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NATURAL PHILOSOPHY, EXPERIMENT AND DISCOURSE: BEYOND THE KUHN/BACHELARD PROBLEMATIC

1. INTRODUCTION

The eighteenth century witnessed the constitution of mature domains of experimental research in chemistry, physical optics, electricity and magnetism, and heat theory. This poses one version of the problem of the eighteenth century in the history of science: What is the shape of the period between the end of the Scientific Revolution and the 'Second Scientific Revolution', that era in the late eighteenth and early nineteenth centuries that saw the accelerated development of experimental fields, the mathematization of some, and the development of their institutional and increasingly professional structures? Attention has been lavished upon other related construals of the eighteenth-century problem, notably the meaning and scope of Newtonianism, the relations between science and the Enlightenment, and the exigencies of eighteenth-century matter theory including, recently, its social relations. But, the maturation of new experimental or mathematico- experimental fields remains fundamental for our understanding of the shape of the history of European science, theoretical perspectives on discipline formation, and the nature of experiment.

Gaston Bachelard and Thomas Kuhn recognized the historiographical import of the eighteenth-century maturation of fields of experimental science and their respective models of theory dynamics were conditioned by this problem. The influence exerted by their works alone justifies a reconsideration of their views. This conclusion is only reinforced when one further considers that Kuhn's problematic lingers on, shaping and constraining even the best Anglo-American historiography of eighteenth-century science and that Bachelard's account of what constitutes experimental sciences bears some relations of analogy and paternity to current sociological perspectives on experiment. However, today the problem of eighteenth-century experimental fields has even greater salience due to the confluence of two strands of research in the history of science and the sociology of

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scientific knowledge. On the one hand, there has been renewed, if somewhat inchoate, attention to the category of 'natural philosophy' as a key to understanding the eighteenth century (Schaffer, 1980; 1983; 1986; Shapin, 1980; 1981; Shapin & Schaffer, 1985; Wilde, 1982; Cantor, 1982). Neither Kuhn nor Bachelard had a coherent and positive notion of eighteenth-century natural philosophy. On the other hand, sociologists of scientific knowledge are increasingly attentive to the socio-politics of experiment and scientific instruments, and the eighteenth century is a key site for applying and refining social constructivist perspectives hitherto forged mainly, though not entirely, by considering contemporary cases.

This paper triangulates amongst three elements: the Kuhn/Bachelard problematic of eighteenth-century experimental fields, the renewed awareness of natural philosophy as an historical category, and the emerging social constructivist literature on experiment. Our chief aim is to sketch a post-Kuhn/Bachelard framework for analyzing and describing the nature and dynamics of the nascent eighteenth-century domains. Our triangulation thus has a definite focus and point of application, for this effort turns on the issue of understanding 'natural philosophy', especially the experimental natural philosophy of the seventeenth and eighteenth centuries, as an historical enterprise and as an historiographical category. It is in this area that we hope to clarify the eighteenth-century problem and connect our solution to historiographical issues about the preceding 'Scientific Revolution' as well. Nevertheless, our other two points of reference are inextricably involved as sources of insight and as objects of critical discussion. First, by establishing a preliminary model of the character and dynamics of experimental natural philosophy, we can turn the Kuhn/Bachelard problematic on its head whilst specifying the standing of their respective contributions. Kuhn and Bachelard practise a 'two-place' historiography, seeking points of origin at which eighteenth-century experimental sciences emerged from pre- or non-scientific enterprises. In contrast we shall argue there are no such science-founding events or achievements: these are misunderstandings of moments in the social and technical dynamics of experimental natural philosophy. Still, we will reach this post-Kuhn/Bachelard conclusion partly with the help of their own analyses, thereby contributing to a determination of the current standing of their views on the history of experimental sciences and science dynamics generally. Second, since our model of experimental natural philosophy will also depend in part upon adapting perspectives in the literature on the sociology of experiment, we will in effect address the question of the pertinence of historical

analyses to the sociology of scientific knowledge. The chief example used to illustrate our argument will be the development of electrostatics in the eighteenth century, and especially the role of the Leyden jar and of Franklin's work in that development: an area of eighteenth-century 'science' that had considerable importance in the deliberations of Bachelard and Kuhn.

Our argument involves 'tacking' among the three elements mentioned above and it is, of necessity, rather complex. Sections 2 and 3 examine Kuhn's and Bachelard's two-place historiographies of eighteenth-century fields, with special attention to the case of electrostatics. Section 4 explores the sense in which Kuhn, whilst lacking a positive conception of natural philosophy, was nevertheless groping toward some sense of continuity across the pre-paradigm/paradigm divide, a continuity we suggest can only have been in and of natural philosophy. Then, in Section 5, we articulate the category of natural philosophy, developing a working conception of experimental natural philosophy that requires further development in the form of an explicit model of experiment and experimental hardware as forms of embodied discourse. This model, essential for an understanding of experimental natural philosophy, is developed in Section 6 by 'crossing' our analysis of Bachelard with some perspectives in the constructivist sociology of experiment. This in turn makes possible the treatment of experiment within a refined model of experimental natural philosophy, and Section 7 develops this in the light of the aspects of electrostatics treated by Kuhn and Bachelard. Section 8 offers an overview of the dynamics of experimental natural philosophy. Finally, Section 9 draws some conclusions for the history of science and natural philosophy.

2. KUHN: THE GENESIS OF EXPERIMENTAL FIELDS

Both Kuhn ad Bachelard practiced a 'two-place' historiography; *i.e.*, both seek ruptures or sudden births, where sciences are born, placing such events in the context of the pre-existence of what is 'not-science'. Kuhn identifies the genesis of an experimental science with the emergence of a first 'paradigm', against a field of 'pre-paradigmatic' enterprises in the relevant domain of research. Bachelard identifies the emergence of an experimental field with the sudden creation of a 'science' that replaces 'pre-scientific' enterprises or discourses. Neither Kuhn nor Bachelard seriously entertains 'natural philosophy' as an historical category and so neither of them can discern the possibility that what they call sciences or first paradigms actually emerged as a result of the characteristic dynamics of experimental natural philosophy.

At the beginning of *The Structure of Scientific Revolutions*, Kuhn uses electrostatics to illustrate his theory of paradigm formation. The pre-paradigm stage of electrical research was dominated by competing schools with differing metaphysical commitments (*e.g.*, Cartesian or Newtonian), preferred problems, and starting points for surveying the burgeoning field of known electrical phenomena (*e.g.*, stressing the phenomena of attraction/repulsion rather than of conduction). For Kuhn, the founder of the first real electrical paradigm was Benjamin Franklin, initially an adherent of the pre-paradigm school concerned mainly with conduction effects. This school 'tended to speak of electricity as a "fluid" that could run through conductors, rather than as an effluvium that emanated from non-conductors'. They could deal with simple conduction effects, but not very well with the known attraction/repulsion effects. The work of Franklin and his successors, however, issued in a successful first paradigm 'that could account with something like equal facility for very nearly all these effects and therefore [provided] a subsequent generation of electricians with a common paradigm for its research' (Kuhn, 1970: 14-5).

The 'exemplar' of the Franklinists, their exemplary problem solution out of which their full paradigm was articulated, was their 'successful' explanation of the Leyden jar. In whiggish terms the jar was a primitive form of condenser. It might typically consist of a glass container filled with water and stopped with a cork . A metal pin was inserted through the cork into the water. The jar was charged by grounding the outside surface of the glass and bringing the pin into contact with a prime conductor deriving electrical fluid from the rotating globe of an electrostatic generating machine via its collecting brushes and chain. If the circuit between the pin and the outer surface were closed, the jar would discharge. The jar was devised in an attempt to store electrical fluid, but it was found to work effectively only when the usual rule for charging 'non-electrics' (derived from Dufay) was not followed; that is, the jar had to be grounded, rather than insulated in order to be charged. According to Franklin, the electrical fluid added to the inside of the jar repels an equal quantity of fluid off the jar's outside surface. In discharge to neutrality, the surcharge within the jar flows out through the pin and restores to the outer surface a quantity of fluid equal to that originally repelled off that surface.

According to Kuhn, to understand the basis of Franklin's exemplar one must look back to the origins of the pre-paradigm 'fluid' school of

electricians. Only in the 1740s had it become legitimate to invoke imponderable, aether-like fluids: weightless, property-bearing, conserved and capable of acting at a distance. Kuhn (1970: 106) argues that for aether- or fluid-theorists,

> electrical phenomena increasingly displayed an order different from the one they had shown when viewed as the effects of a mechanical effluvium that could act only by contact. In particular...the phenomenon we now call charging by induction could be recognized. Previously, when seen at all, it had been attributed to the direct action of electrical 'atmospheres' or to the leakages inevitable in any electrical laboratory.

It was this possibility of conceptualizing electrostatic induction, and hence the possibility of distinguishing between, on the one hand, the flow and conduction of electrical fluid and, on the other hand, its exertion (upon itself) of repulsion at a distance, that, according to Kuhn, facilitated Franklin's paradigm-making achievement of explaining the Leyden jar (1970: 117-8). Indeed, for Kuhn it was these insights that constituted the essence of the Franklinian paradigm, as he makes quite clear (1970: 118) by conflating the establishment of that paradigm with the widespread comprehension and acceptance of his clarification of inductive effects and of conduction versus distance effects:

> ... after the assimilation of Franklin's paradigm, the electrician looking at a Leyden jar saw something different from what he had seen before. The device had become a condenser, for which neither the jar shape nor glass was required. Instead, the two conducting coatings--one of which had been no part of the original device--emerged to prominence. As both written discussions and pictorial representations gradually attest, two metal plates with a non-conductor between them had become the prototype for the class. Simultaneously, other inductive effects received new description , and still others were noted for the first time.

By packing this understanding of what we might term 'the condenser as such' into the paradigm from the point of its 'assimilation', Kuhn endows Franklin's paradigm with a destiny of unproblematically maturing through articulation of its own pre-given conceptual and technical resources (1970: 21-2, 118; 1963: 356-7).[1] We shall see in Section VII that Kuhn's account of the core of Franklin's exemplary achievement is quite misleading. For the moment we shall take Kuhn's claims at face value and examine his 'two-place' historiographical use of the concept of a 'first paradigm' in the case of electrostatics.

On the one hand, Kuhn stresses the rupture with the ineffective, not really cumulative, indeed not scientific pre-paradigm state; a rupture pinned upon the Franklinians having achieved clarity about induction. On the other hand, once the rupture has occurred, an epistemological predestination hovers around Franklin's paradigm. Assuming, as Kuhn does, that Franklin was clear about distinguishing induction from conduction, and, in general, flow effects of the electrical fluid from its distance effects, then a Kuhnian can gaze almost whiggishly at the cumulative articulation of Franklin's paradigm. One learns (1970: 18):

> What the fluid theory of electricity did for the sub-group that held it, the Franklinian paradigm later did for the entire group of electricians. It suggested which ex eriments would be worth performing and which, because directed to secondary or overly complex manifestations of electricity, would not. Only the paradigm did the job far more effectively, partly because the end of interschool debate ended the constant reiteration of fundamentals and partly because the confidence that they were on the right track encouraged scientists to undertake more precise, esoteric, and consuming sorts of work. Freed from the concern with any and all electrical phenomena, the united group of electricians could pursue selected phenomena in far more detail, designing special equipment for the task and employing it more stubbornly and systematically than electricians had ever done before.

