1. The Boundedness, Autonomy, and Purity of Disciplines

For our purposes, a discipline is "bounded" by its procedure for adjudicating knowledge claims. This procedure consists of an *argumentation format* that restricts (i) word usage, (ii) borrowings permitted from other disciplines, and (iii) appropriate contexts of justification/discovery (for example, some claims may be grounded on "reason alone," some on unaided perception, some on technically aided perception). A discipline that is fully bounded is *autonomous*: it controls its own academic department, program of research, historical lineage, and so forth. T.S. Eliot (1948) coined the term *autotelic* to express a rather strong sense of autonomy, namely, when a discipline not only controls its own affairs but sees those affairs as worth pursuing for their own sake.

Although Eliot was specifically interested in distinguishing art from art criticism (only art is autotelic), his concept applies equally to the humanities and the sciences. For example, we may distinguish degrees of disciplinary purity according to how one understands the idea of a discipline being practiced "for its own sake" or "an end in itself":

(a) *Holy of Holies*: The discipline's very performance of its practice is self-justifying without having recourse to either internal or external forms of justification.

Here are some examples of each from the natural sciences:

(a1) It is enough of a reason for doing experimental science that it is designed to get at the truth; hence, it needs no further justification, say, in terms of its technological byproducts.

(b1) The very doing of science—the experience of experimenting and calculating—is its own justification. Polanyi (1957) describes this sort of scientist, who treats his work aesthetically and hence does not care whether he ultimately gets the right results.
Now, here are some analogous examples from literary criticism:

(a2) It is enough of a reason for doing criticism that it is designed to identify good works of art, regardless of whether the criticism has any practical impact in changing art or even tastes.

(b2) The very doing of criticism—the experience of creatively misreading texts and constructing conceits and puns that only the erudite can decipher—is its own justification. This attitude is associated with the more "playful" followers of Derrida (1976).

Matthew Arnold (1972) is to be credited with the insight that disciplines can remain bounded without being fully autotelic. Arnold justified the pursuit of criticism as necessary for a proper appreciation of art. In the nineteenth century and earlier, art was generally seen as a source of highly refined forms of sensory gratification. Thus, Arnold argued that the public had to be taught how to regard art "distinterestedly," or for its own sake. Consequently, criticism occupied the unique position of being the means by which something else, art, can be treated as an end in itself. However, as Davidhazi (1986) has pointed out, criticism has largely failed in its public mission, yet this dysfunctionality has coincided with the rise of criticism as an academic discipline pursued for its own sake. This suggests that by the time it became clear, in the early twentieth century, that criticism had failed on Arnoldian terms, it had accumulated a large enough body of its own literature to justify its continuation. Another case in which dysfunctionality made a discipline more autotelic may well be natural history, which first gained epistemic legitimacy as being the record of divine messages to man. However, by the Scientific Revolution, it was no longer believed that natural history functioned in such a communicative capacity. But by then a body of knowledge had already developed that was worthy of pursuit in its own right (Foucault 1970, ch. 2).

An interesting picture of disciplinary formation is implied by the above account that goes against the more orthodox account offered in Kuhn (1970a). On Kuhn's view, fields of study become disciplines (or "paradigms") once a wide range of previously unrelated phenomena are gathered together under a set of unifying principles, which can be verified through different but converging methods. In addition, once a paradigm is formed, the direction in which research should proceed is clear. Newton's synthesis of terrestrial and celestial mechanics under one set of laws is the typical example. In contrast, the view implied in the preceding account of the history of criticism is that disciplines form not by staking out a clear domain for itself, but rather by successively failing to control some other body of knowledge. We can imagine the successive instrumental failures of criticism—The Retreat to Purity—running as follows (each stage probably corresponds to a position actually taken in the history of criticism; see Hirsch [1976], chs. 7-8):
(c) Criticism aims to prepare the audience to receive great artworks once they are made (and hence prevents bad taste from ever arising).

(d) Failing that (because bad taste arises), criticism aims to change bad taste to good.

(e) Failing that (because it has no real influence), criticism aims simply to identify good and bad taste in artworks.

(f) Failing that (because critics cannot agree on what is good taste), criticism aims to record the history of attempts at identifying good and bad taste, and show how the attempts have neglected to see the essentially contested nature of "good taste." (At this point, critics spend more time talking about each other than about artists.)

(g) Failing that (because critics cannot agree on what their disputes are about), criticism takes on a poetic quality, done primarily because of the experience one gets from doing it.

