prior proposition)

These beans are white (result)

Are these beans from this bag? (case as a conclusion)

Or in another form:

All M is P (rule)

S is P (result)

S is M (case)

In other words, the hypothesis aims both at explaining an unexpected event and classifying it to a known class of events, that is to say, it finds whether the observed event is a member of a certain known class. Thus abduction begins with a surprising fact (discovery) and turns to hypothesis, which is nothing but the dynamical (logical) *interpretant* of the *sign* of the surprising fact, as follows:

The surprising fact, C, is observed

But if A were true, C would be a matter of course

Hence, there is a reason to suspect that A is true (hypothesis)

(CP 5.189)

To make it clearer: let’s suppose we found some white beans among ten bags of beans mostly brown (Nr. 1 up to Nr. 10 are brown except Nr. 3), then by abduction we can infer:

The surprising fact (C) is observed (these beans are white)

But if ‘all the beans in the bag Nr. 3 are white’ were true

Hence, there is a reason to suspect that ‘these beans are from the bag Nr. 3’ (hypothesis as a conclusion)

Peirce thinks that whenever any observation **C** obtains, there will be some hypotheses, which entail **C** or make **C** probable. He considers consequence to be a relation between the antecedent and the consequent; therefore whenever a consequent is observed, it is justified to search for the antecedent in a form of hypothesis, which is the cause of the unexpected fact. The hypothetical syllogism infers antecedence from a consequence, which is presented to us in experience, because it is determined by something without the mind. This realistic interpretation implies that the antecedence-consequence relation exists independently of its representation, since it involves the aspect of Thirdness, as we have seen.

He adds that this hypothesis may render the observed facts necessary or highly probable. Other characteristics of the hypothesis are: it must be capable of being tested but not necessarily directly verified; therefore it can deal with unobserved facts that could be resulted from observed facts. It must be broad and inclusive, and when one has to choose between two hypotheses, begins with testing the one that is most readily refuted or verified, since the economy of money, time and energy is important for the inquirer. More about these characteristics and criteria will be discussed later.

With regard to the verifiability of hypotheses, as I said, Peirce does not adopt the extreme positivist position that only the empirically verified hypotheses have the higher status of reliability. He insists that only the premises, but not the conclusion, of a syllogism must be directly observable; since the premises consist of empirical data, but the explanation of the data need not to be empirical hypotheses. This allows us to infer hypotheses about the past (e.g. archaeology), about unobservable entities (e.g. molecules, electrons, particles, electromagnetic waves etc.) and metaphysical hypotheses, therefore he states:

> ‘Physicists do not confine themselves to a strict positivistic point of view. Students of heat are not deterred by the impossibility of directly observing molecules from considering and accepting the kinetic theory, students of light do not brand speculations on the luminiferous ether as metaphysical…All these are attempts to explain phenomenally given elements as products of deeper-lying entities’ (CP 8.60. 1891).

In the abductive phase of scientific inquiry (generation of a hypothesis) Peirce claims that the inquirers’ imagination, experience and skill play important role, since allow them to form the most fruitful hypotheses. Although there can be innumerable hypotheses, the skilful instinct of the scientists will allow them in some finite number of verifications to find out the true explanation. I shall return to this topic in the relevant Chapter in detail.

The **verification process** of a hypothesis is consisted of the deductive and the inductive phase of research. In the **deductive phase** the hypothesis must be tested by reason, by drawing its experimental consequences, which would follow from its true. Here the inquirer draws virtual predictions of possible experiments from his hypothesis, which is the so-called 'purpose' of deduction. This sort of deduction I call **'pragmatic deduction'**, for it is distinguished from the theoretical deduction e.g. in mathematics, where the consequences that follow from a hypothesis are pure theoretical. While, as I explained in the relevant Chapter, the 'pragmatic' meaning of an idea (*pragmatic maxim*) lies in its practical consideration, or else its practical consequences. For example: 'If this solid thing is a glass' (belongs to class of glasses), 'then it should have the observable characteristics x, y of the glasses'. This inference is represented in the following deductive syllogism:

> All glasses are hard (x) and breakable (y) (rule)
> If this thing is a glass (case)
> Then it should be hard (x) and breakable (y) (result)

The **inductive phase** that follows is the experimental testing of the hypothesis, so to say, the inquirer tests the observable characteristics of the event x, y, z, etc. and compares it with the observed characteristics of the class. Peirce suggests that the verification process of a hypothesis must consist of a large number of random samples, due to the probabilistic justification of induction. If the hypothesis is refuted, then the inquirer has to proceed with more experience to making a new abduction and modify the hypothesis. Afterwards the same process has to be repeated again, which means, drawing deductive experimental consequences of the new hypothesis and testing them inductively.

The probabilistic and fallibilistic aspect of Peirce's SCT is referring to both deductive and inductive phase of scientific enquiry. With regard to induction Peirce holds that there are two types of it: 'crude' and 'quantitative' induction. Crude form of induction is what Hume characterizes as induction, which is very weak sort of inference[75], according to Peirce, for similar reasons given by Hume[76]. However, 'quantitative' induction with statistical

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[75] See CP 2.756. 1905: '*The first and weakest kind of inductive reasoning is that which goes on the presumption that future experience as to the matter in hand will not be utterly at variance with all past experience… I promise to call such reasoning crude induction'.*

[76] David Hume in (1993), *Enquiry Concerning Human Understanding*, Section IV, *Sceptical doubts about the operations of the understanding*, expresses his objection to induction, as follows: '*However regular the course of things has been, that fact on its own doesn't prove that the future will also be regular. It's no use your claiming to have learned the nature of bodies from your past experience. Their secret nature, and consequently all their effects and influence, may change without any change in their sensible qualities. This happens sometimes with regard to some objects: Why couldn't it happen always with regard to all? What logic, what process of argument, secures you against this? You may say that I don't behave as though I had doubts about this; but that would reflect a misunderstanding of why I am raising these questions. When I am considering how to act, I am quite satisfied that the future will be like the past; but as a philosopher with an*

syllogisms and direct inference serve as the testing grounds for hypotheses. Crude induction is a generalization on the basis of a limited number of samples, as follows:

S is M (case)
S is P (result)
All M is P (rule)

While quantitative induction deals with statistical ratios, as follows:

S1, S2, S3, etc. are a numerous set taken at random from among the Ms
A certain proportion p of these Ss are Ps
Therefore the same proportion p of the Ms are Ps

Or else:

These beans have been taken at random from this bag (case)
3/4 of these beans are white (result)
Therefore 3/4 of the beans in the bag are white (conclusion)

Here one concludes from an observed relative frequency ($f = m/n$) in a randomly drawn sample a hypothesis about the relative frequency in the population (f: hypothetical limit of relative frequency). Therefore, the inquirer in the inductive phase of verification has to test a large number of samples. This means that induction does not lead to the truth, but, rather, it lends a probability ratio to its conclusion. The same probabilistic principle is attached to the probable deductive inference, where the rule or major premise is a statistical hypothesis but not a universal law, as follows:

The proportion *p* of the Ms are Ps
S1, S2, S3, etc. are a numerous set taken at random from among the Ms
Hence probably and approximately the proportion *p* of the Ss are Ps
(CP 2.701)

Or else:

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enquiring - *I won't say sceptical* - *turn of mind, I want to know what this confidence is based on. Nothing I have read, no research I have done, has yet been able to remove my difficulty'.*

3/4 of the beans in the bag are white

These beans have been taken at random from this bag

Hence probably and approximately 3/4 of these beans are white

These two forms of inference, probable induction and deduction, both depend upon the same principle of equality of ratios, so that their validity is the same[77].

> 'Now that is not the way in which induction leads to the truth. It lends no definite probability to its conclusion. It is nonsense to talk of the probability of the law, as we could pick universes out of a grab-bag and find out to what proportion of them the law held good.' (CP 2.780.1901).

As we can see, for Peirce, it is quite clear that probability measures attach to leading principles and success rates (*f*) to inferences, but assigning a probability to a hypothesis or a theory is nonsense.

Scholars disagree about the definitions of induction and abduction given by Peirce in different stages of his life. In his early writings he holds that induction '*if duly persisted in, must in the very nature of things, leads to a result indefinitely approximating to the truth in the long run' (CP 2.281)*, and it is probable only in the sense that it is successful approximate process: *'The ratio (which is its conclusion) may be wrong because the inference is based on but a limited number of instances, but on enlarging the sample the ratio will be changed till it becomes approximately correct' (CP 2.709).* However, in the later part of his life, as we have seen above (CP 2.780), he holds that induction only lends its probability ratio to the conclusion: '*It may be conceived, and often is conceived that induction lends its probability to its conclusion. Now that is not the way in which induction leads to the truth'*.

Peirce in his earlier papers treated induction, deduction and abduction as independent forms of inference, since induction is 'reasoning from particulars to a general law, while abduction is reasoning from effect to cause'[78] or else from consequent to antecedent. But in his later papers after 1898 induction, deduction and abduction become closely interlinked, as we can see:

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[77] See CP 2.703. 1883.

[78] See Goudge T. (1969), *Inquiry in Logic*, pp. 195-198.

> 'Induction consists in starting from a theory previously recommended by abduction, deducing from it a number of consequences, and then observing whether the predicted consequences are substantiated by experimental tests.' (CP 5.170) or else 'It (abduction) suggests the theories, which induction subsequently verifies by reference to a large number of random samples' (CP 6.100).

These last writings give evidence for the development of Peirce's method of scientific inference, since they show the interconnection between abduction, deduction and induction, as well as the probabilistic aspect of induction in the verification process. As I have shown, abduction generates the hypothesis, deduction draws the practical consequences of the hypothesis and induction refutes or verifies statistically the hypothesis.

The whole process (abduction, hypothesis, deduction, verification through quantitative induction), according to Peirce, helps the forward progress of science by pointing to more future fruitful hypotheses, by closing off certain useless ways previously open, by furnishing new observations and by increasing the experience and the skill of the scientist. On the other hand, to avoid the private subjective interpretation of the inquiry data, Peirce introduces the social aspect of scientific knowledge open to public verification. All these factors, according to Peirce, contribute to self-correction and progress in scientific inquiry, which leads to the fixing of beliefs that cohere with experience (CP 2.172.1902). I shall return to these points in the next Chapters so as to explain them in detail and defend the SCT. But before that I discuss Peirce's concept of the 'real', since it is important for his definition of the progress of scientific inquiry.

2.4. The concept of the 'real'

As explained, Peirce being an epistemological realist holds that exists an external real world consisted of the 'reals', which are independent of our opinions about them, and that this world is knowable by the scientific community, with the condition that the scientific inquiry is carried out ad infinitum. Since the definition of Peirce's SCT presupposes his definition of the 'real', therefore in this Chapter I discuss his notion in detail, so as to evaluate his criteria of externality.

The arguments that Peirce provides for the existence of the 'reals' consist mainly in the following: This is a hypothesis that presupposes the scientific method of inquiry and has never been falsified, therefore 'the method and the conception upon which is based remain ever in harmony'. *'If investigation cannot be regarded proving that there are real things, it at least does not lead to a contrary conclusion; but the method and the conception upon which is based remain ever in harmony'* (CP 5.384). In other words, the existence of the external world is a sufficient condition for the legitimacy of the inductive inference. Apart from that, Peirce provides another argument showing the existence of the external world, which consists in defining the 'reals' as a source of doubt and dissatisfaction. *'Nobody therefore can really doubt that there are reals, for if he did, doubt would not be source of dissatisfaction' (CP 5.384)*. On the other hand, since *'Scientific investigation has had the most wonderful triumphs in the way of settling opinion' (Ibid),* it follows that the existence of the 'reals' is the necessary and sufficient condition for the stimulation of our doubt and of scientific inquiry, based on inductive inference and aiming at fixing of beliefs.

These arguments do not show, however, that the aim of inquiry is to get true beliefs that correspond approximately to the external real world. Therefore Peirce provides another argument showing that the aim of inquiry is to find true answer to any question, since everybody believes that each question has one answer, as follows:

> 'You certainly opine that there is such a thing as truth. Otherwise reasoning and thought would be without purpose… Most persons, no doubt, opine that for every question susceptible of being answered by yes or no, one of these is true and the other false' (CP 2.135. 1902).

Hence, the aim of using reasoning and studying logic is to get true beliefs, and this aim stimulates our doubt and prompts us to the conduct of scientific inquiry, which is based upon the inductive inference. As we can see, what remains now to be shown, is that the inductive inference is a relation of correspondence between the external real facts, which are independent of our opinion about them, and our thought.

As I explained in the Section of *Secondness,* Peirce emphasizes the compulsive character of experience, which cannot be ignored and dismissed, and compels us to confess that it appears, for if it were a construction of our mind, we would be able to get rid of it easily by another mental entity; however, to dismiss the *percept* we need an exertion of physical force. The *percept,* then, since it is not of mental origin and has a sensory content, is *what which is given* to the perceptual act, that is to say, the percept is a physical object directly perceived in a perceptual experience, therefore it corresponds to the external physical object. This implies that the external objects, which have independent existence from us, through the *percepts,* strike our senses and forces us to acknowledge the idea of duality of their and our existence. *'Whatever strikes the eye or the touch, whatever strikes the ear, whatever strikes the nose or palate contains something unexpected. Experience of the unexpected forces upon us the idea of duality' (CP 5.539)*. But in order to distinguish the states of hallucinations and daydreaming from the real *percepts*, which constitute experience of the real facts, Peirce provides some criteria of externality.

With regard to the daydreaming, it is well known that it can be easily dismissed by a direct effort of our own will, but if it consistently persists in spite of our own will, then we can apply a second test of externality in order to find out whether it is hallucination or real flow of percepts. This test consists in asking some other person whether he sees or hears the same thing, and this can be repeated with several people. But if after this test there persists a real doubt about what is perceived (e.g. hallucination affects the whole group of people), we have to apply a third test of externality that consists in the method of prediction and confirmation (scientific inference), which can also be fallible. For instance, we can get a photo with a camera to confirm the existence of what is perceived (CP 8.144).

We can infer from these examples given by Peirce that the first two tests of externality are within the context of the compulsiveness of perceptual experience (Secondness) and entail commitment to the indubitable beliefs of the common sense, whereas the third one is within the context of Thirdness, since it involves the use of regularities of nature and scientific inference (prediction and confirmation). However, although the *percept* involves the compulsive character of experience and corresponds to the real external world, it is not of itself knowledge; therefore it cannot establish the relation between the external real world and our thought. As I previously said, knowledge enters into perception only through the mediating function of the *perceptual judgment*, which is a proposition asserting the nature of the *percept*. Thus the relation between the real external world and thought can be established only by means of abductive inference from perceptual judgment in a form of a proposition.

To be more precise, by returning to my previous example of the observed stick that is partly in the water, we can say that after the *percept* arises the first *perceptual judgment,* which ascertains: 'this stick is bent'. This judgment is infallible in the context of ascertaining faithfully the nature of the *percept*, since ‘the stick appears to be bent’. The memory that remains after this perceptual judgment (‘this stick is bent’) is contradictory with the memory of another perceptual judgment that was made by another percept, when the stick was out of the water, because the latter would have affirmed that ‘this stick is not bent’. This generates two hypotheses in the mind of the observer, as follows: ‘this stick partly in the water is bent’ and ‘this stick out of the water is not bent’. Since there are two contradictory hypotheses, the observer is in doubt about the nature of the same ‘stick’; therefore the inquirer must appeal to the scientific method of inquiry by examining the conditions of each hypotheses (e.g. stick partly in the water and out of the water), in order to find out the experiments that he has to conduct (e.g. with different light rays in the water), so as to form hypotheses about the influence of reflection and refraction of the light on the appearance of the stick. Afterwards has to be followed the deductive and the inductive phase, which will verify or correct his hypotheses, as explained. This example shows the dynamical process of the following unity, which contributes to the correction of the first instinctive perceptual judgment:

> *Percept – perceptual judgment – surprise – doubt – hypothesis – deduction – induction – verification through new percepts*

Returning to our discussion, as we have seen, the relation of correspondence between the external real world and thought (abductive inference) is established by the perceptual judgment, which is abstract, interpretative and ascertains the nature of the percept. However, when there is a doubt, due to contradictory percepts, and as a matter of fact due to contradictory perceptual judgments, the observer has to appeal to the second test of externality (group of people), a criterion that Peirce applies also in his definition of the ‘real’ as the object of ultimate opinion of the scientific community.

At first sight there is an inconsistency with Peirce’s definition of the ‘real’, since, if the ‘real’ is independent of our opinion, it cannot be dependent on the opinion of the scientific community about it, because this second definition implies that the ‘real’ has not independent existence in itself. As I previously said, the second definition with reference to scientific community concerns the knowability of the ‘real’ and not its ontological existence, which means it is epistemological definition, distinguished from the ontological one; therefore this definition is in relation to its interpretant (subject) and not in itself (object). *'The essence of the realist's opinion is that it is one thing to be and another thing to be represented' (CP 8.129. 1902),* as Peirce claims. Peirce in his attempt to avoid the definition of metaphysical truth, as explained, holds that truth could be defined only in terms of belief and doubt[79], namely as '*belief unassailable by doubt' (CP 5.416.1905*). Therefore in order this belief to be '*unassailable by doubt';* it has to be the product of the idealized and indefinite inquiry of scientific community, and the object of its ultimate opinion. Peirce being an epistemological realist holds that the 'real' external world although is ontologically independent of our mind (for it is not a creation of our mind), it is also knowable, therefore it is dependent on mind for its being known. The latter implies that Peirce is not committed to the Kantian things-in-themselves, which are unknowable, but on the contrary he holds that the 'real' is immediately perceived, therefore it is knowable, and it is the object of the ultimate opinion of the scientific community.

However, although the 'real' might be knowable, it does not follow, in my view, that it will be completely known, as the object of ultimate opinion of the scientific community. Because if the real is independent of our thought, it follows that we have not access on that directly, but rather indirectly. Besides, in case that we approach it indirectly through the scientific method, this notion presupposes that true theories from all particular sciences will be systemized in one single theory through the unification of sciences in order to unify the fragmentary view of each discipline. But this notion is very ambiguous for many distinct reasons. Because, although the unification of some theories (e.g. electromagnetism) or disciplines (particle physics and chemistry), may give evidence of this possible development to that direction, it is questionable whether e.g. biological phenomena that entail intentionality or even social phenomena that are concerned with human behavior, and not with natural regularities, can be represented by the same universal laws of physics and chemistry. In addition, arise some other problems, i.e. the limits of each discipline and of their particular language, due to their fragmentary view, the fallibilism of knowledge etc. Therefore I am going to return to this point later to examine this view in detail.

To continue with the tests of externality, if the doubt after the second test still persists, the inquirer has to appeal to the third test of externality, which consists in scientific inquiry,

[79] *'If your terms truth and falsity are taken in such sense as to be definable in terms of doubt and belief and the course of experience… well and good; in that case you are talking about doubt and belief. But if by truth and falsity you mean something not definable in terms of doubt and belief in any way then you are talking of entities of whose existence you can know nothing…' (CP 5.416.1905, What Pragmatism Is*).

namely prediction and confirmation of the cause of percept. The question that may arise here is whether the thesis of common sense or of scientific inference is more reliable. Adherents of common sense claim that common sense, which testifies the existence of the external real world, is an unerring source of knowledge, while adherents of science claim that is impossible to provide verification for all perceptual claims without appealing to some propositions, which are not empirical questions.

Peirce, in my opinion, provides a challenging synthesis of the two theses in question, because he acknowledges the force of each thesis without countenancing their respective limitations, and without maintaining that either the empirical (which appeals to the perceptual judgment) or the scientific one (which appeals to abduction, deduction and induction) are infallible. Therefore one cannot rely only on the inductive verification without appealing to abductive or deductive propositions and vice versa. As I have shown, abductive inference that generates hypotheses presupposes the relation between cause and effect (sufficient reason), as well as the reality of the general dispositions or habits, which exist potentially in the observed particulars even if they are not actualized, while deductive inference is reasonably established and applied in mathematics, and they both do not appeal to empirical data. On the other hand, the inductive falsification or verification of a hypothesis, which is always based upon the empirical data, is subject to further elaboration based upon the principles of probability and statistics.

However, in my view, although Peirce holds that crude induction is very weak sort of inference, since it generalizes on the basis of a limited number of samples and is supposed to represent the whole class, and that abduction is based upon the reality of universal qualities *(Thirdness)*, he misses here one important point, namely, our ability to recognize a universal quality, e.g. red, which we attach to the particular in the perceptual judgment, e.g. ‘this book is red’, we have acquired by generalization, which means by induction, and after having perceived many members of the class, i.e. ‘red’ things. It follows, then, that induction is not only important for falsification or verification of the hypotheses, but it also contributes much to the formation of the first abductive inferences.

As we can infer from the above discussion, in each scientific inquiry there is dialectical relation between reasoning (abduction, deduction and induction) and common sense, which form a dynamical process and correct one another on the way to the cessation of doubt and the settlement of secure beliefs, i.e. unassailable by doubt, and which represent approximately the regularities in nature.

2.5. Process of scientific inquiry, scientists' skill

After having discussed Peirce's definition of the 'real', I can proceed now to examine, how the scientists' skill can contribute to the process of scientific inquiry in all the stages of inquiry, and to explore the question: why is it justified to hold that this sort of inquiry is approaching the 'real'?

As explained, the inquiry begins with the observation of a surprising fact, which is somewhat contrary to the inquirer's expectation, in other words, a certain experience breaks the habit of expectation in the observer and changes his mind from the state of *belief* to the state of *doubt*; therefore it forces the inquirer to undertake a process of inquiry in order to explain the unexpected fact (CP 7.198). After that the first activity that is occurring in the mind of the inquirer is the *abduction,* which is nothing but the formation of a *hypothesis* that attempts to explain the unexpected fact. Since the unexpected fact is the *consequent*, then there is a *sufficient reason* to search for the *antecedent* in a form of hypothesis, which entails the unexpected fact or makes it probable, and this is the *minor proposition* (case) of the *abductive syllogism.* However, some questions remain still open: 1. how the hypothesis is generated, 2. why its formation is justified, 3. what contributes to the formation of better hypotheses, and 4. which hypotheses are of progressive and corrective nature and as a matter of course contribute to approaching the 'real' and not getting away from that? All these questions I am going to discuss here, since I hold that their answers are the crucial parts of the Self-Corrective Thesis of scientific inquiry.

2.5.1. Abductive phase

First of all, as explained, abduction occurs after the memory of the perceptual judgment, which in turn is instinctive, subconscious and asserts in a propositional form the character of the percept (e.g. the unexpected fact). Furthermore, the perceptual judgment is interpretative and involves a generality of Thirdness, which means that contains generality in its predicate, e.g. if the unexpected fact is: 'these beans are white' of our example, its predicate is the universal term 'white', which is given in perception. Abduction on the other hand aims at finding the cause of this fact, therefore tries to find out whether this individual case (unexpected fact) is included in the more general class represented by the predicate of the major premise ('all the beans in the bag Nr. 3 are white'), which means that there is a sufficient reason to infer the hypothesis: 'these beans are from the bag Nr. 3'. The question that may arise in this process is: what justifies the true value of the perceptual judgment and of the hypothesis?

In order to answer the first question Peirce appeals to his notion that human instinct consists of certain natural beliefs that are true, because the human mind is a part of the universal intelligence[80]. Since it is continuous with the rest of the cosmos, it is expected that there will be an agreement between the ideas, which present instinctively themselves to the human mind (perceptual judgments) and those which exist in the laws of nature. The whole concept of this thesis is based upon Peirce's doctrines of *objective idealism, tychism, synechism and evolutionary cosmology*.

First, *objective idealism* holds that all that is, is mind, and the matter is effete mind, therefore it can explain the properties of feeling and intelligence in the universe '*The only intelligible theory of the universe is that of objective idealism, that matter is effete mind, inveterate habits becoming physical laws (CP 4.551)*. Second, according to *tychism* the responses of matter to stimuli initially are governed by absolute chance[81], but finally matter tends to take on habits of activity, which entails law, generality and continuity in the universe. Therefore there are no absolute individuals, but what we call individual are rather fragments of general systems or symbols of its kind: *all that is, is general and universal*, as we have seen according to Peirce's *synechism*. Furthermore, according to his evolutionary cosmology, the universe evolved by chance from irregularity (chaos) into regularity; therefore evolution assures both the growth of intelligibility through law and of diversity through chance.

> 'Variety, uniformity and the passage from novel variety to uniformity characterize the universe, which we experience' (CP 6.97).

[80] This concept is similar to Anaxagoras' concept, who holds that throughout the living beings is distributed a part of the universal Intellect (*νούς*: nous). Compare Kirk et al. (1983), pp. 352-84, Bakalis (2005), pp. 75-80.

[81] Peirce uses the Greek word for chance, *τύχη:* tyche.

Since nature due to the factor of chance (tychism) is not rigidly rational, man's knowledge of nature cannot be achieved only by a purely deductive rationalistic process, but it also involves both abduction and induction that arise naturally in his mind.

We can leave aside these metaphysical notions, which are ambiguous, in my opinion, and focus on the concept of chance against the mechanical laws, which is what Peirce wishes to introduce, in order to explain the existence of spontaneity in nature. His point is that we cannot explain the increase in complexity in terms of mechanical laws, since mechanical laws can never produce diversification and they are reversible, while the natural laws are not[82]. The consequence of the latter is that if the world were governed by them, the past would be no less determined by the future than the future by the past. Therefore Peirce concludes that both law and chance exist in nature. Alfred Ayer (1968) in his attempt to argue in favor of determinism and against 'free will' criticizes Peirce's notion of chance[83] by the use of the well-known arguments of *reductionism*. However, in my opinion, Peirce's conception of chance and variety does not concern only laws of physics, for in quantum mechanics and chaos theory its predictive vision was anyway borne out, as Prigogine Ilya[84] argues. It embraces also the laws of biology i.e. in biology it is accepted that random mutations of species contributed most to its evolution and variety; therefore there is an indication that chance and variety works in all levels of the phenomena.

Furthermore, according to Peirce's objective idealism, human mind is a part of the Absolute Mind in a developed state through evolution, while physical objects are parts of the Absolute Mind in an undeveloped state, as they are of its nature. Certainly, this union of his epistemological realism and objective idealism may solve the problem of the knowability of the external real objects, because both the human mind and the external real world are of

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[82] Peirce in (CP.1.174) argues: '*The increase in complexity cannot be explained in terms of mechanical laws, for mechanical laws cannot produce diversification… anybody can see that mechanical law out of like antecedents can only produce like consequents'*.

[83] Ayer J. Alfred in (1968), pp.103-111, *The Factor of Chance*, raises an objection against Peirce's *tychism*, and argues on p. 106: *'It does not follow, however, that we are bound to attribute the variety to pure spontaneity. It can still be maintained that there are laws of some kind to which the phenomena are entirely subject, and even that these laws are all derivable from the laws of physics…'*. Since laws of physics are basically statistical (due to the statistical regularities working in quantum mechanics, as he admits), and biology can be reduced to chemistry and chemistry to physics, it follows that due to the distribution of atoms all the variety was destined to exhibit. By the use of these arguments of *reductionism* he rejects the doctrine of the existence of chance and the evolution of laws in the universe, therefore he concludes in favor of determinism and against 'free will'.

[84] See Prigogine Ilya (1984), p. 303: *'But, in fact, today Peirce's work appears to be a pioneering step towards the understanding of the pluralism involved in physical laws'*.

the same nature; therefore exists a true relation of correspondence between the external word and thought in the perceptual judgment.

However, this thesis, as R. Almeder[85] argues, poses the problem of independence of the external real objects, as follows: how, on the one hand, the 'reals' are causally independent of finite minds, but they are not causally independent of the Absolute Mind, since they are its creations. This implies that there must be a real distinction between the sum of finite minds (scientific community) and the mind of the Absolute, while Peirce suggests the contrary. In my view, this problem cannot be reduced so easily to that sort of argument: 'finite is not identical with infinite'. Because, if on the one hand we take into account the definition of the 'real' given by Peirce - the opinion which the scientific community will approximate gradually and inevitably with the condition that the inquiry is carried out ad infinitum - which is defined as an ideal limit, and on the other hand, if we consider that the scientific community includes the sum of all finite minds of all the times in all over the universe, we cannot conclude that the one is finite and the other infinite. Apart from that, we are not in a position to know in which context human mind is finite or infinite, if we take into account the generality of the universals, which the human mind perceives and are inexhaustible. Finally Peirce does not suggest that the Absolute Mind and the Scientific Community are identical, when claiming that human mind is a part of the Absolute Mind, for it can be a part of the whole that is not a self-determined copy of the whole or that the whole cannot be all alike.

Returning to our discussion, according to the doctrine of *synechism* the human mind due to the existing regularities can understand the laws of nature, since in the perceptual judgments that are formed after each perception are given these general elements, *'Every general form of putting concepts together is, in its elements, given in perception' (CP 5.186)*. Furthermore, perceptual judgments are the first abstract interpretations of the external world, the first instinctive abductive inferences that lead to the formation of the first provisional hypotheses. The human mind, as Peirce says, 'is akin to the truth', and humans move towards truth instinctively, since they have instinct at guessing the truth. In order to establish this hypothesis, apart from the previous mentioned doctrines (*objective idealism, tychism, synechism and evolutionary cosmology),* Peirce provides some historical arguments. He compares this human instinct with the instincts of animals, which guide their

85 See Almeder R. (1980), pp. 156-158.

activities of feeding and breeding, so to say feeding is an instinctive application of physics (mechanics), while breeding is an instance of psychics. When we humans are dealing with the mechanical forces of bodies or with estimating of how other human beings think or feel we move instinctively towards truth (CP 6.500, 6.531). This instinct of selecting provisional explanatory hypotheses under the control of inductive testing has been developed into science, therefore he says: *'Science is nothing but a development of our natural instincts' (CP 6.604)*. The same affinity of the human mind with the universe has enabled the human race to survive; otherwise, if man were not equipped with a mind adapted to his requirements, he would have become extinct. Knowledge, then, of the history of nature and of science leads us to inferring abductively that the affinity of the human mind with nature is responsible for both the success of science and for the survival of man.

But although all these Peirce's metaphysical notions may explain the existence of the human instinct, in my opinion, we have to distinguish the instinct of human beings of guessing the truth, from the scientists' instinct or skill. The first one, as being the outcome of the intellectual development of mankind, can justify the formation of perceptual judgments and the simple explanatory hypotheses, while the second one consists in selecting the most possible explanatory hypotheses among the innumerable ones. Because instinct is erroneous and instinctive hypotheses are fallible and subject to deliberate, self-controlled thought and criticism, as I already have shown. The question, then, that remains to be answered is: in what consists the instinct or skill of the scientist, and what is its relation to the abduction and the selection of the most 'fruitful' hypotheses?

First, since the hypothesis turns from experience of the unexpected fact to its abstract interpretation, the inquirer's skill and experience make important contribution to the way he interprets the observed fact through his proposed explanatory hypothesis. This notion implies that the personal background knowledge of the inquirer, which is based upon his previous beliefs and the history of the science, is important part of that adjustment. As I previously said, the general element is given and known in perception by virtue of perceptual judgment before the abduction is drawn, or else in logical terms the predicate is given in perception. In my previous example of the observed unexpected fact 'these beans are white' the predicate 'white' of the perceptual judgment formed is given in perception, and it is 'real', since it interprets faithfully the percept.

However, the abductive inference, which is drawn afterwards and aims at classifying the unexpected event to a known class of events, is subjective and depends on the observer's skill. Because the hypothesis that is drawn abductively and aims at classifying the particular event to one of the general classes, on the one hand presupposes the knowledge of all those different general classes, i.e. it requires background knowledge. But as I have already argued, this background knowledge is based partly on inductive generalizations, since the ability to recognize universal qualities or classes we have acquired by generalization and after having perceived many members of the class, i.e. qualities are discovered and abstracted from the particulars of experience. Peirce in his '*Pragmatism as the Logic of Abduction*', (CP 5.181. 1903) admits also that 'the abductive inference shades into perceptual judgment without any sharp line of demarcation between them', as it comes from the unconscious part of the mind, which includes many adopted inductive generalizations[86]. Furthermore, classification of the particular event to one of the general classes presupposes the mental act of comparison between the particular event and the class of events. This means, that it involves the capacity of mind 'for discerning Difference and Likeness', as William James[87] said. And this capacity of the human mind is subject to further development; therefore it is gained much by the inquirer's skill and experience.

On the other hand, abduction involves originality and skill that consists in the idea of putting together all the elements known already in perception in one suggested explanation. As I am going to show in the historical study, both Lavoisier and Priestley concluded abductively from the same observation data to two diametrically opposed hypotheses. Therefore, abductive inference is subjective and depends on the inquirer's background and skill. Peirce, in order to solve this problem suggests some normative 'criteria of admissibility of hypotheses' for the rational appraisal of the drawn hypotheses.

2.5.2. Admissibility of hypotheses

Since in the abductive phase there are several possible hypotheses that present themselves, the inquirer has to follow some criteria of selecting among them the most 'fruitful' ones, which means that the instinctive ability of the inquirer can be improved through logic that

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[86] Hilary Putnam in (1995), p. 58, also argues: '*The Peircean contrast between data and abductive hypotheses is not absolute (as Peirce himself knew) because, in theoretical science, abductive theories can play the role of hinge propositions'*.

[87] See more James Williams (1950), *Necessary Truths-Effects of Experience*, pp. 617-678.

determines the criteria, which contribute to the progress of inquiry. After having studied Peirce's writings I concluded to the following criteria, which, in my opinion, can secure the progress of scientific inquiry towards the approximate representation of natural regularities[88]:

1. Choose hypotheses that may render the observed facts necessary or highly probable (today's inference to the best explanation[89]), because abduction proceeds from the observed facts to the most probable explanation, as a true relation of a consequence between consequent (observed fact) and antecedent, or else a relation between cause and effect.
2. Choose hypotheses that their premises (not the conclusions) are capable of being tested by induction (CP 5.599, 7.220), since the verification process can lead us to true conclusions. But this does not imply that the explanatory hypothesis about the observed fact is also a result of an observed event, but may be any other supposed unobserved truth from which the observed event would result. For according to pragmatism, the premise of hypothetical inference and its conclusion both cannot be observed, otherwise one will come to the strict Positivist position, which, for Peirce, fails to explain the unobserved truths.
3. Choose logically 'simpler' hypotheses, abided by Ockham's razor[90] (CP 6.535), because the simpler hypothesis adds the least to what has been observed, and

[88] To classify these criteria apart from the Collected Papers (especially CP 7.164-231. 1901) and Peirce's bibliographical sources, I have taken especially into account the classification of the criteria quoted in: Goudge Thomas (1969), *The Thought of C. S. Peirce,* pp. 199-201, *Some Requirements for Choosing Hypotheses,* in Reilly F. (1970), *Charles Peirce's Theory of Scientific Method,* pp. 38-45, and *Abduction, Deduction, Induction,* in Misak C. J. (1991), *Truth and the End of Enquiry,* pp. 91-100. However in my opinion, the category of economical hypotheses must include as sub-categories the 'highly falsifiable hypotheses' as well as the 'experimental consequences' classified as separate categories by Misak (1991), while the analysis of the hypothesis in its components and the formation of broad hypotheses are separate categories independent of economic considerations, as Reilly (1970) puts it. Apart from that, I believe that the analysis of a hypothesis into its components and the pragmatic maxim are very important criteria, since they connect all the stages of inquiry.

[89] See Lipton, P. (1991).

[90] The principle suggested by W. Ockham states that the explanation of any phenomenon should make as few assumptions as possible: '*entities should not be multiplied beyond necessity*', which has been paraphrased as 'the simplest explanation is to be preferred' and it is considered to be as a heuristic maxim. Peirce's interpretation is that the inquirer should test the simpler explanation, namely the one with the fewest elements first. Lavoisier appeals to the same principle of simplicity in order to reject the phlogiston theory, as I am going to show: '*If all of chemistry can be explained in a satisfactory manner without the help of phlogiston, that is enough to render it infinitely likely that the principle does not exist, that it is a hypothetical substance, a gratuitous supposition. <u>It is, after all, a principle of logic not to multiply entities unnecessarily</u>*' (Lavoisier 1965; underlying mine).

therefore it is closer to the interpretation of reality. Moreover, the simpler hypothesis is more natural, as it is the one suggested by instinct due to the affinity of human mind with nature, which Galileo characterizes as '*il lume naturale'*[91] (CP 6.477).

4. Choose hypotheses that are economical in money, time and energy[92], since the scientist is subject to financial and temporal limitations. Certainly, this thesis has some practical consequences, as follows: a. When several hypotheses present themselves to the inquirer's mind he should try to find a critical test to halve the number of suggestions (CP 7.220), b. Derive the consequences of each hypothesis before testing, so as to find out which experimental testing is economical in time and money (CP 2.775), c. Highly falsifiable hypotheses are preferred, because the cost of their testing is lower in time and money, and negative knowledge gaining by rejecting hypothesis proven false becomes more valuable; as it closes useless ways of thought previously open and may be instructive with reference to the next hypothesis (CP 7.221). Thus, when two hypotheses occur to the mind of inquirer, he should begin with testing the one that is most readily refuted or verified, even if its apparent likelihood is less (CP 5.598, 6.408, 6.528, 6.530).
5. Not attach too much importance to subjective antecedent probabilities or likelihood of a hypothesis (CP 2.777, 7.220), for preconceived ideas and 'likelihoods are treacherous guides', for Peirce, and sometimes are only the fruit of *a priori* speculations so thoroughly discredited by him.
6. Break the hypothesis into its smallest components[93] (analysis) and then derive the practical consequences of each one, so as to synthesize all the components into one broad hypothesis (hypotheses with the more explanatory power, CP 7.220). This aspect, although it is not specified but only implied by Peirce different notes, in my view, has to be added in the criteria of admissibility of hypotheses as very significant, because it can help the inquirer to examine all the conditions of the observed fact and explore the contribution of each factor to the observed event, and as a matter of fact to dismiss the irrelevant to the event factors. Furthermore, it helps

[91] Peirce submits as reference to that Galileo's *'Dialogues Concerning the Two Great Systems of the World'*.

[92] Peirce developed these views on economy in research due to personal experiences during his experimental studies in physics and astronomy with the Costal Service. See more *'Notes of the Theory of the Economy of Research'* (CP 7.139-157).

[93] The method proposed here is similar to Plato's method of division (*διαίρεσις*: *dihairesis*), which is used in his latest Dialogues *Sophist*, *Statesman* and *Philebus*, and aims at defining the kinds by successive division of a genus into its parts. See Cooper M. John (1997), *Plato: Complete Works.*

the inquirer to design a series of experiments by excluding or including each time one factor of the event. To make it clearer, in my example of the ‘bent stick’, this means to examine all the different factors of the event e.g. light, water, air, stick, light through air-water, light through water-air, stick out of the water etc.

7. Choose hypotheses that are broad and inclusive, so to say, bring the most facts under a single formula (CP 7.410), or else substitute for a series of predicates a single one. As I said, since the hypothesis aims at classifying the unexpected observed fact to a general class of events, the latter should be the unified predicate, as it is justified by the reality of the dispositions or habits (universals) and the true relation between antecedent and consequence.
8. Adopt a hypothesis, which leaves open the greatest field of possibility (CP 1.170), because it contributes to the progress of knowledge. This aspect of the chosen hypothesis Peirce compares to the billiard player’s ‘leave’ (CP 7.221) that helps the player to proceed easily to the next steps. The theoretical justification of this suggestion lays in Peirce’s thesis that every thought is a *sign*, therefore *‘it translates itself into another sign in which it is more fully developed’ (CP 5.594).*
9. Adopt hypotheses that are in accord with the *pragmatic maxim*, which implies, to consider the possible practical effects of each one, and those hypotheses that do not bear conceivable effects have to be excluded. This criterion of admissibility of a hypothesis looks like the Nr. 2, since it implies that only the directly or indirectly verifiable hypotheses are admissible; however the pragmatic maxim, as I said, can be applied also to the deductive phase of verification, where the inquirer draws the practical consequences of a hypothesis and decides what sort of experiments has to conduct. For example, from the proposition of the pragmatic maxim ‘*if I conduct myself in a manner x, I will have experience y*’ (practical deduction), by inversion I can infer: *‘if I want to have experience y, then I will conduct myself in manner x*’, which means that I can find out ‘*what sort of experiments do I have to conduct (quantitative induction) to have the experience y?*’. Apart from that, the pragmatic maxim constitutes the theoretical and reasonable foundation and justification of the dialectical relation between abduction, deduction and induction in the whole process of inquiry. Because the inductive phase of verification alone cannot arise without both the abductive inference and the derivation of practical consequences in the deductive phase, determined by the pragmatic maxim.

10. The last criterion of admissibility of a hypothesis is related to the inquirer's insight, experience and instinct, as Peirce urges investigators to trust the power of the mind to hit instinctively on the right explanation of observed facts (CP 6.530). Certainly, this criterion presupposes experienced inquirers with a good background of scientific knowledge, who by the skilful use of their instinct can dismiss foolish explanations without verifying them and test the right hypotheses rather early in the inquiry. *'It is through instinct that the scientist dismisses as irrelevant the configuration of the planets, the actions of the dowager empress, the color of the daughter's dress at the time of experimentation (CP 7.220).* This criterion has its theoretical foundation on the kinship of man's mind with the rest of cosmos, therefore *'although the possible explanations of our facts are strictly innumerable, yet our mind will be able, in some finite number of guesses to guess the sole true explanation' (CP 7.219).*

To sum up with the criteria of admissibility of hypotheses, following Peirce's notes I have tried with each one of them to establish a true relation between experience and abductive inference, so as to secure the progressive and corrective character of this first stage of inquiry, which is very significant for approaching the regularities, because even though *'predicted experience strengthens the hypothesis, it is still the hypothesis itself that makes the real contribution to the progress of science' (CP 2.625).*

2.5.3. Deductive phase

The second stage of the inquiry is the ***deductive phase***, where the abductive expectability has to be secured by the deductive necessity, in other words, once a hypothesis is adopted have to be examined its conditional experimental consequences, which would follow from its truth. This process is theoretically justified by the pragmatic meaning of ideas, which lies in considering their practical consequences, as I have shown; therefore the pragmatic meaning of a hypothesis lies in drawing the experimental consequences from it.

To be more precise, since the hypothesis drawn in the abductive phase has classified the unexpected observed event A to a general class of events B, and the members of the class B have the observable characteristics *a, b,* and *c,* then according to **deductive reasoning** the event A should have the same observable characteristics, as follows:

All the members of class B have observable characteristics *a, b* and *c*
Observed event A may belong to class B (hypothesis)
Event A should have observable characteristics *a, b* and *c* (practical consequence)

This reasoning helps the inquirer to plan the sort of experiments that he has to conduct in order to verify his hypothesis. Because, as I said, from the inversion of the proposition of the ***pragmatic maxim*** ('if I conduct myself in a manner *x*, I will have experience *y*') I can infer 'if I want to have experience *y*, then I will conduct myself in manner *x*', which can be translated into 'if I want to have experience *y'*, then 'I have to conduct that sort of experiments *x*)', and in our case:

'If I want to have experience of the observable characteristics *a, b* and *c* that A has, then I have to conduct that sort of experiments'

In this stage of inquiry the scientist's imagination, skill and experience are also very significant, since they contribute to inventing the right experiments, which might verify his hypothesis. As I am going to show in the historical study, although Lavoisier's skill in abductive inference was higher than Priestley's, Priestley was more skilful in experimental designing, whose data Lavoisier used to infer his hypothesis. This is another aspect that signifies the importance of the contribution of scientific Community to the progress of the inquiry towards the approximation of natural regularities.

To sum up, the inquirer in this phase draws the 'virtual predictions' of the hypothesis that he has adopted for testing, because scientific inquiry is nothing but 'a conversation with nature', where the scientist asks nature through certain experiments whether e.g. '*event A has observable characteristics a, b and c'*, and waits for an answer to see how close his hypothesis is to nature. Finally, if the predictions or else the expectations of his hypothesis are proved to be right (event A has the same observable characteristics a, b and c), then his hypothesis should be true. Therefore the last stage that follows is the inductive phase of verification, where the hypothesis has to be experimentally tested, since, as the unexpected event emerges from facts of experience and concerns general class of events (dispositions or habits), then its explanatory hypothesis has to be brought back to experience for testing[94].

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[94] William James in (1975), *Pragmatism's Conception of Truth*, also points out that the verification process makes the potential truths actual, since truths emerge from facts, therefore have to be brought back to the facts

2.5.4. Inductive phase

As I said, Peirce rejects the ***crude induction*** in this phase of inquiry and adopts as more reliable the ***quantitative induction*.** The latter consists in the **ratio** ***(p)*** or **relative frequency** ***(f_n)*** of successful predictions of the hypothesis, namely, the number of times that the hypothesis has predicted successfully ***(m)*** as related to the total number of times ***(n)*** that the conditions of the prediction were fulfilled, $p = m/n$ or $f = m/n$[95]**.** If the experimental test of the hypothesis is repeated several times with a large number of samples ***(n)***, then will arise different ratios, as follows: $f_1 = m_1/n_1$, $f_2 = m_2/n_2$, $f_3 = m_3/n_3, \ldots, f_n = m_n/n_n$. Hence, the limit of those relative frequencies ***($\lim f_n$)*** represents the probability ratio of the sample, which in our case shows the proportion ***(p)*** of the successful predictions of the hypothesis in the sample. For Peirce, a reliable value of proportion or probability ratio for accepting the verification of a hypothesis is when $p \geq 0.9$ or else more than *90 percent,* while when it is lesser, we have to suspend judgment. As induction lends the probability ratio to its conclusion, we can infer 'probably and approximately' that the same ratio would be found in the whole population, according to the following inductive syllogism:

> S1, S2, S3, etc. are a numerous set taken at random from among the Ms
> A certain proportion *p* of these Ss are Ps
> Therefore the same proportion *p* of the Ms are Ps

Or else:

> These beans have been taken at random from this bag (case)
> 9/10 of these beans are white (result)
> Therefore 9/10 of the beans in the bag are white (conclusion)

Returning to our example of the observable characteristics, *a, b* and *c*, let's suppose that the observable event A is a chemical substance, which we have abductively inferred that

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for evaluation, as follows: '*Truth for us is simply a collective name for verification-processes, just as health, wealth, strength, etc., are names for other processes connected with life, and also pursued because it pays to pursue them. Truth is made, just as health, wealth and strength are made, in the course of experience'*, (p. 104), and '*Truths emerge from facts; but they dip forward into facts again and add to them; which facts again create or reveal new truth'*, (p. 108).

95 See more Appendix 1.

belongs to a certain class of substances B. Afterwards through deduction we conclude that the predicted observable consequences of the hypothesis is that the substance should have observable characteristics (a) density $\boldsymbol{d}$, (b) boiling point of its solution $\boldsymbol{T_b}$ and (c) freezing point $\boldsymbol{T_f}$; therefore we decide to test experimentally these qualities of the substance A. After having tested a several number of samples of the substance A, we have found that the 95 percent of the samples have proved to have the same qualities like of the class B, namely density $\boldsymbol{d' = d}$, boiling point $\boldsymbol{T_b' = T_b}$ and freezing point $\boldsymbol{T_f' = T_f}$. From this fact we can infer 'probably and approximately' that the same ratio would be found in the whole population of the substance A, and as a matter of course the substance A probably belongs to the class of substances B.

Very important in this phase of inquiry is to name and to evaluate the importance of the various qualities of the subject class under investigation, and this estimation Peirce calls ***qualitative induction***[96]. In order to explain the function of the qualitative induction he quotes the following example: '*In testing the hypothesis that a certain man is a Catholic priest, the inquirer should put more value on the man's role in ceremonial functions than on the style of clothing he wears*'. In my previous example, if more valuable quality of the class B is its acidity, then we should better test also the acidity ***(pH)*** of the substance under investigation A. Other examples: if it is supposed a golden object to be examined, we can test the atomic number of it or in case of radioactive substance, we can detect the ionizing radiation through a Geiger counter.

Hence, both qualitative and quantitative induction can lead to probable conclusions and affect a closer convergence on true conclusions, but they never attain full certainty due to their probabilistic and fallible character. The question that remains to be answered is: in what consists the contribution of induction to the approximate representation of the regularities in nature? First, the principle of induction is the generalization from particulars to generals; that is to say, in induction we infer from a number of cases of which something is true that the same is true of a whole class. However, since the inference is based on a limited number of instances and is supposed to represent a true ratio of the whole class, it may be erroneous, therefore, Peirce holds that 'when the sample tested is enlarged, the ratio begins to approximate the truth' (CP 2.709) and in the long run will approximate the truth. In that way can be secured the inductive inference, and as a matter of fact, it is justified as

[96] More about qualitative induction will be discussed in the next chapters.

approximation to the truth by attaching true probability ratio to its conclusion. Furthermore, though the series of singular events is endless, since it is incomplete in a finite number of testing, Peirce claims that every law tends to be manifested in a finite series of samples. Because if the law under investigation is real, then its character will be manifested, although the series of events is incomplete, for *'it absurd to say that experience has a character which is never manifested'* (CP 2.784), so to say the ratio of singular events in the long run shows its limit.

Apart from that, Peirce suggests that the process of sampling must be fair and honest, which means that the sampling must be random, unregulated and free from any control, while the inquirer must be guided more by his love for truth than by any enthusiasm for his explanatory hypothesis (CP 2.757). If the inquirer is looking for instances, which will only confirm his hypothesis, has a prejudice in selecting those instances, and as a result the instances observed do not represent the whole class. In other words, *'the testing must be fair, unbiased and representative'*. Because the law of nature is a generalization formed from the results of observations, therefore the observation must be formed so as to conform to the outward conditions, which presupposes that the instances observed must represent the whole class as far as possible. A new significant element that is introduced by Peirce in this phase, as we can see, is the moral quality that the inquirer must have, and it presupposes the 'fair' inquiry, which means the 'love for truth'. Therefore I explore also these ethical aspects of inquiry.

2.5.5. Ethical aspects

Peirce in his late papers[97] was committed to the view that the search for truth must be for its own sake, and it is more admirable enterprise than the quest for practical, useful knowledge. The individual scientist, although begins his inquiry motivated by the irritation of doubt and searches for settled beliefs, 'he gradually becomes better acquainted with the character of cosmical truth and learns that human reason can be brought into accord with it, and conceives a passion of its fuller revelation' (CP 8.136). That motive, search for truth for its own sake, according to Peirce, has inspired men of science more than their pursuit of pleasure and the promotion of their own way of life, because during their investigation they have realized the deeper meaning of all things in the universe. This thesis brings us back to

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[97] See *Why Study Logic*, in Peirce's manuscript *Minute Logic*, (CP 1.203-1.282), also Reilly F. (1970), *Charles Peirce's Theory of Scientific Method, Greek emphasis in Theory*, pp 131-138.

the definition of the 'highest good' (***άριστον αγαθόν:*** ariston agathon) by the Greek philosophers, which is considered to be the theoretical, speculative knowledge (***θεωρείν***: theorein)[98] and is admirable for its own sake (*per se*), but not for the sake of anything else. As Peirce says, the admirable in itself must be general and precisely known in a unitary ideal, which turns out to be the development of reason (CP 1.615). Therefore, for science, nature is the object of its worship and aspiration, as nature is something great, beautiful, sacred and eternal, and real (CP 5.589). The development of reason, then, if it is applied correctly, reveals gradually and fallibly the secrets of cosmos.

Certainly, the definition of speculative knowledge as highest good (*summum bonum*) associated with Peirce's naturalism appears to be at first sight contradictory with the pragmatism, the value of practical knowledge and the distinction of scientific method from the *a priori* method. But if we consider that the universals are inexhaustible and the rational purport is general, it can be justified that the aim of pragmatism in an indefinitely prolonged action is the understanding of nature. Furthermore, the practical judgment that '*x* is useful for a need' can ultimately be evaluated only in terms of theoretical knowledge, which is operating with freedom and is beyond the useful. From this point of view we can see the relation between theoretical and practical knowledge, therefore we can say that theoretical knowledge is the source of all practical knowledge. Finally the 'love for truth' increases the inquirer's awareness of his possible failure[99], but that failure will be helpful for the next generations, since, as Peirce says, 'they will climb over his failure as they storm the fortress of knowledge'

However, pure knowledge as a goal of science presupposes pure motivation and action, which is regulated by ethical principles, therefore very significant is the ethical aspect of scientific knowledge apart from the logical aspect, as ethics indicates the end of life, whereas logic deals with the means of attaining the end of thought. *'It is therefore impossible to be thoroughly and rationally logical except upon an ethical basis'* (CP 2.198). In this sense, the goal of scientific inquiry (knowledge for its own sake) belongs to an ethical inquiry, and Logic depends on Ethics[100]. On the other hand, since Aesthetics

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[98] See Plato's *Politeia*, Book VI, in Cooper M. John, (1997), and Aristotle's *Nicomachean Ethics*, Book I, 1096-97, Book X 1177-78, in Barnes J. (1998).

