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Social theory and the de/reconstruction of agricultural science

Local knowledge for an alternative agriculture[1]

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> [We] have a dual task before—a deconstructive project and reconstructive project that are intimately linked. Our deconstructive task requires close attention to, and the dismantling of, technostrategic discourse. The dominant voice of militarized masculinity and decontextualized rationality speaks so loudly in our culture, it will remain difficult for any other voices to be heard until that voice loses some of its power to define what we hear and how we name the world—until that voice is delegitimated.
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> Our reconstructive task is a task of creating compelling alternative visions of possible futures, a task of recognizing and developing alternative conceptions of reality, a task of creating rich and imaginative alternative voices—diverse voices whose conversations with each other will invent those futures.
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> (Carol Cohn, "Sex and Death in the Rational World of Defense Intellectuals," 1987: 717–18)

INTRODUCTION

No less than those who would challenge the way in which the defense intellectuals have defined our world, we who believe that contemporary agricultural production is neither socially just nor ecologically benign also face dual tasks. In part, the deconstructive task entails the demonstration that agricultural science as currently constituted provides neither a complete, nor an adequate, nor even a best possible account of the sphere of agricultural production.[2] Indeed, it is in large measure an historical overreliance on this partial knowledge—and a failure to recognize how specifically situated that knowledge is—that has brought our agriculture into its present straits.

The reconstructive task will be the more difficult, for it will entail the identification and legitimation of alternative sources of knowledge production for agriculture—sources which now have no voice, or speak without authority, or simply are not heard in contemporary agroscientific discourse. It is out of conversations among this fuller range of knowledge sources—conversations that should include, but must not be limited to, what is now known as agricultural science—that an alternative and a truly sustainable agriculture may emerge.

The deconstructive project has enjoyed considerable success since what the Agricultural Research Institute dubbed "Hurricane Rachel" Carson appeared on the horizon in 1962. In no other sector of science has as much space been opened for reconstructive possibility as in agriculture. And this space has been created at a time when new resources are available both for extending the deconstructive project and for initiating the reconstructive task. In contemporary sociological interpretations of science and in feminist analyses we have new theoretical resources for challenging that voice of decontextualized rationality which agricultural science has used to such dominating effect. And in the diverse literatures on what I will provisionally call "local knowledge" and in the knowledge contained in the heads of farmers and

agricultural workers, we have the material resources for a plausible reconstruction of what Sandra Harding (1986) has termed "successor science."

THE GREENING OF THE NATIONAL RESEARCH COUNCIL?

The deconstructive project—actually, it is more a diffuse historical tendency than a coherent project—has been gathering momentum for nearly three decades now. Rachel Carson (1962) was midwife to the birthing of a wide variety of initiatives directed to forestalling the kind of ecological apocalypse described in *Silent Spring*. Subsequent critiques have focused not only on the social and environmental externalities associated with modern agricultural technologies (Berry 1977; Commoner 1972; Strange 1988), but also on the manner in which particular social interests gain differential influence over the institutional structure of knowledge production. There is concern that corporations and agribusinesses have managed to shape to their own advantage the choice of the problems that public agricultural science has undertaken and the way solutions to those problems are expressed in technologies (Busch and Lacy 1983; Buttel 1986; Friedland 1978; Hightower 1973; Kenney 1986; Kloppenburg 1988).

More recently, criticism has been directed not simply at the priorities to which agricultural science has been directed, but at the validity and utility of the methodologies employed in research and the epistemic constitution of knowledge production itself. Suppe (1988) argues that agricultural research of the sort performed by experiment stations can have only limited applicability to actual farming operations because of limitations intrinsic to the probabilistic extrapolation of experimental results to highly variable biological and social systems. A growing number of biological scientists are concerned that the reductionistic and positivistic approaches characteristic of modern science constrain pursuit of unorthodox but potentially productive research initiatives, obscure important connections between organisms and phenomena, and actively inhibit achievement of holistic understanding of ecological systems (Allen and Starr 1982; Levins and Lewontin 1985; MacRae *et al.* 1989; Odum 1989; Prigogine and Stengers 1984).

This discursive opposition by academics has helped to inform and complement activists who seek to transform the scientific and technical bases of agricultural production and who have found a great deal of support in growing popular disaffection with the continuing deterioration of the environment. This activist movement has been given institutional expression not only in national level environmental groups (e.g. Greenpeace, Natural Resources Defense Council), but also in organizations with a specifically agricultural focus (e.g. Pesticide Action Network, Rodale Institute, the Land Institute) and, most importantly, in countless local groups organized around a wide variety of issues of local concern. In whatever terms these organizations may frame their particular vision of a transformed agriculture—reduced-input, biological, sustainable, organic, permanent, ecological, regenerative—it is clear that they are seeking an *alternative* to conventional agricultural practice.

To the list of organizations calling for an alternative to conventional agricultural practice we may now append the National Academy of Sciences. In a book length analysis titled *Alternative Agriculture*, the National Research Council's (NRG) Board on Agriculture has affirmed the benefits of "alternative systems." The chair of the committee that conducted the study goes so far as to say in the preface that "the committee believes that farmers, researchers, and policymakers will perceive the benefits of the alternative systems described in this report and will work to make them *tomorrow's conventions*" (NRC 1989: vi, emphasis added). The nation's premier scientific body has placed itself in support of an approach to agricultural production the designation of which is "alternative." Such a designation reflects a long sojourn in the wilderness of scientific marginality.

The NRC's report is a clear indicator of just how successful the deconstructive project has been. Activists and academic critics have struggled long and hard to illuminate and focus attention on the link between contemporary modes of agricultural production and contaminated waters, eroded lands, human cancers, pesticide residues, foreclosed farms, and declining rural communities. The NRC has been led to see the connection between the hard tomatoes and the hard times. And with this partial delegitimation of conventional production practices has come a concomitant questioning of the scientific and technical bases of those practices. The NRC's recognition of the need for an alternative agriculture is evidence that the deconstructive project has succeeded in opening up a space in which the hegemonic forms of science, though

still powerful, are no longer completely secure. Given the existence of this space, it may be possible to initiate the reconstructive task of building an alternative science as part of the process of building an alternative agriculture.

