
Project MUSE®
Today's Research. Tomorrow's Inspiration.

Novelty and Revolution in Art and Science: The Connection between Kuhn and Cavell

Vasso Kindi



Perspectives on Science, Volume 18, Number 3, Fall 2010, pp. 284-310 (Article)

Published by The MIT Press

For additional information about this article
http://muse.jhu.edu/journals/posc/summary/v018/18.3.kindi.html

Access Provided by University of Pittsburgh at 07/05/10 9:47AM GMT

Novelty and Revolution in Art and Science: The Connection between Kuhn and Cavell

Vasso Kindi
University of Athens

In the paper I maintain that there is a connection between Thomas S. Kuhn and Stanley Cavell as regards novelty and revolution in the history of science and in the arts. I argue that the influence is not unidirectional, from Cavell to Kuhn, as it is usually taken to be the case, but, rather, that Kuhn's understanding of revolution contributed to a similar understanding of novelty by Cavell in relation to the arts. Novelty, in this latter conception, is tied to tradition and it is brought about to preserve the integrity of the practice to be changed. In this sense, radical novelty or revolution combines the original meaning of revolution as restoration but also the modern meaning of radical break and new beginning. Kuhn's contribution to the concept of revolution is that he disassociates it from modernity's idea of progress giving it a postmodern twist. I further examine a possible dissimilarity between Cavell and Kuhn, namely that Cavell, but not Kuhn, in invoking tradition, is in pursuit of essence. I show that neither is involved in an essentialist project and that the alleged dissimilarity is only apparent. Finally, I consider several problems that their common view faces and offer a possible way to address them.

I. The connection between Kuhn and Cavell

Both Kuhn and Cavell acknowledge their indebtedness to each other in their respective books of the 60s. Cavell in *Must We Mean What We Say* (1969) and Kuhn in *The Structure of Scientific Revolutions* (1962). They were

I would like to thank Theodore Arabatzis, James Conant, Gürol Irzik, Alexander Nehamas, Katerina Paplomata and the anonymous referees for very helpful comments and suggestions. I would also like to thank my students at the University of Athens, Greece, who attended the seminar on "Following a rule in the arts and law" (Spring 2008) and the audience at the Program in Hellenic Studies Workshop at Princeton University in February 2009.

Perspectives on Science 2010, vol. 18, no. 3
©2010 by The Massachusetts Institute of Technology

together at Berkeley where they had both moved in 1956 as assistant professors after their first encounter at the Society of Fellows at Harvard (Kuhn 2000d, p. 197). In Berkeley, Cavell and Kuhn discovered a mutual understanding and an intellectual affinity. They had regular conversations which Cavell describes as "extremely important" (Conant 1989, p. 40; cf. Cavell 1979, p. xix). Cavell says that he felt he wanted to assure Kuhn that "philosophy did not have standing answers to the questions [Kuhn] was asking" while he learned from Kuhn about history of science and history of ideas (Conant 1989, p. 41; cf. Cavell 1976, p. xii). In Cavell's view "in the learning going on between [them] it was all [him] learning from [Kuhn], about the history of science, about history as such, especially in relation to ideas" (Conant 1989, p. 41). Cavell mentions, in particular, in that respect, the role of thought experiments in the development of the scientific revolution. Kuhn, in the paper entitled "A Function for Thought Experiments" (1977b), first published in 1964, maintains that thought experiments do not supply the scientists with new information and data, but disclose problems in the conceptual framework of the available theory, thereby inducing a crisis which brings about re-conceptualization and, thus, revolution. Cavell takes notice of the connection between re-conceptualization and revolution in Kuhn.

Now, Kuhn, on his part, who also acknowledges generously his indebtedness to Cavell, sees the situation the other way around. In his view, it was Cavell who was in the position of authority giving him instruction and encouragement on issues that concerned him. In his last autobiographical interview he gave in Athens, he singles Cavell out among his colleagues at Berkeley: "The person who was *extraordinarily* important was Stanley Cavell. My interactions with him taught me a lot, encouraged me a lot, gave me certain ways of thinking about my problems, that were of a lot of importance" (emphasis in the original, Kuhn 2000d, p. 297). Already in the Preface to *The Structure of Scientific Revolutions* Kuhn gives Cavell a position of prominence among the figures who had exercised an influence on him. "[A]fter my departure from Cambridge, [J. B. Conant's] place as a creative sounding board and more was assumed by my Berkeley colleague, Stanley Cavell. That Cavell, a philosopher mainly concerned with ethics and aesthetics, should have reached conclusions quite so congruent to my own has been a constant source of stimulation and encouragement to me. He is, furthermore, the only person with whom I have ever been able to explore my ideas in incomplete sentences. That role of communication attests an understanding that has enabled him to point me the way through or around several major barriers encountered while preparing my first manuscript" (Kuhn 1970, p. xiii).

It is clear from the above, I think, that there was a close connection be-

tween Kuhn and Cavell in the very formative years for both at Berkeley. Undoubtedly, they had a similar bend of mind and they were both exploring new fields. Both can be seen as outsiders of their respective disciplines: Cavell of philosophy, Kuhn of history and philosophy of science. They were both young and, as Cavell put it, they "didn't quite know what they were doing" (Conant 1989, p. 41), even though he later admits that they shared a sense that something was happening at the time in different fields (Jones 2000, p. 510). Kuhn, who always had trouble with philosophers and felt insecure about his philosophical claims, must have sought reassurance from Cavell on philosophical matters while Cavell was learning from Kuhn a new way of looking at science and its history. In particular, Kuhn's ideas regarding thought experiments and their relation to conceptual revolutions had an influence on Cavell's idea of novelty. The historical facts cited show, I think, first, that there *was* indeed a connection between Kuhn and Cavell and, second, that the influence was not unidirectional, from Cavell to Kuhn as it is usually taken to be the case.[1]

II. Cavell and Kuhn on novelty and tradition

In *The Claim of Reason,* a book published in 1979 as a revised version of Cavell's dissertation *The Claim to Rationality* which was submitted to Harvard in 1961, and which discusses extensively Wittgenstein's *Philosophical Investigations,* Cavell writes:

> It is because certain human beings crave the conservation of their art that they seek to discover how, under altered circumstances, paintings and pieces of music can still be made, and hence revolutionize their art beyond the recognition of many. This is how in my illiteracy, I read Thomas Kuhn's *The Structure of Scientific Revolutions:* that only a master of the science can accept a revolutionary change as a natural extension of that science; and that he accepts it, or proposes it, in order to maintain touch with the idea of that science, with its internal canons of comprehensibility and comprehensiveness, as if against the vision that, under altered circumstance, the normal progress of explanation and exception no longer seem to

1. See, for instance, Jones (2000). It is usually assumed that Cavell's knowledge of Wittgenstein's philosophy put him in a position of authority in comparison to Kuhn. Cavell (2001a), however, says that he started reading Wittgenstein seriously as late as 1958, when he had to review the joint publication of the *Blue and Brown Books,* and he first taught Wittgenstein in 1960 when he gave seminars on the *Philosophical Investigations,* first together with Thomson Clarke and later on his own. Rorty (2005, p. 18) is one philosopher who thinks that the influence was mostly from Kuhn to Cavell.

> him to be science. And then what he does may not seem scientific to the old master. (Cavell 1979, p. 121)

Earlier in the same chapter, Cavell maintains that "only a slave of [convention] can know how it may be changed for the better, or know why it should be eradicated. Only masters of a game, perfect slaves to that project, are in a position to establish conventions which better serve its essence. This is why deep revolutionary changes can result from attempts to conserve a project, to take it back to its idea, keep it in touch with its history" (Cavell 1979, pp. xv, 120–121).[2]

Key terms in these two passages are: *conservation* and *revolution* but also *essence* to which I will return. What Cavell is saying is that only masters of a particular game (e.g., science or art), slaves of the respective practices, can carry out revolutions. The revolutions will be brought about in the name of finding, of conserving, of reaching back to the idea of the practice to be revolutionized. This requires that revolutionary agents keep in touch with their history, even if it is idealized. The masters of the game will see the developments as naturally extending their discipline and they will endorse the revolution, whereas the old masters, who did not see and did not realize that the normal development of their practice lost contact with its idea, will resist the revolution and come to see revolutionary developments as not continuing or extending their practice.[3]

A point similar to Cavell's is made by Clement Greenberg, the important art critic, who was a contemporary of both Kuhn and Cavell. In his article "Modernist Painting", written in 1960, he underscores the importance of the past of art and the "compulsion to maintain past standards of excellence" (p. 10) for the advent of modernism's novelty. In a later essay, entitled "Convention and Innovation" (1999), first published in 1976, he maintains that in order to break with a convention you need to possess it (1999, p. 52). He mentions Blake and Whitman, Joyce, Schönberg, Manet, Pollock and Picasso as masters of the conventions of their respective arts who were able, because of that fact, to break with these conventions radically.

