Values in Doing and Writing Science:  
The Case of Barbara McClintock

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The attention currently directed to science as “manufactured knowledge” challenges a long-standing belief in its “objectivity.” Instead of finding science to represent “truth,” this perspective argues science is a discipline that, by the kinds of questions asked, methods of investigation used, and means of interpretation applied, constructs different models of nature, that these models reflect their makers, and that these models change. In other words, science is “culturally and historically specific, modified, involved” (Haraway 12). This paper, which adopts this perspective, identifies some of the values in doing science, argues that these values are reflected in writing science, and suggests that some of the communication difficulties suffered by at least one scientist, Barbara McClintock, may be traced to the poor match between these values and the values of her unconventional scientific practice. In this case, it may be that, like Cinderella’s sisters, if she wanted the Prince (an audience), she had to deform herself for the shoe to fit.

Doing Science

Consider then the white European standing before an object, before the exterior world, before nature, before the Other. A man of will, a warrior, a bird of prey, a pure act of watching, the white European distinguishes himself from the object. He holds the object at a distance, he immobilizes it, he fixes it. Equipped with his instruments of precision, he dissects it in cold analysis. Moved by the will to power, he kills the Other and, in a centripetal movement, he makes it a means to use for his own practical ends. He assimilates it.

Doris Lessing

Highly conventionalized, science has long been characterized by “impersonal, procedural routines which are generally applicable and universally effective” (Gilbert and Mulkay 56). These days, however, it is common to acknowledge that science is neither impersonal nor universal; it is simply one version of a story that masquerades as “truth,” a version that often describes a world in terms of hierarchy and competition. It is another commonplace to claim that the values associated with this story are the values that Western
culture has long associated with males: “our very definition of a scientist as someone who is rational and analytic, objective and dispassionate, able to put emotion aside and to maintain at all times a distance between self and object matches our cultural construct of masculinity” (Martin 135-36). Scientists solve puzzles; they reveal secrets; they describe, dominate, control, and penetrate a passive/powerless/distanced and female Other. Power, mastery, aggression—these have been the goals of many scientists at least from the Renaissance when they aimed, as Bacon explains, not only to be “the servant and interpreter of nature” but also to study her “when by art and the hand of man she is forced out of her natural state, and squeezed and moulded” (323, 320).1 These goals are still valued today when scientists search for those laws of nature that “are not apparent . . . waiting to be plucked like fruit from a tree” but “hidden and unyielding” and difficult to grasp (National Academy 6). Some imagine their roles in less bucolic terms: this contemporary scientist explains that he “liked to follow the workings of another mind through . . . minute, teasing investigations to see a relentless observer get hold of nature and squeeze her until the sweat broke out all over her and her sphincters loosened” (Keller, “Dynamic Autonomy” 123).2 [McClintock: “I feel that much of the work is done because one wants to impose an answer on it . . . “ (179; emphasis added).]3

Certainly, this definition of science has been productive. But in a context where the role assigned to the scientist often reflects the cultural image of male as powerful investigator and the role assigned to nature frequently epitomizes “her” as powerless object of investigation, women (and men who reject these stereotypes) are clearly at a disadvantage:

In a science constructed around the naming of . . . (nature) as female and the parallel naming of . . . (mind) as male, any scientist who happens to be a woman is confronted with an a priori contradiction in terms. . . . Only if she undergoes a radical disidentification from self can she share masculine pleasure in mastering a nature cast in the image of woman as passive, inert, and blind. (Keller, “World” 174-75)

Women can take on the role of “honorary men” (Rose 86), conform to the existing code of the scientific community they wish to enter, and uncritically accept the values of that community, with the “male norm as the measure of excellence” (Schiebinger 6). At best, they then become “aliens, immigrants, whose presence is tolerated not on their own terms, but on the natives’ terms” (Code 120). They can withdraw from the community, and sacrifice the shared values and interests that give significance to their work. And/or they can practice a different kind of science, one that does not assume “a heterosexual fantasy of control and submission” (Longino 205). Whatever choices they make in the doing of science, in the writing of formal science reports there seems to be no choice.
Writing Science

Scientists have different repertoires available to them in different contexts, as Gilbert and Mulkay have pointed out:

Formal accounts are couched in terms of an empiricist representation of scientific action; . . . this empiricist repertoire exists alongside an alternative interpretative resource . . . [that] tends to be excluded from the realm of formal discourse. (40)

It is these formal accounts that concern me here since McClintock's informal accounts are almost entirely unavailable. Formal scientific accounts follow a "not-so-hidden agenda": "the mastery and possession of what is deemed to be 'nature'—where nature is seen as whatever is an 'otherness,' as something mysterious, wild, full of unforeseen possibilities of exploitation" (Shotter and Logan 74). Like the positivist model which for so long has defined a conventional relationship between scientist and nature, formal accounts exemplify a positivist model of discourse where facts speak for themselves and speak with authority, the authority that comes from "simply and unproblematically report[ing] on nature as revealed through [simple and unproblematic] empirical investigation" (Bazerman, "Writing" 39). Mirroring a "male" cast of mind, these documents are "intolerant of 'messy' arguments—i.e., ones that lack formal structure and are, variously, intuitive, empathetic, indeterminate; . . . [instead, they] emphasise the virtues of dispassion and objectivity" (Hudson and Jacot 57). [McClintock: "I actually felt as if I was right down there [with the chromosomes] and these were my friends. . . . As you look at these things, they become part of you" (Keller, "World" 165).]