But agroindustry and elements of the public agricultural science community have already begun their counterattack. A central theme of this counterattack is that if an alternative agriculture is necessary, it will be up to *scientists* to determine what that alternative will be. The objective is to control the shape that alternative agriculture will take by insisting upon the hegemony of existing science and thereby limiting the type and range of knowledges that can be brought to bear upon the construction of an alternative agriculture. For example, in its critique of the NRC's report, the agribusiness-oriented Council for Agricultural Science and Technology (CAST) asserts that "The extensive coverage and dependence on [farm] case studies reflects the paucity of solid factual information . . . this renders certain findings and related recommendations more philosophic than scientific" (CAST 1990: 2). CAST will allow knowledge produced by farmers no credibility independent of validation by scientists. The deconstructive project may have opened space, but there is no question that the use that is made of that space will be contested. And the object of that contest is not simply what should constitute alternative agriculture but—even more fundamentally—who is even to have the power to speak authoritatively in that debate, who is to have a voice at all.

The NRC's report itself reflects the same class of sin, though it is less one of commission than of omission; the NRC does not take farmers' knowledge seriously enough. Fully half of *Alternative Agriculture* is indeed given over to eleven farm case studies. When the NRC study committee wanted to see how alternative farming worked, it had little choice but to seek out farmers who had themselves developed alternative practices since the agricultural science establishment had virtually nothing to offer. Far from disparaging this farmer-generated knowledge as CAST does, the NRC staff praises its richness and creativity. But the conclusions reached in the report relate almost entirely to the need for the application of more *scientific* effort to the development of alternative agriculture, and the report's recommendations focus on how this scientistic strategy might best be accomplished. Farmers are regarded as recipients of technology, advice, and information. The authors of the NRC report simply do not conceive of any potential for farmer-generated knowledge except in connection with or translation through agricultural "science."

There is broad agreement that American agriculture should move toward some "alternative" form. But the extent to which this alternative future will be a change in kind rather than degree—that is, the extent to which it approaches reconstruction rather than reproduction—will depend in significant measure upon whether agricultural science itself is reconstructed or simply reproduced. Despite their differences, both CAST and the NRC propose to achieve an alternative, sustainable, regenerative, low-input, diversified agriculture through the application of the same methods and institutions of knowledge production that have given us a conventional, non-sustainable, non-regenerative, high-input, homogeneous agriculture in the first place. If we are to achieve a truly alternative agriculture, might we not also require an alternative science? And should not that alternative science encompass—at a minimum—the knowledge production capabilities of farmers who by their very survival outside conventional agriculture have already demonstrated their capacity for the generation of useful and workable alternatives?

Now is the time for bold hypotheses and innovative research. Rural sociologists can and should play a central role in the struggle to create a truly alternative science, as well as an alternative agriculture. What follows is an effort to survey the resources available for both the deconstructive and the reconstructive projects, to suggest ways in which these resources might be used, and to outline productive areas for research.

FROM DECONSTRUCTION TO RECONSTRUCTION

In contemporary society, what we call science enjoys a privileged status among the possible ways of establishing knowledge about the world (Aronowitz 1988; Marcuse 1964; Mulkay 1979). And for a long time, social theorists joined the public, the business community, and policymakers in treating scientists as virtual truthsayers. By virtue of its methodological foundation and normative characteristics, the community of scientists was held to be capable of generating knowledge that—unlike the products of any

other way of knowing—bears no traces of its birthing in a particular social context (Merton 1973; Polanyi 1962). But, just as the assertion that Adam had no navel because he was created and not born was challenged, so now has the absence of the scars of social contingency on the bodies of scientific "facts" been brought into question.

Over the past fifteen years there has emerged a wide variety of provocative new sociological interpretations of science which constitutes a rich and diverse body of theoretical and empirical resources to draw upon in challenging positivist and realist epistemologies of scientific knowledge (see, e.g., Barnes and Bloor 1982; Callon and Law 1989; Cozzens and Gieryn 1990; Knorr-Cetina 1981; Latour 1987; Latour and Woolgar 1979; Longino 1990). The analytic frameworks associated with the new sociology of science are theoretically and methodologically diverse. Still, all of these programs share a distinctive point of departure: the central insight that the mental productions we call scientific knowledge are no less subject to social influences than are the products of any other way of knowing and are, therefore, the fruits of a scientific enquiry that must be envisioned as, in Knorr-Cetina's (1981: 3) succinct phrasing, "constructive rather than descriptive." A number of important points follow from this characterization.

First, the recognition that the "facticity" of science is not comprised of objective descriptions of a determinate natural world but of socially contingent constructions provides a foundation for a powerful new critique of science. Socially contingent objectives can be recognized not just in the uses to which science is put, but in scientific facts themselves. Second, the inadequacy of criteria for the epistemic demarcation of science as a uniquely legitimate way of knowing means that what we call modern science is itself an historical product of continuous social struggle not only to define science in a particular way, but also to exclude other ways of producing knowledge from that definition (Gieryn 1983). Third, if scientists do not have a uniquely privileged capacity to speak authoritatively on nature's behalf, then knowledge claims arising outside the institutions of science can no longer be summarily dismissed because they are "nonscientific." And fourth, if science is socially constructed and is therefore subject to social deconstruction, then certainly it must also be amenable to social *reconstruction*. The boundaries of what we might call "actually existing" science are in fact negotiable and might be redrawn to include other ways of producing knowledge, to effect new articulations and combinations between modes of knowledge production whose essential complementarity is now obscured, or even to produce a radically transformed science whose contours we can now only dimly foresee.[3]

But, for the most part, the new sociologists of science have not applied themselves to the transformative task to which their work appears to logically lend itself (Amsterdamska 1990; Kleinman 1991; Restivo 1988). In contrast, feminist analysts bring a commitment to activist social change to their own distinctive yet constructivist approach to the critique of actually existing science. Some feminists came quickly to the realization that the source of the evident hostility of science to women and the earth is located in the very fabric of scientific rationality itself, in the language and the concepts and the methods deployed in the construction of scientific meanings and scientific "facts." As Keller (1987: 37–38) points out, this conclusion follows logically from a central premise of feminist studies generally: just as gender is a socially constructed representation (rather than a precise reflection) of sex, so is science a socially constructed representation (rather than a precise reflection) of nature.

This essentially constructivist premise guides feminists to the sociopolitical implications that practitioners of the new sociology of science have only partially acknowledged and have generally failed to pursue. Feminists regard constructivist insights not simply as a foundation for the interpretation of actually existing science, but as tools for social as well as epistemological criticism. They recognize the role that the exclusion of other knowledges has played in the historical demarcation of science and understand that this creates space for legitimation of other ways of knowing (especially women's knowledges). And, most importantly, motivated by a desire to transform what they perceive as an intrinsically androcentric mode of knowledge production, many feminists are actively engaged in the search for alternatives to the way in which hegemonic science is presently constituted.