Given that the Franklinians had their exemplar well sorted out, Kuhn can see the maturation and articulation of Franklin's paradigm as the progressive mathematization of electrostatics (1970: 21, 28-9; 1977: 47-8; 1963: 356-7). Here, Kuhn's historiography partially echoes whiggish histories of electrical science. Garden variety whiggism weaves a story of the clarification and generalization of facts and the instrumentalist rejection of the imponderable electric fluid; for Kuhn, mathematization is made possible by and played out in the Franklinian paradigm.[2] Hence Kuhn, like ordinary positivist whigs, is ultimately a seeker after 'scientificity'. Both historiographies are dramas of science versus non-science. But, there is one telling difference. Whig history can afford to see a science maturing gradually, because it attends to the accrual, generalization and perfection of 'facts'. The drama is long and slow. Kuhn's catastrophism of conceptual frameworks, inherited from the historiography of Koyre, inoculated him against naive empiricism and led him to the notion of a *first* paradigm in a field, and thence to a history of discontinuity since first paradigms constitute ruptures with the non-scientific past *and* pre-program the maturation of their field.[3] The net result, therefore, is a dramatic two-place historiography, of early electrostatics and other experimental fields, premised on locating the first outbreak of scientificity.

The inadequacies of Kuhn's two-place historiography have been highlighted by recent historical research on electrostatics, some partially indebted to Kuhn. Consider the path to mathematization programmed into the paradigm by the exemplary 'condenser as such'. The work of

Heilbron, Home and others has well shown that Franklin had no modern text-book conception of induction. Moreover, it is clear that Franklin, committed as he was to electrical 'atmospheres' did not consistently distinguish between flow and distance effects of his fluid, let alone have a conceptual-practical distinction enforceable upon his presumed paradigmatic followers (Home, 1972; Heilbron, 1979: 334-9, 376-7). More importantly, careful study of such post-Franklinian electricians as Beccaria, Aepinus, Coulomb and Cavendish fails to display a clear line of internally driven and programmed 'paradigm articulation' leading toward an instrumentalist quantification of electrostatics. Rather, one has a story of conceptual and institutional struggle, of contingencies and negotiations centring on the nature, function and significance of a variety of Leyden jars, condensers, air condensers, electrophori and Coulombian torsion balances (Heilbron, 1979: 373-489).[4] Where Kuhn packs the exemplary condenser into the paradigm from its beginning, Heilbron documents the battles to constitute and enforce any consensus about the basic conceptual issues and their concomitant instrumental manifestations. In any case, for a sub-set *only* of late eighteenth-century electricians, this struggle issued in a quantitative electrostatics, still requiring a more thorough mathematical formulation, by Poisson in 1811. Ironically, by that time such a culmination of the mathematicizing destiny proved to be temporary and unstable; for, whatever mathematical electrostatics was in the hands of Poisson, his proffered canonical version soon required revision and reinterpretation in the light of electro-chemical, and then electro-magnetic effects, hardwares and theories, hotly contested and promoted by their own overlapping sets of devotees.[5] In all this there is little sign of the Kuhnian paradigm with a destiny.

3. BACHELARD: THE GENESIS OF EXPERIMENTAL SCIENCES

Bachelard, like Kuhn, is concerned with the emergence of fields of mathematical-experimental science out of largely qualitative and discursive bodies of thought. These he terms 'pre-science', a generic term subsuming all non-scientific systems of thought addressed to nature or technics; *e.g.*, craft knowledges, common-sense rationalizations of phenomena, traditional medical theories, alchemy, magic, as well as all traditional systematic philosophies of nature from Aristotle to Priestley. All these pre-scientific discourses are seen as constituted by cognitive errors and pitfalls that must all be fully expunged before any scientific discourse can emerge. Some of these pathol gies include: appeal to analogy and metaphor drawn from any realm whatsoever; projection onto natural phenomena of fundamental affective structures and dispositions; naive empiricism, the grasping and generalization of one's first impression of the phenomenon; and classification and natural history that merely place phenomena within a set of conceptual pigeon holes imported from some other realm of discourse. Generally speaking, the conceptual frame is external to science itself, having psychological or cultural origins, and the phenomena are viewed passively through these frames (Bachelard, 1949; 1965; 1975). Any genuine science, according to Bachelard, is the antithesis of all this. Its concepts and indeed its objects are created from within itself. A science takes shape in some limited and strictly controlled technical-experimental context in which simultaneously, (a) phenomena are produced and systematically varied in their relevant parameters, and (b) a system of interdefining ('interfunctioning') mathematicized concepts is created which directs the material realization of the phenomena and the variation of their aspects. A science is not built from natural facts, nor deduced from a priori concepts; it subsists in the resonation or interplay of the couple (a) and (b), denoted by Bachelard respectively as 'technical materialism' and 'applied rationalism':

Technical Materialism <=> Applied Rationalism

Research in a constituted field of science is a dialogue of the two moments. The right side leads the way, by unfolding theoretical possibilities of variation and suggesting modes for their realization or objectification in the experimental hardware on the left (1949; 1975; Lecourt, 1977: 40-7, 60-70, 76-9).[6]

When a science comes into being, that is, when such a couple is initially constituted, an artificial technical realm comes into being in which phenomena are literally manufactured under the joint guidance of the system of mathematicized concepts and the instruments and experimental hardware in which those concepts have been realized. In an ironic jibe at positivist dogma, Bachelard terms any such realm of theoretically dominated artificial experience a 'phenomeno-technique'. By this he means that the phenomena of science are not discovered but made; not natural, but artificial. They are created and commanded in the light of theory and theory-loaded instruments and not by common sense, natural philosophical systems, or any other 'pre-scientific' mode of conceptualization (Bachelard, 1965: 60-1; 1975: 17; Gaukroger, 1976: 212-23).[7] It follows that any given science is born through a discontinuity, an 'epistemological rupture' with the relevant preceding pre-scientific discourse (Lecourt, 1977: 86; Gaukroger, 1976: 228).[8] The limited technical realm and the neat system of scientific concepts must be summoned into existence together; and the new-born couple lacks any relation to pre-scientific discourses.[9]

For Bachelard the Leyden jar is a pre-scientific conceptual monstrosity, created through pre-scientific imagination run amok. Electricity is a fluid; to store a fluid you use a jar -- so pump fluid from your generator into water contained in a glass jar (Bachelard, 1949: 148-50; 1965: 76). But, Bachelard counters, true electrical science would later conclude that electricity is not a fluid; that is not held in the water (something the eighteenth-century actors soon learned on a practical level); that the capacity of the jar for electricity relates not to its volume but to the inner surface area and, paradoxically to common sense, the thinness of the glass (short of being so thin as to rupture in discharge). There is a complete mismatch between the metaphors guiding the invention and practical use of the jar, and the true concepts later controlling its properly scientific use and construction. Electrical science begins not with the jar but with its transcendence: when it is shown that any object can be a capacitor which can realize the fundamental equations linking charge, potential, specific inductive capacity, surface area of plates and thickness of dielectric (Bachelard, 1949: 151). Such objects are scientific objects -- artifacts, not natural facts -- their construction and functioning commanded through the equations expressing the interfunctioning of the concepts. Novel conceptual possibilities, for example, equations giving the capacitance of sets of capacitors in series or parallel, equations which take counter-intuitive forms, can be foreseen and attempts can be made to realize them, hence enriching the concept of capacitance and developing the instrumental hardwares and their capabilities (1949: 154). Accordingly, Bachelard would date the science of electrostatics from the work of Coulomb, Cavendish and then Poisson; not from the pre-scientific fantasies of the Franklinists.

Bachelard's model of experimental science bears some relations of paternity to 'social constructivist' approaches to experiment. Later we shall develop these in articulating our notion of experimental natural philosophy. For the moment, however, we note some ways in which Bachelard's conception of the phenomeno- technique rests upon and legitimates a whiggish two-place historiography and is profoundly antithetical to the content and spirit of recent sociology of experiment.

Bachelard, like later sociologists of experiment, saw experimental hardwares as embodying conceptual structures, yet he saw them as 'frozen' conceptualizations whereas sociologists of experiment stress the fluidity and negotiability of meanings embodied in hardwares. By 'frozen' we mean, in the first place, that for Bachelard the development and articulation of a science can involve only minimal alteration of concepts once they have been acquired. Even the initial creation and embodiment of a set of science-making concepts constituted a permanent scientific achievement (1951: 25*ff*). In any given field of experimental science, the conceptual fabric initially constituted in the science-making rupture with pre-science cannot be undone by radically transforming, re-negotiating or rejecting the key concepts of the science. Although Bachelard saw mutually conditioning changes in the terms of the hardware-discourse couple as the content of serious research in mature fields, the conceptual changes he envisioned are of a moderate and mainly incremental kind: additions to the conceptual/hardware armoury, refinement or increasing precision on both sides of the couple, and the excision of the occasional gross error (Gaukroger, 1976: 227-34). 'Frozen' also means that Bachelard restricts the content of true sciences; only certain kinds of concepts can be frozen. For Bachelard the theoretical or 'applied rationalism' side of the hardware-theory couple is in fact always an 'interdefining' set of mathematicized concepts. Qualitative concepts or merely discursive theorizing plays no part. The birth of a science depends upon the creation (and phenomeno-technical embodiment) of such a system of mathematicized concepts bearing no relation to the previous pre-scientific discourse except that of negation. The distinction between pre-science and science is, on the conceptual side, a distinction between ordinary discourse and mathematics: only mathematics can be scientifically embodied in hardware, and the articulation of the theoretical side of a science is really the unfolding of new mathematical relations in terms of the existing set of mathematicized concepts. The relation 'science is to pre-science as mathematics is to discourse' underpins Bachelard's two-place historiography.

We can now begin to discern that Bachelard's enterprise falters in much the same way as Kuhn's. Their problems do not arise from their failure to agree on the site of the advent of scientificity, in the case of electrostatics or any other; but rather, from their attempts to locate such sites at all. As with Kuhn's search for first paradigms, Bachelard's regard for cataclysmically novel phenomeno-techniques is essentially whiggish in the most profound sense of the term, since it involves the search for some event or achievement upon which to pin the medal for 'finally-

before and indeed after the moment for the award of the prize.

In contrast we argue that here experimental natural philosophy is the relevant historical category. It was experimental natural philosophy that was undergoing change and transformation, its tortuous and contingent history issuing some times and some places in states and events to which whigs apply the epithets 'science', 'phenomeno-technique', or 'paradigm' in their scrambling to award epistemological merit points. The historiographies of Kuhn, Bachelard, and garden variety whigs beg for deployment of the category of experimental natural philosophy. The problem is how to accomplish this: ironically, both Kuhn and Bachelard provide hints as to how to go about displacing their own two-place historiographies. As we shall see in the next section, Kuhn tacitly admits to there being some sort of continuity of conception and practice across the pre-paradigm/paradigm divide. We shall suggest that Kuhn's intimations of continuity and process can and must be articulated through a conception of experimental natural philosophy lacking in his work. After we have begun in section 5 to define experimental natural philosophy, we shall find in section 6 that Bachelard's conception of the phenomeno-technique, suitably revised in constructivist terms, provides a key element for our model of the dynamics of experimental natural philosophy.

4. KUHN'S BRUSH WITH CONTINUITY

Despite the message conveyed by a first reading of *The Structure*, Kuhn never managed decisively to distinguish between pre-paradigm/paradigm stages in the development of an experimental field. But in his hesitation and ambivalence, Kuhn recognized the dynamics of something like what we would term experimental natural philosophy, a something he terms the 'Baconian sciences'. We will first examine his difficulties in *The Structure* and then scrutinize his conception of these Baconian sciences.