[99] For more about right scientific attitude, see Susan Haack, *'First Rule of Reason'*, in Brunning Jacqueline and Forster Paul (1997), *The Rule of Reason: The Philosophy of Charles Sanders Peirce*, pp. 241-261.

[100] See Bernstein R. J., *Action, Conduct and Self-Control*, pp. 67-91, in Bernstein R. J. (1965), *Perspectives on Peirce.*

attempts to define the quality of the ‘good’ that is admirable in itself, it is also very important for studying Logic, for Logic depends on Ethics and Ethics on Aesthetics, as Peirce states:

> ’Aesthetics, therefore, although I have terribly neglected, appears to be possibly the first indispensable propedeutic to logic and the logic of aesthetics to be a distinct part of the science of logic that ought not to be omitted’ (CP 2.199).

But if we consider the motives of scientists in modern society, Peirce’s notions sound very ideal. Science is a social enterprise and scientific communities, like all other communities, are governed by some ethical norms or values. William James seeing science from the individual scientist’s point of view argues that emotional motivation of each investigator has been very significant for the advancement of science[101]. These considerations gave rise to the introduction of sociology of science, which concluded that science in its historical development has come to the adoption of some basic moral values, as follows: *universalism, communism, disinterestedness* and *organized skepticism.* The main motive that encourages original thinking in inquiry, according to this view, is the *reward system* of science that involves recognition and publishing of one’s original ideas[102]. These views have been further developed[103], since the structure and the common values of the scientific community are very important for its autonomous and inquiring role. In my opinion, competition of different opinions within a pluralist and autonomous scientific community – as long as the community has the necessary institutional characteristics that can secure its independent, democratic and inquiring role – constitutes an inner dynamic that provides motivation for more qualitative scientific work, and as a result it can contribute much to the solution of this problem.

2.5.6. Dialectical unity

Returning to the process of scientific inquiry, after the inductive phase and according to the testing results the inquirer is able to characterize his explanatory hypothesis as: proved,

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[101] See William James, *The Will to Believe* (1956), VIII: ‘*science would be far less advanced than she is if the passionate desires of individuals to get their own faiths confirmed had been kept out of the game … The most useful investigator, because the most sensitive observer, is always he whose eager interest in one side of the question is balanced by an equally keen nervousness lest he become deceived’.*

[102] See more Merton, R. S. (1973).

[103] See more Mulkay (1979), Barnes B. and Bloor D. (1982), Habermas (1983), Kitcher (1993), and Ziman (1994).

partially proved, unworthy of further investigation, in need of modification, highly dubious and so forth (CP 2.759). In any case, positive or negative inductive verification contributes to the forward progress of scientific inquiry by closing off certain useless avenues previously open, by pointing to more fruitful areas for future modified and hypotheses after new abductions, and by increasing both the experience and the skill of the inquirer. Because the experience gained in the process of testing the old hypothesis can be used as a basis for forming more accurate, revised hypotheses.

On the other hand, the new revised hypothesis will be closer to the true interpretation, as long as the adequate background of the investigator has increased his sense of evaluating the hypotheses. The new abductive inference will be formed by increased experience and more accurate instinct for truth, as well as the deductively followed experimental predictions of the new hypothesis will be predesignated more reliably and with more experience and skill, and as a result with better experimental tests. Hence, both experience and skill contribute to the gradual process of scientific inquiry towards the approximate representation of the natural regularities, which are gained in all the stages of investigation. Therefore all the three stages form a dialectical unity, where each one contributes to the correction of the other, and all of them to the gradual approximation to natural regularities. As Peirce states:

> 'It is mathematically certain that the general character of a limited experience will, as the experience is prolonged, approximate to the character of what will be true in the long run' (CP 6.100).

What is valid for the individual investigator is also true for the community of inquirers, as I am going to show in the next Chapter, since each member is informed about the work of the other member, uses his experience, learns from his failures, and as result the whole community proceeds gradually and fallibly towards the approximate representation of the regularities.

2.6. Fallibilism, Probabilism and Scientific Community

As I previously said, Peirce considers knowledge gained by scientific inquiry fallible, because he holds that '*it cannot be absolutely exact, absolutely universal or absolute certain' (CP 6.607).* Fallibilism arises both from the nature of scientific inquiry, as explained in the last Chapter, and from the nature of perception (*'No cognition and No sign is absolute precise, not even a percept', CP 5.543),* therefore the best interpretation of the 'real' that we can achieve is an approximate one.

With regard to the nature of scientific inquiry, it consists: First, of abductive inference, which is fallible and subjective, as it depends on the inquirer's skill that lays in his background knowledge (personal set of beliefs). Second, of the deductive reasoning, used in drawing the consequences of hypotheses, which is also fallible, since even 'mathematical certainty is not absolute certainty' (CP 4.478). Moreover, the testing process of inductive reasoning that generalizes on the basis of a limited number of samples and is supposed to represent the whole class enters the region of the probable and uncertain. The latter is based on the frequency account of probability. For instance, if we compare the sequence of events to coin tosses, and from these some have property A (heads) and some –A (tails), the relative frequency of A-type events (coin landing tails) as related to the total number of events (tosses) on an infinite number of events and *in the long run* will stabilize towards some limit (0.5 in our case), which will be the true probability ratio[104]. Therefore the inductive verification must consist of a large number of random samples, so as to stabilize its probability ratio.

On the other hand, granted *synechism,* we cannot fully specify the properties of the individuals, for the individuals are rather symbols of their kinds, namely universals, and the interpretation of the *signs* (symbols) that represent the individuals cannot be fully exhausted. Besides, the universal dispositions or habits that really exist potentially in the particulars can never be exhausted due to their infinite potentiality. Apart from that, the object of scientific inquiry is subject to further evolution due to the existence of both law and chance in nature. All the above mentioned factors, based on the logic of probability and Peirce's evolutionary metaphysics, contribute to fallibilism of scientific knowledge. Granted fallibilism, every belief is subject to review, confirmation, correction or rejection by subsequent belief. The question that may arise here is: what are the true beliefs and how can we attain them?

Taking into account the ontological definition of the 'real' – it exists independent of what individuals may think of it, since each individual at different times perceives it from a

[104] See more Appendix 1.

different view and it is represented in a different way[105] - then we can assume that all theses perspectives under public inquiry will tend to cohere with each other and will arrive to the ideal state of knowledge of the 'real', a true belief, which will be its approximate representation. For example, let's suppose that different persons observe a cup from different angles, in different lights and at different times, then in the end they will arrive at some beliefs about the nature of the cup, which will cohere with each other. Since the object of study (the cup of our example) is one and real, and as a matter of fact, its existence and nature is independent of the human vagaries, the opinions about it will cohere and eventually arrive at the same result. Otherwise, if this conception was a mental construction, they would have arrived at different beliefs.

The same test of externality, which we have seen that Peirce has suggested, is applied to the community of inquirers in order to stabilize the beliefs. He quotes the following example to support his argument:

> 'A dozen ways measuring the velocity of light, if they are accurate will eventually yield the same result, because the object which different scientists study, perhaps by different kind of inquiry is one and is independent of the vagaries of individual men' (CP 7.335).

Certainly, this can be fulfilled only under the condition that the beliefs, which represent the 'real', will be agreed upon by the community of inquirers of all times, and that the inquiry will be pursued indefinitely far.

With this conditional argument Peirce exhibits the potential character of ideal, cooperative knowledge, which, as he says, although may never be actualized, it is destined to be fulfilled due to the logic of probability and to the growth of intelligibility in the universe. According to the logic of probability, the process of inquiry (it is destined) tends to converge on a limit, therefore, as he explains, the meaning of 'destined' is in the probabilistic sense, as follows:

> 'To be destined: which is sure to come about although there is necessitating reason for that. Thus a pair of dice, thrown often enough, will be sure to turn sixes some time,

[105] We can compare the different perspectives of the 'real' to Leibniz's different points of view of the *Monads*: '*And as the same town, looked at from various sides, appears quite different and becomes as it were numerous in aspects [perspectivement]; even so, as a result of the infinite number of simple substances, it is as if there were so many different universes, which, nevertheless are nothing but aspects [perspectives] of a single universe, according to the special point of view of each Monad*'. (Monadology 57, Montgomery, 2006).

> although there is no necessity that they should do. The probability that they will is 1' (CP 7.335).

According to Peirce's evolutionary metaphysics, the universe is growing in regularity and law (*Thirdness*), which implies that there is a gradual growth in reasonableness and intelligibility in the world. Since the world evolves from chance and irregularity (chaos) into regularity (habit) and reasonableness, as a consequence its reality is this intelligibility, which is not referring to the individual minds, but, rather, to the mind of community (CP 7.336). Furthermore, since each individual as object of inquiry is an *index (sign)* and is real representation of its kind, therefore its final representation (*final interpretant*) demands a community of inquirers and indefinite investigation. In order to verify this hypothesis Peirce appeals to the history of science, which has shown us that many questions concerning the phenomena have been successfully answered. Since scientists have attained a partial perfect knowledge about certain given questions, by analogue Peirce assumes that we would attain a universal knowledge or else perfect knowledge about any given question, with the condition that inquiry will be indefinitely carried out[106].

In my view, this analogy that Peirce draws, apart from interdisciplinary work that he adopts and supports with his principle of scientific community, as I said, it presupposes one single theory through the unification of sciences and many other valid premises, as follows: First, whether the different scientific theories that arise from particular disciplines allow that or have limits due to the different irreducible levels (layers) of the world. Second, whether can be solved the problem of different languages that each particular science uses, since the ideal single theory that could describe completely the 'real' must be expressed by a *universal* language, and third, whether there is such a guarantee that the world can be explained by a single theory that is true, taking into account the limits of our language and the finitude of minds of scientific community, compared with the increase of diversity of nature and the inexhaustibility of the laws as infinite potentialities. Because, granted fallibilism, the definition of the real is inexhaustible, this means without definite limits.

Peirce holds that the background knowledge of each inquirer, which contributes to the scientist's instinct, is based upon this social character of knowledge, because each one is

[106] By 'perfect knowledge' Peirce means that no matter how far inquiry is pushed, has no surprises for us in this question (CP 4.62). See also (CP 4.63):*'Perhaps we have already a perfect knowledge about a number of questions, but we cannot have an unshakable opinion that we have attained such perfect knowledge about any given question'.*

fully informed about the work of his contemporaries and predecessors, and he uses this information for his own inquiry, as we can read in the following citation:

> 'Coming down to the ore immediate and pertinent causes of the triumph of modern sciences, the considerable number of the workers and the singleness of heart with which … they cast their whole being into the service of science lead, of course, to their unreserved discussions with one another, to each being fully informed about the work of his neighbor, and availing himself of that neighbor's results; and thus in storming the stronghold of truth one mounts upon the shoulders of another who has to ordinary apprehension failed, but has in truth succeeded by virtue of the lessons of his failure' (CP 7.51).

That emphasis on cooperative endeavor raises the question of the nature of the community, which also involves ethical aspects. Because it presupposes that each individual guided by his love for truth and being aware of his existence in relation to the community[107], he is willing to sacrifice what is private and personal to him, and bind himself by the rules of an interpersonal method that involves free exchange of views and results. Apart from that, there is a practical reason for the social character of the inquiry, because the vastness and complexity of nature calls for cooperation among inquirers, as each one can specialize only in a certain object of inquiry. However, as I said, this conception of motivation within the scientific community is very ideal; therefore, in my opinion, have to be explored the institutional characteristics of the scientific community, which might secure its autonomous and inquiring role, and which might provide encouragement in original thinking.

Furthermore, as explained, what distinguishes the scientific method from the Cartesian *a priori* method, except the experimental character, is its public and cooperative character, which can secure the knowability of reality, so to say the adequate representation of the 'real'. Because the *a priori* approach to knowledge is a private affair, and as long as the belief is consistent with the apriorist's preconceived ideas, it will be considered as true, but when it is compared with another man's different belief, doubt will arise, which will never cease, and as a result belief will never be settled. Therefore the Cartesians appeal to immediate and intuitive ideas, and they claim that 'the very realities of things can never be known in the least' (CP 5.310). But for Peirce, as for Hegel, reality is knowable; therefore

[107] Peirce's view meets here Aristotle's notion that is referring to the double, composite nature of the human being, namely as rational (*λογικόν*: logikon) and social or political being (*πολιτικόν όν*: politikon on). The virtue of man as '*logikon on*' consists in the speculative activity of the pure intellect, while the virtue as a '*politikon on'* consists in practical wisdom that determines the virtue of the *mean (μεσότης).* See Barnes (1998), Nicomachean Ethics X, 1178-1179, Politics I, 1253, IV, 1295.

truth can be attained through an organic, dynamical and dialectical process, as *'reality is the dynamical reaction of certain forms upon the mind of community'* (CP 6.612).

Thus science is an actual process of inquiry, a continuous action by a community of investigators on actual problem posed by the independent reality. The awareness between the real and the unreal calls for a gradual comparison and criticism of views and results under long and public investigation, and it contributes to the self-correction and progress of science, as well as to the preservation of the objective and the universal at the expense of the private and subjective. Since this process requires a series of ideas and logical operations, it excludes any immediate or intuitive operation of *a priori* thought, as it goes beyond the boundary of any single or isolated idea. Therefore this community principle of Peirce concerns not only community of inquirers but also community of ideas that grow and develop gradually from the subjective to the objective and the universal (real).

Objections have been raised against Peirce's community principle, concerning both the definition of the 'real' as an ultimate opinion agreed upon by the community of inquirers and the identification of the 'real' with the future, which neglects the importance of the present experience. To begin with the latter, John E. Smith[108] argues that reality for Peirce is 'a process of experience that can be reached only in the future and never manages to establish itself in the present, which implies that the present integrity of the real individual is lost, although it is denoted by the indexical sign'.

It is true that the real individual is denoted by the sign of *index*, but its interpretation by each inquirer is partial and subjective, for it depends on the observer's background knowledge. According to Peirce's principle of habits, each one's background knowledge consists of his beliefs that are his habits and are formed under the influence of his previous personal experience. As a result, the individual interpretation of the 'real' through abductive inference is partial, subjective and erroneous, therefore there is a need of different observers, namely of the scientific community. Furthermore, one cannot doubt all his beliefs at once, and supposing that we overcome this difficulty, then comes another problem, i.e. what we perceive in the individual is the general and universal aspect, since we perceive it as a symbol of its kind (*We perceive what we are adjusted for interpreting*

[108] See John Smith, *Community and Reality,* in Wiener Philip P. (1952), *Studies in the Philosophy of Charles Sanders Peirce,* pp. 93-119.

CP 5.158), therefore its interpretation cannot be fully exhausted by one person or in the present.

Moreover, its complete interpretation in the present is impossible due to fallibilism arising from the existence of chance in nature, as previously explained. Although nature evolves in the direction of increasing law, regularity and uniformity, this uniformity is not universal and exists only within a limited range of events, while an element of pure chance survives and will remain in the indefinitely distant future (CP 6.33). Inductive inference as a generalization on the basis of a limited number of individual samples, which represents the whole population of samples, if the uniformity of nature existed, it would have been infallible, but it is not; therefore it is only approximation to the truth

Finally, history of the race has shown us that there has been a transition from a primitive mode of life, when instinct was more influential, to our more advanced culture, in which instinct is subject to deliberate self-control. On the other hand, history of science has also shown us that there has been a gradual growth in the human knowledge as a whole from a more acritical acceptance of beliefs to the criticisms that the verification process imposes on the free and brilliant suggestions of instinct (CP 5.442).

As for the first objection, I have already shown that according to the definition of the ‘real’ in relation to the *object* of thought, is that the ‘real’ is independent of thought, whereas its definition in relation to its subject, namely the inquirer (interpretant), since the object has influence on the inquirer, is that is dependent on the mind of the community for its knowability. I have already expressed my objections about the guarantee of this knowability, because, if the ‘real’ is independent of our thought, it follows that we have no access on that directly, but rather indirectly through the scientific method. But in the second case, even the approximate knowability presupposes one single true theory, a notion that is also based upon ambiguous premises.

Moreover, granted Peirce’s fallibilism, the definition of the ‘real’ should be without definite limits and scientific community should contribute to the growth of knowledge and not to the finalization of theories. Peirce in his late works became aware of this difficulty and tried to correct this view, however the contradiction remained. As he states, with his community principle he does not mean the simple and definite collection of minds[109], but an actual process of knowledge, and that the settlement of opinion agreed by a community of scientists is not definite and limited, but rather temporary and open to further reaffirmation and denial[110]. In a letter to Lady Welby written at the end of 1908, he writes:

> 'I do not say that it is infallibly true that there is any belief to which a person would come if he were to carry his inquiries far enough. I only say that that alone is what I call Truth. I cannot infallibly know that there is any Truth'.

From this point of view, the definition of the real, as an ultimate opinion agreed upon by the community of inquirers, can only be explained in an ideal context, namely, 'potentially agreed upon' though never being 'actualized', as we have seen, where by the use of the analogue of the diamond he argued:

> 'Just as a diamond that sits on the ocean floor destined never to be touched is hard, a belief which would have been in the final opinion, despite the final opinion never coming to pass, is true'.

2.7. Conclusion of the Chapter

We can infer from the previous discussion that the two definitions of the 'real', given by Peirce, are contradictory, because, if the 'real' is independent of our thought in accordance with the first definition, it follows that we have no access on that in order to know it. From this point of view, we can only have an indirect and approximate knowledge of it through the scientific method. Therefore the only possible and alternative explanation for the second Peircean epistemological definition of the 'real' is the following: the growth of knowledge within the scientific community could lead, potentially and not necessarily, to progressively better theories, which in the long run could approach the approximate representation of reality, asymptotically and not definitely. And those last theories would be agreed upon by the scientific community, as the outcome of consensus, and they could correspond to the approximate representation of the 'real'.

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[109] See CP 5.565: *'The mode of being by virtue of which the real thing is as it is, irrespectively of what a single mind or any definite collection minds may represent it to be'*; underline mine.

[110] See CP 5.311: *'And so those two series of cognition-the real and the unreal-consist of those which, at a time sufficiently future, the community will always continue to reaffirm; and of those which under the same conditions, will ever after be denied'.*

But this conception presupposes the unification of particular sciences and the explanation of the world by a single theory; it has to solve the problem of the language and of the inexhaustibility of regularities as infinite potentialities, which are also questionable, as I said. Apart from that, it presupposes that scientific community will carry out the inquiry in the indefinite long run autonomously. But Peirce's view of scientific community is very ideal, as explained; because it does not take into account that scientific communities, like all other communities, are governed by some ethical norms or values, and that the emotional motivation of each individual is very significant for the advancement of inquiry. Therefore the structure and the common values of the scientific community are very important for its autonomous and inquiring role. In this sense, Peirce's conception does not explore the necessary conditions and institutional characteristics of the scientific community, which can secure its independent, autonomous, democratic and inquiring role. However his notion of the scientific community gave rise to the introduction of sociology of science and contributed to the development of social sciences in that direction.

With regard to abduction, as I have shown, the classification of the particular event to one of the general classes in abduction presupposes the knowledge of all those different general classes. And this ability to recognize universal qualities or classes we have acquired by generalization and after having perceived many members of the class, that is, by inductive generalizations; therefore abduction is based partly upon crude induction. But this does not underestimate the important function of abduction, which is the one that leads to the inference about causal relation and classification of entities; therefore abduction is the only kind of inferences that can introduce new ideas into our body of beliefs.

Because, as I said, according to the definition of *Thirdness*, as a causal principle of relation that exists between two entities under certain conditions, it can be justified the consequent-antecedent relation in abduction, therefore it allows us to infer from the known consequent about the unknown antecedent and to make predictions. Apart from that, the reality of *Thirdness*, in a sense of regularities or real potentialities of habits, it permits us to systemize the particulars in general laws. Furthermore, due to the existence of universal qualities, as dispositions of all things of the same class to behave in a certain way under certain conditions (Peircean *'would be's')* – though conditional and relative to our actions – we can classify particular events to one of the general classes, as long as these dispositions are proved by the inquiry to be real.

In sum, as I have shown, there is a problem with Peirce’s notion of convergence upon truth in the long run, and with his epistemological definition of the ‘real’ as an ultimate opinion. However his conception of the SCT is independent of these notions, because it can work without those presuppositions, therefore its validity is not affected by these problematic premises. On the other hand, we cannot ignore the fact that history of science has shown us that there is an increase of knowledge, which has gradually led to better theories that represent approximately the regularities in nature, with which each particular discipline is busied. In order to justify this claim, I am going to proceed to the next Chapter and discuss the justification of Peirce’s SCT.

3. Justification of the SCT

After having provided the required background of Peirce's principles, I can proceed now to the justification of the SCT, which includes the initial Peirce's notions and their evaluations that followed the development of his thought. Afterwards I discuss this justification of the SCT given by Peirce in comparison with the objections against it, which concern each phase of inquiry, so as to defend it and to distinguish in which context SCT is valid.

3.1. Development of Peirce's Self-Corrective Thesis

Peirce in his earlier papers treated induction, deduction and abduction as independent forms of inference and held that only induction is self-corrective if it persisted in long enough, because, as he said, 'by enlarging the number of samples the ratio will inevitably stabilize to the approximate one'. Here are some typical citations of his early view:

> 'If (induction) duly persisted in, must in the very nature of things, leads to a result indefinitely approximating to the truth in the long run' (CP 2.281.1878).
> 'The ratio (which is its conclusion) may be wrong because the inference is based on but a limited number of instances, but on enlarging the sample the ratio will be changed till it becomes approximately correct' (CP 2.709. 1878).
> 'Nor we must lose sight of the constant tendency of the inductive process to correct it. This is of its essence. This is the marvel of it' (Studies in Logic, Probable inference, 1883).

However, in his later writings, as his thought was developed and matured – after his study of the Logic of Sciences[111] from 1878 to 1885 – induction, deduction and abduction became closely interlinked and contributed to self-correction. In the year 1903 he clearly states that the three types of inference are complementary to one another, since only if they are used in concert would lead in the long run to the truth.

> 'Persistent and judicious use of <u>abduction, deduction and induction in concert</u> would lead from the arbitrary state of belief, however erroneous, to knowledge of the truth' (CP 7.327. 1903; underline mine).

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[111] I am referring to his works: *The Fixation of Belief* (1877), *How to Make Our Ideas Clear* (1878), *The Doctrine of Chances* (1878), *The Probability of Induction* (1878), *The Order of Nature* (1878), *Deduction, Induction and Hypothesis* (1878), *Studies in Logic* (1880-85).

Peirce began to explore more the self-correcting properties of induction, deduction and abduction (which he called it also *retroduction*), as well as the contribution of these three types of inference to support beliefs, in his lecture *The First Rule of Logic* given in 1898, which is published in parts in CP 5.574-79 and 7.135-40. In this work, as he claims, deduction is self-corrective, since it can correct its conclusion, when the premises are true, and vice versa. Peirce appeals to Aristotle's works *Prior and Posterior Analytics* to support this argument.

> 'Reasoning tends to correct itself, and the more so the more wisely its plan is laid. Nay, it not only corrects its conclusions, it even corrects its premises' (CP 5.574. 1898).

However, although deduction is necessary reasoning, it is subject to error, as he explains with the example of 'theoretical acceleration of the moon's means motion that deceived the astronomers[112] for more than a half century' (Ibid). In that case deductive inquiry can correct its errors by means of observation and experiment just like induction. The difference is that in deduction we make only one diagrammatic experiment, while in induction we have to enlarge the number of samples. To justify this claim he argues, for instance, in a question: 'how many rays can cut four rays fixed in space', an experienced mathematician would content himself with performing only one experiment to discover that are only two, unless the rays are so situated that an infinite multitude of rays will cut them, and the latter can be found out through observation made upon a diagram.

The same is true with abduction, however, as he says, here we adopt a hypothesis to explain facts, which is not final, but it is subject to correction. Because there are various inconsistent ways of explaining the same facts, which means that the same hypothesis that explains some facts, it may in the future lead us to erroneous expectations about other facts. Therefore hypothesis is adopted on probation and receives gradually modifications and corrections after its verification, as he says:

> 'But as our study of the subject of the hypothesis grows deeper, that hypothesis will be sure gradually to take another color, little by little receive modifications, corrections, amplifications, even in case no catastrophe befalls in' (CP 5.579-82. 1898).

[112] Peirce is referring here to J. L. Adams '*On the Secular Variation of the Moon's Mean Motion*', in Philosophical Transactions of the Royal Society of London, 143 (1853), pp. 397-406.

After the consideration of the role of observation in self-correction Peirce concludes: *Thus it is that inquiry of every type, fully carried out, has the vital power of **self-correction** and of growth' (Ibid)*. But in this paper he does not provide any justification for the self-corrective character of abduction except his notion of the 'affinity of human mind with nature', and his thesis 'the will to learn', namely the one who desires to learn, he will learn[113]. As for the criteria of admissibility of hypotheses, he presents them three years later in his work (1901) '*On the Logic of Drawing History from Ancient Documents, Especially from Testimonies*'.

There, Peirce connects the three types of inference by proposing their application to certain phases of inquiry. The first step (abductive) is the adoption of the hypothesis as being suggested by the facts that aims at explaining these facts, as he states: *'This step of adopting a hypothesis suggested by the facts, is what I call abduction'* (MS 690 or CP 7.164-231.1901, '*On the Logic of Drawing History from Ancient Documents, Especially from Testimonies*'). The next step that follows is the deductive step, i.e. tracing out the experimental consequences of the adopted hypothesis:

> 'The first thing that will be done, as soon as the hypothesis has been adopted, will be to trace out its necessary and probable experimental consequences. This step is deduction' (Ibid).

After having drawn the ideal predictions (expectations) from a hypothesis by deduction, as he states, the next step is to make the experiments and compare these predictions with the actual results of experiment, and this step is the inductive step of inquiry.

> 'This sort of inference it is, from experiments testing predictions based on hypothesis, that is alone properly entitled to be called induction' (Ibid).

Peirce in this work presents also the different sorts of induction (qualitative and quantitative), and he concludes that they are self-corrective in nature, but each one in different sense. Quantitative induction if is persisted in the long run of cases, after a random sample has drawn from a finite collection of members, it will correct any error because it is *'morally certain'* that it would discover a ratio of frequency to where the event would

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113 See CP 5.582. 1898: *'If you really want to learn the truth, you will, by however devious a path, be surely led into the way of truth, at last. Now matter how erroneous your ideas of the method may be at first, you will be forced at length to co correct them so long as your activity is moved by that sincere desire'.*

approximate. This means that the ratio of frequency of an event is morally certain that would indefinitely (in the long run) converge toward a definite limit, as he says:

> 'Now, what is meant by the long run? The phrase is only used in saying that the ratio of frequency of an event has such and such value in the long run… If the occasion referred to upon which the event might happen were to recur indefinitely, ..., as the occasions went on, would indefinitely converge toward a definite limit' (Ibid; underline mine).

'Morally certain' is meant here that the probability of the event is 1, but it is not absolute certain. On the other hand, qualitative induction has nothing to do with probabilities, therefore if persisted in will correct any error, not gradually but certainly, as he states:

> 'The second genus of induction comprises those cases in which the inductive method if persisted in will certainly in time correct any error that it may have led us into; but it will not do so gradually, inasmuch as it is not quantitative' (Ibid).

Apart from that, Peirce here presents his criteria of admissibility of hypotheses that enable the inquirer to choose the right hypothesis among the innumerable ones, as explained in the relevant Chapter. Furthermore, he presents here his 'hope' doctrine, which means that our mind will be able to guess among the innumerable explanations the sole true explanation of them. This principle is based upon his hypothesis that the human mind has affinity with nature as a development of the instincts.

Peirce returns to the discussion of the three forms of inference (abduction, deduction and induction) in his *Harvard Lectures of Pragmatism* (26 March-17 May 1903) and especially in the fifth Lecture (*The Three Normative Sciences*; CP 5.120-50. 1903), where he connects clearly these three sorts of scientific inference and explains the special features of each one. Moreover, he confesses that in his early writings has wavered about their reducibility; therefore he states that induction and abduction neither are reducible to the other nor to deduction, as well as that deduction is not reducible to either of them. However, the *rationale* of both induction and abduction is essentially deductive, since their conclusion premise that follows the rule and the case in their syllogism is necessary, if those two premises are valid.

> 'Among these opinions I have constantly maintained is that while abductive and inductive reasoning are utterly irreducible, either to the other or to deduction, or deduction to either of them, yet the only rationale of these methods is essentially deductive or necessary. If

> then we can state wherein the validity of deductive reasoning lies, we shall have defend the foundation of logical goodness of whatever kind' (CP 5.146. 1903).

Furthermore, he distinguishes their characteristics, as follows: abduction is the only inference that brings new elements in the inquiry, because after studying the facts devises an explanatory theory, while deduction is the only necessary reasoning among the three, therefore it can draw the ideal and necessary consequences of the adopted theory. As for induction is the experimental testing of a theory, since it measures quantitatively the concordance of that theory with the facts, and as a result with further application it corrects the error of the ideal conclusion of deduction, as we can see below:

> 'Deduction is the only necessary reasoning. It is the reasoning of mathematics. It starts from a hypothesis (adopted by abduction) … and of course its conclusions are equally ideal… Induction is the experimental testing of a theory. . . It sets out with a theory and it measures the degree of concordance of that theory with fact. It never can originate any idea whatever. No more can deduction. All ideas in science come to it by the way of abduction. Abduction consists in studying facts and devising a theory to explain them' (CP 5.145. 1903; parenthesis mine).

In the following sixth Harvard Lecture, *The Nature of Meaning*, Peirce returns to the three types of inference in order to explore further their validity, the relation of each type of reasoning to perception and their application to the different stages of inquiry. Here he claims that deduction is the necessary reasoning of mathematics, it is always diagrammatic, it starts from a hypothetical state of things defined by abstract terms, namely an *icon*, and it proceeds to observe whether this hypothetical state is true or not. In other words, it is related to the sort of the *signs* called *icons*, since its representation is based on analogy and similarity, and its observation is always diagrammatic, therefore we pay no attention the concordance with the outward world. It is always valid, as long as there is a relation between the states of things supposed in the premises (rule and case) and the states of things stated in the conclusion. However, its certainty is not due to the reality of perceptual judgments (like abduction), but *'is due to the circumstance that it relates to objects, which are creations of our minds' (MS 314, 316, The Nature of Meaning, 1903).*

> 'All necessary reasoning is without exception diagrammatic. That is, we construct an icon of our hypothetical state of things and proceed to observe it' (Ibid).

As for induction, Peirce in the same lecture repeats that it is the experimental testing of a theory, and he distinguishes the application of its probability ratio. He repeats, as explained, that the probability ratio *'applies to the question whether a specified kind of event will occur when certain predetermined conditions are fulfilled' (Ibid),* but not to a theory or a law. *'But you cannot ask what the probability is that the law of universal attraction should be' (Ibid).* In this lecture he uses the word 'induction' in a broad sense[114] and connects clearly the three sorts of inference with the three phases of scientific inquiry: *'Induction consists in starting from a theory (abduction), deducing from it predictions of phenomena (deduction), and observing those phenomena in order to see how nearly they agree with the theory* (induction in a narrow sense)' (MS 314, 316 *The Nature of Meaning*; parentheses mine). Apart from that, he provides a justification for the validity of induction (applied in the long run), which is based on the necessary relation between the general and the singular. He argues that all endless series of objects or events that have beginning and no end must be discoverable to an approximation by examining a sufficient finite number of cases at the beginning of the series. For, as he claims:

> 'whatever has no end have no mode of being other than that of a law,.., and the only way of describing an endless series is by stating explicitly or implicitly the law of the succession, …, if it presents any regularity for all finite successions from the beginning, it presents the same regularity throughout' (Ibid).

By the use of the mathematical law of a large numbers and series Peirce tries to demonstrate the justification of induction, which, when it is applied in a finite number of experimental testing in the long run, can show its character and its limit, without the need of exhausting the whole series of cases.

With regard to abduction, Peirce here repeats his notion that it is the only inference that contributes to discovery, for all new ideas come by the way of abduction. At this point Peirce connects clearly the three sorts of inference in the process of scientific inquiry, as follows:

> 'Deduction proves that something must be, Induction shows that something actually is operative, Abduction merely suggests that something may be. Its only justification is that from its suggestion deduction can draw a prediction, which can be tested by induction' (Ibid).

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[114] Peirce uses the term 'induction' sometimes in a narrow sense, namely the inductive experimental testing phase of inquiry, and sometimes in a broad sense, as a synthetic inference, which involves all the three phases of scientific inquiry (abduction, deduction and deduction in a strict sense). In this text is apparent that he means the induction in a broad sense.

Here he repeats that the validity of abduction lies in the affinity of human mind with nature as a development of the instincts, a notion that he tries to demonstrate here with historical arguments. He namely claims that among the trillions of hypotheses, which might be made, the scientists after some dozens of guesses hit nearly always on the right hypothesis. Because if this guess was by chance each scientist would have needed some millions of years to form the correct hypothesis about a certain problem. In order to support this notion, except this argument, Peirce introduces for first time a logical account based on the ontology of *Thirdness*. He claims that general conceptions are given in perceptual judgment, which can only be the predicate or an element of the predicate; therefore abduction is the only process by which a new element can be introduced into thought. This account he elaborates further in the last seventh Harvard lecture, namely *Pragmatism as the Logic of Abduction*.

In this lecture Peirce elaborates the three crucial propositions, which are the basis of pragmatism, as follows: *1. nothing is in the intellect that is not first in the senses*[115]*, 2. perceptual judgments contain general elements and 3. abductive inference shades into perceptual judgment without any sharp line of demarcation between them.* (CP 5.180-81.1903; *Pragmatism as the Logic of Abduction*). As for the second proposition, it is already explained in the previous lecture that perceptual judgments contain general elements, therefore universal propositions can be necessarily inferred from them, otherwise, if not, only particular ones could be inferred from them. The crucial proposition, whose truth is important for the development of the Self-Corrective Thesis, is the third one; therefore Peirce elaborates it here in detail.

First, he has to show that there is relation between abduction and perceptual judgment. By the use of the example of visual illusions he argues that all the different interpretations of an elusive figure are given in a perception, however, unconsciously we choose the one which is the general aspect. For example, the following Figure 1, which Peirce quotes[116], although is a serpentine line drawn continuously, when it is completely drawn, it appears to be a stone wall, and we interpret it so.

[115] Peirce states that this proposition is taken from Aristotle's *On the Soul*, 432a, 3-8 (*εν τοις είδεσι τοις αισθητοίς τα νοητά έστι*: en tois eidesi tois aesthetois ta noeta esti). However, with intellect (*νοητά*: noeta) he understands the meaning of any intellectual representation in any form, virtual, symbolic or whatever.

[116] Charles Peirce in CP 5.183.1903, *Pragmatism as the Logic of Abduction*, sketches here (Fig. 1) an example of his father (Benjamin Peirce, Prof. of Mathematics at Harvard College) serpentine line, recalling his lecture on *Potential Algebra*.

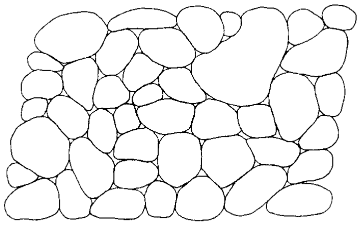

Fig.1

This example shows that perceptual judgment is related to abductive inference, for the proper interpretation is nothing but abduction. Furthermore, our preference of the one mode of the interpretation, which is in the context of generalization (stone wall), it shows us that general elements are contained in the perceptual judgment. Therefore, we sometimes perceive objects differently from how they really are, since we accommodate them to their manifest intention, for example, we miss seeing misprints, because we misinterpret them unconsciously. On the other hand, this shows that the abductive inference, which arises from the perceptual judgment, is fallible and subject to further correction after many repetitions of the experiment e.g. in our case proofreading. Moreover, in this lecture Peirce claims that not only every general element is given in the perception, but also the form of putting concepts together and forming the hypothesis is given in the perception.

To support this claim he examines by the use of the norms of logic the supposed three objections: (1) the normative justification of abduction according to the laws of logic, (2) the acceptance of every however fantastic hypothesis and (3) the entailment of the antecedent in the perceptual judgment. As for the first objection, as explained in the previous lecture, according to the norms of logic, a fallacy in the conclusion of a syllogism arises when a fallacy is contained in the premises (law and case). In the same way a weak argumentation in the premises lead to weak conclusions, therefore the fallacies in abduction are due to weak argumentations, which are given in perceptual judgment and are contained in the premises of abduction. This is the reason that we cannot accept any hypothesis when is presented itself (objection 2), but through deduction we colligate the different perceptual judgments into a copulative proposition and bring the certain parts into more intimate connection[117]. In this sense, in scientific inquiry after the abductive inference, it is always required to draw the deductive consequences of a hypothesis.

[117] See CP 5.193. 1903; *Pragmatism as the Logic of Abduction: 'Deduction accomplishes first the simple colligation of different perceptive judgements into a copulative whole, and then with or without the aid of*

As for the third objection, which Peirce regards as the most serious one, he argues by the use of reduction ad absurdum that, if the antecedent was not given in the perceptual judgment, it must have been inferred from the premises as conclusion. But each mode of inference is characterized by self-control, and since self-control is inhibitory, therefore it originates nothing. It follows, then, that the antecedent must come from the uncontrolled part of the mind, or else where the perceptual judgments are formed. To elaborate further this argument Peirce associates it with the Category of *Thirdness*, since, as he claims, all the forms of logic can be reduced to combinations of the forms of *Firstness, Secondness* and *Thirdness*. Unquestionably the forms of *Firstness* and *Secondness* are given in perception, but what remains to be shown by Peirce is that the form of *Thirdness*, which is associated with perceptual judgment and as a matter of course with abduction, it is directly perceived and from which the other cotary propositions cannot be separated. Therefore he examines the three alternative following propositions:

> '(1) Although Thirdness is an element of the mental phenomenon ought not to be admitted to the theory of the real, because is not experimentally verifiable, (2) Thirdness is experimentally verifiable, since inferable by induction, but it cannot be directly perceived and (3) it is directly perceived and from which the other cotary propositions cannot be separated' (CP 5.209-12. 1903).

As for the first proposition, if one admits that *Thirdness* is only a mental construction, he has to abstain from any prediction, but, as it is known, the laws are operative in nature and the knowledge of them allow us to make predictions. Therefore, since this method of predictions it has been successfully worked in the history of science, this proposition is in contradiction with the practice of scientific method.

With regard to the second proposition, it implies that *Thirdness* although is not perceived in experiment, it is justified by experiment. Its contradiction lies in separating reality from perception, as well as in rejecting the notion of continuity. For, according to the second proposition, reality would be the instant and ultimate result of inquiry, but not a continuous sequence of events, a notion that implies that the contents of time consists of separate and unchanging states. Apart from that, taking into account that the 'real' is independent of any man's opinion on that, and rejecting the whole process to be real, then one has to admit that

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other modes of inference, is quite capable of so transforming this copulative proposition so as to bring of its certain parts into more intimate connection'.

the instant and final interpretation is better than all the previous, but he cannot claim that is real, for he rejects the whole process.

But the one who admits to the third proposition, he acknowledges that the contents of perceptual judgment cannot be controlled, since they arise from the uncontrolled part of the mind, therefore abductive inference shades into perceptual judgment without any sharp demarcation line between them. Certainly, the consequence of this proposition is that one cannot know how to exclude from hypothesis everything unclear and nonsensical. But since there is a relation between the elements of action and action itself, for both are given in perception, one can apply the pragmatic maxim of purposive action, which is equivalent to a conception of 'conceivable practical effects', in order to distinguish the unclear and uncontrolled from the logically controlled hypotheses. In this same lecture Peirce delivers his famous dictum:

> 'The elements of every concept enter into logical thought at the gate of perception and make their exit at the gate of purposive action; and whatever cannot show its passports at both those gates is to be arrested as unauthorized by reason' (CP 5.212. 1903).

In this way, based on his theory of perception Peirce connects abduction with *Thirdness* and action, a notion which is the basis of pragmatism. However, in his later works after the Harvard lectures, Peirce bases his pragmatism more on his theory of signs than of perception. Especially in his work *'A Syllabus of Certain Topics of Logic'*[118] he classifies the three types of reasoning under the types of *signs* called *arguments*. Therefore in his '*Sundry Logical Conceptions'* he states: *'Arguments can only be Symbols, not Indices nor Icons. An Argument is a Deduction, an Induction or an Abduction' (MS 478. 1903; Sundry Logical Conceptions).*

In this same work Peirce clearly defines the three sorts of reasoning as complementary to one another that work together in the course of scientific inquiry, which begins with abduction, continues with deduction and ends up with induction. As he describes the process of inquiry, begins with the surprising fact that beaks up the expectation of belief, and in this phase comes abduction as an act of generalization, which aims at suggesting a new conception that embraces and explains the new phenomenon.

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[118] This work was composed mostly in October 1903 to supplement the Lowell lectures on *'Some Topics of Logic'* delivered 23 Nov. – 17 Dec. 1903, and contains six sections among them *'A Syllabus of Certain Topics of Logic'* and *'Nomenclature and Divisions of Triadic Relations'*.

> 'The whole operation of reasoning begins with Abduction, which is now to be described. Its occasion is a surprise. That is some belief, active or passive, formulated or unformulated, has just been broken up…. The mind seeks to bring the facts, as modified by the new discovery, into order; that is, to form a general conception embracing them… This synthesis suggesting a new conception or hypothesis, is the Abduction' (Ibid).

For first time Peirce introduces in this lecture the trichotomy of *signs* in relation to its *interpretant*: (1) *sumisigns* (later called *rhemes*), (2) *dicisigns* and (3) *arguments*. He defines the conclusion of the abductive syllogism as the *interpretant* of the abduction, which is a *symbol* and entails a general concept that approaches the truth in an indefinite sense, since the phenomena are like (constitute an *Icon*, or a replica of) this general conception, namely the *symbol*; therefore abduction cannot assert the truth, but only implies it[119].

But as the hypothesis drawn in the conclusion of the abductive syllogism does not assert the truth and it is only imaginary, it is now the turn of deduction, which examines the necessary consequences of this hypothesis. In other words the hypothesis, as case of a deductive syllogism, has to be brought under a known or universal truth (law), which is the rule of syllogism, so as to draw the virtual effects or predictions of this law in the conclusion, as explained with the example:

> All glasses are hard (x) and breakable (y) (rule)
> If this thing is a glass (case)
> Then it should be hard (x) and breakable (y) (result)

Peirce describes it as follows:

> 'Abduction having performed its work, it is now Deduction's turn… The case will be brought under a known or evident truth. Thus the argument sets out from a law represented to be known actually to hold throughout the universe of the hypothesis, and in the conclusion interprets the effects of this law' (Ibid).

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[119] See *MS 478. 1903: Sundry Logical Conceptions: 'It is recognized that the phenomena are like, i.e. constitute an Icon of, a replica of general conception, or Symbol. This is not accepted as shown to be true, nor even probable in the technical sense… but it is shown to be likely, in the sense of being some sort of approach to the truth in an indefinite sense… This conclusion, which is the Interpretant of the Abduction, represents the Abduction to be a Symbol, - to convey a general concept of the truth, - but not to assert it in any measure'.*

Since deduction draws the necessary and virtual predictions of a hypothesis, namely the expectations of a hypothesis, what remains to be examined, is whether these predictions are fulfilled in experience, so as to prove the value of true of a hypothesis. Therefore after the deductive predictions of a hypothesis, as Peirce puts it, comes the work of induction, which consists in conducting the experiments in order to see how good the hypothesis holds.

> ‘Deduction produces from the conclusion of Abduction predictions as to what would be found true in experience in case that conclusion were realized. Now comes the work of Induction, which is not to be done while lolling in an easy chair, since it consists in actually going to work and making the experiments, thence going to settle a general conclusion as to how far the hypothesis holds good’ (Ibid).

Peirce in the next section of the Syllabus, ‘*Nomenclature and Divisions of Triadic Relations’ (*published in *MS 540* and in *CP2.233-2.272. 1903)*, completes the semiotic trichotomy of the signs and he quotes his ten-fold classification of the signs. Here he also gives an account of the class of signs in relation to its *interpretant* called *argument*, which must be both *legisign* (sign in itself) and *symbol* (sign in relation to its object). In the context of his semiotic theory, he explains now his division of arguments into deductions, inductions and abductions, and quotes two types of deduction and three types of induction.

In this section he also divides *deduction*, according to the *Dicent* symbol they produce, into *Necessary* and *Probable*. *Necessary* deduction produces true conclusions from true premises and forms a *Dicent* symbol by the study of a diagram. It is further divided in *Corollarial* and *Theorematic*. The first one represents the conditions of the conclusion in a diagram and through its observation finds the truth of the conclusion, whereas the second one performs an experiment upon the diagram and through observation of the modified diagram ascertains the truth of the conclusion. *Probable* deductions, on the other hand, are concerned with ratios of frequency and are divided in *Statistical* deductions and *Probable deductions proper*. The first one, although is concerned with ratios of frequency, its conclusion is certain, because it concerns theories, while the second sort of Probable deduction is the one that in the long run lends its probability ratio to the conclusion (CP 2.267-68; *Nomenclature and Divisions of Triadic Relations, 1903*).

As for *induction*, it is a method that produces a Dicent symbol; however, if the method persisted in, it will in the long run yield an approximation to the truth in its conclusion. Peirce here divides induction in three sub-kinds: (1) *Pooh-pooh Argument*, (2) *Experimental Verification of a general Prediction* and (3) *Argument from a Random Sample*. *A Pooh-pooh Argument* is a method which consists in denying that a general kind of event ever will occur on the ground that it never has occurred. This type of induction is, as explained, the *crude induction*, which is self-corrective, since it must ultimately be corrected in case it should be wrong, and thus will ultimately reach the true conclusion. For example, the argument 'the sun will rise tomorrow' will be corrected, if one day 'the sun will not rise'.

Experimental Verification of a general Prediction is the *qualitative induction*[120], as 'it consists in finding or making the conditions of the prediction and in concluding that it will be verified about as often as it is experimentally found to be verified' (CP 2.269; *Nomenclature and Divisions of Triadic Relations' 1903*). In other words, it will be observed whether certain qualities are present in a large number of samples in order the hypothesis to be verified. This sort of induction is also self-corrective, since in the long run the experiment will ascertain approximately the proportion of cases that the quality is present or not.

An *Argument from a Random Sample* is a method of ascertaining what proportion of the members of a finite class possess a predesigned or virtually predesigned quality, by selecting instances from that class according to a method, which will present any instance as often as any other, and concluding that the ratio found for such a sample will hold in the long run' (CP 2.269; *Nomenclature and Divisions of Triadic Relations, 1903*). This one is the *quantitative induction*, which Peirce also calls statistical induction[121]. Quantitative

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[120] Compare also with CP 7.110-120. 1903: '*The second order of induction consists in the argument from the fulfilment of predictions … The strength of an argument of the Second Order (qualitative induction) depends upon how much the confirmation of the prediction runs counter to what to what our expectation would have been without the hypothesis. It is entirely a question of how much; and yet there is no measurable quantity*', and with CP 2.759. 1905: '*The remaining kind of induction, which I shall call Qualitative Induction, is of more general utility than either of the others, while it is intermediate between them, alike in respect to security and to the scientific value of its conclusions. In both these respects it is well separated from each of the other kinds. It consists of those inductions which are neither founded upon experience in one mass, as Crude Induction is, nor upon a collection of numerable instances of equal evidential values, but upon a stream of experience in which the relative evidential values of different parts of it have to be estimated according to our sense of the impressions they make upon us*'.

[121] See CP 7.110-120. 1903: '*The third order of induction, which may be called Statistical Induction, differs entirely from the other two in that it assigns a definite value to a quantity. It draws a sample of a class, finds a numerical expression for a predesignate character of that sample and extends this evaluation, under proper qualification, to the entire class, by the aid of the doctrine of chances. The doctrine of chances is, in itself, purely deductive. It draws necessary conclusions only. The third order of induction takes advantage of the information thus deduced to render induction exact*'.

induction is self-corrective because if persisted in the long run of cases, after a random sample has drawn from a finite collection of members, it will correct any error, as it is certain that would discover a ratio of frequency to where the event would approximate.

Peirce returns two years later to the same point and connects closely the three sorts of reasoning in the course of inquiry in his *Letter to Calderoni*[122]*(CP 8.209. 1905),* where he states that after the abductive conjecture and the deductive consequences of the hypothesis through induction, we can finally approach certainty concerning the reality of the theory. Therefore he calls inductive method as the only essential to the ascertainment of the intellectual purport of any *symbol*. However, this does not imply that induction alone can stand for the whole inquiry, since, for Peirce, all new ideas in science have come through abduction. Therefore he repeats this notion three years later in his *'A Neglected Argument for the Reality of God', CP 6.475, 1908,* where he now calls abduction *Retroduction*[123] or hypothesis:

> ‘Observe that neither Deduction nor Induction contributes the smallest positive item to the final conclusion of the inquiry. They render the indefinite definite; Deduction Explicates; Induction evaluates: that is all… Yet every plank of its advance is first laid by Retroduction alone, that is to say, by the spontaneous conjectures of instinctive reason; and neither Deduction nor Induction contributes a single new concept to the structure’.

Here in this work he analyzes more the interconnection between these three forms of inference by naming retroduction (abduction), deduction and induction respectively first, second and third stage of inquiry.

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122 *‘Abduction having suggested a theory, we employ deduction to deduce from that ideal theory a promiscuous variety of consequences to the effect that if we perform certain acts, we shall find ourselves confronted with certain experiences. We then proceed to try these experiments, and if the predictions of the theory are verified, we have a proportionate confidence that the experiments that remain to be tried will confirm the theory… Induction gives us the only approach to certainty concerning the real that we can have. In forty years diligent study of arguments, I have never found one which did not consist of those elements’.*

123 See *Letter to Paul Carus*, CP 8.227-228, two years later in 1910, where Peirce criticizes the logicians that they have not recognized abduction (hypothesis) as a valid sort of inference, a mistake that he also made when he confused induction with abduction: ‘*the division of the elementary kinds of reasoning into three heads was made by me in my first lectures and was published in 1869 in Harris's Journal of Speculative Philosophy. I still consider that it had a sound basis. Only in almost everything I printed before the beginning of this century I more or less mixed up Hypothesis and Induction . . . The general body of logicians had also at all times come very near recognizing the trichotomy. They only failed to do so by having so narrow and formalistic a conception of inference (as necessarily having formulated judgments for its premises) that they did not recognize Hypothesis (or, as I now term it, retroduction) as an inference’.*

Inquiry begins with a conjecture that furnishes a possible explanation, namely a syllogism that exhibits the surprising fact as necessary consequent, so to say, the adopted as plausible conjecture is the hypothetical antecedent of the observed wonder externally or internally. But this hypothesis is subject to further evaluation, since due to the lack of experience it is fallible.

> 'Plausibility, I reckon as composing the First Stage of Inquiry. Its characteristic formula of reasoning I term Retroduction, i.e. reasoning from consequent to antecedent' (A Neglected Argument for the Reality of God, CP 6.469-470, 1908).
> 'Retroduction does not afford security. The hypothesis must be tested' (Ibid).
> 'I call this mode of inference, or, if you please, this step toward inference, in which an explanatory hypothesis is first suggested, by the name of retroduction, since it regresses from a consequent to a hypothetical antecedent' (A Neglected Argument for the Reality of God, MS 842: 29-30, c).

Deduction constitutes the second stage of inquiry that collects the consequents of the hypothesis, whose first step is the *Explication* of the hypothesis, which means to render it as perfectly distinct as possible. The second step is *Demonstration*, or *Deductive Argumentation* that consists in analyzing the meanings of the sign, which should be *Corollarial* or *Theorematic*, as already explained in his work '*Nomenclature and Divisions of Triadic Relations'*.

> 'This constitutes the Second Stage of Inquiry. For its characteristic form of reasoning our language has, for two centuries, been happily provided with the name Deduction. Deduction has two parts. For its first step must be by logical analysis to explicate the hypothesis, i.e. to render it as perfectly distinct as possible. This process, like Retroduction, is Argument that is not Argumentation. But unlike Retroduction, it cannot go wrong from lack of experience, but so long as it proceeds rightly must reach a true conclusion. Explication is followed by Demonstration, or Deductive Argumentation' (A Neglected Argument for the Reality of God, CP 6.470-472, 1908).