This is not to say that there is complete agreement among feminists regarding the shape that a transformed science might best assume. Harding (1986) identifies three tendencies in feminist thinking about the production of scientific knowledge: "feminist empiricism," "feminist postmodernism," and "feminist standpoint" theory. Feminist empiricism recognizes the

distortion introduced into the construction of scientific facts by the historical legacy and contemporary reality of sexism, but it holds that this androcentric bias can be, in large measure, mitigated through more rigorous adherence to the existing scientific method. What is sought is "a larger canon rather than a different one; a richer, perhaps even multi-faceted, representation of reality, but not a separate reality" (Keller 1987: 46). If feminist empiricism maintains a faith in the possibility of scientific consensus enriched by multiple voices, feminist postmodernism recognizes the diversity of voices but denies their commensurability. A maximally objective view of the world is to be sought not in essentializing universals, but in alliances between partial knowledges which are capable of generating richer understandings when "federated in solidarity" (Harding 1986: 55; see also Haraway 1988 and Smith 1987). Finally, feminist standpoint theory proposes a transition to a "successor science" which would be superior to actually existing science because it would be founded on a feminist epistemology that is itself intrinsically superior (Bleier 1986; Merchant 1980). This epistemic superiority is derived not from biological differences between men and women, but from the distinctive *experience* associated with women's lives in gendered society. Hartsock (1983) and Rose (1983, 1986) suggest that it is specifically the character of women's *labor*—especially caring labor and manual labor—that structures and shapes a feminist as opposed to a masculinist understanding of the world.

The differences among these approaches to the feminist deconstruction and reconstruction of science are perhaps less important than the characteristics they share. Women's distinctive historical experiences—of their bodies, of oppression, of caring (about and for)—make possible alternative ways of thinking about nature and knowing the natural world. A feminist science would be one in which

> no rigid boundary separates the subject of knowledge (the knower) and the natural object of that knowledge; where the subject/object split is not used to legitimize the domination of nature; where nature itself is conceptualized as active rather than passive, a dynamic and complex totality requiring human cooperation and understanding rather than a dead mechanism, requiring only manipulation and control.
>
> (Fee 1986: 47)

Knowledge production founded on a feminist epistemology would draw—as contemporary patriarchal science allegedly does not—on the "integration of hand, brain, *and* heart" (Rose 1983: 90).

But to what extent is this emphasis on *experience* uniquely feminist? Sandra Harding (1986: 165) has noted the "curious coincidence of African and feminine 'world views'" regarding the relationship between the self and the phenomenal world. Elizabeth Fee (1986) extends this insight and identifies parallels between feminist epistemology and Native American, Chinese, and even working-class perspectives on nature and knowledge. What feminists criticize as masculinist science is also criticized from other standpoints—differently situated knowledges, one might say—as European science, or imperialist science, or bourgeois science. Elizabeth Fee (1986: 53) concludes that "Clearly these different critiques need to be brought together . . . It seems to me that any one of these critiques provides a partial, but incomplete, perspective—and each adds important elements otherwise missing in the analysis."

I agree with Fee that this spirit of eclecticism—of "shared conversations in epistemology" (Haraway 1988: 584)—is the most fruitful analytical approach in a world of multiple identities and hyphenated commitments. One of the central themes in the feminist analysis of science is the importance of legitimating and reaffirming the value of producing knowledge through "sensuous activity" (Rose 1986: 72) and "personal experience" (Harding 1986: 240) that is necessarily and specifically "local" (and therefore neither universalizing nor essentializing) in character (Smith 1987). I suggest that what I will call "local knowledge" is an expression of such production and that it is the global ubiquity of this form of knowledge production that accounts at least in part for the curious coincidences noted by Harding and Fee.

Feminists are neither the only nor the first analysts to mark reliance on sensuous activity and personal experience as a fundamentally different kind of knowledge production than that commonly called scientific. True, in one sense all knowledge is both personal and sensuous inasmuch as it must be obtained by individuals who have no access to the natural world except through their senses. But while the Ojibwa herbalist and the NIH biochemist both rely on sensuous observation to obtain knowledge, they do so from quite different epistemological stances (as well as within quite different social contexts, with quite different

objectives, and with quite different tools). A wide variety of analysts from the phenomenologist philosophers to contemporary anthropologists have tried to illuminate this epistemic distinction through elaboration of a range of paired concepts: "tacit knowledge/scientific knowledge" (Polanyi 1966), "science of the concrete/science" (Levi-Strauss 1962), "life-world knowledge/scientific knowledge" (Bohme 1984; Husserl 1970), "craft knowledge/scientific knowledge" (Braverman 1974), "practical labor/science" (Bittner 1983), "folk wisdom/processed knowledge" (Krimsky 1984), "indigenous knowledge/scientific knowledge" (Richards 1985), "working knowledge/scientific knowledge" (Harper 1987).

In providing the foregoing list I do not mean to imply that these analysts are saying precisely the same thing. They are not. However, their thoughts are clustered in such a way as to constitute an identifiable constellation of analysis that provides a rich set of resources for exploring the production of knowledge by obstetrical nurses in Chicago, blacksmiths in Nairobi, Jivaro shamans in the Peruvian Amazon, and hog farmers in Iowa. The practical, sensuous, personal labor of such people "is always controlled by full regard for the timely and local features of the environment within which it takes place" (Bittner 1983: 253). It is the *locality* of such knowledge production which most completely intimates the many dimensions of its character. Such knowledge is *local* in the sense that it is derived from the direct experience of a labor process which is itself shaped and delimited by the distinctive characteristics of a particular place with a unique social and physical environment.[4]

One dimension of locality is an intimacy between the worker and the materials and objects of labor. The "many-sided gestalt of theoretical, tactile, and auditory input" which constitutes the craft skill of Harper's mechanic/bricoleur, Willie, enables him to "reduce the gap between the subject—the worker—and the object—the work" (Harper 1987: 133). Thus, like the tribal bricoleurs of Levi-Strauss (1962), Willie "speaks not only *with* things . . . but through the medium of things." This (feminist) elimination of the boundary between subject and object and the intimacy of the conversation between the knower and the known permits the craftsperson to "see beyond the elements of a technique to its overall purpose and coherence" (Harper 1987: 21). This holistic sense of the substance and context of the labor process produces a unified field of knowledge that is finely tuned to the concrete exigencies, needs, and requirements of local conditions.

