This article traces certain rhetorics of knowledge-change as well as a few models of such change. In particular, it focuses on models that emphasize novelty and sudden transformation. To this end, the works of Thomas Kuhn, and the debates surrounding his celebrated modeling of the paradigm, are explored. Having established – at least in an illustrative fashion – the role of novelty in Kuhn’s philosophy of science, we then look more briefly at the mid-career work of Michel Foucault (his Order of Things and the Archaeology of Knowledge), and the debate between Jürgen Habermas and Jean-François Lyotard to find (in Foucault’s case) analogies with the earlier models and debates surrounding Popper and Kuhn, and then (in the Habermas/Lyotard discussion), to see how revolutionary and reactionary status count in assigning value to models of knowledge. In all these inquiries, we seek less to criticize particular theorists (that has already been done) than to understand a dominant strand of understanding of knowledge and knowledge-change in the contemporary academy.

URL: http://researchoutput.csu.edu.au/R/-?func=dbin-jump-full&amp;object_id=39079&amp;local_base=GEN01-CSU01 http://dx.doi.org/10.1177/0191453711421603

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CRO Number: 39079
Imagine a race in which the task involves being ever more radical or revolutionary, or even novel, than one’s predecessors. It’s not so far from the world we actually inhabit. From morning newspapers to evening news programs, we are regaled with tales of progress, breakthroughs, discoveries, and revolutions in crucial matters of this and that. It is not just a matter of advertising new contraptions or of commercial exploitation. Nor is it just media sensationalism. It applies also to scholarship. Universities, supposed repositories of knowledge, are themselves preoccupied with novelty. They organize research programs and restructure teaching curricula in the competitive knowledge-market. In so doing, they seek to orient scholarship and learning towards cutting-edge rupture and breakthrough. Such bureaucratically underwritten novelty-competitiveness is part of a wider modern agonistic scene. Vincent Descombes put it very well when he wrote that

> It is a fact of utmost importance that the greatest thinkers of our age seem to be engaged in a race in which they continually ‘outstrip’ one another…. Each of them acts as though his most illustrious predecessor were a rival to be bested through a kind of one-upmanship: a more radical point of departure, a more ‘original’ breakthrough, a more all-encompassing totalization. (1993: 100)

In many fields of inquiry, novelty is explicitly or implicitly incorporated into the actual structure of justification. Something can be deemed meritorious simply because it is new. The basis of the orientation is now so long-standing that we confront a paradox: the novelty is itself an orthodoxy.

Novelty itself implies a model of knowledge-change entailing rupture. To be sure, not all novelty entails large scale transformation, and not all models of knowledge entail rupture at all. Indeed, until the seventeenth century, most versions of knowledge emphasized tradition, and did not value the disruptive force of innovation. There are ironies indeed in seeking to write about the sudden appearance of an apparent belief in the reality of rupture and innovation in knowledge – though these are less peculiar than the contention
that these were more than ascription. A short and brilliant paper by René Girard, titled ‘Innovation and Repetition’, made this precisely this point. Girard goes so far as to call the change a ‘cult’:

The new cult meant that a new scourge had descended upon the world – ‘stagnation’. Before the 18th century, ‘stagnation’ was unknown; suddenly it spread its gloom far and wide. The more innovative the capitals of the modern spirit became, the more ‘stagnant’ and ‘boring’ the surrounding countryside appeared….In an amazingly short time, a systematically positive view of innovation replaced the systematically negative one. Everything was reversed and even the least innovative people found themselves celebrating innovation. (11)

Many gradual or even purely imaginary versions of knowledge-change were quickly characterized in terms of radical transformation, rupture, or revolution, especially after the onset of the nineteenth century. In former times, many of the more mundane sorts of shifts would have been equally well characterized as adhering to traditions and precedents – the value lay in the claim, rather than the substance, of novelty.

The rhetoric of change raises other questions too. In our view, some of the most profound insights into our conceptions of change are locatable in the debates about the nature of science. In this essay, we examine debates which took place between Karl Popper and Thomas Kuhn and their various supporters) almost half a century ago. If the debates are half-forgotten, key ideas are remembered – or at least invoked, and this is part of the way in which this view has developed. We are especially interested in the popularity of Kuhn’s work. We trace Kuhn’s ‘revolutionary account of revolutions’, his model of the paradigm. Kuhn wrote about how new knowledge, in the narrow sense of the word, comes about, and in so doing, somehow convinced most people that he had successfully accounted for it.

It is hardly for us to pretend to venture a new critique of Kuhn or the other writers surveyed here. This phase of analysis has long been concluded, and by writers in a range of fields. Instead, we wish to re-stage all this, to see the contribution and scope of the debates to our conceptions of knowledge and of science. In so doing, we offer a different
kind of critique of a deeper underlying belief-structure concerning knowledge-change. From this point of view, Kuhn’s model and all those we treat as his successors (like Foucault’s epistemes) are deeply problematic. Therein, however, lies the rub: we have to figure out why something like the Kuhnian model proves so widely attractive as an epistemology. His idea that knowledge-paradigms change, and change utterly, has proven resilient, even though his presentation of the notion of ‘paradigm’ itself is notoriously elusive. His work is so widely accepted that it is often seen as no more than common sense. It even contributed to a popular redefinition of the role of philosophy vis-à-vis other intellectual fields. At the level of reception, his work has enjoyed popularity rarely seen in theoretical work in the humanities: according to Stanley Fish, writing in the late 1980s, The Structure of Scientific Revolutions was ‘arguably the most cited book in the humanities and social sciences in the last twenty-five years’ (Fish 1989: 486).¹

We fully recognize that Kuhn drew on already-existing notions of revolution. He draws on socio-political revolutionary rhetoric when he terms his own models of change ‘scientific revolutions’. The idea of ‘revolution’ was worked hard in the nineteenth and twentieth centuries, applying not just to violent insurrection, but also, to radical changes in life, such as industrialization. For some, such revolutions appear more important than the writing of a book like Kuhn’s Structure of Scientific Revolutions (hereafter SSR), or the debates of the Vienna circle. If so, the importance of these ‘real revolutions’ is already visible in the headlines of newspapers, and in the books of history that are based upon them. While we do not treat them, we do not seek to minimize their significance; rather, we would point out that they are correlated with those we do treat. We would further contend that the importance of underlying conceptions like knowledge-change is readily under-emphasized, and less well understood than the grand march of commerce, or politics.