This interpretation of the meaning of the sign is carried out by attaching pure ideas to the signs[124] and ends up with collecting the consequences of the hypothesis. As Peirce admits, the validity of deduction is correctly analyzed by his master Kant[125] in his *Critique of Pure*

[124] See CP 6.472-473, 1908, *A Neglected Argument for the Reality of God: 'The validity of Deduction was correctly, if not very clearly, analyzed by Kant. This kind of reasoning deals exclusively with Pure Ideas attaching primarily to Symbols and derivatively to other Signs of our own creation'.*

[125] In his earlier work Peirce admitted that he had learned philosophy from Kant, therefore he named his new theory *pragmatism* instead of *practicism* or *practicalism*: *'Some of his friends wished him to call it practicism or practicalism (perhaps on the ground that* πρακτικός *(praktikos) is better Greek than* πραγματικός *(pragmatikos). <u>But for one who had learned philosophy out of Kant, as the writer</u>, along with nineteen out of*

Reason (A7, 303-305 and B11, 360-61), therefore he does not need to provide any arguments.

After having collected the consequents of the hypothesis follows the third stage of inquiry, which has to ascertain the accordance of those consequents with experience, so as to judge the hypothesis, which means that induction through experimental probation has to prove if the hypothesis is true or false or it has to be modified.

> ‘The purpose of Deduction, that of collecting consequents of the hypothesis, having been sufficiently carried out, the inquiry enters upon its Third Stage, that of ascertaining how far those consequents accord with Experience, and of judging accordingly whether the hypothesis is sensibly correct, or requires some inessential modification, or must be entirely rejected. Its characteristic way of reasoning is Induction’ (A Neglected Argument for the Reality of God, CP 6.472-473, 1908).

In the same work Peirce quotes the three sorts of induction, namely crude, qualitative and quantitative induction, from which the last two characterizes gradual inductions. Crude induction is the weakest form of argument[126] and is founded upon experience in one mass, while quantitative is the strongest of all three, for it consists of measurements and statistics; therefore it can determine what the ‘real probability’ is that an individual member of a certain experiential class, say the S’s, will have a certain character, say that of being P’s. As for qualitative induction, it is based upon a stream of experience in which the relative evidential values of different parts of it have to be estimated according to our sense of the impressions they make upon us, as already explained in the *Neglected Argument for the Reality of God CP, 2.756-759.*

> ‘The Probations, or direct Inductive Argumentations, are of two kinds. The first is that which Bacon ill described as “inductio illa quæ procedit per enumerationem simplicem”… I call this Crude Induction. It is the only Induction which concludes a logically Universal Proposition. It is the weakest of arguments, being liable to be demolished in a moment, as happened toward the end of the eighteenth century to the opinion of the scientific world that no stones fall from the sky. The other kind is Gradual

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every twenty experimentalists who have turned to philosophy, had done, and who still thought in Kantian terms most readily, praktisch *and* pragmatisch *were as far apart as the two poles, the former belonging in a region of thought where no mind of the experimentalist type can ever make sure of solid ground under his feet, the latter expressing relation to some definite human purpose’ (What Pragmatism Is, The Monist 15, 161-81, CP 5.412. 1905; parentheses, underline mine).*

126 See CP 2.756. 1905: ‘*The first and weakest kind of inductive reasoning is that which goes on the presumption that future experience as to the matter in hand will not be utterly at variance with all past experience… I promise to call such reasoning crude induction’.*

> Induction, which makes a new estimate of the proportion of truth in the hypothesis with every new instance; and given any degree of error there will sometime be an estimate (or would be, if the probation were persisted in) which will be absolutely the last to be infected with so much falsity. Gradual Induction is either Qualitative or Quantitative and the latter either depends on measurements, or on statistics, or on countings' (A Neglected Argument for the Reality of God, CP 6.472-473, 1908).

With regard to the **validity of induction**, Peirce already had claimed in his *Prolegomena for an Apology to Pragmatism, (MS 293. 1906),* that 'if it sufficiently persisted in (in the long run), will correct any error, and this proposition can be ***deductively demonstrated'***:

> 'The validity of Induction consists in the fact that it proceeds according to a method which though it may give provisional results that are incorrect will yet, if steadily pursued, eventually correct any such error. The two propositions that all Induction possesses this kind of validity, and that no Induction possesses any other kind that is more than a further determination of this kind, are both susceptible of demonstration by necessary reasoning'.

However, here *(A Neglected Argument for the Reality of God)* Peirce argues that induction is a method, which if sufficiently persisted in, is ***inductively certain*** that will correct any error. We can notice that *'inductive certainty'* is no longer *'demonstration by necessary reasoning'* (deductive), but, rather, it appeals to the forceful aspect of experience. In other words, Peirce corrects his notion that the validity of induction can be proved through deductive reasoning and meets Hume's notion that is justified only by experience. For justifying further this claim he calls upon his astronomical observations and experiments, when he was employed by the U.S. Coast and Geodetic Survey. Inductively certainty is based on the frequency account of probability, as with the example of the coin tosses. Because the relative frequency of A-type events (coin landing tails) as related to the total number of events (tosses) for a fair coin, on an infinite number of events and *in the long run* it is (inductively) certain that will stabilize towards some limit (0.5), which will be the true probability ratio.

> 'Induction is a kind of reasoning that may lead us into error; but that it follows a method which, sufficiently persisted in, will be Inductively Certain (the sort of certainty we have that a perfect coin, pitched up often enough, will sometime turn up heads) to diminish the error below any predesignate degree, is assured by man's power of perceiving Inductive Certainty. In all this I am inviting the reader to peep through the big end of the telescope; there is a wealth of pertinent detail that must here be passed over' (A Neglected Argument for the Reality of God, CP 6.474, 1908).

However, the self-corrective character of induction, as Peirce repeats three years later[127], cannot stand alone in the course of inquiry without the formulation of a hypothesis (retroduction or abduction), since the new ideas are always suggested by abduction, while induction can only render certain (definite) ideas and evaluate them, which they have already been suggested by abduction and explicated by deduction.

With regard now to the validity of abduction (retroduction), Peirce, apart from his arguments based on his theory of perception, as presented in his Harvard lectures and on his semiotics exhibited in his Lowell lectures, adds his historical and evolutionary arguments. Here he argues that the history of development of human being from primitive moner into scientific man has shown that this development is in the first instance, due to our ability of guessing or of forming conjectures, which is the result of the development of our instincts.

> ‘Retroduction and Induction face opposite ways. The function of retroduction is not unlike those fortuitous variations in reproduction which played so important a role in Darwin's original theory. In point of fact, according to him every step in the long history of the development of the moner into the man was first taken in that arbitrary and lawless mode. Whatever truth or error there may be in that, it is quite indubitable, as it appears to me, that every step in the development of primitive notions into modern science was in the first instance mere guess-work, or at least mere conjecture. But the stimulus to guessing, the hint of the conjecture, was derived from experience. The order of the march of suggestion in retroduction is from experience to hypothesis’ (A Neglected Argument for the Reality of God, CP 2.755. 1905).

Since retroduction only infers from an actual fact of experience a supposition in a form of ‘may-be’ or ‘may-be not’, its validity cannot be deductively proved, but the facts of the history of development of science assent to its affirmation[128]. His hypothesis is that retroduction is suggested by instinct due to the affinity of human mind with nature, since perceptual judgment, as explained, is also instinctive. As verification of the deductive consequences of this hypothesis he provides the facts of the history of science, which have

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[127] See *A Letter to J. H. Kehler*, NEM (*New Elements of Mathematics*, CP 3.178. 1911): *‘An Induction can hardly be sound or at least is to be suspected usually, unless it has been preceded by a Retroductive reasoning to the same general effect. Induction chiefly serves to render more certain ideas that have already been otherwise suggested. I use "Induction” in a wider sense than usual. It is usually regarded as a reasoning by which one passes from asserting something of a number of single things to asserting the same of the whole class to which those things belong’.*

[128] See *A Letter to Paul Carus*, CP 8.238. 1910, where Peirce argues: *‘As for the validity of the hypothesis, the retroduction, there seems at first to be no room at all for the question of what supports it, since from an actual fact it only infers a may-be (may-be and may-be not). But there is a decided leaning to the affirmative side and the frequency with which that turns out to be an actual fact is to me quite the most surprising of all the wonders of the universe’.*

shown us that the human instinct was developed from the most primitive methods of thought to the most sophisticated ones.

> 'The reason for accepting the Retroductive conclusion, is that man must trust to his power of getting at the truth simply because it is all he has to guide him; and moreover when we look at the instincts of various animals, we are struck with wonder at how they lead those creatures toward rational behavior.
> Retroduction gives hints that come straight from our dear and adorable Creator. We ought to labor to cultivate this Divine privilege. It is the side of human intellect that is exposed to influence from on high' (A Letter to J. H. Kehler, NEM 3.203-206. 1911).

Moreover, Peirce faithful to his fallibilism does not exclude another kind of reasoning, however, as he states, he has good reasons to believe that probably there is no fourth kind of reasoning.

> 'I am unable yet quite to prove that the three kinds of reasoning I mean are the only kinds of sound reasoning; though I can show reason to think that it can be proved, and very strong probable reasons for thinking that there is no fourth kind' (A Letter to J. H. Kehler, NEM 3.177-178. 1911).

In the end of his life, a few months before his death, Peirce continues to assess the completeness of his logic and the scope of his pragmatism with the three types of reasoning, which involves now a trade off between security and uberty[129] (rich suggestiveness, fruitfulness). As he insists, while deduction is the most secure reasoning, abduction is the most fruitful, for it brings the new ideas in the inquiry. Here repeats also his 'hope' doctrine, which is based upon the affinity of human mind with nature, a hypothesis, as explained, which has been verified by the history of science.

> 'I have always, since early in the sixties, recognized three different types of reasoning, viz: 1st, Deduction which depends on our confidence in our ability to analyze the meanings of the signs in or by which we think; 2nd, Induction, which depends upon our confidence that a run of one kind of experience will not be changed or cease without some indication before it ceases; and 3rd, Retroduction, or Hypothetic Inference, which depends on our hope, sooner or later, to guess at the conditions under which a given kind of phenomenon will present itself.
> Each of these three types occurs in different forms requiring special studies.
> From the 1st type to the 3rd the security decreases greatly, while the uberty as greatly increases . . . .' (A Letter to F. A. Woods, CP 8.385-387. 1913).

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[129] See also *MS* 682, October 1913, *An Essay toward Improving Our Reasoning in Security and in Uberty*.

We can infer from the previous presentation of the development of the Self-Corrective Thesis **(SCT)** that, for Peirce, the growth of knowledge in science, is due to the fact that science uses methods that are self-correcting. Scientific method consists, for him, of three sorts inferences: first, **abduction** or retroduction that leads to the formation of hypotheses, second, **deduction** that draws conditional, experimental consequences and predictions of the hypotheses, and third, **induction** (qualitative and quantitative) that tests them experimentally. Each one of the three inferences involves a distinct self-correcting principle. Abduction begins with experience and turns to hypothesis that classifies a certain event to a known class, deduction, as a necessary inference, draws the consequences of the hypothesis, namely the characteristics which have to be found in each member of the class, and induction, based on the principles of generalization and probability, tests experimentally whether these consequences are actually present in that member of the class. Scientific inquiry although is fallible, it is self-corrective in nature, as it allows us, *in the long run*, to correct our errors by gradual modification of our hypotheses after recurring series of abductive inferences, deductive predictions and inductive testing. Therefore it is sufficient for the temporal cessation of doubt and the establishment of new beliefs that cohere with experience, i.e. new theories that explain more phenomena. Furthermore, when it is applied in the long run by a community of inquirers could lead to the establishment of true beliefs, or else beliefs that represent approximately the reality of natural laws, as regularities in nature.

I can proceed now to the discussion of the arguments presented by different philosophers of science, who have interpreted Peirce's Self-Corrective Thesis in a different way, some of them have rejected it and some have defended a part of it or the whole thesis. This interpretation has given rise to further development, application and evaluation of Peirce's SCT, as I am going to show.

3.2. Discussion and justification of the Self-Corrective Thesis

The main critics against Peirce's SCT, as explained, can be classified into the following groups: Those who reject the self-corrective character of Peirce's account of scientific method, those who regard the self-corrective character of abduction as being without logical justification, those who maintain that from all three inferences only induction is proved to be self-corrective, and those who argue that there is no deductive justification for the self-corrective character of induction. In this Chapter, I discuss the objections against Peirce's SCT concerning all the phases of inquiry, so as to show, first, that each method of scientific inference (abduction, deduction and induction) involves a distinct *leading principle* that contributes to the self-correction, second, that the three forms of scientific inference are irreducible, and third, that the whole unity forms a dialectical and gradual process, which in the long run can lead to the correction of errors and the growth of knowledge.

Furthermore, I argue that critics' mistakes lie in the dissociation of the three sorts of inference from one another, or in the underestimation of one of them, while, for Peirce, it is only when they are closely interlinked and complementary to one another that they can contribute to self-correction. Apart from that, I am going to show that this self-corrective character is independent of Peirce's notion of convergence on the truth, since it can work without that presupposition and that this notion is based on problematic accounts. I first begin the discussion with the self-corrective character of abduction.

3.2.1. Abduction as self-corrective

Main critics of Peirce's SCT refer to the justification of the self-corrective character of abduction, since, according to them, Peirce provides no arguments for the self-corrective character of abduction. Alfred Ayer calls abduction 'crystal gazing method'[130], because perceptual judgments that lead to abduction are unconscious. Larry Laudan[131] raises serious

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[130] See Ayer J. Alfred (1968), *The Origins of Pragmatism*, p. 100: *'This (guess work) is akin to Peirce's third method of fixing beliefs, the method of accepting whatever seems 'agreeable to reason', except that reason does not enter into it. Let us call it the crystal-gazing method…'.*

[131] Laudan Larry in (1973) and in (1981), *Science and Hypothesis*, pp. 227-247, *Peirce and the Trivialization of the Self-Corrective Thesis*, criticizes Peirce's SCT. Laudan begins his critique (p. 236) against Peirce's SCT, as follows: *'Although he (Peirce) was presumably obliged to show that all three methods of science are*

and plausible objections against the SCT; therefore his critique has been adopted by many modern philosophers of science up to this day. Laudan claims that, although Peirce holds that *'all scientific inquiry is self-corrective in nature'*, he does not show that abduction and deduction are self-corrective, but he limits his proof only for induction and especially quantitative induction. Moreover, he criticizes Peirce's conception of the affinity of human mind with nature[132], upon which he bases his justification of abduction, as an inarticulate faith similar to Galileo's '*il lume naturale'*.

To begin with the justification of abduction, one may claim that Peirce is not obliged to provide a justification of abduction, since, as Popper holds, the selection of hypotheses is a matter of pure chance[133], or one could adopt James' notion[134], i.e. it is not important where the hypothesis come from, as long as it is proved to be good by confirmation. But Peirce, in order to establish the justification of abduction against the old skeptic argument, i.e. 'we cannot make an assumption, as concession, without proof'[135], he provides an account for its justification in his Harvard Lectures[136]. Therefore, in my opinion, although Laudan examines the background of SCT historically and raises crucial questions about scientific method in general[137], in this particular critique against abduction he ignores the whole

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self-corrective, he ignores deduction and less excusably, abduction, and limits his discussion almost entirely to induction'.

132 See Laudan Larry (1981), *Peirce and the Trivialization of the Self-Corrective Thesis*, p. 241, where he concludes: '*Unable to find a rational justification for his intuition that science is self-corrective, the otherwise tough-minded Peirce had to fall back on Galileo's il lume naturale, on an inarticulate faith in the ability of the mind to ferret somehow the truth'.*

133 Popper Karl in (1972), *Objective Knowledge*, presents his version of epistemological Darwinism, where hypotheses approached from the point of their 'fitness to survive by standing up tests' (p. 19), he holds (p. 30) that 'we cannot give justifications for our guesses' (hypotheses) and the selection of hypotheses is by 'the method of trial and the eliminations of error' (p. 70).

134 See William James (1956), VI: '*It matters not to an empiricist from what quarter an hypothesis may come to him: he may have acquired it by fair means or by foul; passion may have whispered or accident suggested it; but if the total drift of thinking continues to confirm it, that is what he means by its being true'.* See also William James (1975), *What Pragmatism Means*, pp. 27-44.

135 Sextus Empiricus (1964), *The Five Modes*, presents the five modes of suspension of judgment taught by the Skeptics, and he argues against the mode of assumption without proof, as follows: '*For if a person is worthy of credence when he makes an assumption, then we shall in each case also be not less worthy of credence if we make the opposite assumption. And if the person making the assumption assumes something which is true, he renders it suspicious by taking it on assumption instead of proving it. But if what he assumes is false, the foundation of what he is trying to prove will be unsound…'.*

136 See *The Essential Peirce (**EP**)*, Volume two, *Harvard Lectures on Pragmatism*, pp. 133-241, and Patricia Ann Turrisi (1997), *Pragmatism as a Principle and Method of Right Thinking: the 1903 Harvard Lectures on Pragmatism by Charles Sanders Peirce.* Peirce's account is summarized mainly in his last two lectures: *The Nature of Meaning* and *Pragmatism as the Logic of Abduction.*

137 Laudan Larry in (1981), p. 229, formulates his thesis of SCT in two claims, which he both criticizes: *(1) Scientific method is such that, in the long run, its use will refute a false theory (2) Science possesses a method*

Peirce's theory of perception that establishes the justification of the self-corrective character of abduction; therefore he fails to do justice to Peirce's SCT and to see its establishment. Peirce provides an account for the justification of abduction with his theory of perception presented in Harvard Lectures, as follows:

As I said, abduction aims both at explaining this unexpected event and classifying it to a known class of events, so as to find whether the observed event is a member of a certain known class. Abduction begins with a surprising fact (discovery) and turns to hypothesis, which is its conclusion. The justification of abduction, as presented in his Harvard lectures, lies, for Peirce, first in the reality of perceptual judgments. In each observation arises the first spontaneous uncontrollable judgment (*perceptual judgment*), which involves the Category of *Thirdness;* in this sense, *Perceptual judgments* provide the primitive units of our knowledge or else are *'the first premises of our reasoning'*[138]. Therefore, as Peirce states, *'Perceptual judgments are the vehicles by which generality and universality enter into our knowledge'*, since general conceptions are given in perceptual judgment, which can only be the predicate or an element of the predicate. *Perceptual judgment* provides the first point of contact between experience and abstract reasoning, for it is the first true abstract interpretation of the external world *(percept)* or else the formation of the first proposition about it that contains a universal term (e.g. red, light, large etc.). The *self-corrective* nature of scientific inference is based upon this contact, for these spontaneous and instinctive perceptual judgments through abduction generate the first hypotheses.

However, hypotheses generated by the perceptual judgments are subject to further criticism and control, because even though universal terms (*Thirdness*) are given in perception, logically abduction is the weakest of the three sorts of inference[139]. And this is the crucial

for finding an alternative T', which is closer to the truth than a refuted theory T. I will discuss both premises in detail in this Chapter, so as to show the context of their validity.

[138] As Peirce claims in his Harvard Lectures, these are simple mental propositions like 'This is red', 'This building is large' etc., which we form in each observation. Each perceptual proposition consists of a subject, who is concrete and *particular* (this, that, he, it etc.), and of a predicate, which is a *general* (universal) term (red, light, large etc.) attached to the particular through the being (e.g. the verb is).

[139] Peirce in CP 5.180-212. 1903, *Pragmatism as the Logic of Abduction,* points out that according to the norms of logic a fallacy in the conclusion of a syllogism arises when a fallacy is contained in the premises (law and case). In the same way a weak argumentation in the premises lead to weak conclusions, therefore the fallacies in abduction are due to weak argumentations, which are given in the perceptual judgement and are contained in the premises of abduction.

difference between Peirce's theory of perception and the philosophy of Common-sense[140]. Therefore, after the abductive inference we cannot accept any hypothesis, when is presented itself, but we must trace out the necessary (deductive) consequences of each hypothesis, so to say, through deduction we colligate the different perceptual judgments into a copulative proposition and to bring the certain parts into more intimate connection[141].

The justification of the notion that we conclude the existence of a fact quite different from anything observed lies in the true relation between consequent and antecedent, since each consequent (surprising event) presupposes the existence of an antecedent, which the cause of that, due to the principles of *sufficient reason* and *causality*, for the relation antecedent-consequent is a cause-effect relation[142]. As the hypothesis about the antecedent comes from the uncontrolled part of the mind, we cannot know how to exclude from hypothesis everything unclear and nonsensical. But since there is a relation between the elements of action and action itself, for both are given in perception, one can apply the pragmatic maxim of purposive action, which is equivalent to a conception of 'conceivable practical effects', in order to distinguish the unclear and uncontrolled from the logically controlled hypotheses[143].

As for the justification of the human ability to classify the certain event, it lies, first, in the knowability of the *universals* or classes, because this ability to recognize universal qualities or classes we have acquired by generalization and after having perceived many members of the class. At this point, as I have argued, inductive generalizations provide the background

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[140] As explained, those initial vague premises (*perceptual judgements*) will be replaced by scientific hypotheses, which in turn, after having drawn their deductive consequences, are subject to verification or falsification through observation and experiment.

[141] See CP 5.193. 1903; *Pragmatism as the Logic of Abduction: 'Deduction accomplishes first the simple colligation of different perceptive judgements into a copulative whole, and then with or without the aid of other modes of inference, is quite capable of so transforming this copulative proposition so as to bring of its certain parts into more intimate connection'.*

[142] Peirce argues that if the antecedent was not given in the perceptual judgement, it must have been inferred from the premises as conclusion. However, each mode of inference is characterized by self-control, and self-control is inhibitory, therefore it originates nothing. It follows that the antecedent must come from the uncontrolled part of the mind, namely where the perceptual judgements are formed (CP 5.180-212. 1903; *Pragmatism as the Logic of Abduction).*

[143] As Nordmann emphasizes, in Heidelberger and Schiemann (2009), *The Hypothesis of Reality and the Reality of Hypotheses*, this is the crucial difference between Popper's conception of hypothesis and Peirce's abduction. Popper's hypotheses are semantically determinate, since they have definite truth-conditions, therefore Popper views inquiry as a static logical sequence rather than as a fluid continuity the succession of abduction, deduction and induction, and as a result he neglects the formation of auxiliary hypotheses, perceptual judgments, creativity and other aspects of scientific discovery.

for the formation of the first abductive inferences about universals, since they are deduced from the data that are given in perception. Second, classification of the particular event to one of the general classes presupposes the act of comparison between the particular event and the class of events. And this capacity of the human mind is subject to further development in the course of inquiry; therefore it is gained by the inquirer's skill and experience. Furthermore, after having present in our mind all the different elements of the hypothesis (universals, subject, and predicate), then we put together all these elements in a form of suggested hypothesis through the flash of abductive inference, which is an act of insight.

Concerning the human ability and insight to discover the closer to the truth explanatory hypothesis of the unexpected event after repeated explanatory hypotheses (in the long run), it rests in the affinity of human mind with nature, as a development of the instincts, since the human mind is a part of the universe, therefore it shares the same aspect of *Thirdness,* which is working in nature in a form of true regularities or habits[144]. The verification of this hypothesis lies in the facts of history of science, which has shown us that among the trillions of hypotheses, which might be made, the scientists after some dozens of guesses hit nearly on the right hypothesis[145]. For if it was by chance, each scientist would have needed some millions of years to form the correct hypothesis about a certain problem.

These laws (Thirdness) or *'general principles that are really operative in nature'*, according to Peirce, are *universals* in Aristotelian sense, not simply *ens ratonis*, but exist in *re*, so to say, they correspond to something that really have in common the members of the same class (e.g. man, horse, hardness etc.) and determine their behavior under certain circumstances (Peircean *would be's:* disposition to behave in a certain way under certain conditions); therefore, although conditional, they are really operative laws in universe[146].

[144] Peirce argues that taking into account that regularities exist in nature in a form of Laws then they must be a part of the qualities of universal intelligence. In humans also exists the same disposition in a form of regularities or habits, therefore it has this affinity (MS 314, 316 *The Nature of Meaning).*

[145] See more MS 314, 316, *The Nature of Meaning.*

[146] See more *(CP 1.27. 1901)*: *'Anybody may happen to opine that "the" is a real English world; but that will not constitute him a realist. But if he thinks that the word "hard" itself be real or not, the property, the character, the predicate "hardness" is not invented by men, as the word is, but is really and truly in the hard things, and is in one in them are, as a description of habit, disposition or behaviour, then he is a realist'*, also *(EP Vol. 2, p. 457 'A Sketch of Logical Critics', CP. 2.457-8. 1911): 'Consequently, any habit, or lasting state that consists in the fact that the subject of it would, under certain conditions, behave in a certain way, is Real, provided this be true whether actual persons think so or not; and it must be admitted to be a Real Habit, even if those conditions never actually do get fulfilled'.*

Furthermore, as explained with the example of a stone released, we know that the stone will fall towards the earth, i.e. everyone can make the same predictions, for its behavior is governed by law (habit), which is real fact and not a mere figment of the mind, so to say independent of our thought. The proof that the law of gravity is real is the fact that we have no influence or control over the fall of the stone, while if it were a construction of the mind we would have control over it by our will. This disposition is also conditional, because it works with the condition that no other force disturbs the free fall.

However, since there are many alternative hypotheses present themselves to the inquirer's mind, here the personal background knowledge of the inquirer, which is based upon the previous experience and the history of science, is important part of that adjustment; because the hypothesis that is drawn abductively and aims at classifying the particular event to one of the general classes, on the one hand presupposes the knowledge of all those different general classes, and on the other hand involves originality and skill (experience) that consists in the idea of putting together all the elements known already in perception in one suggested explanation.

Apart from that, as I have shown, Peirce suggests some the normative criteria of admissibility that the inquirer can follow, to select among all the hypotheses the most 'fruitful' ones (CP 7.164-231. 1901), which are important for the economy of research, as follows: render the observed facts necessary or highly probable (today's *inference to the best explanation*), 'simpler' hypotheses, abided by Ockham's razor (CP 6.535), most readily refuted[147] or verified hypotheses, those who leave open the greatest field of possibility or what Thomas Kuhn (1977) characterizes as '*fertility criterion*', economical in money, time and energy, 'not attach too much importance to antecedent likelihood of a hypothesis (CP 2.777, 7.220) for preconceived ideas and likelihoods are treacherous guides' or what Paul Feyerabend called *proliferation principle*[148], break the hypothesis into its smallest components and find broad and inclusive ones (*hypotheses with the more explanatory power*), hypotheses that do not bear verifiable effects have to be excluded

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[147] Peirce (CP. 1.120) clarifies the meaning of this criterion, as follows: '*But if a hypothesis can quickly and easily be cleared away so as to go towards leaving the field free for the main struggle, this is an immense advantage*', which implies that the 'main struggle' concerns the less falsifiable hypotheses. This is another aspect that distinguishes Peirce's notion from Popper's falsificationism, since the latter would deny that science ever enters the 'main struggle' but rather holds that science deals with more and more falsifiable hypotheses.

[148] See Paul Feyerabend (1993), p. 34f: '*the principle of proliferation does not only recommend the invention of new alternatives*'.

(*pragmatic maxim*), namely their premises (not the conclusions) are capable of being tested (today's *predictability of hypotheses*).

To sum up, the justification of abduction neither is based upon 'inarticulate faith', as Laudan argues, nor is a 'crystal gazing method', as Ayer claims, and it is not a matter of pure chance, as Popper holds. But though not deductive, it is logical, since it is based on the empirical data that are given in perception and are contained in the premises of the abductive form of inference, therefore we can infer a conclusion about the antecedent. Apart from that, it is based upon Peirce's evolutionary epistemology, which shows that from the instinct for searching the truth was evolved the methodology for searching the right explanatory hypotheses; therefore it is not relied on a mysterious instinct. But since abduction is the weakest of all three sorts of inference, it has to be followed by the deductive phase of inquiry.

3.2.2. Deduction as self-corrective

After the abductive phase of inquiry that ends with the formation of hypothesis, as explained, follows the deductive phase, in other words, once a hypothesis is adopted, it has to be examined the conditional experimental consequences or predictions, which would follow from its truth. This process is based upon the pragmatic meaning of ideas, which lies in considering their practical consequences. And the *practical consideration* of a universal quality (heaviness, hardness etc.) consists in the premise:

> *'If I conduct myself in a manner x, I will have experience y'*

This is based on Peirce's ***pragmatic maxim*** *(CP 5.9. 1905)*, as I argued. To be more specific, since the inquirer has classified the unexpected observed event *A* to a general class of events *B*, and the members of the class *B* have the observable characteristics *a, b,* and *c,* then according to deduction the event *A* should have the same observable characteristics, as follows:

> All the members of class *B* have observable characteristics *a, b* and *c*
> Observed event *A* may belong to class *B* (hypothesis)
> Event *A* should have observable characteristics *a, b* and *c* (practical consequence)

This reasoning helps the inquirer to plan the sort of experiments that he has to conduct in order to verify his hypothesis, for from the inversion of the proposition of the *pragmatic maxim (‘if I conduct myself in a manner x, I will have experience y’*) I can infer *‘if I want to have experience y, then I will conduct myself in manner x’*, which can be translated into:

> ‘If I want to have experience of the observable characteristics *a, b* and *c* that A has, then I have to conduct that sort of experiments’

Since the whole process is based on deduction, it is self-corrective, because deduction can correct its conclusion, when the premises are true, as well as vice versa. Even in the statistical or probable deduction applied in scientific inquiry, the conclusion is in most cases true because the syllogism lends the probability ratio of the rule to the conclusion, as Peirce says: *‘(Probable Deduction) … would from true premises produce true conclusions in the majority of cases, in the long run of experience’ (CP 2.268).* Probable deduction is as follows:

> The proportion p of the Ms are Ps (rule)
> S1, S2, S3, etc. are a numerous set taken at random from among the Ms (case)
> Hence probably and approximately the proportion p of the Ss are Ps (conclusion)

Therefore, apart from Laudan’s weak argumentation[149], there is no serious objection to the self-corrective character the deductive form of inference, proposed by Peirce as second phase of the inquiry. But there is an underestimation of the contribution of deduction to the whole process of inquiry by Ayer (1968), who misses the point that, for Peirce, through deduction we colligate the different perceptual judgments into a copulative proposition, therefore after the abductive inference it is always required to draw the deductive consequences of a hypothesis.

Nicholas Rescher[150], although defends Peirce’s SCT, and, correctly in my view, argues that scientific methodology as a whole is self-corrective, in his attempt to support this argument,

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[149] Laudan Larry in (1981), *Science and Hypothesis*, p. 236, claims:*’Although he (Peirce) was presumably obliged to show that all three methods of science are self-corrective, he ignores deduction and less excusably, abduction, and limits his discussion almost entirely to induction’ (underline mine).*

[150] See more Rescher Nicholas (1978), *Peirce's Philosophy of Science*, pp. 1-17, *The Self-correctiveness of Science*. Especially on p. 15 he makes the controversial claim that the whole can be self-corrective without the need of its parts being self-corrective: *‘Induction as a whole – the scientific method in general and not each of*

he holds that quantitative induction can monitor qualitative induction by statistical means and reveals that theory improvement is necessary. In my opinion, to justify this claim one has to show that the three forms of scientific inference are irreducible, and that each one involves a distinct *leading (guiding) principle*[151] that contributes to the self-correction, because, in order the whole to be self-corrective, each of its autonomous parts must be also self-corrective, otherwise there is an underestimation of the deductive phase of inquiry. This phase is very significant, since it shows us what the hypothesis means by demonstrating its various necessary consequences, in other words determines the pragmatic meaning of the hypothesis; therefore deduction is required after the weak inference of abduction and besides can be used for refuting many hypotheses (economy of research), whose consequences are logically inconsistent or contradictory with the facts of experience[152].

Afterwards follows the inductive phase of the inquiry, which is the experimental testing of the hypothesis, which means the inquirer tests the observable characteristics of the event *a, b, c,* etc. and compares it with observed characteristics of the class. This is the method that Peirce calls *quantitative induction*.

3.2.3. Induction as self-corrective

The main critics of the self-corrective character of quantitative induction, i.e. Laudan along with the other critics: Thomas Goudge[153], Madden[154], Lenz[155], von Wright[156], Burks (1964),

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the several parts of induction – is self-corrective, in the sense of self-monitoring. The job of producing new theories is done by scientific method at large, and not specifically and particularly by quantitative induction' (underline mine).

151 Peirce (CP 5.367) argues that in order each inference to be valid, its *guiding principle* has to be true: *'The particular habit of mind which governs this or that inference may be formulated in a proposition whose truth depends on the validity of the inference which the habit determines: and such a formula is called a guiding principle of inference'.*

152 See for similar views about the contribution of deduction and its significance for this phase of inquiry: Goudge (1969), Moore (1961), Reilly (1970), Levi (1980), Riemer (1988), Misak (1991), Forster (2001) and Cooke (2006).

153 See Goudge Thomas (1969), *The Thought of C. S. Peirce: The Justification of Induction*, p. 194: *'That he had solved the problem of Induction, I do not suppose Peirce would have claimed…. Yet Reichenbach is correct in saying that Peirce's view marks the first forward step towards a solution of this problem since it was pointed out by David Hume'.*

154 See Madden Edward (1960), *Charles Sanders Peirce's Searching for Method,* pp. 254-55.

155 See Lenz John (1964), *Induction as Self-Corrective,* pp. 151-162, in Moore Edward and Robin Richard (1964), *Studies in the Philosophy of C. S. Peirce,* where he argues that *'it is difficult to comment critically upon Peirce's view'* (p.158), therefore we have to restrict ourselves to quantitative induction.

Ayer (1968), Shimony[157] and Losee [158] hold that Peirce is the forerunner of Reichenbach's '*vindication of induction by enumeration'*. Laudan, after examining the three forms of induction (crude, qualitative and quantitative), he concludes that Peirce provides no arguments for the self-corrective character of the first two forms, and that he only proves quantitative induction to be self-corrective[159], a notion whose validity Reichenbach also has shown. Therefore he is rather a forerunner of Reichenbach's[160] 'vindication of induction', and he should have adopted the same notion, so to say all three forms of scientific inference should be reduced to induction. To provide an answer to this question, I will explore Reichenbach's method and compare it with the one of Peirce's.

Hans Reichenbach in response to Hume's objection to the validity of crude induction, as explained, he developed a probabilistic solution to that problem with his notion of 'vindication of induction', which is as follows:

Suppose we have an in infinite sequence of events ***A*** (e.g. coin tosses of a fair coin) and some of these have the property ***B*** (e.g. tails). The relative frequency of observed events ***m*** that have property ***B*** divided to the total number **n** of events ***A***, namely ***f (A,B)***, if the sample is enlarged and after continuous application of a series of samples, it will converge towards a limit, in other words, ***lim fn (A,B) = 0.5***, which is the value of the theoretical

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156 See more Von Wright H. G. (1965), *The Logical Problem of Induction.* Especially on p. 226 he argues: *'The Peircean idea of induction as a self-correcting approximation of the truth has no immediate significance … for other types of inductive reasoning than statistical generalization (i.e. specifically quantitative induction).*

157 See Shimony A. (1970), *Scientific Inference,* p. 127: *'The only clear example of asymptotic approaches which he (Peirce) offers is the simple one which is at the heart of Reichenbach's treatment of scientific inference: the evaluation of the limit of relative frequencies in infinite sequences of events (for example 2.650, 6.100, 7.111, 7.120). Since this kind of inference (statistical or quantitative induction) is the only one of the three kinds of induction which he recognizes … even sympathetic commentators on Peirce have found that his demonstrations (of the self-correctiveness of science) fall short of realizing his program'.*

158 See Losee John (2004), *Theories of Scientific Progress: An Introduction* in *Scientific Progress and Convergence upon Truth,* pp. 99-100.

159 See Laudan Larry (1981), *Peirce and the Trivialization of the Self-Corrective Thesis,* pp. 240-241, where Laudan concludes that: *'Peirce began, as I claimed before, with a very general and a very interesting problem: that of justifying scientific inference by showing that the methods of science (including all species of induction) are self-corrective… Unable to find a general solution to that problem, Peirce tackles the more limited task of that one of the family of inductive arguments, quantitative inductions are self-corrective… Seemingly unwilling to admit, even to himself, that he failed in his original intention to establish SCT for all the methods of science, Peirce acts as if his arguments about quantitative induction show all the other species of induction are self corrective as well'.*

160 See Laudan Larry (1981), *Peirce and the Trivialization of the Self-Corrective Thesis,* p. 241: *'he could have gone the way of Reichenbach and argued that quantitative induction was the only species of scientific inference, to which all other legitimate methods could be reduced'.*

probability $\boldsymbol{P\ (A,B) = 0.5}$. Given that exists the theoretical probability of the event A to have property B, $\boldsymbol{P\ (A,B)}$, which is the relative frequency $\boldsymbol{f(A,B) = m/n}$, then the rule of Reichenbach's induction by enumeration says:

$$\text{Given that } f\ (A,B) = m/n, \text{ then } \lim fn\ (A,B) = m/n \pm \delta \quad (\text{when } n \to \infty, \text{ then } \delta = 0)$$ [161]

For Reichenbach there are two possibilities in this procedure: 1. if the limit does exist so does the probability, because the relative frequency will approach a limit, and if we persist in for larger samples, we find that beyond some point the sequence will always match the actual limit. 2. If the limit does not exist, we have lost nothing, because no other method can ascertain the value of a nonexistent probability[162]. Although this is a deductive justification, it is not in the way that Hume argues, since it does not prove that all inductive inferences with true premises will give true conclusions, but only that induction enables us to make accurate inferences about frequencies.

Peirce proposes a similar calculation of the relative frequency with his *statistical* or *quantitative induction* in the inductive phase of inquiry, which is the experimental testing of the hypothesis. Here the inquirer tests the observable characteristics of the event *a, b, c,* etc. and compares them with the observed characteristics of the class. Peirce distinguishes the two types of induction, as explained ('crude' and 'quantitative' induction), because 'crude' form of induction is very weak sort of inference. However, 'quantitative' induction with statistical syllogisms and direct inference serve as the testing grounds for hypotheses. Here one concludes from an observed relative frequency ($\boldsymbol{f_i = m_i/n_i}$) in a randomly drawn sample a hypothesis about the relative frequency in the population, in other words, the number of times that the hypothesis has predicted successfully $\boldsymbol{(m)}$ as related to the total number of times $\boldsymbol{(n)}$ that the conditions of the prediction were fulfilled, $\boldsymbol{f = m/n}$. If the experimental test of the hypothesis is repeated several times with a large number of samples $\boldsymbol{(n)}$, then will arise different ratios, as follows: $\boldsymbol{f_1 = m_1/n_1}$, $\boldsymbol{f_2 = m_2/n_2}$, $\boldsymbol{f_3 = m_3/n_3, \ldots, f_n = m_n/n_n}$. The limit, then, of those relative frequencies $\boldsymbol{(\lim fn = p)}$ represents the probability ratio of the sample, which in our case shows the proportion $\boldsymbol{(p)}$ of the successful predictions of the hypothesis in the sample.

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161 See more Appendix 1.

162 See more Reichenbach Hans (1949), *The Theory of Probability,* p. 446, sec. 91.

This means that induction does not lead to the truth, but, rather, it lends a probability ratio to its conclusion. The justification of this probabilistic induction is based upon an inversion of Peirce's concept of *probable deduction*, since is the syllogism that lends the probability ratio *p* of the rule to the conclusion, as follows:

The proportion *p* of the Ms are Ps (rule)
S1, S2, S3, etc. are a numerous set taken at random from among the Ms (case)
Hence probably and approximately the proportion *p* of the Ss are Ps (conclusion)

But since the inference is based on a limited number of instances and is supposed to represent a true ratio of the whole class, it may be erroneous; therefore when the sample tested is enlarged, the ratio is gradually corrected, begins to converge towards its limit and in the long run will be revealed the true value of *p*. Furthermore, although the series of singular events is endless, for it is incomplete in a finite number of testing, Peirce claims that it is justifiable because every law tends to be manifested in a finite series of samples. Thomas Goudge[163] here argues that even though Peirce rejects Mill's '*Principle of the Uniformity of Nature*'[164], his justification of induction, apart from the randomness of sampling, rests mainly upon this same principle. On the other hand, Ayer[165] raises an objection against the 'long run' application of quantitative induction by claiming that it cannot secure us.

As explained, Peirce in (CP 2.784. 1905)[166] shows that the character of a series of events (finite or infinite), if there is one, will manifest itself in the long run, although the endless series is incomplete, since it tends however irregularly to stabilize itself and approach its limit gradually. This notion, in my opinion, excludes Mills' 'uniformity of nature' as it

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[163] See more Goudge Thomas (1969), *The Thought of C. S. Peirce*, *The Justification of Induction*, pp. 180-194.

[164] See more Mill John Stuart (1974), *Collected Works of John Stuart Mill*, Volumes 1-2, *A System of Logic* (1866).

[165] See more Ayer J. Alfred (1968), *The Origins of Pragmatism*, p. 95*: 'But the conclusion 'in the long run' robs this conclusion of all its value. It means that if the reference class is finite, we have to examine all or nearly all its members; if it is infinite we are sure of getting the right answer in an infinite time'*.

[166] *'For the endless series must have some character; and it would be absurd to say that experience has a character, which is never manifested. But there is no other way in which the character of that series can manifest itself than while the endless series is incomplete. Therefore if the character manifested by the series up to a certain point is not the character, which the entire series possesses, still, as the series goes on, it must eventually tend, however irregularly, towards becoming so… This inference does not depend upon any assumption that the series will be endless, or that the future will be like the past, or that nature is uniform, nor upon any material assumption however'* (CP 2.784. 1905; underline mine).

approaches irregularly, and it is not contradictory with Hume's objection to induction (the future will be like the past). On the other hand, if there is no regularity to be discovered, then we have to return to the premises of abduction to correct them and as a result to modify the initial hypothesis.

To illustrate this process we can recall my example of a chemical substance A under investigation. There we supposed that the inductive phase of inquiry consisted in testing a several number of samples of the substance A, and we had found that the 95 percent of the samples had proved to have the same qualities like of the class B. Therefore we could infer 'probably and approximately' that the same ratio (p = 0.95) would be found in the whole population of the substance A, and as a matter of course the substance A probably belonged to the class of substances B. But if in that process after continuous application of a series of samples the probability ratio was lesser than 0.9, i.e. it did not manifest its character, then we had to suspend judgment and to return to the premises of abduction to correct them, and as a result to modify the initial hypothesis. In that case we would infer that our initial hypothesis was false, therefore it did not possess any character of regularity to be discovered.

Very important in this phase of inquiry, as I said, is to name and to evaluate the importance of the various qualities of the subject class under investigation, which Peirce calls ***qualitative induction***[167]. In the same example, if more valuable quality of the class B were its acidity, then we had to test also the acidity ***(pH)*** of the substance under investigation A. Other examples given were: if it is supposed a golden object to be examined we can test the atomic number of it, in case of radioactive substance we can detect the ionizing radiation by the use of a Geiger counter. Some interpreters of Peirce confuse qualitative induction with retroduction, which is the abductive inference: Laudan[168] mistakes abduction (retroduction)

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[167] See the definition of *qualitative induction* given by Peirce in CP 7.110-120. 1903: '*The strength of an argument of the Second Order (qualitative induction) depends upon how much the confirmation of the prediction runs counter to what to what our expectation would have been without the hypothesis. It is entirely a question of how much; and yet there is no measurable quantity*', and in CP 2.759. 1905: '*The remaining kind of induction, which I shall call Qualitative Induction... It consists of those inductions which are neither founded upon experience in one mass, as Crude Induction is, nor upon a collection of numerable instances of equal evidential values, but upon a stream of experience in which the relative evidential values of different parts of it have to be estimated according to our sense of the impressions they make upon us*'.

[168] Laudan Larry (1981), *Peirce and the Trivialization of the Self-Corrective Thesis*, on p. 241, quotes incorrectly the following citation (CP 1.81. 1896): '*It is certain that the only hope of retroductive reasoning ever reaching the truth, is that there may be some natural tendency toward an agreement between the ideas*

for qualitative induction, because he quotes Peirce's citation of his writings (CP 1.81. 1896), about rertoductive reasoning, as an example of absence of methodological rationale, and translates it as qualitative induction. While Losee[169] regard it as 'the entertaining of hypotheses', which means retroduction. However, Peirce clearly states that *'Retroduction and Induction face opposite ways'* (CP 2.755. 1905), and also in *A Letter to Paul Carus*, (CP 8.227-228. 1910), he says that his former term 'hypothesis' is now called retroduction[170].

After having examined both forms of induction we can conclude that, although both sorts (Reichenbach's *induction by enumeration* and Peirce's *quantitative induction)* are based on the limit of the relative frequencies, Peirce proposes his *qualitative induction* that evaluates the importance of the various qualities of the subject class under investigation that can lead to the modification of the hypothesis. Apart from that, if the hypothesis is refuted, Peirce proposes that the inquirer has to proceed with more experience to making a new abduction and form a new hypothesis. While Reichenbach with his distinction between context of discovery and of justification underestimates the logical function of hypothetical presumptions (abduction)[171]. For Peirce, in both cases the inquirer has to return to the minor premise of the abductive inference and modify it; therefore he says that *induction corrects its premises*.

To illustrate the contribution of qualitative induction to the modification of a hypothesis, I can give the following example: If in our previous example the substance A was initially identified by some method as pure salt (it belongs to the class B of salts, NaCl), and we have found from an observed relative frequency (***f* = *m/n***) in a randomly drawn samples

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which suggests themselves to the human mind and those which are concerned in the laws of nature', as an example of qualitative induction.

169 See Losee John (2004), *Theories of Scientific Progress: An Introduction* in *Scientific Progress and Convergence upon Truth*, pp. 99-100.

170 See *A Letter to Paul Carus*, (CP 8.227-228. 1910), where Peirce criticizes the logicians that they have not recognized abduction (hypothesis) as a valid sort of inference: '*They only failed to do so by having so narrow and formalistic a conception of inference as necessarily having formulated judgments for its premises) that they did not recognize Hypothesis (or, as I now term it, retroduction) as an inference'* (underline mine). Compare with the previously quoted Peirce's definitions of qualitative induction (CP 7.110-120. 1903) and (CP 2.759. 1905). See for similar views about the difference between retroduction and qualitative induction: Reilly (1970), Riemer (1988), Forster (1989, 2001) and Cooke (2006).

171 See Gregor Schiemann, *Criticizing a Difference of Contexts*, p. 243, in Stadler (2003), *The Vienna Circle and Logical Empiricism: Re-evaluation and Future*.

that the melting point tends to stabilize itself in $\boldsymbol{T_f{}'=780°}$ **C** and its solution in $\boldsymbol{pH = 8}$, then we can return to the abductive inference to correct it, as follows:

> All pure salts (NaCl) have melting point $\boldsymbol{T_f =800,8\ °}$ **C** and $\boldsymbol{pH = 7}$
> This quantity of salt has $\boldsymbol{T_f{}'=780\ °C}$ and $\boldsymbol{pH = 8}$
> This quantity of salt is not pure salt (NaCl)

Although the initial hypothesis was that it was pure salt, the new modified hypothesis says that though this substance is salt, it is not pure one. Afterwards the same process has to be repeated again, in order to find out the percentage of the substance's purity by drawing deductive experimental consequence of the new hypothesis and by testing it inductively.

With regard to the justification of induction, Peirce, in my opinion, provides an account for the legitimating of the self-corrective character of induction, since in his later writings claims that induction is 'inductively certain'[172]. On the contrary Reichenbach claimed that he proved deductively the vindication of induction[173].

Due to these differences counter critics (Levi and Hacking) suggest that Peirce is rather a forerunner of Neyman and Pearson's statistical approach to confidence interval estimation and hypothesis testing. This method was first unearthed by Isaac Levi[174], who by comparing the two methods argued that the self-corrective character of the Peircean method lies in this statistical aspect of the quantitative induction, since in the long run by continuous modification in a random selected sampling it will reveal the true value of p (as

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[172] See CP 6.474. 1908, *A Neglected Argument for the Reality of God: 'Induction is a kind of reasoning that may lead us into error; but that it follows a method which, sufficiently persisted in, will be Inductively Certain (the sort of certainty we have that a perfect coin, pitched up often enough, will sometime turn up heads) to diminish the error below any predesignate degree'.*

[173] Reichenbach in his letter to Russell writes in 1949: *'It was proved for the first time in my axiomatic construction of the calculus of probability, which shows the axioms are derivable from the frequency interpretation and that therefore the application of the calculus to physical reality is ultimately reducible to induction by enumeration'.* (Reichenbach Hans, 1978, *Selected Writings, 1909-1953,* Maria Reichenbach R., S. Cohen, p. 409).

[174] See Levi (1980), *Induction as self-correcting according to Peirce,* pp. 127-139, in Mellor (1980), *Science Belief and Behavior*. On p. 128, he concludes: *'Peirce's conception of induction anticipates the approach to confidence interval estimation and hypothesis testing later developed by Neyman and Pearson'*. Ian Hacking in the same paper in Mellor (1980), *Science Belief and Behavior,* pp. 141-160, *The Theory of Probable Inference: Neyman, Peirce and Braithwaite,* argues that the theory of Neyman and Pearson is based on the theory of probable inference of Peirce and provides a satisfactory answer to Hume's objection to inductive inference.

explained, the proportion of S's being P's). Furthermore, as Misak and Mayo[175] argue, both methods agree that we cannot assign degrees of probability to hypotheses (CP 2.780. 1901), but, rather, they provide procedures for testing and calculating a special character x_0 of the observed events, which will show us in the long run whether a hypothesis should be rejected or not[176]. However, in my view, the main difference is that for the selection of this special character x_0 Peirce provides a special technique (*qualitative induction)*. Therefore, although Peirce's induction is closer to Neyman and Pearson's statistical approach, his whole method is not statistical, due to his proposed qualitative induction, abductive and deductive phase of inquiry. Apart from that, both methods, namely Reichenbach's and Neyman and Pearson's, do not involve two significant aspects of Peirce's method, that is, the co-operative aspect of inquiry within the scientific community and his fallibilism. Their model is based upon a single scientist who tries to apply and interpret a certain algorithm, while, for Peirce, scientific method requires cooperative and inter-subjective interpretation of the different maxims by the inquirers, who try different hypotheses and different criteria of their evaluation [177].

In this phase of inquiry there is an objection raised by Cheng[178] concerning the *predesignation*, which along with randomness of sampling in Peirce's earlier conception of induction[179] are the two necessary conditions for the validity of induction,. He argues that Peirce's conception of predesignation is not clear, as it contains a weak and false claim, therefore it cannot provide clear account of justification of induction and as a result of the

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175 See more Misak (1991) and Mayo (2005).

176 Neyman and Pearson in *On the Problem of the Most Efficient Tests of Statistical Hypotheses*, first published in 1933 in *Philosophical Transactions of the Royal Society*, A:231, pp. 289-337, and reprinted in Neyman J. and Pearson E. S. (1967), *Joint Statistical Papers*, pp. 140-185, on page 142, argue: *'To decide whether a hypothesis H of a given type be rejected or not, calculate a specified character xo of the observed facts; if x> xo reject H; if x≤xo accept H';* in my example the selected character *x* is *Tf* and *pH.*

177 Putnam in (1995), *Pragmatism and the Contemporary Debate*, pp. 70-72, emphasizes also these two aspects of the Peircean conception of scientific inquiry.

178 See more Cheng, C. (1968), pp. 392-402, and Cheng C. (1969), where he discusses his objection to predesignation and on p. 67 (1969) remarks: *'Peirce does not make it clear what the self-correcting process of induction means'*, while in p. 71 *'As we cannot ascertain the true or the real until we have carried out inquiry in a long run, we cannot ascertain the validity of inductive inquiry or the truth of any inductive conclusion at any finite stage of inquiry'.*

179 See CP. 2.738.1883, where Peirce characterizes these two factors as '*conditio sine qua non of valid induction'*. There Peirce by the use of the example of a biographical dictionary illustrates the failure of inductive generalizations and attributes it to the fact that *'the character in which the instances agree was not predesignated'*. Because *'if the character be not previously designated, then a sample in which is found to be prevalent can only serve to suggest that it may be prevalent in the whole class'.*

self-correcting process of induction. Riemer's[180] answer to that objection is that Peirce's later development of his abduction theory solves the problem of predesignation. Francis Reilly[181] also discusses the problem of predesignation and he sees it in connection to the deductive phase of inquiry. In my opinion, apart from the deductive phase, qualitative induction and scientists' skill can contribute to the solution of predesignation, as I am going to show in the second part of the study[182]. Because after the deductively predictions of the hypothesis the inquirer has first to evaluate the qualities of the class under investigation and then to observe whether those certain qualities are present in a large number of samples. Furthermore, after each series of abduction-deduction and induction the scientist becomes more qualified to design better experimental tests, which means better predesignation.