Kuhn's difficulties with the pre-paradigm/paradigm distinction appear in the opening portions of *The Structure*, and remain unresolved in his 1970 'Postscript'. Recall Kuhn's (1970: 14-5) description of the competing pre-paradigm schools of electricians, each characterized by its own metaphysical commitments and preferred domain of problems and phenomena. This makes the pre-paradigm schools seem very paradigm- or science-like, and it is a description holding for the pre-paradigm stages of other fields as well. For example, in discussing pre-paradigm

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or science-like, and it is a description holding for the pre-paradigm stages of other fields as well. For example, in discussing pre-paradigm schools in physical optics, he (1970: 12-3) even observes that they each had different respective 'paradigmatic observations'. To critics he responded in the 'Postscript' in the spirit of his earlier ambivalent assertion that pre-paradigm researchers 'were scientists, [but produced] something less than science' (1970: 13), arguing that the transition to maturity of a science 'need not be associated with the first acquisition of a paradigm', for the members of the schools of the pre-paradigm period of a field 'share the sorts of elements which I have collectively labelled a paradigm' (1970: 179). What changes, Kuhn (1970: 179) now suggested, is not

> the presence of a paradigm but rather its nature. Only after the change is normal puzzle-solving research possible. Many of the attributes of a developed science which I have associated with the acquisition of a paradigm I would therefore now discuss as consequences of the acquisition of the sort of paradigm that identifies challenging puzzles, supplies clues to their solution, and guarantees that the truly clever practitioner will succeed.

This certainly seems a distinction without much of a difference, when we recall Kuhn's views on the effectiveness and maturity of the fluid school of electricians even before Franklin. Kuhn needed a rupture, a transition to maturity, but was left with weak indicators of its supposed occurrence: relatively greater consensus (the end of inter-school debate) apparently dependent upon relatively greater puzzle-defining and puzzle-solving power (which also instills confidence). But, given the paradigm-like virtues of the pre-paradigm schools, it is still not possible to see, in Kuhn's terms, why and how such points of superiority are made out and enforced upon the debating parties.

We conclude that Kuhn's difficulty in grounding his two-place historiography leads him in the direction of these vague intimations of 'continuity'. The underlying cause of his problem, we suggest, resides in his failure to recognize and deploy a category of natural philosophy. We shall now see how the absence of this category vitiates much of his understanding of the Scientific Revolution and permits (and demands) his use of the term 'Baconian sciences' to articulate the continuity that looms through his failure to enforce his own pre-paradigm/paradigm distinction.

As several of Kuhn's essays show, he sees the Scientific Revolution as having advanced on two loosely connected fronts (1977: 31-59, 136-7, 213-21). The first is the radical change in the pre-existing classical

sciences, such as the mathematically oriented ones of geometrical astronomy, optics, mathematics itself and the study of motion, as well as in the bio-medical ones of medical theory/physiology and anatomy. The second is the initial development of some empirically and experimentally oriented Baconian sciences, the fields that, in *The Structure*, mature to their first paradigms at various stages in the eighteenth century. His schema largely ignores the great natural philosophical systems of the early modern period, the conflicts of which, we shall argue in Section V, defined the rhythm and moments in the Scientific Revolution. To Kuhn (1977: 53) they provide the 'metaphysical' elements or 'new intellectual ingredients' of the existing, shifting and nascent sciences in play on the two fronts.[11] He does not recognize the role of systematic natural philosophies *per se* in demarcating and conditioning the content of what he terms the sciences, both existing and nascent. Clearly, to speak of atomism as a 'new intellectual ingredient' bearing the metaphysics of the cluster of classical sciences, is not the same thing as to trace the construction and triumph of corpuscular-mechanical philosophies, their ongoing interaction with the classical sciences, and their relations with the emergent domain of 'Baconian' experimentation (Schuster, 1989)

Kuhn's (1977: 31-59) story of the Baconian sciences amounts to saying that a cluster of experimental 'sciences' [*sic*] emerged or were extracted from alchemy, magic and the crafts; were loosely fitted under the corpuscular-mechanical umbrella and imbued with the Baconian rhetoric about utility and progress; and then matured into true sciences. This is little more than an articulation of the model of the maturation of new eighteenth-century experimental sciences presented in *The Structure*, developing the narrative of the 'pre-paradigm' states of these sciences. But, because this story grounds the experimental *sciences* in their Baconian origins, it must veer away from the categories and two-place historiography of *The Structure*, and toward the hints of pre-paradigm/paradigm continuity present there. After all, Kuhn assumes that these sciences really existed from their initial seventeenth-century crystallization and that, in general, there were less mature scien es in the pre-paradigm stage, and more mature sciences in the post-paradigm stage. Kuhn's continuity, therefore, can only be a continuity of some kind of scientificity. Once again, he falls into difficulties by not having a working category of natural philosophy. Despite intimations of continuity and process, he fails to theorize natural philosophy as an historical category, and hence neglects to consider if the dynamics of natural philosophy might have had anything to do with these putative Baconian sciences and their long gestation to 'full maturity'.

5. NATURAL AND EXPERIMENTAL NATURAL PHILOSOPHY

The history of science in the early modern period, the age of the Scientific Revolution, and the eighteenth century is very much the history of natural philosophy, or more properly of competing systems of natural philosophy, and their relations with a set of more narrow, technical fields of inquiry. Some fields like astronomy or optics were mature, long constituted and undergoing significant alteration; others, like the experimental fields of the eighteenth century, were just emerging in the period. To miss this point is to forfeit any firm grasp on the historical trajectory of European science. Not only was the history of science made in the struggle for cognitive and social hegemony amongst the systems of natural philosophy but also the field of competing systems of natural philosophy constituted the matrix from, and ultimately against which, the experimental fields emerged (Schuster, 1989). It is necessary, first, to understand in generic terms what natural philosophy consisted in as a social and cognitive undertaking. This involves looking beyond the existence of numerous competing systems of natural philosophy in order to examine their general form, contents and aims as well as their mode of production and dissemination within a sub-culture. This will allow us to characterize experimental natural philosophy generically, and hence articulate its dynamics, whilst avoiding some explanatory pitfalls awaiting the historian of natural philosophy in general and the eighteenth-century problem in particular.

Every system of natural philosophy, whether of a generally Aristotelian, Neo-Platonic, mech nistic or Newtonian type, purported to describe and explain the entire universe and the relation of that universe to God. The enterprise also involved a concern with the place of human beings and human society in that universe. Each system of natural philosophy rested upon bour structural elements the contents and systematic relations of which went far toward defining the content of that particular system. These elements were: (1) a theory of substance (material and immaterial), concerning of what the cosmos consists, what kinds of bodies or entities it contains: (2) a cosmology, an account of the macroscopic organization of those bodies; (3) a theory of causation, an account of how and why change occurs; and (4) an epistemology and doctrine of method purporting to show how the discourses under (1), (2) and (3) were arrived at and/or how they could be justified, and how they constitute a system.

The existing specialist fields of science, those broadly delineated above in Kuhn's categorization of 'classical mathematical and bio-medical sciences', were pursued under the aegis of, and hence subject to contingencies within, the enterprise of natural philosophy. Each science was considered to be part of, or conditioned by, one or another system of natural philosophy. In the terms of Kuhn, and of Koyre whom he largely followed in this regard, the ' etaphysical' presuppositions of a science were supplied or enforced by a system of natural philosophy. The shape of a science, its direction of development, and indeed its very legitimacy often depended upon the character of its natural-philosophically enforced metaphysics, which itself might have been the outcome of conflict and debate. So, for example, the mechanical philosophy could be used either to marginalize scientific undertakings which were subsumed by and hence supported antagonistic systems of natural philosophy or to co-opt portions of otherwise 'dubious' scientific enterprises by reinterpreting them in terms of a mechanistic metaphysics, as was done with work of such non-mechanists as Harvey, Kepler, Gilbert and Bacon. Conversely, rapid change in a science and/or a shift in its social evaluation could lead to the constitution, alteration or abandonment of a natural philosophy through the borrowing or rejection of privileged images or models. Paracelsus' 'alchemization' of natural philosophy comes to mind here, as does the rise of the mechanical philosophy, which was grounded in the concerns of practical mathematicians and devotees of 'mechanics'.

Natural philosophies, however, were not mere collections of cognitive forms and aims. They were products of a sub-culture or, better, something like a Bourdieuian field of power and discourse relations (Bourdieu, 1975; 1981) in which the construction, modification and purveying of natural philosophies was a rich *sui generis* social enterprise, pursued in various institutional locations, with overlapping and partially competing bodies of discursive resources. There existed competing sub-species of natural philosophical discourse, all subject to complex and shifting pressures coming from, or issuing toward neighbouring fields of discourse, for example, in theology, politics, the practice and rhetoric of the practical arts, and the existing and nascent 'sciences'. To be a natural philosopher was to be engaged in the construction, interpretation and communication of the sorts of discursive entities we have described above. The individuals involved devoted themselves to natural philosophizing with serious and hard-won skills because of the value placed upon, and power conferred by, having the 'correct' view of nature. A correct natural philosophy would be expected to yield social and cognitive hegemony over the other players and over the institutional loci they occupied. It was also expected to enable the victors to determine and enforce their own particular notions about the proper content, order and goals of the existing and nascent 'sciences'. Finally, it was also expected to enable the victors to enforce their own views about the correct relations between natural philosophy and any and all 'contextual' discourses (and their institutional relations) of *perceived* relevance -- theological, political, cultural.

It is important to understand natural philosophy as a generic field of social and cognitive struggle, for it will allow us to define and explain the sense in which the advent of 'experimental natural philosophy' after 1650 was a mutation of the genus, a generic shift across the entire field of natural philosophy. We shall see that what Kuhn misleadingly describes as the birth of the Baconian sciences was actually a function of this change in the generic character of natural philosophizing, a change centring upon the discursive reconfiguration of notions of experiment and experience and of the relation of natural philosophy to the practical arts. What Kuhn calls the Baconian sciences, we shall see as experimental natural philosophy; where he sees those sciences continuously evolving over a century and a half -- albeit punctuated with the achievement of first paradigms -- we shall see the dynamics of the field of experimental natural philosophy, fostering the crystallization and demarcation of regional domains of experimental work and discourse. To explicate this shift, we require a brief account of aspects of the Scientific Revolution, centring on the field of natural philosophizing, the pattern of natural philosophical struggle, its key stages, and especially the struggle to define and enforce notions of experience and experiment, and of the role and function of the practical arts.

The Scientific Revolution may be viewed as a process of change and displacement within the field of competing systems of natural philosophy. The process involved the erosion and downfall of the dominant Aristotelian philosophy of nature and its replacement during the middle third of the seventeenth century by variants of the newly constructed mechanistic natural philosophy, which after a period of consolidation and institutionalization, were modified and partially displaced by the post-mechanist natural philosophies of Leibniz and especially Newton. The erosion of Aristotelianism in the sixteenth and early seventeenth century was connected with the proliferation of alternative natural philosophies of magical, alchemical and Hermetical colourations, and the mechanical philosophy was as much a response to the social, political and theological threats seemingly posed by these competitors as it was a response to Aristotelianism. The crucial moment in this process lies in the first two generations of the seventeenth century, an era of

heightened conflict amongst Aristotelianism, its magical/alchemical challengers and nascent mechanism. Newtonianism was hardly the teleological goal of the process, but rather a complexly conditioned, contingent (and surprising) modification of the classical mechanism of the mid-seventeenth century.

Viewed this way, the Scientific Revolution takes on an interesting rhythm as a process of change and transformation of systematic natural philosophy (Schuster, 1989). There is a preliminary sixteenth-century stage, which will be termed the Scientific Renaissance, characterized by the erosion of Aristotelianism in some quarters, its further entrenchment in others, and by a ferment of revived alternative natural philosophies. There follows a 'critical' period (*ca.* 1590-1650) of natural philosophical conflict marked by the initial construction of mechanistic philosophies, and then a brief period of relative consensus on and institutionalization of a range of variants of the mechanical philosophy (*ca.* 1650-1690), punctuated and complicated by the advent of Newtonianism. It is during this post-1650 stage that we shall speak of a generic shift in the field of natural philosophy to 'experimental natural philosophy'. As we shall show, this shift was first instanced in the advent and acceptance of varieties of experimentally oriented, 'Baconized' mechanical philosophies but as a permanent generic shift across the field of natural philosophizing it persisted through the advent of Newtonian natural philosophy. Generic experimental natural philosophy, actualized in mechanistic and Newtonian species of natural philosophy, was the domain within which occurred the eighteenth-century formation of experimental fields. We shall argue that this occurred through processes largely having to do with the altered dynamics of the natural philosophical field, once it had become generically 'experimenal'.

