# The Scope and Limits of Scientific Objectivity*

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The aim of this paper is twofold: first to sketch a framework for classifying a wide range of conceptions of scientific objectivity and second to present and defend a conception of scientific objectivity that fills a neglected niche in the resulting hierarchy of viewpoints. Roughly speaking, the proposed ideal of scientific objectivity is *effectiveness* in the informal but technical sense of an *effective method.* Science progresses when "higher levels of communicative discourse" are reached by transforming *subjective* judgments regarding the generation and reduction of data or the testing of theories into *objective* decision procedures that are automatic or mechanical.

**1. Introduction.** It is generally agreed that one of the distinguishing virtues of science is its objectivity. The scope of science is the *objective world* and the limits of science are determined by the limits of the *objective methods* of formal and empirical research. This distinction between the rational aim of science (to truly represent an independently existing reality) and the rational methods of science (designed to eliminate prejudice and bias in formal and empirical investigations) has generated a host of divergent ideals of scientific objectivity, ranging from Popper's (1963) strong realist view of objective truth as a regulative ideal for science on one extreme to Rorty's (1991) complete rejection of the notion of truth and proposed substitution of *methodological solidarity* for *methodological objectivity* on the other extreme.

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In her forceful critique of the dominant conceptions of 'objective' and 'objectivity' in contemporary philosophy, Elisabeth Lloyd (1995, 353) emphasizes this dual aspect or "hybrid" character of scientific objectivity. She notes that sometimes:

> —*objective* means *detached,* disinterested, unbiased, impersonal, . . . *not having* a point of view. . . ;
> —*objective* means *public,* publicly available, observable, or accessible (at least in principle);
> —*objective* means *existing independently* or separately from us;
> —*objective* means really existing, Really Real, the way things really are.

The first two conceptions identified by Lloyd are *internal* or *methodological,* having to do with the rational methods of science; the latter two conceptions are *external* or *representational,* having to do with the rational goals of science.[1] The central theme of her critique of contemporary notions of scientific objectivity—a theme that complements much of the present analysis—is that many recent epistemological theories of science seem to invoke a double standard. As Lloyd expresses the point:

> The belief that 'feminist epistemology' is an oxymoron—because real knowledge of reality involves 'objectivity,' and 'objectivity' *just means* 'neutral,' 'non-ideological,' and 'distanced from any personal interests or idiosyncrasies'—collapses under scrutiny.
>
> It is 'pure epistemology,' 'value-free inquiry,' and 'disinterested knowledge' that are oxymorons. Mainstream contemporary epistemologists and metaphysicians acknowledge this in their own work, so why do they implicitly hold feminist philosophers to a different standard? (1995, 374–375)

Lloyd has identified a deep and disturbing tension in contemporary theories of science. How can one coherently reject the claim that science is "aimed at" an independently existing, objective reality, while maintaining that scientific methods can (and ought to) be neutral, non-ideological, and unbiased? If we cannot make sense of the notion of an external reality that is independent of our expectations, interests, and intentions, then how can we maintain that scientists ought to follow a method of investigation that is neutral and disinterested?[2] The tension is disturbing because once

1. In an illuminating discussion of the history of the concept of scientific objectivity, Lorraine Daston (1992) also emphasizes its complex, multi-layered origins.

2. An anonymous referee has remarked that this paper " . . . begins from the premise that no sense can be made of 'an external reality that is independent of our expectations, interests,

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we give up the ideal of neutral, disinterested methods of investigation it seems that we have lost the objective basis that is the unique and important virtue of the scientific method.

The aim of this paper is twofold: first to sketch a framework for classifying a wide range of conceptions of scientific objectivity and second to present and defend a conception of scientific objectivity that fills a neglected niche in the resulting hierarchy of viewpoints and suggests at least a partial resolution of the disturbing tension that Lloyd has so convincingly portrayed. Roughly speaking, the proposed ideal of scientific objectivity is *effectiveness* in the informal but technical sense of an *effective method.* Science makes objective progress when decision procedures requiring *subjective* human input are replaced by decision procedures that are automatic or mechanical. The most compelling metaphor for this notion of scientific progress and scientific objectivity is computerization—the increasing role that electronic computers play both in the collection, reduction, analysis and interpretation of data and in the formulation, articulation and testing of theories.