Like much scientific practice, much written science follows accepted patterns: quantitative data, symbolic language, generic forms, and passive voice create conventionalized descriptions of the scientist's activities in regard to a conventionalized nature. These texts define impartial and distanced scientists whose conclusions are inevitable:

Neither the author's own involvement with or commitment to a particular analytical position nor his social ties with those whose work he favours are mentioned.... Scientists' actions and beliefs . . . [follow] unproblematically and inescapably from the empirical characteristics of an impersonal natural world. (Gilbert and Mulkay 56)

In these texts, scientists project "an image of impersonal authority and absolute confidence in the accuracy, objectivity, and importance of [scientific] observations" (Namenwirth 23), and they maintain this authority "largely through exclusion and intimidation" (Bazerman, Shaping 294). [McClintock: "It is becoming increasingly apparent that we know little... " ("Significance" 798).] Behind facades of politeness and ritual, scientists argue for the superiority of their research, their model, their interpretation—often by denying the roles that intuition and emotion and error play in their work, and by relying on tidy and elegant and simple models of nature. [McClintock: "It
was so striking that I dropped everything, without knowing—but I felt sure that I would be able to find out what it was that one cell gained and the other cell lost, because that was what it looked like. And I couldn’t get it out of my head that one cell gained what the other cell lost, and that I would be able to find it out... I don’t know why, but I knew I would find the answer" (124).

Therefore, to publish successfully, scientists “use the language of [their] community, and all must write in ways deemed appropriate to and by [their] community” (Brodkey 3). Producing an appropriate scientific text requires the adoption of this mask of a rational, distanced, confident and victorious investigator who reports the secrets of a defeated nature—a mask that privileges competition, objectivity, and simplicity. Certainly, failing to wear this mask is to risk having no audience. Yet donning it can be just as alienating as the “fantasy of control and submission” Longino mentions, limiting the scientist—since “to adhere to the conventions [of Western rationalism] is to uphold a male standard of rationality” (Tompkins 170)—and perhaps the science he or she reports as well.

McClintock’s Science

Genes are neither “beads on a string” nor functionally disjoint pieces of DNA. They are organized functional units, whose very function is defined by their position in the organization as a whole.

Evelyn Fox Keller

Barbara McClintock, a classical geneticist who works with maize, has had a long and rewarding career. Born in 1902, she received her Ph.D. from Cornell at a time when women in science were rarer than they are today. [McClintock: “At this stage, in the mid-thirties, a career for women did not receive very much approbation. You were stigmatizing yourself by being a spinster and a career woman, especially in science. And I suddenly realized that I had gotten myself into this position without recognizing that that was where I was going” (72).] While she never held a regular academic position, the value of her work was recognized by her being named Vice President of the Genetics Society of America (1939), Member of the National Academy of Sciences (1944), and President of the Genetics Society of America (1945). In 1980, she was saluted by the Genetics Society of America for her “brilliance, originality, ingenuity, and complete dedication to research” (Keller, Feeling 13); in 1981, she received a number of honors: the First MacArthur Laureate Award with a lifetime fellowship, the Lasker Award for Basic Medical Research, and the Wolf Foundation Prize; in 1982, she shared Columbia University’s Horwitz Prize (Keller, Feeling 13); and in 1983, she was awarded the Nobel Prize for her discovery of transposition—work she began almost forty years earlier. [McClintock: The “prize is such an extraordinary honor. It might seem unfair, however, to reward a person for having so much pleasure, over
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the years, asking the maize plant to solve specific problems and then watching its responses" ("Barbara McClintock" 131).

McClintock represents a science that is different from the distanced, combative relationship with nature described earlier in these pages. Borrowing from Piaget, Keller (whose relationship with McClintock seems in some ways analogous to Huxley's relationship with Darwin) describes this kind of science as "dynamic objectivity":

The scientist employs a form of attention to the natural world that is like one's ideal attention to the human world: it is a form of love. The capacity for such attention, like the capacity for love and empathy, requires a sense of self secure enough to tolerate both difference and continuity. ("Dynamic Objectivity" 117-18)

Instead of trying to control the natural world, to force it into conformity with previously-held conceptions, to impose answers on it, McClintock first attempts to understand it: "Precisely because the complexity of nature exceeds our own imaginative possibilities, it becomes essential to 'let the experiment tell you what to do" (Keller, "World" 162). As a result, her science is based on an intimacy between herself and her subject, which allows her to recognize and respect its complexities. [McClintock: "No two plants are exactly alike. They're all different, and as a consequence, you have to know that difference. . . . I start with the seedling, and I don't want to leave it. I don't feel I really know the story if I don't watch the plant all the way along. So I know every plant in the field. I know them intimately, and I find it a great pleasure to know them" (198).]

