Narrowing the Mind and Page: Remedial Writers and Cognitive Reductionism

Mike Rose

There has been a strong tendency in American education—one that took modern shape with the I.Q. movement—to seek singular, unitary cognitive explanations for broad ranges of poor school performance. And though this trend—I'll call it cognitive reductionism—has been challenged on many fronts (social and political as well as psychological and psychometric), it is surprisingly resilient. It re-emerges. We see it in our field in those discussions of basic and remedial writers that suggest that unsuccessful writers think in fundamentally different ways from successful writers. Writing that is limited to the concrete, that doesn't evidence abstraction or analysis, that seems illogical is seen, in this framework, as revealing basic differences in perception, reasoning, or language.¹ This speculation has been generated, shaped, and supported by one or more theories from psychology, neurology, and literary studies.

Studies of cognitive style suggest that people who can be characterized as "field-dependent" (vs. those who are "field-independent") might have trouble with analytical tasks. Popular articles on brain research claim a neurophysiological base for some humans to be verbal, logical, analytical thinkers and for others to be spatial, holistic, non-verbal thinkers. Jean Piaget's work on the development of logical thought seems pertinent as well: some students might not have completed their developmental ascent from concrete to abstract reasoning. And orality-literacy theorists make connections between literacy and logic and suggest that the thinking of some minority groups might be affected by the degree to which their culture has moved from oral to literate modes of behavior.

The applications of these theories to poor writers appear in composition journals and papers at English, composition, and remedial education conferences. This is by no means the only way people interested in college-age remedial writers talk about thinking-writing connections, but the posing of generalized differences in cognition and the invoking of Piaget, field depend-

¹ Mike Rose is Associate Director of Writing Programs at UCLA. His books include Writer's Block (1984), When a Writer Can't Write (1985), and Perspectives on Literacy (1988), edited with Eugene Kintgen and Barry Kroll. He has recently completed Lives on the Boundary, a book on educational underpreparation in America.
ence and the rest has developed into a way of talking about remediation. And though this approach has occasionally been challenged in journals, it maintains a popular currency and encourages a series of bold assertions: poor writers can’t form abstractions; they are incapable of analysis; they perceive the world as an undifferentiated whole; the speech patterns they’ve acquired in their communities seriously limit their critical capacity.

I think we need to look closely at these claims and at the theories used to support them, for both the theories and the claims lead to social distinctions that have important consequences, political as well as educational. This is not to deny that the theories themselves have contributed in significant ways to our understanding of mental processes (and Piaget, of course, shaped an entire field of research), but their richness should not keep us from careful consideration of their limits, internal contradictions, and attendant critical discussions and counterstatements. Consideration of the theories leads us naturally to consideration of their applicability to areas beyond their original domain. Such application often overgeneralizes the theory: Ong’s brilliant work on orality and literacy, for example, moves beyond its history-of-consciousness domain and becomes a diagnostic framework. A further problem—sometimes inherent in the theories themselves, sometimes a result of reductive application—is the tendency to diminish cognitive complexity and rely on simplified cognitive oppositions: independent vs. dependent, literate vs. oral, verbal vs. spatial, concrete vs. logical. These oppositions are textbook-neat, but, as much recent cognitive research demonstrates, they are narrow and misleading. Yet another problem is this: these distinctions are usually used in a way meant to be value-free (that is, they highlight differences rather than deficits in thinking), but, given our culture, they are anything but neutral. Social and political hierarchies end up encoded in sweeping cognitive dichotomies.

In this article I would like to reflect on the problems with and limitations of this particular discourse about remediation. To do this, I’ll need to provide a summary of the critical discussion surrounding each of the theories in its own field, for that complexity is too often lost in discussions of thought and writing. As we move through the essay, I’ll point out the problems in applying these theories to the thought processes of poor writers. And, finally, I’ll conclude with some thoughts on studying cognition and writing in less reductive ways.

Cognitive Style: Field Dependence-Independence

Cognitive style, broadly defined, is an “individual’s characteristic and consistent manner of processing and organizing what he [or she] sees and thinks about” (Harré and Lamb 98). In theory, cognitive style is separate from verbal, quantitative, or visual intelligence; it is not a measure of how much people know or how well they mentally perform a task, but the manner in which
they perform, their way of going about solving a problem, their style. Cognitive style research emerges out of the study of individual differences, and there have been a number of theories of cognitive style proposed in American and British psychology since the late 40's. Varied though they are, all the theories discuss style in terms of a continuum existing between two polar opposites: for example, reflectivity vs. impulsivity, analytic vs. global, complexity vs. simplicity, levelling vs. sharpening, risk-taking vs. cautiousness, field-dependence vs. field-independence. Field dependence-independence, first described by Herman A. Witkin in 1949, is, by far, the most researched of the cognitive styles, and it is the style that seems to be most discussed in composition circles.

The origins of the construct are, as Witkin, Moore, Goodenough, and Cox note, central to its understanding. Witkin's first curiosity concerned the degree to which people use their surrounding visual environment to make judgments about the vertical position of objects in a field. Witkin devised several devices to study this issue, the best known being the Rod and Frame Test. A square frame on a dark background provides the surrounding visual field, and a rod that rotates within it is the (potentially) vertical object. Both the frame and the rod can separately be rotated clockwise or counter-clockwise, and "[t]he subject's task is to adjust the rod to a position where he perceives it as upright, while the frame around it remains in its initial position of tilt" ("Field-Dependent" 3). Witkin, et al.'s early findings revealed some interesting individual differences:

For some, in order for the rod to be apprehended as properly upright, it must be fully aligned with the surrounding frame, whatever the position of the frame. If the frame is tilted 30 [degrees] to the right, for example, they will tilt the rod 30 [degrees] to the right, and say the rod is perfectly straight in that position. At the opposite extreme of the continuous performance range are people who adjust the rod more or less close to the upright in making it straight, regardless of the position of the surrounding frame. They evidently apprehend the rod as an entity discrete from the prevailing visual frame of reference and determine the uprightness of the rod according to the felt position of the body rather than according to the visual frame immediately surrounding it. ("Field-Dependent" 3-4)

A subject's score is simply the number of degrees of actual tilt of the rod when the subject claims it is straight.

Witkin and his associates later developed another measure—one that was much less cumbersome and could be given to many people at once—The Embedded Figures Test. 2 Witkin, et al. considered the Embedded Figures Test to be similar to the Rod and Frame Test in its "essential perceptual structure." The subject must locate a simple geometric design in a complex figure, and "once more what is at issue is the extent to which the surrounding visual framework dominates perception of the item within it" (6). A subject's score on the test is the number of such items he or she can disembody in a set time.
The "common denominator" between the two tests is "the extent to which the person perceives part of the field as discrete from the surrounding field as a whole, rather than embedded in the field; or the extent to which the organization of the prevailing field determines perception of its components" (7). Put simply, how strong is our cognitive predisposition to let surrounding context influence what we see? Witkin soon began to talk of the differences between field dependence vs. independence as differences between articulated (or analytic) vs. global perception:

At one extreme there is a consistent tendency for experience to be global and diffuse; the organization of the field as a whole dictates the manner in which its parts are experienced. At the other extreme there is a tendency for experience to be delineated and structured; parts of a field are experienced as discrete and the field as a whole organized. To these opposite poles of the cognitive styles we may apply the labels "global and articulated." ("Psychological Differentiation" 319)

Witkin's tests were tapping interesting individual differences in perception and cognition, but the really tantalizing findings emerged as Witkin and his colleagues began pursuing a wide-ranging research agenda that, essentially, sought correlations between performance on field dependence-independence tests and performance on a variety of other cognitive, behavioral, and personality tests, measures, and activities. Hundreds of these studies followed, ranging from the insightful (correlating cognitive style with the way teachers structure social science concepts) to the curious (correlating cognitive style with the shortness of women's skirts). Some of the studies yielded low correlations, and some were inconclusive or were contradictory—but, in general, the results, as summarized by educational psychologist Merlin Wittrock, resulted in the following two profiles:

- To the degree that people score high on field independence they tend to be: "relatively impersonal, individualistic, insensitive to others and their reinforcements, interested in abstract subject matter, and intrinsically motivated. They have internalized frames of reference, and experience themselves as separate or differentiated from others and the environment. They tend to use previously learned principles and rules to guide their behavior" (93).

- To the degree that people score low on field independence they are, by default, field-dependent, and they tend to be: "more socially oriented, more aware of social cues, better able to discern feelings of others from their facial expressions, more responsive to a myriad of information, more dependent on others for reinforcement and for defining their own beliefs and sentiments, and more in need of extrinsic motivation and externally defined objectives" (93).

The tendency of the field-independent person to perceive particular shapes and orientations despite context, and the tendency of the field-dependent person to
let "the organization of the field as a whole dictate the manner in which its parts are experienced" seemed to be manifesting themselves in motivation, cognition, and personality. A few relatively simple tests were revealing wide-ranging differences in the way people think and interact.

The psychometric neatness of this work seems a little too good to be true, and, in fact, problems have been emerging for some time. My discussion of them will be oriented toward writing.

