Kuhn and Gombrich: Creativity, continuity and discontinuity in science and art

J. C. Pinto de Oliveira

Department of Philosophy, IFCH, State University of Campinas, Brazil

jcpinto@unicamp.br

Abstract

This paper discusses creativity, continuity and discontinuity in science and in art by drawing parallels between Kuhn and Gombrich. It seeks to show that, while the idea of strict cumulative progress in the history of science, as well as in the history of art, was abandoned as a new historiography emerged that was sensitive to ruptures, this does not imply denial of all continuity. However, continuity in this context is not a mere logical continuity, but rather a more complex theoretical and historical relation.

Keywords: Kuhn, Gombrich, history of science, history of art, creativity continuity, discontinuity.

1. Introduction

The question of creativity, continuity and discontinuity, which I intend to address here, is concisely expressed in Gauguin´s *boutade*, cited by Francis Haskell at the beginning of his article "Enemies of modern art": "the curious and mad public (...) demands of the painter the greatest possible originality and yet only accepts him when he calls to mind other painters".[1]

Along similar lines, Gombrich writes in *The Story of Art*:

> The general public has settled down to the notion that an artist is a fellow who should produce Art much in the way a bootmaker produces boots. By this they mean that he should produce the kind of paintings or sculptures they have seen labelled as Art before. One can

[1] Francis Haskell, *Past and Present in Art and Taste*, New Haven, Yale University,1987, p. 207.

> understand this vague demand, but, alas, it is the one job the artist cannot do. What has been done before presents no problem anymore. There is no task in it that could put the artist on his mettle. But critics and `highbrows', too, are sometimes guilty of a similar misunderstanding. They, too, tell the artist to produce Art; they, too, are inclined to think of pictures and statues as specimens for future museums. The only task they set the artist is that of creating `something new' - if they had their way, each work would represent a new style, a new 'ism'.[2]

I believe that this question of the reception of a work of art can be considered in parallel to the history of science, and Kuhn can be invoked here. The traditional historiography of science, according to Kuhn, values the individual scientist whose merits and professional recognition are determined essentially by the discoveries (or inventions) associated with his/her name. Kuhn criticizes this conception and proposes a "new historiography" based on the fact that the old one not only operates within a dualism of discovery-justification, but also fails to recognize a second fundamental type of scientific discovery, which is precisely the most creative and revolutionary type, for which it is much more difficult – or even impossible – to attribute credit to any given individual.

I seek to show, in this work, that Kuhn´s conception can be extended to art, given the parallels between the history of science and the history of art present in the very origin of Kuhnian philosophy of science. Or more precisely, due to the fact that the history of art – at the time he was criticizing the historiography of science for neglecting the change that corresponded to the second type of discovery – had already recognized this characteristic kind of change.

In section 2, I present briefly the distinction between the two types of discovery proposed by Kuhn. Section 3 explains the links between the history of science and the history of art (using Kuhn and Gombrich), and also establish a limit for the analogy: Kuhn and Gombrich agree with respect to the development of art but disagree about science. I conclude in section 4 by outlining an analysis of the issues raised and a few comments that I hope will contribute to shed some light on them.

---

[2] Ernst Gombrich, *The Story of Art*, London, Phaidon, 1995 [1950], p. 596.

2. Kuhn and the two types of scientific discovery

In well-known passages of Section I of *Structure*, Kuhn describes the traditional historiography of science in these terms:

> If science is the constellation of facts, theories, and methods collected in current texts, then scientists are the men who, successfully or not, have striven to contribute one or another element to that particular constellation. (…) And history of science becomes the discipline that chronicles both these successive increments and the obstacles that have inhibited their accumulation. Concerned with scientific development, the historian then appears to have two main tasks. On the one hand, he must determine by what man and at what point in time each contemporary scientific fact, law, and theory was discovered or invented. On the other, he must describe and explain the congeries of error, myth, and superstition that have inhibited the more rapid accumulation of the constituents of the modern science text. Much research has been directed to these ends, and some still is.[3]

However, Kuhn emphasizes the need for a reaction to this practice:

> In recent years, however, a few historians of science have been finding it more and more difficult to fulfill the functions that the concept of development-by-accumulation assigns to them. As chroniclers of an incremental process, they discover that additional research makes it harder, not easier, to answer questions like: When was oxygen discovered? Who first conceived of energy conservation? Increasingly, a few of them suspect that these are simply the wrong sorts of questions to ask.[4]

Thus, for Kuhn, the result of this reaction was "a historiographic revolution in the study of science". According to him:

> Gradually (…) historians of science have begun to ask new sorts of questions and to trace different, and often less than cumulative, developmental lines for the sciences. Rather than seeking the permanent contributions of an older science to our present vantage, they attempt to display the historical integrity of that science in its own time.[5]

---

[3] Thomas Kuhn, *The Structure of Scientific Revolutions*, Chicago, University of Chicago, 1970 [1962], pp. 1-2.
[4] Thomas Kuhn, *The Structure of Scientific Revolutions*, Chicago, University of Chicago, 1970 [1962], pp. 2-3.
[5] Thomas Kuhn, *The Structure of Scientific Revolutions*, Chicago, University of Chicago, 1970 [1962], p. 3.

This explanation is complemented by "The historical structure of scientific discovery" (1962). In this article, republished in *The Essential Tension*, Kuhn establishes a distinction between two types of discoveries, seeking to point out more precisely how difficulties in traditional historiography, to which he refers in Section I of *Structure*, emerge.