The sixteenth century saw the recovery, assimilation and publication of a wide and confusing array of non-Aristotelian, 'alternative' natural philosophical systems: neo-Stoicism and Lucretian atomism; varieties of neo-Platonism, some more or less flavoured with Hermetic influences and variously amalgamated with alchemy, natural and demonic magic and cabala; and more eclectic and idiosyncratic ones derived from figures such as Telesio, Paracelsus, or even the radical Aristotelian Pompanazzi. One of the many factors powering this effusion of natural philosophical systems was a re-evaluation of the practical arts which first began to gain momentum amongst sections of the educated and natural philosophically literate elite in the sixteenth century. Important questions surrounded the social role of such practical arts as navigation, cartography, architecture and fortification, surgery, mining, metallurgy and other chemical crafts and the status of their methods, tools and results as 'knowledge'. These questions and the answers they received on the plane of natural philosophical discourse helped shape the aims and contents of competing systems of nature, some developments in the individual sciences, and, indeed helped prompt the emergence of generic 'experimental' natural philosophy (Rossi, 1970; Schuster, 1989).

The questions were promoted by the deeper social and economic structures of the 'long' sixteenth century, which extended from the late fifteenth to the early seventeenth century: by the historically high levels of population increase and price inflation; the expanding commercial capitalist economy; the enhancement of the power, and to some degree the size, of central governments; and the growth of the size, cost and technical complexity of military and naval hardware, logistics and administration. The number and diversity of potential patrons and clients of the practical arts increased. This, along with expanded literacy and access to print on the part of some master practitioners, their social allies and patrons, brought into being a social/cognitive field. There, practitioners competed for recognition and honour, whilst simultaneously contributing to the disparate chorus of claims that the practical arts and artisans deserved higher status, and that their skills and craft knowledge warranted the cultural status of 'science', in the contemporary sense of orderly discipline of intellectual worth (Rossi, 1970; Eisenstein, 1979: 520-74). The alternative philosophies of nature were important vehicles for those currents of opinion placing a premium on operative knowledge and on the search for command over the powers of nature. These systems could marshal powerful sentiments in favour of the combined practical and spiritual value of mathematics, and in favour of the natural philosophical relevance of the practical arts.

What we have termed the critical period of the Scientific Revolution (*ca.* 1590-1650) is characterized by a conjuncture between an unprecedented burst of conceptual transformation in the classical sciences -- instanced above in Kuhn's conception of the Scientific Revolution -- and a heightened, often desperate competition amongst systematic natural philosophies (some tied to utopian and irenic programmes of religious and social reform). The tendencies corrosive of Scholastic Aristotelianism -- the challenge of Paracelsianism, of Hermetically or alchemically tinged neo-Platonism, of calls for the re-evaluation of practical knowledge, of anti-Aristotelian rhetoric tied to the practice of the mathematical arts or classical mathematical sciences - all took on greater virulence and urgency. From this emerged the mechanical philosophy which was self-consciously designed and sold by

a handful of innovators in an effort to finesse and resolve (in their own favour) the natural philosophical crisis of the age (Schuster, 1989). All this occurred within a context of political, religious and intellectual turmoil which raised the stakes that actors perceived in the conflict of natural philosophies: stakes so perceived because of the presumed generic relations of natural philosophizing with political and religious issues and domains of discourse (Rabb, 1975; Popkin, 1979; Easlea, 1980). Since a genuine culture of natural philosophizing existed for the contemporary actors, problems and tensions in religion, politics and society were variously read and interpreted through the filter of natural philosophizing thereby entailing that these problems had some of their basis in the very confusion of competing systems.

The founders of mechanism hoped to resolve the conflict of natural philosophies in a way which was cognitively progressive, but religiously and politically conservative: by exploiting and co-opting recent achievements in the classical sciences, including the Copernican initiative, and by raising the premium placed upon mathematics and operative knowledge by sections of Renaissance opinion whilst avoiding the perceived religious, political and moral pitfalls of the alchemical, Paracelsian, Hermetic and eclectic atomistic systems then bidding to displace the Scholastic Aristotelianism (Lenoble, 1971; Easlea, 1980). Accordingly, the selection and moulding of discursive resources to form the mechanistic systems was a nice and dangerous task: first because it involved endorsing some values and aims characteristic of magical-alchemical systems, whilst explicitly opposing them as such; second, because the resulting product was itself intended to displace Aristotelianism in the institutional centres of natural philosophy. Mathematics was construed to avoid any hint of neo-Platonic mathematical fancies. Yet, as in neo-Platonism, mathematics was to be the very language of nature. Experience was identified with experiment, itself rhetorically modeled upon the disassembly and reassembly of machines, so as to marginalize alchemical and Paracelsian accounts of experience as an affect-laden, spiritually sanctioned and uplifting intuition of otherwise hidden relations and correspondences. Yet, as in magic and Paracelsianism, operative command over nature was sought through an active experiential engagement with nature. The mechanical philosophy was also constructed so as to embody an arguably orthodox 'voluntarist' vision of God's relation to nature and to mankind, so as to avoid collapsing the divine into nature and/or elevating man, as seeker of operative knowledge as well as wisdom, to the level of a 'magus', a status morally and cognitively unacceptable to mainstream Catholic and Protestant thought alike.

Francis Bacon, whose writings would in the third stage of the Scientific Revolution be melded with varieties of mechanism to form the first species of experimental natural philosophy, also needs to be placed in the context of the critical stage. He was a brilliant 'bricoleur' of disparate sixteenth-century values and a filter and refiner of the catch-cries, polemics and attitudinal shifts characteristic of the Renaissance stage. He addressed at the level of natural philosophical culture the debates over the status of practical knowledge, the aims and methods of 'useful' education of gentlemen, and the Protestant stress on cultivating socially useful, this-worldly vocations. Like the mechanists he ntended the simultaneous displacement of Aristotelianism and its magical, neo-Platonic and alchemical challengers. Of particular importance was his co-option and re-working of the issue of practical knowledge and the practical arts (Rossi, 1970). He proposed through his method discourse to capture the labour and results of practical artisans, whilst denying their potential status as natural philosophers (Martin, 1988). Through this tactic and his method discourse in general, he recast the notions of experiment and experience over against Scholastic conceptions on the one hand, and Paracelsian, magical and alchemical ones on the other. He rhetorically constructed a category of sober, public, replicable, and communicable experience, to be instantiated mainly in experiment and in the findings of the practical arts, suitably winnowed and sorted. This methodically defined experience was, of course, aimed as much for control and command over nature as for explanation and understanding of it. In the succeeding generation, all this was open to selective adoption and reinterpretation by triumphant mechanists.

The third stage in the process of the Scientific Revolution is characterized by the dissemination and acceptance of varieties of the mechanical philosophy, and by the melding of mechanism's own discourse of mathematics and experiment with a rhetoric of method and experiment loosely attributable to Bacon which emphasized experimental grounding, tentative theorizing, exploitation of instruments and possible technological benefits. A consensus formed around an experiment-oriented corpuscular-mechanical natural philosophy [hereafter ECM]. It was a loose consensus, to be sure, for ECM was not one definitive system, but rather a rough template from which were fashioned specific variants. The consensus was nevertheless real, especially when compared with the conflict of natural philosophies in the two earlier stages (Schuster, 1989).[12]

ECM's Baconized rhetoric of experiment hardly sufficed to guide the articulation of the natural philosophy or the sciences now subordinated to it (Schuster, 1989). Yet, like similarly ineffective and vague method doctrines, it helped shape the way knowledge claims were assembled, negotiated and entrenched or rejected (Schuster & Yeo, 1986; Richards & Schuster, 1989). It also functioned at the institutional level in providing rhetorical resources for solidifying and delimiting legitimate practitioners and practices, as in the apologetical and programmatic rhetoric of the Royal Society (Wood, 1980; Hunter, 1981; Shapin & Schaffer, 1985; Dear, 1985; Golinski, 1989). In these two related guises the method rhetoric of ECM helped constitute the sense in which natural philosophy had become 'experimental', at least in an initial 'declaratory' sense.

The crucial point is that because consensus over ECM had replaced previous fragmentation and dissensus, the advent of ECM constituted a shift in the generic field of natural philosophy. In addition, because ECM drew upon the resources of Baconianism and early mechanism to reconfigure the *declared* meaning and place of experience and experiment in natural philosophical discourse, the field of natural philosophizing as a whole now begins to deserve the generic title of experimental natural philosophy. This characterization holds across the later advent and relative triumph of varieties of Newtonian natural philosophy, including Newton's inductivist method rhetoric. We can therefore see that Kuhn's description of the opening phase in the maturation of the Baconian sciences applies to this very situation but that for any talk about Baconian sciences we must substitute the foregoing conception of the newly triumphant natural philosophy of ECM. That is, the continuity in the development of experimental fields that Kuhn discerned belongs not to 'Baconian sciences', but to a shifted generic field of natural philosophizing.

6. A SOCIAL CONSTRUCTIVIST VERSION OF THE PHENOMENO-TECHNIQUE

We must now begin to unpack the dynamics of experimental natural philosophy. The central problem is: What did it mean in a natural philosophical context to do experiments, and to be concerned with instruments and experimental hardwares, their production and their accounted outcomes? If we lose our way here, our reconstruction of Kuhn and Bachelard's problematic will have been in vain. Interestingly, Bachelard comes to our rescue, provided he is viewed through spectacles provided by the constructivist sociology of scientific knowledge. The key to our problem resides in Bachelard's conception of a phenomeno-technique, the coupling of experimental hardware and a specific theory or discourse. Two generations before the advent of a sociology of experiment, Bachelard saw that the phenomena or objects of inquiry of experimental sciences are manufactured in and by experimental hardwares, because such hardwares are themselves developed, used and understood as embodying conceptual structures. The conception of the phenomeno-technique is Bachelard's major bequest to the emerging sociology of experiment and new historiography of experimental sciences. Through it he began to suggest the constructed rather than given character of scientific 'facts', since they literally are the product of hardware-discourse couples, and, strictly speaking, exist only in relation to and in the vicinity of such couples.[13]

Construed as Bachelard literally intended, the phenomeno-technique holds little promise for our problem. Indeed, it is the cornerstone of his two-place historiography; as we saw in section III, it trades upon his distinction between qualitative discourse which cannot be phenomeno-technically embodied and mathematics which can. The notion of the phenomeno-technique can, however, be rendered more historical and therefore more useful, by refracting it through recent findings in the sociology of scientific knowledge. A revised conception of the phenomeno-technique can then be transplanted into a conception of experimental natural philosophy so as to suggest a basis for understanding the dynamics of the latter and the processes of crystallization of fields from within it. There is in Bachelard no concession to the interpretative flexibility of conceptual and practical resources commonly granted in the recent empirical literature on the sociology of scientific knowledge, especially constructivist sociology of experiment. Accordingly, there is no sense in Bachelard that hardware-discourse couples, no matter how apparently stable, are contingent products, outcomes of social processes of closure, and always subject to re-negotiation of the elements on either side of a couple. We must conserve Bachelard's fundamental recognition that hardwares are embodied theory and that the meanings of their 'outputs' or performances -- the nature of the 'objects' or phenomena they manifest -- are functions of that same embodiment of theory. We must, however, correct Bachelard's rigid insistence that embodied concepts can only be mathematical, permanent and non-contingent.