To avoid a serious misunderstanding, it must stressed at the outset that the proposed notion of scientific objectivity does not imply that human reason or *human communicative discourse*—including human interests, values, perceptions and intuitions—play no significant role in objective science. Progress does not consist in the elimination, tout court, of communicative discourse—or of the associated social and epistemic values and interests—but in raising the level of that discourse. The habits of action in the everyday life-world upon which all human discourse is founded are not absolutes. No doubt, there was a period in human history when the simple activity of counting was a discursive activity rather than a straightforward matter of habit. Eventually, as counting became just a matter of *techne* it was possible for human culture to tackle more complex aspects of arithmetic, such as addition and multiplication. The point is

and intentions' " but that " . . . this [is] an assumption not being directly defended within the paper itself ." Actually, the structure of the argument is more complex than that. In the first place, I've argued that the artifactual data—both empirical and formal—produced and recorded in the course of scientific research are Really Real: as Real as the food that we eat and the clothes that we wear. Secondly, those artifacts are always products of an *interaction*—whether by humans or (increasingly) by automated measuring or calculating devices—with the very world that is being investigated. Thirdly, those artifacts—including measures of their precision, reliability, and correlation (which are just further artifacts)—provide the only grounds there are for inferences to the existence and properties of theoretical entities and magnitudes; and those inferences are only *internally* and *implicitly* accessible to individuals who are enculturated in the relevant discipline. Finally, in limited but significant ways, the theory and practice of the professional community functions as a *blueprint* (that may be more or less *effective*) for the construction of the very artifacts that support the continued vitality of the scientific discipline.

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very general: every discipline presupposes a foundation of "unconscious habit" and the more complex and penetrating the discipline the more broadly and deeply entrenched must be the foundation of habits (*techne*) that support activity at the frontier.

To elaborate upon this metaphor of "raising the level of communicative discourse," the empirical base of science consists of complex technological *artifacts* (photographic plates, meter readings, digital outputs of recording instruments, etc.) which are just as much a part of the *objective everyday life-world* as the desk and computer in front of which I am currently sitting.[3] The empirical base of advancing contemporary science does not consist of anyone's sense data or perceptions. Communicative discourse (conscious reflection, application of epistemic and social values, creative elaboration of analogies and metaphors) occurs at a higher level in judgments of whether existing *artifacts* are "objectively relevant"—whether they count as evidence for or against a given theory—and in judgments of whether or how the experimental context that has produced those *artifacts* ought to be modified or elaborated. Science makes progress through the replacement of *subjective* protocol judgments by *objective* artifactual data.

In a similar fashion, methodological progress in the articulation, application and testing of scientific theories results when decision processes requiring *subjective* human judgment are superseded by decision processes utilizing *objective* automated computation. Again, this does not imply that communicative discourse has no role in connection with scientific theories, but rather that the automation (objectification) of lower level decision processes makes possible more complex communicative discourse at a higher level.

The artifacts—empirical, computational and technological—that result from the development, testing and application of scientific theories are accessible to "public inspection," and in a significant sense they are "Really Real" and "exist independently or separately from us." However, like all artifacts they embody human interests, intentions and values; there are no value-neutral or disinterested artifacts. As will be argued below in agreement with Lloyd, to deny this last point is to embrace a "double standard" of scientific objectivity.[4]

3. Note that an *artifact* is, indeed, a type of fact. I will refer below to these *foundational* items as *exogenous facts* (as distinct from *endogenous facts*) to indicate that they are independent of scientific theory.

4. The present approach is also sympathetic to (and provides a further epistemological rationale for) Janet Kourany's (2003) project for "A Philosophy of Science for the Twenty-First Century," which she outlines in her recent "debate" in this journal with Ronald Giere (2003).

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**2. External versus Internal Objectivity.** The technical concept of *representation* represents one of the earliest and most pervasive themes in western philosophy. It seems that it must be virtually coextensive with philosophical self-consciousness—our awareness of our internal subjectivity contrasted with an external world of objects. This apparently unavoidable aspect of everyday realism gives substance and force to the scientific realists' conception of objective reality, grounded in an intuitive version of the correspondence theory of truth. There are at least three distinct carriers of external objectivity: namely, i) entities or processes; ii) kinds, constructs, or magnitudes; and iii) interpreted sentences or theories. We will say that an entity, process, magnitude, construct or kind is *externally objective* if and only if its existence and properties are independent of its representation, while a linguistic statement or theory is *externally objective* if and only if its truth value is independent of its representation. Intuitively, external objectivity amounts to independence from subjective or inter-subjective factors or influences—our human hopes, fears, desires, expectations, etc. are irrelevant to the existence, properties, or truthlikeness of objective features of the world. According to scientific realists, objective reality provides an ideal toward which scientific investigations are aimed. In short, the emphasis of external objectivity is on the veridical character of any proposed representation of the world.