Later in the essay, we trace related models of knowledge-change. In the Order of Things, Michel Foucault, like Kuhn before him, attempted both to classify and to achieve groundbreaking shifts of his own. When it first appeared in France, this book was a best-seller (Foucault Society), and his subsequent works (especially the first volume of The
History of Sexuality and Discipline and Punish, remain influential). In the Order of Things, he felt the need to ‘abandon the great divisions that are familiar to us all. I did not look in the seventeenth and eighteenth centuries for the beginnings of nineteenth century biology (or philosophy or economics)’ (1970: x). This is a claim about Foucault ‘going it alone’ and not a declaration that he, any more than Kuhn, abandons the notion of change. Quite the contrary: ‘my main concern has been with changes. In fact two things in particular struck me: the suddenness and thoroughness of change’ (1970: xii). Foucault’s totalization was, as we shall see, at once blatant and yet artfully cloaked in rhetorics of region and specificity.

Knowledge-revolutions also entail a certain temporal stability within which things can be overturned. Foucault, at times, showed awareness of this. In this journal, Laura Hengehold contended that such ‘staged revolutions’ imply a problematic of temporality. Her reading of the later Foucault sees a questioning of the very horizon of time and the event it would enclose, and this especially in relation to the putative ‘revolutionary’ event (2002: 346). Indeed, spectators to history struggle to grasp the significance of events, unless it is somehow staged for them (2002: 339-41). This threatens to collapse the opposition between progress and rupture-based accounts of knowledge-change, suggesting to us at least, that the staging is part of the effect, and indeed, of the event itself. Yet Foucault in all phases of his career, was not beyond offering his own revolutionary rhetoric, even if this sometimes involved little more than mocking earlier revolutionary modelings (such as those by Marx) as trapped in discursive naivety.

What follows is laid out in three broad sections – the first deals with Kuhn (and his celebrated interlocutor, Karl Popper); the second analyses and situates Foucault in relation to the first fuller analysis. The final section, in the same spirit, looks briefly at a more recent – and archetypal – ‘controversy’ concerning Jürgen Habermas and Jean-François Lyotard. This leads us finally to more general speculations about epistemological ‘revolutions’, and a fuller explanation of why we might concur with Descombes that this agonistic scene is indeed ‘a fact of utmost importance’ (1993: 100).
Kuhn – The Historical Importance of the ‘Paradigm Paradigm’

While Kuhn’s work itself, doesn’t represent some absolute ‘rupture’ in the history and philosophy of science, we contend that it is central to any understanding of thought about knowledge in the later twentieth century up to the present time. One doesn’t have to share its conclusions (however these might be specified) to agree that Kuhn’s work led to a widely-accepted redefinition of the role of philosophy vis-à-vis other fields, such as the sciences. Construing our world in this way—rendering it and having it rendered for us—in terms of paradigms and revolutions, of breaks, ruptures, and incommensurability, is everywhere around us. Ours is indeed, still a world according to Kuhn.

Our first task therefore entails understanding both the significance of Kuhn’s work and the context within which it appeared. Even though the passage of years has dimmed precise memory of the fact, Kuhn’s work appeared in social and epistemic contexts of its own. He wrote in the turbulent wake of the period of Wittgenstein and the Vienna circle. At that time there was a shift in the philosophy of science in relation to its object, with many seeing it as primarily descriptive, not normative. Hence, where Karl Popper, like the philosophers of the Vienna Circle, offered a normative criterion of scientific rationality, falsifiability, for the purposes of assessing the rationality or scientificity of any claim with empirical entailments, Kuhn, by contrast but not on his own, appeared to endorse the descriptive view that the shape of scientific rationality could not be so easily seized in advance. Kuhn was not alone in his contentions: along with a range of other influential thinkers in the twentieth century, he thought philosophy to be an excellent servant of science, but a very poor master.

As for the theory itself within this framing backdrop, in large part, Kuhn’s contribution to the philosophy of science can be attributed to the apparent cogency of two challenges he made regarding scientific history itself. He challenged the ideas that: (1) science—epistemologically speaking—exemplified a continuous, progressive series of achievements; and (2) that ‘theories’ were the only aspect of science deserving of rational philosophical examination. Kuhn argued—with varying degrees of success—that what passes for ‘scientific rationality’ is considerably broader than the scientific theories of
which it is only partly constituted. Rather, he advanced an account of scientific history in which vast periods of scientific activity were characterized initially by relatively stable periods of research (‘normal science’), followed by times of wholesale upheaval and epistemic discontinuity (‘scientific revolutions’). In Kuhn’s worldview, normal science is governed by near-universally accepted ‘paradigms’: the collective, and often tacit, field of theoretical and practical commitments shared by a community of scholars.

In positioning Kuhn contra Popper (see Popper 1975: chap. 2), Kuhn himself argued that when conflicts (Popper’s ‘falsifications,’ Kuhn’s ‘anomalies’) between theory and observation arise, they do not precipitate any straightforward undermining of the theoretical grids onto which they are mapped. Historically, such anomalies have been regularly dealt with by scientists on an ad hoc basis. That is, if irregular or few anomalies arose, other explanations were advanced to service the attempt of preserving the theory or theories that the anomalous phenomena ostensibly infirmed. In other words, scientific theories were not infirmed by anomalies. It would take a host of these at multiple levels as well as the existence of seemingly viable explanatory rivals to provoke a ‘crisis’ – and so the preconditions of a true change. The subsequent transition, the shifting of the theoretical allegiances of the scientific community to a new (previously ostracized) scientific paradigm, which would then become a part of ‘normal’ science, Kuhn dubbed a ‘scientific revolution’.