2. Three Techniques for Detecting Disciplinary Boundaries

First, examine disciplines that adjudicate "ostensibly similar" knowledge claims (Jones 1983, p. 132). For example, philosophers (especially epistemologists) appear to argue about claims that are also contested by linguists, psychologists, sociologists, and sometimes even physicists. Yet philosophy's argumentation format is quite different from the formats of these other disciplines. Failure to note this fact has led to premature reports of philosophy's obsolescence. In honor of a famous recent case (Rorty 1979), we shall dub this oversight The Rorty Fallacy. It is remedied by using a discipline's argumentation format to infer the attitude that one is supposed to adopt to a given proposition (Fuller 1982). Another way of distinguishing disciplines under these circumstances is to determine the background knowledge implicit in the ceteris paribus clause presupposed in a fair test of the claim. For example, many of the social sciences seem to test the same claims, yet have great difficulty in pooling their results, largely because their ceteris paribus clauses contain radically different conceptions of the human being, which are simply presupposed but never directly put to any "crucial experiment." Indeed, as we shall later see, it is only a short step from rival theories that cover roughly the same domain, but presuppose incommensurable ceteris paribus clauses, to entirely discrete disciplines.

Second, examine the metascience implicit in a discipline's argumentation format. When the claims of one discipline conflict with those of another, which discipline yields to the other's cognitive authority? The answer,
which reveals the balance of cognitive power between the two disciplines, should be expected to change over time, as in the case of natural theology vis-a-vis natural philosophy from the Middle Ages to the nineteenth century. When the cognitive resources of one discipline are insufficient to solve one of its own problems, which other discipline "just outside" its boundary is invoked for help? When the validity of claims in one discipline is challenged, the validity of claims in which other disciplines is most threatened? Not only should the answers to these questions be expected to change over time, but they are also likely to be asymmetrical.

For example, classical political economy's model reasoner was originally drawn from the rational egoist psychology then current in the eighteenth century; yet once psychology surrendered the model (in the early twentieth century) economics also did not immediately follow suit. Whereas the model was completely undermined in psychology once the significance of unconscious irrational factors on behavior was appreciated, economists have so far attacked the model largely on practical grounds, especially in terms of the economist's inability to predict real economic behavior (Simon 1976). And while granting that criticism, neoclassical economists still believe that even in practical situations, the only problems with the model are due to "interference" effects. An effort has been made to draw attention to (and perhaps criticize) the fact that economics benefitted from a particular psychological model when it was warranted without suffering the consequences after the model lost its warrant. Thus, Willard (1983, p. 269) has argued that interdisciplinary borrowing should be treated like any other case of borrowing, namely, that it "incurs obligations." Thus, economics should have given up the rational egoist model immediately after psychology did or suffer the consequences.

Third, examine the strategies used to synthesize the research of two or more disciplines. Often a metalanguage is constructed for reducing the claims of the two disciplines to some "common ground" that mainly takes into account the synthesizer's intended audience. If the synthesizer relies on the lexicons of the original disciplines at all, it will be often by metaphorically extending word usage, perhaps so much so that the extensions would be deemed too ambiguous by intradisciplinary standards. And so, unless the synthesis itself spawns a new discipline, it is unlikely to affect day-to-day workings of the original two. There is, however, one "intrinsically synthetic" discipline, pedagogy, whose recent British theorists have come to realize that while disciplinary boundaries may be the "grown-up" solution to the problem of knowledge, the idea of education presupposes that children can be taught the distinctive methods of all the disciplines (Degenhardt 1982). Nevertheless, more typical of the fate of syntheses are the checkered attempts at fusing cognitive psychology and neurophysiology via artificial intelligence into "cognitive science," which is arguably a branch of philosophy (Haugeland 1981, Churchland 1984).

The example of cognitive science also suggests that two disciplines separated by a boundary need not be limited to mutually exclusive domains
of inquiry (Darden & Maull 1977). Instead, they may cross-classify the same general subject matter. We shall follow Jerry Fodor (1981, ch. 5) in calling this phenomenon orthogonality. For example, cognitive psychology and neurophysiology are orthogonal disciplines: reports of a particular type of mental state do not always correspond to reports of the same type of brain state, yet each report of a mental state corresponds to a report of some type of brain state. And so, despite the fact that both disciplines study the thought processes of the human organism, laws interrelating types of mental states do not appear as laws when translated into the neurophysiologist's discourse. This is a key reason why the two disciplines cannot simply "build on" each other's work.