By contrast, *internal objectivity* is concerned with the processes or methods by means of which science investigates the world; so it might also appropriately be called *methodological objectivity.* Popper's multifaceted conception of science provides an illuminating illustration of the complementary relationship that is often, but not invariably, claimed to hold between the pursuit of objective methods and the goal of objective truth. In his early classic, Popper states "... I hold that scientific theories are never justifiable or verifiable, but that they are nevertheless testable. I shall therefore say that the objectivity of scientific statements lies in the fact that they can be *inter-subjectively tested*" ([1934] 1968, 44). In his later writings Popper calls this approach to science, which relies upon the inter-subjective testing of all empirical statements by critically evaluating their deductive consequences, the method of conjecture and refutation. There is very little in Popper's early work to suggest that he is advocating any notion of external objectivity. Indeed, he claims (273–274), "In the logic of science here outlined it is possible to avoid the concepts 'true' and 'false.' Their place may be taken by logical considerations about derivability relations."[5] However, in his later works Popper (1963) explicitly

5. In his contribution to the Schilpp volumes on Popper, Eugene Freeman (1974) develops a distinction, similar to the present distinction between external and internal objectivity, using the terms 'factual objectivity' and 'rule objectivity.' Freeman (1974) emphasizes the close

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introduces a concept of *verisimilitude*—motivated by Tarski's correspondence theory of truth—and argues for the external objectivity of science, claiming that objective truth constitutes a regulative ideal for the goal of science. Virtually every philosopher who holds that external objectivity is the ideal toward which science is aimed also advocates an objective method that guides the process of scientific research toward its goal. So, strictly speaking, internal (or methodological) objectivists are those, such as Kuhn, Rorty, and the later Carnap, who deny that there is any coherent sense to be made of the concept of external objectivity.

Lloyd (1995) attributes a double standard to philosophers who recognize, on the one hand, that the ideal of external objectivity is incoherent—those who, in her terminology, reject *ontological tyranny* and *type/law convergent realism*—but who seem to claim, on the other hand, that there is an ideal method of science that can (and should) be value neutral. However, this important criticism leaves open the question of how (or whether) to replace this incoherent notion of external objectivity (and its associated notion of value-neutral methods of inquiry) with an alternative notion of internal or methodological objectivity. The main purpose of the present paper is to address this latter question.

*2.1. Internal Objectivity and Human Agency.* The distinction between external objectivity and internal objectivity derives from a fundamental distinction between representation as a static relation between structures of thought or language and independent structures in the world, on the one hand, and representation as an active engagement in the world driven by human interests, purposes and values, on the other hand. The latter emphasis upon active representation, woven into the fabric of twentieth century philosophy, was a significant strand in each of the three major philosophic threads: formal language analysis, ordinary language analysis and phenomenology. It was exemplified in the formal language analytic tradition by operationalism and the verifiability theory of meaning; in the ordinary language analytic tradition by the later Wittgenstein's emphasis upon rule following and "forms of life"; and in phenomenology by Husserl's concept of the "life-world," Heidegger's emphasis upon hermeneutics and Sartre's dictum "existence precedes essence." For present purposes, the two most significant patterns in the fabric of "process philosophy" are traceable to the pragmatism of Peirce and Dewey and to

affinity of Popper's version of methodological objectivity to Peirceian pragmatism, noting: "It is evident that rule objectivity is thoroughly pragmatic—it is objectivity as it effects human behavior. In a word we behave toward that which has rule objectivity as though (als ob) it were true in fact instead of by agreement, and 'intersubjective agreement' is seen to be the translation into pragmatic or operational language of the term 'objectivity'" (466).

