Contextualizing Science: From Science Studies to Cultural Studies[1]

Vassiliki Betty Smocovitis

University of Florida

1. Introduction

For once I find myself in the unusual position of disagreeing with Joe Rouse for I do not think that science studies can or should be simply "modeled" after cultural studies as he has suggested.[2] Instead I will argue for a stronger relationship between science studies and cultural studies that follows historically, if not logically from the progression from HPS to science studies and thence to the cultural study of scientific knowledge. The end-point of the cultural study of scientific knowledge, at least as I will locate it in this paper and elsewhere, for it depends on how one locates the meaning of culture and its study, is a return to the perspective of the scientist having made roughly a 359 degree angle of departure; that one degree more or less gives the critical distance to "defamiliarize the familiar" perspective of the scientist and to "contextualize" scientific practice.

"Discourse, Practice, Context: From HPS to Interdisciplinary Science Studies": Summary of Discussions to Date

The transition from HPS to science studies and thence to cultural studies was the subject of discussion at the spring 1994 University of Florida workshop titled *The New Contextualism: Science as Discourse and Culture*. The seeds of the workshop had been sown earlier at the *Narrative Patterns of Scientific Disciplines* conference in Tel Aviv and Jerusalem in 1992 where Joe Rouse and I had the chance to confer on science, narratives, and disciplines within our own narrower domain of science studies, as well as with the wider domain of other participants from both the sciences and humanities.[3] Earlier still in 1990 many of us had assembled at a Stanford conference organized by David Stump and Peter Galison with the title *Disunity and Contextualism* to discuss the philosophy of science studies. When Alison Wylie invited us to participate in the planning for the present PSA symposium, we decided to pool our resources and come together at the University of Florida in March 1994 as part of a pre-PSA symposium discussion. Thus, as Alison Wylie has already indicated, the present panel walked through some of these issues last spring.

The program organization for the Florida workshop followed the transition from HPS to science studies and thence to cultural studies. The subtitle of the workshop *Science as Discourse and Culture* was a direct response to Andrew Pickering's recently edited book titled *Science as Practice and Culture* (Pickering 1992) with the substitution of the word discourse for practice. The heading or lead title, *The New Contextualism* was meant to reflect the view of science as a "contextual" practice,

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and the "new" in the title was meant to draw a distinction between newer views of contextualism that are distinct from the more conventional views of context as mere synonym or substitute for "external". The goal of the workshop in Florida had been to discuss to what extent science could be viewed as discursive or non-discursive practice, how to move away from the sterile distinction between external and internal understandings of scientific knowledge, and what exactly "contextual" approaches to science meant for each of our respective disciplines.

The first session drew together philosophers David Stump, Brian Baigrie, and Alison Wylie with Andrew Pickering, who served as an able representative of the sociology of science. All participants in that session, titled *From History and Philosophy of Science to Science Studies*, took for granted the union between the H (history) and P (philosophy), and began their discussion by assessing previous attempts to incorporate the newer S (sociology) especially with respect to philosophical procedures. The transition that was implicit in the organization of this session therefore took participants from HPS to science studies.

The remaining sessions took participants logically from HPSS (also termed science studies) to the cultural study of scientific knowledge. Participants in the first of these sessions included Joseph Rouse and Elazar Barkan who were assigned the task of discussing what such a transition meant and how it would be achieved. This was followed by the third session which gave concrete examples of what discursive analysis of science looks like in the work cultural and gender historians. The final session brought together intellectual and cultural historian Harry Paul with graduate students at the University of Florida all of who had examined current literature in science studies and cultural studies and who served as critics/commentators to the entire workshop.