The seminal work on experiment of such sociologists of scientific knowledge as Collins (1985), Pinch (1986a) and Pickering (1984) points

precisely in the direction needed. Collins pioneered the attempt to show that decisions about how to make and use experimental hardwares and about the meanings of their outputs are social rather than natural or methodological accomplishments. His work on debates about the existence of gravity waves -- and supposed gravity wave detectors -- prompted his well-known conclusion that debate 'about what counts as a competently performed experiment...is coextensive with the debate about what the proper outcome of the experiment is' (Collins, 1985: 128). Hence, closure of a debate about the meaning of competence constitutes the 'discovery' or 'non-discovery' of a phenomenon and the determination of its properties, for the time being. We see the same interpretative fluidity and correlative need to study the social modes of closure of debate about what an instrument is and what 'objects' it discloses, in Pinch's empirical studies and in his conception of the 'externality of observation' (Pinch, 1986a; 1986b).

Broadly speaking, the goal of the social processes of closure, or specification of relevant parameters, is to achieve a relatively unproblematic understanding of the experimental hardware so that its resulting 'output' can then be accounted to be saying something relatively unambiguous about the nature of the objects of inquiry. This would apply to any historical-sociological characterization of Franklin's attempt to impose his theory of the Leyden jar, and hence his version of the jar and of the phenomena or objects -- such as his electrical fluid -- it discloses. When and where the Leyden jar, that 'monstrosity' for Bachelard, was agreed to be what Franklin said it was, and to be doing what Franklin said it was doing, there and then Franklin's version of the 'electrical fluid' existed. Franklin's version of electrical fluid existed in a certain way of using and glossing this hardware. As ways of glossing -- the discourse of electricity -- changed, so did the objects manifested by this hardware, which was also thereby changed because somewhat differently glossed and managed.

Sociological analyses of experiment suggest that Bachelard's conception of the phenomeno-technique needs to be 'loosened up'. We require a Bachelard with a fluid and reinterpretable hardware-discourse couple, a Bachelard who would acquiesce in the following premises of a 'social constructivist' Bachelardian historiography. First, instrumental and experimental hardwares are accountably 'adequate' materializations of theories and concepts agreed/enforced as 'adequate' at some point in the past and 'until further notice'. Second, the performance and output of a hardware is doubly discursive and socially negotiated because of the character of the hardware as consensually frozen discourse and because

226

any output requires further interpretation, evaluation and deployment in its intended argumentative contexts. Third, the existence and continued use of a hardware does not show that the discourse accountably manifested in it is 'true to reality'. It merely shows that in the past discursive resources were used to devise hardware and standards of its 'adequate' performance and output, and that its performance is still judged adequate to goals and interests themselves open to revision and contestation. Fourth, adequate command over a hardware does not depend upon possessing a good scientific theory of that hardware. The history of the practical arts demonstrates this, and so does the history of experimental natural philosophy. In sum it would seem, contra-Bachelard and pro-Collins *et al.*, that phenomeno-techniques are fluid and socially constructed, not given in the canon of true or permanent science.

Something, nevertheless, is still missing from our conception of the situation and tactics of early experimentalists. In these early struggles to construct and impose hardware-discourse couplings, what was at stake were embodiments within hardwares of *qualitative* conceptions. One was not dealing with 'inscription devices', those highly standardized hardwares which 'black-box' many layers of theory and are used in large arrays in modern experimental science. The study of inscription devices, as originally conceived by Latour and Woolgar (1979: 45-53, 63-9), emphasizes scientists' literary construction of 'facts' through their mobilizing, interpreting and deploying in further 'texts' the already textual outputs of highly black-boxed devices. Early experimental science or natural philosophy was not characterized by such inscription devices, but by debates and struggles over the initial black-boxing of qualitative theories and concepts into non-inscription hardwares.[14] This entails that we must identify the sources, sites and dynamics of the discursive materials being so embodied. The historical fact of the matter is that many of these discursive resources that Franklin and his contemporaries were struggling to mould to, and embody in various hardwares were of natural philosophical provenance. Natural philosophy was the paramount discursive medium from which was drawn the conceptual stuff for embodiment, and to which such embodiments, if successfully imposed, were supposed to speak: as applications, confirmations, explications or justifications of natural philosophical claims. The dynamics of experimental natural philosophy involved, therefore, attempts to constitute and establish embodiments of natural philosophical discourse within experimental hardwares.

Kuhn's pre-paradigm/paradigm distinction and Bachelard's pre-science/science distinction really mark moments in larger and longer

processes of negotiation and enforcement of particular hardware-discourse couplings. These were carried out under the perceived social and cognitive hegemony of natural philosophical discourses that had already become generically 'experimental'. Indeed, below we shall argue that the tendency of experimental natural philosophy to fragment into what Kuhn and Bachelard would like to see as new experimental sciences arose from the largely unintended consequences of this dynamic of seeking couplings between hardwares and natural philosophically relevant discourse. But, before tackling the wider question of the nature and dynamics of experimental natural philosophy, we begin with the case of the Leyden jar and Franklin, looking at it as an example of a moment or node in this sort of process.

7. FRANKLIN AND THE LEYDEN JAR: EMBODIMENT OF NATURAL PHILOSOPHICAL DISCOURSE

The Leyden jar was an artifact of which the design, construction and use was based upon well established consensual understandings of concepts drawn from natural philosophical discourse and from the reservoir of contemporary common sense discourse. The coneption and construction of the jar hardware was, of course, dependent upon ordinary language understandings of terms like 'jar', 'pin', and 'water', terms which might acquire extra technical 'spins' as they were associated with the design, use and agreed 'output' of the newly stabilized artifact. More important from our standpoint, in order to build a jar in the first place, some stability and consensus had to exist about terms like 'electrical effluvia' or 'fluid'; about 'electrics' and 'non-electrics'; and about properties of the flow of fluid or effluvia and the sparking, seepage and possible containment of the same. The understandings of these objects and their properties or relations were already variously embodied in a collection of 'electrical' hardware-discourse couplings: electrostatic generators and other canonical pieces of apparatus for producing and manipulating electrical effluvia or fluid. In other words, there existed a domain of identifiably 'electrical' hardwares and the discourse they embodied. The embodied discourse about objects and properties, most importantly electrical effluvia or fluid, owed its origin and its ultimate point of reference and significance to the realm of natural philosophical discourse, with its agreed intellectual/social hegemony over ways of approaching, understanding and commanding nature. But, the borrowed elements of natural philosophical discourse were already acquiring 'spin' and specificity by their use and embodiment in this electrical realm. Terms like 'electrical effluvia' were supposed to be able to be reintegrated into overarching discourses about the system of nature but their meanings were increasingly the product of the contingencies of struggle and negotiation within the more narrow, but growing domain of electrical hardwares and embodied discourses.

Within the nascent electrical domain, there also existed routine rules of procedure, rules of thumb for working and manipulating hardwares. One such rule was derived from Dufay's work on conductors and non-conductors: in modern terms, to charge a conductor, the conductor has to be insulated; in their terms, to transfer fluid or effluvia to a non-electric, that non-electric has to be supported by an electric. As we mentioned above in Section 2, the jar's apparent violation of this rule of procedure provided one of the leads for Franklin's attempt to reinterpret what the jar was, what it did, and hence what were the relevant properties of the electrical fluid.

Franklin viewed the jar as a self-discharging device rather than a tool for delivering dramatically large jolts through discharge chains, although he did that as well. This concentration on the phenomenon of self-discharge to neutrality was the key to shaping his discourse of the jar and thus helps explain the densities and gaps in that discourse. His coming to 'clarity' about the jar occurred roughly as follows. After discharge the jar could be shown to be in a neutral condition. Now, electrical fluid had certainly entered the water (later the glass) from the generator via the prime conductor and pin. In discharge the only possibility was that the surcharge of fluid had travelled back up the pin and around the circuit to the outer surface, which must therefore have been in an under-charged condition. That in turn helps explain why the jar had to be grounded to be charged, in apparent contravention of Dufay's rule, for when the inside of the jar is charged somehow an equal amount of fluid is driven off, repelled, into the ground from the outside of the jar. If there is no grounding, there is no effective repelling of the fluid and hence no effect.

Viewing the situation this way allows us to conclude firmly that Bachelard is wrong to denigrate Franklin's understanding and construal of the jar as monstrous and pre-scientific. We have seen that the jar in intention and performance represented a reasonable artifactual embodiment of accepted natural philosophical and common sense understandings. It 'worked'; but only on condition that one arguably violated Dufay's rule, another apparently stable element in the emerging electrical domain and one which itself summarized a range of stabilized

couplings in that domain. In Franklin's own grappling with the Leyden jar, everything proceeded reasonably from the standpoint of the culture of experimental natural philosophy as the available discourse and hardware were reinterpreted and renegotiated into a new coupling, proffered for wider acceptance. In his accounts of the jar, he revised available discourse about electrical fluid, its flow, conservation and self-repulsion across the glass in a way 'adequate' in his and others' view to allow manipulation of the jar, explanation of its salient effects, and improved understanding of the rationale of its design and construction (Cohen, 1956: 452-7; Heilbron, 1979: 330-1; Franklin, 1774: 180-2).

Nevertheless, all of this does not signify that Franklin had achieved some sort of paradigmatic, science-making breakthrough in the senses conveyed by Kuhn's two-place historiography. We can see this very clearly by recalling the nub of Kuhn's understanding of the Franklinian paradigm, or exemplary problem solution. As noted earlier, Kuhn claims that the key to Franklin's understanding of the jar was his possession of a concept of electrostatic induction, paradigmatically embodied in his explanation of the jar and making possible the critical distinction between the conductive phenomena of the fluid and its exertion of repulsive force at a distance (upon itself). This distinction allowed Franklinist electricians to pass beyond the Leyden jar itself and to conceive of and manipulate instances of what we termed the 'condenser as such'. Therefore, this distinction ultimately permitted the emergence of a fully mature, *i.e.*, mathematical, electrostatics.

What did Franklin 'know' about this modern conception of 'induction'? Strictly speaking only this: his newly revised discourse of the jar had a moment in it which we recognize as the place where *we* would insert the term 'induction' in all its modern connotations. Franklin simply spoke of repulsion across the glass (Franklin, 1774: 229) or of fluid 'driven out' of the outer surface when the inner surface is charged (1774: 180; Heilbron, 1979: 331). The momentum of his account drove him in this direction: his awareness of the Newtonian possibility of speaking about repulsion gave him the natural philosophical warrant and some materials for so doing. But, Franklin did not thematize this repulsion as a central theoretical problem or topic. He simply inserted this 'repulsion' into his ongoing talk as a plausible move in his universe of discourse, given what he did thematize--the equivalence of input and output of fluid. In further rounds of negotiation of this and other hardware-discourse couples, Franklin never consistently seized upon, thematized or deployed the distinction between fluid flow and its distance effects. At best, the distinction lurks in his discourse. He had a fitful, unfocused recognition of it, for example, when he improved upon Canton's discourse of Canton's own 'induction' experiments, as they are termed in standard histories of science (Franklin, 1774: 302-6; Cohen, 1956: 524-31; Heilbron, 1979: 376-7, 336-9). The main thrust of Franklin's subsequent articulation of his discourse was away from focusing on the distinction and toward a discourse of the dynamics of 'electrical atmospheres' (Home, 1972). As we argued earlier, the construction and deployment of the distinction was a long, contingent struggle played out over two generations amongst the very electricians whom Kuhn saw as working within a Franklinian paradigm-with-a-destiny.

Kuhn's conception, we reiterate, is not tenable. We do not therefore say in whiggish fashion that Franklin missed or clouded the distinction; nor, in the style of Heilbron (1979: 335-6) do we accuse Franklin of 'bad physics' and try to salvage some progressive 'phenomenological core' of his achievement hidden within his increasing concern for atmospheres and their dynamics. Franklin had good reasons to proceed in the directions he did. In the felicitous terminology of Barnes (1982: 30, 45-53), Franklin and the Franklinists had to negotiate the next applications of concepts such as 'repulsion across the glass', and the 'flow/distance effects distinction' because there is no algorithm for doing this, no prescriptive paradigm with a destiny. Hence nobody was being unscientific, pre-scientific or downright stupid for not negotiating in the direction of those 'applications' that would be approved of by Kuhn, Heilbron and other implicit or explicit whigs.