Kuhn saw no difficulty with the type of discovery, for example, of the neutrino, radio waves, or the missing elements in Mendeleev's periodic table. According to him, the existence of these objects "had been predicted from theory before they were discovered, and the men who made the discoveries therefore knew from the start what to look for".[6] In such cases, therefore, the practice of the "old" traditional historiography would be perfectly admissible and feasible.

Many scientific discoveries, however, "particularly the most interesting and important", as Kuhn stresses, are not of this type, and it would be inappropriate to ask when and where they occurred and who was responsible for them. Even if all the relevant information were available, he says, "those questions would not regularly possess answers". More complex discoveries of this type include oxygen, electric current, X-rays, and the electron, which according to Kuhn "could not be predicted from accepted theory in advance and which therefore caught the assembled profession by surprise". And further on he adds: "there is no single moment or day which the historian, however complete his data, can identify as the point at which the discovery was made. Often, when several individuals are involved, it is even *impossible* unequivocally to identify any one of them as the discoverer".[7]

A passage in *Structure* allows us to compare directly the two types of discoveries considered by Kuhn. He asks: "Why could not X-rays have been accepted as just one more form of a well-known class of natural phenomena? Why were they not, for example, received in the same way as the discovery of an additional chemical element?" His answer:

> New elements to fill empty places in the periodic table were still being sought and found in Roentgen's day. Their pursuit was a standard project for normal science, and success was an

[6] Thomas Kuhn, *The Essential Tension*, Chicago, University of Chicago, 1977, p. 167.
[7] Thomas Kuhn, *The Essential Tension*, Chicago, University of Chicago, 1977, p. 174, my emphasis.

> occasion only for congratulations, not for surprise. X-rays, however, were greeted not only with surprise but with shock. Lord Kelvin at first pronounced them an elaborate hoax. Others, though they could not doubt the evidence, were clearly staggered by it. Though X-rays were not prohibited by established theory, they violated deeply entrenched expectations.[8]

The discovery of a new element in the periodic table, for example, corresponded to a "standard project for normal science". Whereas for the second, more complex type, like the discovery of X-rays, despite being an accidental discovery, it could, in principle, induce a subversion of normal scientific practice, in the same way the discovery of a chemical element with unexpected characteristics could lead to an alteration in the periodic table. As Kuhn wrote:

> Previously completed work on normal projects would now have to be done again because earlier scientists had failed to recognize and control a relevant variable. X-rays, to be sure, opened up a new field and thus added to the potential domain of normal science. But they also, and this is now the more important point, changed fields that had already existed. In the process they denied previously paradigmatic types of instrumentation their right to that title.[9]

And it is worth emphasizing, with Kuhn, to complete the comparison between the two types of discoveries, that

> discovering a new sort of phenomenon is necessarily a complex event, one which involves recognizing both *that* something is and *what* it is (...) But if both observation and conceptualization, fact and assimilation to theory, are inseparably linked in discovery, then discovery is a process and must take time. Only when all the relevant conceptual categories are prepared in advance, in which case the phenomenon would not be of a new sort, can discovering *that* and discovering *what* occur effortlessly, together, and in an instant.[10]

Thus, according to this endogenous approach of Kuhn regarding the historiographical change, traditional historiography of science was in no condition to

---

[8] Thomas Kuhn, *The Structure of Scientific Revolutions*, Chicago, University of Chicago, 1970 [1962], pp. 58-59.
[9] Thomas Kuhn, *The Structure of Scientific Revolutions*, Chicago, University of Chicago, 1970 [1962], p. 59.
[10] Thomas Kuhn, *The Structure of Scientific Revolutions*, Chicago, University of Chicago, 1970 [1962], pp. 55-56.

respond to the difficulties presented by the second type of discovery in its context, or to be practiced according to the cumulativistic proposals that defined it. It sought to respond to two distinct types of discoveries in the same way, as though there were only one type. The change in the new historiography of science would therefore necessarily signify overcoming these difficulties or anomalies which emerged in the effective practice of the 'old historiography'.[11]

In this context, we can understand Kuhn's role in the new historiography of science, from his own perspective. To begin with, he deliberately avoids posing certain questions and seeking certain answers, as traditional historians did, including Koyré. Secondly, Kuhn admits, with Koyré, that his theory fills the void between internal and external histories.[12] It is not fitting here to specify what this fusion or this bridge would be, nor is it so important at this point in time, when a clear distinction between the genesis and justification of knowledge is no longer prescribed.

I limit myself to remembering that, in an interview published by Borradori[13], Kuhn goes so far as to say that he would perhaps classify *Structure* as a work in the sociology of knowledge. He certainly emphasizes the importance of studying scientific communities as producers and legitimaters of knowledge, with their psychological, sociological, and historical differences. For him, scientific knowledge "is intrinsically *a group* product" and "neither its peculiar efficacy nor the manner in which it develops will be understood without reference to the special nature of the groups that produce it". In this sense, says Kuhn, his work has been deeply sociological, however contradicting the venerable distinction between discovery-justification, "not in a way that permits that subject to be separated from epistemology".[14]

---

[11] See my "Kuhn and the genesis of the new historiography of science", *Studies in History and Philosophy of Science*, 43, 2012.
[12] See Thomas Kuhn, *The Road since Structure*, Chicago, University of Chicago, 2000, p. 286. On the historiography of science, see my papers: "Kuhn and the genesis of the new historiography of science", *Studies in History and Philosophy of Science*, 43, 2012; "Carnap, Kuhn, and the history of science: A reply to Thomas Uebel" (forthcoming) and (with Amelia Oliveira) "Kuhn, Sarton, and the history of science" (forthcoming).
[13] Giovanna Borradori (Ed.), *The American philosopher*, Chicago, Un. of Chicago, 1994, p. 157.
[14] Thomas Kuhn, *The Essential Tension*, Chicago, University of Chicago, 1977, p. xx.