Although the first session generated significant agreement between members, each successive session magnified our differences. As these differences grew, communication across our respective disciplines appeared to diminish. By the end of the workshop, it was clear that not all of our goals could be met, in that forum at least. Little consensus was reached about what we meant by contextualism, and attempts to define terms such as discourse, and practice served only as an opening to further discussions. Despite these disappointments, three important things came out of that conference, especially for the historians and philosophers of science present. The first came as a result of what was *not* discussed or given only passing notice. These included some of the more conventional topics within science studies, i.e., problems with the "social construction of scientific knowledge", concomitant problems of relativism, and discussion of scientific realism. This was also one of the first meetings in science studies to problematize outright the use of "practice" and the application of practice-oriented philosophy of science; and in keeping with the absence of discussions on relativism/realism, nearly all participants within history and philosophy of science agreed to move away from the "constraint-talk" that had previously stultified science studies discussions.

The second important thing to emerge from the conference was the introduction and subsequent discussion of some key terms, including contextualism, discourse, culture and practice, which also made their way through varied audiences at the 1995 New Orleans joint meetings of History of Science Society, Philosophy of Science Association, and the Society for Social Studies of Science. Because too, the Florida workshop had included cultural historians, discussions there also took place over the knowledge/ power nexus, and power relations as a whole. Additionally, the role of the "other" within such power relations was problematized, varying forms of cultural imperialism were addressed with the aim of informing science studies, and the roles that aesthetics, emotionality, and agency play in the scientific enterprise were all discussed.

Nearly all of the participants agreed, moreover, that a return to the technical details of science was a necessary requirement for future endeavors that any of us was to make in science studies. Nearly all agreed, additionally, that existing approaches that re-

moved the scientific perspective were unsettling and unsatisfying; and nearly all urged a move away from the unquestioned belief in the unity of scientific knowledge, and toward a multicultural, contextual theory of knowledge. In keeping with this move, discussions in the workshop blurred epistemic, political and existential dimensions of knowledge. By the end of the workshop this blurring had become so evident, that Frederick Gregory, President-Elect of the History of Science Society, commented on the profoundly intricate nature of the contextualist project that we were there to discuss.

2. Contextualizing Science: From Science Studies to Cultural Studies

In the way of introducing the second part of this paper that argues that the move towards cultural studies is a historical, if not a logical progression, I wish to return to the year 1985, the year that saw the appearance of an especially influential form of contextual historiography of science. Up to that point, what had existed as contextualist historiography would have also fallen under the category of "externalist" history. Such externalist histories had become increasingly popular with historians of science, who, in professionalizing their discipline drew farther away from the more traditional internalist histories of scientists-turned historians. No longer the mere mnemonic devices, illegitimate children or historical "handmaidens" to the scientific disciplines, professionalized historians of science increasingly distanced themselves from their scientific objects of study. By 1987 historians of science had distanced themselves so much from scientists that they frequently disregarded more traditional histories: the occasion of the 300th anniversary of the publication of Newton's *Principia*, which should have been reason for pause and reflection, if not celebration, went by largely unnoticed by the leading American journal of the history of science, *Isis*. By the late 1980s the movement for independence had been so successful, that the subtitle for *Isis* reading "An international review for the history of science and its cultural influences," weighed so much in favor of the latter half of the phrase, that it began to work to the detriment of traditional histories, and to the perspectives of scientists.

3. Contextualist Historiography in the Wake of *Leviathan*

The watershed year—or rather the apostrophe mark— for contextualist historiography was 1985, when Steven Shapin's and Simon Schaffer's *Leviathan and the Air-Pump: Hobbes, Boyle, and the Experimental Life* appeared. Informed by the sociology of knowledge (in Shapin's paraphrased terms "getting on with the job of doing the sociology of knowledge rather than just debating it"), Shapin and Schaffer demonstrated in a historically convincing manner how matters of scientific facts were constructed by the complex interplay of material, literary, and social technology within local contexts of activity in Restoration England. They summarized their argument in a critical statement in their introduction: "We argue that the problem of generating and protecting knowledge is a problem in politics and, conversely, that the problem of political order always involves solutions to the problem of knowledge"(Shapin and Schaffer 1985, 21). (Readers who miss or disagree with this argument may effectively count themselves as HP's without the S.) So compelling was the historical discussion and the argument for the emergence of modern experimental science within the sociopolitical context of Restoration England, that even historians of science who had long resisted the sociological framework introduced by Thomas Kuhn in *The Structure of Scientific Revolutions* admitted—if somewhat reluctantly—to a view of science as historically rooted and culturally embedded practice. The dichotomy between "internal" and "external" determinants of scientific practice, had been demonstrably collapsed.