Interestingly, the same situation holds for such incommensurable domains as phlogiston chemistry and oxygen chemistry. In that case, though not all reports of phlogiston correspond to reports of the same substance in oxygen chemistry, each report of phlogiston corresponds to a report of some substance in oxygen chemistry (Kitcher 1978). Indeed, orthogonality simply is incommensurability, but without the connotation that only one of the two disciplines can survive in the long run. But even here the difference may boil down to one of historical perspective: two disciplines that appear merely orthogonal now may later be shown to have been incommensurable. It should come as no surprise, then, a recent school of metapsychologists, eliminative materialists have preempted history by arguing that only the familiarity of cognitive psychology's theoretical discourse (which refers to beliefs, desires, and other intentional entities) keeps it from being replaced by the scientifically more promising neurophysiology (Churchland 1979). If such claims are generally correct, then disciplinary boundaries many be seen as fault lines that conceal future scientific revolutions (Mccauley 1986).

3. Are Disciplinary Boundaries Necessary for the Growth of Knowledge?

Here is a thought experiment to test your intuitions on this issue. Suppose you were given our current corpus of knowledge and were asked to design the most efficient division of cognitive labor that would have produced the corpus. How different would your design be from the disciplinary boundaries drawn in the actual course of history? Start by treating this as a problem in bureaucratic management. You would then want to eliminate task redundancy by having each department of knowledge work on a discrete domain, with the research of several such departments coordinated at a higher level in the organization. Moreover, you would want perfect communication flow, with the work of the lower departments informed by, yet corrective of, the work of the higher ones. If you find this systematic strategy attractive, then you probably think that disciplinary boundaries are in principle dispensable, for, as we have seen, disciplines often cross-classify
the same subject matter and impede any mutually useful synthesis. (Whitley [1986] is an entire theory of disciplines based on this thought experiment.)

However, history tells against the systematic approach. Its ideal hierarchy of domains—social groups that are successively decomposed into multicellular organisms, cells, molecules, atoms, elementary particles—overlooks that many key objects of knowledge have been products of orthogonality, including that emergent entity "man" (contra Oppenheim & Putnam 1958). Indeed, anthropology is not merely a part of primatology, but the linguistic and technical reorganization of all bioevolutionary phenomena. Evidence of this reorganization may be seen in that while man's genetic similarity to apes seems to best explain our intelligence, man's ecological similarity to wolves must be invoked to account for our sociability (Graham 1981, chs. 6-7). Thus, two features traditionally thought by anthropologists to develop concomitantly, cognitive and social structures, appear unrelated from a bioevolutionary standpoint. This point is made more generally by the tendency of sociobiologists to draw on an assortment of species from various branches of the evolutionary tree—ants to apes—in order to model the full range of human phenomena (Rosenberg 1980).

Still, the systematist need not be deterred at this point. He can simply argue that these "products" of orthogonality are really problems that have arisen due to failures of communication and coordination; hence, anthropologists continue to practice their trade only from ignorance of sociobiology. We shall call an extreme version of this response Boundary Berkeleyism, after Bishop George Berkeley, the eighteenth-century radical empiricist. It holds that boundaries arise solely because a discipline's activity is typically not monitored by other disciplines or the public, which allows a hermetic "insider's" discourse to develop. However, once "outsiders" enter this discourse—say, when a biology lab is invaded by sociologists—the boundaries dissolve as the insiders account for their work in terms quite familiar to the outsiders (Latour 1981).

Admittedly, failures to meet the systematist's ideal may be by design, since disciplines actively set up boundaries to expand, protect, and monopolize their cognitive authority (Gieryn 1983b). And even if not by design, there may be more of such failures now than ever before, as the expertise of the average researcher decreases and his need to rely on the unquestioned authority of other experts increases (Friedson 1984). Yet the systematist may generally agree with the eliminative materialist that these sociopolitical obstacles will be overcome in the long run. Indeed, he may facilitate matters by pursuing a policy of reductionism (Neurath 1983), which calls for the construction of, so to speak, an interdisciplinary Esperanto. But on what shall this Esperanto be based? The logical positivists were torn between an authoritarian and a democratic solution: the former solution, physicalism, would have the lower disciplines recast their claims in terms of the cognitive authority delegated to them by the executive discipline, physics; the latter solution, phenomenalism, would
force all the disciplines to recast their claims in terms of a neutral medium of cognitive exchange (as in "sense data"), which the positivists would provide in the form of a theory of evidence.