In order to clarify the problem of the more complex discovery, which is what is of interest to us here, we must consider, along with Kuhn, that "factual and theoretical novelty are intertwined in scientific discovery". He writes:

> We must now ask how changes of this sort can come about, considering first discoveries, or novelties of fact, and then inventions, or novelties of theory. That distinction between discovery and invention or between fact and theory will, however, immediately prove to be exceedingly artificial. Its artificiality is an important clue to several of this essay's main theses. (…) Assimilating a new sort of fact demands a more than additive adjustment of theory, and until that adjustment is completed --until the scientist has learned to see nature in a different way--the new fact is not quite a scientific fact at all.[15]

Let us consider this passage together with another already cited above in which Kuhn emphasizes that "discovering a new sort of phenomenon is necessarily a complex event, one which involves recognizing both *that* something is and *what* it is" and that "both observation and conceptualization, fact and assimilation to theory, are inseparably linked in discovery".[16] The bridge between the question of the discovery of a phenomenon and the discovery or invention of a theory can be established through the fact that it is theory that will say what the phenomenon discovered is. In the first type of discovery, as we saw, we already have a previous theory. In the case of the second type, however, a theory has yet to be presented to account for the fact – for the anomolous fact that the previous theory allows one to know *that* it is but not *what* it is.

3. History of science, history of art, and change

It has been said that Thomas Kuhn`s first sentence in *The structure of scientific revolutions* is "perhaps the most famous sentence in the philosophy of science of the second half of the twentieth century".[17] The sentence, it is worth noting, does not refer to the

---

[15] Thomas Kuhn, *The Structure of Scientific Revolutions*, Chicago, University of Chicago, 1970 [1962], pp. 52-53.
[16] Thomas Kuhn, *The Structure of Scientific Revolutions*, Chicago, University of Chicago, 1970 [1962], p. 55.
[17] Alan Richardson & G. Hardcastle (Eds.), *Logical Empiricism in North America*, Minneapolis, University of Minnesota, 2003, p. vii. The sentence is the following: "History, if viewed as a

theory of paradigms, the main element in *Structure* that had such widespread repercussions, but rather to history; and it is a methodological observation, in two senses: it refers to the method of the history of science (proposes a change in methods, a new historiography of science) and, at the same time, to the method of philosophy of science (proposes a role for the new historiography of science in the philosophy of science).

In the essays published in *The Essential Tension*, Kuhn develops his famous sentence in these two directions, which he calls "Historiographic Studies" and "Metahistorical Studies". And he begins, as befits a historian, with an autobiographical preface in which he emphasizes that the new historiography is not new. At the moment he was proposing it, it was a historiography that was already being practiced in other disciplines, such as literature, art, music, and philosophy, and sensitive to conceptual ruptures.[18] What is important in Kuhn´s work is his controversial extension of this historiography to science. Science (particularly physics) was seen until then as being characterized by a peculiar cumulative development which vested it with the exclusive authority and legitimacy to be associated with progress (and rationality).

In the "Postscript – 1969", added to the second edition of his *Structure*, Kuhn commented on the fact that many considered his main theses to be applicable to other fields besides physics. He admitted that "to the extent that the book portrays scientific development as a succession of tradition-bound periods punctuated by non-cumulative breaks, its theses are undoubtedly of wide applicability". And he wrote:

> But they should be, for *they are borrowed from other fields*. Historians of literature, of music, of *the arts*, of political development, and of many other human activities have long described their subjects in the same way. Periodization in terms of revolutionary breaks in style, taste, and institutional structure have been among their standard tools. If I have been original with respect to concepts like these, it has mainly been by *applying them to the sciences*, fields which had been widely thought to develop in a different way.[19]

---

repository for more than anecdote or chronology, could produce a decisive transformation in the image of science by which we are now possessed".

[18] Thomas Kuhn, *The Essential Tension*, Chicago, University of Chicago, 1977, pp. xii and 348.

[19] Thomas Kuhn, *The Structure of Scientific Revolutions*, Chicago, University of Chicago, 1970 [1962], p. 208, my emphasis. See also Thomas Kuhn, *The Essential Tension*, Chicago, University of Chicago, 1977, pp. 150-151.