At the same time that it demonstrated that the distinction between internal and external could be removed, *Leviathan and the Air-Pump* also served to blur the perspective of the historian, sociologist, and philosopher. The result was what its proponents termed "science studies", a transdisciplinary configuration in which history, philosophy and sociology of science became "inextricably linked". Amplifying *Leviathan's* impact further for historians, was not only the concomitant increase in sociological

literature available, but the much wider historical movement towards social histories of the structured collective that grew out of the anti-elitist politics of the 1960s. No longer would historians contemplate the political and intellectual worlds of elite "great men", but they would instead look to the everyday life of peasants, workers, the "rank and file" to explore how their community structures functioned.

But while social historians could bask in the light of the structured collective, and while some historians of science adapted easily to the social history of science (along with weaker contextualism), others recognized the difficulties inherent with stronger contextualist historiography. Emerging, in part, from an allegiance to the sociology of knowledge (and to the sociological "Strong Programme"), the analytic framework operating in *Leviathan and the Air-Pump* supported a crude form of Marxist constructivism that effectively served to *reduce* scientific knowledge to sociology. Thus, at the same time that it admitted social and sociological components into scientific practice, it did so at the expense of the intellectual and philosophical features.

Equally problematic in its historiography, was the silencing of the voices of the historical actors. This was an especially unacceptable problem that grew out of Shapin and Schaffer's sociological approach to the writing of history that effectively approached the study of science from an outsider's or in their terms, the "stranger's" perspective. But while they could claim to use the tools of sociologists or anthropologists to give such stranger's or outsider's accounts of the science of Hobbes and Boyle, they also remained firmly *inside* the scientific and positivistic *sociology* of science. Thus, while they could argue persuasively for the social construction of scientific knowledge, they were unwilling to apply the same sociology of knowledge to their own practices as historians/analysts. Shapin and Schaffer's argument, and other such attempts to argue for the social construction of scientific knowledge, therefore bore a serious contradiction: *while they argued against simple-minded scientific empiricism, they argued for similar historical empiricism;* and at the same time that they acted to "de-privilege" the knowledge-making claims and positions of scientists, they also served to privilege their own knowledge-making claims and positions as historians/analysts of science. To summarize, *Leviathan and the Air-Pump* was effective in convincing its readers to diminish the distinction between internal and external components of science, in so doing opening the door to sociological and anthropological approaches, but ineffective as far as problematizing historiography as *the writing of history.*

4. Science as Discourse and Culture: Power/Knowledge in the Cultural History/Study of Scientific Knowledge, The Anthropology of Knowledge

Although *Leviathan and the Air-Pump* generated controversy to the point of acrimony (as did the introduction of other sociological accounts of science; see Woolgar (1988) for a synopsis of this literature), the book convinced younger historical scholars of the worthiness of sociological and anthropological approaches. As workers increasingly entertained transdisciplinary reconfigurations like "science studies," they added to the proliferation of approaches that traveled under the banner of "contextual". But rather than draw on the strictly sociological views of knowledge that had been introduced, newer contextual approaches began to lean in the direction of cultural theory and explored seriously the tools of the anthropologist. As these anthropological approaches turned to ethnographic studies, science itself, became "a culture". Because such anthropological analyses examined closely the discursive, or language-based features of cultures, as well as the rites and practices that emerged from and sustained these cultures, they also introduced discussion of discourse and practice at the same time that they discussed culture and context (Traweek 1988; Abir-Am 1992).