4. When Disciplines Collide: The Bernard Principle

In either case, would the result look anything like the deep structure of knowledge growth? There is reason to think not (Feyerabend 1981a, ch. 4), which may mean that disciplinary boundaries are, after all, necessary for the growth of knowledge. A strategy that recognizes this possibility is encyclopedic, so named for the very embodiment of cross-classified knowledge, first proposed in 1751 by Diderot and d'Alembert with many of the above concerns in mind (Darnton 1984, ch. 5). In particular, the Encyclopedists held that the orthogonality of disciplinary domains fostered the growth of knowledge by permitting one discipline to problematize the research of another discipline, thereby ensuring that the highest critical standards were maintained by everyone, a situation that did not obtain when theology was "queen of the sciences." In fact, Diderot was so averse to the systematist's ideal that he suspected Newton's mathematical physics of trying to replace theology in the role of cognitive despot (Prigogine & Stengers 1984, ch. 3).

Once a discipline's domain of inquiry has been "staked out" (Cambrosio & Keating 1983), its practitioners must define and maintain the "normal" state of objects in the domain. This involves experimental and textual techniques for foregrounding the problematic claims under study against a background of claims that are stipulated to be unproblematic. The need for a vacuum in which to demonstrate the laws of motion is perhaps the most famous of these normal states. Philosophers have referred to this activity as "filling in the *ceteris paribus* clause," whose recognized function is to protect the claims that are problematized by a discipline from being too easily falsified by extradisciplinary considerations (Lakatos 1970). However, philosophers have generally overlooked the fact that in order to fill such a clause, objects and processes may need to be obscured that might have otherwise been the concern of other disciplines. The term *ontological gerrymandering* (Woolgar & Pawluch 1984) nicely captures this phenomenon.

Consider Claude Bernard's demarcation of experimental medicine from the rest of biology. Bernard defined the normal state of an organism in terms of its *milieu interieur*, the equilibration of the organism's blood and lymph flow. For example, a rise in temperature in a mammal's *milieu exterieur* leads to vasodilation, which allows some heat to escape from its body, thereby restoring normal body temperature. Disease is simply the failure to make such homeostatic adjustments, as judged by a physician upon seeing the organism function in the new *milieu exterieur* (Canguilhem 1978, pp. 29-45). It would seem that Bernard had drawn the boundary around his discipline very tightly—perhaps too tightly. Nineteenth-century France
was slow to accept microbiology and evolutionary biology, and the autonomy of Bernard's discipline may be partly to blame (Mendelsohn 1964). First, Bernard defined disease from the patient's standpoint, organic disequilibrium, rather than from the standpoint of a pathogenic agent. Second, experimental medicine had no conceptual place for a disequilibrium that could not in principle be medically corrected, such as an organism's inability to adapt to a radically new milieu exterieur. Thus, microbes and natural selection, respectively, were disallowed. It is, therefore, fitting that we label the thesis that a disciplinary boundary can be drawn only at the risk of excluding other possible disciplines the Bernard Principle.

The Bernard Principle assumes many forms, which are particularly well illustrated in the various attempts to stake out the foundational discipline of the human sciences. Consider these seven easily overlooked cases:

1. Descartes foreclosed the possibility of a sociology of knowledge when he declared at the start of the Meditations that one must entirely withdraw from daily life in order to consider the nature of things. Virtually all previous philosophers had recommended a restricted institutional setting, such as a school.

2. Classical political economy, in search of a unified theory of value, followed the Newtonian strategy of reducing the different kinds of value in objects to their lowest common denominator, namely, commodity value. As a result, the discipline could not in principle distinguish between the value of human labor and the value of a product of human labor, which implied that political economy had taken the "human" out of the human sciences. Moreover, as Marx saw, the alienation of labor under capitalism threatened to remove all reminders that such a distinction needed to be made (Althusser 1970).

3. John Stuart Mill's A System of Logic was intended to lay the groundwork for the "moral sciences" (R. Brown 1984, ch. 8). That the book did, especially after it was translated into German in 1849 and read by Wilhelm Wundt, who founded experimental psychology as a discipline. Mill's Logic also unintentionally rendered the discipline of history, and later all the humanities, problematic. History, as practiced by Ranke and Niebuhr, claimed to study the past in its specificity. However, Mill's empiricist epistemology did not permit this possibility: either one knows the past in its generality (by induction, the psychologist's way) or one knows the present in its specificity (by direct acquaintance, the ordinary way). Verstehen was Wilhelm Dilthey's attempt at bridging this difficulty. But notice that the Geisteswissenschaften (the German translation of "moral sciences") would not have appeared problematic, had Mill's radical empiricism not been presumed unproblematic by late nineteenth-century German methodologists (Fuller 1983b).
In order to draw a clear boundary between psychology and physics, Wundt trained subjects to report only their "sensations" and not the physical objects "inferred" from them, which figure in ordinary accounts of experience; hence, in a vision experiment, one would report a "red-round-shiny-presence" instead of an "apple." This stricture also served to prevent psychology from studying higher mental processes. Wundt justified the move on the grounds that reliable results could not be ensured for the higher processes. Nevertheless, the Wuerzburg School performed the forbidden experiments and were promptly accused of committing "stimulus error" and practicing (mere) philosophical introspection (Boring 1950, ch. 18). In a 1913 review of the curious difficulties encountered by Wundt in training subjects to issue proper reports, Wolfgang Koehler (1971, ch. 1) concluded that Wundt was so concerned with protecting the scientific autonomy of psychology that he missed the point of all his difficulties, which was that the distinction between raw sensations and cognitive meaning is an empiricist philosophical myth whose survival was due entirely to the "artificiality" of Wundt's own technique. Soon thereafter Koehler and Max Wertheimer spearheaded the Gestalt movement.