You'll recall that it is central to the theory that cognitive style is not a measure of ability, of how well people perform a task, but a measure of their manner of performance, their style. If we applied this notion to writing, then, we would theoretically expect to find interesting differences in the way discourse is produced, in the way a rhetorical act is conceived and executed: maybe the discourse of field independents would be more analytical and impersonal while field-dependent discourse would be richer in social detail. But these differences should not, theoretically, lead to gross differences in quality. By some general measure, papers written by field-dependent and field-independent students should have equal possibility of being acceptable discourse. They would just be different. However, the most detailed and comprehensive cognitive style study of college-level writers I've yet seen yields this: papers written by field-dependent students are simply poor papers, and along most dimensions—spelling, grammar, development (Williams). This doesn't fit. Conclusions emerge, but they don't jibe with what the theory predicts.

Such conceptual and testing perplexities are rooted, I believe, in the field dependence-independence work itself. My review of the psychological literature revealed seven problems with the construct, and they range from the technical to the conceptual level.

For cognitive style to be a legitimate construct, it has to be distinct from general intelligence or verbal ability or visual acuity, because cognitive style is not intended to be a measure of how "smart" someone is, but of the manner in which she or he engages in an intellectual task. Unfortunately, there are a number of studies which suggest that field dependence-independence significantly overlaps with measures of intelligence, which are, themselves, complex and controversial. As early as 1960, Lee J. Cronbach wrote in his authoritative Essentials of Psychological Testing: "General reasoning or spatial ability accounts for much of Embedded Figures performance as does difficulty in handling perceptual interference" (549). In 1972, Philip Vernon, also a prominent researcher of individual differences, reviewed studies that investigated relations between scores on field dependence-independence and various measures of "visual intelligence." He concluded that "the strong positive correlation with such a wide range of spatial tests is almost embarrassing" (368). And after conducting his own study, Vernon decried that Embedded Figures Tests "do not define a factor distinct from general intelligence . . . and spatial ability or visualization" (386). Things become more complicated. Vernon, and other researchers (see, for example, Linn and Kyllonen), present
factor-analytic data that suggest that determining the position of the rod within the frame and disembedding the hidden figures tap different mental constructs, not the unitary construct Witkin had initially postulated. It is possible, of course, that different aspects of field dependence-independence are being tapped by the different tests and that two of them should be administered together—as Witkin, in fact, recommended. But even if researchers used multiple measures (as few have—most use only the Embedded Figures Test because of its utility), the problem of overlap with measures of intelligence would remain. In short, it’s not certain just what the field dependence-independence tests are measuring, and it’s very possible that they are primarily tapping general or spatial intelligence.

There is a further testing problem. In theory, each pole of a cognitive style continuum “has adaptive value in certain circumstances . . . neither end of [a] cognitive style dimension is uniformly more adaptive . . . adaptiveness depends upon the nature of the situation and upon the cognitive requirements of the task at hand” (Messick 9). Now, there have been studies which show that field-dependent people seem to attend more readily than field-independent people to social cues (though the effects of these studies tend to be small or inconsistent—see McKenna), but it is important to note that Witkin and his colleagues have never been able to develop a test that positively demonstrates field dependence. The Rod and Frame Test, the Embedded Figures Test—and all the other tests of field dependence-independence—assess how well a person displays field independence. Field dependence is essentially determined by default—the more a person fails at determining the true position of the rod or the slower he is at disembedding the figure, the more field dependent he is. This assessment-by-default would not be a problem if one were testing some level of skill or intellectual ability, say, mechanical aptitude. But where a bipolar and “value-free” continuum is being assessed—where one is not “deficient” or “maladaptive” regardless of score, but only different, where both field-independent and field-dependent people allegedly manifest cognitive strengths as well as limitations—then it becomes a problem if you can’t devise a test on which field-dependent subjects would score well. Witkin, et al. admit that the development of such a test is “an urgent task” (16). It has not yet been developed.

But even if a successful test of field dependence could be created, problems with assessment would not be over. All existing tests of field dependence-independence are, as Paul L. Wachtel points out:

> in certain respects poorly suited for exploration of the very problem [they were] designed to deal with—that of style. It is difficult to organize ideas about different directions of development upon a framework which includes only one dimension, and only the possibility of “more” or “less.”

(186)

Consider the notion of style. It would seem that style is best assessed by the observation and recording of a range of behaviors over time. Yet the Rod and
Frame and Embedded Figures Tests don't allow for the revelation of the cognitive processes in play as the person tries to figure them out. That is, there is no provision made for the subject to speak aloud her mental processes or offer a retrospective account of them or explain—as in Piagetian method—why she's doing what she's doing. We have here what Michael Cole and Barbara Means refer to as the problem of drawing process inferences from differences in task performance (65). It would be unfair to lay this criticism on Witkin's doorstep alone, for it is a general limitation with psychometric approaches to cognition. (See, for example, Hunt.) But Witkin's work, since it purports to measure style, is especially vulnerable to it.

Let us now rethink those composite profiles of field-independent vs. field-dependent people. You'll recall that the correlations of all sorts of measures suggest that field-dependent people are more socially oriented, more responsive to a myriad of information, etc., while field-independent people tend to be individualistic, interested in abstract subject matter, and so on. These profiles can be pretty daunting; they're built on hundreds of studies, and they complement our folk wisdom about certain kinds of personalities. But we must keep in mind that the correlations between tests of field independence and personality or cognitive measures are commonly .25 to .3 or .4; occasionally, correlations as high as .5 or .6 are recorded, but they are unusual. That means that, typically, 84% to 94% of the variance between one measure and the other remains to be accounted for by factors other than those posited by the cognitive style theorist. Such studies accrue, and eventually the theorist lays them all side by side, notes the seeming commonalities, and profiles emerge. You could consider these profiles telling and veridical, but you could also consider them webs of thin connection.

We in the West are drawn to the idea of consistency in personality (from Renaissance humors to Jungian types), and that attraction, I think, compels us to seek out similar, interrelated consistencies in cognition. Certainly there are regularities in the way human beings approach problems; we don't go at our cognitive tasks willy-nilly. But when cognitive researchers try to chart those consistencies by studying individual people solving multiple problems they uncover a good deal of variation, variation that is potentially efficient and adaptive. William F. Battig, for example, found in his studies of adult verbal learning that most subjects employed different strategies at different times, even when working on a single problem. At least in the cognitive dimension, then, it has proven difficult to demonstrate that people approach different problems, in different settings, over time in consistent ways. This difficulty, it seems to me, presents a challenge to the profiles provided by cognitive style theorists.

There are, finally, troubling conceptual-linguistic problems with field dependence-independence theory, and they emerge most dramatically for me when I try to rephrase some of Witkin's discussions of the two styles. Here is one example:
Persons with a global style are more likely to go along with the field "as is," without using such mediational processes as analyzing and structuring. In many situations field-independent people tend to behave as if governed by general principles which they have actively abstracted from their experiences . . . In contrast, for field-dependent people information processing systems seem to make less use of such mediators. (Witkin, et al. 21)

Statements like this are common in Witkin, and they flow along and make sense in the discussion he offers us—but you stop cold if you consider for a minute what it might mean for people to have a tendency to operate in the world "without using such mediational processes as analyzing and structuring" or, by implication, to not "behave as if governed by general principles which they have actively abstracted from their experiences." These seem like pretty extreme claims, given the nature and limitations of tests of cognitive style. All current theories of cognition that I'm familiar with posit that human beings bring coherence to behavior by abstracting general principles from experiences, by interpreting and structuring what they see and do. When people can't do this sort of thing, or can only do it minimally, we assume that something is seriously wrong with them.

Witkin and his colleagues faced the dilemma that all theory builders face: how to find a language with which to express complex, abstract ideas. (For a Wittgensteinian analysis of Witkin's language, see Kurtz.) And given the nature of language, such expression is always slippery. I think, though, that Witkin and company get themselves into more than their fair share of trouble. The language they finally choose is often broad and general: it is hard to operationalize, and, at times, it seems applicable post hoc to explain almost any result (see Wachtel 184-85). It is metaphoric in troubling ways. And it implies things about cognition that, upon scrutiny, seem problematic. I would suggest that if we're going to apply Witkin's notions to the assessment of writing and cognition we'll need more focussed, less problematic definitions. Now, Witkin does, in fact, occasionally provide such definitions, but they raise problems of a different order. And here again we see the complications involved in connecting Witkin's theory to composing.

In an admirably precise statement, Witkin, et al. note:

The individual who, in perception, cannot keep an item separate from the surrounding field—in other words, who is relatively field-dependent—is likely to have difficulty with that class of problems, and, we must emphasize, only with that class of problems, where the solution depends on taking some critical element out of the context in which it is presented and restructuring the problem material so that the item is now used in a different context. (9)

Consider rhetoric and the production of written language. For Witkin's formulation to apply, we would have to define rhetorical activity and written language production as essentially involving the disembedding of elements
from contexts and concomitant restructuring of those contexts. It seems to me that such application doesn’t hold. Even if there were a rhetorical-linguistic test of cognitive style—and there isn’t; the tests are visual, perceptual-orientational—I think most of us would say that while we could think of linguistic-rhetorical problems that might fit Witkin’s description, it would be hard to claim that it characterizes rhetorical activity and linguistic production in any broad and inclusive way.