Franklin's hardware-discourse couple was neither a pre-scientific monstrosity without a future nor an exemplary problem solution constituting a paradigm with an algorithmically derivable destiny. It was neither a science-making achievement nor a cognitively pathological failure to constitute a science. Franklin constructed and attempted to enforce upon his peers a revised natural philosophical discourse about electrical fluid, its flow, conservation and self-repulsion, a revised discourse adequate to explaining and manipulating the jar which in turn came to be seen by him and his allies as embodying that discourse. He did not have classical electrostatics, or even its concept of induction; the application of his revised discourse of electricity to new or revised hardwares was not obvious or algorithmic. His was a relatively successful bid to establish and stabilize a particular couple in an expanding and contested region of experimental natural philosophy. His couple is precisely the sort we expect to see coming and going under the regime of experimental natural philosophy. Its subsequent history under that

regime, and its role -- as resource and as object of renegotiation -- in the development of the electrical domain within experimental natural philosophy, consists in a concatenation of socially negotiated contingencies like the ones that first constituted it.

In other words, Franklin's proffered couple was a node in a process consisting in a struggle to constitute, stabilize and institutionalize such couples, and, if possible interlink them with additional couples to which they were accountably 'related'. Experimental natural philosophy was the generic field of discursive resources, social sites and power relations in which this process took place. The relative stabilization of a couple, until further notice, constituted a node. On the one hand, such stabilization always involved some shift, reinterpretation or renegotiation of existing objects of natural philosophical discourse -- their nature, properties and relations -- and their embodiment in new or reconsidered hardware. On the other hand, any stabilized couple provided a resource, a point of reference, for possible flexible use in further rounds of struggle to constitute new or revised couples. A node, therefore, was a relatively settled couple, in which stabilization (for the time being) involved some drift or shift in existing natural philosophical understandings and which was available for deployment in a fluid and reinterpretable way in further rounds of attempts at similar stabilizations.

All this suggests an analogy that we shall develop further in later studies. The process of constructing and negotiating couples under the aegis of a supervening reservoir of natural philosophical discourse may be likened to the process of making particular, contextually shaped utterances within a given natural language. The proffered couple resembles the utterance, and the hegemonic natural philosophical discourse resembles the background language system which both shapes and makes possible particular utterances, and yet which exists largely in virtue of them and their unfolding, contingent history. On this analogy, Franklin, like any other experimental natural philosopher, was, so to speak, conversing about the hardware -- with himself and with his fellow natural philosophers. His language, his reservoir of resources for talking and the rules for doing so were supplied by his knowledge of natural philosophy with its already existing electrical domain and, of course, by the resources of ordinary language and usage. But, as conversations are always in context, so Franklin's hardware-discourse coupling for the jar was one such utterance and, like an utterance in natural language, it might or might not have become a resource, topic or source of models for further rounds of utterance (Watchirs & Schuster, 1987).

8. DYNAMICS OF EXPERIMENTAL NATURAL PHILOSOPHY

With the example of Franklin in place and armed with our sketch of the concepts of hardware-discourse couples, nodes, and an electrical domain, we proceed to a fuller articulation of our general conception of experimental natural philosophy and its dynamics. We have introduced the conception of an emergent electrical domain within the field of experimental natural philosophy. A general understanding of such domains and their formation is central to our model of experimental natural philosophy. It should already be clear that the idea of phenomeno-technical nodes must be viewed in relation to it. We now proceed to articulate the idea of emergent domains, making use of our heuristic analogy between the dynamics of language and experimental natural philosophy. Domain formation fundamentally arose from the imperative to understand and command hardwares through natural philosophical discourse, an imperative built into generic experimental natural philosophy. After all, to have discoursed about hardwares in, say, traditional craft languages would not have been part of natural philosophy' at all, and could not have triggered the dynamics we are about to explore.

Consider first the simple case of an isolated hardware-discourse coupling which has settled down as a 'fact', the hardware taking on, for the time being, a settled meaning. This would mean that the hardware's construction, use and output are established and it would also mean that the phenomena or objects realized or manifested by the hardware, so understood, also exist. The very settling down of the couple means that there has been produced a particular utterance, a particular version of the natural philosophical discursive elements in question. This process would be compounded by the local, technical 'spin' given to those elements of ordinary language deployed in that coupling. The discourse of the hardware in this coupling would thus be an amalgam of mutually conditioning natural philosophical and ordinary language terms.

In our simple case the utterance would have little significance for domain formation. It would likely either be ignored at the level of grand natural philosophical systematics or be trivially reabsorbed into one or another system as a small fact, illustration or piece of evidence. The situation would be different, however, if there were a set of accountably 'related' couples and if several actors or groups of actors were involved in the process of 'settling' and relating them. This would then constitute a nascent domain. Specific couples within the domain could become resources and objects of contention among and within schools of natural

philosophizing, as competition over priority and hegemony within the domain begins to emerge as an issue. In later rounds of debate, understandings of some couples might be taken for granted by some or all of the actors, as they mobilize them in their respective endeavours to define and impose new couples. This in turn would tend further to crystallize the domain and give jt a density throug a shared repertoire of couples and understandings.

Domain formation may be likened to the formation of a dialect, if we focus upon the subtle meaning-shifts of natural philosophical terms and their amalgamation with terms derived from ordinary language. That is, a domain is like a dialect within an overarching 'official' language of natural philosophy. The existence of such an emergent regional natural philosophical discourse, a dialect, is tolerably clear in the case of electricity on the eve of the advent of the Leyden jar. As we saw above, 'conduction', 'generation', 'sparking' and 'leakage' were all examples of ordinary language terms increasingly given technical spins and connotations in the emergent domain of electrical work prior to the advent of the jar. These had merged with such more properly natural philosophical terms as 'fluid', 'effluvia' and 'electric' to form an electrical dialect within natural philosophy. The dialect itself was used in projecting and constructing the jar. Elements within the dialect were shared by such actors as Franklin, Nollet and Watson who then contended definitively to construe the jar. The dialect was in turn modified through the enforcement of Franklin's discourse of the jar as definitive, for the time being.

In a nascent domain, and in and around its growing cluster of couples, there will develop low-level rules, maxims of procedure, state-of-the-art expectations about entities in the domain and their uses, relations and behaviours. This is symptomatic of domain and dialect formation. Typically, such rules, principles and maxims have no partisan natural philosophical significance. That is, such 'domain rules and principles' do not serve as resources for one school of natural philosophy over against others, because they are neutral amongst the contending schools, being easily communicated and/or translated amongst them, and because they are agreed to be necessary for anybody's functioning within the domain. We have already instanced Dufay's rule for charging conductors, a rule of thumb that needed to be followed and could be understood by electricians regardless of their natural philosophical predilections. Indeed, Dufay's rule was a working maxim within a larger discourse on 'electrics' and 'non-electrics', a classification of their instances and properties. This entire classificatory discourse might be considered to have been a set of domain rules in that it did not uniquely presuppose or support one natural philosophical ontology as opposed to any other and yet was necessary for navigation within the electrical domain. The initial development and acceptance of Gray's work on conduction in rather ontologically neutral terms also indicates the presence of a set of domain rules (Cohen, 1956: 370; Heilbron, 1979: 245-9). No particular species of natural philosophy had a special purchase on the discourse of conduction; it was intelligible and necessary for anyone playing the electrical game. One can go so far as to say that the emergent electrical dialect consisted precisely in such domain rules, principles and maxims. Their ontological neutrality (or easy translation) bespeaks the crystallization of a domain as well as the tendency for natural philosophical systematics and conflict to be displaced by engagement within the domain.

It is important to see that domains and dialects would have formed above and beyond the conscious intentions of any natural philosophers to demarcate and dominate them. The mid-seventeenth-century shift in the generic character of natural philosophy set in train the struggle to attach natural philosophical discourse to hardwares, to speak about hardwares in the name of something as important as natual philosophy. When an experimental natural philosopher tried to enrol his peers in a bid to embody natural philosophical discourse in a couple, he was speaking in the name of his favoured species of natural philosophy. Peers might or might not follow such a bid, but the issue was engaged at the level of natural philosophizing, meaning that for all concerned there were at stake the existence and properties of the fundamental furnitures of nature and the laws of their behaviour. Nevertheless, although the main intention may have been to extend the empires of particular systematizations of natural philosophy, domain and dialect formation were set in train as unintended consequences. Conversely, as these processes continued and domains and dialects began to crystallize, natural philosophers could then thematize these tendencies and began to contend for supremacy in demarcating and dominating the nascent domains, thereby accelerating the process.

Our conception of domain and dialect formation casts further light on Kuhn's quandaries over the pre-paradigm 'schools'. These were defined primarily in terms of metaphysical predilections or, in our terms, natural-philosophical commitments. As we saw, Kuhn had trouble factoring such schools into his model. On the one hand, he had to depict some of the schools as too 'paradigm-like'; on the other, to introduce into the schools a suitable degree of 'Baconian' fact gathering so that they would remain on the pre-paradigm side of the historical divide. The phenomenon of domain and dialect formation cuts across the terrain marked by such Kuhnian schools even when we construe them in our terms as divergent and variously organized strands of natural philosophical opinion. When Kuhn sought science-making breakthroughs in one or another school he was looking for the wrong thing in the wrong place. His alternative strategy of tracing the long-term maturation of the 'Baconian sciences' also missed the mark for he misconceived experimental natural philosophy and its peculiar dynamics.

The conceptions of domains and dialects and of their dynamics constitute the kernel of our model of experimental natural philosophy, the key to our reconfiguring of Kuhn's and Bachelard's problematic. In order to make further headway with this model, we must turn to the issue of the earliest stage of experimental natural philosophy, grafting our notions of domains and dialects onto it. Experimental natural philosophy was the terminal phase in the history of the generic field of natural philosophy, during which practitioners were concerned to assert the hegemony of natural philosophical discourse over existing and yet-to-be constructed hardware-discourse couples. For example, it declared its relation to, and hegemony over, much of the existing field of the practical arts, hoping thereby to replace or displace localized, parochial craftmen's discourses with natural philosophical discourse.

At first, in the hands of Bacon and the early mechanists, experimental natural philosophy was merely a 'declaratory' doctrine. It asserted its in-principle connection to, and right of command over, existing constellations of couples in the practical arts and those couples already co-opted into competing Paracelsian or natural magical species of natural philosophy (Bennet, 1986). Initially, it had little to show for this effort in terms of actual co-option and reconfiguration of the discourses of hardwares. When Bacon, the early mechanists or the triumphant advocates of ECM addressed the practical arts and artisans, it was initially in this imperative and declaratory fashion, the cash value of which was, employing Pinch's terminology (1986b), to invoke a new, global 'evidential context '-- natural philosophy -- for the use, evaluation and understanding of the hardwares of the practical arts. Moreover, in so far as Bacon, the early mechanists and the advocates of ECM also sought to defeat and displace competing systems of natural philosophy, systems that had already co-opted portions of the practical arts, the exponents of experimental natural philosophy had either to reject such co-opted portions, or redescribe them in terms of their own favoured Baconian/mechanist discourse.