Because McClintock's work seems to be based on understanding, not domination, on empathy, not distance, on the addition of the mystical, she has been called "a philosophical and methodological deviant" (Keller, "A World" 159). In a field that does not encourage self-reflexiveness, she is a scientist who not only has experienced the limits of rationality but who has publicly acknowledged those limits. For instance, listen to her explanation of what happened to her when she found herself unable to identify particular chromosomes: she sat under some eucalyptus trees and "brought about a change in herself that enabled her to see more clearly, 'reorienting' herself in such a way that she could immediately 'integrate' what she saw" (Keller, Feeling 117). Almost magically, she transformed herself. [McClintock: "The point is that when these things happen—when you get desperate about something and you have to solve it—you do solve it and you know you've solved it. You do something with yourself!" (117).] She is also a scientist who values the whole complex and connected picture, not the isolated problem. [McClintock: "The problem is not something that's ordinary, but it fits into the whole picture, and you begin to look at it as a whole. . . . It isn't just a stage or this, or that. It's what goes on in the whole cycle" (67).] She is a scientist who sees nature as resourceful and complex, not as "a passive, mechanical object ruled by externally imposed law" (Fee 48). As a result, McClintock
thinks of herself as a collaborator, one who “listens to the organism” to learn what she should do.

Watson and Crick’s Theory
Valuing connectedness and complexity led her to a series of experiments which were to raise questions about the master molecule theory put forward by Watson and Crick and rewarded by the Nobel Prize—a theory regarded for a time as “the greatest modern triumph of the standard methodology that has ruled orthodox science ever since Descartes” (Gould 157). It was simple and elegant and seemed to fit the available data; and it defined, for a while, a central dogma of biology. [McClintock: “Trying to make everything fit into set dogma won’t work…. There’s no such thing as a central dogma into which everything will fit” (179).] As you recall, Watson and Crick were interested in identifying the structure of DNA. (Because I am—to paraphrase Dr. McCoy of “Star Trek”—only a rhetorician, not a scientist, I can only hope that my explanation of these theories is just.) They found its structure to consist of “two helical chains each coiled round the same axis” (“Structure” 239). These chains are held together by purine bases bonding with pyrimidine bases. Each chain is a template that, once unravelled, enables the DNA to replicate itself. The template is fixed; mutations rarely occur (“spontaneous mutation may be due to a base occasionally occurring in one of its less likely tautomeric forms” [Watson and Crick, “Genetical Implications” 246]). Through the structure they identify, Watson and Crick argue that genetic transfer of information is a simple mechanical activity in which genetic information in a cell flows in one direction under the orders of the DNA: “Many lines of evidence indicate that [DNA] is the carrier of a part of (if not all) the genetic specificity of the chromosomes and thus of the gene itself” (Watson and Crick, “Genetical Implications” 241). No outside forces can penetrate and no inside forces can change the blueprint the DNA creates:

If the actual order of the bases on one of the pair of chains were given, one could write down the exact order of the bases on the other one, because of the specific pairing. . . . Each chain acts as a template for the formation on to itself of a new companion chain. . . . The sequence of the pairs of bases will have been duplicated exactly. (Watson and Crick, “Genetical Implications” 245)

The metaphor underlying this theory implies a nature that the scientist can describe with certainty, a nature whose behavior the scientist can predict and control. (It is no wonder that Watson was asked to head the Human Genome Project, the largest blueprint even imagined.)

McClintock’s Theory
In the 1930s, McClintock began work with Indian (variegated) corn, which led to her theory about how some mutations can occur. [McClintock: “As I was going through the field, I saw plants that were variegated--part dominant
and part recessive. I didn’t look at the variegated plants, but somehow or other they stuck in my mind” (65).] McClintock’s theory, “transposition,” accounts for unexpected genetic behavior and provides a different (and more flexible) picture of nature than Watson and Crick’s static model. Instead of defining mutation as a fault in the stable sequence of the bases in the DNA chains as Watson and Crick suggest, McClintock explained that the agent directing some of these mutations consisted of “extragenic units, carried in the chromosomes . . . . [C]hanges in genic expression result from chromosome alterations at the locus of a gene and these are initiated by units other than those composing the gene itself” (“Induction” 598, 579).

Thus, McClintock’s model presented an alternative to the structural explanation of mutations provided by Watson and Crick. It also argued for “extragenic units” (emphasis added): her earliest work with particular extragenic material discovered that it functioned in pairs, with changes occurring only when both partners were present. According to McClintock, half of this pair, the “Dissociator” (Ds), disrupts the normal function of the gene: Ds could move from its original position to a new position in the chromosome; and as it changed position, it could change the function of genes it was located near. She also discovered that the Dissociator only acts under orders from its partner, “the Activator” (Ac). The Ds sits “next to the structural gene it controlled,” with the Ac “located almost anywhere, far away on the same chromosome, or even on a different chromosome entirely” (Gribben 301). Furthermore, the action of this pair could be unpredictable, making some mutations unstable:

She analyzed a case in which Ds had transposed from its original site to a locus, designated C . . . . The C locus makes a factor required for the synthesis of a purple aleurone pigment, and mutations at C were known to interfere with pigment production. McClintock meticulously collected evidence that the new mutation at C was caused by the insertion there of Ds . . . . In the absence of Ac the mutation was a stable one: kernels were colorless and the entire plant was green. With Ac in the genome, on the other hand, the mutation reverted in some cells [emphasis added], so that both the kernels and the plant showed sectors of purple pigment. (Federoff 87)

The relationship of Ds-Ac, and the language that describes it, demonstrates McClintock’s interest in both change and interdependence: “Ds initiates changes in genic expression and Ac controls when they will occur” (“Induction” 580):