Second of all, it’s important to remember that Witkin is talking about a general disembedding skill, a skill that would be effective in a wide range of contexts: engineering, literature, social relations. A number of contemporary students of cognition, however, question the existence of such general cognitive skills and argue for more domain-specific strategies, skills, and abilities (see, for example, Carey; Fodor; Gardner, Frames; Glaser; Perkins). Given our experience in particular domains, we may be more or less proficient at disembedding and restructuring problem areas in literature but not in engineering. Our ability to disembed the hidden geometric figures in Witkin’s test may be more related to our experience with such visual puzzles than to some broad cognitive skill at disembedding. If a student can’t structure an essay or take a story apart in the way we’ve been trained to do, current trends in cognitive research would suggest that her difficulties have more to do with limited opportunity to build up a rich network of discourse knowledge and strategy than with some general difference or deficit in her ability to structure or analyze experience.

Hemisphericity

The French physician Paul Broca announced in 1865 that “we speak with the left hemisphere”; neurologists have had clinical evidence for some time that damage to certain areas of the left side of the brain could result in disruptions in production or comprehension of speech—aphasia—and that damage to certain areas of the right could result in space and body orientation problems; laboratory experiments with healthy people over the last 25 or so years have demonstrated that particular linguistic or spatial capacities seem to require the function of regions in the left or right brain respectively (though it is also becoming clear that there is some degree of right hemisphere involvement in language production and comprehension and left hemispheric involvement in spatial tasks); and radical neurosurgery on a dozen or so patients with intractable epilepsy—a severing of the complex band of neural fibers (the commissures) that connect the left and right cerebral hemispheres—has provided dramatic, if highly unusual, illustration of the anatomical specialization of the hemispheres. It is pretty much beyond question, then, that different areas of the brain contribute to different aspects of human cognition. As with any biological structure there is variation, but in 98% of right handers and 70% of
"non-right" handers, certain areas of the left hemisphere are critical for the processing of phonology and syntax and for the execution of fine motor control, and certain areas of the right hemisphere are involved in various kinds of visual and spatial cognition.

These conclusions evolve from either clinical observation or experimental studies. Most studies fit the following paradigm: a set of tasks is presented to a subject, and the tasks are either isomorphic with the process under investigation (e.g., distinguishing nonsense syllables like "pa," "ta," "ka," "ba" as a test of phonetic discrimination) or can be assumed, in a common sense way, to tap the activity under investigation (e.g., mentally adding a list of numbers as a test of serial processing). The subject's speed or accuracy is recorded and, in some studies, other measures are taken that are hypothesized to be related to the mental processes being studied (e.g., recording the brain wave patterns or blood flow or glucose metabolism of the cerebral hemispheres while the subject performs the experimental task).

Studies of this type have enabled researchers to gain some remarkable insight into the fine neuropsychological processes involved in understanding language and, to a lesser degree, in making spatial-orientational discriminations. But it is also true that, ingenious as the work has been, the field is still at a relatively primitive state: many studies are difficult to duplicate (a disturbing number of them yield conflicting results), and the literature is filled with methodological quarrels, competing theories, and conceptual tangles. (For a recent, and very sympathetic, overview see Benson and Zaidel.)

In spite of the conflicts, there are various points of convergence in the data, and, in the yearning for parsimony that characterizes science, the areas of agreement have led some neuroscientists to seek simple and wide-ranging characterizations of brain function. They suggest that beneath all the particular findings about syntax and phonetics and spatial discrimination lie fundamental functional differences in the left and right cerebral hemispheres: each is best suited to process certain kinds of stimuli and/or each processes stimuli in distinct ways. A smaller number of neuroscientists—and many popularizers—go a step further and suggest that people tend toward reliance on one hemisphere or the other when they process information. This theory is commonly referred to as "hemisphericity" (Bogan, DeZure, TenHouten, and Marsh). And a few sociologically oriented theorists take another, truly giant, step and suggest that entire dominant and subdominant groups of people can be characterized by a reliance on left or right hemispheric processing (TenHouten).

We have, then, the emergence of a number of cognitive dichotomies: the left hemisphere is characterized as being analytic while the right is holistic (or global or synthetic); the left is verbal, the right non-verbal (or spatial); the left a serial processor, the right a parallel processor—and the list continues: focal vs. diffuse, logical vs. intuitive, propositional vs. appositional, and so on.

The positing of hemispheric dichotomies is understandable. Human beings are theory-makers, and parsimony is a fundamental criterion by which we
judge the value of a theory: can it account for diverse data with a simple explanation? But, given the current state of brain research, such generalizations, to borrow Howard Gardner’s phrase, leapfrog from the facts (“What We Know” 114). Gardner is by no means alone in his criticism. My reading of the neuroscientific literature reveals that the notion of dichotomous hemispheric function is very controversial, and the further notion of hemisphericity is downright dismissed by a broad range of neuroscientists, psychologists, psycholinguists, and research psychiatrists:

[T]he concepts [analytic/synthetic, temporal/spatial, etc.] are currently so slippery that it sometimes proves impossible to maintain consistency throughout one paper. (John C. Marshall in Bradshaw and Nettleton 72)

[M]uch of perception (certainly of visual perception) is very difficult to split up this way. The alleged dichotomy [between temporal-analytic and spatial-holistic] is, if it exists at all, more a feature of laboratory experiments than of the real world. (M.J. Morgan in Bradshaw and Nettleton 74)

[T]he idea of hemisphericity lacks adequate foundation and . . . because of the assumptions implicit in the idea of hemisphericity, it will never be possible to provide such a foundation. The idea is a misleading one which should be abandoned. (Beaumont, Young, and McManus 191)

The above objections rise from concerns about method, subjects, and conceptualization. Let me survey each of these concerns.

A significant amount of the data used to support hemisphericity—and certainly the most dramatic—is obtained from people in whom accident or pathology has highlighted what particular sections of the brain can or can’t do. The most unusual group among these (and they are much-studied) is the handful of people who have had severe and life-threatening epilepsy alleviated through a radical severing of the neural fibers that connect the right and left hemispheres. Such populations, however, present a range of problems: tumors and wounds can cause disruptions in other areas of the brain; stroke victims could have had previous “silent strokes” and could, as well, be arteriosclerotic; long disease histories (certainly a characteristic of the severe epileptics who underwent split-brain surgery) can lead to compensatory change in brain function (Bogan, “The Dual Brain”; Whitaker and Ojemann). Furthermore, extra-pathological factors, such as education and motivation, can, as Bradshaw and Nettleton put it, also “mask or accentuate the apparent consequences of brain injury.” (51). And, as a final caution, there is this: the whole enterprise of localizing linguistic function through pathological performance is not without its critics (see Caplan).

Studies with healthy subjects—and there are increasing numbers of these—remove one major difficulty with hemisphericity research, though here methodological problems of a different sort arise. Concern not with subjects but with instruments and measures now comes into focus. Space as well as my own technological shortcomings prohibit a full review of tools and methods,
but it might prove valuable to briefly survey the problems with a representative research approach: electroencephalographic methods. (Readers interested in critical reviews of procedures other than the one I cover can consult the following: Regional Cerebral Blood Flow: Beaumont; Lateral Eye Movements: Ehrlichman and Weinberger; Tachistoscopic Methods: Young; Dichotic-Listening Tests: Efron.)

If you hypothesize that certain kinds of tasks (like discriminating between syllables or adding a list of numbers) are primarily left-brain tasks and that others (like mentally rotating blocks or recognizing faces) are primarily right-brain tasks, then neuroelectric activity in the target hemisphere should vary in predictable ways when the subject performs the respective tasks. And, in fact, such variation in brain wave activity has been empirically demonstrated for some time. Originally, such studies relied on the electroencephalogram (EEG)—the ongoing record of brain wave activity—but now it is possible to gain a more sophisticated record of what are called event-related potentials (ERP). ERP methods use the electroencephalographic machinery, but rely on computer averaging and formalization to more precisely relate brain wave activity to repeated presentations of specific stimuli (thus the waves are "event-related"). The advantage of EEG and ERP methods is that they offer a direct electrophysiological measurement of brain activity and, especially in the case of ERP, "can track rapid fluctuation in brain electrical fields related to cognitive processing . . . " (Brown, Marsh, and Ponsford 166). Such tracking is important to hemisphericity theorists, for it can lend precision to their claims.