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Wittgenstein's *Philosophical Investigations.* Wittgenstein's 'use theory of meaning' figures prominently in the multiple senses that Kuhn attaches to the role of paradigms in science and pragmatism has had a pervasive influence upon all three threads of "process philosophy"—e.g., upon the formal-language empiricist views of Carnap and Quine, upon Rorty's (1991) post-analytic anti-philosophy, and upon Habermas' (1971) critical theory. It is this multi-shaded pragmatic tradition that is the inspiration for the concept of scientific objectivity that is articulated in this paper.

Robert Brandom's (1994) enterprising attempt to substantiate a normative basis for the ideal of external objectivity illustrates a persistent ambivalence in the pragmatist tradition. Brandom rejects the internalist, Wittgensteinian notion that objectivity is ultimately grounded in the norms of social practice—a viewpoint that is articulated in Kuhn's (1977) discussion of the objectivity of theory choice—arguing instead that:

> It is part of the representational purport of conceptual contents that thoughts and talk give us a perspectival grip on a nonperspectival world. To say that objects and the world of facts that comprises them are what they are regardless of what anyone takes them to be requires a notion of *objective correctness* of claiming and concept application that is *not* perspective-relative in the way that what is *taken* to follow from what is. (Brandom 1994, 594–595, italics in original)

The intricate details of Brandom's reconstruction of pragmatism are beyond the scope of the present concerns, but two of the unsupported assumptions upon which it rests provide a useful foil for "zeroing in" on the primary target of this paper: the concept of *explicit, internal* objectivity. The first is an assumed dualism between human perception and human action and an associated distinction between the "propositional attitudes" (e.g., directed toward scientific observations or theories) of *taking true* and *making true.* The second is an assumed dualism between *implicit* doing (knowing how) and *explicit* saying (knowing that) according to which the former is always primary. These interconnected assumptions are far reaching: ultimately, they undergird the very possibility of drawing a principled distinction between science—as an investigation of *what is* or of *the natural*—and technology—as an investigation of *what ought to be* or of *the artificial* (*artifactual*).

The concept of representation—of the relationship between syntactic structures and the objective world that they represent—has been one of the most pervasive concerns of modern philosophy; and the analogy of a map has been one of the most often invoked means of examining this concept. Maps, of various sorts, provide a visual instrument for "representing" the complex relationships among the syntactic, semantic and pragmatic features of representation. However, despite the emphasis that can be placed

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on the pragmatic aspects of the use of maps, the map analogy fails to adequately represent the role of human agency in the process of representation. A blueprint (or other sort of *project*) is a better analogy than a map; a map is a *passive* representation that can be actively used to *predict* empirical outcomes, while a blueprint is an *active* representation (a *projection*) that plays a substantive role in *forecasting* empirical outcomes.[6] The dictum ''facts are theory laden'' is commonly cited as a consequence of the classical pragmatists' emphasis upon the role of human expectations and human needs in guiding scientific inquiry. But the point is not simply that scientific theories constrain or condition the empirical facts that we can *take to be true;* more importantly, scientific theories are blueprints (projects) for forecasting facts—they provide a framework for facts that we can *make to be true.* As will be argued in Section 4, Galison's (1997) detailed description of the increasingly complex feats of twentieth century particle physics—utilizing the most sophisticated academic, industrial, military, and governmental techniques and resources—provides a compelling demonstration of the role that scientific theories play in forecasting scientific observations. That is why it is appropriate to refer to observations as artifacts and why it is not possible to draw the sharp contrast between perception and action that is implicit in the traditional concept of representation.

Brandom's comment that ''A distinction becomes a dualism when its components are distinguished in terms that makes their characteristic relations to one another ultimately unintelligible'' (1994, 615) is applicable to his distinction (cf. Chapter 4) between perception and action. He claims

> Only a properly trained physicist can *noninferentially observe* the presence of a mu-meson, and then only with a bubble chamber; only a properly trained pianist can *noninferentially produce* a performance of the Moonlight Sonata, and then only with a piano. (1994, 236, emphasis added)

There is a scope and limit to the effects a trained pianist can—through a sophisticated mixture of perception and action—produce with a piano and similarly a scope and limit to the effects a trained particle physicist can—through a sophisticated mixture of perception and action—produce with a bubble chamber; there is no principled ground for viewing the pianist

6. Here I am relying on the distinct etymological senses of these terms: to *predict* an outcome is to ''issue a prior dictum regarding it,'' while to *forecast* an outcome is to ''cast it in advance—including taking steps to bring it about that the outcome occurs''(cf., sense 1a in the *Oxford English Dictionary*). So forecasting involves human agency in a way that predicting does not.