To assess the importance of this transposition to science, one need only consider that, for Kuhn, his book on scientific revolutions was a "belated product" of the "discovery of the close and persistent parallels between the two enterprises", art and science, which he had previously seen as polar. On one side, the world of values, the subjective, the intuitive; on the other, the world of facts, the objective, the inductive. And he refers to the work of Gombrich, who Kuhn said, was moving in the same direction and "has been a source of great encouragement to me". [20]

In the same text ("Comment on the relations of science and art"), which is specifically about the relation between the two disciplines, but focuses on the differences between them, Kuhn writes:

> Elsewhere [in *Structure*], as Ackerman points out, I have been concerned to emphasize the similarity of the evolutionary lines of the two disciplines. In both the historian can discover periods during which practice conforms to a tradition based upon one or another stable constellation of values, techniques, and models. In both he is also able to isolate periods of relatively rapid change in which one tradition and one set of values and models gives way to another. That much, however, can probably be said about the development of any human enterprise. With respect to gross developmental pattern my originality, if any, was only the insistence that what has long been recognized about the development of, say, the arts or philosophy applies to science as well.[21]

It could be suggested that, as in a feedback process, Kuhn´s thinking arises from more intuitive notions of "paradigm" and "incommensurability", like those found in the history of art and other disciplines (as in the idea of style and comparisons among styles). He develops them within his philosophy of science and then, at a higher level of

---

[20] Thomas Kuhn, *The Essential Tension*, Chicago, University of Chicago, 1977, pp. 340-341.

[21] Thomas Kuhn, *The Essential Tension*, Chicago, University of Chicago, 1977, p. 348. Kuhn concludes the passage, adding: "Recognizing that fundamental resemblance can therefore be no more than a first step. Having made it, one must also be prepared to discover a number of revealing differences in developmental fine structure". I could say that my project about Kuhn and the relations between the history of science and the history of art takes the recognition of the fundamental resemblance between the disciplines as a first step to discovering other 'revealing resemblances' between them. See my works in Spanish: "Thomas Kuhn, la historia de la ciencia y la historia del arte", Sergio Menna (Ed.), *Estudios contemporáneos sobre Epistemología*, Córdoba (Argentina), Sarmiento, 2008, pp. 29-47; and "Historia de la ciencia, historia del arte y racionalidad práctica", *Escritos Filosóficos*, vol. LX, no. 173, 2011.

conceptualization, they awaken the interest of the disciplines from which they originated, and virtually all cultural fields.

Kuhn discusses the history of art already in at least two earlier drafts of *The Structure of Scientific Revolutions*, previous to the version so-called *Proto-Structure* (1960). [22] He takes history of art into account at the very Introduction to the book in order to establish the supposed contrast between the development of science (cumulative) and that of art (non-cumulative) as traditionally conceived, and so presents his conception of science as revolutionary or not strictly cumulative. [23]

If the pair cumulative/non-cumulative cannot be understood as yet another element of opposition between science and art, it can be understood, however, as opposites that characterize the old and the new historiography. In this way, in the historiographical revolution in science that Kuhn addresses directly, and in the historiographical revolution in art, what we see is essentially the abandonment of the idea of a continuous cumulative progress throughout the history of both disciplines, and of the notion of rationality to which it is committed.[24]

The new historiography of science will distinguish itself from the old by admitting ruptures. This does not mean denying all continuity, but rather strict cumulativity. Kuhn is clear about this in his "structure" of the development of science, in which normal and extraordinary science alternate, and the anomalies that lead to overcoming a theory (or paradigm) are born within the theory itself and are guided by it.[25]

In an essay in which he refers directly to the question of creativity (the essay that gives the title to *The Essential Tension*, originally published in *The Third (1959) University of Utah Research Conference on the Identification of Creative Scientific Talent*), Kuhn speaks for the first time about paradigms, and emphasizes what he calls convergence,

---

[22] Cf. Hoyningen-Huene, "Context of discovery versus context of justification and Thomas Kuhn", Schickore, J. et al (Eds.), *Revisiting Discovery and Justification*, Dordrecht, Springer, p. 125.
[23] See my forthcoming paper "Kuhn's *Structure* and the history of art".
[24] See my article in Spanish "Historia de la ciencia, historia del arte y racionalidad práctica", *Escritos Filosóficos*, vol. LX, no. 173, 2011.
[25] See, for example, Thomas Kuhn, *The Structure of Scientific Revolutions*, Chicago, University of Chicago, 1970 [1962], pp. 52-53.

disagreeing with most collaborations that emphasize "divergent thinking".[26] But he does not fail to observe, as well, that "contrary to a prevalent impression, most new discoveries and theories in the sciences are not merely additions to the existing stockpile of scientific knowledge". And, simultaneously evoking ruptures and continuity, he adds that "only investigations firmly rooted in the contemporary scientific tradition are likely to break that tradition and give rise to a new one"[27].

This idea of rupture with continuity is also present in Gombrich, who writes in his history of art:

> Some form of art exists everywhere on the globe, but the story of art as a continuous effort does not begin in the caves of southern France or among the North American Indians. There is no direct tradition which links these strange beginnings with our own days, but there is a direct tradition, handed down from master to pupil and from pupil to admirer or copyist, which links the art of our own days, any house or any poster, with the art of the Nile Valley of some five thousand years ago. For we shall see that the Greek masters went to school with the Egyptians, and we are all the pupils of the Greeks. Thus the art of Egypt has a tremendous importance for us.[28]

And he justifies:

> For even the artist who is in revolt against tradition depends on it for that stimulus which gives direction to his efforts. It is for this reason that I have tried to tell the story of art as the story of a continuous weaving and changing of traditions in which each work refers to the past and points to the future. For there is no aspect of this story more wonderful than this - that a living chain of tradition still links the art of our own days with that of the Pyramid age.[29]