Possibly, the most idiosyncratic of these approaches has come from Bruno Latour. In a series of widely read and influential books, Latour has argued for an anthropological approach to understanding science as a culture (Latour 1976 and 1979; 1987). This involves "following scientists" around the laboratory and tracing out the process by

which facts are made in a laboratory setting. How the individuals arrange themselves within laboratory collectives, how they generate scientific "facts" within the social setting of the scientific community through the use of inscriptional and then persuasive devices involving the marshaling of resources, enrolling of allies, and finally through subsequent "trials of strength" between rival fact-producing cultures, has been mapped systematically by Latour. Latour's work has received much attention and criticism by a range of scholars within science studies. Among the most unpalatable features of his social framework is his casual overuse of militaristic metaphors, his profoundly ahistorical orientation, his eager dismissal of cognitive content, and his rather dim view of human motivators and activities. All these criticisms stem from his naive acceptance of the colonialist ethnography that he eagerly adopts. That the power relations of ethnographer to ethnographic object have been problematized, and that ethnographers have struggled with "empathy" issues to understand the perspective of the "native" (here scientist) within a staggering diversity of post-colonial ethnographies that permit the voice of the native to speak, seems to have completely escaped Latour (despite the fact that critics like Donna Haraway have repeatedly pointed this out).

More recently, Mario Biagioli (1993), Paula Findlen (1990), Steve Shapin (1994) and especially Jay Tribby (1991; 1994) have actively reworked contextualist historiography of science by exploring the emergence of science within court culture. Although Biagioli's theoretical grounding for his contextual theory of knowledge is the most lucid and complete application of cultural theory to the history of science to date, his emphasis on Galileo and the system of patronage within court culture still suffers from some of the same problems in *Leviathan and the Air-pump* (though for different reasons). Not so much because of its theoretical scaffolding, but because of its actual writing, Biagioli's history serves to reduce science to court culture. One reason for this is due to Biagioli's choice of synchronic historical analysis for his diachronic theory of knowledge (the latter is part of the "new historicism" of some schools of literary history). To deal with the historicity of scientific knowledge—as he so wishes— Biagioli would have to explore the narrativity of scientific knowledge to rework the grand narrative of the history of science that constructs, locates, and determines the character of Galileo. Without this consideration, Galileo becomes hardly more than a courtly parvenu: his passionate aestheticism is forgotten, and the language of his physics is silenced.

The importance of narratives, and the fundamental narrativity of all knowledge underscores the work of Donna Haraway (1989). For Haraway all of knowledge is narrative, or story-constituted. Drawing creatively on some of the most recent post-colonial ethnography, Haraway's historical practice seeks to disrupt or diffuse existing power structures inherent in all social systems through which race, class and gender become structurated. In keeping with post-colonial ethnography, "objectivity" becomes a function of the observer's critical positionality (their vantage point or point of view). Positioning herself as feminist critic, Haraway appropriates critical tools that enable the reworking of the narratives of science that serve to unmask, expose, and disrupt notions of race, class, and gender embedded within the scientific system of power relations. Critical knowledge, in her view, becomes a *tool for social action.* The problem with this approach comes from what appears an ideological epistemology: is all knowledge *merely* a tool for social action that seeks to break deterministic structures? This is clearly not *always* a tenable, nor I confess, a much desirable option.

Successful applications of contextualism have also made their way to the philosophy of science through the work of philosophers of science like David Stump, Peter Galison, and other members of this panel (Pickering 1992; Pickering forthcoming).[4] As part of the move away from theory-dominated and representational accounts of science, the focus instead is on the practice of science to understand what role instruments, models, experiments and other such interventionist procedures play in science. While the contextualism adopted holds that knowledge is localized practice, it fails to confront the textuality or discursivity of knowledge. Although their retreat from theory and representational practices permits the growing "practice industry" to avoid charges of destructive relativism in favor