The last critique of the self-corrective character of induction in general (Ayer, van Fraassen and Losee)[183] is based on Hume's objection, in other words, there is no deductive justification of induction. The counter argument is that statistical induction provides a sufficient solution to Hume's problem (Salmon, Rescher, Levi, Hacking, Misak and Mayo)[184], since the long-run probability approach does not face the problem of crude induction, because it lends a probability ratio to its conclusion and enables us to make

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[180] Riemer Ines in (1988), pp. 24-32, discusses Cheng's objection.

[181] Reilly Francis in (1970), pp. 65-72, discusses Peirce's notion of predesignation and claims on p. 66: '*This requirement (predesignation) is closely linked with the deductive stage of inquiry, the operation that predicts observable phenomena from the hypothesis since prediction involves predesignation… The observable quality must be named, and not merely left vague'.*

[182] In the second part I am going to show the contribution of qualitative induction to better predesignation, and in addition, both Priestley's form of predesignation, as the outcome of experimental skill, and Lavoisier's form, applied in his last experiments, as the outcome of both the deductive predictions of the hypotheses and of his gradually increased skill.

[183] See more Ayer A. (1968), *The Origins of Pragmatism,* p. 91, where Ayer argues: *'The prevailing view that we are bound to rely on past experience as a guide to the future, it has been demonstrated once for all by Hume that no procedure of the kind can offer us any assurance of success. It is held indeed that it is a mistake even to look for justification of induction'.* Van Fraassen in (2000), *The False Hopes of Traditional Epistemology,* after criticizing the justification of the self-corrective character of induction by both Peirce and Reichenbach concludes: *'There was in all this, to use Kant's words, only so much science as there was mathematics* -- *and besides science there was, happily for us, art, enterprise, and good fortune as well… So here is my conclusion. We supply our own opinion, with nothing to ground it, and no method to give us an extra source of knowledge.' Only the 'empty' techniques of logic and pure math are available either to refine and improve or expose the defects of this opinion. That is the human condition. But it is enough'.* Losee in (2004), *Theories of Scientific Progress: An Introduction* in *Scientific Progress and Convergence upon Truth,* pp. 98-104, begins his critique with the following argument: '*No amount of evidence can prove that unexamined instances resemble examined instances. Hume was correct about this'.*

[184] Salmon in (1967), *The Foundations of Scientific Inference,* p. 96, argues: *'The frequency approach does not face the same difficulty (where Hume appeals to). If we know the long-run probabilities, a certain type of success is assured by following the methodological rules'.* See also Rescher (1978), Levi (1980), Hacking (1980), Misak (1991) and Mayo (2005).

accurate probable inferences about frequencies. However, although I find the Humean objection plausible, the proposed process by Peirce is still justified. Because in each series of events (finite or infinite), if there is a character of regularity to be discovered, it will manifest itself in the finitely long run, even though the endless series is incomplete, since it tends however irregularly to stabilize itself and approach its limit gradually; therefore in the long run it can correct its ratio and reveal its true value.

Losee[185] by appealing to Strawson's[186] argument against the pragmatic justification of induction, criticizes Peirce for his 'grain example' (relation between the properties of samples of grain taken from a ship's hold and the properties of the population as a whole), and he claims that 'the sample may or may not be representative of the population'. But for Peirce, inductive verification concerns hypotheses about regularities in nature and not accidental events, since for single case problem probability is meaningless[187], because if in this process there is no regularity to be discovered, which means if the probability ratio is less than 0.9, then we have to suspend judgment and return to the premises of abduction to correct them, and as a result to modify the initial hypothesis. In this sense, Peirce claims that '*induction corrects its premises*'.

3.2.4. Irreducibility and unity of the three forms of inference

Laudan, as we have seen, holds that all methods could be reduced to quantitative induction; on the other hand, Francis Reilly[188] argues that through the continuous application of inductive testing the inquirer becomes more qualified to select better hypotheses, and because of his increased experience, his mind is sharpened by more accurate selective instinct for truth. But this conclusion, in my view, implies underestimation of the

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185 See Losee John, (2004), *An Introduction* in *Scientific Progress and Convergence upon Truth,* pp. 100-101.

186 See Strawson P. F. (1952), *Introduction to Logical Theory*, pp. 254-257, where Strawson examines the two propositions: *1. The probability that a sample matches a given population increases with the size of the sample and 2. The probability that a population matches a given sample increases with the size of the sample,* and he concludes that proposition 2 is false.

187 Ian Hacking in Mellor (1980), *Science Belief and Behavior,* pp. 141-160, discusses the single case problem and holds the same. Peirce even in his later writings states it clearly: *'An individual inference must be either true or false, and can show no effect of probability; and therefore in reference to a single case considered in itself, probability can have no meaning' (CP 2.661. 1910).*

188 See Reilly Francis (1970), *Charles Peirce's Theory of Scientific Method,* pp. 72-80, where he discusses this aspect of Peirce's self-corrective method.

originative function of abductive inference or even reduction of abduction to induction. Therefore it has to be explored whether the three forms of inference are irreducible or not.

First, it is apparent the difference between the necessary deduction and the other two types of inference, because it is *analytic* and *necessary true*, since is the application of the rule, as explained with the syllogism BARBARA, while both induction and abduction are *synthetic* sorts of inference and inversions of deduction. Deductive inference is always necessary true, therefore deduction is a method applied in mathematics. Even in the statistical or probable deduction applied in scientific inquiry, the conclusion is true, because the syllogism lends the probability of the rule to the conclusion. Moreover, deduction, as a typical inference of formal logic, can deal only with abstract or mathematical entities, whereas induction and abduction deal with real physical phenomena, as both of them are based on data of experience. But in Peirce's schema, as we have seen, deduction can be used for tracing out the necessary consequences of a hypothesis experimentally.

Furthermore, induction is a generalization from a number of cases of which something is true and inference that the same thing is true for the whole class, while abduction is where we find some very curious event, which would be explained by the supposition that it was a case of a certain rule. In other words, in induction we synthesize to find the rule, while in abduction we synthesize to classify the case under a class of events. Another difference is that induction is reasoning from particulars to the general law, while abduction is reasoning from effect to cause. Induction cannot possibly yield any hypothesis about the causes of the phenomena nor can it introduce new entities[189].

However, the crucial difference between abduction and induction concerns the unobserved facts, for induction infers the existence of phenomena such as we have observed in similar cases, whereas by abduction we conclude the existence of a fact quite different from anything observed, from which something that is observed would necessarily result; therefore abduction supposes something, which would be impossible for us to observe directly (unobservables). The consequence of this difference is that inductive inferences can be directly verified, while abductive inferences can only be indirectly verified; therefore induction is stronger sort of inference than abduction. We cannot verify directly e.g. the existence of Napoleon, the kinetic theory of gases, uncertainty theory, DNA mechanisms

[189] In the second part of this work by appealing to Lavoisier's method I am going to show how abduction can introduce new entities (i.e. oxygen).

etc., but only through their effects, while we can directly verify the existence of a chemical substance, of a close to earth planet, etc.

From all the above differences it is apparent that the three forms of inference cannot be reduced to one sort, and only in some cases abduction can be reduced to induction (e.g. when concerns observed facts), but in most cases, when the inference concerns effects of unobserved entities, abduction cannot be reduced to induction. Therefore, Peirce argues that different sciences use different types of techniques and reasoning: Sciences such as zoology, mineralogy and chemistry are purely inductive, while geology, biology, history, archaeology etc. are mostly sciences of hypothesis or abductive. But, in my opinion, modern theories of sciences such as in physics, chemistry, biology and astronomy use a mixture of induction and abduction supporting one another.

The last critique of Laudan concerns the view that Peirce does not provide a technique of generating better hypothesis that the refuted one[190], a position that also Losee and Van Fraassen adopt in like fashion[191]. Therefore I am going to discuss here Peirce's proposal for the solution of that problem, i.e. of finding better alternative theory than the refuted one.

As I said, after the inductive phase and according to the testing results the inquirer is able to characterize his explanatory hypothesis as: proved, partially proved, unworthy of further investigation, in need of modification, highly dubious and so forth. In any case, positive or negative inductive verification contributes to the forward progress of scientific inquiry. Even the case of negative verification (falsification) contributes to the progress of inquiry more, since it excludes certain useless avenues previously open, is instructive with reference to the next hypothesis, as it points to more fruitful areas for future modified hypotheses after new abductions; while the whole process increases both the background knowledge and the skill of the inquirer. The latter can be used as a basis for forming a more accurate, revised hypothesis with more truth-content. In other words, when the hypothesis

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[190] Laudan Larry (1981), *Peirce and the Trivialization of the Self-Corrective Thesis*, on p. 239, argues: *'Given that an hypothesis is refuted, qualitative induction provides specifies no technique for generating an alternative, which is closer to the truth than the refuted hypothesis'.*

[191] Van Fraassen in (2000), *The False Hopes of Traditional Epistemology*, concludes: *'Peirce could see no way to demonstrate that the ones (hypotheses) we then come up with will be increasingly better in some concrete sense. Nor could he show that if this process is carried out under varying historical circumstances, the results will converge!'*. See also Losee John (2004), pp. 100-101: *'given that an hypothesis has been refuted, qualitative induction specifies no technique for discovering an alternative H´ which is (or is likely to be) closer to the truth than the refuted H'.*

is falsified or is need of modification, the increased scientists' skill and background knowledge contributes to generating a better hypothesis than the refuted one, as the scientist becomes familiar with the certain regularity.

Moreover, the experimental predictions of the new hypothesis will be predesignated more reliably and with more experience and skill, and as a result they will provide better experimental tests. Therefore all the three stages form a dialectical unity, where each one is complementary to another and contributes to the correction of the others. In this way Peirce's proposed method can overcome both the weakness of induction that cannot yield any hypothesis about the causes of the phenomena and introduce new entities, and the weakness of the traditional Hypothetico-Deductive method (HD) that lacks in accuracy and epistemic austerity.

Particularly, Peirce with his notion '*induction corrects its premises'* provided a technique for drawing better hypotheses, as he implied a way of modifying the hypotheses by correcting the premises of abductive inference, that is, by returning to the minor premise of the abductive inference and modify it, as explained with my example of salt. To the choice of the new modified hypothesis contribute: the consideration of some criteria of admissibility of the hypotheses (economy of research), in association with the pragmatic maxim (consequences of a hypothesis) and the development of the scientific skill through scientific community[192]. The hope to find *in the long run* the right hypothesis – after a continuous process of deducing the predictions from each suggested hypothesis, testing inductively these predictions and re-modifying the hypothesis – is based upon this conception, but not on 'inarticulate faith' or *a priori* 'intuition' that Peirce so thoroughly discredited. Furthermore, as I said, what is valid for the individual investigator is also true for the community of inquirers, since each member is informed about the work of the other member, shares in his experience and learns from his failures and as result the whole community proceeds gradually and fallibly towards the approximate representation of the natural regularities.

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[192] In the second part of this work by appealing to Lavoisier's method I am going to show how the scientists' skill and the consideration of the different criteria of admissibility can contribute to the selection of better hypotheses than the refuted one.

3.2.5. Fallibilism and Scientific Community

As I said, Peirce holds that knowledge *'cannot be absolutely exact, absolutely universal or absolute certain' and 'No cognition and No sign is absolute precise, not even a percept' (CP 5.543*). Therefore the best interpretation of the 'real' that we can achieve is an approximate one, since scientific inquiry consists of abductive inference, which is fallible and subjective, as it depends on the inquirer's skill that lays in his background knowledge (personal set of beliefs). Furthermore, the inductive testing process that generalizes on the basis of a limited number of samples and is supposed to represent the whole class enters the region of the probable and uncertain.

On the other hand, granted Peirce's *synechism,* we cannot fully specify the properties of the individuals, for the individuals are rather symbols of their kinds (universals), and the interpretation of these universal *signs* (symbols) cannot be fully exhausted in a finite number of observations, due to their infinite potentiality. Apart from that, the object of scientific inquiry is subject to further evolution, due to the existence of both law and chance in nature. Therefore, due the above mentioned factors, which are based on the logic of probability and Peirce's evolutionary metaphysics, led him to adopting the fallibilism of scientific knowledge. Granted fallibilism[193], every belief is subject to review, confirmation, correction or rejection by subsequent belief.

Moreover, since each individual is real representation of its kind, therefore its final representation (*final interpretant*) demands a community of inquirers and indefinite investigation. From this point of view, Peirce introduces the social aspect of scientific knowledge open to public verification through the indefinite inquiry by the scientific community, which in the long run will reach its limit and converge upon truth. In order to verify this hypothesis Peirce appeals to the history of science, which has shown us that many questions concerning the phenomena have been successfully answered, when the inquiry approached its limit. This implies that since scientists have attained a partial perfect

[193] Freeman and Skolimowski (1974) maintain that Peirce's fallibilism is distinguished from Popper's falsificationism, while Levi in (1967), p. 387, argues that Popper with his conception has betrayed fallibilism in the original Peircean sense. Nordmann (2009) holds that Popper's conception is within the confines of a nominalist epistemology, because the world 'out there' is unknowable except indirectly through evidences gathered from observation and experiment, while Peirce associates the hypothetical with productive anticipations of reality. The crucial difference, in my opinion, is that, for Peirce, science and experience have a cognitive value, while Popper with his critical rationalism underestimates this value.

knowledge about certain given questions in science, by analogue we can assume that we would attain a universal knowledge, namely perfect knowledge[194], about any given question, with the condition that inquiry will be indefinitely carried out. In this way, according to Peirce, scientific community contributes to self-correction and progress in scientific inquiry, which leads to the fixing of beliefs that cohere with experience.

Some critics see his proposed method contradictory with his fallibilism (Ayer, Levi and Margolis)[195]; therefore Ayer and Levi conclude that science will terminate the inquiry in a final irreversible answer to any question, while Margolis holds that, for Peirce, ‘there is no rational *telos* in science’[196]. Almeder holds that ‘there is no difference between endorsing a final theory and approaching it by a margin of error infinite small’, and he criticizes Peirce for failing to provide convincing reasons that scientific inquiry will continue indefinitely[197]. Niiniluoto[198] argues against Almeder’s view that mathematically there is a difference between reaching a limit in a finite time and approaching a limit indefinitely, so to say, mathematically there is a difference between the series of successive corrective steps of theories $T_1, T_2, T_3, \ldots T_n$ and $T_1, T_2, T_3, \ldots T_n \ldots$ Furthermore, he argues that final irreversible answer to any question would imply that all genuine possibilities will eventually be realized. In my opinion, Peirce’s notion of asymptotic approach to the truth does not entail final irreversible opinion, since Peirce’s thesis is that potentiality of universal dispositions can never be exhausted, and that the contrary would have ‘stopped the way of inquiry’. Therefore, rather, it implies that the progressively better and more accurate theories in the long run could approach the approximate representation of reality[199], asymptotically, because fallibilism entails continuous correction and revision of theories. But, as I have already shown, this notion is a product of idealized inquiry and presupposes other valid premises which are also questionable for distinct reasons.

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[194] By ‘perfect knowledge’ as explained, Peirce means that no matter how far inquiry pushed, it has no surprises for us in this question (CP 4.62).

[195] See Ayer (1968), Levi (1967) and Margolis (1993).

[196] Margolis in (2002), p. 135, concludes: ‘*Hence, both in Peirce and Dewey (though for very different reasons), “truth” and “reality” are no more than constructive posits thought to be apt enough for a realist account of science but utterly opposed to any form of Cartesian realism. There is no rational telos in science, and there is no way to overtake the provisional standing of any “pragmatic realism’*.

[197] Almeder Robert in (1980), *The Philosophy of Charles S. Peirce: A Critical Introduction*, pp. 49-98, discusses this notion. On p. 61 he states : ‘*We can conclude with Peirce that the inductive method is essentially self-corrective, and it could not be so if we were not to assume that in the long run inquiry will converge to a definite answer on any answerable question*’, and on p. 97 concludes: ‘*Peirce pretty much failed to provides with convincing reasons for thinking that scientific inquiry will continue indefinitely*’

[198] See Niiniluoto I. (1984), *Is Science Progressive*, pp. 85-86.

[199] For similar views, see Rescher (1978), Niiniluoto (1984) and (1999).

Another question that may arise with Peirce's thesis is: what secures that the sequence of the explanatory hypotheses (H_1, H_2, H_3, … H_n) is progressive, or even why the second revised explanatory hypothesis (H_2) is better than the first one (H_1), the third one (H_3) better than the second one (H_2) and so on? To answer these questions I have to explore the relation between scientific method, progress and to self-correction.

3.2.6. Scientific progress and convergence upon truth

In this Section I examine the scientific method in relation to progress and to self-correction. This examination includes the relation between scientific method, problem solving activity, discovery, continuous growth of knowledge, truthlikeness and self-correction. Afterwards I compare and discuss the cumulative and revolutionary character of scientific progress in comparison with Peirce's evolutionary view of scientific knowledge, with his definition of truth as limit of scientific inquiry and with his notion of the progressive approximation to the 'real'.

First, since the definition of science[200], given at different times by different philosophers of science, involves many contradictory elements, as well as the *problem of demarcation*, which is also contradictory, therefore I am going to proceed directly to the features of scientific method. Because, if we regard science as an institution, part of the society, which maintains work in order to produce new knowledge by the use of scientific method, then science can only be defined by means of scientific method. Taking into account Peirce's pragmatic maxim, namely the meaning of any proposition lies in its conceivable practical

[200] Plato in *Republic*, VI, 508-511, considers genuine knowledge (*επιστήμη*: episteme) that proceeds from hypotheses about the visible things to the knowledge of abstract Ideas, which includes also the study of Geometry. Aristotle in his *Prior Analytics* I, 1, and *Nicomachean Ethics,* VI, 1139b, defines scientific knowledge (*επιστήμη*: episteme) as the study of the things that are eternal and by necessity (principles), it is demonstrative and proceeds both by induction and deduction; see Bakalis (2005). Francis Bacon in his *Novum Organum* suggests that the scientist as proceeds through cautious and gradual generalization to interpret nature; he should purify his mind of all prejudice. However, Thomas Kuhn (1970) argues that there is no 'pure' research without initial assumptions, for 'normal science' employs always a given 'paradigm'. Norman Campbell in his *What is Science,* (1921) defines science as 'study of those judgments concerning which universal agreement can be obtained'; however this definition cannot exclude e.g. astrology from science. Therefore, Karl Popper proposes some *demarcation criteria* to distinguish science from non-science in his *Logic of Scientific Discovery,* (1992), however his falsifiability criterion, which excludes from science propositions as e.g. 'there are planets' for it has not been yet falsified, is too tight. On the other hand, Paul Feyerabend (1975) concludes that the problem of demarcation has no solution because science cannot be distinguished from politics, arts, myths and fairy tales.

effects, and the practical effects are its properties, then the definition of scientific method should involve the different features of scientific method. Bacon in his *Novum Organum* suggests it as the method of critical and gradual inductive generalizations based on observation. David Hume in his *An Enquiry Concerning Human Understanding* provides two characteristics of scientific method: it must contain abstract reasoning about quantities and facts of experience, as follows:

> 'Does it contain any abstract reasoning concerning quantity or number? No. Does it contain any experimental reasoning concerning matters of fact and experience? No. Commit it then to the flames; for it can contain nothing but sophistry and illusion'.

W. Whewell rejects the descriptive character of scientific method claimed by the inductivists (Bacon, Comte, Mill), and he holds that scientific method does not only involve inductive generalizations, but also hypotheses, which he calls *consilience of inductions*, in a form of a theory that re-interprets and connects the facts in terms of a new conceptual scheme[201]. John Kemeny (1959) provides some normative standards of scientific method that involve 'the cycle of induction, deduction and verification', and right scientific attitude that consists in 'eternal search for improvement of theories, which are only tentative held', whereas L. Laudan (1981) emphasizes the functional role of science and argues that scientific method follows the history of science according to his problem solving principle.

As concluded in the relevant Chapter the main features of scientific method, according to Peirce, are: 1. It is a persistent pursuit of truth, 2. It begins with some common basic beliefs, which are subject to further correction and are replaced by hypotheses, 3. It is cooperative and public venture, 4. All its data must be obtained by some form of observation, 5. The method dealing with these data is that of reasoning, 6. The conclusions of scientific method must be verifiable by observation and experiment, and 7. The conclusions of scientific method are fallible and open to further correction. As for reasoning, this deals with the interpretation of data; it involves the abductive, deductive and inductive phase and it is repeated several times, according to the following schema:

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[201] See Whewell W. (1847), p.77: *'Besides mere collection of particulars there is always a new conception, a principle of connection and unity supplied by the mind, and superinduced upon the particulars'*, also his Aphorism XV (1847):*'The facts are not only brought together, but seen in a new point of view'*, as well as Aphorism VII concerning science (1847), pp. 467-68: *'The conceptions by which facts are bound together are suggested by the sagacity of discoverers. This sagacity cannot be taught. It commonly succeeds by gue*ssing' and *'this success seems to consist in framing several tentative hypotheses and selecting the right one'.*

(1) *Abductive inference (hypothesis) – deductive prediction – inductive verification – new modified hypothesis etc.*

The question that may arise here is: what is the relation between scientific method that has the previously mentioned characteristics with the progress of knowledge? In order to explore this relation, first we have to inquire into the concept of progress in terms of accumulation, growth, evolution of knowledge and progress as approach to the truth. The classical view in the 17th and 18th century, as defined by both empiricists and rationalists, was that scientific knowledge grows by accumulating reliably established truths[202], which means new truths are added to the body of accepted results of scientific inquiry. This view, however, was criticized as naïve, for it does not take into account that the earlier results of science may be reinterpreted or rejected by new theories, and apart from that it idealizes the theories, which may be false. The second concept concerns the growth of knowledge and its comparison to a *tree of knowledge*, as defined by Popper[203], or the *consilience of inductions* as defined by Whewell[204]. Both views, as Niiniluoto[205], argues share in common the same notion, so to say, knowledge grows in the opposite direction of a 'family tree', because the new theories include the old theories as the old branches merge into a new branch of knowledge.

In my view, the evolutionary model of growth of knowledge introduced by Peirce[206] includes the previous suggested notions and solves the problem of revolutionary changes in the progress of scientific knowledge, which was introduced later by Kuhn's scientific revolutions[207], according to the following schema:

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202 Bacon F. (2000) compares his method to a machine, which produces results when the 'stuff is gathered from the facts of nature'. Comte A. (2001) describes it as follows: '*Its character will be henceforth unchangeable, and it will then have only to develop itself indefinitely, by incorporating the constantly increasing knowledge that inevitably results from new observations and more profound meditations*'.

203 See Popper K. (1972), pp. 262-263: '*Assuming the same upward direction of time, we should have to represent the tree of knowledge as springing from countless roots, which grow up into the air rather than down, and which ultimately, high up, tend to unite into one common stem'.*

204 Whewell W. in (1847) defines the growth of knowledge as Inductive Table: *This gradation of truth, successively included in other truths, may be conveniently represented by Tables, resembling the genealogical tables by which the derivation of descendants from a common ancestor is exhibited; except that it is proper in this case to invert the form of the Table, and to make it converge to unity downwards instead upwards'.*

205 See Niiniluoto I. (1984), *Is Science Progressive?* pp. 22-25.

206 See (CP 1.103): '*the evolutionary theory in general throws a great light upon history and especially upon the history of science'.*

207 See more Kuhn T. (1970), *The Structure of Scientific Revolutions.*

(2) *Belief – surprise – doubt – inquiry – new belief – new surprise – doubt – inquiry - ... etc.*

As explained, *inquiry* in schema (2) consists of three phases, as in schema (1).

According to Peirce, there are three types of evolution: Darwinian, Lamarckian and the 'cataclysmal' one. The Darwinian mode of evolution consists in the following: when studying the phenomena various tentative explanations recur to our minds from time to time, which are modified by omission, insertion or change in a fortuitous way, and finally we are led to accepting the one explanation and to dismiss all the others as impossible (CP 1.107). The Lamarckian mode of evolution involves not fortuitous changes and it consists in *'the form of perpetually modifying our opinion in the effort to make that opinion represent the known facts as more and more observations came to be collected' (CP 1.108)*; therefore we can compare it to Kuhn's 'normal science'[208], which consists of work undertaken to articulate the paradigm theory.

However, the real progress in science, for Peirce, involves the '*cataclysmal*' mode of evolution, which proceeds by leaps in the abductive phase, since the observation and the discovery of a surprising fact triggers the imaginative leap, which consists in the explanatory hypothesis, as Peirce states:

> 'But this is not the way (Lamarckian) that science mainly progresses. It advances by leaps; and the impulse for each leap is either some new observational resource or some novel way of reasoning about the observations. Such novel way of reasoning might, perhaps, be considered as a new observational means, since it draws attention to relations between facts, which would previously have been passed unperceived' (CP 1.109)[209].

This mode of evolution, as we can see, is similar to Kuhn's (1970) *scientific revolutions*. However, Peirce based on his *synechism* holds that this progress is also continuous and gradual, and it follows successive corrective steps from worse to better theories[210], because every new imaginative leap in abduction presupposes background knowledge that is gained

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[208] Peirce in *The Monist 3, (January 1893), Evolutionary Love, EP*, pp. 360-361, argues that the Lamarckian evolution is driven by the force of habit. Therefore *'where habits are abound, originality is not needed and is not found; but where they are in defect spontaneity is set free'*. Compare also this notion with the characteristics of normal science, as described by Kuhn (1970), pp. 23-42.

[209] See also *(CP 1.48)*: *'The scientific imagination dreams of explanations and laws'*, and: *'But it is by the explaining imagination that science progresses' (CP 1.109).*

[210] Scientific inquiry, for Peirce, is an infinite process between momentary states of doubt and belief, for every belief that is inconsistent with experience is a starting point for a new doubt and every doubt the starting place for a new inquiry, which leads to the fixation of a new belief (CP 5.397).

gradually and continuously by the scientific community[211]. I shall return to this topic in the second part of the study, to discuss in detail Peirce's notion in comparison with the one of Kuhn, so as to examine whether continuity of knowledge and scientific revolutions can co-exist.

We can also compare Peirce's schema with the one that Karl Popper (1972) introduced for the growth of knowledge, as follows:

(3) $P_1 – TT – EE – P_2$

Here $\boldsymbol{P_1}$**:** problem from which we start, ***TT***: tentative theory, proposal of a conjectural solution, ***EE***: error elimination, severe critical examination, and $\boldsymbol{P_2}$: new problem solution.

By comparing the two schemas, we can easily infer that Popper's $\boldsymbol{P_1}$ in (3) represents the **surprise – doubt** of Peirce in (2), since the arousal of doubt after the discovery poses a problem to be solved by the inquirer. As for the ***TT*** it represents the **abductive inference**, while the ***EE*** represents the **deductive prediction – inductive verification** in Peirce's schema (1), and finally $\boldsymbol{P_2}$ the **new belief** in Peirce's schema (2); therefore there is no essential difference between Popper's schema (3) and Peirce's schemata (1) and (2). Furthermore, Peirce's schema includes the definition of scientific method as a problem-solving activity, which is directly connected with the progress of science and the truthlikeness of theories.

Indeed, as Losee argues,[212] the critique 'adopted by contemporary defenders of the 'pessimistic meta-induction'[213] holds that the proper inductive generalization from the available evidence from the history of science is that it is probable that today's theories, and tomorrow's theories as well are false'; therefore he concludes that are better progressive theories in problem-solving, but not true ones that represent reality. However, other studies of the same historical events (e.g. Galileo's theory of falling bodies, Newton's mechanics,

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[211] Nicholas Rescher in (2005), pp. 58-81, supports this view and states that scientific questions have a dynamic of their own (one question gives rise to another), since today's scientific problems could not even have arisen a generation ago, for they could not have been formulated within the cognitive framework of the then-existing state of knowledge.

[212] See Losee John (2004), *Theories of Scientific Progress: An Introduction* in *Scientific Progress and Convergence upon Truth,* p. 101.

[213] Laudan (1981a), p. 33, provides a list of twelve theories to support this view.

Lavoisier's oxygen theory, kinetic molecular theory and the wave theory of light, etc.) conclude that the replacement of a theory is gradual and successive[214], because some basic entities or structures from the old theory remain in the new theory, and apart from that, rival theories speak of the *same unknown entities*, identified indirectly through their causal role and influences; therefore successive theories may give conjectural but increasingly accurate descriptions of the nature of these things[215]. As N. Rescher maintains, scientific progress is a matter of 'complexification', since old theories give way to more sophisticated new ones, so as to correct the oversimplification of the old ones that prove untenable in a complex world[216].

In my view, we cannot ignore the fact that there is a growth of knowledge in the history of science, which has gradually led to better theories with increasing approximation to the representation of certain regularities in nature. Otherwise, if we ignore that, then we are likely to commit the skeptical *all-or-nothing* fallacy of a false dichotomy. Because many past 'false' theories have worked sufficiently, as their predictions were successful, i.e. they were 'true' within certain borders of experience[217]. Therefore though they were not true theories in absolute context, they were true within a certain context. For example, Newton's theory although it was proved false in absolute terms, it has worked successfully and it has been used up to nowadays within the range of macrocosm of the earth. This means that although it is false in absolute terms, it is true within certain borders of experience, since it represents approximately the regularities within the gravitation field of the earth. The same is true with relativity theory and quantum mechanics, which although is extended concerning the borders of experience, it can explain only the 4.6 % of the mechanisms of

[214] In the second part of this study I am going to show that in the Chemical Revolution the progress was also continuous, gradual and successive.

[215] See Niiniluoto (1999), pp. 120-144. For counter reconstructions of these historical events and arguments against 'pessimistic meta-induction' see also Niiniluoto (1984), Boyd (1990), Thagard (1993), Worrall (1994), Psillos (1999), Pyle (2000) and Ladyman (2009). Furthermore, Hilary Putnam in (1975) with his *Principle of Charity* argues that the successive changes of belief refer always to the same objects (e.g. electron), because we have to discount some differences in belief when we interpret; and in (1987) he continues that even if our *theory* of simultaneity, or whatever, has changed, still there is *something* invariant about the kind of prediction.

[216] See more N. Rescher (2005), *Studies in Epistemology, Oversimplification*, p. 121: '*The ancient Greeks had four elements; in the nineteenth century Mendeleev had some sixty… Aristotle's cosmos had only spheres; Ptolemy's added epicycles…'*.

[217] William James in (1975), *Pragmatism's Conception of Truth*, p.107, also maintains that a past theory although is false in absolute terms, it is true within certain borders of experience, as follows: '*Ptolemaic astronomy, euclidean space … were expedient for centuries, but human experience has boiled over those limits, and we now call these things only relatively true, or true within those borders of experience. Absolutely they are false'.*

mass and energy of the universe, if we take into account that 72 % of it is consisted of the dark energy and 23 % of the dark matter, both with unknown mechanisms. This means that modern physics theories are also true within certain borders of experience, i.e. 4.6 % of mass and energy of the universe[218]. In the same way we can assume that a supposed future 'true' theory will be also true within certain borders of experience, which will be more extended than the borders of the previous one. However, in this process, from old theories to new ones, there is a growth of our knowledge and increase of the approximation of a theory to the representation of certain regularities.

We can also draw an analogy here with an empty glass, where there is a drop of water left. If we call it empty, this judgment is not justified – it is false in absolute terms – but it is true approximately, since the glass is 99% empty. But the question that may arise here is whether an absolute justification exists[219], because no one has ever claimed that. In the same way, since theories are man-made formulas that involve justification and interpretation, they cannot be true absolutely. Besides interpretation presupposes the use of language, which is also man-made and 'tolerates much choice of expression', as James also said[220]. Therefore, we cannot have true theories that represent the regularities in nature *absolutely*, but, rather, we can only formulate theories that represent the regularities *approximately*, namely theories with increasing truthlikeness.

Moreover, the closer a hypothesis is to the approximate representation of the regularities the more problems can solve, but not vice versa. On the other hand, predictions of theories are fulfilled when the theories represent the natural regularities approximately; therefore science, apart from its functional role of problem solving activity, is also concerned with understanding and explaining nature. According to the pragmatic account of truth, scientific method makes a progress, as long as it provides gradually better and more comprehensive representations of reality. Assuming that there are two theories (or Hypotheses) T_1 and T_2 concerned with the same problem area of science, we say that the theory T_2 is better than the theory T_1 because it is closer to the to the approximate representation of a regularity than T_1, when it has a greater degree of truthlikeness, which is proved by evidence of inductive

218 See Kane Gordon (2001), also Cline David B. (2003).

219 Dretske in (1981) discusses this problem and shows that the skeptical requirement of absolute justification is irrelevant.

220 William James in (1975), p. 33, points out: *'They are only a man-made language, a conceptual shorthand, as someone calls them, in which we write our reports of nature; and languages, as is well known, tolerate much choice of expression and many dialects'.*

verification. Then we can say that the step from T_1 to T_2 is progressive, and T_2 provides better representation of reality. Apart from that, T_2 can solve more problems than T_1, because the theory has more truthlikeness and ceases the doubts produced when holding the beliefs of the theory T_1, therefore theory T_2 better represents reality.

Because in accordance with the pragmatic maxim, the definition of the 'real' in the context of its consequences, is that both produces beliefs and ceases beliefs (it produces doubts), and when the doubt arises comes the problem, which has to be solved by the inquiry; therefore reality is ontologically prior to the problems, as it poses the problems that inquiry has to solve and not vice versa. In this context, inquiry aims at approaching the representation of the 'real' and not only at solving problems. As explained (CP 3.254), according to the definition of the 'reals', they are independent of our opinions, affect our senses and produce beliefs about them. Hence, it is apparent that the closer a hypothesis is to the approximate representation of reality the more problems can solve, but not vice versa. In this way we can see the relation between scientific progress, truthlikeness, approach to the truth and problem-solving activity.

Furthermore, a theory T_2 arises from a correction of the theory T_1 due to the awareness of the contrast between real and illusion, correct and error, namely T_1 cannot explain unexpected facts (anomalies), while T_2 *can*, and this can be proved through the inductive verification. Correction, then, of theory T_1 leads to the progress of inquiry, because the step from T_1 to T_2 is progressive. In the same way after successive corrective steps of theories $T_1, T_2, T_3, \ldots T_n$, scientific inquiry, will reach its limit *in a finite number of steps*, as in the statistical induction, and approach the approximate representation of certain regularities with which each particular discipline is busied. Peirce by analogue assumes that, if the investigation is carried out indefinitely far by an ideal unlimited scientific community, after successive corrective steps of theories $T_1, T_2, T_3, \ldots T_n$ …, it will reach an ideal limit of scientific inquiry *in the infinite long run*, and the beliefs attained in the ideal last theories will be true beliefs, since they will represent approximately the 'real'. Peirce, just like Whewell[221], as being influenced by German philosophical tradition from idealists (Schelling, Hegel) to

[221] See Whewell W. (1860), pp. 307-309: *'Hegel traces the manifestation of the identity of the idea and fact in the progress of human knowledge"* and *'the only way in which we can approach to truth is gradually and successive'.*

Marxists[222], holds that progress and approach to the truth is gradual and follows successive corrective steps from worse to better theories. Although this corrective and progressive aspect from worse to better theories with more truth content it has worked in the history of science for each discipline, as I explained, the notion, that one single theory through the unification of sciences, which will represent approximately the ‘real’, faces difficulties.

At this point there is also an objection raised by Quine, which concerns the notion of convergence upon truth[223]. As he argues, the fallacy of Peirce’s notion lies in the consideration that theories can converge on a limit, whereas only numbers can converge on a limit; therefore Quine concludes that we cannot expect that the ideal theory is approached as a limit. Except Misak’s[224] objection to that notion, in my view, when Peirce states that we cannot attach probabilities (which is based anyway on hypothetical limit of relative frequency) to theories (CP 2.780.1901), excludes also the notion of limit of theories. However, as I pointed out, Peirce’s notion of convergence upon truth faces other difficulties, as it is based upon problematic premises.

To continue with our discussion, the method that leads to the way from T_1 to T_2, from T_2 to T_3, etc., we regard it as self-corrective, as long as it allows this corrective process, by forming successive better theories, namely with more truth content, and not by producing successive theories $T'_2, T'_3, \ldots T'_n$ that are worst than the previous ones, i.e. with lesser truth content. This presupposes that this method should possess certain principles that can secure this process. But this crucial question has been answered, because I already have shown, first, that each form of inference involves a distinct leading principle that contributes to the self-correction, second, that the three forms of scientific inference are irreducible, and third, that the whole unity forms a dialectical and gradual process, which in the long run can lead to the correction of errors and the growth of knowledge, with the condition that the inquiry would be carried by autonomous scientific community.

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222 Engels F. (1941) in 1888 says that dialectical philosophy destroys illusion about final and absolute truth: *‘that which is now recognized as true will later manifest its latent false side’.*

223 See Quine W. V. (1960), p. 23: *‘Peirce was tempted to define truth outright in terms of scientific method, as the ideal theory which is approached as limit when the (supposed) canons of scientific methods are used unceasingly on continuing experience … But … there is a faulty use of numerical analogy in speaking of a limit of theories, since the notion of limit depends on that of “nearer than” which is defined for numbers and not for theories’.*

224 See Misak C. J. (1991), pp. 119-124, where she discusses this objection and concludes that Peirce’s position is similar to Quine’s, as Peirce holds that the true beliefs would be agreed upon if the inquiry were to be pushed to its limit, but not that theories converge on a limit.

3.2.7. Summary

I can return now to the main critique of Peirce's SCT, raised by Laudan, concerning the justification of abduction and the reducibility of the three modes of inference. First, as explained, abduction, though weak sort of inference, is logical, since it is based on the data that are given in perception, and it is the outcome of evolution of the human instinct; therefore it allows us both to infer a conclusion in a form of a hypothesis about the antecedent event and to classify it to one of the general classes. But since abduction is the weakest sort of inference it must follow the deductive phase of inquiry, so as to draw the logical consequences of the initial hypothesis and examine its coherence. This phase helps the inquirer to examine the conditional consequences or predictions of the hypothesis as well as to design better experiments for the inductive test of the adopted hypothesis, i.e. better predesignation, which will follow as third phase of inquiry.

Second, concerning Reichenbach's vindication of induction, although is similar to Peirce's quantitative induction it cannot stand alone for the course of inquiry. Because, as I argued, each form of inference, i.e. abduction, deduction, qualitative and quantitative induction, have a distinct contribution to the progress of inquiry; therefore all these inferences though complementary to each other, they are irreducible. Apart from that, Reichenbach's method, as it is based upon a certain algorithm that a single scientist tries to apply it, underestimates the cooperative and inter-subjective character of scientific inquiry, which is required for the avoidance of subjective interpretation of the experimental data. As I am going to show in the second part of the study with the example of the Chemical Revolution, this cooperative and inter-subjective character – where inquirers in a pluralist scientific community tried different hypotheses and criteria of their evaluation – has a great share in the progress of scientific inquiry.

I can also get back to Laudan's schema of SCT that is summarized in the following two formulations, and which he both rejected:

(1) Scientific method is such that, in the long run, its use will refute a false theory
(2) Science possesses a method for finding an alternative *T'*, which is closer to the truth than a refuted theory *T*.

Concerning the first thesis, indeed, as we have seen, science has the capability to refute a false theory because of its method. This method consists of self-corrective and closely interlinked sorts of inference, since each form of inference involves a distinct leading principle that contributes to the self-correction. After a finite number of recurring series of abductive inferences, deductive predictions and inductive testing of the different theories science will refute a false theory, with the condition that inquiry is carried out in the long run. However, the long run application can be taken here pragmatically – as a finitely long run, distinguished from the infinite long run[225] – in other words, until the 'false' theory will be refuted and a new theory that explain more phenomena will arise. Peirce also holds that the history of science has shown us that many questions concerning the phenomena have been successfully answered, i.e. scientists have attained a partial perfect knowledge about certain given questions in a finite long run. This means, when the inquiry approaches its limit the 'false' theory will be refuted, as I am going to show in the second part of the study with the refutation of the phlogiston theory, when the limit of inquiry was reached.

With regard to the second one, scientific method actually allows us to find a better theory closer to the truth than the refuted one. However, here truth has to be considered not in absolute terms but pragmatically, as an approximate representation and within certain borders of experience, as I said. From this point of view, the new alternative theory will be with increased approximation and with more truth content (truth content, in a sense that it explains the regularities better and without anomalies). This can be achieved by means of Peirce's view 'induction corrects its premises', i.e. by modifying and correcting gradually the hypotheses after the inductive testing of the theory's predictions, science can lead us to finding an alternative *T'*, better than a refuted theory *T*. To the choice of the new modified hypothesis contribute: the consideration of some rational criteria of admissibility of the hypotheses (economy of research), in association with the pragmatic maxim (consequences of a hypothesis) and the development of scientific skill through scientific community. I am going to explore in the second part of the study the contribution of each of these aspects to this process. Certainly this can work only with the condition that inquiry is allowed to be carried out in the long run and within an autonomous scientific community.

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[225] William James (1975), *Pragmatism's Conception of Truth*, pp. 107-8, points out the importance of temporary truths for the progress of inquiry, as follows: '*Like the half-truths, the absolute truth will have to be made, made as a relation incidental to the growth of a mass of verification-experience, to which the half-true ideas are all along contributing their quota. I have already insisted on the fact that truth is made largely out of previous truths'.* Skagestad P. (1981), pp. 195-199, by interpreting Peirce's SCT emphasizes also the achievements of scientific inquiry in a finitely long run.

3.2.8. Conclusion of the discussion

Although Peirce's account of scientific progress through successive corrective steps is justified, in my opinion, there is a problem in his account of convergence on the truth, because it presupposes that true theories from all particular sciences can be systemized in a single theoretical framework or in one single theory through the unification of sciences. But, in order this account to be true, one has to examine:

1. Whether the complexity of nature, which consists of the infinite multiplicity of particulars, can be comprehended by progressively better theories of each particular science, which are concerned with general regularities (universals) that represent adequately the natural phenomena.
2. Whether the different scientific theories from the different layers of reality, which arise from particular disciplines, allow that or they have limits due to the fragmentary view of each discipline and the different irreducible levels (layers) of the world.
3. Whether can be solved the problem of different languages that each particular discipline uses, since the ideal single theory that could describe completely the 'real' must be expressed by a *universal* language.
4. Whether there is such a guarantee that the world can be explained by a single theory that is true *in all possible worlds*, taking into account the limits of our language and the finitude of minds of scientific community, compared with the increase of diversity of nature and the inexhaustibility of the laws as infinite potentialities in macrocosm, microcosm and other space-time dimensions. Moreover, granted fallibilism, the definition of the 'real' is inexhaustible, therefore it should be without definite limits.

Peirce provided a sufficient account for the first question with his notion of the reality of *Thirdness*, in a sense of both real regularities (*habits*) and universal qualities (*'would be's':* dispositions to behave in a certain way under certain conditions), which allows us to systemize the particulars in general laws. He showed that scientific inquiry is self-corrective for single disciplines; therefore it can gradually approach the approximate representation of these real laws of nature as an ideal limit. He showed, first, that the three forms of scientific inference (abduction, deduction and induction) are irreducible, second, that each one involves a distinct leading principle that contributes to the self-correction, and third, that the whole unity forms a dialectical process that in the long run of application could lead to the cessation of doubt and the settlement of fixed beliefs, which represent approximately the real external world.

His view of abduction (only the premises not the conclusions must be directly observable) provided a sufficient tool for inquiries into unobservable entities in the twentieth Century. He developed the probabilistic account of induction, which has been applied to current statistical theory and has shown the direction in which (theory of probable inference with long-run probabilities) can be found a solution to Hume's problem and Carnap's[226] requirement of total evidence. In this sense Peirce's proposed method provided a solution to overcome both the weakness of induction and the weakness of the traditional Hypothetico-Deductive method (HD).

His notion of continuity (*synechism*) and his weak fallibilism provided a sufficient account for the predictability of scientific method, the knowability of universal laws, and it is distinguished from strong fallibilism that can lead to skepticism or agnosticism, because it can contribute to the establishing true beliefs open to further modification.

But Peirce's historicism and metaphysical notion of cognitive evolution failed to give an account of the convergence upon truth, so to say, science will finally reach asymptotically to the true representation of reality by a single theoretical framework. Because Peirce did not explore the question nr.2, since although there are signs of unification of theories or disciplines, it is questionable whether e.g. biological or social phenomena that entail intentionality and teleological features can be represented by the same universal laws of physics and chemistry; therefore this notion is subject to further inquiry.

As for the question nr.3, Peirce tried with his theory of signs to find a solution to this problem, but he did not complete this work. Certainly, this notion gave rise to further inquiry in that direction by philosophers in the twentieth century (language as representation or correspondence), since representation does not involve only our language

[226] Carnap Rudolf in (1950), *Logical Foundations of Probability*, p. 211, holds: '*In the application of inductive logic to a given knowledge situation, the total evidence available must be taken as basis for determining the degree of confirmation'*.

but also symbols, models, mathematical and diagrammatic representations. Finally, concerning question nr. 4, which is the most questionable and unexplored from all until today, he only drew an analogy from one particular discipline to all disciplines.

Peirce's ideal view of scientific community, did not take into consideration that scientific communities, like all other communities, are governed by some ethical norms or values; it consists of individual scientists, therefore emotional motivation of each investigator has been very significant factor for the advancement of inquiry. These considerations gave rise to the introduction of sociology of science and contributed to the development of social sciences in that direction[227]. From this point of view, Peirce's notion of scientific community was not developed enough, so as to explore the necessary conditions and institutional characteristics, which can secure its independent, autonomous, democratic and inquiring role, against the influence of prevalent notions (*paradigma*) of the 'normal science', as Thomas Kuhn (1970) argues.

Peirce provided an account for the justification of abduction through his notion of *Thirdness* (*Thirdness* is given in perception), as a causal principle between two entities under certain conditions, which can justify the consequent-antecedent relation in abduction and allows us to infer from consequent to the unknown antecedent and to make predictions. Furthermore, his notion of universal qualities as dispositions of all things of the same class to behave in a certain way under certain conditions, though conditional and relative to our actions (Peircean *'would be's')*, it justifies the classification of particular events to one of the general classes. But this ability to recognize universal qualities or classes, as I have argued, we have acquired by generalization and after having perceived many members of the class, that is, by inductive generalizations; therefore abduction is based partly upon crude induction. This aspect Peirce did not take into consideration. However, this does not underestimate the important function of abduction, which can lead us to inferring about causal relation and classification of entities; therefore abduction is the only kind of inferences that can introduce new ideas into our body of beliefs.

As Laudan pointed out, Peirce did not develop the account of the self-corrective character of abduction enough and especially the technique to find better alternative hypotheses. But, as I argued, Peirce with his view 'induction corrects its premises' implied a way of

[227] See Merton (1973), Mulkay (1979), Habermas (1983), Kitcher (1993), and Ziman (1994).

modifying the hypotheses by correcting the premises of abductive inference. This in association with his proposed 'economy of research' (criteria of admissibility of the hypotheses) and the development of scientific skill through scientific community provided a technique for drawing better hypotheses.

But sometimes modern inquiry deals with problems such as confounding factors that produce similar data; therefore it is not clear whether the inductive verification concerns the hypothesis in question or its background assumptions, as there is no clear distinction between them. Furthermore, modern inquiry deals with unobservable phenomena, underdetermination, theory-laden devices, lack of detecting devices at all etc. All these problems may require more careful planning and predesignation of experimental verification, so as to diminish these factors. It might also require suspension of judgment until new evidential data favors one of the rival hypotheses or the background assumptions (auxiliary hypotheses), or some more criteria of admissibility of hypotheses, or even an algorithmic way of connecting all these criteria with appropriate weights. For all the above mentioned reasons it is apparent that the abductive phase of scientific research is subject to further inquiry. Therefore in the second part of the study I am going to appeal to the scientific methodology that Lavoisier followed in the Chemical Revolution for the construction of the oxygen theory, so as to evaluate the different aspects of Peirce's SCT (scientists' skill, self-corrective sorts of inference, criteria of admissibility of the hypotheses, fallibilism, scientific community etc.), and in case of discovering any weaknesses of Peirce's SCT to introduce some new aspects.

4. Self-Corrective Thesis in the Chemical Revolution

In this second part of the study I explore the scientific methodology that Lavoisier followed in the Chemical Revolution for the construction of his oxygen theory, so as my conclusions of the self-corrective character of scientific method from the first part to be tested, supported or corrected. Following the method of Lavoisier, I draw conclusions about his scientific methodology, which I discuss in comparison with my conclusions of Peirce's scientific methodology from the first part of the study, so as to examine the self-corrective character of the scientific method applied in the Chemical Revolution. Since the Chemical Revolution is one particular episode in the history of science, the study of this episode cannot lead to generalization about the whole history of science, therefore its aim is to test Peirce's conception of the SCT and its different aspects (self-corrective sorts of inference, progress of knowledge, criteria of admissibility of the hypotheses, scientists' skill, fallibilism and continuity of knowledge within the scientific community).

However, since there are many interpretations of the Chemical Revolution given by different modern methodological views, I discuss them in comparison with Peirce's proposed scientific methodological theory, in order to explore whether Peirce's account is consistent with the historical data, and whether it is plausible account of this well-known episode in the history of science. This discussion will also help me to discover any omissions or weaknesses of Peirce's account, and in that case, to integrate some new views into Peirce's conception of the SCT, which might secure the self-corrective mechanisms of scientific method better.

On the other hand, as I concluded, Peirce did not develop the abductive phase of inquiry enough, as he did not provide an account or a technique for finding better alternative hypotheses than the refuted one, but, rather, he implied a way of modifying the hypotheses by correcting the premises of abductive inference and by the use of some normative criteria of admissibility of the hypotheses. Therefore I am going to appeal to Lavoisier's method of gradually corrected hypotheses, so as to evaluate these different aspects of Peirce's SCT.

This assessment will enable me to provide evidence of how the 'scientists' skill' contributes to correcting and finding better hypotheses, and how the 'scientific community' even in a scientific revolution approaches, gradually and continuously, the limit of inquiry and the approximate representation of the natural phenomena concerned with a certain particular discipline. Because as I concluded in the first part of the study, self-correctivenes and approach to the approximate representation of regularities in nature can work for single disciplines, while Peirce's notion of convergence upon truth is problematic, as it presupposes, apart from the unification of sciences, other valid premises that are questionable.

4.1. Historical study

Historical investigation though deals with past events through the examination of historical data, it faces difficulties, since it is theory-laden with philosophical background assumptions of the observer (historian in our case) or, as Peirce says, abductively drawn hypotheses are influenced by the background knowledge of the inquirer. Thomas Kuhn[228] also points out these difficulties, which had influence on the old historiographic tradition. Hilary Putnam emphasizes that there is interdependence between observations and inductive generalizations, as well as that there is no clear demarcation between facts and value judgments[229]; therefore one cannot distinguish clearly the presentation of facts from their interpretation. From this point of view, historical data, i.e. presentation of certain historical episodes, can serve as a basis to begin with, and its plausibility has to be examined through critical discussion of the different interpretations. For that reason after the presentation of the historical events I proceed to the discussion.

I have chosen to present historical data from different bibliographical sources expounded by historians of different schools of thought[230] that concern certain historical significant episodes. In case of controversial presentations of the historical episodes, I discuss them by taking into consideration their plausibility, the historical and social background as well as

[228] See Thomas Kuhn (1970), *Introduction: A Role of History*, pp. 1-9.

[229] See Hilary Putnam (1995), pp. 57-8.

[230] The historical data have been chosen from the works of the most well known historians of international bibliography, as well as the works of Lavoisier, Priestley, Kirwan and Cavendish. See for bibliographical sources and list of the historians of different schools: Partington '*Short History of Chemistry*', Gillispie Charles (1970-80) *Dictionary of Scientific Autobiography*, Roy Porter '*Cambridge History of Sciences: Eighteen-Century Science*' (2003), Cambridge University Press, Jan Golinski, *Science as Public Culture: Chemistry and Enlightenment in Britain, 1760-1820,* Cambridge: Cambridge University Press, (1992), and '*Readers Guide to the History of Sciences*' (2000), Fitzroy Dearborn Publishers, Chicago, pp. 127-29, and 407-8.

the original works of the protagonists of the story (Lavoisier, Priestley, Cavendish, Kirwan etc.), in order to evaluate them; while in case of different and essential interpretations, I discuss them in the next Chapter. As for the choice of the episodes, I took into consideration some criteria, which in turn will enable me to draw my conclusions, and, by following Kuhn's suggestion[231], I paid attention not to disturb the continuity and plausibility of the story. Such criteria were the different aspects of the SCT (self-corrective sorts of inference, progress of knowledge, criteria of admissibility of the hypotheses, scientists' skill, fallibilism and continuity of knowledge within the scientific community), as the heart of my investigation is the self-corrective character of the scientific method. Since each historian, according to his evaluation, is focused more on some aspects of a certain historical case, at the expense of some others; therefore there are no historical studies that include all the above mentioned aspects of the Chemical Revolution, which I have looked for. For that reason I had to inquire into the most, which explored these aspects, so as to complete the reconstruction. Certainly, the story of the Chemical Revolution has been told many times, but in this way it can be shed new light upon the well-known episode in the history of science by a new methodological theory.