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as *acting* and the particle physicist as merely *perceiving*. This more positive comparison of the bubble chamber and the piano is reinforced by Galison's discussion of the antipositivists' notion of a "'theory-laden 'tuning' of the experiment on the basis of a 'theoretical construct' " (1997, 543). Like Galison, I believe that the proper account of science lies in a synthesis of positivist and anti-positivist views, but—I would argue—a synthesis that recognizes *active forecasting* rather than *passive representing* as the fundamental relation of science to the world.

*2.2. Internal Objectivity and Scientific Progress.* The external realists' intuitive notion of scientific progress is clear enough: science makes progress by producing theories and data that correspond ever more closely to the independently existing, objective world—in the language of the later Popper, by increasing verisimilitude. However, it has proven to be very difficult to give a formal definition of 'verisimilitude' that can coherently explicate the implicit, informal intuition. A major obstacle to this effort is that the standard ways of evaluating theories and data are all internal to the process of inquiry. Ultimately, judgments of scientific progress rest upon three interrelated criteria: precision, reliability, and correlation. Roughly speaking, precision is a matter of the "'fine grain'' texture of experimental outcomes and theoretical predictions. Instruments are made more precise by refining the partition of experimental outcomes they are capable of recording; theories are made more precise by increasing the number of "'significant digits'' they are capable of predicting (forecasting). Reliability is a matter of the repeatability (in the sense of replication) of experimental results and theoretical predictions. Experiments are made more reliable by increasing the consistency of *intra*observer and *inter*observer experimental outcomes; theories are made more reliable by assigning likelihoods to empirical outcomes that more accurately reflect the actual relative frequency of their occurrence. Reliability, in the present sense, is a special form of correlation; more generally, correlation is a matter of the structural or statistical consistency or similarity of outcomes among various experimental or theoretical conditions or contexts. The range of correlational concepts and procedures is vast: including information theory; statistical methods of description, estimation, and inference; and a wide range of modeling techniques.

Questions of construct-precision,-reliability, and-correlation are internal to—objectively determinable within—the theoretical and experimental process of scientific inquiry in a way that questions of *construct-validity* are not. For example, physiologists have effective methods of assessing the precision and reliability of a proposed measure of toxicity and through analysis of variance and other techniques are able to assess the correlation of the proposed measure with various measurable responses

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of an organism. However, there is no effective method of determining whether the construct is valid—whether it *really* measures toxicity. The external realist can replace the early operationalists' slogan "the *meaning* of the construct is the method of its measurement" by the causal theorists' slogan "the *external referent* of the construct is that *Really Real* magnitude that *causes* scientists to measure it in the way that they do"; but that empty slogan is hand waving—the only grounds there are for the claim that the construct is valid (i.e., Really Real) lie in the internal evidence of precision, reliability, and correlation. Ultimately, these internal criteria provide the only effective measure of scientific progress.[7]

**3. Explicit versus Implicit Objectivity.** In one significant respect the influence of Wittgenstein's use theory of meaning on the pattern of process philosophy has been relatively uniform: knowledge, embodied implicitly or tacitly in a practice or form of life, is context dependent or *open textured.* The implicit "knowledge how" that one acquires through exposure to the paradigms of a practice cannot be transformed into the explicit "knowledge that" of a linguistic representation. By contrast, the pragmatic tradition has produced a remarkably intricate pattern of distinctively shaded viewpoints on this same issue, ranging from the logical empiricists' emphasis upon explicit operations, formal syntax, and effective decision procedures at one extreme to Rorty's anti-representationalist emphasis upon intersubjective agreement or solidarity at the other extreme—with viewpoints as diverse as Kuhn's and the early Popper's falling in between. Pragmatism has also had significantly varying shades of influence on this question of the explicitness of meaning within the formal language tradition—most notably in the well-known disagreement between Carnap and Quine over the analytic-synthetic and the observation-theory distinctions.[8]

Like the intuitive distinction between external objectivity and internal objectivity, the intuitive distinction—an internal or methodological distinction—between explicit objectivity and implicit objectivity seems

7. An anonymous referee observed, regarding the concept of *reliability,* "Many readers will find it most plausibly an externalist criterion." It is true that *epistemological reliabilists* are *causal externalists,* so 'reliability' is ambiguous. But for any *X,* a causal theory of *X* has no *effective* content when divorced from the pragmatic context in which *X* is embodied. In part, this is the message that Brandom (1994, 206–21) is conveying in his quasi-pragmatist " . . . *Inferentialist Middle Way between Justificatory Internalism and Reliabilist Externalism*."