of instrumentalism/realism and pragmatic theories of knowledge (and suitable variations on these themes), their philosophy of science has *only* limited validity with respect to sciences whose procedures effectively diminish theory, representational, or narrative practices—sciences, for example, like experimental physics. What sort of "contextualist" account could they then give of historical sciences like archaeology, cosmology, geology, and evolutionary biology whose textuality and narrativity is the most transparent feature of the science and which have limited use of material or observational evidence? Equally problematic with this practice-oriented philosophy is an obsession with the underlying question of "how science (actually) works"; that science has a historicity that may defy attempts to define, essentialize, or typologize "it", that it may, like a "form of life", defy attempts to freeze it (synchronically) for analysis, and that equal consideration should be given the questions like "how did science come to be?" have not received proper discussion. That science consists of more than just material practices, that it functions as a belief-system whose narratives lend coherence to the community (or any of the myriad terms that have been invented like life-world, *Weltanschauung*, forms of life, thought-collective, paradigm, and discursive *mentalité,* etc.) seem to have gained little serious discussion (despite the gallons of ink that have been spilled on such subjects); worse still, that science emerges from, and is inextricably linked to humanistic practices that serve existential and aesthetic needs and that it may be modeled after aesthetics in the way of being an expression of humanistic desires, seems to have been forgotten completely. To sum: the problem with much of the practice-oriented contextual philosophy is an over-emphasis on the material culture of the science and the materiality of knowledge to the exclusion of the narrative worlds, discursive mentalities or—in whatever word we choose here—something approximating the *perspective* of the scientists.

Here I wish to return to those forms of contextualism that draw heavily on post-colonial ethnography, the anthropology of knowledge, and the new intellectual/cultural history; it is on their theoretical scaffolding that the transition from science studies to cultural studies may be supported. This transition hinges on the form of contextualism that is adopted, for the meaning of *this* term is clearly context-bound.

5. What is contextualism? Defining contextualism

The two questions that appear to occur with the greatest frequency in circles engaging such a transition (certainly evident at the University of Florida workshop) are 1. what is contextualism? and 2. what is culture? Growing out of these two questions is also the question of what exactly one means by cultural studies of scientific knowledge. In one recent article titled "What are cultural studies of scientific knowledge?", Joseph Rouse (1993) did an excellent job of telling readers what cultural studies *are not.* While this position is consistent with the aims of contextualism (see discussion below), it does not help inform a wide audience. In this next section, I would like to attempt to transmit some of the meanings of these relevant terms in response to the questions posed. Since philosophers like terms like rigorous, robust, and strong, let us make the distinction between weaker and stronger versions of the contextualist project (weaker versions won't interest us here, but strong versions may help to transmit more recent uses of the term).

Strong contextualism is very strong: the notion that one can define "context" i.e., to limit or restrict the meaning of the term; or even the notion that one would attempt to essentialize, delimit and typologize *the* context (emphasis here on the definite article) without the concomitant consideration of the shifting critical position or standpoint of the observer, is antithetical to the strong contextualist project. Those of us still needing some kind of "fix" or some provisional working meaning of the term (despite this admonition) may note that the term "culture"—another equally nebulous term—nearly always has *something* to do with "context"[5]; but the conjunction of the two terms in the oft-used phrase "cultural context" would be redundant to adherents of strong contextualism, for there is no meaning outside of context, and culture represents the processes by which meaning is attached.

Another way to understand the stronger version of contextualist movements is to recognize their strong commitment to the view that knowledge is grounded not in foundational, first, or axiomatic principles, but is instead best seen as a cultural artifact (for some it is a product) emerging from more localized and specific contexts of cultural activity. Knowledge is thus "culturally embedded" and may be seen as an artifact of the culture—a cultural construction—though not necessarily holding any artifice or falseness in its meaning. Rather than upholding the view that knowledge is universal and transcendent, therefore, contextualism instead emphasizes the local, situated and embodied features of knowledge.

Although the turn towards contextualism has met with opposition from traditionalists within all the relevant fields of knowledge, the opposition has been the strongest in those fields whose historical development has been to make the greatest commitment to belief in universal, transcendent knowledge. Standard scientific disciplines, the so-called "hard" sciences on the bottom (or the top, depending on one's vantage point) in the disciplinary ordering of knowledge, largely compartmentalized or sheltered from currents in the humanities, have been spared these controversies. The "softer" sciences that border the humanities like anthropology, history, and philosophy have experienced the greater turbulent activity as forms of contextualism make their way into general discussions.