4.1.1. Historiography

Historians who dominated from the mid-nineteenth to the mid-twentieth century belong to the historiography, which was characterized as 'positivist'[232], and it is represented mainly by Gillispie, Herschel, McKie, Meldrum, Partington and Rodwell. The main features of this school of thought were: (1) progressivism, in a sense that considered the Chemical Revolution as an episode in the emergence of a scientific approach to nature from a pre-scientific age identified with the phlogiston theory, (2) inductivism, as it emphasized the quantitative character of Lavoisier's method, which proceeded through experiments and moments of discovery, and (3) objectivism, since it regarded this episode as the triumph of truth over the darkness of ignorance.

Thomas Kuhn (1970) by characterizing the Chemical Revolution as example of a paradigm change, challenged this 'positivist' view of historiography with his model of Gestalt switch,

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[231] Kuhn in (1970a), *Notes on Lakatos*, p. 142, suggests writing history, as follows: *'His narrative must be continuous … his story must be plausible … must be constructed without doing violence to the data'.*

[232] For more about this characterization, as positivist historiography, and its features, see: McEvoy (1997), Bensaude-Vincent (1983), Rachel Laudan (1993), Musgrave (1976) and Donovan (1993).

rendered phlogiston chemistry more rational and provided a fresh impetus for a detailed historical investigation of Lavoisier's path to the Chemical Revolution that was underway (e.g. Guerlac). As a consequence, most of historians of the post-Kuhn era, due to this newly evaluated historical knowledge, rejected the 'positivist' historiography and explored other significant aspects of the Chemical Revolution, than observational data and logic. Among them are historians like Guerlac, Perrin, A. Thackray, Schofield, Bensaude-Vincent, Golinski, Holmes, Donovan, Beretta and Kim.

Henry Guerlac (1961) with his pioneering work provided the historical background of the development of chemistry before Lavoisier, and he suggested that the Chemical Revolution involved the synthesis of continental analytical chemistry and the results of British pneumatic chemistry[233]. Thackray and Schofield pointed out the 'profound failure of the Newtonian program' in the eighteenth-century chemistry and argued that Lavoisier did not free chemistry from the influence of George Ernst Stahl, so much as rationalize Stahl's program in a way that resisted the premature and unhelpful solicitations of corpuscular physics'[234].

According to Perrin[235], while Lavoisier pursued a program of methodological reform, in which he used physical instruments and measurements to solve chemical problems, the revolution he envisioned in the fall of 1772 'was a conceptual and theoretical one', based on the introduction of the idea of the gaseous state into the preexisting science of chemistry promulgated by his chemist teacher Guillaume Francois Rouelle. Perrin inclined to the positivist idea that scientific progress involves the replacement of older, less adequate statements about the world with newer, more adequate statements; therefore he doubted whether there was a sudden Gestalt switch from phlogiston to oxygen.

Bensaude-Vincent[236] explores Lavoisier's contribution to the Chemical Revolution within social and cultural context, and she reveals the attribution model imposed by the French chemist Dumas and the historians; therefore she raises the question whether 'too much importance has not been attached to the name of Lavoisier'. Jan Golinski identified

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[233] See Guerlac (1961), xvii: *'In the person of Lavoisier two largely separate and distinct chemical traditions seem for the first time to have been merged. At his hands, the pharmaceutical, mineral and analytical chemistry of the Continent was fruitfully combined with the results of the British chemists'.*

[234] See more Thackray (1970) and Schofield (1970).

[235] See Perrin (1986).

[236] See Bensaude-Vincent (1983).

eighteenth-century chemistry as a ‘didactic discipline’, and emphasizes the ‘capacity of individuals for autonomous self-expression’. ‘Resisting the powerful sway of disciplinarity’, some individuals, he says, ‘creatively” manipulate available resources to forge their own autonomous identity’[237].

Following Guerlac’s suggestion, Holmes[238] argued that whereas the continental tradition represented the activity of successive generations with a distinct disciplinary identity, pneumatic chemistry was pursued by ‘people who were not identified primarily as chemists’ and whose results were ‘not necessarily seen by contemporaries as more particularly belonging to chemistry rather than physics, or medicine’. Donovan[239] emphasizes the functional role of Lavoisier’s scientific work and sees him also as an experimental physicist due to his methodology, which consists of complex instrumentation and quantitative precision, as he incorporated into chemistry methodological and ontological principles derived from physics. Moreover, he accepts the epoch-making nature of Lavoisier’s work that involved the creation of a new science and brought revolution into chemistry that led to the formation of new theoretical statements and principles.

Beretta[240] in his thorough survey of the history of chemical language emphasizes the role of the new chemical nomenclature, expressed in algebraic terms, in establishing the new theory, and he suggests that the difference between the phlogistic chemists and Lavoisier lies in the different philosophies of language, i.e. on the one hand, the British philosophy of language outlined by Bacon and Locke and on the other hand, the French one inherited from Descartes. He also provides a detailed account of the reaction of the international scientific community to the new theory across Europe and America. Kim[241] is focused on the relation not only between physics and chemistry but also between chemistry and medicine in social context, and he suggests the contribution of the interdisciplinary work to the Chemical Revolution. All the above mentioned views of the historians along with the different methodological accounts given by the philosophers of science will be discussed in the relevant chapters together.

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[237] See Golinski (1998), pp. 66-78.
[238] See Holmes (2001), pp. 735-53.
[239] See Donovan Arthur (1993).
[240] See Beretta (1993).
[241] See Kim (2003) and (2008).

After this short presentation of the historiography about the Chemical Revolution, we can realize that in order to be able to merge all these different aspects into one study, one has to inquire into the works of all the previous mentioned historians. Therefore my research went through all these studies and took into consideration all these different aspects of the Chemical Revolution. In this historical study I exhibit all the four stages of Lavoisier's advancement, so to say, from his first hypothesis that 'air might be absorbed in calcinations and combustion' up to his theory of oxidation. But before that I present the historical background knowledge of that time, beginning with Boyle's experiments.

4.1.2. Boyle, calcination, combustion and phlogiston theory

Robert Boyle published in 1673 in his *Essays of Effluviums* the results of his experiments made on calcinations of metals, which showed that metals when heated in a closed container gained weight and turned into powdery substances, called calxes (nowadays metal oxides, M_xO_y). His explanation was that the gain in weight was produced by material particles of fire, which had been taken by the metal[242]. As Musgrave in his reconstruction argues, Boyle had not weighed the container before and after the calcination, but only the metal[243], therefore John Mayow in 1674 offered another explanation for the gain in weight, due to his experiments, which showed that in respiration and combustion air was consumed, namely air contained 'nitro aerial particles', which were essential for respiration and combustion. However, his quantitative results were not good enough to establish his claim (diminution of air in combustion 1/13 and in respiration 1/14). Therefore, although Mayow was the first to realize the importance of air in combustion, Boyle's notion remained prevalent and served as a basis for the 'phlogiston theory'[244].

This theory of combustion was first formulated by the German chemist Johann Joachim Becher (1635-1682) and developed by another German chemist Georg Ernst Stahl (1660-

[242] See McKie (1952), pp. 33-57, Aykroyd (1935), pp. 46-47, and Musgrave (1976). Guerlac, who in his thorough study (1961) provides the historical background before Lavoisier, on p. 10, characterizes Robert Boyle the pioneer of pneumatic chemistry and claims that his successors' work influenced Lavoisier's thought.

[243] Musgrave in (1976), pp 191-2, cites as evidence for this claim Lavoisier's notes on Boyle's experiments.

[244] See Aykroyd W. R. (1935), p. 48. McKie also in (1952), pp. 44-46, presents Mayow's experiments and concludes: '*He (Mayow) showed that a burning candle and kindled camphor both consumed air; and that a mouse consumed air in breathing … He had, however, established the fact of these decreases and he was the first to do so'*. Guerlac in (1961), pp. 118-9, argues that those experiments conducted first by Simon Boulduc and then by John Francis Vigani with calcination of antimony in an open vessel, have shown loss of weight; therefore they encountered Mayow's theory.

1734), who coined the term 'phlogiston'. According to this theory, all combustibles contained an 'inflammable principle', as Becher called it, and when combustibles were burned this principle of fire, which Stahl called 'phlogiston', was given off, in accordance with the following representation:

Metal → Calx + Phlogiston

According to the phlogiston theory, calcination was a slow combustion; therefore it could explain many phenomena and was initially accepted widely, as it gave a unified explanation of the apparently distinct phenomena of combustion and calcination[245]. However, there were several anomalies with this theory, since it could not explain the decrease of volume of air, the increase in weight in combustion, and certainly phlogiston could not be experimentally isolated. The proposed explanation for the decrease of volume by defenders of the phlogiston theory was that air was saturated with phlogiston and as a result took up lesser room than the ordinary air. The anomaly of the weight's increase was more troublesome, but it was not enough to reject the theory, since many phlogistonists apart from Boyle's explanations, committed to the Aristotelian view holding that phlogiston was lighter than common air, and when it was removed from a body, it immersed in air, and as a result caused a body to weigh more[246].

After Boyle's experiments several investigations of the aerial substances had been carried out by British chemists, like Joseph Black (1728-99) and Stephen Hales (1677-1761), which Henry Cavendish (1731-1810) developed further. Black showed that when charcoal (C) was burned, 'fixed air' (today's CO_2) was produced, and this 'fixed air' (he called 'fixed' because he thought it to be common air fixed in the mild alkalis) was the same like the one was produced in respiration[247]. Hales on the other hand, had improved Boyle's simple apparatus and was able to conduct experiments with air and collect the 'fixed air'. In his work *Vegetable Staticks* (1727) described his experiments and the properties of the two

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[245] See Beretta (1993), pp. 124-33, Donovan Arthur (1993), pp. 47-48, Guerlac (1961), pp. 113-25, McKie (1952), pp. 19-22, Partington (1937), pp. 87-8, and Aykroyd W. R. (1935), pp. 46-47.

[246] See more Guerlac (1961), pp. 113-25, McKie (1952), pp. 47-49, Musgrave (1976), pp 188-89, and Aykroyd W. R., (1935), pp. 48-49.

[247] See Beretta (1993), pp. 171-2, Guerlac (1961), pp. 12-14, McKie (1952), pp. 49-53, and Aykroyd W. R., (1935), pp. 48-49. The reaction, as we know today, is represented as follows: $C + O_2 \rightarrow CO_2$.

forms of air, namely free and fixed air[248]. Cavendish, exponent of the phlogiston theory, in 1766 with an improved apparatus of Hales (eudiometer) isolated over water and differentiated from common air Black's 'fixed air' (CO_2) and his new discovery 'inflammable air' (today's hydrogen), which was given off, when metal was calcinated in vitriolic acid. His interpretation was that, when metals were immersed in acid, their phlogiston flew off and formed 'inflammable air', a notion that implied that phlogiston could be identified with 'inflammable air'[249].

4.1.3. Lavoisier's first vague hypothesis

Antoine Lavoisier (1743-1794) became interested in combustion in early 1772, after some experiments on diamonds, conducted by the French chemist Macquer (1718-84), one of Lavoisier's seniors, which showed that diamonds were destroyed by heating, while in the absence of air heating had no effect on them. The same experiments had done Boyle and Mayow before, but with presence of air, therefore they concluded that it was evaporation of diamonds. These new results, however, connected, for Macquer, destruction of diamonds more with combustion[250]. On the other hand, Hales' experiments had shown that air exists in two forms, as free and as fixed, and in the latter form looses all its properties; therefore effervescence could be connected to the air's release [251]. Lavoisier, as he had reservations about phlogiston theory, he became interested in experiments on fixed air, which could be removed from substances by heating. Guerlac in his historical study (1961) concludes that Lavoisier being influenced by Hales was led to attribute effervescence to the release of air, and already in August 1772 held that weight increase of metals in calcinations was due to the addition of air[252], while Meldrum (1930, p. 6) characterizes this belief as 'fixed idea'. In my view, Lavoisier held this vague hypothesis that 'in calcinations and combustions might be absorbed air' not only due to Hales' work, but also to Mayow's theory; therefore he

[248] See McKie (1952), pp. 49-53. Guerlac in (1961), pp. 25-35, argues that Lavoisier through his teacher chemist G. F. Rouelle knew Hales' work and came to appreciate the important role of air. Donovan Arthur also in (1993), pp. 92-93, shares the same view and cites as evidence Lavoisier's notes from his *Ouevres*.

[249] See Aykroyd (1935), pp. 79-81, McKie (1952), pp. 53-56, Guerlac (1961), p. 24, Beretta (1993), p. 208, and Musgrave (1976), on p. 190, who cites notes of Priestley. The reaction of zinc with vitriolic acid is represented today as follows: $Zn + H_2SO_4 \rightarrow ZnSO_4 + H_2$.

[250] Guerlac (1961), pp. 79-90, by reading Lavoisier's *Memorandum* concludes that although Lavoisier initially supposed that diamond's destruction was connected with combustion, after August 1772 he abandoned this view and connected combustion more with the phenomenon of chemical effervescence.

[251] See Donovan Arthur (1993), pp 75-109, Guerlac (1961), pp. 79-90, McKie (1952), pp. 101-107, and Aykroyd (1935), pp. 51-52.

[252] See Guerlac (1961), pp. 192-96, also Donovan (1993), pp 75-109.

repeated the same experiments that Mayow conducted to confirm his hypothesis[253]. Indeed, in October 1772, as Lavoisier reported on a manuscript, his experiments showed that phosphorus and sulphur gained weight in combustion and became calxes (nowadays metal oxides, M_xO_y)[254], according to the following representation for sulphur:

Sulphur + air → Calx of sulphur, or in nowadays representation: $S + O_2 \rightarrow SO_2$

On the other hand, calx of lead (PbO) when heated[255] with charcoal and turned into metal, air was liberated. This fact, for Lavoisier, was the confirmation of his conjecture that the same air that was absorbed in calcinations and combustions it was liberated in reduction with charcoal. Therefore he recorded his results on a note, sealed it and deposited in with the Academy on November 1, 1772[256]. Certainly, as we know today, in his experiment, taken as a confirmation of his hypothesis, it was not oxygen that was liberated, but carbon dioxide (CO_2), in accordance with the following representation:

$2PbO + C \rightarrow 2Pb + CO_2$

While Lavoisier held that this reaction is represented as follows:

Calx + charcoal → metal + air

On the other hand, the exponents of the phlogiston theory[257] held that the same reaction could have been represented as follows:

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[253] Aykroyd in (1935), pp. 51-52, holds that Lavoisier repeated Mayow's experiments and he also tried to obtain a copy of Mayow's work. As we can also read in Bernadette Bensaude-Vincent (1983), p. 55, Lavoisier's disciple Fourcroy in his article *Chimie*, 1792, attributed the Chemical Revolution more to the advent of pneumatic chemistry from England, while the German chemist W. Ostwald in his *L' evolution d' une science la chimie*, 1914, on p. 66, emphasized the influence of Mayow's work on Lavoisier's success. Compare also with Beretta (1993), p. 22.

[254] The original manuscript was reprinted in French and published by Guerlac in (1961), pp. 224-28.

[255] See more Donovan Arthur (1993), pp 92-103, also McKie (1952), pp. 98-102.

[256] See Guerlac (1961), 224-28. Prof. McKie in (1952), pp. 101-102, translates in English this note of Lavoisier's manuscript, as follows: '*… This increase of weight arises from a prodigious quantity of air that is fixed during the combustion … This discovery, which I have established by experiments that I regard as decisive, has led me to think that what is observed in the combustion of sulphur and phosphorus may well take place in the case of all substances that gain weight in combustion and calcinations… experiment has completely confirmed my conjecture; I have carried out the reduction of litharge (calx of lead), …, and I observed hat just as the calx changed into metal, large quantity of air was liberated…*'.

[257] See Gibbs (1965), *Joseph Priestley*, pp. 119-120.

Calx + charcoal (which included phlogiston) → metal + dephlogisticated air

By February 1773 Lavoisier had stated three priorities in his vision of chemistry: 1. to examine the chemical role of air, 2. to formulate a coherent theory of combustion and calcination and 3. to devise a new nomenclature for pneumatic chemistry[258]. Following this vision he continued his experiments and repeated Boyle's work, but this time he weighed the entire sealed vessel before and after heating, and he found that the weight was the same before and after, while only the lead and tin inside had increase in weight. These results, presented in 14 of April 1774 to the Academy, refuted Boyle's claim that gain in weight was produced by material particles of fire, which had been taken by the metal, because the increase in weight came from inside the container, whereas Boyle had claimed that it came from outside. Lavoisier's explanation was that the increase in weight was due to the air contained in the vessel, which during the combustion was absorbed by the metal[259]. Although Lavoisier was the first to realize that the increase in weight on calcination was due to atmospheric air, he had not yet established his theory experimentally until Joseph Priestley (1733-1804) discovered and isolated experimentally his new 'dephlogisticated air'.

4.1.4. Priestley's 'dephlogisticated air', Lavoisier's second hypothesis

Priestley in August 1774 conducted experiments with heating *mercurius calcinatus per se* (mercury calx: HgO), a red powder produced by heating mercury for a long time in air, and he found out that air was readily given off[260]. This air separated from the precipitate of mercury had some unexpected properties that differed completely from those of fixed air: it proved to be insoluble in water; a candle burned in it with remarkably vigorous flame, and a piece of red-hot wood sparkled in it and was rapidly consumed[261]. Priestley visited Paris in

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258 Beretta (1993), who in his study emphasizes Lavoisier's linguistic realism and his contribution to the establishment of the new chemical nomenclature, on p. 171, concludes to these three Lavoisier's priorities. He cites as evidence Lavoisier's Memorandum of February 20, 1773, cited originally in French by Guerlac (1961), pp. 228-30, and translated by Meldrum (1930), pp. 9-10.

259 McKie in (1952), pp. 107-108, cites Lavoisier's memoir, which was read before the Academy on November 12, 1774 and published in the December issue of Rozier's journal (*Observations sur la Physique*). See also Aykroyd (1935), pp. 51-52. Musgrave in (1976), pp 191-2, cites Lavoisier's notes on Boyle's experiments from his *Oeuvres* II, pp. 105-21.

260 This experiment is regarded as one of the most 'crucial' in the Chemical Revolution, as I am going to show later, and by some historians as the discovery of oxygen. See Conant (1950).

261 See Aykroyd W. R. (1935), pp. 54-5, McKie (1952), pp. 115-16, and Donovan Arthur (1993), pp 136-7. Priestley's account in his (1775-79), vol. 2, section iii, is full of words like 'astonishment' and 'surprise'

October 1774, and in conversation with Lavoisier and other members of the Academy he mentioned his unpublished discovery of the new air, and he spoke of its properties[262].

This property of mercury calx, that it could be converted into metallic mercury on heating without charcoal, was known to the French chemists before Priestley's informal report, however, the properties of the air released from it were not known. Actually, on September 3, 1774, Cadet, colleague of Lavoisier, had reported to the Academy that he had found a way to reduce the precipitate of mercury to the metallic state without using charcoal[263]. Therefore Lavoisier repeated Priestley's experiment in November and again, more carefully, in February and March of the following year. Afterwards, on April 26, 1775, he read before the Academy a Memoir entitled: '*On the Nature of the Principle which combines with the Metals during their Calcination and increases their Weight*', a paper which he revised later and it is regarded as one of the most interesting in the history of science[264].

There he described his earlier experiments, i.e. when he had heated calxes with addition of charcoal, in that reduction the charcoal was consumed. It followed therefore, for Lavoisier, that the air given off in the reduction was not a simple substance, but a combination of at least two, and therefore he concluded that it was 'fixed air' (CO_2 as explained with the chemical representation of the calx of lead), which means that with this paper Lavoisier *corrected his initial hypothesis* of November 1772. In the same paper he also explained that due to the difficulties of reducing iron oxides without charcoal he decided to use calx of mercury[265]. Then by appealing to his experiments with mercury calx he described the properties of the air given off in the reduction, as follows: '*it supported animal respiration, it lighted candles and burning bodies, and it burned with brilliant flames*'. Therefore he

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about his discovery. It is also cited by Musgrave (1976), p 194. The reaction, as we know today, is represented as follows: $2HgO \rightarrow 2Hg + O_2$.

262 See Cooper Thomas (1806), p. 257, who cites Priestley's memoir. Also Aykroyd W. R. (1935), pp. 61-2, McKie (1952), pp. 115-16, Donovan Arthur (1993), pp 136-7, and Bensaude-Vincent, Stengers (1996), p. 81.

263 See McKie (1952), pp. 122-3 and Donovan Arthur (1993), pp 136-37. Lavoisier gives a detailed description in Chapter 3, *Elements of Chemistry*.

264 This is Lavoisier's famous 'Easter Memoir', which was published in Rozier's *Journal de Physique*, May 1775. Its revised version appeared in 1775 volume of the *Memoires de l' Académie des Sciences*, published in 1778. Translations of the two versions are printed in Conant (1950), pp. 22-28.

265 See Ibid, also Aykroyd W. R. (1935), pp. 64-5, Lavoisier's notes from this memoir: '*The difficulties arising from the nature of iron, due to the refractory nature of his calxes and the impossibility of reducing them without addition of charcoal I came to regard it as un-surmountable. Therefore I decided to study another kind of calx, which would have the property of being reducible without addition. Mercury percipitate per se … seemed to be suitable for this purpose I had in mind*'.

concluded that this air appeared to be common air but in a purer state[266], and as a result, the principle combined with metals during calcination and combustion was air, but purer than the one of atmosphere. In the first version of the memoir Lavoisier concluded that it was *'elemental air in a highly purified form'*, however after having been criticized for the word 'elemental' by Priestley in December 1775, as we are going to see, he revised it to *'more pure, more respirable … than the air of the atmosphere'*[267].

At the same time Priestley continued also his experiments with his new air obtained from mercury calx. This time he put a mouse into its sample and found out that survived much longer than it would have in common air. He also breathed himself and found out that the feeling to the lungs was better than that of common air in respiration; therefore he described it as 'pure air'. On March 15, 1775 he reported his discovery in a letter to the President of the Royal Society that was published in the *Philosophical Transactions*[268], and which is also considered to be one of the most interesting papers in the history of science. But his interpretation of the quality of the new air was different than Lavoisier's, since he was exponent of the phlogiston theory. According to this theory, in respiration phlogiston was exhaled from the lungs, just as it was given off by all burning bodies, which was absorbed by the common air, and it was supposed that a limited amount of air could absorb only a limited amount of phlogiston, as afterwards it became saturated. The fitness of this new air for combustion and respiration was due to its pureness, for Priestley, because he thought it to be free from phlogiston, therefore he named it *'dephlogisticated air'*[269].

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[266] See Aykroyd W. R., (1935), pp. 64-5, who cites Lavoisier's notes from his *Elements of Chemistry:* '*A taper burned in it with a dazzling splendour; and charcoal instead of consuming quietly as it does in common air, burnt with a flame…*' Also Donovan Arthur (1993), pp 137-38, cites Lavoisier's notes from his memoir, as translated by Conant (1950): '… *It is the air itself entire without alteration and decomposition, even to the point that if one sets it free after it has been so combined, it comes out more pure, more respirable, if this expression may be permitted, than the air of the atmosphere, and is more suitable to support ignition and combustion'.*

[267] See Donovan Arthur (1993), pp 137-38, who cites Lavoisier's two versions of the memoir, as translated by Conant (1950).

[268] See McKie (1952), pp. 118-21, Aykroyd W. R., (1935), pp. 66-7, and Donovan Arthur (1993), pp 139-40. Priestley quotes it in his *Experiments and Observations* Volume 2, p. 90. The description of this new gas was as follows: *'But the most remarkable of all the kinds of air that I have produced by this process is, one that is five or six times better than common air for the purpose of respiration, inflammation, an, I believe, every other use of atmospheric air… A candle burned in this air with an amazing strength of flame… But to complete the proof of the superior quality of this air, I introduced a mouse into it; … it lived at two different times, a whole hour and was taken out quite vigorous…'*

[269] See Priestley, Ibid: *'As I think I have sufficiently proved that the fitness of this air for respiration depends upon its capacity to receive the phlogiston exhaled from the lungs, this species may not be improperly called depholgisticated air'.*

4.1.5. The nature of the 'pure air', Lavoisier's third hypothesis

After these discoveries a new problem arose for Lavoisier, because, if this 'new air' was more pure than atmospheric air, then atmospheric air might be not an elementary body, as he considered it to be, but a compound. Apart from that, in his experiments with tin (Sn) he had observed that the part of the air combined with the tin and the part of the air left after calcination had different density[270]. On the other hand, Priestley in December 1775 with his publication of his *Experiments and Observations* challenged Lavoisier's notion that the atmospheric air was an elementary body[271], therefore, as we have seen, this critique forced Lavoisier to revise his 'Easter Memoir' 1775. Priestley believed that atmospheric air was composed of nitrous air (today's nitric oxide), earth and phlogiston. Lavoisier argued against that 'when the calx of mercury (HgO) was heated and reduced to the metallic state, the metal had not gained or lost weight; therefore it could have not gained or lost phlogiston'[272]. For that reason Lavoisier between 1775 and 1776 continued his experiments, in order to find out the connection among nitrous air, air and acidity, since it was known that calxes of sulfur and phosphorus formed acids when mixed with water. In his Memoir read to the Academy in April 1776 he wrote that the same air, which was contained in it those calxes, was produced by analyzing acid of phosphorus and nitrous acid. Therefore he concluded that 'the same air that combined with metals and produced calxes it was given off by the analysis of acids'[273].

What was left for him was to examine whether atmospheric air was a simple body or a compound, and in the latter case he had to distinguish the components of the atmospheric air. He designed now his experiments in this way, so as first to take the atmospheric air in pieces and then to put it together again, as he explained in his memoir read to the Academy

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[270] See McKie (1952), pp. 132-3, Aykroyd W. R., (1935), pp. 67-8, and Donovan Arthur (1993), pp 140-41.

[271] Priestley quotes it in his *Experiments and Observations* Volume 2, pp. 320-3 and Conant in (1950), p. 90: '*Mr. Lavoisier, as well as Sig. Landriani, Sig. F. Fontana, and indeed all other writers except myself , seems to consider common air as a simple elementary body; whereas I have for a long time considered as a compound*'.

[272] See Lavoisier *Oeuvres* II, pp. 137-8: '*… I would reply then that when the mercury leaves the operation exactly as it entered it, there is no evidence that it has lost or gained phlogiston…*'.

[273] See McKie (1952), pp. 135-36, and Donovan Arthur (1993), pp. 144-45. Lavoisier quotes also these experiments in his *Oeuvres*, volume II, pp. 137-8, and he concludes: '*It is clear that air is not composed of a nitrous acid (nitrous air plus water), as Mr. Priestley supposes, but rather nitrous acid is composed of air*' (parenthesis mine).

on May 3, 1777[274]. First, he deprived the atmospheric air of his supposed 'pure air' (oxygen) through the calcination of mercury and observed that the air was decreased by about one sixth of its volume (8-9 cubic inches out of 50)[275]. The remained air (nowadays nitrogen), he found, would not support life and combustion, because it asphyxiated animals and extinguished flames. Then he reduced it by heating the mercury calx and recovered the same quantity of the 'pure air' (8-9 cubic inches) that had been combined in the calx. After having added this recovered air to the remained asphyxiating residue from the calcination, he obtained air with the properties of the original common air. This restored air no longer extinguished flames but supported also respiration[276]. As it had been shown earlier by Black, when animals breathed in air, their respiration led to the formation of 'fixed air' (CO_2); however, Lavoisier noted that in respiration only the respirable 'pure air' (oxygen) was changed into the 'fixed air' (CO_2), while the asphyxiating part of the common air, the noxious air, *mofette* (nitrogen), passed into the lungs and came out again unchangeable.

To sum up, Lavoisier with his experiments established that the air of the atmosphere was not a simple substance but a compound, composed of 'pure air', which Priestley called *'dephlogisticated air'*, and of *mofette* or noxious air, whose nature was unknown[277]. Besides, he introduced a new classification of gases, as follows: (a) common atmospheric air, (b) pure air, (c) noxious air and (d) fixed air.[278] What was left for Lavoisier then was to classify and define that 'pure air', since he had become familiar with its chemical properties.

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274 See Aykroyd W. R., (1935), pp. 68-9, and Donovan Arthur (1993), pp 147-48. Lavoisier quotes also these experiments in his *Oeuvres*, volume II, pp. 176-81.

275 Lavoisier in *Oeuvres*, volume II, pp. 176-81, describes the result of this experiment as follows: '*I observed that the air it had contained was diminished by 8 to 9 cubic inches, that is to say, by about a sixth of its volume; at the same time there had been formed a considerable quantity, approximately 45 grains, of mercury percipitate per se, or calx of mercury'*.

276 Ibid: *'By this operation I recovered almost the same amount of air as had been absorbed by the calcination, that is to say, 8 or 9 cubic inches, and on combining these 8 to 9 inches with the air vitiated by the calcination of mercury, I restored this air exactly enough to its state before calcination, i.e., to the state of common air: the air thus restored no longer extinguished flame, no longer caused the death of animals breathing it, and finally was almost as much diminished by nitrous air as the air of the atmosphere'*

277 See Lavoisier's *Oeuvres*, volume II, pp. 176-81*: 'I have established in the foregoing memoirs that the air of the atmosphere is not a simple substance, an element, as the Ancients believed and as has been supposed until our own time: that the air we breathe is composed of respirable air to the extent of only one quarter and the remainder is a noxious air, which cannot alone support the life of animals or combustion or ignition'*.

278 Ibid*: 'I feel obliged, consequently, to distinguish four kinds of air or air-like fluids: First, atmospheric air; that in which we live, which we breathe. Secondly, pure air, respirable air; that which forms only a quarter of atmospheric air, and which Mr. Priestley has very wrongly called dephlogisticated air. Thirdly, the noxious air, which makes up three quarters of atmospheric air and whose nature is still entirely unknown to us. Fourthly, fixed air, which I shall call henceforward by the name of acid of chalk'*.

As explained, Lavoisier knew that calxes of sulphur and phosphorus, when they mixed with water formed powerful acids, and that these calxes contained ‘pure air’ that was fixed during their calcination. By analyzing nitrous acid he had found out that it contained also ‘pure air’; therefore he concluded that this ‘pure air’ makes nitrous acid and all acids acidic, since when metals combined with ‘pure air’ in calcination, formed calxes, while these calxes when mixed with water, they formed acids[279]. His final conclusion was that this ‘pure air’, part of the atmospheric air, played a central role, besides respiration and combustion, also in the formation of acids; therefore in a memoir submitted to the Academy in September 1777, he named it *oxigene*: begetter of acids (from the Greek words, *οξύς*: oxys and *γείνομαι*: geinomai)[280]. Certainly, this name (begetter of acids), as principle of acidity, could not represent all acids (e.g. HCl); therefore this definition of acidity after nearly 100 years was proved to be false by Arrhenius.

4.1.6. Lavoisier’s fourth hypothesis, acceptance of his theory

Although Lavoisier had demolished the foundation of phlogiston theory, his theory of combustion had not been accepted by the majority of the scientific community, because it could not explain the combustion of ‘inflammable air’ (hydrogen), which had been isolated and studied by Cavendish, as we have already seen. Since according to Lavoisier combustion was combination with oxygen and the product that ‘inflammable air’ gave on combustion was unknown[281], therefore its combustion was a serious hindrance to the acceptance of his new theory until 1783.

On the other hand, Priestley in 1781 made experiments on the explosion of ‘inflammable air’ (hydrogen) with common air by means of electric spark and noticed the formation of ‘dew’ (moisture), which was accompanied with loss of weight. Therefore he informed Cavendish, who had studied thoroughly the ‘inflammable air’ and was also exponent of the phlogiston theory, in order to provide, as Priestley thought, further support for this theory. Cavendish decided to repeat the experiments himself with a great care, of which he was

[279] See Lavoisier *Oeuvres* II, pp.134-7.

[280] See Ibid. pp.248-60. This memoir was not read until November 1779 and was not published until 1781. See also Beretta (1993), p. 178, Crosland (1973), McKie (1952), pp. 134-8, and Donovan Arthur (1993), pp. 143-45.

[281] See McKie (1952), pp. 159-66, Aykroyd W. R., (1935), pp. 79-85, Gillispie (1960), pp. 227-9, and Donovan Arthur (1993), pp. 153-5.

very capable, and he found out no change in weight, but his extraordinary discovery was that one fifth of common air and ‘inflammable air’ formed this dew, which was proved to be pure water[282]. Priestley had also informed James Watt, engineer inventor of the steam engines and disciple of Joseph Black. Watt in his letter to Black of 21 April 1783 and to Priestley of 26 April 1783 expressed the idea that ‘water was composed of dephlogisticated and inflammable air’[283]. This view, as Watt wrote to Priestley, he intended to read to the Royal Society of England. However, Priestley convinced Watt to withdraw it. This letter provoked the well known *Water Controversy*[284], one of the most prolonged battles about scientific discovery; because later Watt with a letter claimed that he was the inventor of the water’s composition[285]. Priestley in his letter, dated 14 December 1783, addressed to the President of the Royal Society, Sir Joseph Bans, agreed with Watt’s claim[286]. I do not aspire here to provide a solution to the Water Controversy, which has been disputed since years, however, by taking into consideration the two letters of Watt and Priestley respectively, I would rather agree with Miller (2003), who in his thorough historical study on water’s discovery concludes that Watt was before Cavendish in this theory; however this discovery was attributed to Cavendish due to his aristocratic origin and as a part of attribution model.

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[282] See Partington (1961), pp. 332-35, Aykroyd W. R., (1935), pp. 79-80, and Donovan Arthur (1993), p. 155. Priestley describes this experiment in *Experiments and Observations* (1781), Vol. 2, pp. 395-98. Cavendish describes his experiments in (1784-5), cited by McKie (1952), pp. 164-5, and concludes: *‘By the experiments with the globe it appeared that when inflammable air and common air are exploded in a proper proportion, almost all the inflammable air and one fifth of the common air lose their elasticity and are condensed into dew. And by this experiment it appears that this dew is plain water’.* The representation of this reaction, as we know today, is as follows: $2H_2 + O_2 \rightarrow H_2O$.

[283] See Robinson E. and McKie (eds.) (1970), *Partners in Science*, pp. 124-27, and Aykroyd W. R. (1935), p. 82, Watt’s conclusion*: ‘Are we not to conclude that water is composed of dephlogisticated air and phlogiston deprived of part of their content of elementary heat?’*

[284] For more about water controversy, see Aykroyd W. R., (1935), pp. 79-85, Edelstein S. (1948), Partington (1961) vol. 3, pp. 345 -62, Schofield (1964) and Miller (2003).

[285] See Watt’s letter to Mr. Fry cited in Aykroyd W. R., (1935), p. 85, Edelstein S. (1948):*’The papers which I mentioned to you that I had written on the composition o f water, have been read at the Royal society, I am told with great applause. But I have had the honour, like other great men to have my ideas pirated. Soon after I wrote my first paper on the subject, Dr. Blagden explained my theory to Mr. Lavoisier at Paris; and soon after that Mr. Lavoisier invented it himself and read a paper on the subject to the Royal Academy of Science. Since that, Mr. Cavendish has read a paper to the Royal Society on the same idea, without making the least mention of me’.*

[286] See Priestley’s letter to Sir Banks, cited in Edelstein S. (1948) and Miller (2003), pp. 44-5:*’Mr. Watt is the person who is properly concerned in this business. For the idea of water consisting of pure air and phlogiston was his, I believe before I knew him; and will find it in the letter, which he addressed to me, which was delivered along with the last paper which I sent to the Royal Society, but which he afterwards withdrew’.*

Returning to Cavendish’s experiments, these experimental discoveries led him to the formulation of a new version of the phlogiston theory, which the Irish chemist Richard Kirwan (1733-1812) partly also adopted[287]. Cavendish regarded inflammable air (hydrogen) water united with phlogiston, while dephlogisticated air (oxygen) regarded as water deprived of its phlogiston. Therefore in the reaction between the two airs (condensation) water was produced, since the excess of phlogiston in the one air was supposed to make up for the lack of it in the other air, as follows:

$$\begin{array}{lcl} \text{Dephlogisticated air} & = & \text{Water} - \text{Phlogiston} \\ + \text{ Inflammable air} & = & \text{Water} + \text{Phlogiston} \\ \hline \end{array} \qquad (1)$$

$$\text{Dephlogisticated air} + \text{Inflammable air} = 2\ \text{Water}$$

Although the experiments had been made in 1781, his memoir was read before the Royal Society in January 1784[288]. In June 1783, Cavendish’s assistant Charles Blagden visited Paris and gave Lavoisier a full account of the experiments on water, as we also have read in Watt’s letter. Lavoisier decided to carry out the synthesis of water together with Laplace on June 24th in the presence of Blagden and he gave the Academy the next day a brief account of his experiment[289]. Even though the quantitative results of that experiment were inferior to those of Cavendish, the conclusion reached by Lavoisier was of greater importance. He concluded that water was not a simple substance, but it was composed of inflammable air and oxygen, just like air a few years previously was shown to be a compound. These two discoveries marked the birth of modern chemical science, since the long lived doctrines of Peripatetics were finally disproved.

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287 Kirwan in his *Essay on Phlogiston and the Constitution of Acids* (1789), pp. 38, 168, cited by Pyle (2000), identified phlogiston with inflammable air. According to his theory, the phlogiston in the metal reacts with dephlogisticated air to form fixed air.

288 See Cavendish’s conclusion, *Experiments on air*, Philosophical Transactions, 74, pp. 119-53: *‘We must allow that dephlogisticated air is in reality nothing but dephlogisticated water, or water deprived of its phlogiston … and that inflammable air is water united to phlogiston’.*

289 See McKie (1952), pp. 163-68, Aykroyd W. R., (1935), pp. 79-87, and Donovan Arthur (1993), p. 155. The brief note, as cited in Aykroyd W. R. (1935), p. 83, is as follows: ‘*Lavoisier and de Laplace announced that they had lately repeated the combustion of combustible air with dephlogisticated air; they worked with about sixty pints of the air and the combustion was made in a closed vessel: the result was pure water*’.

But Lavoisier's theory had not been able yet to provide better explanation of combustion than the new revised by Cavendish phlogiston theory. The controversial reaction was the solution of a metal in an acid with the formation of a salt and the evolution of inflammable air, which we may represent as follows:

Metal + acid + water → salt + inflammable air (2)

Lavoisier's interpretation was that both inflammable air and oxygen had come from the water. Its oxygen had converted metal into a calx, and calx in turn had combined with the acid to form salt. We may represent it as follows:

Metal + acid + water (inflammable air + oxygen) = calx (metal + oxygen) + acid + inflammable air = salt (calx + acid) + inflammable air (3)

On the other hand, for Cavendish and the supporters of the phlogiston theory, the salt was produced by the combination of the calx with the acid and a part of water with phlogiston formed the inflammable air according to the reaction (1). We may represent it as follows:

Metal (calx + phlogiston) + acid + water = calx + acid + phlogiston + water = salt (calx + acid) + inflammable air (phlogiston + water) (4)[290]

The crucial reaction for the validity of the reaction (4) was the validity of the reaction (1); therefore Lavoisier's next series of experiments were to decompose the water, so as to falsify this reaction (1), and to show that the decomposition of the water could have taken place without any presence of phlogiston. Indeed, he proceeded with these experiments in 1784, which demonstrated that water can be analyzed into oxygen and inflammable air. Furthermore, he showed that *the weight of oxygen and inflammable air was nearly equal to the weight of the water*[291]*, whereas no quantity of phlogiston was isolated*[292].

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[290] See McKie (1952), pp. 172-73, and Donovan Arthur (1993), pp. 155-56.

[291] The representation of this reaction, as we know today, is as follows: $2H_2O \rightarrow 2H_2 + O_2$.

[292] See McKie (1952), pp. 167-74, Aykroyd W. R., (1935), pp. 84-87, Donovan Arthur (1993), pp. 155-6, and Gillispie Charles, (1960), pp. 202-59. As reported, the engineer Meusnier, guided by Lavoisier, in a series of experiments in 1784 demonstrated that water could be analyzed into oxygen and inflammable air. Priestley in his *Scientific Autobiography*, p. 221, mentioned producing inflammable air from water.

This marked the beginning of the end, but not the end, of the phlogiston theory, since although the main objection to Lavoisier's theory had been removed they were still doubts about the validity of that crucial experiment. McKie[293] concludes that after the discovery of the decomposition of water *'the long standing objection to Lavoisier's theory was removed'* and *'soon afterwards chemists began to accept the new theory'*. While Donovan[294] regards as crucial experiment the one of 1785, when Lavoisier and Meusnier produced inflammable air from water in order to use it as lifting agent for balloons therefore he concludes: *'The happy conjunction of truth and utility in eighteenth century science had once again been demonstrated'*.

However, if we study closely this analysis of water conducted by Lavoisier, we can realize, in my view, that it was conducted indirectly, i.e. water was admitted at the upper end of a slopping iron gun barrel heated. The iron was calcinated in heating and was calculated the increase in weight, while the inflammable air was collected and measured. But in this way it was not possible to distinguish the increase in weight of the outer from the inner surface of the iron, because the outer surface was calcinated by the air and not by the decomposed water. As a result the quantitative results that showed the weight of oxygen and inflammable air nearly equal to the weight of the water, in my opinion, they were not so accurate, as they were presented[295].

Therefore they were still doubts about the decomposition of water, correctly in my view, and as a result about Lavoisier's theory, posed mainly by the British chemists, i.e. Cavendish, Keir, Dickson, Kirwan, Black etc.[296]. Kirwan and Black after having failed to isolate phlogiston abandoned the phlogiston theory in 1791, accepted the new Nomenclature (new chemical names and symbols) introduced by Guyton, Lavoisier, Berthollet and Fourcroy, but they did not accept completely the new theory. This new nomenclature was introduced in 1787 and reflected the chemical constituents of substances in accordance with Lavoisier's new theory. Historians disagree about the origin of the idea of the *Méthode*. Guerlac (1961) and Partington (1961) suggest that Guyton was the one who played the most important role, while Beretta (1993) and Crosland (1978) attribute it more to Lavoisier. In my opinion, Guyton de Morveau, who was engaged in reforming the

293 See McKie (1952), pp. 173-4.
294 See Donovan Arthur (1993), p. 156.
295 See McKie (1952), pp. 167-74, and Aykroyd W. R. (1935), pp. 84-87.
296 Beretta (1993), pp. 231-3, and pp. 289-99, provides a detailed account of the reactions to Lavoisier's theory. See also Russell Collin (2003).

nomenclature in 1782, after having accepted Lavoisier's theory in early 1787, he agreed to work with him in devising the new chemical nomenclature; however the idea of reforming the new chemical nomenclature was already presented first in Lavoisier's Memorandum of February 20, 1773, as we have seen[297].

To return to the doubts about Lavoisier's theory, Kirwan had held that Lavoisier's theory could not explain the production of another 'inflammable air' produced by him after heating dry charcoal with water[298], because carbon monoxide was also inflammable. As he admitted in 1791, he tried to produce fixed air by inflammable air but he failed, and this fact led him to abandon phlogiston[299]. This anomaly was cleared up after the discovery of carbon monoxide in 1800 and contributed to the acceptance of Lavoisier's theory[300]. In addition, after the discovery of the electrolysis of water by the British Anthony Carlisle and William Nicholson in 1800 and by the use of the Voltaic pile, which showed the exact composition of water, the last doubts were removed by both British and the Italian adherents of galvanism[301]. We can infer, then, from all the previous data that by 1800 the whole idea of phlogiston was abandoned, as it became unnecessary, since Lavoisier's theory could explain the same phenomena without it, while phlogiston was never isolated.

Certainly, Lavoisier still held the false view that heat was a material, though imponderable entity, which entered into combination with all substances and was responsible for the heat. He called it 'caloric', an entity which was never isolated[302]. Later on by the mid-19th century it was proved that heat was a motion of matter and not a substance, a theory that evolved into the science of thermodynamics[303]. But this error was disregarded in experiments, since it was imponderable, and probably Lavoisier being aware of this error

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[297] The new nomenclature, *Méthode de nomenclature chimique*, was published by the end of August 1787. See more Beretta (1993), pp. 184-87, Crosland (1978), p.174, Guerlac in Gillispie (1970-80), vol. 7, p. 80, and Partington (1961), volume 3, p. 481.

[298] See Pyle (2000), p. 115. Kirwan cites these experiments in (1789), pp. 182-3, which can be represented with the following, nowadays reaction: $C + H_2O \rightarrow CO + H_2$.

[299] Beretta (1993), on p. 292, cites Kirwan's letter to Berthollet: '*Finally I lay down my arms and abandon phlogiston. I see clearly that there is no authenticated experiment attesting to the production of fixed air by pure inflammable air'.*

[300] The reaction can be represented as follows: $2C + O_2 \rightarrow 2CO$.

[301] See Russell Collin (2003), Beretta (1993), pp. 309-15.

[302] See Gillispie Charles, (1960), pp. 202-59, Gregory (1934), p. 190, 203, and Musgrave A. E., (1976), p. 186, who cites Lavoisier's notes from 1787, as follows: '*When a metal is heated to a certain temperature … it becomes capable of decomposing vital air, from which it seizes the base, namely oxigene, and sets the other principle , namely the caloric at liberty'.*

[303] See Cardwell, D.S.L. (1971).

did not insist on this view, therefore he never figured the caloric on his balance-sheets. Some historians (i.e. Gillispie and Beretta) tried to underestimate the importance of this error and presented rather as a mathematical hypothesis than as a real substance. After having compared this notion with Lavoisier's notes and other studies (Bensaude-Vincent and Musgrave) I incline to regard it as mistake, of which Lavoisier became aware later[304]; therefore he tried to correct his initial conception in 1789, as we can read in his *Elements of Chemistry*[305].

We can infer from the previous historical review that Lavoisier's success lies in both his oxygen theory, which explained calcination, combustion and metal acid reactions, and in the introduction of the new chemical nomenclature. But his principle of caloric as well as his acidity principle, though gave rise to further investigation to those directions, they were later falsified and abandoned by the scientific community.

[304] See Gillispie Charles, (1960), pp. 236-7, Beretta (1993), pp. 208-9, and compare with Bernadette Bensaude-Vincent (1983), pp. 60-78, and Musgrave A. E. (1976), p. 186.

[305] Lavoisier later in 1789 in his *Elements of Chemistry*, pp. 5-6, writes: *'We are not obliged to suppose this (caloric) to be real substance… we are still at liberty to investigate its effects in an abstract and mathematical manner'* (parenthesis mine).

4.2. Discussion of the different interpretations

After having presented the historical events, in this Chapter I can proceed now to discuss the different interpretations of the Chemical Revolution, given by different methodological views from both philosophers and historians of science, in comparison with Peirce's proposed methodology. However, there are many interpretations, among them some, who erroneously, in my view, identify Lavoisier with the forces of light and the phlogistonists with those of darkness, or those who try to show the victory of the French over the English and German system[306]. Therefore I will focus on the contribution of the scientific community as a whole to this progressive step of scientific inquiry, which, as I am going to show, behaved rationally and abandoned gradually the phlogiston theory in favor of Lavoisier's oxygen theory that was superior for many distinct reasons. Because the triumph of Lavoisier, as we have seen, would have been impossible without the discovery by the phlogistonists of the dephlogisticated and inflammable air, as well as of the water composition.

Furthermore, I am going to discuss the accounts given by the different methodologies with reference to some criteria, which in turn will enable me to draw my conclusions about Peirce's conception of the SCT. Such criteria will be the different aspects of the SCT (self-corrective sorts of inference, progress of knowledge, criteria of admissibility of the hypotheses, scientists' skill, fallibilism and continuity of knowledge in the scientific community). The discussion will include the following accounts given by: Positivist, postpositivist, conventionalist, falsificationist, Kuhn, Lakatos, constructivist or sociologist of science, in comparison with Peirce's scientific methodology and with reference to the above mentioned criteria. Its aim will be to test and evaluate the different aspects of Peirce's SCT, and if necessary to integrate some new views into the Peircean conception, which might secure more the mechanisms of the self-corrective character of scientific method.

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306 See Musgrave A. E., (1976), pp. 181-209, and compare with Miller David P., (2003), pp. 34-39, and McEvoy John G., (1997), pp. 1-33.

4.2.1. Positivist, postpositivist accounts

Traditional positivist historians as dominating from the mid-nineteenth to the mid-twentieth century viewed the Chemical Revolution as the progressive moment, when chemistry transitioned from a 'nonscientific', which is identified with phlogiston theory, to a 'scientific' discipline identified with Lavoisier's theory. According to the positivist view, scientific propositions are the ones that describe hard facts or inductive generalizations derived from them; therefore respectable theories are derived from and proved by experiments. From this point of view, only Lavoisier's theory was scientific, as it was proven truth that replaced premature and erroneous speculation[307]. The diversity of opinions regarding phlogiston indicated, according to Partington and McKie, '*the last attempts of an incorrect theory to accommodate growing experimental knowledge. Based on an unsound and insecure foundation, such a theory … at last becomes so top-heavy that it crashes by mere instability of bulk*' (Partington & McKie 1938, p. 361).

Indeed, phlogiston and oxygen theory both were progressive[308], since the exponents of phlogiston also discovered new substances, and they both appealed to experimental verification, as they continued their experiments to prove their theory. But the critique focused only on the monolithic demarcation between scientific and nonscientific disciplines, in my opinion, is misleading and it cannot include the different aspects of the Chemical Revolution. Because quantitative results of experiments of the phlogistonists (i.e. Cavendish) were superior to Lavoisier's and they were both 'scientific'. Cavendish and Priestley had improved chemical apparatus (eudiometer) for their experiments that allowed them to discover the new substances such as inflammable and dephlogisticated air. On the other hand, Lavoisier used also his new set of apparatus (hydrometer, thermometer and calorimeter[309]) for his experiments.

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[307] For more about positivist view, see Rodwell (1868), Herschel (1938), Partington (1937), White (1932), and McKie (1952). For critique of the positivist and postpositivist view, see Agassi (1963), Musgrave A. E., (1976), Bensaude-Vincent (1983), McEvoy (1997), and Kim (2003).

[308] The progressive aspect of both theories will be discussed in Lakatos' account.

[309] As Kim (2008) points out: '*Lavoisier targeted phlogiston because it hampered his effort to rationalize chemistry as a logical discursive system with a new set of 'metric' instruments (hydrometer, thermometer, gasometer, and calorimeter) that would allow him to represent chemical action in an algebraic form*'. However, as we have seen, phlogistonists developed also metric instruments (e.g. eudiometer).

Moreover, in this account is missing the relation between conceptual frameworks and experiments, since experimental and empirical evidence alone, without the corresponding theory that accommodates the experimental results, are insufficient to bring out scientific development. The emphasis, even by new historians such as Guerlac[310] (1961), Donovan (1993) and Kim (2003) and (2008), of the use of metric instruments only by Lavoisier is unsuccessful, since his principle of caloric was also imponderable and was cleverly ignored, just like with phlogiston; therefore, we do not find any great 'scientific' superiority of Lavoisier's program.

Certainly, after Kuhn this view was replaced by postpositivist accounts, since it had to include the *'paradigm change'*, Kuhn's model of '*Gestalt switch'* (Kuhn, 1970) and inversion of a new theoretical framework. Therefore postpositivist accounts depicted scientific change as involving 'cognitive inversion' and proceeding via 'crucial experiments' and moments of discovery[311]. Conant characterized the Chemical Revolution as the development of a 'superior conceptual scheme' based on oxygen, which replaced a '*broad conceptual scheme loosely knitted around the hypothetical substance of phlogiston*' which included '*multiple ontologies'*. In this theory-centered account, phlogiston was an *'erroneous' theory that hindered scientific progress*' (Conant 1967). Phlogiston and oxygen became competing theoretical framework that allowed chemists to make sense of an ensemble of empirical data (McCann 1978). According to Perrin (1986), while Lavoisier pursued a program of methodological reform, in which he used physical instruments and measurements to solve chemical problems, the revolution he envisioned in the fall of 1772 'was a conceptual and theoretical one' that involved the 'replacement of older, less adequate statements with newer, more adequate statements without any sudden Gestalt switch'.