2. Cavell is interested in this kind of project because he is trying himself to revolutionize philosophy from within philosophy and he interprets Wittgenstein as doing exactly this in the *Philosophical Investigations* (Cavell 1979, pp. xv, 121).

3. This is reminiscent of Kuhn citing Max Planck and his claim that "a new scientific truth does not triumph by convincing its opponents and making them see the light, but rather because its opponents eventually die, and a new generation grows up that is familiar with it" (Kuhn 1970, p. 151). Planck's claim has been challenged (Hull et al 1978), but it is mentioned here to show the parallelism between Kuhn's and Cavell's views.

> [T]he record shows no case of significant innovation where the innovative artist didn't possess and grasp the convention or conventions that he changed or abandoned. Which is to say that he subjected his art to the pressure of these conventions in the course of changing or shedding them. Nor did he have to cast around for new conventions to replace those he had shed; his new conventions would emerge from the old ones simply by dint of his struggle with the old ones. And these old ones, no matter how abruptly discarded, would somehow keep being there, like ghosts, and give ghostly guidance. (1999, p. 53)

The affinity between what Cavell and Greenberg are saying and what Kuhn said is obvious.[4] Cavell's and Greenberg's view, i.e., that one needs to be immersed in a certain tradition in order to be able to change it, even radically, is indeed reminiscent of Kuhn who has spoken of the function of

4. I am not claiming, however, that Greenberg was influenced by Kuhn. There is no evidence of such a connection. Also Greenberg, unlike Kuhn, was more interested in highlighting continuity rather than break in the developments of the arts: "Nothing could be further from the authentic art of our time than the idea of a rupture of continuity. Art, is among other things, continuity" (1982, p. 10). In fact, to characterize the change associated with Modernism, Greenberg uses the term 'devolution' in the sense of natural development or succession: "[a]rt gets carried on under Modernism in the same way as before. And I cannot insist enough that Modernism has never meant anything like a break with the past. It may mean a devolution, an unravelling of anterior tradition, but it also means its continuation. Modernist art develops out of the past without gap or break, and wherever it ends up it will never stop being intelligible in terms of the continuity of art" (1982, p. 9). It is interesting to note that Herbert Read, writing in 1933 about Modernism, also uses the term 'devolution' to mean the exact opposite of what Greenberg means: "There have been revolutions in the history of art before to-day. There is a revolution with every new generation, and periodically every century or so, we get a wider or a deeper change of sensibility to which we give the name of a period—the Trecento, the Quatro Cento, the Baroque, the Rococo, the Romantic, the Impressionist and so on. But I do think that we can already discern a difference of kind in the contemporary revolution: it is not so much a revolution, which implies a turning over, even a turning back, but rather a break-up, devolution, some would say a dissolution. Its character is catastrophic. Historical revolutions begin as isolated ferments, which gradually spread until they transform, infuse the whole body of a civilisation. But now you have rather the birth of a new body, or bodies, distinct in character and incapable of fusion with the old body" (pp. 58–59). Greenberg later admitted in interviews that he may have overstated his emphasis on continuity to oppose journalistic enthusiasm with novelty and revolution. It is also noteworthy that in the article of 1960, "Modernist Painting", he compares art to science. In his view, both confine themselves to their particular practices making no reference to any external considerations. "[A] problem in physiology is solved in terms of physiology not in those of psychology" (1982, p. 8). This is the reason he takes art to be an activity of immanent self-criticism (ibid., p. 5).

dogma in scientific research.[5] Kuhn has emphasized the importance of tradition-bound research, of commitment to paradigms, of the restricted vision and rigidity of normal science as a prerequisite to novelty and revolutionary change. His idea is that anomaly—literally deviation from normalcy—is recognized only by someone who knows already what to expect. "[N]ovelty ordinarily emerges only for the man, who knowing *with precision* what he should expect, is able to recognize that something has gone wrong. Anomaly appears only against the background provided by the paradigm" (Kuhn 1970, p. 65; cf. pp. 24, 96). Even discoveries which come about by accident, Kuhn says, "could not ordinarily have occurred to a man just looking around" (1963, p. 365).[6] Scientific education, with its emphasis on repeated applications, imposes upon prospective scientists the discipline required while at the same time instills in them the aspiration of extending the tradition and achieving innovation. Kuhn, already in 1959, speaks of an "essential tension" implicit in scientific research.[7] In his view "the successful scientist must simultaneously display the characteristics of the traditionalist and of the iconoclast" (1977a, p. 227). "The scientist," Kuhn says, "requires a thoroughgoing commitment to the tradition with which, if he is fully successful, he will break" (1977a., p. 235; cf. Kuhn 1963, pp. 368–369).

In all three cases, that is, in Cavell, Greenberg, and Kuhn, we see the emphasis on mastering a tradition in order to be able to radically change or even eradicate it.[8] We saw that for Kuhn the reason is that only against an ingrained background of normalcy can one detect an anomaly or even speak of one. For Greenberg it is a matter of interest[9] and for Cavell a matter of relevance. "[W]hat will count as a relevant change is determined by

5. Kuhn stated that "commitment is constitutive of research . . . [and] though a quasi-dogmatic commitment is, on the one hand, a source of resistance and controversy, it is also instrumental in making the sciences the most consistently revolutionary of all human activities" (1963, p. 349).

6. According to Kuhn, the fact that we often have simultaneous innovations in several scientific laboratories attests to how "traditional pursuit prepares the way for its own change" (Kuhn 1970, p. 65).

7. The paper entitled "The Essential Tension: Tradition and Innovation in Scientific Research" (Kuhn 1977a) was first published in 1959.

8. Greenberg's characterization of art as a self-critical activity may be taken to imply that he is depreciative of tradition and that his approach is more akin to Popper's than Kuhn's. I do not think that this is correct. Popper, who encouraged a critical attitude in science, liberal in spirit, was in favour of immediately abandoning a theory when it was found wanting, whereas Greenberg adopts the rationale of the Kantian critique which explores the limits of legitimate claims. It has to be admitted, though, that Greenberg's approach seems to be more conservative compared to Kuhn's.

9. According to Greenberg, art which has been made "too largely in freedom from per-

the commitment to painting as an art, continuing and countering the conventions and intentions and responses which comprise that history" (Cavell 1976b, p. 222). Only changes which are proposed in juxtaposition with the old conventions and established practices are worth considering, are relevant and not boring (Greenberg 1999, pp. 49, 50; cf. Cavell 1976b, pp. 215, 22).[10]

III. Dissimilarity between Cavell and Kuhn? Revolution as restoration and a new beginning

Still, Cavell, but also Greenberg, seem to emphasize one other thing which is not so prominent in Kuhn, one might even argue, it is absent. In the passage cited above from Cavell, we do not only find the insistence on the significance of commitment to tradition as a prerequisite for novelty, Cavell also maintains that a revolution, that is, radical break, is brought about not by openly declaring rupture with the past and announcing a new beginning, but in the name of continuing the tradition or preserving its most essential character. In an article first published in 1965, entitled "Music Discomposed" (1976a), where he discusses revolutionary developments in the music of 20th century, he says so explicitly:

> What looks like "breaking with tradition" in the succession of art is not really that; or is that only after the fact, looking historically or critically; or is that only as a result not as motive: the unheard of appearance of the modern in art is an effort not to break, but to keep faith with tradition. (Cavell 1976a, pp. 206–7)

In Cavell's view, each of the modernist pieces of art, e.g., the music of Schönberg or the sculpture of Caro, "one could say, is trying to find the limits or essence of its own procedures" (Cavell 1976a, p. 219).