From Kuhn’s view of knowledge-change as revolutionary in nature, certain things follow. In particular, Kuhn’s critique of Popper’s ‘progressivism’ is at least partly predicated on the idea that rival paradigms are ‘incommensurable’ with each other: they possess neither internal nor external standards which are common to both (1970: 94-5, 111-35). That is, the world-constitutive function of paradigms entails that, between rival paradigms, there can be no unequivocal employment of theoretical terms or ‘observation statements’.

In its context, another contribution of the theory involved how knowledge is understood as being situated. That is, the question of the fate of a theory at the hands of a particular disciplinary community is de-coupled by Kuhn from the status of its representational
fidelity to the ‘real world’ and is instead replaced by locating the paradigm shift in the context of the dynamics of social psychology. Rather than being taken up by a community of scholars on the basis of its greater veracity or veridicality, the appearance of the new paradigm can be largely accounted for by a collective switch, what one might venture to call a kind of epistemological realignment through *conversion* (150): ‘explanation must, in the final analysis, be psychological or sociological’ (Kuhn in Lakatos 1970: 21).

Ironically, even at its inception, Kuhn struck paradoxical notes of normative theory of scientific change. The ‘paradigm shift’—the epistemic rupture, the knowledge revolution—seemed to become not only an *object* of theoretical reflection but an *objective* of it: not simply a phenomenon to be described, but a goal to be realized - perhaps of all possible criteria, the *sine qua non* of philosophical lucidity. And then, in the final twist, the widespread adoption of the paradigm in this way meant that for a time at least it too achieved this value even if its destiny involved the role of conferring its own ascendency. It is to these aspects of the theory that we now turn.

**Problems with Paradigms: The Black Box, Incommensurability, Denotation**

Imagine for a moment the dour philosopher as magician. Pride of place goes not to his assistant, but to his magic black box, the paradigm – a miracle of modern engineering which explains almost everything, stands for almost anything, can be as large as an episteme, as small as an idea.iii Standing, as it were, at the center of Kuhn’s hermeneutics—as a kind of organizing trope for *SSR*—the notion of ‘paradigm’ is a notoriously ambiguous concept. Is a paradigm something like a *Weltanschauung* or world-view (1970: 43-44)? A gestalt (1970: 113-4)? A pedagogical tool or ‘textbook example’ (1970: 44, 10)? A theory (1970: 78)? A disciplinary matrix (1970: 182)? Equally, little is achieved if, instead of combing *SSR* for possible conceptual clarifications or definitions of the term which appear, we choose to attempt—inductively as it were—to disencumber this seemingly crowded set field by extracting meanings of the term as they emerge through *use*—in individual discussions of particular scientific episodes.
But we’re thwarted here, too: Kuhn uses the term paradigm to refer both to the differences between the Ptolemaic and Copernican worldviews (along with all their ideological, political, and theological correlates) and to the development of Lavoisier’s oxygen theory of combustion! Astonishing flexibility for all those who internalize it: but even today, we must raise questions about widespread valorization of such a rubbery model, whose workings are in any case conveniently concealed from us.

The black box effect relies upon vagueness in its version of historiography; it entails that it always confirms successes. That is, Kuhn thinks that, from a state of disagreement, one school of thought—advocates of a particular scientific paradigm—ultimately reigns and its rival lies vanquished, fit only for storage alongside other quaint oddities of yesteryear, whose only interaction with the world of the living is as an epistemological museum exhibit, presented for that kind of titillation and ego bolstering that only archaisms seem to be able to supply. After all, surely Darwin defeated Lamarck…but is it really that easy? Not if we look at the new work in genetics; not if we look at the theory of ‘memetics’ in the work of contemporary neo-‘Darwinists’ (both fields seem to suggest that acquired characteristics can be inherited); not if we examine the neo-vitalism of the French zoologist Pierre Grasse. Outside the paradigm-model, even if we partially accept its merits, the story is never so simple.

Some of the strangest inconsistencies of Kuhn’s work emerge when tested not by cases from scientific history, but when tested against the conclusions of his own philosophical work, and observations he makes about his work. At various points Kuhn presents and defends his own theoretical project (including its viability in the light of other philosophers’ resistance to it), by incorporating and interpreting it within the ambit of the operations of Kuhn’s own theory itself. At one point, and in response to Popper, Kuhn argues that his own theoretical reflections on scientific history, as captured in SSR, represent overall a new paradigm for that field, and are therefore ‘incommensurable’ with Popper’s philosophy. This tack produces some peculiar results. Witness Kuhn’s account of this:
We shall not, I suggest, understand the success of science without understanding the full force of rhetorically induced and professionally shared imperatives….That he [Popper] does not…ever see them for the socio-psychological imperatives that they are is further evidence of the gestalt switch that still divides us deeply. (‘Logic’, 1970: 22).

Kuhn is here attempting to travel in two opposite directions simultaneously, epistemologically speaking. In the first instance, Kuhn’s claim relies on him ‘understanding’ Popper’s philosophy; if not, how then could he compare it to his own? On the other hand, given Kuhn’s attempt to situate the disagreement between himself and Popper in terms drawn from his own theory, it is difficult to see—given Kuhn’s notion of ‘the given’ as always already paradigm-dependent—how he could claim, as he clearly does, that his own paradigm explains the ‘data’ (i.e. history of science) better than the so-called ‘traditional’ view. And although Kuhn does not quite spell out what his rebuttal of Popper means exactly, his implicit reasoning here tends to affirm the view that the history of science is, at least, constituted by some non-paradigm determined facts which Kuhn’s own explanation purports to be able to explain more satisfactorily. This, if interpreted with fidelity to its own precepts, would entail a most paradoxical conclusion: the success of Kuhn’s argument here would, by virtue of the same, thereby entail a demonstration of the inadequacy of the theory on which it is based.