As we have seen, phlogiston and oxygen became competing theoretical frameworks for explaining combustion, which both appealed to experimental verification, but until the overthrow of the phlogiston theory for both theories there were anomalies that could not be explained (e.g. increase of weight for phlogiston theory, combustion of 'inflammable air' for Lavoisier). Apart from that, it was not only 'phlogiston' hypothetical substance but also

[310] Guerlac (1961), xiv-xvii, although in his critical essay recognizes that the Chemical Revolution was the work of many hands of French and British origin, he adopts without any criticism Dumas' view, namely: *'the use of balance and other quantitative techniques became in Lavoisier's hand a veritable agent'*.

[311] See for postpositivist accounts: McEvoy (1997), Rachel Laudan (1993), Donovan (1988), Perrin (1986), McCann (1978) and Conant (1967).

Lavoisier's 'caloric', which was proved to be also false later. As for 'multiple ontologies', we find in both theories, as Lavoisier's first hypothesis identified oxygen with carbon dioxide, later with pure air and then a combination with caloric, whereas phlogistonists identified it with dephlogisticated air or water minus phlogiston. Concerning Perrin's notion and the contribution of phlogiston and oxygen theory to conceptual changes, formulation of theory and discovery, it will be discussed in detail in Kuhn's account. Finally, different criteria of the theories' appraisal and contribution of the scientists' skill we find in both cases, but it consisted of different aspects[312] (abductive inference, experimental skill, theoretical conception, criteria of appraisal etc.), since Lavoisier's skill was more theoretical while Priestley's was experimental one, as I am going to show after this discussion.

4.2.2. Conventionalist account

Traditional *conventionalism* or *simplicism* holds that theoretical progress of science is only convenience (simplicity) and not in truth-content; therefore discoveries are inventions of simpler *pigeonhole system*[313]. From this point of view, Gregory's (1934) account holds that Lavoisier's oxygen theory superseded phlogiston theory because it was the simpler conceptual framework that explained experimental results. By appealing to Occam's razor and supported by inductivist account he tried to establish this view and concluded: '*The greater simplicity of Lavoisier's system and its greater coherence, undoubtedly prevailed over the complexities of phlogistic chemistry*'[314]. Pyle (2000) in his rational account of the Chemical Revolution argues also that Lavoisier's theory represents a given reaction of calcination involving three terms, while phlogistonists represent the same reaction with four terms involved[315]. To support further this notion conventionalists appeal to the historical

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[312] As Gillispie Charles in (1960), p. 218, argues: '*Chemistry profited, therefore, from the curious, the almost symbiotic relationship between Priestley and Lavoisier, however unwelcome to both. If Priestley's lack of theoretical taste disqualified him from understanding his discoveries, Lavoisier's lucidity disqualified him from making them*'. Beretta (1993), pp. 174-5, holds the same view by citing Lavoisier's statement about Priestley's work from the translation of his work (1776): '*This work may be regarded as the most elaborate and most interesting, which appeared since Dr. Hales work … Dr. Priestley's work being, as it were a train of experiments, not much interrupting by any reasoning, an assemblage of facts, mostly new…*'. See also Schofield (1962), p. 51.

[313] For more about conventionalism, see Duhem (1954).

[314] See Gregory (1934), p. 211.

[315] Pyle in (2000), p. 105, represents the two reactions as follows: $AC + B \rightarrow AB + C$, for Lavoisier, while for the phlogistic chemists: $AC + BF \rightarrow AB + CF$.

data, namely the admittance of the phlogistonists Priestley and Kirwan that Lavoisier's theory was simpler[316] than their own.

The question that may arise here is whether 'simplicity' concerns fewer elements involved in Lavoisier's theory or simpler schemes or even both. Lavoisier's 1784 oxygen theory was not simpler than 1784 phlogiston theory, as we can infer from the comparison between reactions (3) and (4). But if we consider that Lavoisier regarded nowadays oxygen as a combination of an oxygen base and 'caloric', and the latter was imponderable just like phlogiston, then we can infer that Lavoisier's theory did not involve fewer elements. Concerning the different schemes that described the air discovered by Priestley (dephlogisticated air) and Lavoisier (*oxigene*), they were not different, as both descriptions of the properties of the gas were similar[317], while the schemes in metal-acid reaction (reactions 3 and 4) both were not simple.

Apart from that, simplicity is only one criterion of appraisal of the hypotheses, which, for Lavoisier, was not more crucial than the 'more probable hypothesis that explains the facts and conforms to experience', as he admitted[318]. I am going to show later that Lavoisier used different criteria for the appraisal of his theory in different moments, which may give evidence that the criteria are not connected by algorithmic way with appropriate weights, but, rather, they leave room for judgment and as a result for skill in abduction. As for the acceptance of Lavoisier's theory by the scientific community, it began after the decomposition of the water in 1784, which implied the falsity of the phlogiston theory, and it was completed finally after the discovery of carbon monoxide[319] and the electrolysis of water, both in 1800, but in any case, it was not accepted because of its simplicity.

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[316] See Kirwan (1789), pp. 7-8, and Priestley (1779-86), p. 419.

[317] The description was as follows: That which not only supports combustion but also burns with radiance; that which is purer than the air that we live in, being eminently respirable; that which did not dissolve in water like fixed air; that which could be used again for the calcination of metals; did not suffocate animals, rather enabled animals to live longer unlike in the fixed air etc.

[318] Knickerbocker (1962), p. 134, cites Lavoisier's following statement: *'In attacking here the doctrine of Stahl, it was not my purpose to substitute for it a rigorously demonstrated theory, but only a hypothesis, which seemed to me more probable, more in conformity with the laws of nature, and one which appeared to involve less forced explanations and fewer contradictions' (Underline mine).*

[319] Pyle in (2000), p. 115, by citing Kirwan's experimental notes holds that Lavoisier's theory could not explain the production of another 'inflammable air' produced by Kirwan after heating dry charcoal with water, according to the following reaction: $C + H_2O \rightarrow CO + H_2$. Because carbon monoxide was also inflammable, therefore this anomaly was cleared up in 1800, after the discovery of carbon monoxide in a reaction without water.

4.2.3. Falsificationist account

Falsificationist accounts hold that phlogiston theory was refuted by 'crucial experiments', which might be partly true, but historians disagree about the time of the crucial experiments, since some hold that were in 1772, some in 1775, or in 1784 or even in 1800. Two of Lavoisier's crucial experiments have been selected by falsificationists, which decisively refuted the phlogiston theory, the one of 1772 that showed weight increase in combustion[320], and the other of 1775 with mercury calx, where air was given off, and which was called the 'discovery of oxygen'[321]. Furthermore, falsificationists hold that phlogistonists ignored well established facts, and conclude that this would be characteristic of a non scientific theory nowadays[322]. On the other hand, as Agassi (1963) argues, Priestley's discovery of dephlogisticated air was successful and 'planned' refutation[323] (not chance discoveries) of the phlogiston theory. Finally, following Popper's notion of theories' corroboration[324] one could claim that Lavoisier's theory has survived many attempts of falsification (severe tests); therefore it was more rational to be adopted by the scientific community of that time instead of the phlogiston theory.

To begin with the first experiment (1772), in my opinion, the adherents of the phlogiston theory did not ignore the facts of increasing in weight; on the contrary they explained them by adopting Boyle's or the Aristotelian view or Cavendish's and Kirwan's new versions. Certainly, the term of 'crucial experiments' followed many uses and definitions[325], but, by

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320 See Hartley (1971), p. 7: *'Phlogiston theory survived almost a century, until Lavoisier exposed its fallacies by his study of the changes in weight during combustion'.*

321 Conant in (1950), p. 48, claims that Priestley, although effectively discovered oxygen, continued to adhere blindly to phlogiston theory.

322 See Read (1957), p. 121: *'From the very outset the (phlogiston) theory ignored two well established facts… A scientific theory ignoring such patent facts would not hold credence for a moment at the present day'.*

323 Agassi in (1963), pp. 64-74, concludes that five experiments in the history of science were successful refutations of theories, which were not chance discoveries, i.e. Galvani, Oersted, Priestley, Roentgen and Hertz's discoveries.

324 For more about corroboration, see Popper K. (1992), pp. 264-73.

325 The idea of crucial experiment appeared at the seventeenth century in Francis Bacon's *Novum Organum*, as *instantia crucis*, as crucial instance or in the English version 'instance of the fingerpost'. Galileo also used this method referring to the tides that are able to decide between the two world systems, the Ptolemaic and the Copernican. The words "experimentum crucis" was written down first by Descartes, then Boyle and Newton. But Duhem challenged this view, since, for him, the experimental observation is theory-laden, because of the network of hypotheses necessary to the use of a measuring instrument. From this point of view, the conclusive role of experiment is excluded. Therefore later Popper wanted to rehabilitate crucial experiments, only in case of falsifying ones. See more Márta Fehér (1985).

taking into account Popper's definition[326], we may say that crucial experiments should be the ones that involve refutation of a certain theory. From this point of view, the first 'crucial experiment' was the one presented in April 1774, conducted by Lavoisier, which *refuted* Boyle's theory that gain in weight was produced by material particles of fire. Concerning the experiment of mercury calx (1774), Priestley was the first one who performed it, but he continued to support the phlogiston theory, because afterwards followed its another version from Cavendish in 1784, which could explain all these facts. However, the same experiment, which Lavoisier also conducted in 1775, was crucial for Lavoisier's theory, since it *refuted* his initial hypothesis of November 1772 and led him to correct it, i.e. the air given off in the reduction of lead with charcoal was not a simple substance, but it was 'fixed air' (CO_2, as explained with the chemical representation of the calx of lead).

Another crucial experiment, in my view, was the one of decomposition of air in 1777, which established that the air of the atmosphere, was not a simple substance but a compound, composed of 'pure air', which Priestley also called *'dephlogisticated air'*, and of *mofette* or noxious air. Because it *falsified* Priestley's theory that air was composed of nitrous air, earth and phlogiston and as a matter of course the Aristotelian view of composition of air. The last crucial experiment, in my opinion, was the one that falsified Cavendish's last version of the phlogiston theory indirectly, i.e. the decomposition of water in 1784, since it *refuted* the reaction (1) presented by Cavendish, as explained. And the same experiment conducted by Carlisle and Nicholson in 1800, as we have seen, *falsified* directly Cavendish's theory. Concerning Kirwan's remaining version of the phlogiston theory, it was abandoned by Kirwan himself after 1791, as we have read in his letter to Berthollet, and by the scientific community after 1800. Because, as Pyle[327] (2000) argues, the last revised phlogiston theory developed by Kirwan was actually refuted in 1800 after the discovery of carbon monoxide. We can infer, then, that by 1800 all the versions of the phlogiston theory have been falsified by crucial experiments, i.e. Boyle's view in 1774, Aristotelian and Priestley's view in 1777, Cavendish's theory in 1784 and 1800, and Kirwan's theory in 1800. On the other hand, the first false version of Lavoisier's theory was falsified by the same experiment conducted in 1774 by Priestley and in 1775 by Lavoisier himself.

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[326] See Popper (1992), *The Logic of Scientific Discovery*, p. 277: '*It should be noted that I mean by crucial experiment one that is designed to refute a theory (if possible) and more especially one which is designed to bring about a decision between two competing theories by refuting (at least) one of them - without, of course, proving the other'*.

[327] See Pyle (2000), pp. 114-5.

As for Agassi's claim, I am going to show in the next Chapter that Priestley with this experiment actually tried to refute Lavoisier's theory, when he drew the experimental consequences of Lavoisier's hypothesis, i.e. calxes were metals combined with air. On the other hand, Lavoisier adopted this hypothesis as 'most readily falsified or verified', because presence of air could be verified, while of phlogiston not. This criterion of appraisal, as I said, is distinguished from Popper's falsificationism, which says that science deals with more and more falsifiable hypotheses, because the 'most readily falsified or verified' hypothesis helps the inquirer to examine its predictions quickly and easily, as Peirce states (CP. 1.120).

Certainly, if one applies Popper's notion of corroboration, he might argue that after 1775 Lavoisier's theory survived many severe tests of rationality and experiments, while all the versions of the phlogiston theory have been falsified by crucial experiments. Furthermore, as Musgrave[328] (1976) argues, chemists continued rationally to adhere to phlogiston theory long after 1775, Indeed, Lavoisier's theory began to be accepted by the scientific community after his experiments of the decomposition of the water that showed the falsity of the phlogiston theory. But one has to distinguish between the success of Lavoisier's theory of combustion, due to its experimental support, and the refutation of the phlogiston theory by crucial experiments; because the latter alone does not guarantee the truth of the first. It could simply lead to another revised version of the phlogiston theory, just like with the first versions. Lavoisier's success does not lie in the falsification of the rival theory, since the falsification of one theory does not imply that the rival theory will be accepted[329], if the latter does not explain the experiments (decomposition of air and water) that have falsified the first one.

Lavoisier's theory had the following features: (a) it could explain, combustion, calcination, decomposition of air and water and solution of a metal in an acid, (b) it had no anomalies concerning oxidation (the last anomaly of Lavoisier's theory was cleared up after the

[328] See more Musgrave A. E. (1976), pp. 182-85.

[329] William James in (1956), *The Will to Believe*, VII, pp. 17-18, also argues that if we escape the falsehood of a theory B, it does not necessarily follows that we come to a true theory A, as follows: '*Although it may indeed happen that when we believe the truth A, we escape as an incidental consequence from believing the falsehood B, it hardly ever happens that by merely disbelieving B we necessarily believe A. We may in escaping B fall into believing other falsehoods, C or D, just as bad as B; or we may escape B by not believing anything at all, not even A'.*

discovery of carbon monoxide in 1800), i.e. it was consistent theory, (c) it had the balance of evidence in its favor, (d) it extended its application to acidity and pneumatic chemistry, and (e) it was based upon a referring entity (oxygen), while the rival theory was based upon the entity 'phlogiston' that did not refer to any existing kind. Therefore afterwards his theory was *rationally* accepted by the scientific community as I am going to show. Indeed, Popper does not specify all the above mentioned criteria as criteria of severe tests for corroboration - while Lakatos later specified some of them - but all these criteria imply severe tests of rationality and experiments, therefore I suggest that Popper's conception of corroboration in a broad sense could be applied to the Chemical Revolution.

4.2.4. Kuhn's account

Thomas Kuhn, with his most famous book *'The Structure of Scientific Revolutions'* (1962) about science written during the twentieth century, shattered traditional myths about science and showed that scientific behavior has little to do with philosophical theories of knowledge and rationality. He argued that observational data and logic alone cannot force scientists to move from one paradigm to another, because different paradigms include different rules, methods of assessing data and theories, methods of application and traditions of scientific research. For Kuhn there are two distinct kinds of scientific change, i.e. within the paradigm (normal science) and revolutionary changes. Within normal science there are clear standards of justification, rationality and progress, while within revolutionary science there are not. Changes within normal science are orderly and responsive to evidence, but without debates on fundamentals, while revolutionary changes are non-cumulative, abrupt, and radical and involve challenge on fundamentals. Finally, he argues that revolutionary changes require always a crisis (accumulation of anomalies that the existing paradigm cannot solve) and appearance of a new paradigm and that science is better served by the scientific revolutions.

As I said in the first part of the study, Kuhn's model of revolutionary change is similar to Peirce's 'cataclysmal' model of evolution. On the other hand, one of the main of Kuhn's examples of scientific revolutions is the Chemical Revolution. Therefore I am going to examine Kuhn's account in detail, so as to discuss his crucial arguments. From this account I have chosen certain significant aspects of scientific revolutions, because Kuhn relates them directly with the Chemical Revolution in order to support his arguments. These aspects are, as follows: inseparability between observation and conceptualization, incommensurability, anti-rationalistic appraisal of scientific theories, paradigm shifts, discontinuity, translation failure of ‘phlogiston’ into the language of the new chemistry and debate on fundamentals in scientific revolutions.

To begin with the first, Kuhn raised questions, such as: ‘*Was it Priestley or Lavoisier, if either, who first discovered oxygen?’ When was oxygen discovered*[330]?’ And he concluded that is difficult to provide an answer to these questions, since ‘observation and conceptualization are inseparably linked in discovery’. In my opinion, Kuhn confuses facts, conceptual schemes and theories, therefore he provides the misleading thesis that phlogiston chemistry and oxygen chemistry constitute rival incommensurable paradigms; because one thing is to discover a gas, and quite another thing to embed the discovery in a theory. The descriptions used for individuating the gas in reduction of mercury calx discovered by Priestley and Lavoisier, as we have seen, were similar[331]. These descriptions, based on observed features of the gases, constitute the conceptual schemes, but they are distinguished from the corresponding theories, which are based on the supposed involvement of the gases, their classification and their casual relations that explain the conceptual schemes. The crucial difference between phlogistonists and Lavoisier lies in those theories that interpreted differently those same schemes, which up to a certain time point worked sufficiently for both. While after the discovery of the decomposition of water the new conceptual schemes could not be explained by the phlogiston theory. This also shows that experimental and empirical evidence alone, without the corresponding theory that accommodates the experimental results, are insufficient to bring out scientific development.

With regard to incommensurability, Kuhn’s notion includes ‘different words’ that partisans of the rival theories inhabit, mutual incommensurability and incommensurability of problems and values[332]. As for the first, Kuhn claims that Priestley and Lavoisier saw

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[330] See Thomas Kuhn (1970), pp. 54-5.

[331] The description was as follows: that which supports not only combustion but also burns with radiance; that which is purer than the air that we live in, being eminently respirable; that which did not dissolve in water like fixed air; that which could be used again for the calcination of metals; did not suffocate animals, rather enabled animals to live longer unlike in the fixed air etc.

[332] See Thomas Kuhn (1970), pp. 92-110.

different things[333], when they collected the air given off in the reduction of *mercurius calcinatus per se,* therefore he concludes that *'after discovering oxygen Lavoisier worked in a different world'.* In this interpretation, in my opinion, there is no clear distinction between observation, description and theory, because, as we have seen, they both saw the same thing, since the description of what they saw (conceptual scheme), was that the metallic mercury was restored and a colorless air was evolved, which, for both, had the same properties. The 'different things' consisted of: first, that Priestley named the air given off *dephlogisticated air,* while Lavoisier named it *oxigene,* which for both had the same properties. Second, they both accommodated the 'same things' that they saw to different theories of calcination and combustion.

As for the 'different world', following Kuhn's argument one might also claim that 'Cavendish after discovering inflammable air worked in a different world' or even 'after each discovery the inquirer lives in a different world'. Indeed, every inquiring subject lives 'internally' in a different world that consists in his set of beliefs, which represent the real actual world, and after the surprising event (discovery) that falsifies the predictions of his belief-habits, as Peirce says, begins his inquiry that ends with fixing new beliefs, more stable and cohered with experience (CP 2.173). But this is not a characteristic of scientific revolutions but of every scientific inquiry in 'normal science'. Furthermore, the new established set of beliefs is also another different 'world-view' or else new approximate representation of reality; therefore Peirce emphasizes the need of the indefinite inquiry by the scientific community that will leave the community with the common collection of beliefs, which would be perfectly stable view and represent approximately the real world. In that latter case the interpreted in relation to its subject world (internally by the scientific community) and the independent 'external real world' would be approximately similar. Taking into consideration these factors we can conclude that though Kuhn raised crucial epistemological questions concerning the affection of perception by beliefs and expectations, like Peirce before him did[334], he finally failed to refute the argument that he began to, namely '*Priestley and Lavoisier both saw oxygen, but they interpreted it*

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[333] Ibid (1970), p. 118: *'Lavoisier, we said, saw oxygen, where Priestley had seen dephlogisticated air and where others had seen nothing at all'.*

[334] See CP 5.416. 1905, where Peirce criticizes the notion of clear distinction between perception and beliefs, as follows: '*… forgetting that our very percepts are the results of cognitive elaboration'.*

differently'[335], as the difference between Priestley and Lavoisier lies only in their interpretation of the same observations.

Referring to mutual incommensurability, it involves inability of communication between rival paradigms as they use different standards of evidence and argument. In my view, we can hardly find any difficulty in understanding between the adherents of the two rival theories. Neither did have Lavoisier any problem of comprehending terms such as 'phlogisticated' or 'dephlogisticated air'[336], for he personally used them, nor did his opponents regarding his new terminology[337], as they admitted[338]. Moreover, the experimental apparatus used by Lavoisier and the phlogistonists were similar and, as we have seen, both sides repeated similar experiments. Mutual incommensurability between two scientific theories of e.g. the 18th and 20th century is indeed a real fact, as explained, since the scientific problems of the 20th century theory could not have been even formulated within the cognitive framework of the 18th century existing state of knowledge. But in that revolutionary episode of the Chemical Revolution there were not any mutual incommensurability present, unless we consider phlogiston theory pre-paradigm science.

However, especially Kuhn is the one who regards this episode in the history of science as a representative example of scientific revolution. The 'different worlds' consisted only in the different and competing theories that their adherents interpreted the various terms referred to the same schemes. Lavoisier's terminology was committed to his theory of calcination and combustion, whereas phlogistonists' terminology was embedded in their theory of the same phenomena. Although there was a mutual understanding of terminology, the difference of the rival theories laid in their explanation of the supposed involvement of the gases, their classification and their casual relations.

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335 Thomas Kuhn (1970), on p. 120, begins his critique against this view, as follows: *'Many readers will surely want to say that what changes with a paradigm is only is only the scientist's interpretation of observations … On this view, Priestley and Lavoisier both saw oxygen, but they interpreted their observations differently'.*

336 See Lavoisier *Oeuvres* II, pp. 623-655.

337 See Kirwan (1789), pp. 83-84, 126-7, and Priestley (1966), pp. 292-94.

338 Pyle in (2000), p. 104, cites as evidence for this admittance the writings of Lavoisier and the phlogistonists, and Thagard in (1992), pp. 113-17, discusses the same problem and arrives at the same conclusion.

According to the incommensurability of problems and values, the adherents of the two competing theories should have assigned different weights to the solved problems and the anomalies with regard to each theory. But as we have seen, both Lavoisier and the phlogistonists although began with ignoring several anomalies, they were later forced to attribute more weight to the anomalies that their theory could not explain. Lavoisier was obliged to assign more weight to explaining the combustion of 'inflammable air' (hydrogen), which was an anomaly for his theory, as well as to explaining the solution of a metal in an acid with the formation of a salt and inflammable air. The same is true with the phlogistonists, who were forced to assign more weight to the anomaly of increase in weight on calcination and to the formation of water that led them to revising their theory many times. That force was not only exercised by the adherents of the rival theories, but also by the scientific community of that time.

In that case, both rival paradigms did not use different standards of evidence and argument, but they rather used the same method consisted in explaining theories that rendered the observed facts and in experimental testing of the theories' consequences or predictions. The different standards concerned only the different criteria of appraisal of their theories (like Peirce's criteria of admissibility of hypotheses), as I am going to illustrate, which will show that scientists' skill in abduction is subjective and leave room for judgment, because Lavoisier took into account different criteria for the evaluation of his theory than the phlogistonists did. But this difference of appraisal can be found everywhere in the history of science, not only in scientific revolutions but also in 'normal science', since scientists are always trying different criteria of evaluation of their theories at different times. Neither implies it irrationality in selecting a theory, but rather skilful judgment (rationality) in the abductive phase of inquiry, where the first hypotheses are formed.

One might argue, following Kuhn's notion, that the assignment of different weights to the various problems by the scientific community was irrational and socially relative. Rationality on the contrary would imply that a theory could explain without any anomaly the observed phenomena and its predictions could be experimentally verified. But if we consider the fact that by 1800, a year which was associated with the empirical discoveries of carbon monoxide and electrolysis of water, all the defenses of the phlogiston theory have been swept away, then we can infer that the assignment of different weights to the various problems by the scientific community in the Chemical Revolution was rational. Because after that time there were no anomalies for Lavoisier's theory, and besides it had the balance of evidence in its favor, as it could explain combustion, calcination, formation of water and solution of a metal in an acid.

With regard to Kuhn's paradigm-shift in scientific revolutions, it involves radical changes in classification, part-whole views, part-relations and similarity relations of concepts, which lead to conceptual re-organization and to the establishing of a new conceptual framework. Kuhn likens it to gestalt switches of the sort that occur in perceptual phenomena like the Necker cube, in which either face can be seen as the front, therefore occur all at once[339]. Indeed, we have to admit that after each discovery occur changes in part-relations of concepts that can lead to radical conceptual change. To be more precise, according to the phlogistonists, metals consisted of calxes and phlogiston, while for Lavoisier calxes consisted of oxygen and metals. The change was radical, because it went from a calx being a part of a metal to a metal being a part of a calx[340]. Another re-organization of the concepts consisted in the classification of the concepts 'atmospheric air' and 'water', as it was changed from simple substances into compounds. Pyle (2000) holds that in case of theory-driven classifications occurs re-organization, but in bottom-up the transformation is invisible[341]. In my view, although oxygen was formed by bottom-up method (experimental observations), it led actually to radical re-classification of the known chemical substances. However, the crucial question that arises from Kuhn's notion is whether this radical conceptual change occurred at once or gradually and with links to other 'intermediate concepts'.

First, as we have seen, Lavoisier did not make a shift over to the new conceptual framework before May 1777, but he followed three important steps of 1772, 1774 and 1777 by correcting gradually his initial hypothesis until he formulated his final view. He began with his first hypothesis that 'in calcinations and combustions might be absorbed air', then he modified this hypothesis, namely 'this air was pure air'[342], and he finally discovered that 'this air (oxygen) was the purest part of the atmospheric air' and the latter was a mixture of

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[339] Thomas Kuhn (1970), pp. 111-14, gives a psychological account of the gestalt switches and he concludes (p. 150): '*the transition between competing paradigms cannot be made a step at a time, forced by logic and neutral experience. Like the gestalt switch it must occur all at once (though not necessarily in an instant) or not at all'*.

[340] The phlogistonists held that combustion was represented as follows: Metal → Calx + Phlogiston, while Lavoisier held that the same reaction was: Metal + Oxygen → Calx.

[341] See Pyle (2000), p. 117.

[342] We can disregard here his initial view 'pure elemental air', which he corrected, as explained, to 'pure air'

at least two airs. The intermediate concepts of the new entity 'oxygen' were, first 'air', then 'pure air' and finally 'purest part of air'. This means that the unknown entity was first classified to the known class 'air', then to the sub-class of air i.e. 'pure air', and finally to its sub-class 'purest part of air'. The latter was the new discovered entity 'oxygen' that had certain 'ontological dispositions'[343] or else properties.

Second, the new concept 'oxygen' was formed gradually and rather by practice-driven and bottom-up method – which means, induction or abduction – than by theory-driven and top-down (deduction), since the inference that was applied was a synthesis of Lavoisier and Priestley's experimental observations (ampliative inference), and it aimed at classifying the unknown event to a known class of events. Taking into account all these factors we can explain Lavoisier's reasoning in this phase, rather, by abduction than by induction, because in abduction we synthesize to classify the case under a class of events, and apart from that, abduction is the inference that can deal with unknown or unobservable entities. I shall return to this topic in the next Chapter to explain in detail how Peirce's schema of abduction can be applied to Lavoisier's reasoning and explain the discovery. Here I can simply note that if we compare this process with Peirce's notion of 'modifying the hypotheses by correcting the premises of abductive inference', we can realize, how Lavoisier corrected gradually his initial hypothesis until he settled on the final one that led him to his discovery, and why 'all new ideas in science come by abduction' (CP 5.145. 1903).

To sum up with Kuhn's paradigm-shift, the radical conceptual change occurred actually in the Chemical Revolution, but not at once, since the process followed was gradual and with links to other intermediate known concepts (air, pure air, purest part of air), as a result of recurring classifications. Moreover, the process of discovery of a new entity can only be explained by abduction, since it is by this inference that the unknown event can be classified gradually and repeatedly to different known classes of events that have similar properties, until to be identified.

Related to paradigm-shift is Kuhn's notion of discontinuity that occurs in scientific revolutions, which excludes accumulation of knowledge, can happen all at once and when

[343] As explained before, its properties were the description of the gas discovered by Priestley and Lavoisier.

the transition is complete leads to radical change of views, methods and goals[344]. To establish this account Kuhn draws an analogy between political and scientific revolutions by appealing to historical data and by taking into account two aspects of similarity[345], i.e. inability of problem solving activity by the existing institutions and the division of community into competing parties. To begin with the latter, in my opinion, the analogy is unsuccessful[346], since it misses an important aspect of scientific inquiry, namely science is not only a problem solving activity, but it is also concerned with explaining and understanding the regularities in nature[347]. On the other hand, as I have shown, these regularities exist independent of human opinions about them, whereas political status quo as object of revolution is 'human made'.

Second, before exploring this view, we have to consider the terms *discontinuity, accumulation, gradual, radical* change, as well their interrelation. *Radical* change is abrupt and proceeds by leaps, but it does not necessarily entail *discontinuity* or even denial of *accumulation*. As in Peirce's evolutionary model of scientific knowledge the real progress in science involves the 'cataclysmal' mode, which proceeds by leaps in the abductive phase. But those leaps in the abductive phase of inquiry, which although are radical and may involve some 'novel way of reasoning about the observations, which would previously have been passed unperceived', as Peirce says (CP 1.109) and Kuhn (1970, p.85) also admits it, do not imply *discontinuity*. The background knowledge of the inquirer, who accomplishes that leap, and it is presupposed for that process, is gained through a *cumulative* process within the scientific community, since each of its members is informed about the work of the other member, uses his experience, learns from his failures and as result the whole community proceeds gradually to the growth of knowledge, which in turn all the members can share. As we have seen, Lavoisier, until he settled on his final hypothesis that led him to his discovery, took into account not only the results of Cavendish and Priestley's experiments, but also Boyle, Mayow, Hales and Black's notions and experiments[348].

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344 See Thomas Kuhn (1970), pp. 84-91.

345 See Ibid pp. 92-94.

346 For more differences between political and scientific revolutions, see also Cohen I. B. (1985), pp. 7-14.

347 The purpose of theoretical science is, for Peirce, both to predict in theoretical and diagrammatic context and to explain why its predictions are fulfilled. Toulmin also in (1961), pp. 99-115, argues that 'prediction' entails the idea of 'explanation'; therefore science is concerned more with understanding nature.

348 Here I can agree partly with Perrin's notion (1986) that the Chemical Revolution involved the 'replacement of older, less adequate statements with newer, more adequate statements without any sudden Gestalt switch', however, in my opinion, it led to radical conceptual changes.

Furthermore, the final abductive inference that is drawn and leads to the discovery arises from previous *gradually* corrective and successive steps from worse to better abductively drawn hypotheses. Lavoisier began with his first vague hypothesis in 1772, which he corrected *gradually* after taking into consideration his experimental results and of the phlogistonists in 1774. After the experimental decomposition of the atmospheric air in 1777 he settled on his final hypothesis about oxygen, while this final hypothesis was verified experimentally after the discovery of the water's composition by the phlogistonists in 1781, its experimental analysis by him in 1784 and the discovery of electrolysis in 1800.

On the other hand, if radical change supposed to have occurred at once, then it would be reasonable to expect from the historians to specify the certain time that occurred. However, neither Kuhn nor any from the historians has specified that time, since they disagree about both the time of the crucial experiments that led to falsification of the phlogiston theory, and the time of the radical change or paradigm-shift.

Another crucial aspect of Kuhn's account as a consequence of mutual incommensurability concerns translation failure[349] of 'phlogiston' into the language of the new chemistry, since the term 'phlogiston' as an imaginative entity does not refer to any naturally existing chemical substance. The critique against Kuhn's mutual incommensurability held that if we consider two theories incommensurable, we cannot regard them as really competing, since there is no basis for comparison; therefore Kuhn returned to clarify his notion by arguing that 'there is no language into which the two theories could be translated without residue or loss'[350].

To begin with the latter, first, I would agree with Kuhn, because perfect translation (without residue) between the two conceptual frameworks of phlogiston and oxygen theory is not possible, not only due to their crucial differences in kind and part-hierarchies, but also due to the terms, which are non-referring (phlogiston). But natural language translation encounters the same problems, since there are also differences in kind and part-hierarchies between languages and some of them imply even different 'ontologies'. For instance, a 'chicken' in Hungarian and in modern Hebrew is not a kind of 'bird' but a kind of

[349] See Thomas Kuhn (1983), pp. 669-688.
[350] See Ibid (1983), p. 670.

'animal'[351]; however translations overcome these problems, since terms refer to existing objects or to more general common kinds e.g. a chicken is a kind of animal, since birds are still animals. Considering this, then, the problem of translation between phlogiston and oxygen theory should lie only in the fact that 'phlogiston' is non-referring term, while 'oxygen' is. As a consequence, the question that may arise is whether this 'perfect' translation is necessary for the mutual understanding between two theories.

As we know today, the term 'phlogiston' neither refers to any naturally existing kind nor to 'oxygen', whereas the term 'dephlogisticated air' refers to actual kind, i.e. 'oxygen'. Kitcher[352] argues that Priestley referred to oxygen, when he breathed 'dephlogisticated air', and Cavendish referred also to oxygen, when he produced by heating calx of mercury. From a descriptive point of view this is correct, since both competing theories described the same evidence, namely the air liberated when heating calx of mercury, by attaching to it only different names. In this sense, neither was there any need for translation nor mutual difficulty in understanding the language of the rival theory, as oxygen and phlogiston theory were referring to the same gas with the same properties by attaching to it the terms 'oxygen' and 'dephlogisticated air' respectively.

Furthermore, despite the differences of the two theoretical frameworks that explained the supposed involvement of the gases and their casual relations in calcination and combustion, in my opinion, there was not any difficulty in mutual understanding either. The greater problem would be rather for Lavoisier than the phlogistonists to understand the language of the rival theory, as it used a non-referring term (phlogiston). But, for Lavoisier, this language was a 'mother language', since he grew up with this old theory, therefore he did not need any 'perfect' translation to understand it. Pyle argues that 'translation failure is not incompatible with mutual understanding and rational appraisal'[353], while Thagard holds that 'complete translation is not necessary for judgments of explanatory coherence'. For example, Kirwan and Black were able to overcome this translation failure and went over to the oxygen side, although it took them some years to understand and appraise the new theory[354].

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351 See Thagard (1992), p. 115.
352 See Kitcher (1993), pp. 100-101.
353 See Pyle (2000), p. 117.
354 See Thagard (1992), pp. 116-117, Beretta (1993), pp. 292-3.

However, in my view, it is still the case that a term does not refer at all e.g. the term 'phlogiston' did not refer to any actual kind, and apart from that, Lavoisier's 'caloric' did not refer to any existing kind either; therefore they cannot be translated. On the other hand, Lavoisier's theory of oxidation in comparison to the contemporary theory does not refer so accurately, as it is quite different, and the term 'oxygen' as principle of acidity is also false. But though we cannot translate all the above mentioned terms perfectly, we do not have any problem in understanding them. As we can realize, terms like 'phlogiston', 'calcination' or 'oxygen' began as imaginary theoretical entities or with an introductory description and they were later replaced or extended due to further investigation by the scientific community of that time. Following Peirce's account we can infer that since universal dispositions (*Thirdness*) are inexhaustible, therefore their interpretation is an approximate one, and they grow in accuracy and complexification, as long as further inquiry is carried out.

To sum up, both theories adopted terms that did not refer, therefore they cannot be translated into the language of the new chemistry. But this shows that the new theory after the paradigm-shift can have the same problem in relation to referring terms, just like the previous paradigm that it has replaced. Hence, a question arises, is that a characteristic of scientific revolution or a feature of every scientific inquiry? In my view, scientific inquiry while attempting to describe some unknown entities on the basis of incomplete or indirect information may be sometimes misled or even creates imaginary theoretical entities (Peircean symbols), and some of them do not refer to any actual kind or substance. But in the course of inquiry, as long as new evidential data are gathered, the description of those entities will be corrected and extended or if they are imaginary and do not refer, they will be replaced by referring terms. Lavoisier's 'oxygen', though it was a referring term in relation to actual substance, was rejected as principle of acidity by Humphrey David in 1815, when he showed that muriatic acid (hydrochloric acid) consists only of hydrogen and chlorine (not oxygen). As a result, Lavoisier's theory of acids followed afterwards many corrective steps. Arrhenius in 1887 defined acids as substances that produce hydrogen ions, when they are dissociated, Brønsted and Lowry in 1920 defined them as substances that donate protons, whereas Lewis as the ones that can accept electron pair etc[355].

[355] See Thagard (1992) and Partington (1937).

In all the above mentioned cases, although non-referring terms were replaced, while referring terms were corrected and extended, did not occur any scientific revolution, but the whole process was a continuous scientific inquiry carried out by the scientific community. Ladyman (2009) in his attempt to justify his structural realism holds that phlogiston theory is relational structure expressed by the theory of Redox reactions[356]. However, in my opinion, terms, which do not refer at all or refer partially, have been always adopted by scientists in their attempt to describe unknown entities at the beginning of their inquiry, because of incomplete or indirect information. But in the long run, as long as new evidential data are gathered, if these entities are imaginary and do not refer, they will be replaced by referring terms.

As Peirce also says, after some recurring and corrective cycles of abductions, deductions and inductive testing scientific inquiry carried out by the scientific community could approach more accurate description of the 'real', gradually and through entities or laws (*Thirdness*), which refer to the actual world and represent approximately the regularities in nature. Taking into account this aspect, we cannot regard the problem of non-referring terms as a cause of mutual incommensurability, as the adoption of ideal or imaginary entities and constants is a normal scientific practice, since allows scientists to operate on the outside world or to be affected by it[357].

To return now to the notion of 'pessimistic meta-induction' mentioned in the first part of the study, as Laudan suggests, there are number of past theories whose theoretical terms do not refer, among them phlogiston, therefore changes of theory are not progressive approximation to reality[358]. But as we may infer from the previous discussion, referring or non-referring term is not a criterion of continuity, because imaginative terms have been adopted always by scientists, as initial and vague hypotheses, which in the continuous course of inquiry and in the long run of application have been corrected or replaced. Apart from that, no one can imagine a formulation of a new hypothesis without the use of terms from old theories. As we have seen, terms such as 'weight', 'combined', 'air' etc., which

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[356] Ladyman in (2009) concludes: '*phlogiston theory identified a number of real patterns in nature and that it correctly described aspects of the causal/nomological structure of the world as expressed in the unification of reactions into phlogistication and dephlogistication'*.

[357] Bernadette Bensaude-Vincent in (2008) shows also this pragmatic and operational use of entities in the history of chemistry.

[358] Laudan in (1981a), pp. 33-4, in order to establish his argument quotes his list of the past theories and concludes: *'This list could be extended ad nauseam, involves in every case a theory, which was once successful and well confirmed, but which contained central terms which (we now believe) were non-referring'*

were used by scientists of both rival paradigms in their first hypotheses, served as a basis for the formulation of their theories, and without them it would have been impossible to proceed to any discovery. Besides, these terms were the outcome of a continuous process of age-long inductive generalizations. All these aspects speak for the continuity of knowledge within the scientific community and for its contribution to the progress of inquiry in better approximation to reality.

The last aspect of Kuhn's account concerns the debate on fundamentals in crisis before the scientific revolution by the emerging new paradigm, a debate that normal science tries to avoid in order to achieve detailed understanding of phenomena within the old paradigm[359]. This discussion on fundamentals is just like '*picking up the other end of the stick*' (p. 85), since the scientists, who achieve these fundamental inventions, see the same data from a completely new perspective, and when the transition is complete, it leads to radical change of views, methods and goals; therefore, for Kuhn, the most of these inventors '*have been either very young or very new to the field whose paradigm they change'* (p.90). Taking into consideration these notions, I try to explore whether such a discussion on fundamentals took place in the Chemical Revolution and by which means the new paradigm emerged.

Pyle argues against that '*Kuhn's marks of crisis were absent in the Chemical Revolution, as there was no debate on fundamentals'*[360]. However, if we consider the fact that phlogiston theory, which survived almost a century, was based mainly on an entity (phlogiston), whose existence Lavoisier doubted, we may infer that Lavoisier's notion brought a debate over fundamentals, since doubt about the existence of phlogiston is discussion on fundamentals. On the other hand, within the paradigm of phlogiston theory no one called the existence of 'phlogiston' in question, even though the phlogististic chemists presented many different versions of the same theory (Priestley, Cavendish, Kirwan etc.), yet all of them were based upon the existence of phlogiston. Therefore Lavoisier characterized it as a 'Proteus capable of changing his nature from one phlogistic chemist to the next'[361].

[359] See Thomas Kuhn (1970), pp. 77-91.

[360] See Pyle (2000), p. 114.

[361] See Lavoisier *Oeuvres* II, p. 640:*'Sometimes this principle is heavy and sometimes it is not; sometimes it is free fire and sometimes it is combined with the earthy elements; sometimes it passes through the pores of vessels and sometimes they are impenetrable to it… It is a veritable Proteus, which changes its form every minute'.*

But how Lavoisier, who although grew up with this old theory, was able to see the same data from this completely new perspective? Following Kuhn's notion one might argue that Lavoisier was very young, therefore he was little committed to the traditional rules of normal science (phlogiston theory in our case). First, Kuhn's inductive generalization (young or new to the field) is questionable, since it is based on a limited number of samples and does not include examples from the whole history of science. Or, as Peirce would put it, the inference is based on a limited number of instances and is supposed to represent a true ratio of the whole class, therefore it may be erroneous. Second, if we take it as true, then we might ask by which reasoning and criteria of appraisal Lavoisier proceeded to his discovery, since not all young or new to the field scientists have been inventors.

As I previously said, if we compare this process to Peirce's account of scientific progress i.e. 'the real progress in science involves leaps in the abductive phase of inquiry, which are radical and involve some novel way of reasoning about the observations, which would previously have been passed unperceived' (CP 1.109), then we can realize that we can explain Lavoisier's reasoning better by abduction. Besides, in order to be able to 'pick up the other end of the stick', one has to see the same phenomenon from a totally new perspective without any preconceived ideas about it. In this sense, a crucial criterion of appraisal of Lavoisier's theory should have been something like the Nr. 5 Peirce's criterion of admissibility of hypotheses, namely 'not attach too much importance to antecedent likelihood of a hypothesis, for preconceived ideas and likelihoods are treacherous guides' (CP 2.777, 7.220) or what Paul Feyerabend called *proliferation principle*. I shall return to this topic in the next Chapter to show in detail, how Peirce's criteria of admissibility of hypotheses can be applied to Lavoisier's reasoning in the abductive phase of inquiry and explain the discovery and the rational appraisal of his theory.

As for Kuhn's notion of radical change of views, methods and goals after the revolution, we have to admit that such changes occurred after the Chemical Revolution. First, it altered the long lived Aristotelian view that water and air were elements, and it led to the new chemical classification of compounds and simple substances. It was only the Atomists of the past, who proposed that all of the four basic elements are composed of indestructible atoms, and the differences in a physical property such as the density of these atoms would explain the transformations; therefore the Chemical Revolution opened the way to Dalton's atomic theory that followed some years later, and it showed that variation in density was not a sufficient explanation for the problem of transformations, as held by the ancient Atomists. Furthermore, Lavoisier's converse methods of analysis and synthesis, which have been demonstrated by analyzing air and water into its constituents and by recombining them again, have been established afterwards as regular methods in chemistry. In sum, after the revolution scientists actually changed their views about the structure of the world and their experimental practices, but these effects were far-reaching and did not occur at once.

Another significant aspect that the Chemical Revolution introduced was that discovery cannot be judged by local applicability alone, but, rather, by connections with the established canons of knowledge not only within the domain of inquiry but also outside, namely connection with Atomism and the growing Newton's gravitational theory. First, the growing Newtonian mechanistic world-view, as Kuhn argues, '*led chemists to insist that gain in weight must mean gain in quantity of matter*'[362], which inspired Lavoisier to apply the principle of conservation of weight, introduced by the Atomists and used by the chemists of pneumatic chemistry, in order to explain the gain in weight on calcinations. Second, this application led him to use the reversibility of the crucial reactions of air and water, which contributed to his discovery. Because if one assumes that a substance contains A and B as constituents (hydrogen and oxygen in Lavoisier's case of water), it does not imply that A and B are the only constituents. The confirmation can be obtained only by reversing the reaction, and this is what Lavoisier actually did with the reactions of water and air. Certainly, this confirmation presupposes the hypothesis of the conversation of matter, whose truth Lavoisier assumed, as he admitted in his *Elements of Chemistry*[363]. This aspect of scientific revolutions, i.e. connections of theories outside the domain of local inquiry, in my opinion, is very significant, since it gave rise later to the promotion of the interdisciplinary work within the scientific community[364].

Concerning the radical change of goals and values after the revolution, certainly Kuhn's account emphasizes the social structure of science that affect the change of scientific values. Scientific community as an organized behavior and structure is influenced by social

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[362] See Thomas Kuhn (1970), p. 71.

[363] Gillispie Charles in (1960), p. 231, argues that Lavoisier assumed the law of conversation of matter as a precondition just like the ancient materialists (atomists), and he cites Lavoisier's own words: '*We must lay it down as an incontestable axiom that in all the operations of art and nature, nothing is created; an equal quantity of matter exists before and after the experiment*'. Guerlac (1961), xv, by citing Metzger and Bacon argues that the conversation law had long been a working principle of chemist at the first decades of seventeenth century.

[364] See also Holmes (2001).

and political situations, which could result in the loss of the specific mechanisms of scientific progress and change. This social new approach to the philosophy of science that Kuhn introduced gave rise to further development in this direction, as I previously said. Because Peirce's notion of the scientific community was very 'ideal', as it did not examine the necessary institutional characteristics, which could secure its independent, autonomous, democratic and inquiring role, against the influence of prevalent notions (paradigm). Indeed, Kuhn admits that e.g. Aristarchus' suggestion of heliocentric system was made eighteen centuries earlier than Copernicus, but it could not anticipate the Aristotelian paradigm and the Ptolemaic astronomy, as long as they continued to prove capable in the problem-solving activity[365]. However, though Kuhn emphasizes the social structure of science, he misses here one significant social factor, which was pointed out later by the social constructivists, namely the political and social conditions of that time would not have allowed the development of a rival to the prevalent Aristotle's paradigm.

Moreover, although Kuhn introduced social approach to the philosophy of science, he did not see either that especially this social aspect of science is the one that allows the continuity and growth of knowledge within the scientific community, which in turn prepares the ground and enables the scientists to proceed with leaps to discoveries. As we have seen, although the Chemical Revolution was not strictly cumulative, there was sufficient continuity in it, whereas the adoption and appraisal of the new theory was in general rational, therefore we cannot liken it to religious or political conversions, as this metaphor is inaccurate in goals and motivation. More about social motivations I will discuss in the relevant Section of the account of sociologists of science.

4.2.5. Lakatos' account

Lakatos sees scientific changes as *competition between research scientific programs* (Lakatos uses the British term 'programmes'), which contain a *hard core* and a *protective belt*. Changes within research programs are made only to the protective belt, but never to hard core, while scientific revolutions occur when a *degenerating* research program is overthrown by a *progressive* one. A program is progressing, as long as it expands its application to larger set of cases and its progressive versions make novel predictions, which

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[365] See Thomas Kuhn (1970), pp. 75-76.

are confirmed[366]. Whereas, each new version of a degenerating program is inconsistent with the previous versions i.e. is *ad hoc* with respect to its predecessor[367]. Musgrave (1978) applies all these principles of Lakatos to his reconstruction of the Chemical Revolution, so as to provide an account in Lakatos' perspective, which, as he claims, aspires to be more sophisticated position of falsificationism and conventionalism.[368].

The main argument presented by Lakatos' methodology is that phlogistonism was a degenerating research program, while the oxygen program was a progressive one. To support this argument Musgrave argues that between 1770 and 1785 'each version of the oxygen program was empirically and theoretically progressive', while 'after 1770 the phlogiston program did neither', since each version was inconsistent with the previous one, so to say, it 'consisted of a series of *ad hoc* devices, mutually inconsistent with each other'[369]. Pyle (2000) argues against that 'phlogistic chemistry was a successful and progressive research program when it was overthrown, as it was also generating lots of new confirmed predictions' i.e. Scheele in 1786 used phlogiston theory to produce and isolate successfully new acids[370]. To examine Musgrave's claim, I explore the question whether the two research programs between 1770 and 1785 were progressive or degenerating.

To begin with oxygen program, as we have seen, Lavoisier's first hypothesis in 1772, i.e. 'the same air, which was absorbed in calcinations and combustions, was liberated in the reduction of calx of lead (PbO) with charcoal', it was proved to be false, since this gas was carbon dioxide; therefore Lavoisier corrected this first assumption in 1775. This means that oxygen program generated false predictions between 1772 and 1775. Furthermore, oxygen as principle of acidity introduced in 1777 and supposed to extend to acidity (application to larger set of cases), as Musgrave claims, it was proved to be false in 1815, which means that oxygen program generated false predictions also between 1777 and 1815. The same is

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366 As Lakatos in (1978), p. 112, puts it: *'A research programme is said to be progressing as long as its theoretical growth anticipates its empirical growth, that is, as long as it keeps predicting novel facts with some success (progressive problemshift); it is stagnating as it gives only post hoc explanations either of chance discoveries or of facts anticipated by, and discovered in, a rival programme'*.

367 See Lakatos and Musgrave (1970), pp. 9-15.

368 See Musgrave A. (1978), *Why did Oxygen Supplant Phlogiston?* In *Method and Appraisal in the Physical Science*, pp. 181-209.

369 See Ibid, p. 205 and p. 203, where he cites, as an evidence for his argument, Lavoisier's critique against the contradictory properties of the same entity presented by the phlogistonists, as explained 'just like *Proteus'*, *Oeuvres* II, p. 640.

370 Pyle in (2000), p. 107, argues that Scheele produced many organic and non-organic acids. If we look at my reaction (4), the intermediate products, nowadays oxides, by dephlogistication can give acids.

true with ‘caloric’, which was proved to be false as well. All these retrogressions, with the exception of caloric, do not concern changes of the *protective belt* (auxiliary hypotheses), but of the *hard core*, therefore they are not signs of a progressive research program, according to Lakatos’ definition.

As for phlogiston program, apart from Scheele’s success in 1786, it had done many predictions successfully between 1770 and 1785. First, Priestley was the one who discovered the ‘dephlogisticated air’ 1774, which Lavoisier named oxygen. Second, Cavendish was the one who discovered the ‘inflammable air’ that led to the discovery of the composition of water. Third, it was Priestley who held successfully that air was not a simple substance but a compound in 1775, and, besides, the phlogistic chemists were the ones who discovered the composition of water in 1781. This led to the new chemical classification of compounds and simple substances, namely water and air. Musgrave in order to answer Kuhn’s question i.e. ‘who first discovered oxygen?’ argues that Lavoisier was the one, because he did not only isolated it but he also correctly identified[371] it. Granted Musgrave’s thesis, one might argue that Priestley or Watt, as they had isolated water and correctly identified it as not a simple substance but as composition, they discovered its composition. Finally, it was Kirwan[372] the one who showed in 1789 that dry charcoal when heated emitted inflammable air, which it was anomaly for the oxygen theory and it was proved to be carbon monoxide, as explained. All these were successful predictions of the phlogiston program, which cannot be regarded as signs of a degenerating research program, according to Lakatos’ definition.

In my opinion, Musgrave’ account over-emphasizes the role of the time factor (i.e. between 1770 and 1785); therefore it fails to establish a Lakatosian account of the Chemical Revolution. On the contrary, Peirce’s notion of ‘in the long run’ progress of scientific inquiry within the scientific community as a whole can explain the historical events better. Because, for Peirce, scientific inquiry involves much retrogression in the short run, while only the long run application can secure the progressive character of scientific inquiry. As I have shown, according to *synechism* (CP 5.180-212. 1903; *Pragmatism as the Logic of Abduction*), reality is not an instant result of inquiry, as this notion implies that the contents of time consist of separate and unchanging states, but of a continuous sequence of events, therefore we cannot reject the whole process.

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[371] See Musgrave A. (1978), p. 195.

[372] See Kirwan (1789), p.41.