Kuhn comments on this aspect of the history of art, saying "Artists, whether in imitation or revolt, build from past art".[30] This shows that, for Kuhn as well as Gombrich, the idea of cumulative progress signifies a more restricted continuity in which there is no

---

[26] Thomas Kuhn, *The Essential Tension*, Chicago, University of Chicago, 1977, pp. 225-226. The paper was previously re-edited in Calvin Taylor and Frank Barron (Eds.), *Scientific Creativity: Its Recognition and Development*, New York, John Wiley, 1963.
[27] Thomas Kuhn, *The Essential Tension*, Chicago, University of Chicago, 1977, pp. 226-227.
[28] Ernst Gombrich, *The Story of Art*, London, Phaidon, 1995 [1950], p. 55.
[29] Ernst Gombrich, *The Story of Art*, London, Phaidon, 1995 [1950], p. 595.
[30] Thomas Kuhn, The *Essential Tension*, Chicago, University of Chicago, 1977, p. 152.

conflict. In both cases, however, a less restrictive continuity is allowed, in which conflict, revolt, or rupture is present.

A distinction must be established to avoid misunderstandings. In the same work cited immediately above, Gombrich appears to deny this continuity as he denies a "continuous progress", but he rejects, in fact, what could be called, in Kuhn´s terms, continuous cumulative progress. Gombrich writes:

> Each generation is at some point in revolt against the standards of its fathers; (…) I have tried to make this constant change of aims the key of my narrative, and to show how each work is related *by imitation or contradiction* to what has gone before. (…) There is one pitfall in this method of presentation which I hope to have avoided but which should not go unmentioned. It is the naïve misinterpretation of the constant change in art as a *continuous progress*. It is true that every artist feels that he has surpassed the generation before him and that from his point of view he has made progress beyond anything that was known before. (…) But we must realize that each gain or progress in one direction entails a loss in another, and that this subjective progress, in spite of its importance, does not correspond to an objective increase in artistic value.[31]

This accounting assessment, in terms of gains and losses, also allows Gombrich to explain what appear to him to be the differences between science and art. He writes in an initial pertinent passage:

> While these [Mantegna and Piero della Francesca] and other artists were applying the inventions of the great generation of Florentine masters, artists in Florence became increasingly aware of the new problems that these inventions had created. In the first flush of triumph, they may have thought that the discovery of perspective and the study of nature could solve all their difficulties. *But we must not forget that art is altogether different from science. The artist's means, his technical devices, can be developed, but art itself can hardly be said to progress in the way in which science progresses. Each discovery in one direction creates a new difficulty somewhere else.*[32]

And he understands the gains and losses in the following terms:

---

[31] Ernst Gombrich, *The Story of Art*, London, Phaidon, 1995 [1950], pp. 8-9, my emphasis.
[32] Ernst Gombrich, *The Story of Art*, London, Phaidon, 1995 [1950], p. 262, my emphasis

> We remember that medieval painters were unaware of the rules of correct draughtsmanship, but that this very shortcoming enabled them to distribute their figures over the picture in any way they liked in order to create the perfect pattern. (…) As soon as the new concept of making the picture a mirror of reality was adopted, this question of how to arrange the figures was no longer so easy to solve. In reality figures do not group themselves harmoniously, nor do they stand out clearly against a neutral background. In other words, there was a danger that the new power of the artist would ruin his most precious gift of creating a pleasing and satisfying whole.[33]

And at the end of the book, referring back to the preceding passage and to another in which he cites Cézanne´s art as a "synthesis" between fidelity to nature and formal balance, Gombrich concludes:

> And yet it is more than ever necessary to remember that art differs from science and technology. It is true that the history of art can sometimes trace the steps in the solution of certain artistic problems, and this book has tried to make these intelligible. But it has also tried to show that *in art we cannot speak of 'progress' as such, because every gain in one respect is likely to be offset by a loss in another*.[34]

Perhaps to emphasize the difference between science and art, Gombrich does not explicitly recognize in these passages (as he does in various other writings) that one can also speak of cumulative progress in art. After all, unlike some "highbrows", as he calls them, Gombrich does not expect that every work of art, to be creative, should create a new style.[35] With regard to this, Kuhn refers precisely to Gombrich[36] when he writes about "the inextricable connections between our notions of science and of progress":

> For many centuries, both in antiquity and again in early modern Europe, painting was regarded as *the* cumulative discipline. During those years the artist's goal was assumed to be representation. Critics and historians, like Pliny and Vasari, then recorded with

---

[33] Ernst Gombrich, *The Story of Art*, London, Phaidon, 1995 [1950], p. 262.
[34] Ernst Gombrich, *The Story of Art*, London, Phaidon, 1995 [1950], p. 617, my emphasis. These losses, recognized by Kuhn, as well, in relation to science, have been called "Kuhn-loss". See, for example, John Preston, *Kuhn's The Structure of Scientific Revolutions: a Reader's Guide*, London, Continuum, 2008, p. 57.
[35] Cf. Ernst Gombrich, *The Story of Art*, London, Phaidon, 1995 [1950], p. 596. Passage cited here in Section 1.
[36] Ernst Gombrich, *Art and Illusion*, Princeton, Princeton University, 2000 [1960], pp. 11-12 and 97.