Greenberg also spoke of finding the limits and essence of each art (1982, pp. 1, 8, 9) and was criticized by Michael Fried,[11] among others,

---

tinent esthetic pressures" (1999, p. 51) is "uninterestingly bad" (ibid., p. 50). Cf. ibid., p. 52.

10. Compare T. S. Eliot in his Introduction to Pound's *Selected Poems:* "The poem which is absolutely original is absolutely bad; it is, in the bad sense, 'subjective', with no relation to the world to which it appeals" (Pound 1934, p. x).

11. Fried had a mutually beneficial interaction with Cavell, which started around 1964, and he was well acquainted with both Kuhn's work and Greenberg's. "Fried's articulation of Greenberg's Modernism was indebted to the matrix of ideas circulating around Kuhn's *The Structure of Scientific Revolutions,* many of them mediated by the cogent philosophy of Stanley Cavell" (Jones 2000, p. 523; cf. pp. 489–90).

for being an essentialist.[12] According to Michael Fried (1982), the quest of continuing the tradition in the best way possible, a way which may involve changing or getting rid of some or all previous conventions, has taken the form of an essentialist project, i.e., finding and saving the essence of art or excellence in art.[13] Against Greenberg's essentialism, as Fried understands it, Fried puts forward his own account. In his view "essence [. . .] is largely determined by, and therefore changes continually in response to, the vital work of the recent past. The essence of painting is not something irreducible. Rather the task of the modernist painter is to discover those conventions, which, at a given moment, alone are capable of establishing his work's identity as paintings" (1982, p. 223).[14]

This account of radical change, i.e., the search for the essence of tradition and the effort to reinstate it after its abuse, bears resemblance to what Hanna Arendt says about an early understanding of revolutions. According to Arendt, the first time the word 'revolution' descended from the sky and planetary motion came to be used in politics is 17th century England. In ancient and medieval times, she says, there were rebellions, revolts, uprisings, upheavals, mutinies, forcible overthrows of rulers, but not revolutions. Revolutions had to be associated not only with violence (rebellions had that) but also with the idea of liberation, freedom and new beginning which will not just substitute one ruler for another but one that would make the subjects rulers (Arendt 1990, pp. 29, 41).

In its first political use, Arendt argues, the term 'revolution' had the meaning of restoration, of "revolving back to some pre-established point and, by implication, of swinging back into a pre-ordained order" (1990,

12. Fried is referring to Greenberg's so-called reductionist thesis (1998b, p. 99n11): Modernism advanced by trying to find the bare essentials of every art (for instance, for pictorial art the irreducible conventions are flatness and delimitation of flatness) (Fried 1982, pp. 221–223, 225 n. 12).

13. Here, one may also recall Dworkin who invokes Cavell in order to maintain that interpretation aims at understanding particular instances of art or law as best examples of their respective traditions (1991, pp. 55–59). Already in his (1982, p. 183), Dworkin advanced the so-called "aesthetic hypothesis." This hypothesis says that "an interpretation of a piece of literature attempts to show which way of reading (or speaking or directing or acting) the text reveals it as the best work of art." Dworkin uses literary interpretation as a model for legal interpretation. In his view "an interpretation of any body or division of law . . . must show the value of that body of law in political terms by demonstrating the best principle or policy it can be taken to serve" (p. 194). Any attempt at interpretation by a judge must aim, according to Dworkin, to preserve or respect "the integrity and coherence of law as an institution" (p. 195). The judge's interpretation must fit prior law.

14. Caroline Jones (2000) has criticized Fried for being mired himself in a reductionist project. See also Fried's response to Jones (Fried 2001). Costello (2008, pp. 288, 292–293, n. 35) has also identified similarities between Fried and Greenberg as regards essence in art.

pp. 42–43). "[T]he revolutions started as restorations or renovations and the revolutionary pathos of an entirely new beginning was born only in the course of the event itself" (1990, p. 37).[15] The agents who started these early revolutions, much like Greenberg's "reluctant innovators" (1999, p. 54),[16] did not aim, according to Arendt, at complete novelty.[17] Referring to the French and American Revolutions she writes: "both were played in their initial stages by men who were firmly convinced that they would do no more than restore an old order of things that had been disturbed and violated by the despotism of absolute monarchy or the abuses of colonial government" (1990, p. 44).[18]

'Revolution', in its modern sense, signifies complete novelty, an idea which was absent before and is conceivable only against a rectilinear understanding of time. It has also come to be associated in Arendt's view with some kind of necessity to which the agents of the great revolutions of the Enlightenment had to surrender (1990, pp. 47–48). Revolutionary events, driven by the daily needs of a grand majority, accelerated by the "crimes of tyranny" and the "progress of liberty" were irresistible and carried in their uncontrollable stormy procession, as if in a violent, overflowing current, the people who were caught in it. These people may have started as free agents who took their lives into their own hands, changing

15. I. B. Cohen (1976, p. 262) suggests that the affinity between the original cyclical meaning of 'revolution' and the modern meaning of complete change of affairs can be traced to the association between the cyclical "turning over" and the secular "overturning".

16. Note that Kuhn has also been dubbed the "reluctant revolutionary" for what he did in philosophy of science (Horgan 1991).

17. Arendt notes that unlike the historical agents who undertook to bring about a revolution, scientists and philosophers, much earlier than political actors, were concerned with absolute newness (Arendt 1990, p. 46). Still, I. B. Cohen in his *Revolution in Science* (1985) and in an earlier paper (1976) paints a much more complex picture. He believes that the shift from scientific change as a cyclical continuous process to scientific change marked by radical revolutions was influenced by "the development of concepts and theories of political and social (and cultural) revolution" (1976, p. 259). He says that he had not found "any references to revolutions in the sciences before 1700" (1976, p. 266) and claims that it is "not always a simple task to discover whether an eighteenth-century author may have had in mind a cyclical return (an ebb and flow) or a secular change of significant magnitude" (1976, p. 259). In his view "during the first century or so of modern science, many of the great creative scientists tended to think of themselves (and even to be viewed by their contemporaries) as revivers or rediscoverers of ancient knowledge, even as innovators who improved or extended knowledge, but not as revolutionaries in the sense in which we would commonly use this expression today" (1985, p. 6).

18. Arendt cites Tocqueville who wrote, not unequivocally, as she admits, about the French Revolution: "one might have believed the aim of the coming revolution was not the overthrow of the old regime but its restoration" (Arendt 1990, p. 45). Even in the case of science, according to Steven Shapin, "[t]he people who are said to have made the [scientific] revolution used no such term to refer to what they were doing" (1996, p. 2).

history by their actions but ended up, as Arendt contends, succumbing to the inevitable necessities of history. Yet, this Hegelian understanding of historical process which brought order to the haphazardness of human affairs from the perspective of an haughty spectator—an order that Arendt compares to the lawfulness and regularity of planetary motion (1990, p. 54)—did not eliminate from the modern concept of revolution the element of unpredictability, uncertainty and openness to the future which confronts the people who are caught in the whirlwinds of revolutions.

So, to summarize, we have revolution as restoration and we have revolutionary agents who aim to restore a lost order or a lost purity. In modern times we have two important dimensions attached to the concept of revolution. Revolutions become radical breaks and not restorations and they are for a good cause: they contribute to progress by increasing freedom. The unpredictability of where we go after the revolutionary rupture, was, again in modern times, tamed and taken care of by Hegelian necessities.

Going back to Cavell, Fried and Greenberg, we find the sense of restoration that Arendt detects in the early appearance of revolution on the political scene. Revolutionary artists struggle with the conventions of their respective traditions in the name of restoring the integrity of their art, of saving it from the distortions of consumerism and entertainment. Yet, they don't aim at restoring a particular bygone style—artistic "progress" is as irreversible as the scientific, says Cavell (1976a, pp. 183–5). What they *do* try is to discover and re-instate their lost and abused art. Both Fried and Cavell use the verb "to discover"[19] when they talk of the new conventions and criteria that the modernist painters are after. Since there are no a priori standards for what a painting is, modernist painters have to discover their new conventions and their criteria "in the continuity of painting itself" (Cavell 1976a, p. 219; Fried 1982, p. 223), that is, in the history of painting.