The numerous different phenotypic expressions attributable to changes in the genic components at the locus need not be related, in each case, to changes in the genic components at the locus, but rather to changes in the mechanism of association and interaction of a number of individual chromosome components with which the factor or factors at the locus are associated. According to this view, it is organized nuclear systems that function as units at any one time in development. (“Chromosome Organization” 34; emphasis added)
McClintock further suggests that not only changes in the location of certain genetic material (these came to be known as “jumping genes”) but also changes in the environment might be responsible for changes in genetic traits: “It is tempting to consider that changed environmental conditions may well alter otherwise-established rates of reaction, and thus initiate alterations in the nuclear components at predictable times, leading to strikingly modified phenotypic expression” (“Chromosome Organization” 43). [McClintock: “A genome may reorganize itself when faced with a difficulty for which it is unprepared. . . . [R]apid reorganizations of genomes may underlie some species formations. Our present knowledge would suggest that these reorganizations originated from some ‘shock’ that forced the genome to restructure itself in order to overcome a threat to its survival” (McClintock, “Significance” 793).] In contrast to Watson and Crick’s theory, transposition depends on a nature that is more complex and more flexible than had been previously thought—a nature that is able to adapt to its environment:

In maize and in other organisms it is known that transposable elements are activated when the genome is stressed and chromosome breakage is taking place. Just how this happens remains a mystery, but once the elements are activated they can promote many kinds of mutations and chromosomal rearrangements. It is as if transposable elements can amplify a small disturbance, turning it into a genetic earthquake. Perhaps such genetic turbulence is an important source of genetic variability, the raw material from which natural selection can sift what is useful for the species. . . . There is reason to suspect [transposable elements] can reprogram genes in more subtle ways as well, changing when and where in the organism a gene is active. (Federoff 98)

When she presented her theory in the 1950s, most of McClintock’s colleagues were unable to share this image of a flexible nature that can adapt to its environment. Those who believe still that the natural world should be defined, described, anticipated and controlled—and that its stability is essential to these operations—remain suspicious of McClintock’s vision of an adjustable and adjusting nature.

Problems of Sex, Fashion, Writing Style, and Audience

People must be able to work, to think, to be part of a past tradition and a continuing community, and to have their works received by others so that the inventive act is completed. Too often an ‘invention’ . . . dies or disappears because clear social links are not provided. Karen Burke LeFevre

When McClintock tried to explain transposition at research symposia in the 1950s, she was unsuccessful: “there was mumbling—even some snickering—and outright complaints.” Some thought she was crazy (Feeling 139-40). [McClintock: “It was just a surprise that I couldn’t communicate; it was a surprise that I was being ridiculed, or being told that I was really mad” (140).] In print she seemed to be equally ineffective; a 1953 article published
in *Genetics* received only two requests for reprints. She stopped talking about her work and, "except for the annual reports in the CIW Yearbook, stopped publishing" (Keller, *Feeling* 142). Why was she unable to explain her theory?

Several explanations have been suggested. McClintock may have been the victim of prejudice. [McClintock: Gender is "always there, always intruding" (76).] Because she was a woman, many scientists may not have listened to her carefully or taken her unorthodox work seriously, especially when it challenged accepted dogma, like Watson and Crick's view of nature: "on many occasions her achievements have been overlooked or downgraded simply because she is a woman" (Shapiro 74); "McClintock surely suffered from all the prejudices, subtle and overt, directed against women in science . . ." (Gould 163). But this explanation isn't entirely satisfactory because McClintock's earlier work had been taken seriously; she was a respected scientist.

A second explanation suggests that because McClintock was "wedded to old-fashioned concepts," her work was largely ignored (Keller, *Feeling* 180). It is true that McClintock was a classical geneticist interested in studying patterns of inheritance in multicellular organisms: "It is because of the distinctive advantages that the maize plant offers . . . that it has been possible to obtain precise evidence concerning some of the events associated with the origin and behavior of mutable loci" ("Chromosome" 13). Her choice of studying corn meant that her research was slow since corn only has one growing season each year; her first paper was based on six years' work. It is also true that with the 1950s, more and more attention was given to microbiologists, whose choice of research subjects (simple bacteria) enabled them to conduct experiments much more frequently and to produce more research drawing on more data. As Gribben explains, "Many of the people [McClintock] was talking to were a new breed of biologists . . . [who] lacked the basic training or patience to cope with the complexities of understanding the genetic behaviour of a multicellular . . . organism" (302). This new breed held traditional patriarchal beliefs: "in the absolute adequacy . . . of a particular kind of (linear, causal) mechanism; . . . in the incontrovertible value of simplicity; . . . in the unitary character of truth; and . . . in the . . . equations between power and knowledge, and between virtue and power"—all of which worked against McClintock's complex vision (Keller, "Physics" 406-07). [McClintock: "Plants are extraordinary. . . . They can do almost anything you can think of. But just because they sit there, anyone walking down the road considers them just a plastic area to look at, [as if] they're not really alive" (198-99).] And they redefined biology in ways that narrowed it even further: they located the essence of life in the gene; defined life as "instructions [or information] encoded in the genes"; emphasized the goals of biology as intervening, controlling, and mastering "the processes of making and remaking life" (Keller, "Physics" 391-92). For them, "the older biology, and many of the older biologists, became objects of disdain . . ." (Keller, "Physics" 407).
So it may be that because McClintock’s work did not reflect the values of the up-and-coming microbiologists and because the slow growth rate of corn caused her to publish relatively few articles on transposition, her theory did not get the attention it deserved.