> veneration the series of inventions from foreshortening through chiaroscuro that had made possible successively more perfect representations of nature. But those are also the years, particularly during the Renaissance, when little cleavage was felt between the sciences and the arts. Leonardo was only one of many men who passed freely back and forth between fields that only later became categorically distinct. Furthermore, even after that steady exchange had ceased, the term 'art' continued to apply as much to technology and the crafts, which were also seen as progressive, as to painting and sculpture. Only when the latter unequivocally renounced representation as their goal and began to learn again from primitive models did the cleavage we now take for granted assume anything like its present depth.[37]

Thus, we can say that Kuhn and Gombrich agree with respect to the development of art but disagree about science. Gombrich said that art and science are entirely different because science always presents cumulative progress, or "progress as such". Whereas for Kuhn, the development of science can be better understood if seen in closer proximity to the development of art, or more precisely, to the development of art as understood by Gombrich. This difference between Gombrich and Kuhn can be clearly observed in Gombrich´s references to Kuhn. Gombrich wrote, in "Relativism in the history of ideas":

> I have learned enough from another friend, Sir Karl Popper, not to dismiss human error as something culpable or even useless. I have been told that students of science often refuse to be interested in the history of their subject since they regard it simply as the history of errors which no longer concern us. You surely cannot write the history of ideas if you adopt such a negative attitude, but you cannot do so either, I want to contend, if you eliminate the notion of error altogether and adopt a wholly relativistic stance. I suspect it was Thomas Kuhn's book, The Structure of Scientific Revolution [sic] (1963) [sic], which appealed to the tender conscience of historians by warning them not to feel superior over past centuries and past ideas, though I have heard Kuhn say explicitly that he too believes in the progress of knowledge.[38]

And he finishes with a note:

---

[37] Thomas Kuhn, *The Structure of Scientific Revolutions*, Chicago, University of Chicago, 1970 [1962], p. 161. See also Thomas Kuhn, *The Road since Structure*, Chicago, University of Chicago, 2000, pp. 138 and 157.

[38] In Ernst Gombrich, *Topics of Our Time*, London, Phaidon, 1991, pp. 47-48.

> When I gave the lecture 'Focus on the Humanities' printed in *Tributes* (…), Professor Kuhn subsequently criticized *my statement that he did not believe in progress*, and I added a footnote to this effect. In a series of lectures at University College, London, entitled 'The Presence of Past Science' (November 1987), Professor Kuhn questioned the usefulness of the term 'relativism', but reasserted his belief in the progress of science.[39]

Finally, Gombrich makes the following mention of Kuhn in *Tributes*: "In this balance of optimism and resignation the humanist does not differ from the scientist, who also knows that there is no such thing as an ultimate explanation but believes – if he follows Popper rather than Kuhn – that he can make progress in suggesting better solutions". And he adds in a note: "In a subsequent discussion, Thomas Kuhn remarked that he also acknowledges the possibility of scientific progress".[40] These passages indicate that Gombrich suspected that Kuhn did not admit progress in science, as Kuhn identifies a succession of ruptures in the development of science. As for Gombrich, he is ready to admit such ruptures in art, but categorically refuses to do so in the case of science.

Kuhn is attracted to Gombrich´s conception of the history of art precisely because Gombrich does admit ruptures in the history of art, whereas traditional historians of science, according to Kuhn, do not admit ruptures in the history of science.[41] In addition to the texts cited above, Gombrich writes, for example, in *The Story of Art*, that

> It is fascinating to watch an artist thus striving to achieve the right balance, but if we were to ask him why he did this or changed that, he might not be able to tell us. He does not follow any fixed rules. He just feels his way. It is true that some artists or critics in certain periods have tried to formulate laws of their art; but it always turned out that poor artists did not achieve anything when trying to apply these laws, while great masters could break them and yet achieve a new kind of harmony no one had thought of before.[42]

In *Norm and Form*, he comments on the role of rules and proposes an endogenous explanation for historical overcoming of classical art:

---

[39] Ernst Gombrich, *Topics of Our Time*, London, Phaidon, 1991, p. 213, note 2, my emphasis.
[40] Ernst Gombrich, *Tributes – Interpreters of our cultural tradition*, Oxford, Phaidon, 1984, pp. 23 and 251(note 13).
[41] Cf. Thomas Kuhn, The *Essential Tension*, Chicago, University of Chicago, 1977, p. 340.
[42] Ernst Gombrich, *The Story of Art*, London, Phaidon, 1995 [1950], p. 35.

> It was the classical tradition of normative aesthetics that first formulated some rules of art, and such rules are most easily formulated negatively as a catalogue of sins to be avoided. (…) We have heard some of these sins characterized in the previous quotations -- the disharmonious, the arbitrary and the illogical must be taboo to those who follow the classical canon. There are many more in the writings of normative critics from Alberti *via* Vasari to Bellori or Félibien. (…) Indeed it might be argued that what ultimately killed the classical ideal was that the sins to be avoided multiplied till the artist's freedom was confined to an ever narrowing space; all he dared to do in the end was insipid repetition of safe solutions. After this, there was only one sin to be avoided in art, that of being academic.[43]

4. Final considerations

When we consider all that has been said here regarding continuity and discontinuity, the paradoxical nature of an expression like "rupture with continuity" stands out. But in order to adequately understand the relations between continuity and discontinuity, and between these and creativity, in science and art, it is necessary to consider that the idea of continuity with cumulativity is also paradoxical. This is because continuity, strictly speaking, means that there is no change, whereas cumulativity represents a change as well, even if merely due to additions. Thus, we can think of three types of continuity: strict continuity, continuity with cumulativity, and continuity with rupture. In the first, the expression indicates only that there is no change, or more precisely, that there is no change over time, or that things remain the same over a period of time. In the second case, of continuity with cumulativity, there is a change, but something also remains permanent over time. But what remains permanent?