The introduction of contextualist theories of knowledge has possibly met with the greatest opposition in our own HPS. That this opposition has been especially severe is no surprise given the fact that it is the field, which, in occupying a disciplinary location mid-way between the sciences and humanities, serves as a conduit for intellectual exchange between the so-called "two cultures". A mixing of approaches from these areas is inevitable, and though frequently generating innovative work, the same mixing results in some of the most vituperative of exchanges. The controversies surrounding the application of contextualism to science are so great—and have become so confusing to participants—that, taken as a whole, they are possibly the most divisive of issues in the history and philosophy of science.

The intensity of the opposition to contextualism emerges not only from the fact that the interdisciplinarity of the field facilitates the mixing of approaches, but also by the extraordinarily complex range of problems introduced by the application of stronger contextualism to scientific practice that relies heavily on the use of instruments, experiments, and modeling procedures, all of which seem to resist simple discursive analysis. Given these complications, some of which have been discussed by McGuire and Melia (1989), as well as the range of choices in what can effectively count as science, and to whom, the application of the contextualist project—and what it means for the sciences— requires constant close critical examination. To sum: contextualism holds a variety of meanings, not all of which are compatible with each other (Chatman 1990 includes discussion of some of the contextualist forms in the humanities).

6. Situating Text in Context: The Return of Intellectual History

Historiographic concerns aside, how exactly does one proceed to write history within such a contextualist framework? How can contextualist approaches help to inform our historical work on a practical level? And where can one look for historiographic models or exemplars that can be adapted to the history of science? Here I rely on the well-worn work of intellectual historian Keith Michael Baker (1990) and the more recent work of cultural historians like Roger Chartier (1988) and Lynn Hunt (1989), who remind their historical audiences that context means in or within the text. Knowledge thus becomes (con)textual, in accord with forms of conversation, or in some cases in the forms of dialogues between texts. In this view what was termed the history of ideas or intellectual history now becomes the history of discourse (see discussion below for differences). Following this, contextual historical accounts seek to situate text, within text, stressing the polysemous nature of any reading. Methodologically, the stress is on the close reading and reproduction of texts, which is another way of emphasizing the interpretive

nature of historical practice and at the same time giving "voice" to the text. Combined with post-colonialist ethnographic theory, which recognizes the critical positionality of the historian and the system of power relations between historian and the historical actor, the history of discourse adapted to the history of science, can be an effective way of returning the perspective of the historical actor. Here it should be noted that such histories (like ethnographies) that silence the perspective of the historical actor are not only historiographically, but also morally, politically, and epistemically bankrupt. Where does the voice of the narrator come through in such a project? The answer: in the text proper; rather than being a stranger's account, this approach comes instead from the insider (an enculturated member, for all are "inside" some cultural framework), who adopts critical tools (mostly linguistic) that disrupt conventions, rituals and practices so that a "defamiliarization" takes place. The hopeful outcome is the narrator who can situate their own being or voice within the historical narrative, effectively *writing themselves into the story*. For this reason, some post-colonial ethnographers intentionally play with reflexive modes of inquiry (see Resaldo 1989 for an example of reflexivity in ethnographic practice, and for other sources into post-colonial ethnographies).

An additional feature highlighted in historiographic models that play on narrativity and post-colonial ethnography is an emphasis on the script-like nature of the narrative pattern. Whether the script be for the unfolding of a mega-event like the French Revolution, or for a micro-event like the unfolding of a life, the narrative serves to play itself out through this script, or "runs" itself in the historical actors and their historical account. (Baker 1990). Similarly, knowledge of science emerges from the writing of grand historical narratives that function like scripts that run themselves from scientific micro-events to mega-events and which ultimately lend coherence to the scientific project (see Smocovitis 1992 for an example of this).