So, although Cavell—to concentrate only on him—leans heavily on Kuhn when he underlines the importance of commitment to tradition as a prerequisite of unanticipated novelty, it seems as though he parts company with him when he says that in so doing one is discovering essence. Kuhn's understanding of scientific development is completely opposed to this idea. Kuhn stresses the open-endedness of the process, the contingency and unpredictability of developments, the discontinuity and incommensurability of successive stages, the variability of scientific practice. He does not at all talk in terms of essence and compares scientific advance to

19. Stephen Melville (1986, p. 20) contrasts Cavell and Greenberg claiming that only Greenberg speaks of discovery: "What Cavell calls a recognition, Greenberg reports as a discovery. . . ."

the Darwinian biological evolution, a blind, non teleological process of speciation which is "driven from behind and not pulled from ahead" (Kuhn 2000b, p. 96). In the case of science, behind lies the normal practice of scientists, with its wealth of puzzles generated after a paradigm. Commitments and conventions, embodied in and making up the puzzles to be solved, constrain scientists in what they do and, simultaneously, enable them to move ahead even in directions which were unanticipated. In Cavell's account, true revolutionary artists (at least in the case of modernist art) look backwards, probe the limits of their art from within tradition resisting gratuitous innovation[20] and try to uncover the lost grail, whereas in Kuhn's, revolutionary scientists, who have emerged struggling with problems of their tradition, are looking forward, ready to embark confidently upon the new direction (or new adventure) which has been opened up. What this direction (and adventure) will amount to will depend on how the new paradigms will be articulated.

As I will explain later, the two approaches may not be as different as I have just presented them. Yet, before I show this, I would like to concentrate a little on Kuhn's understanding of revolutions. No matter how reluctant a revolutionary he was, or just because he was reluctant, his understanding of revolution, I will argue, was itself revolutionary and in that respect made contact with Cavell's interests.

IV. What is new in Kuhn's understanding of revolutions

Kuhn says repeatedly (1970 p. 208; cf. 1977a, p. 231, n. 3; 1977c, p. 348) that his account of scientific development as punctuated advancement is not something really novel.[21] The development of any discipline or human enterprise can be described in such a way. "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" (Kuhn 1970, p. 208). His originality lies, he says, in that he applied these concepts to science.

Kuhn, however, was not, of course, the first one to apply revolutions to science. I. B. Cohen (1985, p. 176) traces the use of 'revolution' as radical break in relation to science already in the eighteenth century. Historians, but also other intellectuals and the general public, spoke, long before Kuhn, of the Scientific Revolution, the Copernican Revolution or the Darwinian Revolution. Steven Shapin (1996, p. 2) credits Koyré for coining

20. In conservative thought, which extols the significance of tradition, change itself may be considered injurious. See Pieper (2008, pp. 38–39).

21. I take it that this comment is not a move to avoid criticism by saying that his account does not differ so much from more conventional ones.

the term 'Scientific Revolution' in 1939 while Burtt, already in 1924, in his book *The Metaphysical Foundations of Modern Science* (1952, p. 29) used the term 'revolution' to speak of the substitution of explanations in terms of simple parts for teleological explanations in the period between 1500 and 1700. In Butterfield's book, *The Origins of Modern Science,* originally published in 1949, there are two chapters on "The Scientific Revolution" and a chapter on the "Postponed Revolution in Chemistry" while in 1954 Rupert Hall published a book entitled *The Scientific Revolution, 1500–1800: The Formation of the Modern Scientific Attitude.* Even Hans Reichenbach, in 1951, who belonged to the group, logical positivism, which is most pronounced as Kuhn's opposite on the issue of scientific development, speaks of the *new science* which gave rise to the new philosophy and uses the term 'revolution' to refer to the results of Einstein's theory of relativity and Planck's quantum theory, even though he immediately qualifies it as "rapid evolution" from 19th century science (Reichenbach 1951, pp. 122–123).[22]

But all this talk about revolutions in science was not at all worrisome and certainly did not provoke the reaction Kuhn's *The Structure of Scientific Revolutions* did. Why was that? Is it perhaps the generality implied by the book's title, i.e., that there is a revolutionary pattern in science, that revolutions are not accidental isolated events, but that it is inherent in science to develop, not simply by accumulation, but through revolutions? But even that would not imply a major difference in comparison to other thinkers. Karl Popper, before Kuhn, spoke of revolution in permanence in science.[23] Kuhn was opposed to this idea. "[R]evolution in perpetuity is a contradiction in terms," Kuhn says (2000d, p. 295).[24] What he means is that in order to have revolutions there need to exist in between phases of

22. As H. Floris Cohen notes (1994, p. 21), one should not, of course, conflate the historical term 'The Scientific Revolution' which refers to particular events in the history of science, with the term 'scientific revolutions' which signifies generically radical and discontinuous breaks. For my purposes, though, this conflation is immaterial since what I am interested to show is that long before Kuhn, both among historians and among scientists, scientific revolutions, either as pertaining to particular events in history or as marking a type of scientific advancement, were thought to be present in the history of science.

23. Popper takes the phrase from Marx who, according to Hannah Arendt, takes it from Proudhon who coined the expression 'révolution en permanence' (Arendt 1990, pp. 50–51). Marx was calling on workers to maintain the revolutionary spirit after their initial success and Popper was calling on scientists to maintain their revisionist critical spirit which is cultivated in the effort to falsify theories. In Proudhon's view, as Arendt tells it, "there is only one revolution, selfsame and perpetual." Arendt maintains that ever since the French Revolution, there developed a movement, pushing always forward, which, like a current gone underground, sustained all revolutionary activity and broke up to the surface every time the conditions allowed it.

24. Toulmin (1972, p. 114), though, maintains that Kuhn, by taking revolutions to

relative stability. This is an analytical point.[25] Revolutions and normal science are the two complementary aspects of scientific advance (Kuhn 1977a, p. 227). Even Karl Popper, whose endorsement of permanent revolution is a little curious given his objections to political revolutions,[26] had to recognize that there need to exist some kind of resistance to constant revision and innovation because this is how beliefs are tested to their full potential. If a thesis is abandoned the minute it is criticized, it would not be possible to appreciate its merits (Popper 1994, p. 94).[27]

So, what was the bothersome element in Kuhn's account? It wasn't the talk about revolutions, it wasn't the thesis that science may develop through revolutions, but it was, I think, the claim that revolutions depend upon tradition and that they do not add to the cumulative enterprise of science. Before Kuhn, scientific revolutions, however destructive and transformative of the *status quo ante,* were taken to be the big scale events which, governed by high principles, contributed, massively and decisively, to the grand march of scientific progress. The agents who brought them about were the heroes of science who, endowed with exceptional powers and gifts, executed almost a divine, or Hegelian, plan. Much like the liberal political revolutions of modernity which were not just violent uprisings but were seen as steadily and progressively enhancing emancipation and expanding our freedom, scientific revolutions were seen, before Kuhn, as joltingly putting us on the right track, sweepingly redressing past mistakes committed by minor, less competent scientists, increasing vastly our knowledge and getting us by big leaps closer to our ultimate goal.

Kuhn's account of revolutions is very different from the liberal or the Hegelian one. There is no historical necessity governing particular scientific developments. There is no cumulative progress towards an ultimate

---

cover even minor conceptual changes, the so called "micro-revolutions" (see note 29 below), is himself committed to "revolution in perpetuity."

25. If this is correct, then I. B. Cohen's criticism that, historically speaking, crises do not always precede revolutions (1985, p. xvi) is off the mark. Accordingly, and *pace* Wray (2007), I don't take McMullin's (1993) claim that a variety of cases can be described as revolutions as a criticism of Kuhn. Toulmin (1972, pp. 115–116), on the other hand, recognizes the logical character of Kuhn's distinction between normal science and revolution, but criticizes him for disguising it "in an irrelevantly historical fancy dress."

26. Popper's defense of revolutions may be explained by his desire not be left behind by all the hype of revolutionary talk occasioned by Kuhn's *Structure,* but also by his opposition to dogmatism and commitment. As Lakatos put it (1970, p. 92), to Popper, commitment was considered an "outright crime".