Although behaviorist accounts are notorious for avoiding any reference to mental processes not accessible to the experimenter, it has gone relatively unnoticed that behaviorist experiments are typically designed to minimize the appearance of traditional "outward signs" of mental process. For example, Edward Thorndike operationalized animal intelligence as a function of learning rate, the aim being to lower the time taken by an animal to solve the "puzzle box," a prototype of the Skinner Box. He supposed that once placed in the box, the animal would know what was expected of it. Consequently, any of its behaviors that did not immediately contribute to a solution were counted as errors. However, in Thorndike's day, the two main powers of the active mind were taken to be creativity and deliberation, whose outward signs were, respectively, spontaneity and hesitation. Both were associated with, as William James put it, a "facility of nervous discharge from sense organs to motor response" (Boring 1950, ch. 21). In short, the truly intelligent animal could not be set in its ways, as Thorndike's laws of automatic response would surely make it. The postulation of an active mind rested on a view of the animal as always being in a situation that may be rendered intelligible in many different ways. In fact, this view especially suited nonhuman animals, which were never expected to have understood what the experimenter had planned for them. Thus, an animal's initial fumblings in the puzzle box should be regarded as attempts at defining the problem rather than as failures at providing a solution. Interestingly, Koehler (1971, chs. 10-11) was also behind this critique of behaviorism, from
which he concluded that psychology could not continue to be the "science of mind," unless it relinquishes the efficiency aims of instrumental conditioning.

Yet behaviorism remained the paradigm of academic psychology in the U.S. for forty years, precisely because J.B. Watson was originally able to convince his colleagues that the discipline could exhibit cumulative growth if it conducted research whose results would be unaffected by whichever theory of introspection or neurophysiology turned out to be correct. Indeed, Watson argued that, if nothing else, the range of mental contents that had been reported by introspectionists proved that subjects could be verbally conditioned in any number of ways (Fuller 1986). And with that began the era of "black box" thinking in psychology.

(6) Freud's avowed aim in declaring sanity to be the limiting case of neurosis was to make all psychological phenomena—not merely pathologies—fair game for the psychoanalyst. More interestingly, since all sane people are virtual neurotics, the self-reports made during Wundt's controlled introspection cannot be presumed unproblematic, but must first be decoded for repressed messages from the unconscious. This line of reasoning justified Freud's dismissal of experimental psychology as pseudo-scientific. And although a similar charge would later be levied against psychoanalysis, at the turn of the century it contributed to the fall of the introspective paradigm. However, Freud also refashioned certain introspection techniques for his own ends, largely through the influence of Carl Jung, a student of the Wundtian psychopathologist Emil Kraepelin. For example, Jung introduced free association, which Kraepelin had used as a diagnostic tool for identifying fixations by counting how often the same word appeared in a subject's set of responses. Psychoanalysis supplemented the method, against Wundtian strictures, by drawing inferences about the significance of the repeated words (Maher & Maher 1979, pp. 566-573).

(7) Sensitive to the attempts by Sechenov, Pavlov, and Bechterev to "naturalize" the study of man, the school of critics known as "the Russian Formalists" sought to ground "literary science" on a methodology designed to break the laws of classical conditioning of linguistic reflexes and to extinguish such reflexes once they had been conditioned. The exemplar of this process of "defamiliarization" was difficult Symbolist poetry, whose syntactic ambiguity forced one to reconsider his normal reading habits. The Formalists were philosophically influenced by Ernst Mach (and hence roundly condemned by Lenin in Materialism and Empirio-Criticism), which led them to regard the literary scientist as a technician much in the same
way as the "reflexologist" regarded himself—the difference being, of course, that the literary scientist develops techniques for systematically undermining the effects of reflexology (Lemon & Reis 1965). This unabashedly "neopositivist aesthetic" legitimated several modern art trends, most notably Surrealism's subversion of perspective. After the Bolshevik Revolution, the formalists emigrated to Prague, and eventually to Paris, where one of their number, Roman Jakobson, started the Structuralist movement in linguistics and literary criticism (Merquior 1986).