In broad terms this is the very process by which the generic field of natural philosophy became 'experimental'. As outlined in Section 5, it required Bacon and the mechanists first to reshape the terms experience and experiment in ways different from and 'superior to' their use in Aristotelianism and species of magical natural philosophy, and second to claim hegemony over the practical arts in the name of their revised natural philosophical discourses. With the wide triumph of ECM, this declaratory form of experimental natural philosophy came to define the field of natural philosophizing. It instigated a struggle to co-opt and link hardwares, and that process led, largely unintentionally, toward domain and dialect formation. This is not to say that the declaratory style of experimental natural philosophy could and did produce dramatic and quickly accelerating results in the form of constellations of hardware-discourse couples and a correlative crystallization of domains. Rather, the existence of the declaratory moment of experimental natural philosophy was critical in both starting and shaping the type of natural philosophy that led over the next century and a half to domain formation and the terminal fragmentation of the traditional field of natural philosophy. For, without declaratory experimental natural philosophy, those domains eventually called experimental sciences would not have developed. The evolving domains of couples in the practical arts and technics would not have been declared objects of natural philosophical attention. These domains instead would have continued to evolve apart from the elite culture of natural philosophizing.

One would therefore expect that early in the history of experimental natural philosophy, the main thrust was to cover and co-opt hardwares with discourse aimed at supporting one's own natural philosophy and attacking others. Technical details of hardware and outputs may have been relatively less interesting in themselves than the drive to enrol the hardware and its newly insinuated discourse for systematic purposes: to display the correct natural philosophical system, method, concepts or values. For example, the politics of inter-system debate fuelled the early controversies about the meaning, nature and function of the barometer and the air pump, with each side viewing its own discourse of the hardware as manifestation of, or evidential support for, their preferred natural philosophical objects and relations (Middleton, 1964; Shapin & Schaffer, 1985). Nevertheless, once a hardware became the focus of such inter-system competition, pressures appeared toward the formation of a domain, a dialect and domain rules. For there to have been debate and struggle over accounting a hardware and its output, there must have been some overlap in the glosses and accounts rendered, competing but partially overlapping accounts becoming resources for further rounds of debate about this hardware and other accountably 'related' ones. If the use of a given system of natural philosophy to provide discourses of hardwares produced a regional dialect, then the pressure of inter-system conflict on the level of natural philosophy would only accelerate and inflame the process of domain formation.

The early development of the domain of electricity displays some of these features. Prior to Gilbert the electrical domain, if one may so stretch the term, consisted in the electrostatic properties of amber, effects grasped in ordinary language as well as at the margins of natural philosophical discourses of natural magic and curiosities. Having to hand only the amber effect along with his own lengthened list of similarly 'electric' substances, Gilbert (1600) inscribed the electrical domain within natural philosophy primarily as the 'non-magnetic'. This was in keeping with his goal of d stinguishing magnetic phenomena and their 'spiritual' causes from electrical phenomena and their merely mechanical, effluvial causes. His understanding of the magnet, of the constitution and 'output' of the magnetic compass, and of his magnet experiments in general, were relative to and largely dictated by his concern to establish a novel system of 'magnetic' neo-Platonic natural philosophy that embodied and supported a modified Tychonic cosmology. He claimed hegemony over such craft practices as navigation, mining, and metallurgy, and a right to co-opt discursively findings and facts embedded in them. In fact, he did co-opt and re-gloss existing couples, most notably on the critical issue of the compass.

Gilbert's natural philosophizing of the magnet, its phenomena and chief hardware, was too important and impressive a gambit in the field to be ignored by his competitors. So, for example, the efforts of early mechanists such as Descartes (1644) were directed at re-glossing Gil ert's experimental work in mechanistic terms, rather than at extending the number and type of magnetic couples or articulating their technical specifications, matters that tended to remain in the ambit of master artisans. Descartes also devoted considerable attention to preserving and capturing the 'cosmic' significance of magnetism, the keynote of Gilbert's system. He replaced Gilbert's story of the cosmos making and binding role of the spiritual magnetic force with a mechanist's story of an equally cosmic magnetism which was now the purely mechanical effect of a species of corpuscle of particular and peculiar shape and size moving in suitably configured aggregations of ordinary 'third matter'.

In view of this titanic natural philosophical struggle to co-opt and 'enrol' magnets and magnetism, in Latour's (1987) slightly bizarre terminology, the phenomenon of electricity remained insignificant. Despite Gilbert's multiplication of the category of 'electrics', it was trivially mechanistically explained, and still occupied a marginal place in the hierarchy of natural philosophical topics. In the seething cauldron of seventeenth-century natural philosophical conflict, it was a speck of flotsam. The non-existence of a range of 'electrical' phenomena and the provision of the known effects through the most trivial of 'hardwares' -- the substances themselves -- guaranteed this marginal status. This helps explain the flatness, for example, of Boyle's perfunctory attempt (1675) to co-opt electricity to his brand of corpuscular mechanism. As is well known, this situation only began to change in the early eighteenth century. The electrical domain began to widen and thicken, with heightening natural philosophical interest, only with Hauksbee's multiplication of electrical phenomena and constitution of prototypical electrostatic generators, together with the implication in Newtonian natural philosophy that electrical phenomena might bespeak a significant power in nature. The subsequent multiplication of hardwares and effects both raised the natural philosophical stakes and promoted those phenomena of domain, dialect and domain rule formation discussed above.

9. HISTORIOGRAPHICAL TRIALS OF NATURAL PHILOSOPHY

We have argued that a conception of experimental natural philosophy and its dynamics can reorient our view of the Kuhn/ Bachelard problematic, as well as our first order picture of the trajectory of experimental science in the latter stages of the Scientific Revolution and in the eighteenth century. Our model invokes a conception of generic natural philosophy as a field of social and cognitive struggle and views experimental natural philosophy as a consequence of a shift within that field. Experimental natural philosophy was expressed initially as a declaratory doctrine, which set in train a largely unintended dynamics of domain and dialect formation, through the imperative placed upon the co-opting and commanding of hardware-discourse couples by means of natural philosophically relevant resources. The resulting struggle to establish and stabilize couples bespeaking one or another species of natural philosophy fostered that drift of discourse and clustering of 'related' hardwares that we have termed domain and dialect formation. As regional domains and dialects developed, so too did wider self-identifying communities of practitioners, working in and through their respective domains. At certain times and places actors who had developed skills and reputations within a domain could in turn seize upon it as something worth contending over thereby fomenting more 'proto-disciplinary' struggles to demarcate, define and command particular domains. It is these latter formations and types of struggles that have prompted from actors and historians alike discourses on the 'birth' of this or that field of experimental science.

The implications of this model are clear for any two-place historiography whether of whiggish-inductivist, Bachelardian or Kuhnian temper. We submit that the task of historians of experimental sciences in this period is not to seek the mythic *origins* of sciences. We do not need tales of the sudden discovery of proper methods, metaphysics or models, which supposedly shattered the debilitating fascination exercised by putatively non- or pre-scientific beliefs and enterprises. Nor do we require myths of the slow gestation of happily 'pre-formed' sciences. Rater, the historian of early experimental science must first grasp the social and cognitive nature of natural philosophy, both generically and in its historical manifestations as competing systems, and then describe the dynamics of change, development and ramification by which were produced those moments, turns and crystallizations misread as *de novo* outbreaks or inventions of experimental 'sciences'. We need to know not why and how the experimental fields count as sciences, but what natural philosophy and experimental natural philosophy were, and how and why they ramified and ultimately dissolved into the experimental fields. In short, we need an historiography of latent processes and of unexpected and unintended endings, not an historiography of heroic births, whether miraculously sudden or long gestated (Butterfield, 1965: 43, 51-2, 59-60).

This is not to assert, however, that our model provides a sufficient historical description or explanation. It is preliminary, suggestive and mainly heuristic in intent. This becomes clear if one analytically distinguishes what we might term primary and secondary processes shaping the historical trajectory of experimental natural philosophy. Our model is of the primary process only, and yet the primary process was of course only ever played out in and through specific sites, struggles, and contingencies of a social, institutional and biographical nature that are the stuff of detailed state-of-the-art social history of science and historical sociology of scientific knowledge. Theorizing about the primary process does not entail that we conjure out of existence these contingencies and conditions. It means instead that we place the detailed analysis of local conflicts and contingencies in a new context, emphasizing that in and through those local contingent events and struggles there played upon the actors discursive and social imperatives, resources, opportunities and constraints bound up in the existence of a culture of experimental natural philosophizing. Just as one cannot deduce the details of any instances of secondary process from the model of primary process, so one cannot fully grasp events in the secondary process without understanding the actors moving in and through the field of experimental natural philosophy. One must view actors' machinations as partly enabled and constrained by the field and conversely one must view their machinations as at each turn instancing it, (re)constituting it, altering it and driving on its dynamics.

Understood in this manner, our model prompts three broad sets of historiographical observations about experimental science and natural philosophy. First, it follows from our model that the historiography of experimental natural philosophy cannot simply consist of snapshots of particular contrversies, debates and closures no matter how exquisitely executed these pictures may be. Even Shapin and Schaffer's (1985) brilliant reconstruction of Boyle and Hobbes' debate over the status, meaning and output of the air pump does not capture some generative core or founding step in experimental natural philosophy. Instead, it reconstructs a moment in the early phases of experimental natural philosophy, characterized by continuing debate over systematic hegemony and also by the need to specify and enforce there and then some socially constructed and stabilized protocols for obeying, in this concrete situation and hopefully in others, the imperative to form couples impregnated with natural philosophical discourse. Shapin and Schaffer document a local struggle to constitute what we may term a Latourian social-technical actor network of Anglican divines, gentleman amateurs, and skilled, committed experimental corpuscular-mechanists. Certainly, the newly emergent field of declaratory experimental natural philosophy did not dictate or determine the details of this local passage of struggle. Yet, it was a medium of resources and constraints, of imperatives and possibilities, in which took place this struggle, in so far as it was perceived as a struggle in and over the philosophy of nature, as that enterprise and field of conflict was lived and breathed, not only in London and Cambridge, but also in Paris and Pavia. Moreover, subsequent rounds of doing experimental natural philosophy in other times and places did not in principle or in fact have to occur in or through this locally constituted network. Anglican divines and English latitudinarian gentlemen were not always and everywhere the essential players in other locally woven social-technical-discursive nets, and neither did the particular practices and protocols constructed and enforced in their net have any stability or transferability except in so far as they registered in the repertoire of resources possibly available to others -- elsewhere and at other times -- concerned to play (versions of) the game of experimental natural philosophy. As we do not comprehend an utterance apart from its language, so the language, as a structure undergoing contingent processes of change, does not really exist outside the ongoing concatenation of utterances. So, beyond and within the snapshots, depictions of secondary process, there must be correlative attention to the field of experimental natural philosophy, to the web of imperatives, opportunities and constraints it wove in and through each contingent situation. This web's texture must be taken to have altered over time, as it played and was played through concatenations of contingent events and encounters, from a declaratory moment to nascent and then thickening domains to self-conscious struggles over domains in preference to struggles about overarching systematizations.

Second, just as snapshots will not suffice for recasting the history of experimental natural philosophy, so hasty and total contextualizations of episodes occlude our understanding of the historical processes involved in the trajectory of experimental natural philosophy. For example, Shapin (1980; 1981) and Schaffer and Shapin (1985) advance a broad socio-political contextualization of natural philosophy, asserting connections between natural philosophers' struggles to assert and enforce conceptions of the natural order and the social forms and protocols of experimentation, on the one hand, and struggles to preserve or reform conceptions of the larger social order, on the other hand. As against this, our model would imply that the field dynamics of natural philosophy and experimental natural philosophy cut across and through any such immediate larger contextual determinations, when they did exist. Moreover, it would suggest th t concern with relations between the political order of natural philosophizing and the larger political order was neither essential to the dynamics of natural philosophy, nor a necessary or sufficient cause of its existence and ongoing social reproduction. Any such concerns of particular actors were contingent, secondary factors, certainly conditioning and refracting how they played couplings in their favoured domains, but certainly not the complete set of constraints, resources and aims available to and pressing upon them.