In the case of science, Kuhn says, it is the accepted theory (or paradigm) that remains and the set of achievements, discoveries, and solutions to problems associated with and guided by that theory. And new discoveries or solutions to problems are added to this set of accomplishments like a new chemical element is added to the periodic table. In the case of art, what remains are proposals of style and the effective achievement of its practice, a set of works of art, to which are added new works of art informed by the same

[43] Ernst Gombrich, *Norm and Form*, London, Phaidon, 1971 [1966], p. 89.

style. They could be more explicit achievements related to realistic representation of nature (such as the technical achievements in the evolution of Greek art cited by Gombrich)[44], or less explicit ones, such as the addition of Monet´s last painting in the series on the Rouen cathedral.

No problem up to this point. But what about the third case we are considering? In this case, we no longer have the permanence alluded to in the preceding case. In science, there is a change in theory or paradigm, and in art, a change in style. So what, then, remains?

Kuhn and Gombrich, as we have seen, speak of a continuity with and without rupture. Kuhn conceives of the change in paradigms in science as endogenous, with a new paradigm emerging from an anomaly that stands out against the backdrop of the old paradigm.[45] In what concerns to art, Gombrich says that "each work is related *by imitation or contradiction* to what has gone before"[46] and Kuhn that artists build from past art "whether *in imitation or revolt*".[47]

Gombrich emphasizes the fact that there is history only with continuity. One could point to the drawings found in the caves at Altamira and Lascaux (the so-called rupestrian art) as a point of origin of art, but Gombrich disregards it because continuity cannot be established between this point and, say, Egyptian art. This possibility does exist, however, with respect to Egyptian or Assyrian art and contemporary art, and therefore he begins his narrative in Egypt and Assyria.[48]

---

[44] Gombrich, referring to sculpture, comments on the development of Greek style: "The sculptors in their workshops tried out new ideas and new ways of representing the human figure (...). One discovered how to chisel the trunk, another found out that a statue may look much more alive if the feet are not both placed firmly on the ground. Yet another would discover that he could make a face come alive by simply bending the mouth upwards so that it appeared to smile". Every innovation, he adds, "was eagerly taken up by others who added their own discoveries". Ernst Gombrich, *The Story of Art*, London, Phaidon, 1995 [1950], p. 78.

[45] Cf. Thomas Kuhn, *The Structure of Scientific Revolutions*, Chicago, University of Chicago, 1970 [1962], pp. 52-53.

[46] Ernst Gombrich, *The Story of Art*, London, Phaidon, 1995 [1950], p. 8, my emphasis. Part of a passage already cited in Section 3.

[47] Thomas Kuhn, The *Essential Tension*, Chicago, University of Chicago, 1977, p. 152, my emphasis. Passage already cited in Section 3.

[48] Cf. Ernst Gombrich, *The Story of Art*, London, Phaidon, 1995 [1950], p. 595, cited above.

What Gombrich denies here is the existence of historical continuity, as there is an evident logical continuity between rupestrian art and Greek art. Gombrich considers the Greek revolution to be "unique in the annals of mankind", although he admits pre-Greek examples (or outside of Greek influence) that imitate nature, such as ancient Mexican art, Egyptian art, and even rupestrian art. Nevertheless, he considers them isolated cases that did not become part of a new tradition, unlike what occurred in Greece, with its proposals for perfection and propagation. [49]

Would the same be true for Kuhn with respect to science? Paul Hoyningen-Huene, contesting the notion that incommensurability implies discontinuity, points to clear references to continuity in Kuhn´s work, such as the following description of what remains following a scientific revolution: "much of [the scientist's] language and most of his laboratory instruments are the same as they were before. As a result, postrevolutionary science invariably includes many of the same manipulations, performed with the same instruments and described in the same terms as its revolutionary predecessor".[50] Hoyningen-Huene comments, however, that "Kuhn isn't satisfied with his previous treatment of the continuities persisting through revolutions. The reason for this dissatisfaction is doubtless that, although he attested to these continuities, he didn't analyze them in any depth".[51]

In order to sketch a theoretical link to Gombrich, I call attention to a suggestive observation made by Kuhn. It is an economical suggestion and made in passing, when he contrasts the typical work of translators with the translation work engaged in by science historians and scientists when they compare two rival scientific theories. Regarding the latter, he writes: "They often have the inestimable advantage that the signs used in the two languages are identical or nearly so, that most of them function the same way in both languages, and that, where function has changed, there are nevertheless *informative reasons*

---

[49] Ernst Gombrich, *Art and Illusion*, Princeton, Princeton University, 2000 [1960], pp. 107-108, 141 and 143.

[50] Thomas Kuhn, *The Structure of Scientific Revolutions*, Chicago, University of Chicago, 1970 [1962], p. 130. See other passages in Paul Hoyningen-Huene, *Reconstructing Scientific Revolutions: Thomas S. Kuhn's Philosophy of Science*, Chicago, University of Chicago Press, 1993, p. 222.