5. Disciplinary Ambivalence: Popperian and Foucauldian Versions

Robert Merton (1976) introduced the concept of sociological ambivalence to capture the fact that social roles often serve multiple functions whose performance cannot be jointly maximized. Moreover, society does not usually offer any ready-made rules for making the necessary tradeoffs, which leaves the role occupant in a state of tension. For example, the scientist is supposed to both expedite the flow of knowledge and not rush into print. But how can he "expedite" without also "rushing"? While Merton tends to suppose that all scientists experience the same kinds of ambivalence, a finer-grained analysis of the concept might reveal that each discipline has a characteristic way of resolving its ambivalences, which, in turn, become the basis on which its cognitive status is evaluated by other disciplines and the public at large. This thesis of Disciplinary Ambivalence may be illustrated by considering the multiple linguistic functions performed by the discourses of disciplines. Our model, adapted from Popper (1972, pp. 119-121), specifies four such functions, each associated with a virtue of disciplinary discourse:

(j) The virtue of signalling is efficiency. A discipline aims to convey the most (new) information per unit of discourse expended.

(k) The virtue of expressing is surveyability (Wright 1980). A discipline aims to make each step of its reasoning evident in its discourse.

(l) The virtue of describing is accuracy. A discipline aims to maximize the total amount of truth conveyed in its discourse.

(m) The virtue of criticizing is precision. A discipline aims to maximize the total amount of error eliminated from its discourse.

We shall call the ambivalence that arises between (j) and (k) Foucauldian, after the most famous recent student of disciplines as the means by which knowledge is used to control nature and culture (Smart 1983). Foucauldian
ambivalence has little interested analytic philosophers of science, with the exception of Stephen Toulmin (1972) and the Lakatosian Yehuda Elkana (1982). In contrast, the ambivalence that arises between (l) and (m) has been virtually the sole concern of analytic philosophers. We shall call it Popperian after the philosopher who took a particular resolution of this ambivalence—namely, maximizing the precision of hypotheses at the risk of their accuracy—as the criterion for demarcating "science" from other disciplines (Popper 1972, pp. 193). As we shall now see, both the Foucauldian and Popperian species are needed for providing a framework capable of charting the history of Disciplinary Ambivalence. But first let us explore the considerations involved in each ambivalence.

Foucauldian Ambivalence. Paul Grice's rules of conversational implicature, especially the Quantity Maxim, force a trade-off between efficiency and surveyability (Leech 1983, pp. 84-89). For any given discipline, the most efficient discourse would convey only new information relative to the intended audience. However, this move would also minimize surveyability by making the discipline's discourse accessible only to insiders. Still, the discipline might not at first regard this hermeticism as such a loss, for if its discourse also exhibits a measure of accuracy and precision, then insiders will, in effect, have knowledge over some domain, the source of which would remain a mystery to outsiders unable to survey the discipline's reasoning. This is the cult of expertise, associated with the professionalization of knowledge (Collins 1975, ch. 9). Furthermore, if the insiders are experts on something that affects the behavior of the outsiders, then the formal definition of institutional power has been satisfied (Crozier 1964).

Yet the promise of power is not enough to dispel the ambivalence. Some degree of surveyability is necessary for introducing novices into a discipline. In fact, another such pedagogical function is also relevant to mature practitioners. For if each step in one's reasoning is made explicit, then errors, misunderstandings, and disagreements can be localized and treated as they occur. But this depends on complete surveyability, which is impracticable, since members of the same discipline generally work in disparate communities with only the elusive medium of print connecting them (Collins 1974). If unchecked for too long, incomplete surveyability may lead to deep misunderstandings between such communities, engendering "schools" and perhaps even Kuhnian incommensurability, which is often followed by the formation of new disciplines (Mulkay, Gilbert & Woolgar 1975, p. 198). Thus, while a strategy of maximizing efficiency promises power outside a discipline, a strategy of minimizing of surveyability adumbrates instability within that discipline.

Popperian Ambivalence. There is something paradoxical about Popper's falsifiability thesis that instills the ambivalence bearing his name. The thesis implies that a discipline becomes scientific once its members realize that eliminating errors is, in the long run, the most effective means of accumulating truths—indeed, more effective than simply trying to
accumulate truths in the short run. Furthermore, the short-and long-run pursuits of truth are incompatible—the short run being guided by accuracy and the long run by precision. Accuracy demands that a discipline issue claims that are highly probable given the current knowledge base, while precision demands that it issue highly improbable (high risk) claims that ultimately turn out to be true.