Third, our model gives some orientation to the present proliferation in the literature of confusing aperçu of 'natural philosophy' caused by the awakening of interest in the category of natural philosophy on the part of some historians of eighteenth-century science. Heilbron (1980), for example, is eager to package the developments we have grasped as 'domain/dialect formation' under the rubric of 'experimental physics', an activity he can easily see developing toward the emergence of relatively autonomous fields of mathematico-experimental science. His sober professors, academicians and scientists, practitioners of experimental physics, strive in instrumentalist terms toward the maturation of the mathematical/experimental fields, properly eschewing unhelpful, obstructive questions about 'matter theory'. But what, then, have they to do with the dramaturgical shenanigans of the sort of natural philosophers discovered by Schaffer (1983), maestros of morally and politically edifying public experimental spectacles? How, moreover, do we relate the activities of either group to yet a third body of more utilitarian and pedagogically motivated popular natural philosophical lecturers, the texts of whose courses of natural philosophy are organized around apparently independent chapters on such areas as mechanics, optics, hydrostatics, astronomy, electricity, chemistry and other topics (Cantor, 1982)? Should this proliferation and heterogeneity drive us to downplay the historical validity of the category 'natural philosophy', in the style of Heilbron, or, should we seek to define some metaphysical, methodological or even discursive consensus that really united all parties? We suggest our model does permit a glimpse of the wood comprised by these trees, without the expedients of instrumentalist denial or idealist desperate measures. Heilbron's proto-physicists were, of course, operating in particular nooks and crannies of eighteenth-century natural philosophical space, the academies, societies and universities being their favoured haunts. Hence they tended to seek the definition and control of their favoured experimental domains in and through the resources and rituals worked out in and for such sites. But, it remains the case that Heibron's proto-physicists were playing the game of hardware-discourse couplings within and through nascent domains and dialects; that these were shared with denizens of other sites; and that they accordingly were in fact natural philosophers doing experimental natural philosophy. The secondary process imperatives of their particular sites and styles may have increasingly incited them to define themselves over against competitors as the only legitimate inhabitants of the emerging domains; but this does not excuse them from being numbered by historians amongst the members of eighteenth-century experimental natural philosophy, and it certainly does not mean that they alone should engage our understanding because they partook of some privileged destiny of science-making.

Analogous observations hold for Schaffer's dramaturgical agitators. Their hardware-discourse couplings, their readings of hardware construction, uses and outputs, had different accents and interests attached and they were carried out in different sites for different audiences under different rituals and protocols. Moreover, they of course carried out their work in their own corner of natural philosophical space and they too tended to arrogate to themselves the very definition of natural philosophy in the name of the meanings they had invested in particular couplings before their favoured intended audiences. But, to the degree that they too played the hardware-discourse coupling game, they too were natural philosophers, although not the only ones.

As for natural philosophical courses and textbooks increasingly organized around disparate regions of phenomena, problems and their attendant clusterings of hardwares, they do not bespeak the non-existence of natural philosophy, nor should they cause puzzlement when juxtaposed with the Heilbronian or Schafferian conceptions of natural philosophy. Rather these books and courses are symptoms of that creeping, partly unintended, but increasingly intentionally exploited fragmentation of natural philosophy, marked by the shift from concerns over systematic hegemony to those over definition, demarcation and dominance of domains.

We return to the notion of natural philosophy as a social-cognitive field in which a struggle for hegemony was pursued in various institutional settings on the basis of partially overlapping and competing bodies of discursive resources. As we have argued, this was just as much true in the mid-eighteenth century as it had been in the mid-seventeenth, provided one allows for the relative homogenization of discursive resources ushered in by the advent of ECM and then Newtonianism, and provided that one also factors in the dynamics set in train by the advent of the experimental imperative across the field; that is, the accelerating tendency toward fragmentation of the field, arising from the phenomenon of domain formation, which was fostered by attempts at hardware-discourse couplings wherever they occurred--in the lecture room, public lectures and dramatic displays, the sittings of academies and societies, or the private cabinets of learned amateurs. The existence of heterogeneous types, styles and settings of experimental natural philosophy is perfectly comprehensible on this basis. There was no consensus in natural philosophy in the sense of a dominant style or site of experiment, ontological commitment, or characteristic attention to discourses of religion and politics. There was, at the level of primary process, only the field and its dynamics played in and through local sites and struggles. In sum, then, the advent of a constructivist sociology of experiment challenges us to re-write the history of experimental science in a style and temper unknown to Kuhn and Bachelard, not to mention garden variety whigs; but that enterprise can only be pursued through the formulation and deployment of revised categories of natural philosophy and experimental natural philosophy. This undertaking will immunize our efforts against the dual pathologies of merely micro analysis and hasty, totalizing contextualization, and it will affect our understanding not only of the eighteenth century, but also, as we have hinted, of the process of the Scientific Revolution as well.

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NOTES

1. Kuhn's use of the term 'gradual' does not undermine this judgment, because the overall effect of his discussion is to program the future outcome of much subsequent history into the moment of assimilation. The denouement of 'articulation' in mathematization is given as a destiny from that point.
2. The tendency for Kuhnian internalist historiography to rejoin traditional whiggism is discussed in Watchirs (1983).
3. Kuhn (1970: 21): 'Ever since prehistoric antiquity one field of study after another has crossed the divide between what the historian might call its prehistory as a science and its history proper. These transitions to maturity have seldom been so sudden or so unequivocal as my necessarily schematic discussion may have implied. But neither have they been historically gradual, coextensive, that is to say, with the entire development of the fields within which they occurred. Writers on electricity during the first four decades of the eighteenth century possessed far more information about electrical phenomena than had their sixteenth-century predecessors. During the half-century after 1740, few new sorts of electrical phenomena were added to their lists. Nevertheless, in important respects, the electrical writings of Cavendish, Coulomb, and Volta in the last third of the eighteenth century seem further removed from those of Gray, Du Fay and even Franklin than are the writings of these early eighteenth-century electrical discoverers from those of the sixteenth century.' Kuhn's relation to Koyre in this regard is discussed in Watchirs (1983: 8-15).
4. This, of course, is a reading of Heilbron much conditioned by commitment to a social constructivist position on facts, testing and experiment. Heilbron himself leans in quite a different direction, back toward the point where orthodox Kuhnianism of paradigm-as-destiny encounters good, old fashioned whiggery. For, above and beyond the mass of detail in Heilbron's study; that is, whenever the forest of contingent complexity is glossed for the supposed historical big picture, the literary effect of positivist, instrumentalist progress toward quantified electrostatics is produced. All struggle, debate and indeed stupidity aside, mature electrostatics of textbook form was going to eventuate, once Franklin had done his work (Heilbron, 1979: 336, 377-8, 389, 396, 415, 436-8).
5. The reader can take this remark as a promissory note for a more social constructivist historiography of early nineteenth-century electricity and magnetism that is yet to be written, but which can be spied with the proper set of spectacles.
6. To paraphrase Bachelard (1975: 61): the meaning of a concept must include the technical conditions of its material realization.
7. On Bachelard's view, therefore, the various sciences are each sui generis; each has its own specific system of concepts and related instrumental armoury. No single, transferable, general scientific method can explain the genesis of such sciences or their respective contents and dynamics. Indeed, Bachelard produced a typology of philosophical images of science -- conventionalism, positivism, empiricism -- by assessing the degree to which they each tend toward an idealist or materialist misconstrual of the interaction of concepts and instruments which constitutes fields of science. (Bachelard, 1949: 4-5; 1975: 139; Lecourt, 1977: 60-1).
8. Needless to say there is a problem here. If there is a sharp break and displacement of discourses, in what sense, for Bachelard, was there a relevant preceding pre-scientific discourse in that area, domain, or field? Our revision of Bachelard in the direction of the sociology of scientific knowledge and through the mobilization of the concept of experimental natural philosophy overcomes this difficulty.
9. Pre-scientific discourses, for Bachelard, are culturally, historically and psychologically contingent discourses which mistakenly try to make contact with some 'real' given world. By treating objects as external givens, pre-scientific discourses create obstacles to their own development: there is no hope of creating possibilities of variation within a discourse immobilized by its own delusion of reflecting reality. What Bachelard's account cannot therefore accommodate is the fact that the sciences traditionally have operated under methodological and epistemological glosses making precisely the same sort of realist claims. This raises the post-Bachelardian question of what functions such glosses do possess in the life of those fields Bachelard would denote as sciences (Schuster & Yeo, 1986: xix-xx; Richards & Schuster, 1989).
10. Koyre and Kuhn taught us that whiggism in the history of science means a presentist perspective, a judging of the past by the transient standards of the present. But, if we go back to Butterfield (1965) himself, we find a richer, more complex sense of whiggism. It consists first of all in the creating of hypostatized historical categories which embody what we think we value in the present. These hypostatized categories are then set to work, combatting and overcoming equally hypostatized forces of negativity, so that gradually or suddenly, but always dramatically, our valued category emerges full-blown. Kuhnian and Bachelardian two-place historiography is therefore whiggish in the most serious professional historian's sense of the term. See Section 9.
11. This is the 'Koyréan' Kuhn speaking, first recognizing the often dominant role of deep ('metaphysical') preconceptions in the practice of any mature science and then going beyond Koyre in conceding that metaphysical commitments other than the 'Platonic' can underlay successful, normal scientific traditions.
12. It is fashionable, and correct, to maintain that late seventeenth mechanists, at least in England, made considerable capital out of what often was a necessity -- political, religious or technical -- to acknowledge 'spirit' and active agency in nature (Henry, 1986). It is not correct then to infer that the 'mechanical' philosophy is a misplaced or non-

existent historical category. One simply needs to distinguish within the textual body of any particular mechanist's system a level or dimension of declaratory or self-glossing discourse, where the system purports to gloss itself--sum itself up for presentation to audiences. Such glossing need not have been consistent with, or even an 'adequate' representation of, the nuts and bolts of the system of which it is a part. At this level most mechanical philosophies will declare themselves opposed to magic, alchemy and the claims of 'spirit', quite independently of where and when some concessions need to be made elsewhere in the discourse. Cartesianism is a nice negative example. Completely mechanist and anti-spirit at the declaratory level, Cartesianism harbours that curious 'first matter' which Descartes insists behaves fully mechanically, but which easily might be taken to have non-mechanical capabilities. In an English context, a hypothetical 'Descartes' himself might have seen fit to have pointed this out, whilst still insisting at the declaratory level on the fully mechanical character of his natural philosophy.

13. Moreover, Bachelard's model of science had two other major implications for the history and social studies of science that have only recently begun to be realized: (1) that all set piece method discourses are necessarily ineffective in the terms they proclaim for themselves (Schuster & Yeo, 1986; Richards & Schuster, 1989); and (2) that if science is 'phenomeno-technical', its analogies to and relations with technology may be richer, more subtle and nuanced than existing models of science and technology suppose (Latour, 1987).

14. It is interesting to place these observations in the context of Pinch's (1986b) illuminating analysis of 'externality of observation' in highly black boxed modern instrumental complexes. Experimental natural philosophers were often in a position analogous to those who, in Pinch's account of the 'solar neutrino telescope', would only be willing or able to acknowledge observation of a 'splodge on a graph'; that is, the first 'thing' in the negotiable chain of possible 'things' made observable by the device. Having no hardwares black-boxed, early experimental natural philosophers were attempting to make manifest in their hardwares immediately perceivable 'objects' or properties of objects. The objects and properties were discursively realized and embodied in natural philosophical talk, and correspondingly the hardwares were problematical in themselves as objects--what they did and what they manifested (as said and thought in natural philosophical terms). Another interesting contrast between studying modern experiment and experimental natural philosophy is that even Pinch, who attends closely to levels of hardware embodiment (and debates about them) and to the historical development of such embodiments, is actually more concerned to analyze levels of *argument* than levels of *hardware embodied meaning*.

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