27. In his (1981) Popper again stresses the "conservative or traditional or historical element" in scientific progress which he relates to scientific instruction. Cf. also Popper (1992, p. 49): "[some dogmatic thinking] is to some extent necessary . . . If we accept defeat too easily, we may prevent ourselves from finding that we were very nearly right."

end. Commitment to tradition propels, instead of inhibiting, change. The paradigm after a revolution is not, objectively or neutrally speaking, cognitively superior to the one which is superseded.[28] What is more, scientific revolutions remained for Kuhn unpredictable events, unaccountable by piecemeal rational reconstruction no matter how many, rare or frequent, local or global.[29] Kuhn understands revolutions as the re-drawing of taxonomic categories which results from re-conceptualization, from handling of the *same* data in new ways.[30] They are seen as transformations of vision similar to visual change in Gestalt experiments (Kuhn 1970, pp. 85, 111, 112).[31] Exactly because revolutions are not normal extensions and articulations of scientific practice (Kuhn 1970, p. 84) or piecemeal argued changes (Kuhn 2000a p. 28), Kuhn compares them to phenomena of religious conversion.[32] When the scales fall from one's eyes, one sees the world anew. Then one starts to build a new world-picture by following new exemplars.

28. It is true that Kuhn in the "Postscript" to the *Structure* (1970, p. 206) acknowledges that a later theory is better than earlier ones in terms of puzzle-solving ability. Yet, I believe that this is a defensive move on his part to come to terms with his critics since by singling out this criterion, he goes against his claim, which he never withdrew, that successive theories or paradigms are incommensurable even as regards the puzzles they generate and the criteria for their solution.

29. In the 1959 article "The Essential Tension" Kuhn says that "revolutionary shifts are relatively rare" (1977a, p. 227). Yet, in the Introduction of the *Structure* (1970, p. 6) he explains that the defining characteristics of scientific revolutions "can also be retrieved from the study of many other episodes that were not so obviously revolutionary." Also, later, responding in his "Postscript" to his critics who charged him with massive leaps of faith and a distorted image of science, he tried to make his model less threatening and more conforming to familiar ideas about science. He split scientific communities and multiplied revolutions (1970, pp. 180–181, pp. 249–250). "My concern, in short, has never been with scientific revolutions as 'something that tended to happen in a given branch of science only once every two hundred years or so'. Rather it has been throughout 'a little studied type of conceptual change which occurs frequently in science and is fundamental to its advance'" (Kuhn 1970a, pp. 249–250).

30. The idea of re-conceptualization persists in Kuhn's work. In that sense, his early understanding of revolutions in relation to thought-experiments and in the *Structure* is not so different from his understanding of revolutions as taxonomic changes in his late work.

31. Kuhn repeatedly mentions in his texts the experience he had reading Aristotle. Aristotle appeared to him as "a dreadfully bad physical scientist" (Kuhn 2000a, p. 16; cf 1977, pp. xi, xiii). But, at some point, quite suddenly, "[t]he fragments in my head sorted themselves out in a new way, and fell into place together. My jaw dropped, for all at once Aristotle seemed a very good physicist indeed, but of a sort I'd never dreamed possible. [. . .] That sort of experience—the pieces suddenly sorting themselves out and coming together in a new way—is the first general characteristic of revolutionary change that I shall be singling out . . ." (Kuhn 2000a, pp. 16–17).

32. I. B. Cohen (1985, p. 56) notes that "the noun 'revolutio' had the sense of 'conversio' in classical Latin." He goes on to claim (pp. 10, 464, 467–472) that conversion

The analogy between what is going on in the sciences and what is taken to be typical in religion (mysticism, faith, conversion, persuasion)—a domain which was aspired to be annihilated by science in the first place—was what proved to be particularly bothersome to Kuhn's critics. Equally bothersome is the fact that nothing typically rational (something resembling a proof) or objectively ascertainable (e.g., the prospect of more accurate knowledge) marks, according to Kuhn, the advent of revolution. Revolutionary changes are not ushered in by some kind of necessity, as the result of pressure exercised on scientists by facts. Rather, coming suddenly after a rather dogmatic commitment to well-entrenched rules, revolutions may seem to be a matter of arbitrary decision on the part of the scientists, a leap in the dark or a leap of faith.

Kuhn, of course, has a whole chapter in the *Structure* entitled "The Nature and Necessity of Scientific Revolutions." One might think that, there, he fends off the accusations of arbitrariness and chance. But the necessity he speaks of in this chapter is not some worldly empirical necessity which compels scientists to change their beliefs and practice. Nor is it some historical necessity of the Hegelian type.[33] Kuhn speaks as a philosopher contending that, if science is to develop as a practice, there need to be scientific revolutions. If we need science to move ahead beyond known applications (and it has to go beyond them to be called science), then, there need to be revolutions to accommodate the anomalies that will emerge.[34] And anomalies *will* emerge because the tools provided by the paradigm will fall short of what is required for the solutions of puzzles which, although mapped after a paradigm, expand by definition beyond it. Revolutions, then, do not occur because scientists strike upon some important truth which compels them to revise radically their antecedently upheld beliefs. Rather, they are dependent upon the recognition of anomalies

---

is a feature of revolutions in science. "[W]hoever reads the literature of scientific revolution cannot help being struck by the ubiquity of references to conversion" (1985, p. 468). Still, Cohen is not concerned by possible irrational implications of this metaphor. Kuhn, speaking of conversion, appropriates for his schema the sudden and complete change of perspective whereas Cohen stresses more the zeal of someone who has seen the light (the truth).

33. Nor is it of the type embraced by I. B. Cohen. Cohen says (1985, p. 21) that "revolutions in science are inevitable, in that they cannot be prevented, at least as long as science continues to exist, although they may have to await the arrival of a particular revolutionary genius to ignite the fuse. [. . .] But the pace of such revolutions, or their frequency of occurrence, may be slowed down or speeded up." His idea is that as long we have funds for instruments, jobs and fellowships, we increase the "likelihood that a revolutionary genius will be in the right place at the right time" (1985, p. 21).

34. It is like a tautology, as he admits (1970, p. 100; cf. Kindi 2005) and as he has been criticized (Hanson 1965). Cf. here Fried (1998a, p. 219) and Cavell (1976a, p. 210) for a similar understanding of advances in art.

which are dependent, in turn, on the normalcy established by well entrenched rules. And they succeed when new rules are imposed. Thus, Kuhn's point about revolutions is more conceptual than historiographical. He is not saying that science, as a matter of historical fact, has progressed through particular revolutions, but rather that the practice of science, as it has been observed in history, cannot but advance through revolutions.[35] This is the necessity he speaks of.

Let me summarize what I have done so far: I think I have established that there was a close contact between Cavell and Kuhn and one significant direction of influence from Kuhn to Cavell as regards the importance of tradition to radical innovation. I have, then, pointed out that in Cavell's case revolutionary breaks are connected somehow to the pursuit of the essence of art and the restoration of its integrity. Restoration was originally part of the meaning of 'revolution' but not in the modern understanding of the term, which is associated with radical break and cumulative progress. Kuhn's understanding of revolution may bear some resemblance to the modern notion, namely as regards radical break, but it also has differences: there is no cumulative progress,[36] the development is blind, there is no necessity other than one verging on the analytic. In that sense, his concept of revolution may be taken to be literally postmodern.[37]

V. Are Kuhn's and Cavell's accounts really different?

What does the above discussion say about the relation between Kuhn and Cavell on the issue of revolution and essence? We saw that Cavell, but also Fried, claims that revolutionary artists need to look back to their tradition and discover the criteria and essence of their art. They need to clean it of

35. It may be claimed that Kuhn makes here an essentialist point: that it is in the essence of science to advance through revolutions. It is better to say, I think, that the thesis about revolutions is an implication of the significance of tradition in the practice of science. Scientific education, and the tradition it gives rise to, is not part of the essence of science but rather a condition—which may eventually take other forms—, that makes the practice of science, and its implications, possible.

36. Kuhn has said that he is "a convinced believer in scientific progress" (1970, p. 206). But his idea of progress is very different from the standard one because, in his view, progress is not cumulative and does not take us ever closer to some ultimate truth. Again, according to Kuhn, as I see it, progress is, so to speak, analytically related to scientific change. A new paradigm would not have been accepted unless it was thought to constitute progress over the previous one. That does not mean that we have progress "objectively" assessed from a cosmic exile perspective.

37. Nickles (2003, p. 7) calls Kuhn "postmodern because premodern" because, according to Nickles, Kuhn portrays scientific communities "as surprisingly like medieval guilds with their masters and apprentices learning by example." I call Kuhn literally a postmodern (that is, coming temporally after modernity) because he modifies the modern idea of revolution.

all the added rust and dust and *establish new conventions which preserve it* (as strange as this may sound). Kuhn, on the other hand, has no patience for essence talk. He stresses variability and the clean break with the past once a new paradigm is accepted (however much it carries over). But are the two accounts really as different as I have presented them so far?