Another set of problems with Kuhn’s work reveals much about our penchant for cultural mirages: it concerns how we imagine ourselves as differing utterly not just from each other, but also, from our own imagined pasts. In Kuhn’s own work it concerns the application of paradigm-logic to episodes in scientific history. But in all such discussions (whether Kuhn’s or those that follow in the same vein), a manifest absurdity quickly displays itself. Discussions which claim to attest to the radical incommensurability of theories appear, albeit unwittingly, to employ a hermeneutic framework in which rival paradigms are also in the very moment of their supposed incommensurability, situated in relation to each other. In its extreme form, this would undermine the veracity of the explicit framing by assuming, if for no other reason than comparison is possible between them, that they are—in some respect at least—commensurable. In such cases, the very
claim that ‘rival’ paradigms are ‘incommensurable’ loses cogency. That is, if we may be forgiven the caricature staging it entails: if the thesis of incommensurability were correct, then members of other cultures, including seventeenth-century scientists, would be conceptualizable by us only as animals producing responses to stimuli (including noises that curiously resemble English or Italian). To tell us that Galileo had ‘incommensurable’ notions and then go on to describe them at length is totally incoherent (Putnam 1983: 193).

One could indeed press further here and ask to what extent Kuhn’s model of scientific change disallows the very possibility of the appearance of empirical anomalies. This would be so by virtue of his idea that all observations are determined by theory, and that they possess no significance outside this determination. But surely—and one could frame this objection either in concrete terms of examples or simply in terms of inconsistency in the paradigm model—it is precisely because of the relative autonomy or independence of phenomena, of ‘data’ from their framing by paradigmatic framework, that anomalies are able to surface in the first instance. If Kuhn is correct about this, then it would appear that no one could be in a position to discern precisely what the paradigm shift amounted to, since every element of the previous paradigm—from its putative realia and ‘logic’, through to its observation sentences and regulative ideals—may have undergone a radical change.

For Kuhn (and others like the American logician W.V.O. Quine), the particular character of paradigms is shaped, at least in part, by their being semantically indeterminate—even meaningless—without being embedded in particular, correlative, theoretical frameworks. In this view, the determinate denotative capacities such terms possess emerge conditionally upon theoretical specification. This kind of intra-linguistic figuration of semantic reference is part of a theory of representation that is widely endorsed in theoretical work in the humanities. It should not pass unchallenged, even in this essay. That is to say, while Kuhn’s is not as extreme as some later versions of it, there are a number of serious questions that one should raise about any theory predicated on such a
conception of reference – and the best way to challenge it is to raise an exemplary counter-hypothesis.

In particular, denotation is not always tied to theoretical specification without remainder. Rather, we can cite the notion of partial denotation—a process whereby theoretical terms undergo *denotational refinement*—as evidence of another possibility. As the name implies, the idea is simple enough. Partial denotation occurs where the set of objects and/or relations that a theoretical term partially denote after the scientific revolution is a proper subset of the set of objects and/or relations that it partially denoted before it (Field 1973: 497). One of the advantages of partial denotation as semantic theory is that it refuses equally naïve realism—any simple equivalence between different terms (or the same term) employed by different conceptual schemes—while avoiding the conclusion, inherent in certain theses of incommensurability, and perhaps anathema to a wide variety of intellectual work—that signs cannot even be compared. The notion of partial denotation makes good sense of some ‘scientific history’; indeed, it sometimes seems that the theory of partial denotation is actually being deployed by Kuhn even when the framing heuristic invokes the language of radical incommensurability.

We can illustrate the hermeneutic fecundity of adopting a syntactics of partial denotation through a simple example. Let us compare variations in denotative content of the term ‘mass’ entailed by situating it, firstly, in the world of classical (Newtonian) physics and then, secondly, in a world in which by Einstein’s theory of special relativity is operative. Kuhn is right to observe that Newton’s theory of mass doesn’t stand to Einstein’s in a relation of identity. From the purview of special relativity, Newtonian mass is referentially indeterminate: without further specification, the term could refer either to ‘proper mass’ (total energy / $c^2$) or ‘relativistic mass’ (nonkinetic energy / $c^2$). Such a distinction is not available within Newtonian physics, and hence we concur with Kuhn here that ‘mass’ does not possess identical denotative capacity across the paradigm shift. But what does this mean? For after showing that Newtonian mass and Einsteinian mass are not identical it is not enough to assert a case of denotative chalk and cheese. It might be correct—although analytically insufficient—to label the two theoretical objects
‘Newtonian mass’ and ‘Einsteinian mass’ and leave it at that, as Kuhn does. But this *obscures* important levels of denotational overlap. We know, for instance, that for most intents and purposes, Newtonian mass is an adequate and largely accurate rendering, regardless of whether it is further specified as to mean proper or relativistic mass; moreover, such statements do *not* remain true if we substitute other words for mass, like ‘phlogiston’, ‘regret, or ‘mysterious ether’. Drawing here on the work of Hartry Field, we can affirm that Newton’s term ‘mass’ partially denoted proper mass and partially denoted relativistic mass—and didn’t partially denote anything else (1973: 476).iv

To repeat, we are not the first to raise concerns about Kuhn’s hypotheses, and neither do we seek to diminish his astonishing achievements. Quite the contrary: it is the ‘success’ of the theory that interests us. Kuhn’s own attempts to address the concerns of his critics bear noting, as do his attempts to encourage caution in—or proclaim distance from—self-appointed apologists. Kuhn also seeks to resolve a number of dilemmas brought to his attention appear shortly after the publication of the first edition in a postscript to the second edition of 1969, and then later, in the essays which comprise the greater part of *The Essential Tension*. But even here, we should be on guard when Kuhn resorts to the (Quinean) notion of ‘radical translation’ to answer queries about incommensurability, if for no other reason than the now-familiar one that a successful defense of the former entails the preclusion of consideration of the other. Kuhn’s capacity to answer his detractors is consistently impressive and occasionally formidable; what can be lost in the heat of battle, however, is that success secured through polemical cunning is purchased only by killing his original thesis by a thousand qualifications.