It is the central importance of *local* knowledge to women and to African peoples that accounts for the curious coincidence of African and feminist world views. And it is *local* knowledge that informs the birthing skills of the *sages-femmes* studied by Bohme (1984). It is *local* knowledge that enables the competent farmer to master the "intricate formal patterns in ordering his work within the overlapping cycles—human and natural, controllable and uncontrollable—of the life of a farm" (Berry 1977: 44). It is *local* knowledge that allows Robert Pirsig to keep his bike running through *Zen and the Art of Motorcycle Maintenance* (Pirsig 1974). It is *local* knowledge that enables machinists to "make out" on the shop floor (Burawoy 1979). And it is the *local* knowledge produced by workers that is the object of appropriation and control in both Taylorist and "postindustrial" strategies of industrial management (Braverman 1974; Hirschhorn 1984).[5]

But today it is not the herbalist but the biochemist, not the midwife but the obstetrician, not the craftsperson but the engineer, not the campesino but the agronomist who dominates knowledge production and deployment. What we all know as scientific knowledge has attained virtually undisputed intellectual hegemony, while local knowledge has been pushed to the epistemic peripheries, its utility so poorly recognized that we have difficulty even labeling it. Until recently, the scientific method was held to be not just a different, and not just a better, but the best and the only consistent way of producing reliable knowledge of the world. It is precisely this epistemic uniqueness that has now been so powerfully challenged. But to say that scientific knowledge is not epistemically unique is *not* to say it is not *different* from that produced by other ways of knowing and, in particular, different from local knowledge.

That such a difference between local and scientific knowledge should now exist is rather ironic since science, in fact, grew out of local ways of knowing (Braverman 1974; Gieryn 1983). Indeed, prominent progenitors of the scientific method such as Bacon, Descartes, and Boyle explicitly saw their task as explaining why craftworkers could do what they could (Merchant 1980: 179–89). But the emergent scientists sought their explanations not in order to understand a particular phenomenon or labor process in all its idiosyncratic complexity, but in order to understand

singular identities recognizable across phenomena and labor processes. They were interested not in locality but in translocality. They were interested not in complete understanding of a specifically situated phenomenon, but in partial understandings of widely dispersed but similar phenomena. They were interested in the production not of local knowledge, but of what Latour (1986: 7–14) calls "immutable mobiles," information which is invariant through any change in spatial or social location.

The methodological approach which has historically characterized the production of immutable mobiles—or scientific facts—is Cartesian reductionism. This is the practice of breaking a problem down into discrete components, analyzing these separate parts in isolation from each other, and then reconstructing the system from the interpretations of the parts (Levins and Lewontin 1985: 2; Merchant 1980: 182). There can be no doubt that this approach has been exceedingly powerful, but it also appears to be flawed in a number of ways. For many feminists, the detachment from nature and the objectification of the natural world that are characteristic of the Cartesian method fit all too well with the premium placed on power and control by authoritarian and patriarchal society and have served to reinforce the domination of women and nature (Bleier 1986; Longino 1990; Merchant 1980).

The reductionistic dissection of problems is also seen to involve a loss of context (social and political as well as physical and biological) which encourages a hierarchical and linear rather than an interactive and ecological view of nature (Aronowitz 1988; Odum 1989: 177; Prigogine and Stengers 1984). Inasmuch as it relies on models which are necessarily partial and selective, Cartesian reductionism is biased toward those elements of nature which yield to its method and toward the selection of problems most tractable to solutions with the knowledge thereby produced (Krimsky 1984; Levins and Lewontin 1985). In pursuing the paths along which it realizes successes, Cartesianism neglects those areas where other approaches might prove fruitful. And as its successes and achievements have mounted, Cartesianism has come to appear as the *uniquely* effective mode of knowledge production and is increasingly regarded not just as a methodological tactic, but as "an ontological stance . . . more than simply a method of investigation; it is a commitment to how things really are" (Levins and Lewontin 1985: 2–3).

Corollary to the commitment to Cartesianism is the neglect and delegitimation of local knowledge production and, as Husserl (1970) expressed it, the progressive separation of science from the "life-world." As Cartesian science is elaborated and institutionalized in laboratories, it loses touch with the local knowledge and everyday experience of concrete labor processes which might have informed and shaped its development and application. Science is "no longer guided by a live intelligence, fallibly tuned to actual circumstances; instead it is determined by a detached and externalized intelligence embodied in a formula" (Harper 1987:20). That is, the application of immutable mobiles to particular geographic or social places may fail to respect the exigencies and needs of a specific locality. Because it is reductive, abstracting, and interested in the immutable components of a phenomenon, science loses connection with the variability of local systems. On the other hand, the contextual detail that local knowledge brings to the understanding of a particular place or event has little utility outside that place or event. And because it must be intimately tuned to the totality of continually changing circumstances that define a particular locality, the content of local knowledge is relatively plastic. Indeed, if Cartesian science produces immutable mobiles, local knowledge produces "mutable immobiles."

What we are confronted with, then, is distinctive ways of knowing the world, each with particular strengths and weaknesses. Yet one of these has achieved a hegemonic position from which it dominates epistemic discourse and enjoys a virtual monopoly on the resources that society allocates to the production of knowledge. The new sociology of science has provided a foundation for the deconstruction of actually existing science by demonstrating that Cartesian reductionism has no unique claims to truth. Feminist analysis has produced a similar insight, but uses the deconstructive opening to work toward reconstructive possibility. The recovery of local knowledge should be an important component of such reconstruction.

And what better place to explore the theoretical and practical opportunities for using local knowledge to reconstruct science than the agricultural sector? The deconstructive challenge has brought even the National Research Council to the recognition that the re-doctrinism of actually existing science is not adequate to the task of achieving a sustainable agriculture:

> Most of the new knowledge has been generated through an intradisciplinary approach to research. Scientists in individual disciplines have focused their expertise on one aspect of a particular disease, pest, or other agronomic facet of a particular crop. Solving on-farm problems, however, requires more than an intradisciplinary approach. Broadly trained individuals or interdisciplinary teams must implement the knowledge gained from those individual disciplines with the objective of providing *solutions to problems at the whole-farm level.*
>
> (NRC 1989: 137, emphasis added)

The route to solutions to problems at the whole-farm level—at the local system level—runs not through agricultural scientists, but through those who think in terms of whole farms, those whose experiences are of whole farms, and whose knowledge has been developed by the integration of hand, brain, and heart in caring labor on whole farms—that is, through farmers. We should be exploring how to bring farmers and their local knowledge back into formal knowledge production for agriculture.