There are problems, however. EEG/ERP methods are among the most technically demanding procedures in psychology, and that technical complexity gives rise to a number of difficulties involving variation in cortical anatomy, electrode placement, and data analysis (Beaumont; Gevins, Zeitlin, Doyle, Schaffer, and Callaway). And, when it comes to the study of language processing—certainly an area of concern to writing researchers—ERP procedures give rise to problems other than the technical. Most ERP studies must, for purposes of computer averaging, present each stimulus as many as 50 times, and such repetition creates highly artificial linguistic processing conditions. Even relatively natural language processing studies have trouble determining which perceptual, linguistic, or cognitive factors are responsible for results (see, e.g., Hillyard and Woods). So, though hemispheric differences in brain wave patterns can be demonstrated, the exceptional technical and procedural difficulties inherent in the EEG/ERP studies of language processing make it hard to interpret data with much precision. Cognitive psychophysicists Emanuel Donchin, Gregory McCarthy and Marta Kutas summarize this state of affairs:

[A]lthough a substantial amount of clinical data support the theory of left hemisphere superiority in language reception and production, the ERP data regarding this functional asymmetry are far from consistent. The methodological and statistical shortcomings which exist in some of the
studies cited (in their review article) along with inconsistencies in the others render any decision about the efficacy of ERP's as indices of linguistic processing inconclusive. (239. For similar, more recent, assessments, see Rugg; Beaumont, Young, and McManus.)

In considering the claims of the hemisphericity theorists, we have reviewed problems with subjects, techniques, and procedures. There is yet a further challenge to the notion of hemisphericity. Some hemisphericity theorists believe that since people can be characterized by a tendency to rely on one hemisphere or the other, then such reliance should manifest itself in the way people lead their lives: in the way they solve problems, in the jobs they choose, and so on. Yet the few studies that have investigated this dimension of the theory yielded negative results. Hemisphericity advocates Robert Ornstein and David Galin failed to find overall systematic EEG differences between lawyers (assumed to be left hemispheric) and sculptors and ceramicists (assumed to be right hemispheric). In a similar study, Dumas and Morgan failed to find EEG differences between engineers and artists, leading the researchers to conclude that "the conjecture that there are 'left hemispheric' people and 'right hemispheric' people seems to be an oversimplification" (227). In a more ambitious study, Arndt and Berger gave graduate students in law, psychology, and sculpture batteries of tests to assess verbal analytic ability (for example, a vocabulary test) and spatial ability (for example, a figure recognition test), and, as well, tests to assess hemisphericity (letter and facial recognition tachistoscopic tasks). While they found—as one would expect—a significant correlation between verbal or spatial ability and occupation (e.g., sculptors scored better than lawyers on the spatial tests), they did not find significant correlations between the verbal or spatial tests and the hemisphericity task; nor did they find significant correlation between the hemisphericity task and occupation.

A postscript on the above. Failures to find hemispheric differences between individuals of various occupational groups—along with the methodological difficulties mentioned earlier—throw into serious doubt the neurosociological claim that entire groups of people can be characterized as being left or right hemispheric. The neurosociological literature makes some remarkable speculative leaps from the existence of left-right dualities in cultural myth and symbol to asymmetries in left-right brain function, and relies, for empirical support, on the results of individual verbal and spatial tests (like the sub-tests in I.Q. assessments)—precisely the kinds of tests that a number of psychologists and neurologists have shown to be limited in assessing left or right hemispheric performance (see, e.g., DeRenzi).

Let me try to draw a few conclusions for rhetoric and composition studies.

It is important to keep in mind that the experimental studies that do support hemispheric specialization suggest small differences in performance capacities, and the differences tend to be of degree more than kind: in the range of
6-12%. Researchers have to expose subjects to many trials to achieve these differences. (One hundred and fifty to two hundred is common; one facial recognition study ran subjects through 700 trials.) And the experiments deal with extremely specific—even atomic—functions. (Researchers consider the distinguishing of homonyms in a sentence—"bear" vs. "bare"—to be a "complex verbal task"). It is difficult to generalize from results of this type and magnitude to broad statements about one hemisphere being the seat of logic and the other of metaphor. What happens, it seems, is that theorists bring to very particular (though, admittedly, very important) findings about phonology or syntax or pattern recognition a whole array of cultural beliefs about analytic vs. synthetic thinking and logic vs. creativity and apply them in blanket fashion. There is a related problem here, and it concerns the hemisphericity theorists' assumption that, say, distinguishing phonemes is an analytical or serial or propositional task while, say, facial recognition is synthetic or holistic or appositional. These assumptions are sensible, but they are not proven. In fact, one could argue the other way around: e.g., that recognizing faces, for example, is not a holistic but a features analysis task. Unfortunately, neuroscientists don't know enough to resolve this very important issue. They work with indirect measures of information processing: differences in reaction time or variations in electrophysiological measures. They would need more direct access than they now have to the way information is being represented and problems are being solved.

Because the accounts of cerebral asymmetry can be so dramatic—particularly those from split-brain studies—it is easy to dwell on differences. But, in fact, there is wide-ranging similarity, overlap, and cooperation in the function of the right and left hemispheres:

Complex psychological processes are not 'localized' in any one hemisphere but are the result of integration between hemispheres. (Alexander Luria cited in LeDoux 210)

If Luria's dictum applied anywhere, it would certainly be to the "complex psychological processes" involved in reading and writing. Under highly controlled laboratory conditions researchers can show that phoneme discrimination or word recognition can be relatively localizable to one hemisphere or the other. But attempts to comprehend or generate writing—what is perceived or produced as logical or metaphoric or coherent or textured—involves a stunning range of competencies: from letter recognition to syntactic fluency to an understanding of discourse structure and genre (see, e.g., Gardner and Winner 376-80). And such a range, according to everything we know, involves the whole brain in ways that defy the broad claims of the hemisphericity theorists. When students have trouble structuring an argument or providing imagistic detail, there is little neurophysiological evidence to support contentions that their difficulties originate in organic predisposition or social conditioning to rely on one hemisphere or the other.
Jean Piaget and Stages of Cognitive Development

Piaget’s theory of cognitive development is generally held to be, even by its revisors and detractors, the modern West’s most wide-ranging and significant account of the way children think. The theory, which Piaget began to articulate over 50 years ago, covers infancy to adolescence and addresses the development of scientific and mathematical reasoning, language, drawing, morality, and social perception; it has shaped the direction of inquiry into childhood cognition; and it has led to an incredible number of studies, a good many of which have been cross-cultural. In holding to the focus of this article, then, there’s a lot I’ll have to ignore—I’ll be limiting myself to those aspects of Piaget’s theory that have been most widely discussed in reference to college-age writers.

Though Piaget and his colleagues adjusted their theory to account for the wealth of data being generated by researchers around the world, there are several critical features that remain central to the theory. Piaget’s theory is a stage theory. He posits four general stages (some with substages), and all children pass through them in the same order. A child’s reasoning at each stage is qualitatively different from that at earlier or later stages, though the knowledge and strategies of earlier stages are incorporated into later ones. During any given stage, the child reasons in similar ways regardless of the kinds of problems she or he faces, and Piaget tended to rule out the possibility that, during a given stage, a child could be trained to reason in much more sophisticated ways. Passage, evolution really, from one stage to the next occurs over time, an interaction of genetic processes and engagement with the world. The child continually assimilates new information which both reshapes and is reshaped by the knowledge structures the child currently has—and, as the child continues to interact with the world, she or he experiences discontinuities between the known and the new, and these discontinuities lead to further development of knowledge of how things work. Thinking, then, gradually evolves to ever more complex levels, represented by each of the stages.

It is important to keep in mind that Piaget’s perspective on cognition is fundamentally logical and mathematical. Late in his life he observed that he did not wish “to appear only as a child psychologist”:

> My efforts, directed toward the psychogenesis of thought, were for me only a link between two dominant preoccupations: the search for the mechanisms of biological adaptation and the analysis of that higher form of adaptation which is scientific thought, the epistemological interpretation of which has always been my central aim. (in Gruber and Vonèche xi)

With this perspective in mind, let us very briefly consider the stages of Piaget’s theory that are appropriated to discussions of college-age remedial writers.
Concrete Operational (6-7 to 11-12 years). The cognitive milestone here is that children are freed from immediate perception and enter the realm of logical—if concrete—operations. They can use logic to solve everyday problems, can take other points of view, can simultaneously take into account more than one perspective. In many ways, though, the child's reasoning is still linked to the environment, to tasks that are concrete and well-specified: "Tasks that demand very abstract reasoning, long chains of deduction, or the recognition that the available evidence is insufficient to reach any conclusion are thought to be beyond the reach" of children at the concrete operational stage (Siegler 89). Children have trouble separating out and recombining variables, performing sophisticated conservation tasks, and solving proportionality problems. They also have trouble planning systematic experiments and understanding "purely hypothetical questions that are completely divorced from anything in their experience" (Siegler 90).

Formal Operational (11-15 years). During this stage, children develop into sophisticated logical thinkers—Piaget compared them to scientists—and can solve problems that throw concrete-operational children: like the pendulum task described below. Flavell summarizes the ability of the formal-operational child this way: "His thinking is hypothetico-deductive rather than empirico-inductive, because he creates hypotheses and then deduces the empirical states of affairs that should occur if his hypotheses are correct . . . The older individual's thinking can . . . be totally abstract, totally formal-logical in nature." (145. For a critical discussion of the notion of stages, see Brainerd.)