But these explanations seem partial. For example, they do not account for McClintock’s reception at a Cold Springs Harbor seminar in the 1950s where an attentive audience was eager to hear what she had to say. Yet almost no one understood her. One scientist, Stephen Jay Gould, suggested that McClintock may not have been heard because of her difficult style: “her main papers . . . are . . . tough slogging. With their unrelenting passive voice, and their compression of complex reasoning and experiment into single paragraphs, they are marvels of their genre but not models of optimum communication” (164). But McClintock’s “unrelenting passive voice” (so typical of scientific writing) is unlikely to have caused her audience serious difficulties. And while compression—especially when it omits parts of an argument and sacrifices the needs of the audience—can be confusing, I’m not convinced that McClintock’s compression was necessarily alienating. In reducing her experiments to print, she necessarily simplified them, but her texts provide evidence that she was repeatedly aware of the needs of her readers. McClintock makes explicit the assumptions identifying her hypotheses: “If a sufficiently large number of ears were obtained from the described crosses, one or more variegated kernels should appear. This conclusion is based on the following reasoning” (“Induction” 581). She provides purpose statements to clearly identify the direction of her argument: “It is the purpose of this paper to indicate that, in conformity with expectation, Ac-controlled mutability can appear at other loci of known genic action” (“Induction” 580). She presents preview statements to prepare her readers for sections to come: “Before continuing discussion of the sudden appearance of $Ds$ at new locations, it is necessary to consider another heritable factor called Activator ($Ac$)” (“Chromosome Organization” 22) and, “In considering $Ac$-controlled mutability, the following facts should be kept in mind” (“Induction” 582).

She uses summarizing statements to identify the significance of sections already presented: “The type of test outlined above may be used to detect positions of $Ac$ when it is located in the short arm of chromosome 9” (“Induction” 585). She paraphrases to reinforce particularly important points: “In other words, it is the state and the dose of $Ac$ that control just when and where $Ds$ events will occur, and it is the state of a particular $Ds$ that controls the relative frequency of any one type of event that occurs at $Ds$” (“Chromosome Organization” 26). She draws explicit conclusions:

The direction this discussion is taking may now be apparent. It is towards the conclusion that the type of mutation occurring at a locus is a function of the type of chromatin material that is present at the locus or is transposed to it, and does not involve changes in the components of the genes themselves. (“Chromosome Organization” 29)
And for those unfamiliar with or uninterested in maize research, she indicates, in report after report, the larger significance of her findings: "The author believes that the mechanism underlying the phenomenon of variegation is basically the same in all organisms" ("Origin" 345; emphasis added); "The same mechanisms may well be responsible for the origins of many of the observed mutations in plants and animals" ("Chromosome Organization" 13; emphasis added); "In a discussion of the possible significance of the evidence obtained from studies of genic instability (McClintock 1951), it was suggested that controlling units are present in all nuclei, and that they serve to regulate genic action during the development of an organism" ("Induction" 597; emphasis added).

While I lack the scientific background to judge the ultimate quality of McClintock’s reasoning, Keller (who is a scientist) suggests that it is not so much compressed as cumulative, with each of McClintock’s papers as part of a larger whole, her theory unfolding "as a hierarchy of hypotheses, each more abstract and further removed from the objects of perception than the one before, yet, in concert, providing an internal logic so compelling as to give anyone who grasps that logic the sense of being able to ‘see’ the abstractions themselves" (Keller, Feeling 126). Later publications clearly build on earlier ones, and one article from the 1950s that I examined ("Chromosome Organization and Genic Expression") is characterized by a movement away from the "object of perception" as Keller notes. That is, it does not reflect a tidy forward development of its argument but a forward development frequently interrupted by sideroads and backtracking, as McClintock offers evidence both for her claims and the warrants that those claims rest on. For instance, McClintock ends a section on "The Origin of Ds and Its Behavior" by delaying support for one claim about the significance of changes in Ds in order to present evidence about similarities between Ds and other transpositions: "A discussion of this significance [of changes in Ds and other mutable loci] will be postponed until the behavior of some other mutable loci have been considered" (16). And she begins the section on "Ac Behavior and Inheritance: General Considerations" by admitting that she has to interrupt her previous discussion on the location of Ds: "Before continuing discussion of the sudden appearance of Ds at new locations, it is necessary to consider another heritable factor called Activator (Ac)" (22). The impression from the complex movement of this essay is that McClintock is anticipating and answering every conceivable question about her new interpretation of the gene and the foundations of that interpretation so that, if her premises are accepted, her argument becomes invulnerable. Unfortunately, her premises—particularly her assumption that nature is complex and flexible—were largely unaccepted.