**Foucault**

In the 1960s academic context within which Foucault wrote, the badge of revolution had been associated with structuralism, existential phenomenology, and Marxism. Like Kuhn, Foucault’s task would be to sweep aside entrenched forms of epistemic historiography. We are of course well aware of the distinctiveness of the approaches of the two writers. We could evoke the legion of ‘discourse’ analysis, Foucauldians, and ‘genealogical’ accounts of this or that within the field of cultural studies to indicate a field of influence.
As this is rather more recent history, we can take a little of this for granted, though we will, in the final sections of the essay, return to the debates of our time. Suffice to say, the influence of Foucault is not yet the golden light of suffusion that the now general term ‘paradigm’ radiates, but is still that of a theorist, albeit a highly influential one.

Let us begin by tracing key commonalities in the approach of the two writers to normativity (they are both theorists of the rupture; they both seek to abandon that conception of philosophy as supplying the normative basis of rationality). Like Kuhn, Foucault sought to erect in place of the earlier edifices his own new system of envisaging and describing the world. And as for those who did not recognize the shift, they were ‘half-witted commentators’ who ‘persist’ in ‘labeling’ him (a structuralist) — that indeed, he has been ‘unable to get it into their tiny minds that I have used none of the methods, concepts, or key terms that characterize structural analysis’ (1970: xiv). In other words, perhaps, the greatest danger to a revolution like Foucault’s is not that it might be wrong-headed, but rather, that people might not have actually noticed that it has taken place.

Foucault’s approach involves sweeping claims about the past, and our relationship to it. In the most ingenious form of disavowal of all, however, Foucault actually builds the denial of generality beyond the strictly delimited sphere of discovery into the approach. His will be a ‘regional’ analysis (1970: x). By this, he means he will focus on the particulars and the activities of discourse, and not overwrite them within an explanatory ethos from a subsequent era. Instead of denying positivity, this is redefined; instead of refusing generality, generality itself is problematized and, if the approach is taken seriously, actually described as an effect of other forces that it will be the artifice of the archaeologist to reveal. On the other hand, all this will not be recognizable as a micro-study such that a regional historian or geographer might generate. On the contrary, he promises to let us see that, for instance:

Within a few years (around 1800) the tradition of general grammar was replaced by an essentially historical philology; natural classifications were ordered according to the analysis of comparative anatomy; and a political economy was founded whose main themes were labour and production. (1970: xii)
The changes did not, he says, ‘obey the same laws’; his account will reveal the shifts (in their specificity), and without regard for the usual generalizations that govern such discipline-based histories (xii). To the obvious charge that his is basically a variation on the long-standing ‘history of ideas’ tradition, he devotes a measured chapter in the *Archaeology of Knowledge* (1972: 135-40).vi

One of the most novel aspects of Foucault’s work is the way he sidesteps what he saw as outmoded models of causality, which will be ‘set to one side’ (1970: xiii). A causeless account, one that will not be a symptomatology of deeper things, this then is the project of *The Order of Things*. The laughter we share as he reads us Borges’ account of an imaginary taxonomy presages the project, one which will be ‘not so much a history, in the traditional sense of that word, as an archaeology’ (1970: xxii). As a study situated within a discipline, say the history of ideas, one is given contexts for achievement. The fact that methods and theories themselves can be quasi-anthropologically surveyed as part of the subject matter of the inquiry is a profound hypothesis, and one we readily admit to admiring. But Foucault also wanted to overturn the framework within which one might situate his own work, offering us a heroic self-portrait as one unprecedented in the field.vii

In Foucault’s revolutionary self-description that excited such attention when it first appeared, the field of history of ideas, the approaches of phenomenology, structuralism, and Marxism are explicitly disavowed. We have seen already the vehemence of the denial of structural methods. We have seen too the refusal of the history of ideas. But we can reasonably object, using the tenets of Foucault’s own ideas, that his work can be expected to be riddled with the logics of structuralism, as well as problems – and problematics – of the history of ideas. It is certainly true that Foucault does not use the *terminology* of structuralism, or of phenomenology, or of Marxism.viii But this does not prevent appurtenance between what he does and what structuralists (or epistemologists etc) do; terminological opaqueness does not render the approaches incommensurate. In fact, the focus on discontinuity and on rupture at particular sites is not *necessarily* far removed from the anthropological dimension of certain variants of structuralism in that
they too found wide assertions on observations of cultural black holes, such as taboos. Similarly, the structuralist refusal of surface causal explanation finds its equivalent in Foucault’s approach, except that he advances it as an unprecedented insight of his own. We are not seeking to claim that, after all, Foucault was, deep down, a structuralist (or whatever). Rather, we wish to remark that his claim that he is never general, indeed never theoretical in the old sense, is not established simply because of a method based on rupture and discontinuity.

This brings us to the tension between the claim of specificity and the broad sweep of claims made. Foucault’s version of the black box permits the same kinds of ordinarily inadmissible incongruities as Kuhn’s, but in a different manner. In brief, we can outline it as follows. *The ‘hypotheses’*: that the view that fields of knowledge evolved over time, gradually coming into focus as the fields of knowledge we inhabit today is a modern misrepresentation of what earlier modes of knowing involved; that theories themselves can be analyzed like any other form of statement; that we do better to view what was actually said rather than what we would like to impute a deep significance of it to be; that the fields of knowledge are discontinuous with each other (and that disciplines paper over these differences by giving a narrative of surface continuities). *The approach*: a focus on what is actually said and done from the point of view of repeatability derived from a study of selected interdiscursively positive systems (epistemes). In the *Archaeology of Knowledge*, after outlining the approach, Foucault then spells out what he sees the new approach as claiming. Under the heading the ‘comparative facts’, he recapitulates as follows:

Our aim was not to show that men in the eighteenth century were generally speaking more interested in order than in history, in classification more than development, in signs than the mechanisms of causality. Our aim was to reveal a well-determined set of discursive formations that have a number of describable relations between them. These relations do not spill over into adjacent domains and they cannot be brought closer to the totality of contemporary discourses, even less to what is called ‘the Classical spirit’; they are closely confined to the triad being studied, and are valid only in the domain specified. (1972: 158)
We cannot over-emphasize the importance of the last qualification strongly enough. This limitation is what sets the new method off from its predecessors, is what makes it so novel. Foucault realized it too. He goes on to specify what would and would not be a valid critique of his work:

There are those who would say, ‘Why did you not speak of cosmology, physiology, or Biblical exegesis? Could not pre-Lavoisier chemistry, or Euler’s mathematics, or Vico’s history have invalidated all the analyses to be found in *The Order of Things*?... I will reply: of course, I not only admit that my analysis is limited; I want it so; I have made it so. (1972: 158)

Again the passage is important: the claim’s strength is not founded in its sweep of the century or the time, but rather, is confined to the three domains identified (the analysis of life, of wealth, of language), and no other. Foucault then goes so far as to say that were these tendencies to be found more widely, this would actually be an objection to his approach for then he would not have described a ‘region of interpositivity’ but rather ‘the spirit of science of a period – the very thing to which my enterprise is opposed’ (1972: 159).

When in the first part of *The Order of Things*, Foucault talks of the classical period, then, we understand him to be working within this interpositive regionality of the three defined discursive domains, and not the entirety of the time/place. For us, as readers of this text, we fully accept that despite occasional lapses, Foucault largely adheres to this task. But in the second part of the book, and indeed, in many works after these two books, Foucault makes statements that are far beyond the range of the three discourses themselves. Part two of the book seems to promise (and indeed begins substantially to deliver) more meditations on the way the next episteme construed language, wealth, and life. But Foucault does not confine himself to these three domains. He will speak of metaphysics, of the unconscious, of kingship, of the left, of mathematics, of sociology, and at the end of the book, of the figure of ‘man’ per se.

The figure of man, for Foucault, is less than four centuries old. Its field of interpositive discursive regionality is suspended between ethnology/anthropology on the one hand, and
psychoanalysis on the other. These fields, as we have seen, he has asserted to be linkable to the fields of human sciences, and thence by a roundabout route, to the three fields of language, wealth, and life. We have not accepted this very tenuous claim. But why does Foucault make the claim at all? It is hardly germane to his thesis. This is the basis of the claim:

That psychoanalysis and ethnology are not so much two human sciences among others, but that they span the entire domain of those sciences, that they animate its whole surface, spread their concepts throughout it, and are able to propound their methods of decipherment and their interpretations everywhere. No human science can be sure that it is out of their debt. (1972: 379)

Suspended there, the figure of man is nevertheless not transparent or discoverable even by these fields of inquiry. At this point, Foucault plays his trump card: language-play, as in philosophy then current, reveals a crisis in ‘man’ and perhaps, he suggests, since language is here once more, man will return to that serene non-existence in which he was formerly maintained by the imperious unity of Discourse…One thing in any case is certain: man is neither the oldest nor the most constant problem that has been posed for human knowledge….As the archaeology of our thought easily shows, man is an invention of recent date. And one perhaps nearing its end. (1972: 386-87)

The suggestion is breath-taking in its totalizing sweep—unless, of course, he means us to take it that the situation might be different in another field of discursive interpositivity, wherein ‘man’ may have existed, say for six hundred years, or for twenty years, or maybe for two millennia. Our point here is very simple: he clearly does not mean this. The assertions about ‘man’ are intended to refer to the widely conducted analysis of the nature of the human subject, and while this is a highly suggestive assertion, it is no way consistent with the stated method of the *Archaeology*.IX

We conclude our treatment of Foucault with his attempt to take on the Marxist orthodoxy of his time. Adopting the garb of the super-revolutionary, Foucault outflanks Marxism this way:
The alternative offered by Ricardo’s ‘pessimism’ and Marx’s revolutionary promise are probably of little importance. Such a system of options represents nothing more than the two possible ways of examining the relations of anthropology and History as they are established through economics though the notions of scarcity and labour…these are merely derived differences. (1972: 261)

This claim allows Foucault to jab Marxism in the eye. He does not pass up the opportunity:

At the deepest level of Western knowledge, Marxism introduced no real discontinuity….Marxism exists in nineteenth century thought like a fish in water; that is, it is unable to breathe anywhere else…their controversies may have stirred up a few waves and caused a few surface ripples; but they are no more than storms in a children’s paddling pool. (1972: 261-62)

This may be true. But the difficulty is that Foucault has not shown it to be so. Indeed, in one sense, he ought not to have shown it to be so: his, after all, is a work devoted not to the ‘depths’ of knowledge, let alone to what is or is not radical political change or truth, but rather, to showing regularities and irruptions of knowledge. Marx, by Foucault’s chronology, stands in the early period of a modern episteme. In terms of the analysis of wealth, and its imbrications with language and living beings, we might accept the assertion. But does Foucault mean it to be so restricted? Is there not also a comment on the nature of history, of politics, indeed, of society itself that is needed for us to make sense of the passage cited above? For if not, if we are to read it as merely a comment on economic theory, then it would not need the controversial language, the contemptuous dismissal. But languages of transgression and change are at times more important than what is actually said, as we will now see.

Revolutionary Clothes: Habermas, Lyotard, and the Honor of the Name

We have confined our consideration to two writers of the 1960s whose work we both respect and admire. Kuhn wrote an account of knowledge-change in the sciences. His theory of paradigms was one of utter change (and this is part of the basis of its popularity). His rhetoric of change is deployed in such a way as to show its readership that contrary to appearances, change really did take place at this or that particular point.
His ‘paradigms’ approach was designed to supplant all other explanations of knowledge-change. These things were also true of the work of Michel Foucault.