Piaget and his colleagues developed a number of tasks to distinguish concrete from formal operational thinking. The pendulum task is representative:

Children observed strings with metal balls at their ends swinging from a metal frame. The strings varied in length and the metal balls varied in how much they weighed; the task was to identify the factor or combination of factors that determined the pendulum's period. Plausible hypotheses included the weight of the metal balls, the length of the strings, the height from which the strings were dropped, and the force with which they were pushed. Although the length of the string is in fact the only relevant factor . . . 10- and 11-year olds almost always concluded that the metal ball's weight played a key role, either as the sole determining factor or in combination with the string's length. Thus the children failed to disentangle the influence of the different variables to determine which one caused the effect. (Siegler 89-90)

In the 1970's a number of studies appeared reporting that up to 50% of American college freshmen could not solve formal-operational problems like the pendulum task. The conclusion was that an alarming number of our 18-year-olds were locked at the level of concrete operations, a stage Piaget contends they should have begun evolving beyond by early and certainly by mid-
adolescence. These data quickly found their way to a more general readership, and some people in composition understandably saw relevance in them and began to use them to explain the problems with the writing of remedial students. With support of the data, they wrote that up to 50% of college freshmen were locked into the level of the concrete, couldn’t think abstractly, couldn’t produce logical propositions, couldn’t conceptualize—and, borrowing further from Piagetian terminology, they speculated that these students couldn’t decenter, couldn’t take another’s point of view, were cognitively egocentric. The last two stages of the Piagetian framework became in application a kind of cognitive dichotomy unto themselves. If students couldn’t produce coherent abstractions in writing, if they wrote about what was in front of them and couldn’t express themselves on the conceptual level, if they described something in writing as though their reader shared their knowledge of it—then those limits in written expression suggested something broad and general about the state of their thinking: they might be unable to form abstractions . . . any abstractions; they couldn’t decenter . . . at all. There are problems with this line of reasoning, however, and they have to do with the application of the framework as well as with the framework itself.

As any developmental psychologist will point out, there are major conceptual problems involved in applying a developmental model to adults. Piaget’s theory was derived from the close observation of infants, children, and early-to mid-adolescents; it was intended as a description of the way thinking evolves in the growing human being. Applying it to college-age students and, particularly, to adult learners is to generalize it to a population other than the one that yielded it. There are more specific problems to consider as well, and they have to do with testing.

It is important to underscore the fact that Piaget implies broad limitations in cognition from specific inadequacies on a circumscribed set of tasks. This is not an unreasonable induction—all sorts of general theories are built on the performance of specific tasks—but it must be pointed out that we are dealing with an inference of major consequence. As developmental psychologist Rochelle Gelman put it: “The child is said to lack cognitive principles of broad significance simply because he fails a particular task involving those principles” (326). It is, then, an inferential leap of some magnitude to say that because college students fail to separate out variables and formally test hypotheses in a few tasks typical of the physics lab, they cannot conceptualize or abstract or tease out variables in any other sphere of their lives. Piaget himself said as much in one of his late articles:

In our investigation of formal structures we used rather specific types of experimental situations which were of a physical and logical-mathematical nature because these seemed to be understood by the school children we sampled. However, it is possible to question whether these situations are, fundamentally, very general and therefore applicable to any school or professional environment . . . It is highly likely that [people like appren-
Piaget's tests are clever and complex. To assist in replication, Piaget and his colleagues provided explicit instructions on how to set up the tests, what to say, and how to assess performance. This clarity contributed to the welter of Piagetian studies conducted over the years, many of which supported the theory. A significant body of recent research, however, has raised serious questions about the social conditions created when these tests are given. Most of this research has been done with younger children, and probably the best summary of it is Donaldson's. The thrust of this work is contained in one of Donaldson's chapter titles; when a child performs poorly on a Piagetian task, is it because of a "failure to reason or a failure to understand"? The tasks might be unfamiliar; the child might misunderstand the instructions; because psychological experiments are new to her, she might confuse the experimenter's intentions and "not see the experiment as the experimenter hopes [she] will" (Gelman 324). (See also philosopher Jonathan Adler's Grician critique of Piagetian testing.) What psychologists like Donaldson have done is keep the formal requirements of Piagetian tasks but change the particular elements to make them more familiar (e.g., substituting a toy policeman and a wall for a doll and a mountain), provide a chance for children to get familiar with the tasks, and rephrase instructions to make sure children understand what is being asked. Children in these conditions end up performing remarkably better on the tasks; significantly higher percentages of them can, for example, adopt other points of view, conserve quantity and number, and so on. What limited some children on Piaget's tasks, then, seems to be more related to experimental conditions rather than some absolute restriction in their ability to reason.

A somewhat related set of findings has do with training—one of the more controversial issues in Piagetian theory. This is not the place to recap the controversy; suffice it to say that a large number of studies has demonstrated that brief training sessions can have dramatic results on performance. One such study has direct bearing on our discussion. Kuhn, Ho, and Adams provided training to college freshman who failed at formal-operational tasks. After training, the students were once again presented with the tests, and "most of the college subjects showed immediate and substantial formal reasoning." The authors go on to speculate that the absence of formal-operational performance "may to a large extent reflect cognitive processing difficulties in dealing with the problem formats, rather than absence of underlying reasoning competencies" (1128).

I will conclude this brief critique by considering, once again, the mathematico-logical base of Piaget's theory. There is a tradition in the 20th century West—shaped by Russell, Whitehead, Carnap, and others—to study
human reasoning within the framework of formal, mathematical logic, to see logic not only as a powerful tool, but as a representation of how people actually reason—at least when they’re reasoning effectively. This tradition had a strong influence on Piaget’s theory. In Toulmin’s words, Piaget’s “overall intellectual goal” was to:

discover how growing children “come to recognize the necessity of” conforming to the intellectual structures of logic, Euclidean geometry, and the other basic Kantian forms. (256)

And as Inhelder and Piaget themselves said: “[R]easoning is nothing more than the propositional calculus itself” (305).

Mathematical logic is so privileged that we tend to forget that this assumption about logic being isomorphic with reasoning is highly controversial; it lies at the center of a number of current debates in cognitive psychology, artificial intelligence, and philosophy. Here is one of many counterstatements:

Considerations of pure logic . . . may be useful for certain kinds of information under certain circumstances by certain individuals. But logic cannot serve as a valid model of how most individuals solve most problems most of the time. (Gardner, Mind’s New Science 370)

Formal logic essentially strips away all specific connections to human affairs and things of the world; it allows us to represent relations and interactions within a wholly abstract system. Our elevation of this procedure blinds us to the overwhelming degree to which powerful and effective reasoning can be practical, non-formal, and concrete. As psychologist Barbara Rogoff puts it, “thinking is intricately interwoven with the context of the problem to be solved” (2). She continues:

Evidence suggests that our ability to control and orchestrate cognitive skills is not an abstract context-free competence which may be easily transferred across widely diverse problem domains but consists rather of cognitive activity tied specifically to context. (3)

Much problem-solving and, I suspect, the reasoning involved in the production of most kinds of writing rely not only on abstract logical operations, but, as well, on the rich interplay of visual, auditory, and kinesthetic associations, feeling, metaphor, social perception, the matching of mental representations of past experience with new experience, and so on. And writing, as the whole span of rhetorical theory makes clear, is deeply embedded in the particulars of the human situation. It is a context-dependent activity that calls on many abilities. We may well need to engage in formal-logical reasoning when writing certain kinds of scientific or philosophical papers or when analyzing certain kinds of hypotheses and arguments, but we cannot assume that the ability or inability to demonstrate formal-operational thought on one or two Piagetian tasks has a necessary connection to our students’ ability or inability to produce coherent, effective discourse.
Orality-Literacy

Orality-literacy theory draws on the studies of epic poetry by Milman Parry and Albert Lord, the classical-philological investigations of Eric Havelock, the wide-ranging theoretical work of Walter Ong, and, to a lesser degree, on the compelling, though dated, cross-cultural investigations of thought in primitive, non-literate cultures. The work is broad, rich, and diverse—ranging from studies of the structure of the epic line to the classification schemes of unlettered rural farmers—but as it comes to those of us in composition, its focus is on the interrelation of language and cognition. Various scholars say it in various ways, but the essential notion is that the introduction of literacy into a society affects the way the members of the society think. There seem to be strong and weak versions of this theory.

The strong version states that the acquisition of literacy brings with it not only changes in linguistic possibilities—e.g., subordinative and discursive rather than additive and repetitive styles, less reliance on epithets and maxims and other easily remembered expressions—but necessarily results in a wide variety of changes in thinking: only after the advent of literacy do humans possess the ability to engage in abstraction, generalization, systematic thinking, defining, logos rather than mythos, puzzlement over words as words, speculation on the features of language. And these abilities, depending on who you read, lead to even wider changes in culture, summarized, not without exasperation, by social historian Harvey Graff:

> These characteristics include, in typical formulations or listings, attitudes ranging from empathy, innovativeness, achievement orientation, "cosmopolitanism," information and media awareness, national identification, technological acceptance, rationality, and commitment to democracy, to opportunism, linearity of thought and behavior, or urban residence. ("Reflections" 307)

The operative verb here is "transformed." Writing transforms human cognition.