Consider a claim that has just been shown false. How does the discipline correct the error? The easiest way of increasing the claim’s probability is by specifically excluding the falsifying case—say, by appending an ad hoc hypothesis or by nimbly rewording the original claim. In both cases, the discipline has adjusted its discourse, more to describe past encounters with the world than to anticipate future ones. While some (Skorupski 1976, pp. 205-223) have taken this dogged pursuit of accuracy as emblematic of the subrational mind, others (Bloor 1979) have pointed out that not even mathematicians are immune to its charms. In contrast, Popper would have the discipline replace the original false claim with one equally vulnerable to falsification. And if that claim turns out to be corroborated, then the discipline would be advised to make it even more vulnerable by placing further constraints on the possible situations that will subsequently count as corroborations. Thus, whereas the pursuit of accuracy encourages consensus (and perhaps even stagnation) in a discipline, the pursuit of precision promotes divisiveness as its members undermine each other’s claims in the course of circumscribing their truth content. But contra Popper, the dogged pursuit of precision is more a gamble than a guarantee for ultimate truth. What precision does guarantee is a quick turnover in claims, which may lead to the discipline’s demise, if some fairly uncontested truths are not collected along the way (Mulkay, Gilbert & Woolgar 1975, p. 195).

Can an historical trend be discerned in the resolution of Disciplinary Ambivalence? If we look at the full panoramic sweep of organized knowledge in the West (say, from the pre-Socratics to the present), the trend has been to accord high cognitive status to disciplines whose discourses maximize efficiency and precision at the expense of surveyability and accuracy. Moreover, this cognitive status has been “accorded” not only through the plaudits dispensed by philosophical kibitzers but also through the allocation of economic and political resources. The facts surrounding this trend are by now familiar to sociologists: the increased division of cognitive labor, the increased frequency with which disciplines come and go, the increased technical control over well-defined domains, the increased store of undigestible information.

Of course, even to the most Whiggish eyes, the panoramic sweep of our cognitive development presents several slowdowns and setbacks along the way. One of these seems to have a permanent place in the social structure of knowledge—namely, folk wisdom—which, regardless of content, resolves Disciplinary Ambivalence in a manner diametrically opposed to the trend, by maximizing surveyability and accuracy. A discipline (or its wayward
practitioners) whose discourse heads in this direction is thus engaged in "popularization."

A more interesting deviant resolution of Disciplinary Ambivalence is to maximize surveyability and precision in one's disciplinary discourse. This is the image of science as "conjectures and refutations" on which Popper and most analytic philosophers have fixated. Whatever may be its normative status, as a sociological phenomenon this image has been limited to the Athenian polity and scholastic disputation. Although it is well known that the Athenian citizenry were entertained by public debate, less known is the fact that scholastics were similarly amused by disputation. In fact, there is reason to believe that, at least for Thomas Aquinas, the primary concern in composing a disputation as a series of objections followed by responses was to heighten the suspense of the lecturer's dialectical fate. Moreover, in order to make the lecturer's task seem as formidable as possible, trivial and important arguments were thrown together indiscriminately so as to pad the number of opening objections (McInerny 1983, p. 261). The net result was high drama, indeed, but also a rather ineffective way of gauging cognitive progress, as was observed by those otherwise opposed ideologues of the Scientific Revolution, Bacon and Descartes (Fuller 1985b).

No doubt such unmitigated enthusiasm for dialectics has also contributed to scholasticism's confusing legacy. But most importantly, since scholasticism has more closely approximated, on a large scale, the "up-against-all-odds" attitude to inquiry than any other discipline, its fate suggests that a proviso needs to be added to the Popperian imperative: namely, that a discipline's practitioners should not get so caught up in the activity of falsification that they lose sight of falsification's long term goal of truth, or, in an anti-Kantian spirit, falsification should always be a means and never an end in itself.

The last possible deviant resolution of Disciplinary Ambivalence remains the subject of much controversy, as documented in Adorno (1976). Maximize efficiency and accuracy in one's discourse: so goes the imperative of the hermeneutical disciplines—theology, jurisprudence, comparative literature, as well as several continental European schools of history, philosophy, and even sociology (Baldamus 1976, pp. 18-29). Since our terms for the linguistic virtues seem to obscure more than illuminate in this case, let us consider a piece of hermeneutical discourse, the *brocard* (Tourtoulon 1922, pp. 310).