First of all, the differences I have highlighted may reflect differences in the respective disciplines—art and science—, and not differences in the approaches Kuhn and Cavell adopt. Scientists, in a sense, leave the past behind while the artists try to measure up to it. "Unlike art, science destroys its past," says Kuhn (1977c, p. 345; cf. Kuhn 1963, pp. 352–353). The two thinkers give us two different accounts of revolution which fit two differing fields: one requires that some essence of the past is recovered or restored, as Cavell put it in relation to art and to one particular moment in its history—the modernist revolution—and the other requires breaking with tradition, as Kuhn maintains in relation to science. But how could we explain, then, the fact that Cavell invokes Kuhn and his insistence on the significance of tradition to account for radical novelty in art? What is more, Cavell does not think the two disciplines are so different (1976a, p. 183).

Another possibility is the following: perhaps, when Cavell talks of exploring the limits or the essence of one's art, he is just giving us elements of ideology, the convictions of artists who *think* they are going after essence trying to redeem their art when, in fact, what they do is effect a radical change. In that respect revolutionary artists may be compared to revolutionary scientists who do not deliberately undertake to begin a revolution but who believe, as revolutionary artists do, that they are merely correcting their faltering legacy and accumulating knowledge (Kuhn 1977a, p. 227). One, that is, may compare what Cavell says about the relation of revolution to its past with the way Kuhn describes the handling of the past by the advocates of a successful paradigm. The defenders of the winning paradigm will maintain that the past is preserved in the new theory, usually as a limiting case. Yet, I do not think that Cavell is giving us the ideology, or false consciousness, of the artists themselves. It is his own assessment of the situation and something that, one might say, he recommends. What he is saying is that only the artists who are trying to measure up against past achievements of art are likely to produce genuine pieces of painting, music or poetry. The criteria of genuineness are not known a priori, they are not given. In their effort to find them, revolutionary artists may re-invent criteria (1976a, p. 202) which will even redefine essence. Despite his talk of discovery and essence, Cavell says explicitly that he is not describing an essentialist project:

> To say that the modern "lays bare" [the essential characteristics] may suggest that there was something concealed in traditional art which hadn't, for some reason been noticed, or that what the modern throws over—tonality, perspective, narration, the absent fourth wall, etc.—was something inessential to music, painting, poetry, and theater in earlier periods. These would be false suggestions. (1976b, p. 220)

What Cavell wants to point out by referring back to the past and highlighting its importance in the search for limits and essence is that one cannot simply extend the concept of art or broaden its definition to accommodate the paradoxical novel cases in some kind of normal, cumulative manner (1976b, p. 213). New developments force us not only to consider and evaluate what presents itself as art now, but also to re-consider and re-evaluate what we used to take as art before. The products of revolutionary artists do not lie on the periphery or at the fringes of an already given, fixed and firm concept of art asking to be included in the inner circle, to get accepted or to be tolerated. They claim authority on what art is and, from a position of power so to speak, they demand to be taken seriously as central. They compel us to rethink and alter our *whole* conception of art and not just to accommodate some cases.[38] They force us, that is, to engage, in re-conceptualization in a more sweeping way. And this is not far from what Kuhn was trying to do.

According to Kuhn, re-conceptualization is what revolution brings about in science. The new paradigm takes central stage and defines again what science is (1970, p. 109), not just now, but always. The pressure for re-conceptualization, in both cases, art and science, does not come from without;[39] it is not, that is, an invasion by a foreign enemy but a revolution, an uprising from within, which seeks to restore order, reconstitute

38. Cavell developed these thoughts responding to Monroe Beardsley's and Joseph Margolis' suggestions as to how to deal with the problem that modern art poses. Beardsley suggested a broad definition of art to include the problematic cases while Margolis treated the problematic objects as borderline cases. Cavell says: "[R]edefinitions and borderline cases are irrelevant here. For the question raised for me about these new objects is exactly whether they are, and how they can be *central*" (1976b, p. 215). The problem is not, he says, "to find some definition of 'sculpture' which makes the Caro pieces borderline cases of sculpture, or sculpture in some extended sense. The problem is that I am, so to speak, stuck with the knowledge that this is sculpture, in the same sense that any object is" (1976b, p. 218). Cavell's (and Fried's) Wittgensteinian understanding of essence is discussed by Costello (2008, pp. 288–295).

39. Cf. here Greenberg's distinction between criticism from the inside and the outside (1982, p. 5) and also his insistence on the self-definition of each art. Also Cavell's question (1985, p. 519): "Do professionals really want to be helped to see themselves by outsiders?"

order by imposing a new order.[40] The two thinkers struggle with the same problem, that of radical novelty, but, admittedly, highlight different, even if complementary, aspects of the revolutionary process: Cavell highlights revisionary restoration and Kuhn highlights break.

VI. Problems

Caroline Jones (2000, p. 523) maintains that the evolutionary package in Kuhn's work, that is, his use of the evolutionary metaphor at the end of the *Structure,* can lend itself, and has lent itself, to essentialist and teleological projects in the arts. This is more the case, in her view, with Fried, who, according to Jones, was helped by Kuhn to form a teleological definition of Modernist progress and less so with Cavell who managed "to articulate other notions of aesthetic meaning that proved far more flexible to the ongoing production of contemporary art." Jones thinks that Kuhn, on the one hand, and Fried and Cavell on the other, are all engaged in a more or less, essentialist enterprise because they identify science and art respectively with the dominant paradigm of each practice.[41] For instance, Fried and Cavell reject minimalist art and Pop art respectively as not art at all because they were developed in total disregard of the tradition of modernist painting (Jones 2000, pp. 518, 520). "One cannot choose to work outside a governing paradigm in science and still be doing science. One cannot choose against working in the Modernist tradition and still be making art," says Jones interpreting Cavell and Fried (2000, p. 522).

I do not think that the three thinkers are involved in an essentialist project. Caroline Jones takes engagement with tradition—a requirement all of them set—as necessarily constituting puzzle-solving activity in the tradition of the dominant paradigm whereas I, as I explained earlier, take engagement with tradition to mark also radical advancement and revolutionary change. Forming a tradition based on a paradigm is not essential-

---

It should be noted, however, that although Kuhn typically insists on the importance of being immersed in a tradition in order to be able to change it, he also allows for the possibility of an outsider contributing to radical change. He himself was a case in point.

40. Conant (2005, pp. 69–70) and Cavell (2001b, pp. 264–265) both invoke Arendt to stress the significance, in relation to revolution, not so much of breaking with the prior state of affairs but more importantly of instituting something radically new in its place.

41. Jones says that the equivalent of modernist painting in art (*the* art) is physics in science (*the* science) (Jones 2000, p. 520). But I fail to see how one can compare the two with regard to the Kuhnian paradigm. Even if one admits that modernist painting constitutes the paradigm of art how could the whole discipline of physics play the same role in science? Hasn't this discipline displayed, according to Kuhn, a number of different paradigms?

ist and is not a barrier to change. Actually, on my reading it is a condition of radical change.[42] Kuhn, Cavell and Fried need engagement with tradition so that a break with established conventions is not an irrelevancy. Still, their approach faces a number of problems, some of which are highlighted in Jones's paper, and need to be addressed:

- How do we know whether a particular advancement in a practice follows the tradition and is a puzzle in this tradition or if it marks the beginning of a new tradition, setting a new exemplar or paradigm?
- How do we know if taking liberties from established conventions and rules is an aberration or a mistake, if it is bad science or art, if it is improvisation or simply nonsense?[43] Is it a matter of some kind of experience, as Cavell seems to think (1976b, p. 215)?
- How can we tell that a revolution has taken place, especially if one takes into account that revolutions may be invisible (Kuhn 1970, p. 136), noiseless (Macaulay 1973, p. 84) or long (Williams 1961)? The Scientific Revolution, for instance, has supposedly lasted for 200 years. And if revolution lasts that long how does it differ from Reichenbach's "rapid [or not so rapid] evolution" which he understands, I think, not in the Darwinian sense but as compressed accumulation in a rather short period of time, which turns out not to be so short after all (cf. Kuhn 1970, p. 7)?
- What happens if revolutions are multiplied, as Kuhn seems to suggest, and, correspondingly, if the respective normal sciences that predate them shrink? Some revolutions, he says, will seem as the Balkan revolutions to outsiders: that is, as normal parts of the developmental process. If we have many revolutions, do we fall back to the contradictory concept of 'revolution in perpetuity' (2000d, p. 295)?[44]

42. There are other points of disagreement with C. Jones as regards her interpretation of Kuhn which I cannot pursue here. For instance, Kuhn's evolutionary metaphor does not at all imply a teleological account. Kuhnian evolution, like the Darwinian one, is driven from behind and its development is blind. Also, as noted above, Kuhn's understanding of progress is not at all the standard idea of continuous accumulation and improvement towards a pre-established end.