The weak version of the oral-literate construct acknowledges the role literacy plays in developing modes of inquiry, building knowledge, etc., but tends to rely on verbs like "facilitate," "favor," "enable," "extend"—the potential of human cognition is extended more than transformed. Here's Jack Goody, an anthropologist who is often lumped in with those holding to the "strong version," but who, at least in his late work, takes issue with the oral-literate dichotomy. In discussing various differences between literate and oral expression, for example, he warns that such differences "do not relate primarily to differences of 'thought' or 'mind' (though there are consequences for these) but to differences in the nature of communicative acts" (26). So though Goody grants that writing "made it possible to scrutinize discourse in a different kind of way" and "increased the potentiality for cumulative knowledge" and freed
participants from "the problem of memory storage" dominating "intellectual life," (37), he also insists that:

Even in non-literate societies there is no evidence that individuals were prisoners of pre-ordained schemes, of primitive classifications, of the structures of myth. Constrained, yes; imprisoned, no. Certain, at least, among them could and did use language in a generative way, elaborating metaphor, inventing songs and "myths", creating gods, looking for new solutions to recurring puzzles and problems, changing the conceptual universe. (33)

The theory is a sensible one: literacy must bring with it tremendous repercussions for the intellect. The problem is that when the theory, particularly the strong version, is applied to composition studies, it yields some troubling consequences. Late twentieth-century American inner-city adolescents and adults are thought to bear cognitive resemblance to (ethnocentric notions of) primitive tribesmen in remote third-world cultures (or these adolescents and adults think like children, and children think like primitives): they don't practice analytic thinking; they are embedded in the context of their lives and cannot analyze it; they see things only as wholes; they think that printed words are concrete things; they cannot think abstractly.

A little reflection on this application of orality-literacy theory—given its origins—reveals a serious problem of method. The theory emerges from anthropological work with primitive populations, from historical-philological study of Homeric texts, from folkloric investigations of non-literate taletellers, and from brilliant, though speculative, literary-theoretical reflection on what might have happened to the human mind as it appropriated the alphabet. It is, then, a tremendous conceptual leap to apply this theory to urban-industrial Americans entering school in the penultimate decade of the twentieth century. We have here a problem of generalizability.

Now one could admit these problems yet still see some analogic value in applying the oral-literate construct with a hedge—for it at least, as opposed to the other theories we've been exploring, is directly concerned with written language. Fair enough. Yet my reading has led me to doubt the strength and utility of the theory on its own terms. (My concern rests primarily with the strong version. The weak version makes less dramatic claims about cognition, though some of what I found would qualify weak versions as well.) There are problems with what the theory implies about the way written language emerges in society and the role it plays in determining how people lead their linguistic lives and conduct their cognitive affairs. This is not to deny the profound effects literacy can have on society; it is to question the strength of the orality-literacy construct in characterizing those effects. Let me briefly survey some of the difficulties.

*Literacy and Society.* The historical record suggests that the technology and conventions of literacy work their way slowly through a society and have gradual—and not necessarily linearly progressive—influence on commerce, pol-
itics, bureaucracy, law, religion, education, the arts. (See, e.g., Marrou; Clanchy; Cressy.) Furthermore, it is hard to maintain, as the strong version does, that literacy is the primum mobile in social-cultural change. What emerges, instead, is a complex interaction of economic, political, and religious forces of which literacy is a part—and not necessarily the strongest element. Though there is no doubt that literacy shapes the way commerce, government, and religion are conducted, it, as John Oxenham puts it: “would have followed, not preceded, the formation of certain kinds of society” (59). And Harvey Graff, pointing out all the “discontinuities” and “contradictions” in linear, evolutionary assumptions about the spread of literacy, emphasizes that “[n]either writing [n]or printing alone is an ‘agent of change’; their impacts are determined by the manner in which human agency exploits them in a specific setting” (Reflections” 307).4

Another way to view the problems with the transformational claims about literacy is to consider the fact that a number of societies have appropriated literacy to traditional, conservative purposes. In such societies literacy did not trigger various cultural-cognitive changes—changes in mores, attitudes, etc.—but reinforced patterns already in place. Again, John Oxenham:

We have always to bear in mind that there have been literate social groups, who so far from being inventive and trusting, have been content merely to copy their ancient scriptures and pass them on virtually unaltered. It may be, then, that literate people can respond more readily to leadership for change in culture, technology, social mores, but that literacy by itself does not induce appetites for change, improvement or exploration. (52)

There are a number of illustrations of this; one specific case-study is provided by Kenneth Lockridge, whose inquiry into the social context of literacy in Colonial New England leads him to conclude:

[T]here is no evidence that literacy ever entailed new attitudes among men, even in the decades when male literacy was spreading rapidly toward universality, and there is positive evidence that the world view of literate New Englanders remained as traditional as that of their illiterate neighbors. (4)

It is even difficult to demonstrate causal links between reading and writing and changes in the economic sphere—an area that “modernization theorists” generally thought to be particularly sensitive to gains in literacy. Harvey Graff’s study of social mobility in three mid-19th century towns revealed that “systematic patterns of inequality and stratification . . . were deep and pervasive and relatively unaltered by the influence of literacy.” He continues:

Class, ethnicity, and sex were the major barriers of social inequality. The majority of Irish Catholic adults, for example, were literate . . . but they stood lowest in wealth and occupation, as did laborers and servants. Women and blacks fared little better, regardless of literacy . . . social
realities contradicted the promoted promises of literacy. (The Literacy Myth 320-21)

Similar assertions are made closer to home by Carman St. John Hunter and David Harmon, whose overview of the research on contemporary adult illiteracy leads to this conclusion:

For most persons who lack literacy skills, illiteracy is simply one factor interacting with many others—class, race and sex discrimination, welfare dependency, unemployment, poor housing, and a general sense of powerlessness. The acquisition of reading and writing skills would eliminate conventional illiteracy among many but would have no appreciable effect on the other factors that perpetuate the poverty of their lives. (9-12. See also Ogbu.)

The oral-literate distinction can help us see differences in the communicative technologies available to the members of a society, to get a sense of formats, means, and forums through which communication occurs (Enos and Ackerman). But it appears to be historically, culturally, and economically reductive—and politically naive—to view literacy as embodying an automatic transformational power. What is called for is a contextual view of literacy: the ability to read or to write is a technology or a method or a behavior, a set of conventions that interact in complex ways with a variety of social forces to shape society and culture. It is, to use Harvey Graff’s phrasing, a “myth” to assume that literacy necessarily sparks social change.

Literacy and Cognition. Let us move now from the social-cultural realm to some of the claims made about cognition. These come from two highly diverse sources: classical philological studies of epic poetry and anthropological studies of thought and language. There are problems with both.

The key work in the classicist vein is Eric Havelock’s investigation of Greek culture before and after the advent of the alphabet. In books ranging from Preface to Plato (published in 1963) to The Muse Learns to Write (1986) Havelock has made the strong claim that pre-alphabetic Greeks, ingenious as they were, were barred from philosophical thought because oral discourse could not generate abstract, propositional language or self-conscious reflection on language as language. To be sure, there are times when Havelock’s claims are less extreme, but even in The Muse Learns to Write, a tempered book, one finds questions and statements like these: “May not all logical thinking as commonly understood be a product of Greek alphabetic literacy?” (39) and “it is only as language is written down that it becomes possible to think about it” (112). And such theorizing quickly leads to a troublesome alphabetic determinism.

Havelock’s work is compelling, but we must remember that when it comes to cognition, he is operating very much in the realm of speculation. That is, he infers things about cognitive processes and the limits of reasoning ability
from the study of ancient texts, some of which represent genres that one would not expect to give rise to philosophic inquiry. Furthermore, even if we accepted his method, we could find powerful counterstatements to his thesis—and some of these are contained in a festschrift issued by the Monist Press. Examining the same texts from which Havelock built his case, University of Chicago classicist Arthur W. H. Adkins provides evidence of abstraction, verbal self-consciousness, and the linguistic resources to engage in systematic thinking. He concludes that:

Havelock has not as yet demonstrated any necessary link between literacy and abstract thought . . . he has not as yet demonstrated that in fact the stimulus to abstract thought in early Greece was the invention of writing; [and] some features denied by Havelock to be available in oral speech are found in the Homeric poems. (220. See also Margolis.)