[51] Paul Hoyningen-Huene, *Reconstructing Scientific Revolutions: Thomas S. Kuhn's Philosophy of Science*, Chicago, University of Chicago Press, 1993, p. 222.

for retaining the same sign".[52] And the same signs are maintained, although this identity can be misleading, creating false cognates or making it "excessively easy to ignore functional changes that would be apparent if they had been accompanied by a change of sign".[53] What might be these "informative reasons", vested with so much prestige that they take precedence over clarity itself?

Speaking of 'ways of worldmaking', Goodman writes:

> Facts, as Norwood Hanson says, are theory-laden; they are as theory-laden as we hope our theories are fact-laden. Or in other words, facts are small theories, and true theories are big facts. This does not mean, I must repeat, that right versions can be arrived at casually, or that worlds are built from scratch. We start, on any occasion, with some old version or world that we have on hand and that we are stuck with until we have the determination and skill to remake it into a new one. Some of the felt stubbornness of fact is the grip of habit: our firm foundation is indeed solid. Worldmaking begins with one version and ends with another.[54]

The ascendancy-descendancy relation pointed out here by Goodman is also recognized by Toulmin, who asks: "What makes the later phases of a science the 'legitimate heirs' of the earlier? These different phases are linked, neither by identities nor by logical entailments, but by relationships of legitimate ancestry and affiliation; and our problem is to discover how their legitimacy can be explained". And he adds in a note: "Cf. Ludwig Wittgenstein's reply to critics who complained that what he was doing was 'not philosophy', which was to answer, 'Maybe not, but what I am doing is the legitimate heir to that which has previously been called philosophy'".[55]

It can thus be said that the identity of the sign, like a family name, indicates its historical relation of ascendancy-descendancy in science. Kuhn clearly admits this relation.

---

[52] Thomas Kuhn, *The Road since Structure*, Chicago, University of Chicago, 2000, p. 165, my emphasis.
[53] Thomas Kuhn, *The Road since Structure*, Chicago, University of Chicago, 2000, p. 165.
[54] Nelson Goodman, *Ways of Worldmaking*, Indianapolis, Hackett, 1985, p. 97.
[55] Stephen Toulmin, *Human Understanding*, Princeton, Princeton University, 1972, p. 146. In the same way, speaking of his "epistemology naturalized", Quine writes: "Epistemology, or something like it". Willard V. Quine, *Ontological Relativity and Other Essays*, New York, Columbia University, 1969, p. 82.

For him, as we saw, the new theory or paradigm emerges based on the old in response to anomalies that discredit the latter.[56] He writes:

> The Copernican universe is itself the product of a series of investigations that the two-sphere universe made possible: the conception of a planetary earth is the most forceful illustration of the effective guidance given to science by the incompatible conception of a unique central earth. That is why a discussion of the Copernican Revolution must begin with a study of the two-sphere cosmology which Copernicanism ultimately made obsolete. The two-sphere universe is the parent of the Copernican; no conceptual scheme is born from nothing.[57]

Thus, it may be possible to consider an inter-theoretical competition in science without identity of object. And this continuity with rupture, this historical continuity that is not established through identity or logical implications, as Toulmin points out, would be stressed by an identity of sign. These would be the "informative reasons" Kuhn referred to for retaining the same sign.

If we return now to Gauguin, to our starting point, we can say that he illustrates very well the question regarding creativity and the idea of continuity and discontinuity in the realm of science as well as art. In the terms of the above analysis, Gauguin's sentence can be understood literally, without irony, as if it were part of a text (or context) belonging to Kuhn or Gombrich. Both, as we saw, allow for ruptures, but ruptures with continuity.

On the other hand, as a *boutade*, the sentence is paradoxical and accounts for the fact that the public demands originality, but a clear, immediately understandable and acceptable originality, which appears to negate originality. As though the public expects creativity, but not so much that the result might lie beyond their reach. . .

Gauguin´s spirited comment survives the analysis because the idea of rupture with continuity is itself (apparently) paradoxical. In addition, it is always difficult to cope with

---

[56] Cf. Thomas Kuhn, *The Structure of Scientific Revolutions*, Chicago, University of Chicago, 1970 [1962], pp. 52-53.

[57] Thomas Kuhn, *The Copernican Revolution*, Cambridge, Harvard University, 1977 [1957], p. 41. Kuhn talks explicitly about a relation of descendence among theories also in Thomas Kuhn, *The Road since Structure*, Chicago, University of Chicago, 2000, p. 160.

the really new, which tends to be received, clearly or confusedly, in terms of the old. With respect to this, we saw how, according to Gombrich, it is expected that the artist "should produce the kind of paintings or sculptures they have seen labelled as Art before".[58] However, in the history of science as well in the history of art, in the face of a true novelty (discovery of the second type), it must be resolved simultaneously what the new phenomenon is, and what is science or what is art. The novelty can induce a change in the very conception of what is science, or what is art.

This is not an individual task, but an attribution to the scientific community and the artistic community (Kuhn speaks of a "community of artists"). And this is a question to be solved *in extremis*, because at the same time, the communities themselves (their composition and their very reason for being) are also at stake. In the face of a scenario that could be radical, it can be understood how a thread of continuity is important, after all. There goeth Theseus into the labyrinth.

[58] Ernst Gombrich, *The Story of Art*, London, Phaidon, 1995 [1950], p. 596. Passage already cited here in Section 1.