RECONSTRUCTION: BRINGING THE FARMER BACK IN

It is profoundly ironic to be suggesting now that farmers be brought *back* into rather than simply into knowledge production for agriculture. For until 1862 farmers not only were in, they were just about the *only* ones who were in since prior to that year neither the USDA nor the land grant universities had been established and only the most embryonic forms of what would come to be known as agribusiness had yet emerged. At least through the turn of the century it was farmers, agricultural laborers, and associated craftspeople who were the chief developers of new practices and technologies for U.S. agricultural production.

While a good deal is known of the tactics deployed by agricultural scientists in their efforts to establish the superiority of their way of knowing (Marcus 1985; Rosenberg 1976), much less is known of the process by which farmer-generated knowledge was simultaneously delegitimated and subsequently hidden from history. The principal contours of a relationship in which "scientists preached and farmers applied what they preached" had been established by the last two decades of the nineteenth century (Marcus 1985: 31). Thereafter, the accelerating "academicization of agriculture . . . led to the feeling that the expert knew more than the farmer, and that therefore the communication flow was from the expert to the practitioner" (Bennett 1986: 367). Natural scientists came early to the treatment of farmers as recipients rather than generators of knowledge and, given the evidence of the proliferation of adoption/diffusion studies, it is apparent that by 1950 social scientists had embraced this viewpoint as well.

Neither the existence nor the persistence of this social scientific myopia in regard to agricultural producers as producers of knowledge as well as commodities should be surprising. Rural sociologists—like other social scientists—do, after all, regard themselves as *scientists* and are no less captive of the epistemological assumptions of hegemonic science than are biochemists. Prodded now and then to treat people as subjects rather than objects, to engage in a pedagogy "*with*, not *for* the oppressed" (Freire 1970: 33), or to try "reverse learning . . . to learn from farmers" (Chambers 1983), even the best and the brightest of us cling instead to our own form of scientific hubris. Painfully cognizant of the problems generated by modern agricultural science, even the most progressive rural social scientists have tended to see solutions to these problems in the leavening effect of social science on the natural sciences rather than in challenging the nature of the scientific enterprise itself.

Thus, Buttel and Gertler (1982: 117) conclude "that cooperation between social and biological scientists" is the key to developing "solutions to pressing problems of agricultural resource management, and for the long-term security of the farm population of North America." Busch and Lacy (1983: 237–38) opt for interdisciplinary enlightenment as well. The logical extension of this confidence in the palliative effect of social scientific expertise is its application in forms of social impact assessment (SIA). Friedland (1978: 11) proposes the prevention of "social sleepwalking" in regard to new technology development through the use of predictive assessment of impacts by a "university public" acting on behalf of the wider public. Hightower (1973: 64) expresses a populist faith in the ability of the wider public to act on its *own* behalf, but he anticipates this action as indirect pressure on the land grant complex through direct pressure on politicians. Only Busch (1984: 310) goes beyond populist initiative and the technocratic review to call for "democratization of the problem formation process."

Now there is no question that the social sciences have interesting things to say, that interdisciplinary cooperation is desirable, that social impact assessment can be useful, that political pressure is an important tool, and that actually existing science needs democratization. These are worthy objectives, worth working for. But while all these proposals imply a critique of what established science has accomplished and how it is organized, they take not just the political and intellectual hegemony but also the epistemological hegemony of that type of knowledge production as a given. They do not see beyond the "democratization" of science at the most radical.

But what ought now to be apparent is that if what we now call science is one socially constructed interpretation among many possible interpretations, then we can ask for more. We can ask not simply for the democratization of actually existing science, but for its transformation. It is perhaps appropriate that among all contemporary critics of agricultural science, it is left to a farmer (an uncommon farmer, it is true, but insight is not evenly distributed among scientists either) to grasp what social scientists have missed. Wendell Berry asserts that since

> what we have now in agriculture . . . is a modern scientific orthodoxy as purblind, self-righteous, cocksure, and ill-humored as Cotton Mather's, our history also forbids us to expect it to change from within itself. Like many another orthodoxy, it would rather die than change, and may change only by dying. . . . If change is to come, then it will have to come from the outside. It will have to come from the margins.
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> (Berry 1977: 173–74)

For Berry, those margins are the largely unexplored landscapes of knowledge and skill shaped and maintained by the intelligence and labor of farmers themselves.

What appears as radical revisionism to the U.S. agroscientific orthodoxy—recognizing farmers as sophisticated knowledge producers and bringing them back into the process of technology generation—has already achieved a measure of legitimacy in the field of international agricultural development. While using linguistic analysis to study systems of classification and cognition among indigenous peoples, ethnoscientists found that traditional farmers the world over are, in fact, exceedingly keen observers of the natural environment (Brokensha *et al.* 1980). Not only has it become clear that traditional farmers have accumulated large bodies of empirical knowledge which they apply with great skill and imagination in their agricultural operations, it has also been found that they are frequently engaged in trying out changes in their technologies or practices of production, changes that are informed by simple experimental methodologies and which merit being described as forms of research (Richards 1989). The development and deployment of this stock of "local technical knowledge"[6] is thus a dynamic, living tradition which John Hatch (1976: 17) argues constitutes "the single largest resource not yet mobilized in the development enterprise."[7]

The last decade has seen an outpouring of work from researchers sensitive to the need for more effective and equitable international agricultural development policies and committed to exploring the potential of local technical knowledge for achieving an "indigenous agricultural revolution" that is both ecologically sustainable and socially just (Altieri *et al.* 1987; Chambers 1983; McCorkle 1989; Richards 1985; Thrupp 1989). Though intellectually and disciplinarily heterogeneous, this set of work has enough in common that at least some of its practitioners have proclaimed the emergence of a "new paradigm" of knowledge production for agriculture which replaces "transfer of technology" with "farmer first" (Chambers *et al.* 1989: xiii–xiv). This point of view has by no means been universally embraced, but sufficient progress has been made for Chambers *et al.* (1989: xiii) to suggest that taking local technical knowledge seriously might not only provide a new way forward for resource poor farming in the Third World, but might have "lessons also for all agriculture."

Now "lessons also for all agriculture" surely must be read as a tentative suggestion that a "farmer first" approach to knowledge production for agriculture might be as appropriate for the industrialized nations as it is for the Third World. The U.S. agricultural science community will certainly find this difficult to accept. It is one thing to argue that the technical knowledge of resource poor farmers should be taken seriously precisely because they are resource poor and therefore not in a position to take advantage of the technologies that science has to offer. It is quite another thing to argue that farmers who do have the material and intellectual resources to make use of science-based technologies possess—in addition—knowledge that

should be used to alter the way science develops and deploys those very technologies.