Rather than trace the failure of McClintock to reach her audience (and the failure of the scientific community to hear McClintock) to specific stylistic features, Keller looks at the larger question of audience and "the
force that particular kinds of models have over the understandability and, hence, acceptability of different theories in biology” (“Force” 154). Most of McClintock’s work was done in isolation. “She had developed her ideas alone . . . “ (Keller, Feeling 145), and it may have been her isolation that allowed her to look at data in a new way by giving her the distance to reject many of the “tacit assumptions” of her field. Yet this solitude as a woman, as a classical geneticist, as a mystic, as a single investigator in a field that valued collaboration, Keller explains, may also have created an “unbridgeable” communication gap between herself and her colleagues because it separated her from many of the shared interests and values essential for effective communication: “When . . . a common ‘language’ [‘unarticulated premises, mutual understandings, and assumed practices of the group’] is absent, arguments cease to be effective, even if the equations are impeccable” (Keller, Feeling 146). This lack of a “common language,” in effect, meant that it was almost impossible for her community to understand the significance of what McClintock was saying. [McClintock: “What you had to do was put [new ideas] into their frame. Wherever it came in your frame, you had to work to put it into their frame. So you work with so-called scientific methods to put it into their frame after you know” (203).] Since shared understanding is essential especially when a new theory is presented, and since the emphasis within her community “continued to be on the constancy of genes and chromosomes and on the orderly rules governing inheritance,” her work was largely ignored (Federoff 85).

It was only when molecular biologists came to discover transposition in their research that some of McClintock’s theory was taken seriously. In the 1970s, at a time when, for most investigators, “the work of maize geneticists on transposable loci remained obscure and seemed to be of doubtful relevance . . . “ (Federoff 88), transposable elements were identified in bacteria. More recently, transposable elements have been found in yeast, fruit flies, and mice (Mange and Mange 243). With these discoveries has come a reevaluation of genetic material: it is no longer believed to be necessarily static. This reevaluation did not occur because of any changes in McClintock’s perspective or her rhetoric but because of changes in the assumptions of her audience based on genic information they discovered in their own research subjects. As a result, they had come to “see” part of what she saw, but there are limits to what they have learned to see. And McClintock’s “view of transposition as a survival mechanism available to the organism in times of stress seems to most (although not to all) pure heresy” (Keller, A World” 160). [McClintock: “Various effects of an initial traumatic event . . . alerted me to anticipate unusual responses of a genome to various shocks it might receive, either produced by accidents occurring within the cell itself, or imposed from without . . . [S]tress, and the genome’s reactions to it, may underlie many formations of new species” (McClintock, “Significance” 800).]
The revival of rhetoric as a discipline designed to bracket off questions of content and truth . . . and to make a repertoire of discursive strategies available to people involved on a (potentially) professional basis in the production of meaning may be one sign of a loosening of the bonds that bind us to the single and the singular track, to a paranoid obsession with certitude and fixed and single destinations.

Dick Hebdige

In Keller’s account of McClintock’s inability to provide a persuasive explanation of her theory in the 1950s, both Keller and McClintock imply that the geneticist could have been more effective if she had “translated” her results into the language, values, and so on. of the scientific community. Yet, while adopting the “frame” of the intended audience is a proven rhetorical strategy, it may not always be possible: partiality for macho approaches to nature can distort explanations:

If hierarchical models of causation and control are associated with desirable masculine personality traits, the less hierarchical aspects of nature will be harder to detect, because they are not given reality, made visible, by the preferred hierarchical model.

(Harding 45)

Consider the dominant model of science described earlier in these pages: aggressive, hierarchical, manipulative, it sees conflict between itself and a nature who hides “her” secrets until the scientist defeats her; patriarchal, it privileges knowledge “expressed in a hierarchically arranged, closed system of binary oppositions; [valuing control,] it is concerned with achieving a unity of vision and thought, with everything in its proper place and all conflict eradicated once and for all” (Shotter and Logan 75). It is “permeated by the ideology of domination” (Longino 206). Since for McClintock the scientist should respect and collaborate with a wonderfully complex nature, adopting the “frame” of her audience after she has come to understand some of that complexity (for example, transposition) might distort what she has to say.

In other words, because McClintock’s theories are predicated upon a particular non-patriarchal relationship with nature, using her audience’s perspective might help her communicate with them but might also change what she communicates. As Heldke suggests, “certain very significant aspects of the relationship between McClintock and her corn will be lost, masked, denied by using the hierarchical, dualistic model of inquiring subject/inquired-into object” typical of patriarchal science, the science report, and McClintock’s articles (112). And when her articles occasionally depart from the values associated with scientific writing, her work may have been considered unscientific, as when she suggests surprise—“striking diversity,” “unanticipated alterations of the chromosomes,” “quite unexpected types of chromosome aberration,” “highly unexpected behavior of a genetic
factor," "unexpected types,"—or when she indicates that her work may not be entirely "rational": "some rather unorthodox conclusions have been drawn regarding the mechanisms responsible for mutations arising at these loci" ("Chromosome Organization" 13); "It may be considered that these speculations with regard to heterochromatin behavior and function have been carried further than the evidence warrants. This may be true . . . ("The Origin" 355). Even now, when the importance of much of her theory is acknowledged, because she has not been able to persuade her audience to accept her views of science and nature, McClintockian has become a code word for unscientific" (Keller, Feeling 193). [McClintock: "We know nothing . . . about how the cell senses danger and instigates responses to it that often are truly remarkable" (McClintock, "Significance" 801).]