Although Lakatos emphasizes this continuous Hegelian aspect of progress of inquiry[373], and as a result he does not need to specify time limit for characterizing a research program progressing[374], Musgrave's account, in order to show the progress of Lavoisier's program, is focused only on short-time examination, therefore it degrades this far-reaching and fertile process. Furthermore, even though Lakatos, correctly in my view, argues that historical 'internal' reconstruction should try to explain history of science rationally and to discover novel historical facts[375], Musgrave fails to show the rational process of discovery in the Chemical Revolution. But we have to admit that some criteria of rationality, proposed by Lakatos (1978), contributed much to the progress of inquiry. These criteria are: theories with more explanatory power, with no anomalies, with excess in empirical content, extension of their application to larger set of cases and theoretical growth that anticipate their empirical growth. At first sight, as we have seen, some of the above mentioned criteria characterized Lavoisier's theory (e.g. more explanatory power, no anomalies); therefore I am going to explore this question in the next Chapter in detail. From this point of view, I can agree with Thackray and Schofield concerning the 'rationalization of chemistry', but not with their view of the 'profound failure of the Newtonian program' in eighteenth-century chemistry[376], because, as I argued, the Newtonian mechanistic view inspired Lavoisier to apply the principle of conservation of weight in his reverse reactions.

However, the success of science cannot be attributed to one 'superior' research program, but to the whole scientific community, since the discovery of oxygen would have been impossible without the discovery of the dephlogisticated and inflammable air, as well as of the water composition by the phlogistonists. Neither can a superior and 'ideal' (progressive) research program better contribute to the progress of science, as Lakatos and Laudan [377] after him held by suggesting some methodological rules and reasonable criteria to scientists, so as to join the ideal research program. In my opinion, the progress in the Chemical Revolution was better served by diversity of competing opinions within the scientific community, where different scientists made different choices, due to distinct reasons.

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[373] Ian Hacking in (1981) has usefully drawn attention to the importance of Lakatos' Hegelian background.
[374] See Lakatos (1978), pp. 116-17.
[375] See Lakatos (1978), pp. 133-34.
[376] See Thackray (1970), and Schofield (1970).
[377] See Laudan L. (1977).

First, because of the fact that scientists' skill, which contributes to the progress of scientific inquiry, consists of different aspects applied to different phases of inquiry. As explained, it involves originality in the abductive phase, good predesignation of experiments in the deductive one and efficient and without prejudice experimental tests in the inductive phase. I am going to show in the next Chapter in detail that the contribution of each program (phlogistonists and Lavoisier) to the discovery and to the formulation of the new theory was different in each phase of inquiry. Second, competition of different opinions within a pluralist scientific community constitutes an inner dynamic that forces scientists of rival theories to attribute more weight to the anomalies and the solved problems. As we have seen, both Lavoisier and the phlogistonists even though they began with ignoring several anomalies, they were forced later to attribute more weight to the anomalies that their theory could not explain.

Furthermore, due to social reasons different research programs in different countries develop some topics of inquiry more than others traditionally, whose results scientists from other programs can use for their inquiry. As Kun argues[378], '*Islamic chemists had known that some metals gain weight when roasted*', and Mayow in 1674 held that this increase in weight was due to the atmospheric air. It was this background knowledge that led Lavoisier to explaining the gain in weight on calcinations by the absorption of air. Finally, as I concluded, connections of theories outside the domain of local inquiry contributed much to the progress of inquiry. On the other hand, though phlogiston worked successfully for more than a century as 'sulphur principle' and as a component of salts[379], it could not work any longer in pneumatic chemistry. All these factors contributed much to the discovery of oxygen; therefore we can infer that the Chemical Revolution was better served by diversity of competing opinions within the scientific community.

4.2.6. Social constructivist account

Modern sociologists of science, represented mainly by Bloor, Barnes, Shapin and Latour, have tried in the last decades to explain radically all scientific beliefs in sociological terms. Although they embraced Kuhn's *Structure of Scientific Revolutions*, Kuhn himself did not like this radical sociology of science that followed him, as he was more concerned with the 'internal' (formation of beliefs) and within the scientific community influence on science,

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[378] See Thomas Kuhn (1970), p. 71.
[379] See Kim (2008).

while sociologists of science were concerned with the 'external' social life on science[380]. The principles of the self-called *Strong Programme* say that the acceptance of scientific change *cannot be explained by appeal to reality or rationality*, since scientific methods and beliefs are relative to social interests and are casually influenced by social factors[381]. Although Laudan criticized the *Strong Programme*, he restricted this principle, with his *arationality assumption*, only to 'those beliefs that cannot be explained in terms of their rational merits'[382]. Miller (2003) provides an account of the Chemical Revolution from this point of view, which is based on the historical study of Bernadette Bensaude-Vincent[383] (1983) and he connects it with the 'attributional model of discovery'[384]. According to this model, the birth of modern chemistry has been attributed to French or British chemists by the historians of science in accordance with the different competing national interests among France, Prussia and England.

The main arguments presented there are that French chemists led by J. B. Dumas and linked up to the wider politics of reputation and scientific symbolism, constructed a founder myth around the figure of Lavoisier, who was painted as the founding father of modern chemistry, just like the British, who also tried to present Cavendish as the founding father of the New Chemistry. In the same way historians of science participated in this 'attribution model', as they have tried to attribute different discoveries to Lavoisier, Priestley, Cavendish, etc. In this sense, as Miller claims, '*social interests are invoked as explanations for the acceptance of certain interpretations rather than others*'[385]; therefore he compares the whole network to Latour's 'actor's network theory', where 'an attempt is made at a neutral monistic account of the solidification of certain interpretations rather than others'[386].

Indeed, Dumas tried to idealize Lavoisier, since he painted him as a scientist gifted with a 'divine' intuition and tried to exploit the historical data by presenting Lavoisier as the initiator of the conversation law, incorrectly as we have seen, and with his law of fixed proportions as the forerunner of atomism that followed him with Dalton, although Lavoisier's principles were contrary to atomism. The Alsatian chemist A. Wurtz in 1875

[380] See Godfrey-Smith (2003), p. 127.
[381] See more Bloor D. (1991), and Barnes B. and Bloor D. (1982).
[382] See Laudan L. (1977), p. 202.
[383] See Bernadette Bensaude-Vincent (1983), *A Founder Myth in the History of Science*, pp. 53-78.
[384] See Miller, David P. (2003), pp. 11-83.
[385] See Ibid, p. 36.
[386] See more Latour B. (1987). Miller in (2003), p. 36, cites this conclusion of Latour.

followed Dumas and tried to degrade the importance of Lavoisier's false notion of caloric, by claiming that the '*shift from substantialist concept of heat to a mechanistic theory was not a basic change but merely a variation of form*'[387]. As we have seen, Lavoisier's success does not lie in the caloric, since it was later proved to be false, and, in my view, Dalton's contribution to the progress of science was not lesser than Lavoisier's. However, we cannot disregard the fact that Lavoisier established the oxygen theory, certainly, not with his 'divine' intuition. Therefore I am going to show the rational process followed in the Chemical Revolution, which consisted in explaining theories that rendered the observed facts and in experimental testing of the theories' consequences or predictions.

Furthermore, we have to admit that knowledge is socially developed and that social factors influence our language due to its conventional character, as well as that different interpretations are casually related to different motives and social interests. On the other hand, Peirce's pragmatic definition of the 'real' involves also a consensus agreed by the scientific community. But the object of a belief is prior to collective choices, therefore a consensus comes after observations, experiments and theories, and not vice versa. Because beliefs presuppose the object of belief, as they are influenced by external reality, namely facts. Interpretations of historical episodes are only hypotheses that 'may render the observed facts necessary or highly probable', which are theory-laden with philosophical background assumptions of the observer or, as Peirce says, abductively drawn hypotheses are influenced by the background knowledge of the inquirer; therefore they have to be brought to the test of experience (historical events in our case) for appraisal, as experience constrains beliefs, while beliefs that conflict with recalcitrant experience resign. Since historians and philosophers of science do not disagree about the facts, but about their interpretations, therefore each one brings his interpretation in public for assessment within the scientific community.

Following the same reasoning, we can say that the beliefs established in the Chemical Revolution and led to the discovery were not an outcome of a consensus due to social interests, although each side tried to idealize and promote its 'protagonist'. The phlogiston theory, as we have seen, after the discovery of decomposition of water could not any longer cohere with experience, therefore it was gradually abandoned by the scientific community. It was abandoned not only by French phlogistic chemists like Guyton, but also by British,

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[387] See Bernadette Bensaude-Vincent (1983), pp. 60-78.

Italian, Spanish and Germans[388]. This shows us that the object of a belief is ontologically prior to collective choices and social interests with regard to the formation of a belief about it. Because to reach a consensus about something presupposes the existence of this 'something' (oxygen in our case), otherwise if this 'something' does not exist (e.g. phlogiston), the belief about its existence will be abandoned in the long run, as false beliefs resign in the face of experience; in this sense, external reality and experience constrain beliefs. The method of establishing beliefs imposed by social authorities had worked in some societies, defined by Peirce as second kind of inquiry (method of authority), but not any longer.

Certainly, as I said, scientific community like all other communities, is governed by some ethical norms or values and can be influenced partly by social and political situations that could lead to the loss of the specific mechanisms of scientific progress. As we have seen, except some idealizations by both sides and problems of priority in scientific discovery (e.g. Water Controversy) there were not significant problems of autonomy of the scientific community at this time, but this was a special case[389], due to the structure of the scientific community in the eighteenth century, which consisted mainly of self-trained scientists and amateurs. However, in later episodes in the history of science scientific community did not work so autonomous, due to its institutional structure[390]; therefore, as I concluded in the first part of the study, we have to explore the necessary conditions that can secure its independent, autonomous, democratic and inquiring role.

Concerning the appeal to rationality, the scientific community in the Chemical Revolution behaved rationally, in my view, and abandoned the phlogiston theory that did not have the balance of evidence in its favor. On the other hand, Lavoisier's theory, besides it had the balance of evidence in its favor, it had lesser anomalies and more explanatory power, as it

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[388] See Beretta (1993), pp. 221-244, Donovan Arthur, (1993), p. 154-6, Thagard (1992), p. 49, Aykroyd W. R., (1935), pp. 84-87, and Gillispie Charles, (1960), pp. 202-59. Nordmann also in (1986) provides an account for the reception of antiphlogistic chemistry in Germany.

[389] Here I can agree with Golinski (1998), who emphasizes the 'capacity of individuals for autonomous self-expression' in eighteenth-century chemistry.

[390] Cohen B. I. in (1985), *A Second Scientific Revolution and Others?* pp. 91-103, argues that the earlier institutions like *Royal Society of England* and *The Academy of Sciences of Paris* consisted mainly of self-trained scientists and amateurs. But this structure changed in the nineteenth century, when universities became the centers for research. While since the end of the 19th century government institutions have provided financial support for research with serious consequences upon the autonomy of the scientific community. Compare also with Böhme Gernot et al. (1976), *Finalization in Science*, in Stehr Nico, Grundmann Reiner (2005), pp. 302-26.

could explain combustion, calcination, decomposition of water and solution of a metal in an acid.

Finally, the success of the Chemical Revolution cannot be attributed to one program or one protagonist, because, as we have seen, the whole scientific community contributed to that. This means that the whole tradition of chemists, from Boyle, Black, Mayow and Hales to Priestley, Cavendish, Kirwan and Lavoisier, contributed to the development of the final theory. Indeed, there are always idealizations presented by the one or the other side, but these are properly evaluated by the scientific community. Apart from that, we cannot ignore the fact that within the scientific community each member is informed about the work of the other member uses his experience, learns from his failures, and as result the whole community proceeds gradually and fallibly towards the representation of reality. But all these can be achieved with the condition that scientific community is autonomous. Therefore the structure and the institutional characteristics of the scientific community itself, raised by the social constructivists, are significant for its inquiring role.

4.2.7. Conclusion of the discussion

After having presented and discussed the different accounts of the Chemical Revolution, we can realize that some aspects of the previous discussed accounts are problematic, while some others introduce crucial points and perspectives, which are important for the self-corrective character of scientific method applied in the Chemical Revolution. To be more precise: first, the characterization by the *positivists* of Lavoisier's method as scientific and phlogiston theory as unscientific was unsuccessful, since quantitative results of experiments conducted by the phlogistonists and Lavoisier were both 'scientific'. Moreover, both sides improved and used chemical apparatus (eudiometer, hydrometer, thermometer and calorimeter) for their experiments, which allowed them to discover new substances. Furthermore, the method that they both used consisted in explaining theories that rendered the observed facts and experimental testing of their predictions.

However, Perrin raised a crucial question, which has to be taken into account, i.e. the replacement of the older, less adequate theory with the newer, more adequate, was without any sudden Gestalt switch, an aspect that implies the continuity of the background knowledge. Because, as we have seen, the introduction of the new entity 'oxygen', followed, many steps of identification, i.e. first as 'air', then as 'pure air', and finally as 'purest part of air', which means, it was the outcome of a continuous process, and not of Gestalt switch.

Second, the use of different criteria of appraisal of the two theories and their relation to rationality were not clear shown, since the criterion of simplicity alone, as posed by the *conventionalists*, does not suffice for explaining the progress and the acceptance of Lavoisier's theory. As I began to explain, Lavoisier, apart from the criterion of simplicity, took different criteria into account for the evaluation of his hypotheses. For that reason I am going to discuss in the next Chapter the relation between the different criteria of admissibility of the hypotheses and the formulation of the new theory, as well as with the discovery.

Third, the 'crucial experiments' that falsified different versions of Lavoisier's and phlogiston theory, pointed out by the *falsificationists*, though they were not clear indicated, contributed much to the Chemical Revolution. I began to explain, by taking into consideration Popper's definition of the crucial experiments, which experiments, in my opinion, were crucial for the advancement of inquiry, and I concluded that four experiments were the crucial ones, instead of the two posed by the falsificationists. In order to support this view I am going to show in detail next the whole process in my own reconstruction. Furthermore, the principle of corroboration through severe tests of rationality and crucial experiments had a great share in the acceptance of Lavoisier's theory by the scientific community, as I am going to show as well.

Fourth, *Kuhn*'s account posed crucial questions and showed the radical conceptual change, which actually occurred in the Chemical Revolution, as we have seen; however the method and the kinds of inferences that led to the conceptual change and to the discovery of the new entity have not been explained. Therefore, by taking into account also Perrin's notion, I am going to explore in detail the process, in order to find out the continuity or discontinuity of knowledge, as well as the relation between continuity and conceptual change. With regard to the incommensurability, as I argued, it was absent in that event, since the translation failure of the non-referring terms was not a problem of mutual understanding. Furthermore, as I concluded in the relevant Section, the problem of referring or non-referring terms, posed by the defenders of 'pessimistic meta-induction', was not a criterion of continuity and progress, because both sides used non-referring terms (e.g. phlogiston, caloric) in their initial hypotheses, which served as a basis for the progress of inquiry. For that reason I am going to explore next the process of the formulation of the first hypotheses, as well as their background. Moreover, since the proof of the existence of referring terms might be important for the acceptance of a theory, I am going to investigate the contribution of the experimental proof of oxygen, as a referring entity, to the acceptance of Lavoisier's theory by the scientific community.

Fifth, the crucial aspect of the scientific community and the interdisciplinary work, posed by both Kuhn and *social constructivists*, showed their importance for securing the self-corrective character of scientific method. For, as we have seen, it forced the scientists of rival theories to attribute more weight to the anomalies and the solved problems, led to the interaction of different traditions of scientific research, contributed to the increase of the background knowledge for the adherents of both theories etc. Therefore I am going to examine in detail the contribution of each of these different features of scientific community, as a necessary background that constitutes an inner dynamic, which in turn could lead to the advancement and progress of inquiry.

Sixth, although Musgrave's account was more focused on the time factor, *Lakatos*' normative criteria of progressiveness and rationality have to be taken also into account, as conditions for securing the self-corrective character of scientific method. This means: theories with more explanatory power, with no anomalies, with excess in empirical content, extension of their application to larger set of cases and theoretical growth that anticipate their empirical growth. Therefore I am going to explore whether these criteria characterized Lavoisier's theory.

And seventh, the crucial question whether the scientific method applied in the Chemical Revolution was self-corrective or not has not been answered. For that reason, in case of an affirmative answer, have to be explored the mechanisms that contributed to this process. I can infer, then, that all these questions, which still remain open after the discussion of the different interpretations, have to be examined in comparison with Peirce's conception of the SCT. In this way I will be able to explain the method and the sorts of inference that led to the discovery of the new entity, the formulation of the new theory, the conceptual change, as well as the contribution of the scientific community to the continuity of knowledge and to the Chemical Revolution. Apart from that, this reconstruction will help me to discover any omissions or weaknesses of Peirce's account, so as to integrate some of these views into Peirce's conception of the SCT.

4.3. Self-Correctiveness in the Chemical Revolution

In order to answer all these questions, I shall proceed first to explain the scientific method followed in the Chemical Revolution in the perspective of Peirce's methodology, so as to show the self-corrective character of the scientific method. Furthermore, I am going to explore whether Peirce's methodological theory can provide more rational reconstruction of this episode than the existing ones, and whether it is consistent with the historical data and can answer the questions left open by the other accounts. This reconstruction will enable me to evaluate the different aspects of Peirce's SCT, and in case of some omissions to integrate some new notions into the Peircean account.

4.3.1. Historical reconstruction

As I said, we can explain Lavoisier's reasoning of discovery better by abduction than by induction, therefore I am going to explore Lavoisier's reasoning in the different phases of his inquiry in order to demonstrate this claim. Besides, in my view, Lavoisier's success lies in his corrective method that consisted in gradual modification of his hypotheses through their rational appraisal (criteria of admissibility) and in inductive testing of their consequences. Because, as Lavoisier admitted in 1777 before the Academy, he was exponent of the method of gradually corrected hypotheses through their experimental testing[391]. In this reconstruction of the historical events I am going to exhibit that the scientific method followed in the Chemical Revolution consisted of abduction, deduction and induction, and that it was self-corrective, since it allowed the scientists to correct their false theories in the finite long run and to find an alternative theory that could explain the oxidation. Moreover, as it was applied by a community of inquirers, therefore it could lead to the establishment of beliefs that represented approximately the regularities in nature.

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[391] See Donovan Arthur (1993), p 149, Lavoisier's speech in the Academy, when he presented his new theory of oxidation: '*Facts, observations and experiments are the building blocks of a great edifice, but when gathering them in science one must avoid creating obstacles. On should rather organize them and indicate the classes to which they belong, and to which part of the whole each of them pertains… They are more precisely methods of approximation we employ while solving problems.* ***They are hypotheses, which after being altered, corrected and changed whenever contradicted by experiments, will one day infallibly lead us through exclusions and eliminations, to knowledge of the true laws of nature'*** (Bold letters are mine). Peirce also in (CP 5.363 and MS 334) comments on Lavoisier's method, as follows: '*His way was to carry his mind into his laboratory and to make of his alembics and cucurbits veritable instruments of thought, giving a new conception of reasoning, …, by manipulating real things instead of words and fancies*'.

Lavoisier began in 1772 with his first hypothesis, i.e. 'in calcinations and combustions might be absorbed air', which became more concrete in 1774. He proceeded, as explained, probably by following Mayow's notion and Hales' work, and by taking into account as first criterion that 'the same data could be explained with different theories', and as a second criterion that this hypothesis was 'more probable', as he claimed[392]. If we look back to Peirce's criteria of admissibility of the hypotheses, the first criterion of Lavoisier's appraisal looks like the criterion Nr. 5, namely 'not attach too much importance to antecedent likelihood of a hypothesis, for preconceived ideas and likelihoods are treacherous guides'(CP 2.777, 7.220). While the second criterion we can liken to Peirce's criterion Nr. 1, which means, 'choose hypotheses that may render the observed facts necessary or highly probable'. From this point of view, we can represent Lavoisier's first abductive inference as follows:

First Abduction
Metals gain weight in combustion and calcination and they become calxes (result)
All metals gain weight when combined with another material (rule)
Calxes are metals combined with another unknown material (hypothesis as conclusion)

We can compare it with Peirce's schema of abduction:

These beans are white (result)
All the beans in this bag are white (rule or prior proposition)
Are these beans from this bag? (case as a conclusion)

and

The surprising fact, C, is observed (result)	*S is P (result)*
But if A were true, C would be a matter of course (rule)	*All M is P (rule)*
Hence, there is a reason to suspect that A is true (hypothesis) (CP 5.189)	*S is M (hypothesis)*

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[392] Knickerbocker in (1962), p. 134, cites Lavoisier's following statement: *'In attacking here the doctrine of Stahl, it was not my purpose to substitute for it a rigorously demonstrated theory, but only a* ***hypothesis, which seemed to me more probable, more in conformity with the laws of nature, and one which appeared to involve less forced explanations and fewer contradictions****'* (Bold letters mine).

Where C: ‘Calxes (*S*) are metals that gain weight’ (*P*) and A: ‘metals (*P*) gain weight when combined with another material (*M*)’. Therefore: ‘Calxes (*S*) are metals combined with another material (*M*)’. Here the consequent is that ‘gain in weight of calxes’ that is given in perception and the inference about the antecedent in a form of a hypothesis that entails C, as a cause of the unexpected fact, is that ‘the gain in weight is due to unknown material combined in calxes’.

The definition of this unknown material of the conclusion led to three alternative and different hypotheses: it could be, for Lavoisier, *air*, while for the phlogistonists, it could be *particles of fire*, or, for Priestley, *dephlogisticated air*. Other experiments had shown that calx of lead (PbO) when heated with charcoal and turned into metal, air was liberated. This fact, for Lavoisier, was the confirmation of his conjecture that the same air that was absorbed in calcinations and combustions it was liberated in reduction with charcoal. However, the exponents of the phlogiston theory held that the same reaction could have been represented as follows:

Calx of lead + charcoal (which included phlogiston) → lead + dephlogisticated air

As we have seen, in that experiment, taken as a confirmation of Lavoisier’s hypothesis, it was not oxygen that was liberated, but carbon dioxide (CO_2), in accordance with the following representation:

$$2PbO + C \rightarrow 2Pb + CO_2$$

Here we can notice the problem that I pointed out in the first part of the study, i.e. experimental verification taken as confirmation of a hypothesis might concern the background assumption and not the hypothesis itself, due to confounding factors, which in our case is the presence of charcoal. Therefore in that case it was required to ‘break the hypothesis into its smallest components and then derive the practical consequences of each one’ (Peirce criterion Nr. 6). As I said, in similar cases the inquirer has to explore the contribution of each distinct factor to the observed event and design experiments by excluding or including each time one factor. Lavoisier actually conducted the same experiment without the presence of charcoal this time. In order to verify his hypothesis he drew its experimental consequences; therefore he repeated Boyle’s work, since Boyle’s claim was that 'gain in weight was produced by material particles of fire, which had been taken by the metal'. The deductive reasoning that Lavoisier applied should have been like the following:

'*If the increase in weight is produced by fire, it must come from outside the vessel'*
'If the increase in weight is produced by air, it must come from inside the vessel'

Following now the pragmatic maxim, 'if *I conduct myself in a manner x, I will have experience y'* and its inversion 'if I want to have experience *y*, then I will conduct myself in manner *x*', we can translate it as follows:

'If I want to have experience whether the increase in weight comes from outside or inside, then I will conduct the same experiment with a sealed vessel'

Lavoisier conducted this experiment and weighed the entire sealed vessel before and after heating of calxes of lead and tin (PbO and SnO_2). The result of the inductive testing of his hypothesis was that the weight of the vessel was the same before and after, while only the lead and tin inside had increase in weight. Lavoisier's explanation was that the increase in weight was due to the air contained in the vessel, which during the combustion was absorbed by the metal; hence he took it as a confirmation of his hypothesis. As I initially said, this was the *first planned crucial experiment.* Because, if we take into account Popper's definition, we may say that it was designed to refute Boyle's theory about the increase in weight, which had been also adopted by some of the phlogistonists and served as a basis for their theory.

On the other hand, another deductive consequence of the same hypothesis would have been as follows:

'If calxes were metals combined with air' then '*decomposition of calxes must give air off*''

This deductive prediction Priestley actually tested experimentally, in order to refute probably Lavoisier's hypothesis, or to provide further evidential support for the phlogiston theory. Because Priestley held that the reverse reaction of combustion with presence of air could be represented as follows:

From: Metal → Calx + Phlogiston,
To: Calx + Air (which included Phlogiston) → Metal + (Air – Phlogiston) (dephlogisticated air)

In 1774 Priestley showed that calx of mercury (HgO) when heated gas air-like was given off, and that calx was reduced to the metallic state without the use of charcoal, according to the following reaction, for Priestley:

Calx of mercury → Mercury + (Air – Phlogiston) (dephlogisticated air),

While, for Lavoisier, the representation was as follows:

Calx of mercury → Mercury + Air

And according to the nowadays representation of reaction:

$$2HgO \rightarrow 2Hg + O_2$$

First, we can notice that although the reactions quoted by Lavoisier and Priestley cannot be translated into the language of the new chemistry, we have no problem in understanding them, as I said. Furthermore, this discovery of Priestley was a bottom-up process, as it was an outcome of his experiments, i.e. arising from a new observational resource. According to Peirce's definition, each new discovery can arise from a new observational resource, which might be unintended as well, and from some novel way of reasoning about the observations (CP 1.109). As we can realize, for Priestley, the discovery arose from a new observational resource, whether intended or not it is questionable. Actually, Priestley tried to refute Lavoisier's hypothesis and to provide further evidential support for the phlogiston theory intentionally with the experiment of the reverse reaction of combustion. In this sense, the discovery of dephlogisticated air was not an unintended by-product. But he embedded these results of his induction in the old theory, which included the same classification of the gases and their casual relations, as he held that this new discovered gas was dephlogisticated air (air free from phlogiston). His discovery, then, consisted only in discovering a new existing gas (not imaginative) intentionally, but not in accommodating this gas to a new theory of calcination. From this point of view, this experiment was the *second planned crucial experiment*, conducted by Priestley, since it refuted the first version of Lavoisier's hypothesis about the reduction of the calx of lead with charcoal, and this refutation urged later Lavoisier to correct his initial hypothesis.

For Lavoisier, on the other hand, the discovery arose from a novel way of reasoning about these new observations, which, after some successive corrective steps, led him to forming the new theory of calcination. Lavoisier after having confirmed the same reaction experimentally drew a new abductive inference by modifying his initial hypothesis i.e. made his hypothesis more concrete, from 'Calxes are metals combined with another unknown material' to 'Calxes are metals combined with air'. He probably considered this new hypothesis as the 'simpler one', which 'rendered the observed facts necessary or highly probable'. The representation of the new abduction could be as follows:

Second Abduction
Calxes when heated without charcoal air is given off (result)
Calxes are metals combined with another unknown material (rule)
Unknown material combined in calxes might be air fixed in (hypothesis as conclusion)

In Peirce's schema, where C: 'Air (*S*) is given off when calxes (*P*) are heated' and A: 'Metals combined with unknown material (*M*) are calxes (*P*)'. Therefore: 'Air (*S*) might be unknown material combined in calxes (*M*)'.

Here the consequent is that 'heated calxes give air off' that is given in perception and the inference about the antecedent in a form of a hypothesis that entails C is that 'the unknown material combined in calxes might be air'. Due to this discovery Lavoisier *corrected his initial assumption* of 1772 (reduction of calxes with charcoal), because, when he had heated calxes with addition of charcoal, the charcoal was consumed. It followed, for Lavoisier, that the air given off in the reduction was not a simple substance, but a combination of at least two, and he concluded correctly that it was 'fixed air' (CO_2 as explained with the chemical representation of the calx of lead).

The prevalent notion at this time was that loss of phlogiston and absorption of air in combustion and calcination were at the same time. Lavoisier considering probably other criteria of admissibility of hypotheses i.e. 'simpler and most readily verified hypothesis' and 'adopt hypotheses that are in accord with the *pragmatic maxim'* – since presence of air could be verified while of phlogiston not – held that the 'unknown material' combined in calxes must have been atmospheric air. The subjective character of abduction is apparent here, because Priestley and Lavoisier interpreted the same data in different way: Priestley tried to explain his discovery in the context of phlogiston theory; therefore he failed to understand the nature of the new entity. However, Lavoisier by following other criteria of appraisal came closer to the true interpretation of this discovery. The deductive predictions of his second hypothesis would have been, as follows:

Deduction

All the members of class *B* (atmospheric air) have observable characteristics *a* (respiration) and *b* (burning) (rule)

Observed event *A* may belong to class *B* (hypothesis as case)

Event *A* should have observable characteristics *a* (respiration) and *b* (burning) (practical consequence as conclusion)

The observable characteristics of atmospheric air according to the deductive consequences of that hypothesis was that air contributed to (a) respiration and (b) burning; therefore the air that was given off from calx of mercury had to be tested in respiration and burning. Qualitative induction would have also proposed as more valuable qualities of the class *B*, respiration and burning. This shows us that both deductive predictions of the hypothesis and qualitative induction contribute to good *predesignation*, as I pointed out in the first part of the study.

Inductive experimental testing in respiration and burning accomplished by Priestley first and then by Lavoisier in 1775 showed that air given off from calx of mercury (HgO), when it was tested in respiration[393] and burning, contributed better than atmospheric air; therefore it had not properties of the 'fixed air' (CO_2), but, rather, it had most of the properties of common atmospheric air. After this inductive testing Lavoisier concluded that the unknown entity was 'air in a more pure form', as he considered this hypothesis as 'simpler' and as the one that 'rendered the observed facts necessary or highly probable'. On the other hand, Priestley held that it was pure, because, as being 'dephlogisticated air', it was air free from phlogiston.

[393] These experiments on respiration, conducted first by Priestley and then Lavoisier, shows the connection of chemistry with medicine, as it is also emphasized correctly by Kim (2003).

The modification of Lavoisier's second hypothesis after its inductive testing and by 'correcting the premises of abductive inference' in a new abduction, could be represented as follows:

Third Abduction
Unknown material combined in calxes contributes to respiration and burning *better* than atmospheric air (result)
Atmospheric air contributes to respiration and burning (rule)
Unknown material might be atmospheric air in *higher purified form* (corrected hypothesis)

Thagard, in order to illustrate that his '*rule abduction*' can better explain Lavoisier's reasoning, as source of discovery[394], proposes the following schema:

Puzzling evidence G(a) is to be explained, i.e. why is G
Rule (x) (Fx→Gx), i.e. all F are G, would explain G(a)
So may be F(a), i.e. a might be F

In my opinion, this schema does not show the recurring series of *abduction-deduction-induction* that were required to modify the initial hypothesis gradually until to reach the final one. Because, for Lavoisier, this F(a) was first 'atmospheric air', then 'pure air' and in the end 'purest part of the atmospheric air', as I am going to show next. Although Thagard holds that conceptual change and discovery require links to intermediate concepts, he does not show how his '*rule abduction*' can achieve that. But, Peirce's repeated series of *abduction-deduction-induction* in concert can explain the gradual classification of the unknown entity first to 'air', then to 'pure air' and finally to 'purest part of air', by 'correcting the premises of abductive inference gradually'.

To return to my interpretation, the deductive consequence of Lavoisier's third hypothesis was that atmospheric air might be compound of at least two components and probably heterogeneous; whose one part should be this unknown material. Priestley, who although was an adherent of the phlogiston theory, he examined experimentally this consequence and found in 1775 that one fifth of atmospheric air consists of this air, which he named

[394] Thagard in (1993), pp. 52-55, argues that his *rule abduction* being a 'generalization from abduced hypotheses rather than from observed instances' can explain better Lavoisier's inference, since, as he claims, Peirce's schema of abduction 'does not look like a possible source of discoveries'.

'dephlogisticated air', because, for him, it was air that has given off phlogiston. Therefore Priestley concluded that atmospheric air was composed of nitrous air, earth and phlogiston.

However, for Lavoisier, the explanation of this experimental testing was that 'this unknown material was pure form of air'; therefore he examined the deductive consequences of this hypothesis in another way, as I said, by decomposing atmospheric air. He *predesignated* his experiments in this way, so as first to take the atmospheric air in pieces and then to put it together again. First, he deprived the atmospheric air of his supposed 'pure air' (oxygen) through the calcination of mercury. The remained air (nowadays nitrogen), he found, would not support life and combustion, as it asphyxiated animals and extinguished flames. Then he reduced by heating the mercury calx and recovered the same quantity of the 'pure air' that had been combined in the calx. After having added this recovered air to the remained asphyxiating residue from the calcination, he obtained air with the properties of the original common air. This restored air no longer extinguished flames but supported also respiration. As I initially said, this was the *third planned crucial experiment*, conducted by Lavoisier, since it refuted the Aristotelian view about the composition of air, whereupon was based the explanation by the phlogistonists of the increase in weight holding that phlogiston was lighter than common air, and Priestley's view on the composition of air.

By taking into consideration Peirce's criterion Nr. 6, i.e. 'break the hypothesis into its smallest components and then derive the practical consequences of each one, so as to synthesize all the components into one broad hypothesis' and Nr.7, i.e. 'choose hypotheses that are broad and inclusive', we can explain the reversibility of the crucial reaction of air that Lavoisier applied. Because, if atmospheric air was compound of at least two and probably heterogeneous substances, whose one part was this unknown pure part, then he had to decompose the atmospheric air and to examine the properties of each component with regard to respiration and burning (properties *a* and *b*). Then after having put them together again, he had to examine the properties of the new synthesized compound in comparison with atmospheric air. On the other hand, if one assumes that a substance contains A and B as constituents, it does not imply that A and B are the only constituents. As I have argued, the confirmation can be obtained only by reversing the reaction, and this is what Lavoisier actually did with the reaction of the air.

This last verification led him to reaching the final corrected hypothesis, i.e. this ‘pure air’ was the ‘purest part of the air’, which he named oxygen, as follows probably:

Fourth Abduction

Unknown material combined in calxes is the only component of the atmospheric air that contributes to respiration and burning (result)

Atmospheric air contributes to respiration and burning (rule)

Unknown material combined in calxes is the purest component of atmospheric air (corrected hypothesis as conclusion)

However, Lavoisier’s theory even though had more explanatory power, as it could explain combustion, calcination and gain in weight on calcinations, it could not be accepted until experimental tests by the phlogistonists Priestley and Cavendish or Watt proved the composition of water. In other words, ‘inflammable air’ (hydrogen: air produced when acids react with metals) and ‘dephlgisticated air’ formed pure water equal in weight to the weights of these two gases, according to the nowadays reaction:

$$2H_2 + O_2 \rightarrow 2H_2O$$

The phlogistic chemists, probably, tried with this reaction to refute Lavoisier’s theory by drawing the experimental consequences of the combustion of ‘inflammable air’ and by examining its product. Although this discovery led to new version of the phlogiston theory presented by Cavendish, Lavoisier concluded that water was not a simple substance, but it must have been composed of inflammable air and oxygen, just like air, a few years ago, it was shown to be a compound. In order to verify this hypothesis Lavoisier applied once again the reversibility of the crucial reaction of water, i.e. decomposition of water, after taking into account the same factors again. Because, though his theory could explain combustion and calcination (even of hydrogen now), it could not refute the new revised theory of phlogiston concerning the solution of a metal in an acid with the formation of a salt and the evolution of inflammable air (reaction 4, as explained).

In my view, this was the *fourth planned crucial experiment*, since it refuted Cavendish’s new version of the phlogiston theory with the reaction (4), which explained the solution of metal in acid. For that reason after the reaction of decomposition of water, which Lavoisier demonstrated indirectly and was crucial for the validity of the metal-acid reaction, posed by the phlogistonists, his theory began to be accepted by the scientific community. And finally, after the electrolysis of water, which was conducted by Carlisle and Nicholson in 1800 and falsified directly Cavendish's theory, Lavoisier's theory was established within the scientific community, since it proved also the existence of oxygen as a referring entity (oxygen), while the rival theory was based upon the entity 'phlogiston' that did not refer to any existing kind, as the latter was never isolated. As for the crucial discovery of carbon monoxide in the same year, it was also significant, since it falsified Kirwan's final version of the phlogiston theory.

After this reconstruction of the Chemical Revolution we can draw some conclusions about the contribution of the different aspects of the SCT (self-corrective sorts of inference, progress of knowledge, criteria of admissibility of the hypotheses, scientists' skill, fallibilism and continuity of knowledge in the scientific community) to the discovery of the new entity, the adoption of the new theory and its establishment.

4.3.2. Sorts of inference, scientists' skill and discovery

First, we can conclude that the process up to the discovery of the new entity was continuous and it consisted in gradual and corrective steps after each series of abductive inferences, deductive predictions and inductive testing. The initial hypothesis was modified many times after the inductive phase of inquiry by correcting the premises of abduction, according to Peirce's schema '*induction corrects its premises'*. Moreover, the new modified hypothesis that was drawn after the inductive phase was chosen rationally by some criteria of appraisal (admissibility of the hypotheses), however that choice, though rational, was subjective and left room for rational judgment and scientists' skill. Furthermore, the deductive predictions of each hypothesis had been also examined and contributed to designing the experiments of its verification or falsification. Finally, crucial experiments played an important role for the falsification of Lavoisier's first false version and of the different versions of the phlogiston theory in the different stages of the inquiry.

To be more precise, Lavoisier's first hypothesis was that 'calxes were metals combined with another unknown material', and that this material was 'probably air', while, for the phlogistonists, it was either particles of fire or dephlogisticated air. Lavoisier probably chose this hypothesis, as explained, after taking into account the criteria Nr. 1 and Nr. 5, which Peirce cites. The deductive consequence of this first hypothesis was, for him, 'if increase in weight was produced by air, it must come from inside the vessel', therefore he predesignated the experiment that he had to conduct, in order to verify this conjecture, i.e. Boyle's experiment with sealed vessel. This experimental testing, first, verified his hypothesis indirectly, as it falsified Boyle's notion, *but the real confirmation came from Priestley's experiment with calx of mercury*, which was designed by following another deductive consequence of the same hypothesis. The latter, for Lavoisier, made clearer the nature of the unknown material. Therefore Lavoisier after those inductive tests modified his initial hypothesis, which means he made it more concrete i.e. 'calxes were metals combined with air' or else 'unknown material combined in calxes might be air fixed in'.

After the new corrected hypothesis Priestley once again examined its deductive consequences, so to say, behavior of the new gas in respiration and in burning. Lavoisier followed him again, but his new revised hypothesis after the inductive test was different than Priestley's, i.e. 'Unknown material might be atmospheric air in higher purified form'. As we can realize, scientists' skill contributed to the progress of inquiry, but this skill consisted in different aspects of application to each phase of inquiry. Lavoisier's skill was better in the abductive and deductive phase, namely in formulating hypotheses based on experimental data, and examining their logical consequences, while Priestley's skill was better in the deductive and inductive one, as he could design and conduct the experiments that would follow from the truth of a hypothesis[395].

Priestley, like Cavendish, following the Baconian inductive tradition, although was suspicious of too much theory[396] he was very skilful in experimental chemistry. Because, as I have shown, drew two times the experimental consequences of Lavoisier's and his own hypothesis and designed the relevant experiments, once with heating calx of mercury and once with testing the air given off from calx of mercury in respiration and burning. As for the hypothesis that 'atmospheric air might be compound', he examined also its consequences, as he intended to prove that one part of the atmospheric air was 'dephlogisticated air. These events illustrate that the contribution of the scientist's skill within the community as a whole was crucial for the advancement of inquiry, i.e. from the

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[395] Gillispie Charles in (1960), p. 218, also argues: *'Chemistry profited, therefore, from the curious, the almost symbiotic relationship between Priestley and Lavoisier, however unwelcome to both. If Priestley's lack of theoretical taste disqualified him from understanding his discoveries, Lavoisier's lucidity disqualified him from making them'.*

[396] See Beretta (1993), p. 231.

one side the British inductive experimental tradition that provided many observational data and from the other side Lavoisier's theoretical skill that embedded these data in a coherent theory.

The deductive consequence of this last hypothesis, as explained, were that 'atmospheric air might be compound of at least two components', therefore Lavoisier designed his experimental test with more skill this time (1777), that is to say, to decompose the atmospheric air and to examine the properties of each component with the converse reaction. *This last inductive verification was the one that led Lavoisier to reaching the final modified hypothesis that this 'pure air' was the 'purest part of the air', which was the unknown material combined in calxes and which he named oxygen*.

As for reversibility of reaction, this was the great innovation, which was introduced for the first time in experimental technique by Lavoisier, and it was also used by him in the crucial reaction of water. Here we can see the contribution of scientist's skill clearly, because, as I said in the relevant chapter, after each series of abduction-deduction and induction the scientist becomes more qualified to select better hypotheses and to design better experimental tests (*predesignation*), for his experience and skill are increased.

Concerning the self-corrective character of the scientific inferences, we have seen that the three forms of inference involved a distinct leading principle that contributed to the self-correction and, in addition, they were, though irreducible, complementary to one another, since it was their use in concert that could lead to the correction of errors and to the approximate representation of the observed regularity in the long run. First, each abductive inference was formed logically and not by 'divine intuition', as it was based on the empirical data that were given in perception and led Lavoisier to inferring a conclusion about the antecedent by taking into account the different criteria of admissibility.

Moreover, Peirce's schema of abduction applied to Lavoisier's reasoning could provide an explanation for the discovery of the new entity (oxygen) therefore it has shown us how 'all new ideas in science come by abduction' (CP 5.145. 1903). Because the unknown entity was first classified to the known class 'air', then to the sub-class of air i.e. 'pure air', and finally to its sub-class 'purest part of air', which was identified and Lavoisier named it oxygen. This means that that the process of discovery was gradual and with links to other intermediate known concepts (air, pure air, purest part of air), as a result of recurring classifications, but not with Gestalt switches. On the other hand, although the experimental discoveries of substances such as inflammable and dephlogisticated air by the phlogistonists were the outcome of induction, their correct identification and classification were the outcome of abduction.

Second, abduction being the weakest form of inference, it was required to be drawn the deductive consequences of the hypotheses, which would follow from their truth. The latter contributed to the experimental predesignation of the inquiry, and it was accomplished with skill, first by the phlogistonists and then by Lavoisier. Third, the positive or negative inductive verification of each hypothesis following its deductive consequences led to gradual and recurring modifications of the initial hypothesis by correcting the premises of abduction, according to Peirce's schema '*induction corrects its premises*'. This process enabled Lavoisier to characterize his explanatory hypothesis: as in need of modification in the first phase ('calxes are metals combined with another unknown material), as partially proved in the second phase ('unknown material combined in calxes might be air fixed in') and in the third phase ('unknown material might be atmospheric air in higher purified form'), and as proved in the fourth phase ('unknown material is the purest part of air'). Finally, the choice of the new modified hypothesis closer to the representation of regularity laid in the increase of both the background knowledge and the skill of the inquirers, as well as, in the consideration of some criteria of admissibility of the hypotheses.

The whole process formed a dialectical unity, where each form of inference was complementary to another and contributed to the correction of the others. From this point of view, Chemical Revolution was a continuous process, which consisted in gradual and corrective steps after each series of abductive inference, deductive predictions and inductive testing. Therefore it allowed the scientists to correct their false hypotheses in a finite number of applications and to find an alternative one closer to the representation of natural phenomena than the previous ones. It was carried out by the scientific community of that time and led to the discovery of new concepts, to radical re-classification of the known chemical substances and to the establishment of new stable beliefs in a finitely long run, as I argued in the first part of the study, which represented approximately the certain regularity in nature (i.e. oxidation). All these aspects are the ones that constitute the self-corrective character of scientific method, as they permit us to make progress in science and allow us our knowledge to grow.

4.3.3. Criteria of admissibility

With regard now to the criteria of admissibility of the hypotheses that Lavoisier probably took into account in the previous mentioned corrective steps, they can be summarized as follows:

1st step (first abduction): ‘choose hypotheses that may render the observed facts necessary or highly probable’ (Peirce’s Nr. 1), and ‘not attach too much importance to antecedent likelihood of a hypothesis’ (Peirce’s Nr. 5).

2nd step (correction of Lavoisier’s assumption): ‘choose hypotheses that may render the observed facts necessary or highly probable’ (Peirce’s Nr. 1), and ‘break the hypothesis into its smallest components and then derive the practical consequences of each one’ (Peirce’s Nr. 6).

3rd step (second abduction): ‘choose hypotheses that may render the observed facts necessary or highly probable’ (Peirce’s Nr. 1), ‘choose simpler hypotheses’ (Peirce’s Nr. 3), ‘choose most readily verified hypothesis’ and ‘adopt hypotheses that are in accord with the *pragmatic maxim’* (Peirce’s Nr. 2, 4 and 9).

4th step (third and fourth abduction): ‘choose hypotheses that may render the observed facts necessary or highly probable’ (Peirce’s Nr. 1) and ‘choose simpler hypotheses’ (Peirce’s Nr. 3).

5th step (decomposition of air and water): ‘break the hypothesis into its smallest components and then derive the practical consequences of each one, so as to synthesize all the components into one broad hypothesis’ (Peirce’s Nr. 6) and ‘choose hypotheses that are broad and inclusive’ (Peirce’s Nr. 7).

6th step: ‘adopt a hypothesis, which leaves open the greatest field of possibility’ (Peirce’s Nr. 8). As for this criterion, Lavoisier considered it to form his theory of acidity (oxygen as acidity principle), which although was proved to be false, it gave rise to further inquiry afterwards.

We can infer from the above mentioned criteria of appraisal, that the criterion of simplicity (Peirce’s Nr. 3) posed by the conventionalists, it is true that it was important, but it was not the only one that was crucial. Moreover, we can conclude that Lavoisier took into consideration different criteria of appraisal in different phases of inquiry and sometimes used the same criterion two or three times without any regularity. Therefore, in my opinion, these criteria cannot be connected by algorithmic way with appropriate weights, but, rather, they can be used as maxims, since they are subjective and leave room for personal judgment and as a result for skill in abduction. This is another aspect, which shows the need of cooperative and inter-subjective interpretation of the different maxims by the inquirers[397]; therefore the diversity of different notions and the use of different criteria for the evaluation of hypotheses within the scientific community have a great share in the progress of inquiry. But this should not be understood as a denial of rationality – since skillful judgment in abduction is rational – or as absence of a scientific method or even of a set of methodological criteria; because there can be *rational disagreement* between scientists, who accept conflicting theories on the basis of different methodological considerations[398].

4.3.4. Formulation of the new theory

As I previously pointed out, the theory that explained combustion and calcination, which was based on the supposed involvement of the gases, their classification and their casual relations, followed many steps of development after many corrections. It began first with experiments and inductive generalizations, which were provided by the Baconian and Lockean empiricist tradition of the British pneumatic chemists (from Boyle, Mayow, Black and Hales up to Cavendish, Priestley and Kirwan). For them, the description of the gases (conceptual schemes) and their interrelation was the outcome of the age-long traditional induction, namely it followed the positivist view, i.e. propositions as descriptions or inductive generalizations derived from and proved by experiments. This sort of induction is distinguished from Peirce's conception that sees induction only as a generalization of the results after the experimental testing of a hypothesis. Although Peirce, like Hume, considers

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[397] Hilary Putnam in (1995), *Pragmatism and the Contemporary Discussion*, pp. 70-1, emphasizes also the cooperative and intersubjective character of scientific inquiry concerning the interpretation of the different maxims, as follows: '*For the pragmatists, the model a group of inquirers trying to produce good ideas and trying to test them to see which ones have value… According to the pragmatists, whether the subject be science or ethics, what we have are maxims and not algorithms; and maxims themselves require contextual interpretation… The problem of subjectivity and intersubjectivity was in the mind of pragmatists from the beginning… They insisted that when one human being in isolation tries to interpret even the best maxims for himself … then the kind of certainty that results is in practice fatally tainted with subjectivity*'.

[398] For similar view see also Sankey (1997).

this traditional induction (crude induction) as of lesser importance, we have seen that it provided the necessary background knowledge for the abductive inference, since the description of terms such as ‘weight’, ‘combined’, ‘air’ etc., used in the first abductions, was the outcome of inductive generalizations; therefore it played a significant role in the formulation of Lavoisier’s first hypotheses. For that reason, in my opinion, we cannot reject crude induction as self-corrective sort of inference, because to ignore its contribution to the development of scientific theories is to commit the *all-or-nothing* fallacy.

Lavoisier’s theoretical conception unified afterwards all these inductive descriptions and factual propositions, and explained the causal relations of gases by a new coherent theory. This means, on the one hand, that observations and descriptions of gases, provided by Boyle up to Priestley, were based mainly on the traditional concept of induction, which was applied by the British pneumatic chemists. On the other hand, Lavoisier rendered all these data and embedded in a coherent theory[399]. Lavoisier’s conception of induction was in a Peircean sense, i.e. to confirm or disconfirm theoretical propositions, as he designed his experiments to test his hypotheses, while the British phlogistonists were seeking revelation and surprise through bare collection of more and more experimental facts. Therefore, this different approach marked also the different predesignation of experiments. The same is true with the use of instruments, as it was, for Lavoisier, to deliver data input or a numerical response to theoretical questions, while for the phlogistonists it was the means to provide more facts[400]. From this point of view, these two different conceptions of induction were both important for the advancement of inquiry.

Furthermore, since theory involves interpretation, and interpretation presupposes the use of the right language (in a sense that represents approximately the natural regularities), therefore the use of language is very significant for the formulation of each theory. Lavoisier along with Guyton actually framed the contingent empirical facts within a closed

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[399] Nordmann (1998) stresses the significance of Lavoisier’s theoretical conception, as follows: ‘*Lavoisier required that theory is confronted with the natural phenomena, and Lavoisier's modern chemistry is theoretical chemistry*’, and: ‘*his (Lavoisier’s) construal of chemical problems constitutes a community of researchers engaged in a discourse of contingency, engaged in the production of representations of natural phenomena, representations that take their form from Lavoisier's methods and axioms and that are thus decidable simply by their agreement or disagreement with the facts*’.

[400] See Priestley’s description of his method, as cited by Nordmann (1998): *‘If we could content ourselves with the bare knowledge of new facts, and suspend judgment with respect to their causes, till ... we were led to the discovery of more facts, of a similar nature, we should be in a much surer way to the attainment of real knowledge’.*

system of language and developed a system of pragmatic nomenclature in which the names of substances reflected theoretical suppositions concerning their composition and efficacy[401]. Thus, as Nordmann argues[402], the production of representations of natural phenomena through linguistic representations 'take their form from Lavoisier's methods and axioms, and are decidable simply by their agreement or disagreement with the facts'. Therefore it allowed 'consensus among scientists, since a consensus requires the agreement between those pictures of phenomena'. Beretta maintains that Lavoisier, as being influenced by the naturalist philosophy of Condillac, held that by the use of language we would be able to acquire knowledge[403]; therefore he proceeded to establish the new chemical nomenclature by his *Méthode*.

Peirce also emphasizes the importance of the use of suitable nomenclature in science, whose every term has a single definite meaning universally accepted by scientists and he cites especially the chemical terminology, as an ideal example of the use of language with fixed meanings[404]. In this sense, since language, is not private, but socially developed and with terms universally accepted[405], it allows scientists to reach a consensus. Apart from that as Lavoisier pointed out, there is an interconnection between facts, ideas that represent them and language that expresses these ideas, and an improvement of knowledge about the one factor leads to the improvement of the others[406]. Therefore, in my view, the

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401 See Morveau et al. (1787).

402 See Nordmann (1998).

403 Beretta (1993), pp. 187-206, analyzes the influence of Condillac on Lavoisier's ideas about language. On p. 187, he cites Lavoisier's memoir from the *Méthode*: *'Languages are intended, not only to express by signs, as it is commonly supposed, the ideas and the images of the mind; but are also analytical methods, by means of which, we advance from the known to the unknown'*. See also Bernadette Bensaude-Vincent (2008).

404 Peirce in (CP 5.413. 1905) relates the norms of terminology with the *moral principle*, which involves mutual obligations, as follows: *'he who introduces a new conception is under an obligation to invent acceptable terms to express it, and that when he has done so, the duty of his fellow-students is to accept those terms, and to resent any wresting of them from their original meanings … and furthermore, that once a conception has been supplied with suitable and sufficient words for its expression, no other technical terms denoting the same things, considered in the same relations, should be countenanced'*.

405 Peirce in (CP 5.421. 1905) argues: *'all thought whatsoever is a sign, and is mostly of the nature of language'*, therefore he concludes that the approximate representation of reality could be only agreed upon by the community of inquirers by the use of language, which includes universally accepted terms. Wittgenstein (2001), §256-§271, also argues with his '*private language argument*' that the idea of a language understandable by only a single individual is incoherent.

406 Lavoisier (1788), xiv-xv, as cited by Beretta (1993), p. 261, points out the interconnection as follows: *'The impossibility of separating the nomenclature of science from the science itself, is owing to this, that every branch of physical science must consist of three things; the series of facts, which are the objects of the science, the ideas which represent these facts, and the words by which these ideas are expressed… And as ideas are preserved and communicated by means of words, it necessarily follows that we cannot improve the*

introduction and establishment of the new chemical nomenclature, which was expressed by signs, was a significant factor for the formulation of the new theory, and for the symbolism and classification of the chemical elements, as well as for the chemical formula notation later by Dalton, Berzelius and Mendeleev[407].