Still, even among farmers whose operations are most isomorphic to the "best management practices" promulgated by extension cadres, there must exist a substantial reservoir of local knowledge. And at the margins and in the interstices between technological convention and scientific orthodoxy there are all manner of traditionalists and visionaries—Amish, Mennonites, Native Americans, new alchemists, organic farmers, perennial polyculturists, low input producers, seed savers, biodynamicists, horse farmers—who continuously produce and reproduce a landscape of alternative agricultural possibilities. This landscape comprises institutional as well as technical alternatives, for unconventional producers have been supported in their efforts by a set of unconventional institutions, some of which are of their own making (e.g. Practical Farmers of Iowa, Southwest Wisconsin Farmers Research Network) and others of which have been established by apostates who have defected from conventional science to pursue alternative paths of knowledge production (e.g. the Rodale Institute, the Land Institute, the Seed Savers' Exchange).

Through all the lean decades of official neglect and an agricultural policy environment actively hostile to their interests, many alternative farmers and alternative institutions managed not only to survive but even to thrive. Their persistence, coupled with the increasingly conspicuous failings of conventional industrial agriculture and the pressures applied by agro-environmental public interest groups, have created an intellectual and political space in which the potentials of an improved goodness of fit, or substantive interaction, between scientists and farmers appears even to the NRC and the USDA as a means of developing kinder and gentler agricultural technologies and production practices.

An emerging interest in the potentials of on-farm research is clearly apparent in the agroscientific community (Lightfoot 1987; NRC 1989; Francis *et al.* 1990; Lockeretz and Anderson 1990). As part of the 1985 Farm Bill, Congress passed the Agricultural Productivity Research Act (Public Law 99–198), which actually required the systematic initiation of cooperative research with agricultural producers. In fulfilling its congressional mandate, the USDA has established a Low Input/Sustainable Agriculture (LISA) program intended to fund research projects that take "a whole farm or SYSTEMS approach" and involve "FARMER PARTICIPATION" (USDA 1988). Many state level institutions—land grant universities, state departments of agriculture—have also identified farmer participatory research as an important component of their initiatives in the area of sustainable agriculture. Prominent among these are the Center for Integrated Agricultural Systems at the University of Wisconsin and the Aldo Leopold Center at Iowa State University. The efforts taken by such organizations to enhance articulation and cooperation between scientists and producers are encouraging. But they also reveal just how little expertise and experience there is with such ventures and how difficult it is to counter the powerful forces and incentives that hold scientists to established patterns (Thornley 1990; Stevenson *et al.* 1991).

There is indeed growing interest in bringing the farmer back in. But we need to be clear about what it is that we are bringing together before we can decide how that is to be accomplished. There now exists a window of opportunity in which to reverse the historical marginalization of local knowledge and to move the development of agricultural science out of its established trajectory and onto a reconstructive path. But the existence of this window may only be transitory, and its transparency is already contested as agribusiness mobilizes its resources in an attempt to dominate discourse and to make its meaning of "alternative agriculture" the universal meaning (Kleinman and Kloppenburg 1991; Kloppenburg 1991). How can we foster support for and understanding of local knowledge production in agriculture? What kinds of articulations might it be desirable to establish between local knowledge and scientific knowledge? How might such articulations be achieved? Are these the right questions? Where can we look for guidance?

ACCEPTING PARTIALITY: ARTICULATING SITUATED KNOWLEDGES

The purpose here has been to suggest that there are a variety of places in which to find the guidance required. No one of these intellectual locales by itself offers sufficient resources, and all may be necessary in varying degrees. Haraway's (1988: 583) central precept seems appropriate here: "The moral is simple: only partial perspective promises objective vision." What we need to do is to establish conversations among these partial perspectives and ground them in the

specific and material context of the agricultural sector. Some of the principal topics that might be the subject of such conversations are now outlined.

Defining "local knowledge"

The central question, of course, is "What is 'local knowledge'; is it different from scientific knowledge and, if so, how?" Much of the preceding discussion has been devoted to establishing that such knowledge exists and that investigating its character and content would reward both theory and practice and possibly even provide a basis for the transformation of actually existing science. So far, the concern has been with presenting the various resources available for an exploration of this sort rather than with the exploration itself. Nevertheless, by expressing a preference for the term "local knowledge," the implication is that "locality"—in the sense of inseparability from a particular *place* in the sense of embeddedness in a particular *labor process*—is the key distinguishing feature of this type of knowledge.[8]

Surely, given the theoretical resources available, many other productive interpretations are possible. Anthropologists have begun to examine American agriculture (Chibnik 1987) and have even begun to touch upon the contours of local knowledge (Bennett 1982; Wells 1991). Wells' conclusion that California farmers' knowledge systems are constructed through the operation of specifiable social networks and her description of the ways in which the character of these knowledge systems varies among ethnic groups seem particularly promising. A diverse set of nonacademic analysts—principally farmers or activists working with farmers—also provides a rich fund of information on local technical knowledge in American agriculture (Berry 1984; Irwin 1990; Logsdon 1984; Strange 1988). The focus of this body of work is on understanding the production and reproduction of local knowledge as a "live tradition" (Berry 1984: 25). What should characterize sociological efforts to explore and define the parameters of local knowledge is careful attention to both theorization and the observed evidence of local knowledge production gained through direct contact with farmers and agricultural workers. Harper's (1987) superb study of a rural mechanic is a model of the sort of work that could be accomplished on the farm in order to define local knowledge and to understand the social context in which it is generated, transmitted, and used.

Recovering the historical farmer as a knowledge producer

The understanding gained through direct analysis of contemporary local knowledge production on the farm should guide the recovery and reintegration of the historical farmer as a knowledge producer. Privileging the written records left by the evangelists of an emergent agricultural science, historians have too often accepted and promulgated the image of the "reluctant farmer" and celebrated the rise of cooperative extension as a "victory of change and progress over traditionalism and apathy" (Scott 1970: 3). Like women's knowledge, the skills, practices, and wisdom developed by farmers have been, in Rowbotham's (1973) words, "hidden from history." But if they are hidden, perhaps they are not completely lost. What sorts of information might we be able to recover from primary and secondary historical materials simply by altering our perspective and purposefully searching out what we have so far neglected? Such investigations could result in the recovery of practices and technologies that might constitute "a resource, a fund of experience, a lexicon of proven possibilities and understood mistakes" (Berry 1977: 180) on which an alternative science can draw in developing an alternative agriculture. The connection between the achievement of agricultural scientific legitimacy and the delegitimation and marginalization of local knowledge should prove to be a rich field for socio-historical research.