Keller uses McClintock's experiences to support her argument that science should acknowledge itself as a "human instead of a masculine project," and that, as a human project, it should renounce "the division of emotional and intellectual labor that maintains science as a male preserve" (Reflections 178) and instead value difference, plurality, and "a richer, perhaps even multi-faceted representation of reality" ("The Gender/Science System" 41). I suggest that the example of the geneticist can support a claim about formal writing practiced not only in science but in any academic discipline, a claim about what LeGuin calls "father tongue": it is "the language of thought that seeks objectivity . . . . The essential gesture . . . is distancing—making a gap, a space, between the subject or self and the object or other . . . . The father tongue is spoken from above. It goes one way. No answer is expected or heard" (qtd. in Tompkins 173). Father tongue values competitiveness, power, and detachment—values that not everyone willingly adopts. As Lassner explains,

In academic writing women students have said that they feel assumptions about objectivity, evidence, even subject matter, do not address their experiences and in fact present them with a double bind. They must write about subjects in which women are invisible. They must use linguistic and rhetorical conventions which invalidate the logic of their experiences. And all this turns out to be for the purpose of educating women to identify with those who ignore or dismiss their concerns. (223)

Thus, the different and exciting disciplinary practices Keller envisions may need expression in something more than a single standard of scientific writing. Perhaps scientists need to tell and to write different stories.

But so do we all. Some rhetoricians, particularly feminist rhetoricians, have recently been exploring alternatives to father tongue. One alternative—some call it feminist argument—identifies a range of argument which shares an interest in the multiple and the personal that can supplement the hierarchical and competitive stance associated with professional discourse. In place of a patriarchal epistemology "that perceives the world in terms of categories, dichotomies, roles, stasis and causation," the epistemol-
ogy reflected by feminist argument "perceives the world in terms of ambiguities, pluralities, processes, continuities, and complex relationships" (Penelope 126). Thus, feminist argument asks for dialogue rather than debate, indirection rather than frontal assault, space to explore alternatives rather than premature closure:

Unlike the traditional academic essay, [it] does not rely on positions staked out in advance, on straight arrangements and tightly connected points leading to a single conclusion. Differences can be cultivated...—there is room to talk and room to listen. This is a friendly conversation, a process of composing and recomposing as other voices join in and the writer responds. "I've been thinking about these things for a while," the writer seems to say, "and I'd like to hear what you've been thinking." (Zawacki 33)

Like McClintock, feminist argument values partial and multiple answers, connectedness, and intimacy—values that make it an alternative to the objectivity, causality, and authoritarianism privileged by academic writing. I know that some academic journals such as JAC accept feminist argument, and that, under its previous editor, College Composition and Communication invited "strongly-voiced submissions,...personally-grounded articles,...submissions in various genres besides standard research- or classroom-based articles, and...articles that take risks with structure or style" (Gebhardt 9). Yet most journals still publish, most writers practice, and most teachers teach texts defined by an anonymous voice of knowledge and power and distance. This is the voice we have been trained so well to use. Adopting another voice means risking silence. It means asking audiences to read differently. It means giving up authority. Yet, while I have not found feminist argument in the natural sciences (perhaps because I read few natural science texts), I have enjoyed examples from the humanities and social sciences. I'm thinking of texts by Tompkins who, in "Me and My Shadow" for example, moves between two voices: "One is the voice of a critic who wants to correct a mistake in...[a] view of epistemology. The other is the voice of a person who wants to write about her feelings" (169). By Griffin, who, complaining that "patriarchal thought...claims to be objective, and separated from emotion...does not make that separation" in her own text (xv). By DuPlessis, who writes in order "To honor the plurality, porosity, and mobility of discourses; to combine personal and analytic; to reveal thinking as a heart-felt activity; to bring 'subjective' and 'objective' into dialectical exchange and mutual translation" (169). By Patricia Williams, who uses "an intentionally double-voiced and relational...vocabulary" to create a text that is multilayered, one "that encompasses the straightforwardness of real life and reveals the complexity of meaning" drawing "Howard Beach, polar bears, and food stamps" into legal discussions of surrogate motherhood, the homeless, and violence (6).

I'm thinking of Mulkay's The Word and the World, which departs from the established conventions of sociological texts that "seriously [limit] the
possibility of any helpful practical application of sociological knowledge,” and attempts to “explore the opportunities afforded by new textual forms”: dialogues, debates, plays and parodies (7, xi). As Mulkay explains,

The text is not presented as a linear, univocal argument, but as a series of overlapping, multi-levelled interpretative sequences representing, and themselves open to, multiple readings. The dominant underlying concern is to try to develop a new kind of analytical discourse which is not modelled on the one-dimensional scientific research text, but draws upon and uses creatively other forms of discourse in such a way that the interpretative complexity of both participants’ and analysts’ textuality can be more fully realized. (8)

I’m also thinking of Hudson and Jacot’s *The Way Men Think*, which rejects the conventional format of sociological studies with its “statement of theory, followed by examples which illustrate the theory’s implications” because of its misplaced emphasis and simple perspective:

We are unhappy ... with the subservient position which the conventional format imposes on evidence about particular individuals and the lives they lead. Such evidence should exist in a state of tension with theory, we believe, and ... should be allowed to answer theory back. Equally, we are reluctant to allow a single case or a handful of congruent cases to exert too persuasive an influence. (xi)

Instead, they offer “scientific experiments, statistics, clinical case material, biographical and autobiographical vignettes, paintings, sculptures, photographs, poems, fiction, ‘faction’ and anecdote to rub shoulders with one another on equal terms, each in its own right” (xi). And I’m thinking of collections, like Selzer’s *Understanding Scientific Prose*, which is filled with different voices offering different “truths” about an essay by Gould and Lewontin—“truths” defined by the many critical perspectives the collection offers—and which allows Gould to respond to each “truth” with his own.