Rather than continue with abstruse and arcane history and theory, I would instead like to demonstrate how such contextual historiography can inform the history of science with a concrete example from my field of the history of biology. Within the history of "evolutionary studies", one central concern has been the rise of Darwinism and Darwin's "evolutionary theory" in the nineteenth century. Historians of ideas have conventionally viewed Darwin as a revolutionary thinker who introduced a dynamic view of organic change. Questions in Darwin studies have traditionally taken the following direction: was Darwin a product of his age? was evolutionary theory in the "air"? especially given the simultaneous co-discovery by Alfred Russel Wallace? What was it in Darwin's theory that was so revolutionary or original if others were independently deriving similar theories of organic change? A contextualist here would rework narratives conventionally disengaged from the narrative of evolution like the wider narrative of the history of the "West" and the narrower personal narrative of the figure of Darwin, in a way to bring as many such narratives together. If one assumes that such engagements between narratives always exist, rather than assuming that they are disengaged (so that one then must demonstrate engagements) then it is possible to reframe the questions posed. In more familiar terms, contextualizing involves bringing narratives that had been previously disengaged, together in an overlapping mode within a rewoven grander narrative. The problem in this contextually polysemous scheme, is not to account for "connections" or "causal influences" but to account for the dislocations or breaks between the narratives. A contextualist thus begins his or her historical work assuming that such "connections" exist within a larger discursive formation, and may begin to reweave another story, possibly with a view of explaining the breaks or dislocations within such a discursive formation.

Returning to the concrete historical example from evolutionary studies, the question becomes not was Darwin a "product of his age?" but instead, "what made us think that he was disengaged from his age?"; so too in this contextualist history, Darwin is hardly an "original" thinker for there are few "originary" points in the history of discourse, but is instead himself a part of, or a "node" within a discursive network or formation that actively constructed *him*; in this view Darwin was not revolutionary but conservative, and all such scientific "revolutions" become conservative

moments of ordering the world. That species "transmute" and "transform" had been part of pre-*Origin* scientific discourse. Darwin himself introduced his own "descent with modification" that became "evolution", and which made possible a remarkably orderly view of organic change, given the alternatives. Within this history, if Darwin had not re-ordered the world, someone else would have; for the script for evolution was part of the longer and grander script of the "Enlightened West". In this sense, Darwin's historical "other" was his co-discoverer, Alfred Russel Wallace.

The success of contextualist historiography in the example noted above clearly depends on the existence of narratives that can be rethought, revised, and then rewritten. Thus, this form of contextualism accompanies a historical discipline that has reached some level of maturity (in a sense, the texts and narratives must have accumulated); but similar contextualist methods can be used for less mature disciplines or historical subjects. To sum: this version of contextualism upholds the belief that knowledge is contextual (in and within text); it emphasizes the close reading of texts with attention to precise use of language; it stresses both the polysemy in interpretive readings, and the interwoven nature of narratives. The goal of this contextualist project is to narrate an account that allows the voices and perspectives of the historical actors to speak along with other historical voices, and the narrator(s) all of whom have written themselves into the story. In the best of possible histories, the result is a polyphony of historical perspectives.

7. Closing Thoughts

The contextualist project described above assumes the discursivity and textuality of scientific knowledge. For philosophers of science, this is not an unproblematic position. How the narrative reworking takes place to accommodate material evidence in historical disciplines like archaeology, cosmology, geology and evolutionary biology should be explored through further inquiry (Wylie, forthcoming). If we view scientific narratives as being of "mythopoetic origins", rewoven by historians to accommodate material evidence within a set of unyielding Western values (the Greek word here is *axioma*) such as the value of life, then we have returned to the classic narratives of the history of science in the "west", a position not so very different from the perspective of the scientist.

Last, philosophers of science may quite rightly pick up a call to explore the relationship of discursive and non-discursive elements in the philosophy science; but the call that may not be heard is not only for a philosophy of science, but for a philosophy of *history*. Until the two are adequately addressed, historians, philosophers and sociologists may just as well view themselves as solitary H's, P's and S's, and their hope of transdisciplinarity diminished.

Notes

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[2]See Joseph Rouse, "Cultural Studies as a Model for Science Studies," paper presented to *Philosophy of Science Association*, 1994.

[3]For the volume of proceedings see Joseph Mali and Gabriel Motzkin (eds.) *Science in Context*, 7 (1994).