The other line of argument about literacy and cognition comes from twentieth-century anthropological studies of the reasoning of rural farmers and primitive tribesmen. These studies tend not to be of literacy-orality per se, but are appropriated by some orality-literacy theorists. A good deal of this cross-cultural research has involved classification tasks: a set of objects (or a set of pictures of the objects) is given to a tribesman, and the investigator asks the tribesman to group the objects/pictures. The key issue is the scheme by which the tribesman completes the grouping: does he, for example, place a hoe with a potato and offer the concrete reason that they go together because you need one to get the other, or does he place the hoe with a knife because he reasons abstractly that they are both tools? The Western anthropologist considers concrete reasoning to be less advanced than abstract reasoning, and orality-literacy theorists like to pose literacy as the crucial variable fostering abstract reasoning. It is because the tribesman lacks letters that he is locked into the concrete. This is an appealing conjecture, but, as I hope the previous discussion suggests, literacy is too intertwined with schooling and urbanization, with economics, politics, and religion to be able to isolate it and make such a claim. There are other problems too, not just with the causal linking of literacy and abstraction, but with traditional comparative research itself. Cole and Means put it this way:

[D]epartures from the typical performance patterns of American adults are not necessarily deficits, but may indeed be excellent adaptions to the life circumstances of the people involved . . . Which type of classification is preferable will depend upon the context, that is, the number of different types of objects to be grouped and the way in which the materials are going to be used . . . preference for one type of grouping over another is really no more than that—just a matter of preference. (161-62)

In line with the above, it must be kept in mind that because "primitive" subjects tend to classify objects in ways we label concrete does not necessarily mean that they can think in no other way. Consider, as we close this section, a
wonderful anecdote from anthropologist Joseph Glick, as retold by Jacqueline Goodenow:

The investigators had gathered a set of 20 objects, 5 each from 4 categories: food, clothing, tools, and cooking utensils... [W]hen asked to put together the objects that belonged together, [many of the tribesmen produced] not 4 groups of 5 but 10 groups of 2. Moreover, the type of grouping and the type of reason given were frequently of the type we regard as extremely concrete, e.g., "the knife goes with the orange because it cuts it." Glick... notes, however, that subjects at times volunteered "that a wise man would do things in the way this was done." When an exasperated experimenter asked finally, 'How would a fool do it?' he was given back groupings of the type... initially expected—four neat piles with foods in one, tools in another." (170-71. For fuller cross-cultural discussions of concrete vs. abstract reasoning see Ginsburg; Lave; and Tulkin and Konner.)

**Literacy and Language.** It is problematic, then, to claim that literacy necessarily causes a transformation of culture, society, or mind or that societies without high levels of literacy are barred from the mental activities that some theorists have come to associate with literacy: verbal self-consciousness, abstraction, etc. Perhaps, though, the orality-literacy construct does have value if one strips away the cultural-cognitive baggage; its real benefit might be its ability to help us understand the nature of the language experiences students received in their homes and communities and further help to distinguish between the oral and literate features in their writing. But even here there are problems, for the reality of speaking-writing relationships seems to be more complex than the oral-literate distinction suggests.

Certainly, there are bioanatomical and perceptual differences between speech and writing—differences in the way each is acquired, produced, and comprehended. And if you examine very different types of language (e.g., dinner-table conversation vs. academic prose), you will find significant grammatical and stylistic differences as well. (See, for example, Chafe.) But the oral-literate construct leads us to focus attention too narrowly on the channel, the mode of communication, in a way that can (a) imply a distinctive uniformity to oral modes vs. written modes and (b) downplay the complex interaction among human motive, language production, and social setting. Linguists currently working with oral narratives and written texts suggest that the notion of an oral narrative itself is problematic, for oral traditions can differ in major ways (Scollon and Scollon); that the narrative variations we see may have less to do with literateness than with cultural predispositions (Tannen, "A Comparative Analysis"); that features often defined as literate are frequently found in oral discourse and vice versa (Polanyi; Tannen, "Relative Focus"); that characteristics identified by some as a mark of preliterate discourse—e.g., formulaic expressions—are woven throughout the language of literate people (Fillmore); that while spoken sentences can be shown to differ
from written sentences, they are not necessarily less complex grammatically (Halliday); and so on. Finally, it seems that many of the differences we can find between stretches of speech and writing might, as Karen Beamon suggests, depend on factors such as genre, context, register, topic, level of formality, and purpose as much as whether the passage is spoken or written.

These closer examinations of a wide variety of texts and utterances should make us wary of neat, bipolar characterizations—whether dichotomies or simple continua—of oral vs. written language. And it seems to me that this caution about the linguistic reality of the oral-literate distinction could lead to reservations about its contemporary social reality—that is, can we accurately and sensitively define, in late twentieth-century America, entire communities and subcultures as being oral and others as being literate? By what criteria, finally, will we be able to make such a distinction? In asking these questions, I am not trying to downplay the obvious: children enter school with widely different degrees of exposure to literacy activities and with significantly different experiences as to how those activities are woven into their lives. And these differences clearly have consequences for schooling.

What I do want to raise, though, is the possibility that the oral-literate continuum does not adequately characterize these differences. The continuum, because it moves primarily along the single dimension of speech-print, slights history and politics—remember, it weights literacy as the primary force in cognitive development and social change—and it encourages, because of its bipolarity, a dichotomizing of modes where complex interweaving seems to exist. Finally, the orality-literacy construct tends to reduce the very social-linguistic richness it is meant to describe. Here is Shirley Brice Heath on the language behaviors of two working-class communities in the Carolinas:

The residents of each community are able to read printed and written materials in their daily lives and, on occasion, they produce written messages as part of the total pattern of communication in the community. In both communities, the residents turn from spoken to written uses of language and vice versa as the occasion demands, and the two modes of expression serve to supplement and reinforce each other. Yet, in terms of the usual distinctions made between oral and literate traditions, neither community may be simply classified as either “oral” or “literate.” (Ways with Words 203)

Work like Heath’s challenges the sociological and linguistic utility of the orality-literacy construct; in fact, elsewhere Heath directly criticizes “current tendencies to classify communities as being at one or another point along a hypothetical [oral-literate] continuum which has no societal reality” (“Protean Shapes” 116).

What is most troubling on this score is the way the orality-literacy construct is sometimes used to represent language use in the urban ghetto. What emerges is a stereotypic characterization of linguistic homogeneity—all the residents learn from the sermon but not the newspaper; they run the dozens
Narrowing the Mind and Page: Remedial Writers and Cognitive Reductionism

but are ignorant of print. The literacy backgrounds of people who end up in remedial, developmental, or adult education classes are more complex than that: they represent varying degrees of distance from or involvement with printed material, various attitudes toward it and skill with it, various degrees of embrace or complicated rejection of traditions connected with their speech. Important here is what Mina Shaughnessy and Glynda Hull so carefully demonstrate: some of the most vexing problems writing teachers face are rooted in the past attempts of educationally marginalized people to make sense of the uses of print. Print is splattered across the inner city, and, in effective and ineffective ways, people incorporate it into their lives.

There is a related problem. Some theorists link Piagetian notions of cognitive egocentrism with generalizations about orality and conclude that without the language of high literacy, people will be limited in their ability to “decenter,” to recognize the need to “decontextualize” what they are communicating, to perceive and respond to the social and informational needs of the other. Certainly, people with poor educations will have a great deal of trouble doing such things in writing, but one must be very cautious about leaping from stunted and limited texts to inferences about deficits in social cognition or linguistic flexibility. Developmentally and sociologically oriented linguists have demonstrated for some time that human beings are not locked into one way of speaking, one register, and develop, at quite a young age, the recognition that different settings call for different kinds of speech (Hudson). Poor writers are not as a population cognitively egocentric; they are aware of the other, of “audience”—some disenfranchised people acutely so. What they lack are the opportunities to develop both oral and written communicative facility in a range of settings. Or they may resist developing that facility out of anger or fear or as an act of identity. They may prefer one way of speaking, most of us do, and thus haven’t developed a fluency of voices. But rather than being cognitively locked out of other registers, other linguistic roles, other points of view, they are more likely emotionally and politically barred from them.

It is obvious that literacy enables us to do a great deal. It provides a powerful solution to what Walter Ong calls “the problem of retaining and retrieving carefully articulated thought” (34). It enables us to record discourse, scan and scrutinize it, store it—and this has an effect on the way we educate, do business, and run the courts. And as we further pursue intellectual work, reading and writing become integral parts of inquiry, enable us to push certain kinds of analysis to very sophisticated levels. In fact, as investigations of academic and research settings like Latour and Woolgar’s Laboratory Life suggest, it becomes virtually impossible to tease writing and reading out of the conduct and progress of Western humanistic or scientific inquiry. One of the values of the orality-literacy construct is that it makes us aware of how central literacy is to such inquiry. But, finally, the bipolarity of the construct (as with the others we’ve examined) urges a way of thinking about language, social change, and cognition that easily becomes dichotomous and reductive. “The tyranny of
conceptual dichotomies," Graff calls it ("Reflections" 313). If writing is thought to possess a given characteristic—say, decontextualization or abstraction—then the dichotomy requires you to place the opposite characteristic—contextualization, concreteness—in the non-writing category (cf. Elbow). We end up splitting cognition along linguistic separations that exist more in theory than in social practice.

Conclusion

Witkin uncovered interesting perceptual differences and led us toward a deeper consideration of the interrelations of personality, problem solving, and social cognition. Hemisphericity theorists call our attention to the neurological substrate of information processing and language production. Piaget developed an insightful, non-behaviorist method to study cognitive growth and, more comprehensively than anyone in our time, attempted to articulate the changes in reasoning we see as children develop. And the orality-literacy theorists give us compelling reflection on spoken and written language and encourage us to consider the potential relations between modes of communication and modes of thought. My intention in this essay is not to dismiss these thinkers and theories but to present the difficulties in applying to remedial writers these models of mind. For there is a tendency to accept as fact condensed deductions from them—statements stripped away from the questions, contradictions, and complexities that are central to them. Let me summarize the problems I see with the theories we've been considering.