At this point, we need to think about the garb of the change-maker, the revolutionary, and beyond this figure, the wider figurative firmament. For the revolutionary is ghosted by its doppelganger antonym, the conservative. In grasping the rhetorics of revolution, we have reserved this task for last: the understanding that the knowledge-revolutionary be a controversialist, if not an activist. One such controversy is that of the Lyotard/Habermas ‘disputes’ of the early eighties. This ‘controversy’ was at times serious, absurd, doctrinaire, challenging. It is widely understood as a simple ‘dispute’, an idea we will ourselves dispute (hence the inverted commas). It still has currency in theoretical debates today.

Strange as it may seem, one characteristic of such controversies is that they do not have obvious starting points (or as Girard would have it in relation to rivalries of the same, a starting point would be redundant). Most who know it, do so because of Lyotard’s explicit attack on Habermas in his 1982 essay in *The Postmodern Explained to Children* (1985). In it, he summarizes the work of a ‘reputable thinker who defends modernity against those he calls neoconservatives…Jürgen Habermas (you will have recognized him) thinks that if modernity has floundered, it is because the totality of life has been left to fragment’ (1992: 12). This is already a defense structure; it is already a response. The source of the attack, and hence the assailant who started it would be Habermas. It bears saying that Lyotard was not, at this stage, the subject of any attack of Habermas. But the defense is deployed against a more general attack on postmodernism by Habermas. His essay, ‘Modernity an Incomplete Project’, published subsequently in *Theory of Communicative Action*, models radical postmodernism on the nineteenth century stance of the young Hegelians attacked by Marx. Thus, The ‘young conservatives’ recapitulate the basic experience of aesthetic modernity. They claim as their own the revelations of a decentered subjectivity, emancipated from the imperatives of work and usefulness, and with this experience they step outside the modern world. (1993: 107)
Habermas offers a figurative firmament consisting of new-conservatives (economistic, rationalistic, rightist), old conservatives (those nostalgic for bygone times), and young conservatives (apparent revolutionaries who are not what they appear).

So Habermas started it. Well, not quite. In the 1984 English edition of Lyotard’s highly inventive Postmodern Condition, the illustrious postmodern theorist straddling both sides of this fence (you will by now have recognized him as Fredric Jameson) reminds us that the book develops ‘a thinly veiled polemic against Jürgen Habermas’ concept of a ‘legitimation crisis’ and vision of a ‘noise free’ ‘transparent’, fully communicational society’ (1984: vii). When we look at this work, we do indeed find close scrutiny of key Habermasian ideas, such as legitimation (1984: 36-47), as well as Lyotard’s claim that consensus systems are terroristic in nature (1984: 63-64). As for who is ‘right’ in this argument, even those sympathetic to Lyotard will notice that ‘what Habermas owes to Kant is precisely what Lyotard denies him and he does so in Kant’s name’ (Watson 1984: 13). Finding fire amongst all the smoke (or is it fog?) is difficult.

For these, more than the analyses of Kuhn or Foucault, really are staged revolutions. Both writers adopt the rhetorical cape of the revolutionary, and the confirming arena within which they trail it is the academic controversy. But is either of them revolutionary? Let us briefly consider the claims. Lyotard’s revolution is ironically phrased, albeit explicit:

Beneath the general demand for relaxation and appeasement, we hear murmurings of the desire to reinstitute terror and fulfill the fantasm of taking possession of reality. The answer is: war of totality. Let us attest to the unpresentable, let us activate our differends and save the honour of the name. (1992: 25)

It sounds like a battle. But it does so in much the way that one might imagine in one of J.K. Rowling’s imaginary escapades devised for the heroes of Hogwart’s castle. Habermas might be a ‘Marxist’, but one hardly imagines a revolution following from the labours of his scholarly tracts. That is not to say the rhetoric is absent. As Jean-Pierre Dupuy remarks
The world of ideas is a lot like the world of men. It has fighting for territory, cutthroat competition, struggles for prestige and recognition, jealousy, fear, and mutual fascination. Of course, when it is theories, hypotheses, conjectures – i.e. abstractions – which slaughter each other, it is not the same as if it were men. (1994: 110)

So neither of these writers is revolutionary; neither offers a model of change. The talk of change in Habermas is a sentimental garment of long dormant Marxism, in Lyotard, it is an ironic and yet still absurd posture of the once-ambitious academic. Lyotard’s desire to ‘activate the differend’ reminds us of the scale of what divides the two writers. It is widely assumed that the differend is real, that there really was a major and substantial difference between the two thinkers. Yet Habermas’ late works (cf 2001) have been about a ‘crisis’ in legitimation (not increasing recognition), about problems in consensus, not our progress towards it (cf. Steuermannn 1992: 100). And Lyotard, as Habermas points out:

Still implicitly presupposes the image of a fragmented reason, whose shards are dispersed over many incommensurable – or partially overlapping – discourses.

But if there is no such thing as a form of reason that can transcend its own context, then the philosopher who presupposes this same picture may not lay claim to a perspective that allowed him this overview. (1989: 150)

Their approaches are not, of course, identical. Habermas’ achievement is the institutionalization, indeed the regularization of many of the insights into modern society developed by Adorno and Benjamin. Lyotard’s contribution in this field has been to question the value we place on consensus when, at its limit points, it is not possible, nor ‘always’ desirable.

There is certainly nothing revolutionary about these writers, let alone their disagreements. The supposed novelty, though, has effects of its own on academic work as such. Girard has remarked on this tendency in the following way:

All modern thought is falsified by a mystique of transgression, which it falls back into even when it is trying to escape…. In the currents of thought that have dominated us for a century, there is one tendency that we must never forget: the
fear of being regarded as naïve or submissive, the desire to play at being the freest thinker—the most ‘radical’, etc. As long as you pander to this desire, you can make the modern intellectual say almost anything you like. This is the new way in which we are still ‘keeping up with the Joneses’ (Girard 1987: 287).

Such mystique does not help inquiry, and mere novelty does not, of itself, equal knowledge.