Studies that privilege multiple perspectives, complexity, connectedness, and intimate relationships with their subjects might be more effectively presented in texts that also privilege these values, texts that we are just beginning to find in the books and journals we read. While she was strong enough to ignore some of the patriarchy reflected in traditional scientific practice, McClintock could not conceive of texts like these. As a result, her writing largely silenced many of the values implicit in her practice. Science is not alone in this silencing, though perhaps it is most emphatic. Encouraging the academy to move away from its reliance on the myths of objectivity, power, and “truth” in both what it does and what it writes could lead to a world where unusual voices are more easily heard; to change metaphors, a world where there will be shoes to accommodate various feet.

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Notes

1 See also Descartes's sixth discourse (1651): "We might ... make ourselves ... masters and possessors of nature" (qtd. in Shotter and Logan 73).

2 For different metaphors, see Freeman Dyson on the scientists involved in the Manhattan Project: "Nuclear explosives have a glitter more seductive than gold to those who play with them. To command nature to release in a pint pot the energy that fuels the stars, to lift by pure thought a million tons of rock in the sky, these are exercises of the human will that produce an illusion of illimitable power" (qtd. in Frechet 211).

3 Throughout this paper, you will find bracketed quotations from McClintock. I have included these in this way to offer another voice to this essay. Unless otherwise indicated, these quotations come from Keller's A Feeling for the Organism.

4 See also Latour and Woolgar's Laboratory Life, about which Finke comments that "What Latour's work suggests is that we do not encounter a ready-made world through objective observation. 'We' 'make' 'it.' This we is radically heterogeneous, the making is contentious, dialogic, and its products are complex. To control the destabilizing implications of this process, science traditionally presents for consumption tidy narratives of discovery" (19).

5 A 1984 Scientific American article explains transposition this way:

A transposable genetic element is a bit of DNA that can move from place to place in an organism's genome (its total complement of genetic material). It is excised from one site and inserted at another site either on the same chromosome or on a different one.... The movement of a transposable element can generate mutations or chromosomal rearrangements and thus affect the expression of other genes. Chromosomes are found to be littered with these mobile elements. (Fedoroff 85)

6 A cell contains twenty-three pairs of chromosomes; in each bundle of chromosomes is DNA; in the DNA are thousands of genes that are randomly distributed.

7 The Time article on McClintock's Nobel Prize is accompanied by a four-frame cartoon (Wallis 53). The "Structural Gene" (SG) which colors the kernel (unless disrupted), the Activator, and the Dissociator are all pictured as male.

8 This audience reaction is not unique. One presentation on string theory to an audience of two-hundred scientists at the Institute for Advanced Study, for example, was met with "stunned silence":

There are no questions. It's as if a wave has washed over the heads of these assembled intellects and now they're gasping for breath. They've just beheld the new vision of nature, the key to the cosmos, the secret of creation—it's been spread out in front of them in all its technical minutiae—but people's minds have gone blank, blootto, tabula rasa. There were a few recognizable phrases sprinkled in here and there ... but most of the stuff that [the scientist] was saying was so new to them, and so alien, that he might have as well have been speaking Navajo. (Regis 256)

9 Also see LeFevre: "In McClintock's case, and in many other instances across the disciplines, the absence of social linkages—whether that be due to one's inability to be persuasive, or the inability of one's audience to acknowledge unfamiliar ideas, or a combination of both—results in frustration for the inventor and a delay during which a discovery does not resonate in an intellectual community" (76).

10 A time lapse between the presentation and the acceptance of a new theory is not unusual—it was fifteen years before physicists accepted Einstein's theory that light moves as particles, not waves—but the community has not yet accepted McClintock's full theory, only a "tame" version of it.

11 See, for example, Bleich, who writes that "Academic discourse works in a tradition of deliberately excluding experience, of aiming to purify thought of both experience and feeling so that some ideal of pure truth, linked to the intellectual formulations of one or a few men, may somehow miraculously come to preside over everyone's common experience of living" (19).

12 The rejection of the authoritarian stance of science has been noted in oral presentations given by women. As Namenwirth explains,
It is noteworthy that when women scientists give public lectures about their research, they often call attention to the limitations of their data, to potential flaws in the experimental design, to control experiments that remain to be done. They engage in a kind of public criticism of their own work, taking pains not to overstate their findings or deceive the audience about the work's impregnability. (23)

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**Olson Wins Awards**

*JAC* editor Gary A. Olson was presented an international award as "Distinguished Retiring Editor" by the Council of Editors of Learned Journals, an international organization affiliated with the Modern Language Association, at the annual MLA convention in December.

Numerous scholars in English studies supported Olson's nomination. Literary critic Stanley Fish wrote that thanks to Olson's work "the level of sophistication with which important pedagogical matters are raised and discussed is unsurpassed." Feminist cultural critic Jane Tompkins commended Olson for "his outstanding performance as editor of *JAC,*" and former MLA president, J. Hillis Miller, also lent his support.

At the CCCC convention in Nashville, the executive board of the Association of Teachers of Advanced Composition presented Olson with a similar award. Olson completes his term as editor with this issue of the journal.