[4]See David Stump and Peter Galison (eds.) (forthcoming), *Disunity and Contextualism: Philosophy of Science Studies,* Stanford: Stanford University Press. The literature on practice oriented philosophy is vast and includes the work of Ian Hacking, Nancy Cartwright, and Peter Galison. For a survey of these approaches see Timothy Lenoir and Yehuda Elkana, (1988), "Practice, Context and the Dialogue Between Theory and Experiment," *Science in Context* 2 (1). For another survey of practice philosophy to history of science see Jan Golinski, (1990), "The Theory of Practice and the Practice of Theory: Sociological Approaches in the History of Science," *Isis*, 81: 492-505.

[5]In attempting to articulate the meaning of "culture" Stephen Greenblatt states that culture is "a term that is repeatedly used without meaning much of anything at all, a vague gesture toward a dimly perceived ethos." In "Culture" Frank Lentricchia and Thomas McLaughlin (eds.), *Critical Terms for Literary Study*,(Chicago: University of Chicago Press, 1990), p. 225.

References

Abir-Am. (1992), "A Historical Ethnography of A Scientific Anniversary," *Social Epistemology* 6:323-354.

Baker, K.M. (1990), *Inventing the French Revolution*. Cambridge: Cambridge University Press.

Biagioli, M. (1993), *Galileo, Courtier. The Practice of Science in the Culture of Absolutism*. Chicago: University of Chicago Press.

Chatman, S. (1990), "What Can We Learn from Contextualist Narratology?", *Poetics Today* 11: 309-328

Chartier, R. (1988). *Cultural History. Between Practices and Representations*. Ithaca: Cornell University Press.

Findlen, P. (1990), "Jokes of Nature and Jokes of Knowledge: The Playfulness of Scientific Discourse in Early Modern Europe," *Renaissance Quarterly* 43:292-331.

Haraway, D. (1989), *Primate Visions*. New York: Routledge.

Hunt, L. (ed.). (1989), *The New Cultural History*, Berkeley: University of California Press.

Latour, . and Woolgar, S. [1979] (1986), *Laboratory Life: The [Social] Construction of Scientific Facts*. 2d (revised) ed. Princeton, N.J.: Princeton University Press.

Latour, B. (1987), *Science in Action: How to Follow Scientists And Engineers through Society*. Cambridge: Harvard University Press.

McGuire,J.E. and Melia, T. (1989), "Some cautionary strictures on the writing of rhetoric of science," *Rhetorica* 7:87-99.

Pickering, A. (ed.) (1992), *Science as Practice and Culture*. Chicago: University of Chicago Press.

_ _ _ _ _ _ _. (forthcoming), *The Mangle of Practice*.

Rouse, J. (1993), "What Are Cultural Studies of Scientific Knowledge?", *Configurations* 1:1-22.

Rosaldo, R. (1989), *Culture and Truth*. Boston: Beacon Press.

Shapin, S. and Schaffer, S. (1985), *Leviathan and the Air-Pump. Hobbes, Boyle, and the Experimental Life*. Princeton: Princeton University Press,

Shapin, S. (1994), *A Social History of Truth. Civility and Science in Seveteenth-Century England*. Chicago: University of Chicago Press.

Smocovitis, V.B. (1992) "Unifying Biology: The Evolutionary Synthesis and Evolutionary Biology," *Journal of the History of Biology* 25: 1-65.

Traweek, S. (1988), *Beamtimes and Lifetimes. The World of High-Energy Physics*. Cambridge, Mass.: Harvard University Press.

Tribby, J. (1991), "Cooking with Clio and Cleo: Eloquence and Experiment in Seventeenth Century Florence," *Journal of the History of Ideas* 52:417-429.

_____. (1994), "Club Medici: Natural Experiment and the Imagineering of 'Tuscany'", *Configurations* 2:215-235.

Woolgar, S. (1982), *Science: The Very Idea*. Chichester, Sussex: Ellis Horwood Ltd.

Wylie, A. (forthcoming). *No Return to Innocence: Philosophical Writings in American Archaeology*. Princeton: Princeton University Press.