The curious coincidence of agroecology and feminism

Of the conversations that it may be possible to foster, perhaps the most intimate will be that between feminism and the emergent field of agroecology. In looking to the agricultural sector, feminists will uncover a variety of standpoints with considerable affinity to their own. Wendell Berry (1977: 123) observes that "no matter how much one may love the world as a whole, one can live fully in it only by living responsibly in some small part of it . . . We thus come again to the paradox that one can become whole only by the responsible acceptance of one's partiality." This seems very close to the point Haraway (1988: 583) makes when she argues for "partial perspective . . . limited location and situated knowledge." Now knowledge produced from

a "limited location"—what I have been calling "local knowledge"—provides an alternative to the immutable mobiles of Cartesian science: "All these pictures of the world should not be allegories of infinite mobility and interchangeability but of elaborate specificity and difference and the loving care people might take to learn how to see faithfully from another's point of view" (Haraway 1988: 583). And that is pretty much what Robert Chambers (1983: 201) asks us to do when he argues that "putting the last first" in agricultural development requires epistemic "reversals in learning." Such affinities between feminism and agroecology are multiple and articulable. Feminist concerns for context dependence, diversity, affection, responsibility, accountability, and dialogue in knowledge production find counterparts in the thoughts of agroecologists such as Berry (1977, 1984), Jackson (1980), Altieri *et al.* (1987), Norgaard (1987), and Odum (1989). I would not argue that these multiple points of view are homogeneous. I do believe that they are the kinds of related stances which could be "federated in solidarity" (Harding 1986), and it is precisely solidarity that we need if we are to actually achieve a sustainable agriculture.

Reformed science, successor science, or decentered science?

Feminist theory should prove extremely useful in framing conversations regarding the possible ways in which local knowledge might be involved in transforming actually existing science. In particular, the various feminist interpretations of science can be seen to imply distinct sets of hypotheses about the relationship between local and scientific knowledge, and between farmers and scientists. Feminist empiricism suggests that while the existing canon needs to be enlarged and enriched, modern science is not irremediably flawed. From this perspective, local knowledge and scientific knowledge are fundamentally complementary. The implication is that agricultural scientists need to take what farmers know seriously, but that such knowledge is more or less translatable into existing scientific frameworks (though those frameworks themselves may be partially restructured by such translation). Feminist standpoint theory eschews reform in favor of fundamental epistemological reconstruction. Women's experience does constitute a separate reality and, by extension, local knowledge also constitutes a separate reality. The point is not to establish complementarities or translations (which simply reinforce the hegemony of Cartesianism), but to foster so complete a deconstruction of existing science that the emergence of a successor science on a new epistemological base becomes not only possible but necessary. The practical means for achieving such an epistemic birthing are difficult to imagine, but would surely involve the dissolution of the institutional and intellectual boundaries now separating farmers and agricultural scientists.

Feminist postmodernism suggests that the transition to a successor science is a mistaken project. Multiple and separate realities do exist and to suggest that a universal epistemological stance is possible and desirable—however feminist, holist, or organicist it might be—is simply to replace one hegemony with another (Haraway 1986). While difference must be recognized and valued, productive interactions between ways of knowing can be established through partial connection and "decentered knowledge seeking" (Harding 1986: 55). Farmers know something that agricultural scientists do not know and cannot completely know; and vice versa. Articulations between these different ways of knowing need to be established not in order to combine the knowledges, and not to translate the knowledges, but to permit mutually beneficial dialogue. The problem is not one of choosing between scientific knowledge or local knowledge, but of creating conditions in which these separate realities can inform each other.

Alternative methods for an alternative science

One product of such a struggle has already been criticism of existing methodologies of Cartesian science and the slow emergence of alternative techniques for learning about the world and of articulating differently situated knowledges. Haraway (1988: 584) suggests that "there is a premium on establishing the capacity to see from the peripheries and the depths." In his book *Rural Development: Putting the Last First* Robert Chambers (1983) details a wide range of practical steps—learning reversals—that can be taken by scientists to learn how to "think from below." How might his work be applied to the rural sectors of the advanced industrial nations? How can we foster the engagement of rural peoples' own

knowledge in self-development and self-empowerment? The more we learn about local knowledge and the social integument of its generation and transmission, the better we will be able to respond to those questions.

Further, agroecologists have begun to explore the possibilities of research methods that respect the integrity of farming systems as ecological and social unities (MacRae *et al.* 1989). Proponents of "hierarchical theory" have begun to generate methods which "combine holism and reductionism" to address the structure, function, and interrelation of the different levels of organization which they believe characterize complex systems (Allen and Starr 1982: Odum 1989). Sociological attention to the social constitution of research methodology should provide some interesting insights into this process. Just as technology is a product of social choice, the techniques used to produce knowledge are also selections from among a range of possibilities (Knorr-Cetina 1981; Latour 1987). If as social analysts we must be alert to the lost possibilities and foregone alternatives to the technologies that ultimately emerge from the laboratory, we must also recognize that research methods are also being lost or foregone. And if we now know little about how and why scientists select or construct their methods, we know even less about farmers' methods of experimentation and trial.

Women and the transformation of agricultural science

Finally, the role of women scientists as vectors bearing social codes of epistemological transformation should be an interesting topic for conversation. Keller (1983) has described the distinctive vision and practice—a feeling for the organism—that Barbara McClintock brought to genetics. The degree to which enlarging the participation of women in science can itself be a potent catalyst for epistemic transformation is an important strategic issue for the feminist reconstructive project (Harding 1986: 247; Keller 1988: 241). Hrdy (1986) and Haraway (1989) argue that the accumulation of feminist consciousness that accompanied the increasing number of women in the field of primatology resulted in the toppling of long held disciplinary paradigms and traditions of narrative. There is now occurring a rapid growth in the number of women in the agricultural sciences. And, while there is not yet any substantial population of internal critics within the agricultural sciences, what ferment does exist inside the disciplines appears to be substantially female, and the most expansive and creative thinking is, in fact, explicitly feminist (Crouch 1990a, 1990b; Handelsman 1991; Handelsman and Goodman 1991). Could what happened in primatology be recapitulated in, say, plant pathology?