Another significant factor that contributed to the formulation of Lavoisier's new theory was the falsification of the different versions of the phlogiston theory by *crucial experiments*, i.e. Boyle's and Aristotelian view on the increase in weight, Priestley's view on the composition of air and Cavendish's version of metal acid reaction. Certainly, the deactivation of these versions of the phlogiston theory did not imply automatically the activation and acceptance of Lavoisier's theory, if the latter could not provide unified explanation of all these phenomena that the refuted theory explained.

Moreover, as I said, scientists in the Chemical Revolution, on the basis of incomplete or indirect information, adopted some imaginary theoretical entities, i.e. 'phlogiston' by the phlogistic chemists, 'oxygen' by Lavoisier, which might not refer to any actual kind or substance. But in the course of inquiry, as long as new evidential data were gathered, the description of those entities was corrected and those that were imaginary and did not refer, they were rejected. This happened with the *crucial experiment of decomposition of water* that proved the existence of oxygen, as a referring entity, and disproved the existence of 'phlogiston', since it did not refer to any existing kind, therefore it could not be isolated.

In addition to the rejection of the phlogiston theory through falsification by crucial experiments, Lavoisier's theory had some normative characteristics of superiority (in Lakatos' terminology), as follows: it had more explanatory power, it exceeded its application to a larger set of cases, it had no anomalies concerning oxidation, it had the balance of evidence in its favor (excess in empirical content), it extended its application to acidity and pneumatic chemistry and its theoretical growth anticipated its empirical growth. Therefore it could provide a unified explanation of combustion, calcination, decomposition of air and water, and metal acid reactions, as well as it began to explore the principle of acidity.

language of any science without at the same time improving the science itself; neither can we, on the other hand improve a science, without improving a language or nomenclature, which belongs to it'.

[407] See Partington (1989).

Concerning the epistemological features of Lavoisier's thought, Mc Evoy[408] emphasizes the Lockean empiricist epistemology of Lavoisier. Beretta[409] argues that Lavoisier's epistemology was more Cartesian than Lockean, since he formulated theories on the basis of observed phenomena, while Priestley's epistemology was traditional Baconian. Lavoisier, for Beretta, being influenced by Condillac, followed his analytical method, inspired by Descartes method. This method, for Lavoisier, consisted of: 1. Collect the facts, 2. Break them down into simple parts and 3. Resemble them as they were originally. However, Jessica Riskin (2002) argues that Condillac's and subsequently Lavoisier's epistemology was Lockean. Elisabeth Ströker [410] (1982) argues that his method involved initial formulation of hypotheses, observations of facts and conclusions, which were clearly distinguished; therefore she claims that this was a feature of 'analytical' method of the empirical sciences[411].

We have to admit that the introduction of a new language of chemistry with Lavoisier's *Méthode*, which was expressed by signs and represented the ideas and the regularities without any suppression[412], show the influence of Condillac's epistemology and pragmatic philosophy of language, as Lavoisier himself admitted[413]. But, in my opinion, Lavoisier's so-called 'analytical' epistemological approach and method, is nothing but a combination of the Cartesian Hypothetico-Deductive method (HD) with the traditional British induction (Baconian and Lockean empiricism), for the following reasons: First, Lavoisier although recognized the importance of Descartes mathematical thought and method[414], he admitted

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408 See McEvoy and McGuire (1975).

409 See Beretta (1993), pp. 187-206.

410 Jessica Riskin (2002), p. 237, by citing Condillac's works holds: '*Condillac named John Locke as his chief inspiration. However Condillac's (and subsequently Lavoisier's) Lockean epistemology had been given a sharp interpretive twist, a twist that transformed the function of the language and of a social convention, in natural science'*.

411 Ströker E. (1982), p. 220, argues: *'Lavoisiers Versuchsprotkolle sind von früh an Musterbeispiele präzise Unterscheidung von Ausgangsfragestellung, Tatsachenbeobachtung und Schlußfolgerung. Dank seine ausgeprägten Unterscheidungsfähigkeit von Bedeutungsaussage und Hypothese, Tatsachenkonstatierung und Gesetzesbehauptung wußte er das wichtigste analytisch-methodische Mittel der empirischen Wissenschaft…'*

412 See Lavoisier (1787), translation (1788), pp. 9-10: '*The perfection of the chemical nomenclature … consists in rendering the ideas and facts in their exact truth without suppressing anything, or making any addition whatsoever; it should be nothing less than a faithful mirror'*.

413 Ibid, p. 5: '*An analytical method is a language; a language is an analytical method … This is a fact that has been explained with infinite exactness and perspicuity in the Logic of the abbé de Condillac, a work which can never be too much studied by the youth that dedicate themselves to the sciences, and from which we cannot avoid borrowing a few ideas'*.

414 See Lavoisier's statement about Descartes thought, as translated by Albury (1972), p. 132: '*No author, no professor of chemistry has seen science from this point of view'*.

that a purely Cartesian approach to Chemistry was impracticable[415]. Second, the advance '*from the known to the unknown'*, based on collection of observations, which Lavoisier also claimed, as we have seen, is rather a feature that involves inductive generalizations, as I argued, and abductively drawn hypotheses based on those data. Third, taking into consideration the previous mentioned criteria of theories' appraisal that Lavoisier might took into account and comparing with the three aspects that Beretta points out, we can realize that Peirce's criteria Nr. 6 and 7 include the second and the third step, which Beretta presents as Lavoisier's method, while the first one is similar to Peirce's criterion Nr. 1.

We can infer from the previous analysis of the theory formulation that Lavoisier's epistemology combined the Cartesian Hypothetico-Deductive method (HD) with the traditional British induction; therefore I argued that we can explain it with Peirce's conception, which includes all three inferences, i.e. abduction and deduction from the Cartesian Hypothetico-Deductive method (HD) and induction in the two forms. First, abduction, based on the empirical data that were given in perception, allowed Lavoisier to inferring a conclusion in a form of hypothesis about the antecedent, as we have seen with his four abductions, which led to three recurring classifications of the unknown entity. Second, deduction, in a sense of predicting and drawing the consequences of each hypothesis, contributed to examining its coherence and to designing its experimental verification. And third, induction, both in a form of inductive generalizations of the British chemists, as background knowledge that had a share in the formulation of the first hypotheses, and in a Peircean form that contributed to the experimental testing of the different hypotheses.

4.3.5. Scientific Community and Fallibilism

Taking into account this reconstruction, we can realize how the different aspects of the scientific community contributed to the progress of scientific inquiry in the Chemical Revolution. First, the contribution of the scientists' skill to the distinct phases of inquiry within the scientific community was different, as explained, with Lavoisier's skill mostly in abduction and deduction, and the phlogistonists' skill in deduction and induction. This showed that the progress of scientific inquiry was served better by diversity of different

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[415] See Lavoisier's statement, Ibid, p. 130: *'The order which I believed is that of geometry, but modified and corrupted relative to the state of imperfection which chemistry is in… and which necessitates another procedure'.*

notions and traditions of scientific research within the scientific community, i.e. on the one hand, British inductive experimental tradition provided many observational data, and on the other hand, Lavoisier's theoretical conception accommodated all these data gradually to a coherent theory. It also means that the different and inter-subjective interpretation of the criteria for the evaluation of theories (maxims) within the scientific community has a great share in the progress of scientific inquiry. Apart from that, it shows the significance and irreducibility of the different forms of inference; because, it was Priestley and Cavendish's experimental induction, first, that led to the discovery of inflammable and dephlogisticated air. And then these discoveries allowed Lavoisier to formulate abductively his hypotheses, to draw deductively their logical consequences, and led him to discovering a new theory of calcination and combustion.

Second, the competition of rival theories in a pluralist scientific community contributed much to the discovery of oxygen, because of the discoveries of the dephlogisticated and inflammable air by the phlogistonists, as well as of the water composition. Certainly this competition does not work, if rival theories do not have a common basis for comparison and discussion, i.e. when they have different standards of evidence and argument. But as we have seen, both rival theories did not use different standards of evidence and argument, but, rather, they used the same method, in other words, observed experimental facts, explaining theories that rendered them and experimental testing of their consequences or predictions. Apart from that, competition of different opinions within a pluralist scientific community constituted an inner dynamic that forced adherents of the rival theories to attribute more weight to the anomalies that their theory could not explain.

Third, the finitely long run application by the scientific community proved the existence of oxygen, as a referring entity, and disproved both the existence of 'phlogiston' that did not refer to any existing kind and of Lavoisier's theory of acidity. As I argued in the first part of the study, for Peirce, false beliefs about regularities although in the short run are proved to be correct, they resign in the face of experience in the finitely long run of application, i.e. as long as new evidential data come together; because inductive verification is based on a limited number of samples and can be taken incorrectly as lawlike evidence.

Fourth, the background knowledge was gained and increased continuously within the scientific community, as explained, from Boyle, Mayow, Black and Hale's experiments up to Priestley, Cavendish, Watt, Kirwan and Lavoisier's notions. Indeed, all these aspects of contribution presuppose an autonomous and democratic scientific community. As we have seen, except some idealizations by both sides and problems of priority in scientific discovery (e.g. Water Controversy), there were not significant problems of autonomy at this time, due to the structure of the scientific community in the end of the 18th century. But after the end of the 19th century, when states and private institutions financed certain scientific projects, arose a real problem of autonomy with serious consequences[416].

Fifth, connection with the established canons of knowledge outside the domain of local inquiry and within the scientific community, i.e. connection with Atomism, the growing Newton's gravitational theory and medicine – as we have seen with the experiments on respiration – was very important for the progress of inquiry. This aspect, in my opinion, is very significant, since it gave rise later to the promotion of the interdisciplinary work within the scientific community. Certainly, this interdisciplinary work did not lead to unified explanation of the natural phenomena by one single and true theory (e.g. Newton's mechanic), as in Peirce's conception of convergence upon truth; therefore in the first part of the study I pointed out the problems of this notion. However, it contributed to the discovery of a new theory that represented approximately the regularity of oxidation.

Concerning the reaction of scientific community on the new theory, as explained, it began with rejection of Lavoisier's early notions; however after 1775 it started to be accepted gradually, while after 1784 up to 1800 the recognition of the new theory was more radical. The question that may arise here is whether this behavior of scientific community was rational or no. By applying Popper's notion of corroboration, we can say that after 1775 up to 1800 Lavoisier's theory survived many severe tests of rationality and experiments, while all the versions of the phlogiston theory have been falsified by crucial experiments. As we have seen, the members of scientific community continued rationally, in my view, to adhere to phlogiston theory long after 1775 up to 1800, because of the facts that the decomposition of water had not been directly verified until the discovery of its electrolysis and the anomaly of carbon monoxide had not been cleared up until 1800. Therefore the last version of the phlogiston theory was still accepted until the end of 18th century, but afterwards the new theory was accepted, rationally in my opinion, by the whole of scientific community.

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[416] For more about this problem, see Böhme Gernot et al. (1976), *Finalization in Science*, in Stehr Nico, Grundmann Reiner (2005), pp. 302-26. Compare also with Cohen B. I. (1985).

Moreover, if we compare the two rival theories after these two discoveries with reference to some rational criteria of appraisal, as posed by Lakatos, we can infer that Lavoisier's theory of oxidation was superior than phlogiston, because: (a) it had *more explanatory power*, as it could explain combustion, calcination, decomposition of air and water and solution of a metal in an acid, (b) it had *no anomalies* concerning oxidation, (c) it had the balance of evidence in its favor (*excess in empirical conten*t), (d) it *extended its application* to larger set of cases, i.e. acidity and pneumatic chemistry, (e) its *theoretical growth anticipated its empirical growth*, as it could embed all the experimental data in a coherent theory, and (f) it was *based upon a referring entity* (oxygen), while the rival theory was based upon the entity 'phlogiston' that did not refer to any existing kind. The last criterion is not specified by Lakatos, but, as we have seen, it was important for the acceptance of the new theory after 1800. As for the hypothetical entity of caloric, this was auxiliary hypothesis, but not a fundamental entity of Lavoisier's theory. Taking into consideration all these factors, we can infer that the scientific community, though the idealizations presented by the one or the other side, finally behaved rationally and abandoned gradually the phlogiston theory in favor of Lavoisier's theory.

With regard to fallibilism of knowledge, we have seen that both theories were revised and corrected many times, so as to cohere with new experimental data. This led scientists to abandoning the entities 'phlogiston' and 'caloric', to adopting the new entity 'oxygen', and to developing further the theory of acidity. Moreover, although it was reached the limit of inquiry, carried out by the scientific community of that time concerning the discovery and definition of that particular new entity 'oxygen', the meaning (mechanism) of 'oxidation' has not been fully exhausted until today. Because, due to synechism and fallibilism, as Peirce says, the meaning of universal dispositions, as regularities in nature (*Thirdness*), cannot be exhausted in a finite number of observations. Indeed, as we know today, oxidation involves transfer of electrons between metals and oxygen that enters the field of particle physics and chemistry; therefore it has not been yet fully defined, since it is related to uncertainty principle and wave functions.

4.3.6. Conclusion of the reconstruction

In sum, with this reconstruction I have shown that Peirce's account can explain the historical events and it is plausible account of the Chemical Revolution. Peirce's methodology was able to explain the method and the sorts of inference that led to the discovery of the new entity, to the formulation of the new theory, to the conceptual change, as well as it could explain the contribution of the scientific community to the continuity of knowledge and to the Chemical Revolution. Furthermore, I have provided historical evidence for the self-corrective character of the scientific method, which was applied in this event by the scientific community, as long as it worked autonomous. The process of inquiry in the Chemical Revolution was self-corrective and continuous, and it consisted of gradually corrected hypotheses after each series of abductive inferences, deductive predictions and inductive testing. Therefore it allowed the scientists to correct their false theories and to find an alternative theory closer to the approximate representation of the natural regularities.

As for the technique to find better alternative hypotheses than the refuted one, I have illustrated that to the modification of the hypotheses, by correcting the premises of abductive inference, contributed both the scientists’ skill and the consideration of some normative criteria of admissibility of the hypotheses. These criteria, as I concluded, cannot be connected by algorithmic way with appropriate weights, but they rather can be used as maxims, since they are subjective and leave room for personal judgment and skill in abduction. This conclusion speaks in favor of the need of diversity of competing opinions within a pluralist scientific community, where each scientist tries different criteria for the evaluation of each theory. Especially with the problem of confounding factors that produce similar data – where experimental verification could be taken as confirmation of a hypothesis, while concerns the background assumption – as posed in the first part of the study, I have indicated how the inquirer can solve this problem by the use of the criteria of admissibility, i.e. to design experiments by excluding or including each time one factor, so as to find out the contribution of each one to the observed event. I have also provided evidence of the contribution of both the scientists’ skill and the deductive predictions of the hypotheses to better predesignation of their experimental testing.

Moreover, fallibilism of knowledge was clearly present in and after the Chemical Revolution, since the non-referring entity ‘phlogiston’ was abandoned, the theory of acidity after the discovery of oxygen was further developed and it proved the falsity of Lavoisier’s theory, while the theory of oxidation was further enriched and developed; because the finitely long run application by the scientific community proved the falsity of the non-referring entity 'phlogiston' and of Lavoisier's theory of acidity. Furthermore, the introduction of the new chemical nomenclature, which was expressed by signs, was also a significant factor and had a great share in the formulation of the new theory. Apart from that, it gave rise later to further development of the symbolical language of chemistry as a form of representation.

However, although Peirce's account could explain all these events, as I found out through this historical reconstruction, it has some omissions or weaknesses; therefore some new views have to be integrated into Peirce's conception of the SCT. First, traditional induction, posed by the *positivists*' account, played an important role for the continuity of background knowledge (e.g. Mayow, Hales' notions), and, although the new ideas were introduced by abduction, induction had a great share in the formulation of Lavoisier's first hypotheses, as abstract generalizations from the available empirical data. Since abduction is deduced from the data that are given in perception, it involves abstract generalizations of these data; therefore it is based partly upon crude induction, while for Peirce crude induction is of lesser importance. Apart from that, Peirce's conception sees induction important only for the experimental testing of a hypothesis. As I argued in the first part of the study, the ability to recognize universal qualities or classes we have acquired by inductive generalization and after having perceived many members of the class. Furthermore, classification of the particular event to one of the general classes in abduction presupposes the mental act of comparison between the particular event and the class of events. From this point of view, the description of terms such as 'weight', 'combined', 'air' etc., and their classification, used in the first abductions, were the outcome of a continuous process of inductive generalizations, provided by the Baconian and Lockean empiricist tradition of the British pneumatic chemists, and which served as a basis for the beginning of inquiry; for it would have been impossible to formulate any new hypothesis without the use of terms from old theories, even if the latter were non-referring terms. This is another aspect that shows also the continuity of knowledge within the scientific community and its contribution to the progress of inquiry.

But this contribution of induction does not underestimate the important function of abduction, which is the one that leads to the inference about causal consequent-antecedent relation and to the classification of entities, and as result, it contributes to the introduction of new entities in scientific inquiry. This process we have seen with the introduction of oxygen, which followed, many classifications, i.e. first to ‘air’, then to the sub-class of air i.e. ‘pure air’, and finally to its sub-class ‘purest part of air’. As I have argued, due to the existence of *Thirdness,* as a causal principle between two entities under certain conditions, it can be justified the consequent-antecedent relation, and this in turn allows us to infer from the known consequent about the unknown antecedent. Apart from that, due to the reality of natural regularities, we can systemize the particulars in general laws. Furthermore, the existence of universal qualities, as dispositions of things of the same class to behave in a certain way under certain conditions, though conditional and relative to our actions (Peircean *‘would be’s’),* it permits us, apart from classifying an event, to draw the deductive consequences or predictions of a hypothesis. This happened, as we have seen, with the predictions of Lavoisier’s first and second hypothesis, whose results led to the modification of his hypotheses and to the identification of the ‘unknown material combined in calxes’ first with ‘air’ and then with ‘pure air’.

Second, crucial experiments that falsified the early notions of Lavoisier and the different versions of the phlogiston theory, pointed out by the *falsificationists’* account, had a great share in the formulation of the final theory in the Chemical Revolution, because they opened the way to the correction of Lavoisier’s hypotheses and proved the existence of oxygen, as a referring entity. Although Peirce recognizes the importance of falsification or verification of the hypotheses, he does not specify the weight of each one; because, as I said, after Duhem’s challenge, falsification of the hypotheses is considered to be more reliable than their verification. These experiments were: (1) heating of calxes of lead and tin in a sealed vessel, conducted by Lavoisier, which falsified Boyle’s theory about the increase in weight, (2) the reduction of the calx of mercury without charcoal, conducted by Priestley, which falsified the first false version of Lavoisier’s theory, (3) the decomposition of air, conducted by Lavoisier, which falsified Priestley’s theory and the Aristotelian view of composition of air, and (4) the decomposition of water, conducted first by Lavoisier in 1784 and then by Carlisle and Nicholson in 1800, which falsified Cavendish’s last version of the phlogiston theory and proved the existence of oxygen as a referring entity. Furthermore, the principle of corroboration through severe tests of rationality and crucial experiments contributed much to the acceptance of Lavoisier’s theory by the scientific community.

Third, the crucial aspects of the structure of the scientific community and the interdisciplinary work, posed by the sociology of science and emphasized by both *Kuhn* and *social constructivists,* showed their importance for securing the self-corrective mechanisms of the scientific method. This notion speaks in favor of the need of exploring the necessary conditions and institutional characteristics of the scientific community, which can secure its independent, autonomous, democratic and inquiring role, since Peirce conception of the scientific community is very ideal. Because, as we have seen, the progress in the Chemical Revolution was served better by diversity of competing opinions within a pluralist scientific community, since it constituted an inner dynamic that led: (1) to more qualitative scientific work, as it forced the scientists of rival theories to attribute more weight to the anomalies and the solved problems, (2) to the interaction of different notions, methods and traditions of scientific research, (3) to the increase of the scientists' skill and of the background knowledge for the adherents of both theories, and (4) to connections of theories outside the domain of local inquiry (i.e. Newtonian mechanic, Atomism, medicine), which contributed to interdisciplinary work. All the previous mentioned factors had a great share in the progress of inquiry and finally in the discovery. As for the interdisciplinary work, though led to the formulation of the new theory, did not lead to unified explanation of the natural phenomena by one single and true theory (e.g. Newton's mechanic), but, rather, led to the approximate representation of a certain chemical phenomenon - i.e. the regularity in question was oxidation - an aspect that verified my critique of Peirce's conception of convergence upon truth in the first part of the study.

Fourth, some normative criteria of rationality and progressiveness, as posed first by the falsificationists and developed further by *Lakatos,* contributed to the corroboration of the new theory and to its acceptance by the scientific community. Peirce apart from the criteria of admissibility of the hypotheses, he does not define any criteria of rationality and progressiveness of the theories. These epistemic or cognitive values of theories, which also characterized Lavoisier's theory, are as follows: theories with more explanatory power, with no anomalies, with excess in empirical content, and extension of their application to larger set of cases, as well as theoretical growth that anticipate their empirical growth.

I can infer from all the above mentioned that although Peirce's conception of the SCT provided some necessary conditions for the self-corrective character of the scientific method, it has some omissions and weaknesses in explaining the historical data; therefore there is a need for integrating some other notions, which, as being the outcome of the development of the modern philosophy of science, might secure its self-corrective mechanisms better. This means, first, that inductive generalizations contribute much to the formation of hypotheses, second, that crucial experiments are significant for the formulation of the final theory, third, that normative criteria of rationality and severe tests are important for the corroboration and the acceptance of a theory by the scientific community, and finally, that the structure and the institutional characteristics of the scientific community are significant for its autonomous and inquiring role. This integration, in my view, is in the Peircean sense of the hypotheses' testing and of its fallibilism, therefore it does not violate the Peircean conception of the SCT, but it rather develops it further and improves it.

5.1. Summary

The heart of this project was the justification of the self-corrective character of scientific method; therefore I investigated Charles S. Peirce's Self-Corrective Thesis and its application. Self-Corrective Thesis **(SCT)** is based on the idea that the growth of knowledge and the progress in science lie in its self-corrective method. This view is controversial in the history and philosophy of science up to this day; for that reason in the first part of this paper I explored Peirce's scientific methodology and discussed it in comparison with the objections against it, so as to defend it and distinguish the context of its validity. In the second part I appealed to the historical case of the Chemical Revolution, in order to examine this episode in the perspective of Peirce's methodological theory, to test and evaluate the different aspects of Peirce's SCT, and to show the self-corrective character of the scientific method applied in the Chemical Revolution. While, in case of discovering any omissions of Peirce's account, I had to integrate some new views into the Peircean conception of the SCT, which might secure the self-corrective mechanisms of scientific method better.

Scientific method, according to Peirce, consists of abduction that leads to the formation of hypotheses, deduction that draws conditional consequences and predictions of the hypotheses, and induction that tests them experimentally. It begins with some fundamental beliefs adopted by the individual, as a result of his experience and background knowledge. It is distinguished from other methods of inquiry, because, though it is fallible, it is self-corrective in nature, for it allows us, if it persisted in long enough, to correct our errors by gradual modification of our hypotheses. Furthermore, it is sufficient for the temporal cessation of doubt and the establishment of new beliefs that cohere with experience, while, when it is applied *in the long run* by a community of inquirers could lead to the establishment of true beliefs, or else beliefs that represent approximately the reality of natural laws, as regularities in nature. All the above mentioned aspects, for Peirce, constitute the self-corrective character of scientific method.

To provide enough background required for grasping Peirce's account of the self-corrective nature of scientific inference, I explored his phenomenological and epistemological notions: his new Categories (*Firstness, Secondness,* and *Thirdness*), his pragmatic maxim and his theory of signs. Afterwards I proceeded to explain Peirce's method of inquiry, which is based upon these epistemological and phenomenological notions. To give an account of his proposed scientific methodology, I analyzed Peirce's Logic of scientific method, as a triadic unity (abduction, deduction and induction), his probabilism, fallibilism and the contribution of scientific community to the process of scientific inquiry. Then I went on with the justification of the SCT and I explored through Peirce's works the development of his Self-Corrective Thesis **(SCT),** the initial Peirce's notions and their evaluations that followed the development of his thought, as well as his arguments for its justification.

I discussed next this justification in comparison with the objections raised by the philosophers of science. Critics were classified into different groups: Those who reject the self-corrective character of Peirce's scientific method, those who regard the self-corrective character of abduction as being without logical justification, those who maintain that from all inferences only induction is proved to be self-corrective, and those who argue that there is no deductive justification for the self-corrective character of induction. In order to defend the SCT I argued: first, that each method involves a distinct leading principle that contributes to the self-correction, second, that the three forms of scientific inference are irreducible, and third, that the whole unity forms a dialectical and gradual process, which in the long run can lead to the correction of errors and the growth of knowledge. Moreover, I pointed out that critics' mistakes lie in the dissociation of the three sorts of inference from one another, or even in the underestimation of one of them, while according to Peirce's conception of the SCT, it is only when they are closely interlinked and complementary to one another that they can contribute to self-correction.

Therefore I explained the distinct application of abduction, deduction and induction to each phase of inquiry. Abduction begins with a surprising fact (discovery) and turns to hypothesis that aims both at explaining an unexpected event and classifying it to a known class of events. I showed that abduction, though not deductive, is logical, since it is based on the empirical data that are given in perception and are contained in the premises of the abductive inference; therefore we can infer a conclusion about the antecedent. Apart from that, it is based on the reality of universal dispositions or habits (aspect of *Thirdness),* which are working in nature in a form of true regularities or laws.

Furthermore, in this phase Peirce suggests some normative criteria of admissibility that help the inquirer to evaluate his hypotheses. Such criteria are: 'render the observed facts necessary or highly probable' (today's *inference to the best explanation*), 'simpler' hypotheses, abided by Ockham's razor, 'most readily refuted or verified hypotheses', 'those who leave open the greatest field of possibility' (fertility criterion), 'economical in money, time and energy', 'not attach too much importance to antecedent likelihood of a hypothesis' (proliferation principle), 'break the hypothesis into its smallest components and find broad and inclusive ones' (hypotheses with the more explanatory power), 'hypotheses that do not bear verifiable effects have to be excluded' (*pragmatic maxim*), namely their premises (not the conclusions) are capable of being tested (today's predictability of hypotheses). But, since abduction is the weakest of all three sorts of inference, it has to be followed by the deductive phase of inquiry, which I explained.

In the deductive phase, once a hypothesis is adopted, have to be examined the conditional experimental consequences or predictions, which would follow from its truth. Since the inquirer has classified the unexpected observed event *A* to a general class of events *B,* and the members of the class *B* have the observable characteristics *a, b, c,* etc., then according to deduction the event *A* should have the same observable characteristics. As the whole process is based on deduction, it is self-corrective, because deduction can correct its conclusion, when the premises are true, as well as vice versa. This phase is very significant, because it implies the meaning of the hypothesis by demonstrating its various necessary consequences, that is, determines the pragmatic meaning of the hypothesis; therefore deduction helps the inquirer to design the experiments required for testing each adopted hypothesis.

Afterwards I proceeded to show the inductive phase of inquiry, which is the experimental testing of the hypothesis, i.e. the inquirer tests the observable characteristics of the event *a, b, c,* etc. and compares it with observed characteristics of the class. This is the method that Peirce calls *quantitative induction*, which is based on calculation of the relative frequency, namely, the number of times that the hypothesis has predicted successfully *(m)* as related to the total number of times *(n)* that the conditions of the prediction were fulfilled, ($f = m/n$). Quantitative induction with statistical syllogisms and direct inference serve as the testing grounds for hypotheses. Very important in this phase of inquiry is to name and to evaluate the importance of the various qualities of the class under investigation, and this estimation

Peirce calls *qualitative induction*. For Peirce, both qualitative and quantitative induction can lead to probable conclusions and affect a closer convergence on true conclusions, but they never attain full certainty due to their probabilistic and fallible character.

Following the inductive phase and according to the testing results, the inquirer is able to characterize his explanatory hypothesis as: proved, partially proved, in need of modification, falsified and so forth. I argued that in any case, positive or negative inductive verification contributes to the forward progress of scientific inquiry by closing off useless avenues previously open, by pointing to future modified hypotheses after new abductions, and by increasing both the background knowledge and the skill of the inquirer, which serve as a basis for forming more accurate, revised hypotheses with more truth-content. Therefore I concluded that all the three forms of inference form a dialectical unity, where each one is complementary to another and contributes to the correction of the others.

As for the reducibility of the three types of inference, I emphasized their crucial differences. *Deduction*, as a typical inference of formal logic, is *analytic* and *necessary true*, since it is the application of the rule, while both *induction* and *abduction* are *synthetic* sorts of inference, as they are based on experience. But in Peirce's conception, deduction can be used for tracing out the necessary consequences of a hypothesis. Induction is a generalization from a number of cases of which something is true and inference that the same thing is true for the whole class, while abduction is where we find a curious event, which would be explained by the supposition that it was a case of a certain rule. In induction we synthesize to find the rule, while in abduction we synthesize to classify the case under a class of events. Induction cannot yield any hypothesis about the causes of the phenomena nor can introduce new entities.

Another crucial difference between abduction and induction concerns the unobserved facts, for induction infers the existence of phenomena such as we have observed, while by abduction we conclude the existence of a fact, which would be impossible for us to observe directly (unobservables). The consequence of this difference is that inductive inferences can be directly verified, while abductive inferences can only be indirectly verified; therefore abduction is the only inference that introduces new entities and can lead to discoveries.

With regard to fallibilism and its relation to the inquiry within the scientific community, I pointed out the followings: Peirce holds that knowledge *'cannot be absolutely exact, universal or absolute certain'*, therefore the best interpretation of the 'real' that we can achieve is an approximate one. Since scientific inquiry consists of abductive inference, it is fallible and subjective, as it depends on the inquirer's skill that lays in his background knowledge (personal set of beliefs). Furthermore, the inductive testing process that generalizes on the basis of a limited number of samples and is supposed to represent the whole class enters the region of the probable and uncertain.

On the other hand, granted Peirce's *synechism,* we cannot fully specify the properties of the individuals, for they are rather symbols of their kinds (universals), and the interpretation of these universal *signs* cannot be fully exhausted in a finite number of observations, due to their infinite potentiality. Therefore, all the above mentioned factors, lead to fallibilism of scientific knowledge, which says that every belief is subject to review, confirmation, correction or rejection by subsequent belief. To overcome all these problems, Peirce introduces the social aspect of scientific knowledge open to public verification through the indefinite inquiry by the scientific community, which contributes to self-correction and progress in scientific inquiry, and in the long run can lead to the fixing of beliefs that cohere with experience and represent approximately the reality of regularities in nature.

In the end of the first part I concluded that Peirce provided an account for the self-corrective character of scientific inquiry for single disciplines, but he failed to give an account for his notion of convergence upon truth, since the latter presupposes, apart from the unification of sciences, some other questionable premises. His notion of scientific community was also very ideal, as he did not examine its institutional features, which might secure the autonomous and inquiring role. However, he showed that scientific method can gradually approach the approximate representation of the regularities in nature and allows us to systemize the particulars in general laws. He gave an account for the justification of abduction through his notion of *Thirdness*, but he did not develop the account of the self-corrective character of abduction enough and especially the technique to find better alternative hypotheses. But with his view 'induction corrects its premises' implied a way of modifying the hypotheses by correcting the premises of abductive inference, that is, by returning to the minor premise of abduction and modify it. This in association with his proposed 'economy of research' (criteria of admissibility of the hypotheses) and the development of scientific skill through scientific community provided a technique for drawing better hypotheses.

In the second part of the study I explored the scientific methodology followed in the Chemical Revolution for the construction of the oxygen theory, so as my conclusions of the self-corrective character of scientific method from the first part to be tested, supported or corrected, and if necessary, to integrate some new views into the Peircean conception. Following the method of Lavoisier I drew some conclusions about his scientific methodology, which I discussed in comparison with my conclusions of Peirce's scientific methodology from the first part of the study, so as to explore the self-corrective character of the scientific method applied in the Chemical Revolution and to evaluate the different aspects of Peirce's SCT.

I have chosen to present historical data from different bibliographical sources expounded by historians of different schools of thought that concerned certain historical significant episodes. In case of some controversial presentations, I discussed them by taking into account their plausibility, the historical and social background, and the original works of the protagonists of the story, so as to evaluate them. The choice of these episodes was with reference to some criteria, which in turn enabled me to draw my conclusions. Such criteria were the different aspects of the SCT (self-corrective sorts of inference, criteria of admissibility of the hypotheses, scientists' skill, fallibilism and scientific community). In this historical part I exhibited all the four stages of Lavoisier's advancement, i.e. from his first hypothesis that 'air might be absorbed in calcinations and combustion' up to his theory of oxidation.

Afterwards I discussed the interpretations given by the majority of the existing different methodological theories in comparison with my own interpretation of Peirce's proposed scientific methodological theory. I argued that Peirce's methodological theory could provide a rational reconstruction of this episode, and that it was consistent with the historical data; therefore it could be plausible account of this well-known episode. The discussion included the following accounts given by: Positivist, postpositivist, conventionalist, falsificationist, Kuhn, Lakatos, constructivist or sociologist of science, in comparison with Peirce's scientific methodology and with reference to the above mentioned criteria. Its aim was to test and evaluate the different aspects of Peirce’s SCT, and when necessary to revise the Peircean conception.

After having presented and discussed the different accounts of the Chemical Revolution, I concluded that some aspects of the previous discussed accounts were problematic, while some others introduced crucial points and perspectives, which were important for the self-corrective character of scientific method, applied in the Chemical Revolution, and showed some omissions of the Peircean account. Therefore many questions were still left open, as follows:

First, the characterizations by the positivists of Lavoisier’s method as scientific and phlogiston theory as unscientific were unsuccessful, since both sides used similar methods, inferences and apparatus. But Perrin’s notion against Gestalt switch and in favor of continuity of knowledge had to be explored further. Second, the criterion of simplicity alone, posed by the conventionalists, did not suffice for explaining the progress of inquiry; therefore had to be explored the different criteria of appraisal of the two theories and their relation to rationality. Third, the crucial experiments that falsified different versions of Lavoisier’s and phlogiston theory, as pointed out by the falsificationists, were not indicated clearly, i.e. which experiments were considered crucial and why. For that reason the question was subject to further inquiry. Fourth, although Kuhn’s account showed the radical conceptual change that occurred in the Chemical Revolution, the sorts of inferences that led to that had to be explored, as well as their relation to the continuity of knowledge. Fifth, the crucial aspect of the scientific community posed by both Kuhn and social constructivists showed the importance of its dynamic, but the contribution of its different features to the progress of inquiry had to be examined. Sixth, it had to be explored, whether the normative criteria of progressiveness and rationality, proposed by Lakatos, characterized Lavoisier’s theory, and how they contributed to the self-corrective character of scientific method.

And seventh, the question, whether the scientific method applied in the Chemical Revolution was self-corrective, had not been answered. In order to explore further these questions, I proceeded then through my reconstruction to explain the scientific method followed in the Chemical Revolution from Peirce’s methodological point of view. I exhibited the self-corrective character of the scientific method and argued that Peirce’s conception of the SCT could answer the questions left open.

This interpretation enabled me also to evaluate the different aspects of Peirce’s SCT and to provide evidence of how the ‘scientists’ skill’ contributed to correcting and finding better hypotheses. I showed how the scientific community even in the Chemical Revolution approached the limit of inquiry and the approximate representation of the natural phenomena, gradually and continuously, concerning a certain and particular discipline. After having represented the successive series of abductive inferences, deductive predictions and inductive testing, which might have taken place in the Chemical Revolution, I concluded that this historical episode was a continuous process, which consisted of gradual and corrective steps. The whole process formed a dialectical unity, where each form of inference was complementary to another and contributed to the correction of the others. On the other hand, I stressed the importance of each form of inference for the progress of inquiry, i.e. I exhibited that the discovery arose from both induction and abduction, and that deduction contributed both to formulating coherent theories and to drawing experimental consequences of these theories.

I showed that the inquiry was carried out by the scientific community of that time and led to the discovery of new concepts, to radical re-classification of the known chemical substances and to the establishment of new stable beliefs that represented approximately the regularities in nature (e.g. oxidation). As for the technique to find better alternative hypotheses than the refuted one, I maintained that to the modification of the hypotheses by correcting the premises of abductive inference contributed both the scientists’ skill and the consideration of some normative criteria of admissibility of the hypotheses, which left room for personal judgment and skill. Moreover, I emphasized that fallibilism of knowledge was clearly present in and after the Chemical Revolution, since the theory of acidity after the discovery of oxygen was further developed and proved the falsity of Lavoisier’s theory, while the theory of oxidation was further enriched and developed.

Finally, I concluded that all these different aspects of the SCT present in the Chemical Revolution were the ones that allowed scientists to correct their false theories in a finitely long run and to find an alternative theory closer to the representation of natural regularities than the previous ones. With this reconstruction, I provided evidence for the self-corrective character of scientific method, and I evaluated the different aspects of the SCT by showing the distinct contribution of each one to the progress of knowledge.

From the reconstruction I also inferred that although Peirce’s conception of the SCT provided some necessary conditions for the self-corrective character of the scientific method, it had to integrate some other notions, which, as being the outcome of the development of the modern philosophy of science, might secure its self-corrective mechanisms better. This means: First, that inductive generalizations, posed by the positivists, have a great share in the formulation of the initial hypotheses, second, that crucial experiments, proposed by the falsificationists, are significant for the formulation of the final theory, third, that normative criteria of rationality and severe tests, proposed first by Popper and then by Lakatos, are important for the acceptance of a theory by the scientific community. And finally, that the structure and the institutional characteristics of the scientific community itself, posed by Kuhn and the social constructivists, are significant for securing its autonomous and inquiring role.

Since the Chemical Revolution is one particular episode in the history of science, the study of this episode could not lead to generalization about the whole history of science, as its aim was to test, evaluate and if necessary to improve Peirce’s SCT. Other studies could explore the different revolutionary episodes in the history of science and examine the self-corrective character of scientific method applied, and perhaps discover some additional aspects of the SCT. To carry out this project is a topic for future work.

5.2. Conclusion of the study

To return to the initial question, which was the heart of my investigation, i.e. whether scientific method is self-corrective, I can infer from the previous investigation that Peirce's conception of the SCT provides the necessary conditions for the self-corrective character of the scientific method. However, as I argued in the first part of the study, SCT is valid for single disciplines, since it can gradually approach the approximate representation of regularities in nature with which each particular discipline is busied. This view, which I initially posed, was also verified by the research conducted in the second part of the study, because interdisciplinary work in the Chemical Revolution did not lead to unified explanation of the natural phenomena by one single and true theory (e.g. Newton's mechanic), but it rather led to the explanation of the certain chemical regularity, i.e. oxidation. Therefore Peirce's notion of convergence upon truth is problematic, because it presupposes, apart from the unification of sciences, some other questionable premises, as explained.

Furthermore, this study has shown that according to Peirce's conception scientific method consists of three sorts of inference: abduction that leads to the formation of hypotheses, deduction that draws conditional consequences and predictions of the hypotheses, and induction that tests them experimentally. Concerning the crucial aspects of the SCT (self-corrective sorts of inference, criteria of admissibility of the hypotheses, scientists' skill, scientific community and fallibilism) and the critique against their self-corrective character, I can come now the following conclusions:

Although the three forms of inference are irreducible, it is only when they are closely interlinked and complementary to one another that they can contribute to self-correction. Because the self-corrective character of the three forms lies both in their interconnection and in their distinct and self-corrective principle, which is as follows: First, abduction, as being the outcome of evolution of the human instinct, though weak sort of inference, is logical, because it is based on the data that are given in perception and on the reality of generals (universals), as conditional dispositions (aspect of *Thirdness)*; therefore it allows us to infer a conclusion in a form of hypothesis about the antecedent event, and as a matter of course to classify the surprising event. But since it is the weakest of all three sorts of inference it must follow the deductive phase of inquiry. Because the initial 'vague' hypotheses, which are formulated by abduction, serve as basis only from which one can begin, but in the course of inquiry they have to be replaced gradually by verified scientific hypotheses, which in turn are subject to further verification or falsification through observation and experiment.

Second, necessary deduction draws the experimental consequences and predictions of the hypotheses; therefore it helps the inquirer to examine the coherence of the abductively drawn hypotheses and design the experiments for their verification or falsification. Third, induction (qualitative and quantitative), which is the experimental testing of the hypotheses, though is not deductively justified, it enables the inquirers to make accurate probable inferences about frequencies and can lead to probable conclusions about the validity of the hypotheses.

The results of induction can lead to the verification or falsification or to the need of modification of the hypotheses. In the last two cases it is required a technique to find better alternative hypotheses than the refuted one. With the historical reconstruction I have shown that both the scientists' skill and the consideration of some normative criteria of appraisal of the hypotheses can contribute to their modification by correcting the premises of abductive inference. But though these criteria of admissibility are rational, they cannot be connected by algorithmic way, but, rather, they can be used as maxims, since they leave room for personal judgment and skill. In particular with the problem of confounding factors that produce similar data, which is related with underdetermination, as posed in the first part of the study – when experimental verification could be taken as confirmation of a hypothesis, while concerns the auxiliary hypothesis – I have indicated how the inquirer can solve this problem. I concluded that in similar cases it is required to 'break the hypothesis into its smallest components and then derive the practical consequences of each one', i.e. to design experiments by excluding or including each time one factor, so as to find out the contribution of each one to the observed event.

Moreover, although fallibilism of knowledge implies that every theory is subject to correction or rejection by subsequent, it has been proved not to be in contradiction with self-corrective character of scientific method. Because, as I have explained by the reconstruction of the Chemical Revolution, both theories were corrected many times, non-referring entities were abandoned (phlogiston, caloric), while referring ones were adopted (oxygen). The finitely long run application of the scientific method proved the existence of the entity ‘oxygen’, and disproved both the existence of the entity ‘phlogiston’, which did not refer to any existing kind, and Lavoisier’s theory of acidity. For, as I argued in the first of the study, false beliefs about regularities although in the short run are proved to be correct, they resign in the face of experience in a finitely long run, as long as new evidential data come together. In addition, the theory of acidity was further enriched and developed, and, as for the theory of oxidation, although it has been adopted and developed, it has not been yet exhausted.

Finally, I concluded that all these different aspects of the SCT present in the Chemical Revolution were the ones that allowed scientists to correct their false theories in the finitely long run and to find an alternative true theory closer to the representation of natural regularities than the previous ones. But, as I also initially argued, the new true theory has to be defined pragmatically, i.e. as an approximate representation and within certain borders of experience, which in the new theory are more extended than in the previous one. This conclusion supports the view that science has the capability to refute a false theory, because of its self-corrective method, which involves all the previously explained features, but with some weaknesses.

To be more precise, although Peirce’s SCT provided some necessary conditions for the self-corrective character of the scientific method, my investigation in the second part of the study has shown that some new aspects have to be integrated into it, which might secure the self-corrective mechanisms of scientific method better.

First, it has shown the importance of inductive generalizations (crude induction) to the formulation of the first abductively drawn hypotheses, as posed by the positivists’ account. Although Peirce holds that crude induction is of lesser importance, as we have seen, it contributed as follows: First as background knowledge, which was gained continuously, and second, as a basis for the formulation of the first hypotheses, where all the terms used and represented classes (e.g. weight, combined etc.), were abstract generalizations from the available empirical data. Taking into account that abduction aims at classifying a particular event to one of the general classes – which means presupposes the knowledge of these general classes – and that our knowledge of these general classes is the outcome of abstract generalizations, we may infer, then, that abduction is based partly on these inductive generalizations. This aspect verified also my critique of Peirce's conception of abduction in the first part of the study.

However, this conclusion does not underestimate the significant function of abduction, which can lead to the inference about consequent-antecedent relation and to the introduction of new entities in scientific inquiry. First, due to the existence of *Thirdness*, as a causal principle of relation between two entities, it is justified the inference from the known to the unknown. Second, the existence of universal qualities, as dispositions of all things of the same class to behave in a certain way under certain conditions (Peircean *'would be's'),* it allows us to classify gradually particular events to one of the general classes. And third, after successive classifications by abduction, we may be able to introduce a new entity, as we have seen with the introduction of oxygen after successive classifications.

Second, it has shown that crucial experiments that falsify the different versions of rival theories, as proposed by the falsificationists, are significant for the formulation of a theory, since they are instructive with reference to the next hypothesis until the formulation of the final theory. Besides, it was the crucial experiments that proved the existence of oxygen, as a referring entity, and disproved the existence of 'phlogiston', which did not refer to any existing kind. In addition, the principle of corroboration through severe tests of rationality, as proposed by Popper, has a great share in the acceptance of a new theory by the scientific community. Third, some normative criteria of rationality, posed by Lakatos, are important for the acceptance of a theory and show the rational behavior of the scientific community. These criteria are, as follows: theories with more explanatory power, with no anomalies, with excess in empirical content, and extension of their application to larger set of cases, as well as theoretical growth that anticipate their empirical growth. And as I concluded, all of them were characteristics of Lavoisier's theory.

Fourth, the structure and institutional characteristics of the scientific community, as proposed by Kuhn, the social constructivists and the sociology of science in general, are very important for securing the self-corrective mechanisms of the scientific method. According to Peirce's conception, the social aspect of scientific knowledge, open to public verification through the investigation by the scientific community, contributes much to the self-correction and progress in scientific inquiry. And it can lead in a finitely long run to the fixing of beliefs that cohere with experience and represent approximately the natural regularities, with the conditions that the community is democratic and autonomous. But Peirce's conception of scientific community was very ideal, as it did not explore these conditions of the scientific community. As my reconstruction has shown, the diversity of competing opinions within a pluralist scientific community in the Chemical Revolution constituted an inner dynamic that led to more qualitative scientific work, to the interaction of different methods and traditions of scientific research, to the increase of scientists' skill and the background knowledge, and to connections of theories outside the domain of local inquiry. For that reason I concluded that have to be explored the structure and institutional characteristics of the scientific community, which can secure its independent, autonomous, democratic and inquiring role. As for the motivation for better qualitative results in scientific inquiry, I maintained that competition of different opinions within a pluralist and autonomous scientific community constitutes an inner dynamic that could provide a solution for this problem.

The evaluation of all the above mentioned aspects of the SCT advocates the distinct contribution of each one to the self-correction of scientific method, since all these aspects in association enable us to correct our false theories and to find alternative theories closer to the representation of regularities, and as a result they permit us to make progress in science and allow us our knowledge to grow. Certainly, as I argued, representation of regularities by theories presupposes a relation of correspondence between these two, since theories are formulated by our language; therefore, apart from the epistemological issues that were examined, semantic issues are also significant for this part of philosophy of science, which means, development of philosophy of language, models, signs and of other forms of representation. For, as I have shown, the introduction of the new chemical nomenclature in the Chemical Revolution was a significant part of that representation and later gave rise to further development of the symbolical language of chemistry.

All the previous proposed modifications of Peirce's SCT and the integrations of some new aspects into his conception are, in my view, in the Peircean sense of the theories' testing and of its fallibilism. Because, first, they are the outcome of the inductive testing of Peirce's SCT through the historical data, which means that by the use of Peirce's proposed scientific method, his conception was tested, in order to be examined, whether it was consistent with the historical data. Second, some new aspects of modern inquiry, unknown in Peirce's time were taken also into account. Third, Peirce's fallibilism implies that each view subject to review, confirmation, correction or rejection by subsequent view, as long as new data is gathered. Therefore, in my opinion, this modification does not violate the Peircean conception of the SCT, but it rather develops it further and improves it. Besides, with this study I have shown that my own revised interpretation of the SCT with the proposed modifications can stand against its modern critics, it is within the Peircean framework of scientific inquiry and is valid for single disciplines, as well as that it is consistent with the historical data of the Chemical Revolution.

However, as I pointed out, modern inquiry, e.g. in molecular chemistry and biology, particle physics, cosmology etc., deals with problems such as unobservable phenomena, underdetermination, theory-laden devices, lack of data and detecting devices etc. For all the above mentioned reasons it is apparent that scientific inquiry in these fields may call for some new forms of inference or some more normative criteria, so as to be able to overcome these difficulties and secure its self-corrective mechanisms; therefore scientific sorts of inference are still open and subject to further inquiry. Perhaps some new sorts of scientific inference and practice will arise from these disciplines, and scientists and philosophers of science working in this field of research might discover some new perspectives and dimensions of the self-corrective character of scientific method in the future. Although there has been some progress in the philosophy of science in the recent years concerning the epistemological issues, if we take into account these questions about language and model-building, reward system and structure of science, underdetermination and unobservables, we may conclude that there is still much to do in order to secure the self-corrective character of scientific method.

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Peirce and Self-Corrective Thesis

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Appendix 1

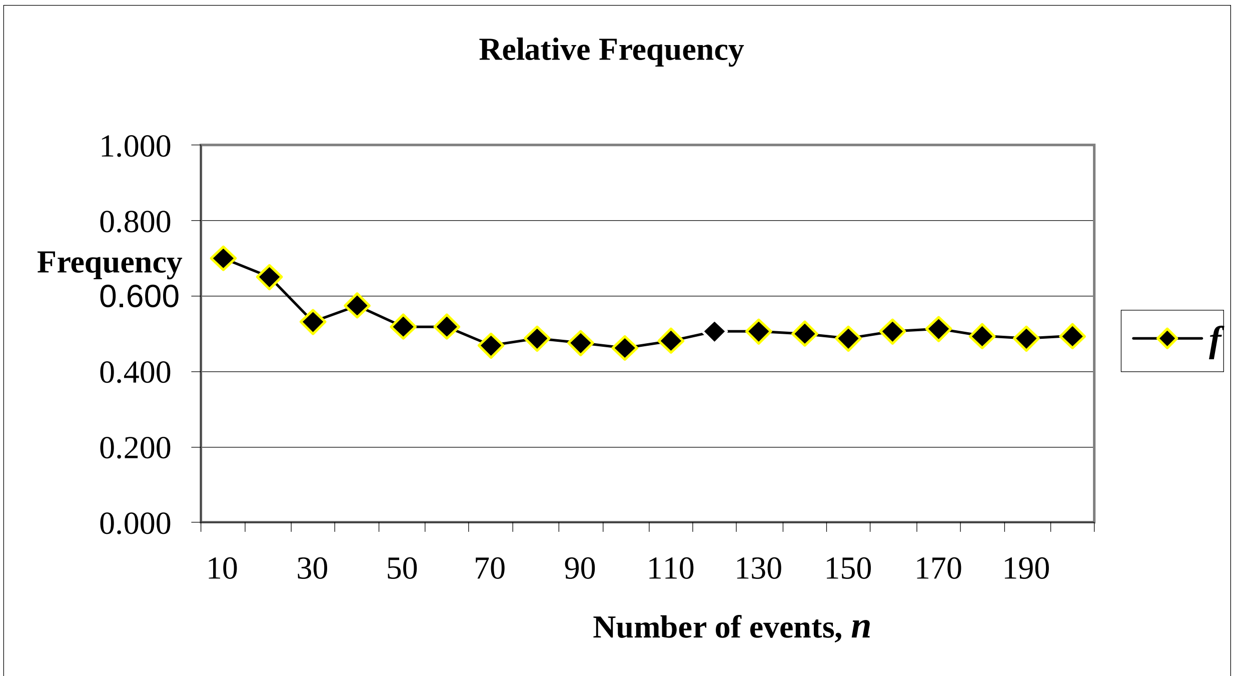
Relative frequency of events *A* that have the property *B*: $\boldsymbol{f_i (A,B) = m_i/n_i}$

$\boldsymbol{m}$ = observed events that have property $\boldsymbol{B}$ (e.g. tails), $\boldsymbol{n}$ = total number of events $\boldsymbol{A}$ (coin tosses)

$\boldsymbol{f_1 (A,B) = m_1/n_1 = 7/10,\ f_2 (A,B) = m_2/n_2 = 13/20,\ f_3 (A,B) = m_3/n_3 = 16/30, \ldots, f_n (A,B) = m_n/n_n}$

n	*m*	*f*
10	7	0.700
20	13	0.650
30	16	0.533
40	23	0.575
50	26	0.520
60	31	0.517
70	33	0.471
80	39	0.488
90	43	0.478
100	46	0.460
110	53	0.482
120	61	0.508
130	66	0.508
140	70	0.500
150	73	0.486
160	81	0.506
170	87	0.512
180	89	0.494
190	93	0.489
200	99	0.495

When $\boldsymbol{n \to \infty}$, *then* $\boldsymbol{\lim f_n(A,B) = 0.5}$



Index