Louis Dumont has suggested that the revolutionary’s pose has its genesis in post-Kantian debate. Dumont traces the motif of ‘Steigerung’ (intensification) in many currents of modern—particularly German—philosophical thought; for him, ‘Steigerung can be taken as the watchword of the whole process: a movement of intensification, of surpassing’ (1985: 598). Although the term was employed readily by the Romantics, it was by no means limited to them. Dumont frames post-Kantian European thought in a series of internecine rivalries that could be seen to begin with Kant and his three successors, Fichte, Schelling, and Hegel (1985: 598-99). Here he captures some kind of primal scene: ‘As if by common agreement, they followed him only to outdo him [Kant]… What he had been at pains to distinguish and separate, they were above all keen to unite. The boundaries he had set as definitive they immediately professed to transgress’ (1985: 598). Kant attempted to open and to settle epistemological questions once and for all. This attracted the fascination of rival thinkers; his own desire to start afresh, ‘proceeding precisely on the lines of Copernicus’ primary hypothesis’ (Kant, ‘Preface’, 22) and to achieve ‘what many centuries have not been able to accomplish; namely, to secure for human reason complete satisfaction in regard to that which it has all along so eagerly occupied itself, though hitherto in vain’ (Kant 1929: A 856, B 884, 669) opened a floodgate of sorts. Moreover, what Kant lay down as regulative ideals, subsequent thinkers claimed as possessions; what Kant had referred to as intellektuelle Anschauung [intellectual intuition]—a form of knowledge available only to God—his rivals claimed to have realized themselves.

Perhaps therein lies the rub we described at the outset: novelty as orthodoxy, revolution as pose. In Kuhn’s attempt to offer a descriptive model of knowledge-change we see
something well worth conceiving and attempting. In his paradigm, or Foucault’s episteme, and most especially, Lyotard’s attack on totalities, we see many shortcomings. But in the attempt to outline or sketch a field, wherever generously offered or even glimpsed in shadowy outlines, we partake momentarily in the honor, the credibility even, of the name.
References


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\[1\] Similarly, Popper’s conceptions of falsification and rationality are embedded in popular conceptions of what counts as knowledge, particularly scientific knowledge.

\[2\] The perceived novelty of Kuhn’s own model also is attributed partly to the fact that it stood as a significant challenge to then widely-endorsed philosophies which grew out of the British empirical tradition: phenomenalism and logical empiricism. These philosophies could not countenance the idea that perception was ‘always already’ theoretical: that ‘seeing’ always involved seeing as or seeing that. For Kuhn, to the contrary, there are no neutral, paradigm-independent, ‘facts’: rather, ontic commitments are present in all perceptions—putative sensory ‘givens’, that is, bear no epistemic significance until they are interpreted.

\[3\] Commonly used in philosophy of science, the term ‘black box’ denotes ‘a single entity with an unknown structure…which realizes some known function’ (Audi 1999: 90).

\[4\] A more recent - and very readable – elaboration of these issues is to be found in Paisley Livingston’s *Literary Knowledge*.

\[5\] We cannot cover all Foucault’s work, or even a representative sample. Of the juvenilia, we can say nothing. Of the later, and in our view generally inferior genealogy, we say little, except where we must qualify claims we wish to make. Of the dross on governmentality and the rash of inferior histories called ‘genealogies’ by Foucault acolytes, we say only this. The essay, ‘On Governmentality’, was a fine sketch, a useful index of a fine and inquiring mind. It analyzed aspects of Renaissance and subsequent political thought, and proposed a systematization. The reduction of this suggestive essay to a series of control-theoretical formulae about subjection does no credit to Foucault and has little to do with his work’s real accomplishment. Our focus will be on a companion pairing, *The Order of Things* and *The Archaeology of Knowledge*. Our interest is in the status of claims made (how wide is the model supposed to be in its extension), as well as the characteristic maneuver of sweeping away that which precedes it.

\[6\] Broadly, he distinguishes them because, unlike the history of ideas, archaeology does not seek meaning buried in discourse, but rather seeks to reveal discourse itself; where the history of ideas posits a continuous history, this is not posited by archaeology; archaeology does not reify oeuvres and artists; and it does not rely upon a restitution of the identity/being of the ones who uttered the statements that structure the discourse (1972: 138-40). Yet it could be responded to all these contentions that, given the history of ideas is supposed to be about ideas rather than subjects or continuous traditions, that Foucault’s main achievement is a truer form of a flawed discipline. It could also be said that his view of change as something producing utterly new terrain is, for all the novelty of its formulation, something the history of ideas has always (wrongly, we believe) taken for granted, the difference lying only in the rapidity and totality he ascribes to the process.

\[7\] One of us has taken an aspect of this issue up. See Chris Fleming and Jane Goodall, ‘Dangerous Darwinism.’ *Public Understanding of Science* 11 (2002): 259-71.

\[8\] He seems, quite deliberately, to have selected a vocabulary drawn from eclectic sources: linguistics (statements, discourse, enunciation, subject, object), philosophy (episteme, regularity), even geology (archaeology) and librarianship (archive).

\[9\] Later, one might imagine, Foucault might have qualified these claims. So in a way he did. Realizing the fact that all writers (including especially himself) tend to impose their values on what they read into the past, he switched from archaeological to genealogical apparatuses. But the shift is not as dramatic as one might be led to imagine. The sympathy for the genealogical approach is already visible in *The Archaeology*, when he refers favorably to Nietzsche’s disruptions of historical method (1972: 13). More importantly, though, genealogy gave license to set aside the caveats and limits of *The Order of Things*, allowing a director engagement with the material, and a reader ability to make sweeping assertions without being restricted to a terrain. In the later books, be it *Discipline and Punish* or the *History of Sexuality*, we
find free references to the ‘classicism’ and the modern without any new redefinition of what these terrains might mean. These are taken-for-granted, and if the new works were again about the analysis of language, life, and wealth, this would be entirely reasonable. But the projects of specifying, of pinning out these discourses, lie far beyond the scope of any of these later works.