CONCLUSION

The agricultural sciences and the agricultural sector as a whole stand now at a pivotal conjuncture. More space is available now for moving agricultural technoscience onto new trajectories than at any time in American history. A critical rural sociology has played a key role in pushing forward the deconstructive project that has been instrumental in creating this space. Many rural sociologists are interested in participating in the reconstructive project as well. But in this effort we need to enlarge not only the canon of our colleagues in the natural sciences, but our own canon as well. In this article I have suggested what the theoretical resources for such an enlargement might be.

Sociological constructivism provides a set of tools for the deconstruction of actually existing science, but has not developed the political or social conscience that would direct the reconstructive use of those tools. Feminist analysis brings such a conscience to bear and actively imagines alternative régimes of knowledge production, but has so far not addressed the agricultural sector as a concrete terrain for the working out and testing of theory and practice. The literature on what I have labeled "local" knowledge constitutes a rich conceptual and empirical resource, but analyses are widely dispersed across time and discipline and lack explicit points of contact and comparison. Studies of indigenous technical knowledge provide a wealth of information on the actual activities of local knowledge production in agriculture, but the field lacks the theoretical base that would give it a self-conscious epistemic stance and a developed awareness that local knowledge might be more than just a complement to Cartesian science in the Third World.

Articulated as partial realities, these perspectives may accomplish in conversation what none of them can alone. The new sociology of science has opened for us a crucial deconstructive door. It is feminist theory that speaks most clearly as to how to proceed through

that door. In turn, the agricultural sector provides a uniquely appropriate concrete terrain for the testing of a whole range of theoretical propositions drawn from both the sociology of science and feminism, and for the necessary work of developing and elaborating the "here-and-now prefigurative forms" (Rose 1986: 73) of what might one day be a transformed science. And that transformed science will need to encompass the distinctive contributions to understanding the world that can be provided by "local knowledge."

Wendell Berry (1977: 160) has written that Cartesian science "accumulates information at a rate that is literally inconceivable, yet its structure and its self-esteem institutionalize the likelihood that not much of this information will ever be taken *home*." That is, it is not sufficiently relevant *locally*; it fails to take home —the distinctiveness of particular cultural, social, and ecological spaces—sufficiently into account. A truly alternative agriculture must be based on a truly alternative science that articulates multiple ways of knowing. Rural sociologists can and should participate in this articulation. We can go home again.

NOTES

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2 The analysis contained in this article is "deconstructive" in the sense that, as Jane Flax (1986: 195) put it, it seeks "to distance us from and make us skeptical about ideas concerning truth, knowledge, power, the self, and language that are often taken for granted within and serve as legitimation for contemporary Western culture."

3 Several of the referees for this article gained the impression that I do not believe that what I call actually existing science is capable of producing valid knowledge. This is a serious misreading of my position and that of the constructivists as well. Let me be as clear as I can. Scientific facts are socially contingent, just as are the conclusions of all other ways of knowing. But "socially contingent" does not mean "false." As Busch (1984: 309) correctly emphasizes, "the problem is not that scientific and technical truths are relative, but that they are partial." Loss of its unique epistemological status does not imply a wholesale invalidation of science. It does imply the creation of space for the consideration of competing modes of knowledge production, which themselves represent partial understandings.

4 In affirming the importance of such locally based experiential knowledge, I do not mean to imply that it is free of social contingency. The scientific method does not produce a reading off of nature unmediated by social relations, and neither does direct experience. The "facticity" of experience is every bit as socially constructed as is the "facticity" of science (see Scott 1991).

5 As one referee correctly noted, scientists produce local knowledge too. It is the existence of such local knowledge—"the largely inaccessible idiosyncrasies of the individual or the laboratory"—that explains, for example, the inability to easily replicate hybridomas across molecular biology labs (Cambrosio and Keating 1988). But, as I hope will shortly become clear, I believe that the knowledge that enables the technician to synthesize the hybridoma is quite different from what I am going to call scientific knowledge.

6 In the anthropological and international development literature "indigenous knowledge" has been the most common term used to refer to what I call "local knowledge." My own analysis of locality and choice of terminology has been influenced by McCorkle's (1989: 4–5) and Thrupp's (1989: 14) assertions that "local knowledge" most fully captures the sense in which this type of knowledge is distinctive. See Chambers (1983: 82–83) for discussions of the relative utility of other terms.

7 Because local technical knowledge is "pre-adapted to its physical and human ecology" (McCorkle 1989: 8), its elaboration and improvement are more likely than exogenous innovation to be environmentally and socially appropriate and therefore more likely to be sustainable in the long term. Moreover, intimate and sustained engagement with their means and conditions of production endow farmers not only with deep knowledge of local particularities, but also with a holistic and systemic understanding of local agriculture that reductionistic science cannot easily approximate. While cautioning that local technical knowledge is not free from error (of course, neither is science), Chambers (1983: 75) concludes that "Rural people's knowledge and scientific knowledge are complementary in their strengths and weaknesses. Combined they may achieve what neither would alone."

8 I will briefly outline the thrust of my own current thinking. A dairy farmer produces new knowledge about milk production in the process of producing milk. But the physical and temporal space available to a farmer for knowledge-producing activity is defined by the nature of commodity production on the farm. For dairy farmers, generally, a necessary condition of new knowledge production is success in milk production. The knowledge production activity of farmers is thus a secondary process which is necessarily simultaneous with, embedded in, inextricable from, and constrained by the primary process of commodity production. The resources available for use by the farmer in knowledge production are limited to those which are also *locally* available for use in commodity production. Such local knowledge production depends on the unaided senses, accumulates in time-bound fashion through aggregative experience, and is holistic.

The scientific labor process is quite different and, in fact, is unique inasmuch as it makes the generation of new knowledge its primary objective rather than a secondary epiphenomenon (Whitley 1977: 25). Scientists are not more rational than farmers, they have no capacity to think more abstractly, they are not necessarily even better experimenters. What dairy scientists do enjoy is release from the constraints of milk production. No longer completely bound by the locality of their labor process, they develop tools which uncouple knowledge production from "situatedness" and "personal" perception (i.e. the microscope which permits access to sub-perceptual entities or the survey form that permits collection of data at a supra-perceptual dimension). The scientific laboratory can (within certain persistent limits of locality) be everywhere and nowhere and the knowledge generated therein is relatively immutable and mobile, whereas local knowledge, bound to the locality of a particular labor process, is relatively mutable and immobile.

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