A brocard is an aphorism that often introduces a judge's decision in civil law countries. The decision itself takes the form of a commentary on the brocard, during which the case under consideration is shown to exemplify the judge's interpretation. Brocards are designed to be used repeatedly for various cases by various judges, the overall effect being to lend continuity to the legal tradition. They are thus short and memorable, but also somewhat oracular, since their intended range of application is wide. Indeed, brocards are generally worded so that contradictory propositions can be read off them. And so, since a given brocard is applicable on virtually any occasion—
under different interpretations—it is quite literally never wrong (hence, brocards maximize accuracy). Furthermore, a brocard does not become less informative with repeated use, since it is always difficult to predict how the judge will use his discretion to interpret a given brocard in a given case (hence, brocards maximize efficiency). Take, for instance, *The end never justifies the means*. There is, of course, the standard Kantian reading of this brocard, but a clever judge can also divine an ultra-Machiavellian reading from it: to wit, the *end alone* never justifies the means used for achieving it, since the means should *also* have some desirable byproducts.

However, the cost of hermeneutical discourse is high. Like the discourse of the natural sciences, hermeneutics is hardly surveyable on a systematic basis; but unlike natural scientific discourse, it systematically avoids confronting error by trying to accommodate all interpretations. As a result, hermeneutical discourse is incapable of registering cognitive change. This feature has clear reactionary methodological and ideological consequences, as Popper (1972, pp. 183) and Habermas (McCarthy 1978, pp. 169-187), respectively, have noted. Curiously, theology has been the one hermeneutical discipline most sensitive to the problem of making progress in its inquiries.

In the field of *redaction criticism*, liberal Protestant theologians have suggested criteria for our coming to a greater understanding of Christ's message. In the late nineteenth century, Adolf von Harnack proposed that Christ's message is whatever is distinctive about the Gospels, once its similarities with other texts of the period have been discounted (Pauck 1965). Fifty years later, Rudolf Bultmann proposed that Christ's message is whatever the Gospels have in common with the canonical texts of the other major religious traditions (Macquarrie 1965). Thus, theological progress is tied, in Harnack's case, to findings in literary archaeology, while, in Bultmann's case, it is tied to findings in comparative religion.

Nevertheless, the cognitive degradation of hermeneutics has been long in the making. Perhaps because many hermeneuticians today define their own activity as having emotive or pragmatic import *instead of* cognitive import, it is often forgotten that the revival of classical learning known as the Renaissance was largely a "Hermeneutical Revolution" (Yates 1968). Cognitive "progress" then consisted of clarifying one's understanding of the wisdom contained in ancient texts. These texts were thought to be wise precisely because of their authors' temporal proximity to the Creation. Indeed, Nature itself was regarded as a book that could be "cross-referenced" with the canonical texts (Gadamer 1975, p. 160). Moreover, this was the attitude taken by the seventeenth century's greatest natural historian, Robert Hooke, and it was prevalent well into the nineteenth century in the work of the *Naturphilosophen*. The point here is that while "hermeneutics" is nowadays used exclusively to characterize the methodology of the humanities, the term primarily identifies one general way of disciplinizing the discourse of one's inquiry, which has applied to what we now call "sciences" as to the humanities.
Interestingly, the hermeneutical sense of cognitive progress also legitimated the Ciceronian theory of translation, which we still accept today, albeit rather uncritically: that a translation should preserve the content of the original text. (Why not aim, instead, to preserve the response that the text elicited from its original audience? This question was treated in chapter five.) Even during the Scientific Revolution, no less than Isaac Newton can be found to justify his harmonious world-system as an explication of the *prisca sapientia* of the early Greeks and Hebrews (McGuire & Rattansi 1966). However, the precedent for regarding hermeneutical knowledge as both old and obsolete had already been set. In an attempt to integrate the recently recovered Aristotelian corpus into the scholastic curriculum, Aquinas argued that Aristotle's natural scientific method fully articulated reliable routes to knowledge that had only been inchoately expressed by the more hermeneutical liberal arts (McInerny 1983, pp. 258).

When and why did the balance of cognitive power weigh decisively against hermeneutics? The answers to these little examined questions are far from clear. But an important key lies in an interdisciplinary history of the theory and practice of translation, which has so far been subject to purely "internalist" treatments (Bassnett-McGuire 1980). In particular, one would want to see under what conditions translators started to challenge the intuition that maximum understanding is necessary for making maximum use of a text. For what probably separates practitioners of the "sciences" from those of the "arts" is the scientist's sense that the time and effort spent in interpreting his precursors is time and effort taken away from contributing to the growth of knowledge.