First, the theories end up levelling rather than elaborating individual differences in cognition. At best, people are placed along slots on a single continuum; at worst they are split into mutually exclusive camps—with one camp clearly having cognitive and social privilege over the other. The complexity of cognition—its astounding glides and its blunderous missteps as well—is narrowed, and the rich variability that exists in any social setting is ignored or reduced. This reductive labelling is going on in composition studies at a time when cognitive researchers in developmental and educational psychology, artificial intelligence, and philosophy are posing more elaborate and domain-specific models of cognition.

Second, and in line with the above, the four theories encourage a drift away from careful, rigorous focus on student writing and on the cognitive processes that seem directly related to it, that reveal themselves as students compose. That is, field dependence-independence, hemisphericity, etc., lead us from a close investigation of the production of written discourse and toward general, wide-ranging processes whose link to writing has, for the most part, been assumed rather than demonstrated. Even orality-literacy theory, which certainly concerns language, urges an antagonism between speech and writing that carries with it sweeping judgments about cognition.
The theories also avert or narrow our gaze from the immediate social and linguistic conditions in which the student composes: the rich interplay of purpose, genre, register, textual convention, and institutional expectation (Bartholomae; Bizzell; McCormick). When this textual-institutional context is addressed, it is usually in simplified terms: the faculty—and their discourse—are literate, left-hemispheric, field-independent, etc., and underprepared students are oral, right-hemispheric, and field dependent. I hope my critical surveys have demonstrated the conceptual limits of such labelling.

Third, the theories inadvertently reflect cultural stereotypes that should, themselves, be the subject of our investigation. At least since Plato, we in the West have separated heart from head, and in one powerful manifestation of that split we contrast rational thought with emotional sensibility, intellectual acuity with social awareness—and we often link the analytical vs. holistic opposition to these polarities. (I tried to reveal the confusion inherent in such talk when discussing cognitive style and hemisphericity.) These notions are further influenced by and play into other societal notions about independence and individuality vs. communal and tribal orientations and they domino quickly toward stereotypes about race, class, and gender.

Let me say now that I am not claiming that the research in cognitive style or hemisphericity or any of the other work we surveyed is of necessity racist, sexist, or elitist. The conclusions that can be drawn from the work, however, mesh with—and could have been subtly influenced by-cultural biases that are troubling. This is an important and, I realize, sensitive point. Some assert that student writers coming from particular communities can’t reason logically or analytically, that the perceptual processes of these students are more dependent on context than the processes of white, middle-class students, that particular racial or social groups are right-hemispheric, that the student writers we teach from these groups are cognitively egocentric.

A number of recent books have amply demonstrated the way 19th and early 20th century scientific, social scientific, and humanistic assessments of mental capacity and orientation were shaped by that era’s racial, gender, and class biases (see, for example, Gilman; Gould; Kamin; and Valenstein). We now find these assessments repellent, but it’s important to remember that while some were made by reactionary social propagandists, a number were made as well by thinkers operating with what they saw as rigorous method—and some of those thinkers espoused a liberal social philosophy. This is a powerful illustration of the hidden influences of culture on allegedly objective investigations of mind. We all try to make sense of problematic performance—that’s part of a teacher’s or a researcher’s job—but we must ask ourselves if speculation about cognitive egocentrism and concrete thinking and holistic perception embodies unexamined cultural biases about difference—biases that would be revealed to us if we could adopt other historical and social perspectives.

These summary statements have a number of implications for research.

The leap to theory is a privileged move—it is revered in the academy and
allows parsimonious interpretations of the baffling variability of behavior. But a theory, any theory, is no more than a best guess at a given time, simultaneously evocative and flawed. Especially when it comes to judging cognition, we need to be particularly aware of these flaws and limitations, for in our culture judgments about mind carry great weight. A good deal of careful, basic descriptive and definitional work must be done before we embrace a theory, regardless of how compelling it is.

A series of fundamental questions should precede the application of theory: Is the theory formulated in a way that allows application to writing; that is, can it be defined in terms of discourse? Given what we know about writing, how would the theory be expected to manifest itself—i.e., what would it mean textually and dynamically for someone to be a field-dependent writer? What will the theory allow us to explain about writing that we haven’t explained before? What will it allow us to do pedagogically that we weren’t able to do as well before? Will the theory strip and narrow experience and cognition, or does it promise to open up the histories of students’ involvement with writing, their rules, strategies, and assumptions, the invitations and denials that characterized their encounters with print?

Beyond such general questions are more specific guidelines for those of us doing psychological research. Once we undertake an investigation of cognition we must be careful to discuss our findings in terms of the kinds of writing we investigate. Generalizing to other tasks, and particularly to broad cognitive processes, is not warranted without evidence from those other domains. If theories like the four we discussed, but others too (e.g., theories of moral development, social cognition, metacognition, etc.), are appropriated that are built on particular tests, then researchers must thoroughly familiarize themselves with the tests beneath the theories and consult with psychologists who use them. People who are going to administer such tests should take the tests themselves—see what they’re like from the inside. My mentor Richard Shavelson also urges researchers to administer the tests to individual students and have them talk about what they’re doing, get some sense of how students might interpret or misinterpret the instructions, the various ways they represent the task to themselves, what cognitive processes seem to come into play as the students work with the tests. Furthermore, it must be remembered that the results of testing will be influenced by the degree of familiarity the students have with the tests and by the social situation created in the administration of them. How will these conditions be adjusted for and acknowledged? Finally, the resulting data must be discussed as being specific to the students tested. Generalizing to others must be done with caution.

A special word needs to be said here about comparative studies. If we employ hi-lo designs, expert-novice studies, and the like—which can be powerfully revealing designs—we need to consider our design and our results from historical and sociopolitical perspectives as well as cognitive ones. That is, if class, gender, or race differences emerge—and they certainly could—they
should not automatically be assumed to reflect “pure” cognitive differences, but rather effects that might well be conditioned by and interpreted in light of historical, socio-political realities. There is currently a lot of talk about the prospect of forging a social-cognitive orientation to composition research (see, for example, Freedman, Dyson, Flower, and Chafe; Bizzell and Herzberg). One of the exciting results of such an endeavor could be an increased sensitivity to the social forces that shape cognitive activity. I’ve argued elsewhere for a research framework that intersects the cognitive, affective, and situational dimensions of composing and that involves the systematic combination of multiple methods, particularly ones traditionally thought to be antagonistic. My assumption is that the careful integration of, say, cognitive process-tracing and naturalistic observation methods can both contribute to fresh and generative insight and provide a guard against reductive interpretation (Rose, “Complexity”).

Much of this essay has concerned researchers and theoreticians, but at the heart of the discussion is a basic question for any of us working with poor writers: How do we go about judging the thought processes involved with reading and writing when performance is problematic, ineffective, or stunted? If I could compress this essay’s investigation down to a single conceptual touchstone, it would be this: Human cognition—even at its most stymied, bungled moments—is rich and varied. It is against this assumption that we should test our theories and research methods and classroom assessments. Do our practices work against classification that encourages single, monolithic explanations of cognitive activity? Do they honor the complexity of interpretive efforts even when those efforts fall short of some desired goal? Do they foster investigation of interaction and protext manifestation rather than investigation of absence: abstraction is absent, consciousness of print is absent, logic is absent? Do they urge reflection on the cultural biases that might be shaping them? We must be vigilant that the systems of intellect we develop or adapt do not ground our students’ difficulties in sweeping, essentially one-dimensional perceptual, neurophysiological, psychological, or linguistic processes, systems that drive broad cognitive wedges between those who do well in our schools and those who don’t.5

Notes


2. For a description of the other tests—the Body Adjustment Test and the rarely used auditory and tactile embedded figures tests—see Witkin et al.

3. Witkin later revised his theory, suggesting that the rod and frame test and the embedded figures test were tapping different dimensions of the field dependence-independence construct. This revision, however, gives rise to further problems—see Linn and Kylonen.

4. Educators and evaluators often seem locked into a 19th century linear progress conception of the way both societies and individuals appropriate literacy. Graff presents a provocative historical challenge to such notions; here's Vygotsky on individual development: "together with processes of development, forward motion, and appearance of new forms, we can discern processes of curtailment, disappearance, and reverse development of old forms at each step . . . only a naive view of development as a purely evolutionary process . . . can conceal from us the true nature of these processes" (106).

5. Particular sections of this paper were discussed with or reviewed by specialists who provided a great deal of expert help: Susan Curtiss (neurolinguistics), Richard Leo Enos (classical studies), Sari Gilman (research psychiatry), John R. Hayes, Richard Shavelson, and Catherine Stasz (cognitive and educational psychology), Thomas Huckin (linguistics), Robert Siegler (developmental psychology). David Bartholomae, Linda Flower, Glynda Hull, David Kaufer, and Stephen Witte commented generously on the entire manuscript. The project benefited as well from rich conversation with Mariolina Salvatori and Kathryn Flannery. Versions of the paper were read at Carnegie Mellon, Pitt, Indiana University of Pennsylvania, UCLA, Berkeley, CCCC (Atlanta), Penn State, and UCSD. My thanks for all the ideas generated at those conferences and colloquia. Finally, appreciation is due to Sally Magargee for her research assistance and the Carnegie Mellon Department of English and the Spencer Foundation for their support.

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