43. Cf. here Kant's discussion of "original nonsense" in relation to genius in (2001, §46) and (1974, § 57).

44. Note that Cavell (1976a, p. 205) also makes reference to the notion of 'permanent revolution' and Fried is credited with the same view (Costello 2008, p. 288, n. 25; Meyer

- Is revolution a matter of hindsight, a term in the analyst's toolkit, or do the revolutionary agents themselves need to know that they are attempting and undertaking a revolution? Do they need to know or just to show that they are struggling with the past? How does what they say relate to what they do? Is it *their* judgment or *our* judgment that a revolution has occurred?
- And, finally, is 'revolution' just an honorific term, part of modernity's rhetoric, or does the occurrence of a revolution produce concrete results which would not have come about had it not taken place? Steven Shapin, for instance, begins his book entitled *The Scientific Revolution* with the statement: "There was no such thing as the Scientific Revolution and this is a book about it." (1996, p. 1).

I cannot address all these issues in the paper. But all of them, to a greater or lesser degree, are related to, are expressions of, the much debated problem of rule following which is usually discussed in connection to Wittgenstein's philosophy. That is, how do we know that a certain rule is being followed, when does a new application of a rule set a new rule and a new tradition, when can we speak of a mistake, an aberration or simply unintelligible behavior or nonsense. Stanley Cavell has contributed himself immensely to the discussion of this topic. Here, I will only say briefly in what direction, I think, both Kuhn and Cavell go, or I think they would go, to deal with these questions. Both thinkers stress that reconceptualization and the establishment of new criteria, new rules or new conventions is not a matter that can come about briskly and conveniently, as if these were mere opinions loosely held which can be simply changed at will, arbitrarily, and at no cost. "[O]nly someone outside an enterprise could think of it as a manipulation or exploration of mere conventions. Very little of what goes on among human beings, very little of what goes on in so limited an activity as a game, is *merely conventional* (done *solely* for convenience)," says Cavell (emphasis in the original, 1979, p. 119).[45] In following rules, being exposed to exemplars, one is not just learning words and concepts in the abstract, one is learning about the world too. As Cavell put it: "In 'learning language' you learn not merely what the names of things are, but what a name is; not merely what the form of expression

2000, p. 75). Greenberg (1999, p. 52) contends that "A tradition in art keeps itself alive by more or less constant innovation."

45. Cf. Wittgenstein (1953, p. 230): "Compare a concept with a style of painting. For is even our style of painting arbitrary? Can we choose one at pleasure? (The Egyptian, for instance.)"

is for expressing a wish, but what expressing a wish is; not merely what the word for 'father' is but what a father is" (1979, p. 177). Kuhn also said: "When the exhibit of examples is part of the process of learning terms like 'motion', 'cell', or 'energy element', what is acquired is knowledge of language and of the world together. [. . .] In much of language learning these two sorts of knowledge—knowledge of words and knowledge of nature—are acquired together, not really two sorts of knowledge at all, but two faces of the single coinage that a language provides" (2000a, p. 31; cf. 1977b, p. 253). So, if this is true, if that is, one acquires language and the world together, one cannot just lightly manipulate and change the conventions or offer new or broader definitions to cover difficult cases. Every move has a cost, every move produces effects which cannot be easily handled. It's not that the world, as some gigantic passive stuff, resists our careless manipulations and follies.[46] There is resistance from language itself, says Kuhn (2000a, p. 32). Language may be tolerant allowing various projections but it is also intolerant, says Cavell (1979, p. 182)—we cannot do anything we like with it, we cannot apply it to anything we fancy. This does not mean that our skeptical concerns are silenced or that we have managed to find a place to neatly accommodate nature in this whole schema, a concern that haunted Kuhn till the end (2000c, p. 110; 2000d, p. 317). What it means, I think, is that we take huge responsibility in what we do and what we bring about. The new is not something we stumble upon which we need to seize so that a revolution occurs; it is not something that is waiting for us in the future, like an iceberg which will emerge as we come closer, something that we will find by looking ahead leaving our past and our current condition behind; it is something *we* initiate,[47] *we* bring about by looking, in a way, backwards, to our history and our past achievements.

VII. Conclusion

The aim of this paper has been to show that the emphasis that Thomas Kuhn laid on the significance of commitment to tradition[48] as regards revolution in science, has contributed to a similar conception of radical nov-

46. Cf. Wittgenstein (1969): "Why, would it be *unthinkable* that I should stay in the saddle however much the facts bucked?" (OC 616).

47. Cf. Greenberg's claim that "the limiting conditions of art have to be made altogether human limits" (1982, p. 9).

48. Of course Kuhn was not the first to underline the significance of tradition (conservatives like Edmund Burke and Michael Oakeshott had done this already). The novel thing was the connection between tradition and revolution, something that conservatives would not do.

elty in the arts, especially in relation to the work of Stanley Cavell. The observations of both thinkers are not historiographical in nature; they do not aim to offer historical accounts of what goes on in the arts and sciences. They consider particular revolutionary developments (for instance, Modernism in art or the Chemical Revolution in science) in order to understand and elucidate what radical novelty involves. Both art and science are marked by the high value they attach to the mastery of a tradition which, according to Kuhn and Cavell, far from committing these disciplines to a confining conservative practice, constitutes the condition for bringing about revolutionary changes. Scientists, confronted with anomalies (i.e., deviations from what tradition inclines them to expect), seek to restore order while artists, striving for originality and excellence, measure up to the standards of past achievements. In their effort to recover what has been lost, practitioners of both fields break new ground and, consequently, redefine their respective disciplines. This is not an attempt to uncover an already given, atemporal, essence but a dynamic process of reorganization and re-conceptualization. Cavell's talk of essence must be seen in this light. In his view, essence is redefined with each attempt to preserve it. The result is not a continuous, piecemeal advancement, but a re-vision involving breaks and the redrawing of boundaries. Here, Cavell's analysis again meets Kuhn's, who understood revolutions in science as re-conceptualizations.

References

Arendt, Hannah. (1963) 1990. *On Revolution.* London: Penguin Books.

Burtt, E. A. (1924) 1952. *The Metaphysical Foundations of Modern Science.* Atlantic Highlands, NJ: Humanities Press.

Butterfield, Herbert. (1949) 1957. *The Origins of Modern Science.* New York: The Free Press.

Cavell, Stanley. (1969) 1976. *Must We Mean What We Say?* Cambridge: Cambridge University Press.

———. (1967) 1976a. "Music Discomposed." Pp. 180–212 in *Must We Mean What We Say?* Cambridge: Cambridge University Press.

———. (1967) 1976b. "A Matter of Meaning It." Pp. 213–237 in *Must We Mean What We Say?* Cambridge: Cambridge University Press.

———. 1979. *The Claim of Reason.* Oxford: Oxford University Press.

———. 1985. "The Division of Talent." *Critical Inquiry* 11, 4: 519–538.

———. 2001a. "On Wittgenstein." *Philosophical Investigations* 24, 2: 89–96.

———. 2001b. "Impressions of Revolutions." *The Musical Quarterly* 85(2): 264–273.

Cohen, H. Floris. 1994. *The Scientific Revolution: A Historiographical Inquiry.* Chicago: The University of Chicago Press.

Cohen, I. Bernard. 1976. "The Eighteenth-Century Origins of the Concept of Scientific Revolution." *Journal of the History of Ideas* 37, 2: 257–288.

———. 1985. *Revolution in Science.* Cambridge, MA: The Belknap Press of Harvard University Press.

Conant, James. 1989. "An Interview with Stanley Cavell." Pp. 35–72 in *The Senses of Stanley Cavell.* Edited by Richard Fleming and Michael Payne. Lewisburg: Bucknell University Press.

———. 2005. "Cavell and the Concept of America." Pp. 55–81 in *Contending with Cavell.* Edited by Russell B. Goodman. Oxford: Oxford University Press.

Costello, Diarmuid. 2008. "On the Very Idea of a Specific Medium: Michael Fried and Stanley Cavell on Painting and Photography as Arts." *Critical Inquiry* 34, 1: 274–312.

Dworkin, Ronald. 1982. "Law as Interpretation." *Critical Inquiry* 9, 1: 179–200.

———. (1986) 1991. *Law's Empire.* London: Fontana Press.

Fried, Michael. 1982. "How Modernism Works: A Response to T. J. Clark." *Critical Inquiry* 9, 1: 217–234.

———. (1965) 1998a. "Three American Painters: Kenneth Nolland, Jules Olitski, Frank Stella." Pp. 213–265 in *Art and Objecthood.* Chicago: The University of Chicago Press.

———. (1966) 1998b. "Shape as Form: Frank Stella's Irregular Polygons." Pp. 77–99 in *Art and Objecthood.* Chicago: The University of Chicago Press.

———. 2001. "Response to Caroline Jones." *Critical Inquiry* 27, 4: 703–705.

Greenberg, Clement. 1961. *Art and Culture.* Boston: Beacon Press.

———. (1960) 1982. "Modernist Painting." Pp. 5–10 in *Modern Art and Modernism: A Critical Anthology.* Edited by Francis Frascina and Charles Harrison. New York: Harper and Row.

———. (1976) 1999. "Convention and Innovation." Pp. 47–58 in *Homemade Esthetics: Observations on art and taste.* Oxford: Oxford University Press.

Hall, Rupert, A. (1954) 1966. *The Scientific Revolution, 1500–1800: The Formation of the Modern Scientific Attitude.* Boston: Beacon Press.

Hanson, N. R. 1965. "A Note on Kuhn's Method." *Dialogue* 4: 371–375.

Horgan, John (ed.). 1991. Interview with T. S. Kuhn. "Reluctant Revolutionary. Thomas S. Kuhn Unleashed 'Paradigm' on the World." *Scientific American* 264: 14–15.

Hull, David L., Peter D. Tessner, Arthur M. Diamond. 1978. "Planck's Principle." *Science* 202, 4369: 717–723.

Jones, Caroline A. 2000. "The Modernist Paradigm: The Artworld and Thomas Kuhn." *Critical Inquiry* 26, 3: 488–528.

Kant, Immanuel. (1793) 2001. *Critique of the Power of Judgment.* Edited by Paul Guyer. Translated by Paul Guyer and Eric Matthews. Cambridge: Cambridge University Press.

———. (1798) 1974. *Anthropology from a Pragmatic Point of View.* Translated by Mary J. Gregor. The Hague: Martinus Nijhoff.

Kindi, Vasso. 2005. "The Relation of History of Science to Philosophy of Science in the *Structure of Scientific Revolutions* and Kuhn's Later Philosophical Work." *Perspectives on Science* 13, 4: 495–530.

Kuhn, Thomas, S. (1962) 1970. *The Structure of Scientific Revolutions.* Chicago: The University of Chicago Press.

———. 1963. "The Function of Dogma in Scientific Research." Pp. 347–369 in *Scientific Change (Symposium on the History of Science, University of Oxford, 9–15 July 1961).* Edited by A. C. Crombie. New York: Basic Books & Heinemann.

———. 1970a. "Reflections on My Critics." Pp. 231–278 in *Criticism and the Growth of Knowledge.* Edited by Imre Lakatos and Alan Musgrave. Cambridge: Cambridge University Press.

———. 1977. *The Essential Tension. Selected Studies in Scientific Tradition and Change.* Chicago: The University of Chicago Press.

———. (1959) 1977a. "The Essential Tension: Tradition and Innovation in Scientific Research." Pp. 225–239 in *The Essential Tension. Selected Studies in Scientific Tradition and Change.* Chicago: The University of Chicago Press.

———. (1964) 1977b. "A Function for Thought Experiments." Pp. 240–265 in *The Essential Tension. Selected Studies in Scientific Tradition and Change.* Chicago: The University of Chicago Press.

———. (1969) 1977c. "Comment on the Relations of Science and Art." Pp. 340–351 in *The Essential Tension. Selected Studies in Scientific Tradition and Change.* Chicago: The University of Chicago Press.

———. 2000a. "What Are Scientific Revolutions." Pp. 13–32 in *The Road since Structure.* Edited by James Conant and John Haugeland. Chicago: The University of Chicago Press.

———. 2000b. "The Road since *Structure.*" Pp. 90–104 in *The Road since Structure.* Edited by James Conant and John Haugeland. Chicago: The University of Chicago Press.

———. 2000c. "The Trouble with the Historical Philosophy of Science."

Pp. 105–120 in *The Road since Structure.* Edited by James Conant and John Haugeland. Chicago: The University of Chicago Press.

———. 2000d. "A Discussion with Thomas Kuhn." Pp. 255–323 in *The Road since Structure.* Edited by James Conant and John Haugeland. Chicago: The University of Chicago Press.

Lakatos, Imre. 1970. "Methodology of Scientific Research Programmes." Pp. 91–196 in *Criticism and the Growth of Knowledge.* Edited by Imre Lakatos and Alan Musgrave. Cambridge: Cambridge University Press.

Macaulay, Thomas Babington. (1828) 1973. "History and Literature." Pp. 72–89 in *Varieties of History.* Edited by Fritz Stern. New York: Vintage Books.

McMullin, E. 1993. "Rationality and Paradigm Change in Science." Pp. 55–78 in *World Changes: Thomas Kuhn and the Nature of Science.* Edited by Paul Horwich. Cambridge, MA: MIT Press.

Melville, Stephen, W. 1986. *Philosophy Beside Itself: On Deconstruction and Modernism.* Minneapolis: University of Minnesota Press.

Meyer, James. 2000. "The Writing of 'Art and Objecthood'." Pp. 61–96 in *Refracting Vision.* Edited by Jill Beaulieu, Mary Roberts and Toni Ross. Sydney: Power Publications.

Nickles, Thomas. 2003 "Introduction." Pp. 1–18 in *Thomas Kuhn.* Edited by Thomas Nickles. Cambridge: Cambridge University Press.

Pieper, Josef. (1970) 2008. *Tradtion.* Wilmington, Delaware: ISI Books.

Popper, Karl. (1963) 1992. *Conjectures and Refutations: The Growth of Scientific Knowledge.* London: Routledge.

———. (1973) 1981. "The Rationality of Scientific Revolutions." Pp. 80–106 in *Scientific Revolutions.* Edited by Ian Hacking. Oxford: Oxford University Press.

———. 1994. *The Myth of the Framework.* London: Routledge.

Pound, Ezra. 1934. *Selected Poems. Edited with an Introduction by T. S. Eliot.* London: Faber & Faber Ltd.

Read, Herbert. (1933) 1937. *Art Now. An Introduction to the Theory of Modern Painting and Sculpture.* New York: Harcourt, Brace and Company.

Reichenbach, Hans. 1951. *The Rise of Scientific Philosophy.* Berkeley: The University of California Press.

Rorty, Richard. (1980–81) 2005. "Cavell on Skepticism." Pp. 10–21 in *Contending with Cavell.* Edited by Russell B. Goodman. Oxford: Oxford University Press.

Shapin, Steven. 1996. *The Scientific Revolution.* Chicago: The University of Chicago Press.

Toulmin, Stephen. 1972. *Human Understanding.* Princeton: Princeton University Press.

Williams, Raymond. 1961. *The Long Revolution.* New York: Columbia University Press.

Wittgenstein, Ludwig. 1953. *Philosophical Investigations.* Translated by G. E. M. Anscombe. New York: Macmillan.

———. 1969. *On Certainty.* Edited by G. E. M. Anscombe and G. H. von Wright. Translated by Denis Paul and G. E. M. Anscombe. New York: Harper & Row Publishers.

Wray, Brad K. 2007. "Kuhnian Revolutions Revisited." *Synthese* 158: 61–73.