8. The ideal, limiting concept of explicit objectivity developed in this paper—in the form of an intuitive principle of effective decidability—was discussed from a somewhat different perspective in Hanna (1985), where it is argued that the ideal—in the guise of principles of methodological atomism and methodological nominalism—guided the evolution of logical empiricism (especially, as reflected in the writings of Carnap).

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clear enough. Brandom expresses the distinction and the assumed order of priority as:

> . . . norms that are *explicit* in the form of rules, principles, or claims (Wittgenstein's 'interpretations') depend for their intelligibility—their determining a distinction between performances that are correct and incorrect, appropriate and inappropriate—on a more fundamental form of norms that are *implicit* in practice—in what is *done* rather than what is *said*. (1994, 62, emphasis added)

The concept of *explicit* scientific objectivity that I'm advancing is in fundamental agreement with the importance that Brandom attributes to the role of logical concepts and techniques in the advancement of human theoretical and practical knowledge:

> Logical vocabulary has the expressive role of making *explicit*—in the form of logically compound, assertible sentential contents—the *implicit* material commitments in virtue of which logically atomic sentences have the contents that they do. Logic transforms semantic practices into principles. By providing the expressive tools permitting us to endorse in what we say what before we could only endorse in what we did, logic makes it possible for the development of concepts by which we conceive our world and our plans (and so ourselves) to rise in part above the indistinct realm of mere tradition, of evolution according to the results of the thoughtless jostling of the habitual and the fortuitous, and enter the comparatively well-lit discursive market place, where reasons are sought and proffered, and every endorsement is liable to being put on the scales and found wanting. (1994, 402–403, emphasis in original)

What I find missing in Brandom's discussion (and in Kuhn's closely related discussion, as well) is an appreciation for the cyclical, interactive "semantic ascent" that is made possible through the role that (explicit) syntax plays in the (implicit) pragmatic constitution of semantics. It's true that implicit doing necessarily precedes explicit saying; but explicit saying—including both the explicit procedures and practices of material technology and the explicit formal techniques of language, logic, and mathematics—make possible a "higher" level of practice (a new pragmatic context) that could not otherwise have been reached. It is not simply a question of rising above "the thoughtless jostling of the habitual and the fortuitous." The higher level provides a platform for a new level of "thoughtless and fortuitous jostling," which we can only rise above by again rendering it explicit.[9]

9. cf. the discussion in Sections 3.1 and 3.2 below.

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If *explicit* objectivity represents a striving for the limit point (*effective decidability* or *mechanism*) of a continuum, then *implicit* objectivity represents a resistance to the pursuit of that ideal. Kuhn 1977 is a paradigm of this emphasis upon *implicit, internal* objectivity.[10] In this article Kuhn is attempting to avoid being impaled on the horns of a purported dilemma that is frequently directed by external realists against advocates of any version of internal realism (those who reject a strong notion of external objectivity). The strategy is to attempt to force anyone who rejects the external realist conception of objective truth to the opposite extreme position of complete subjectivism. In the opening paragraph, Kuhn (1977, 320) recalls his rhetorical question in the closing chapter of his classic *The Structure of Scientific Revolutions* "What better criterion than the decision of the scientific group could there be?" which he had raised in defense of the view that the only (objective) standard of scientific progress is the "collective judgment of scientists" who share a number of characteristics or values by virtue of "the training which licenses their membership in one or another community of specialists"[11] (Kuhn 1970, 170). Subsequently, Kuhn points to "... five characteristics—accuracy, consistency, scope, simplicity, and fruitfulness—[as] ... standard criteria for evaluating the adequacy of a theory" (1977, 322). In view of the role attributed to such "epistemic values," he expresses his continuing surprise that his critics had interpreted the viewpoint in the earlier work as advocating irrationality, subjectivity, and rhetoric in the practice of science.

So Kuhn's (1977) primary goal was to restate and clarify a conception of scientific objectivity that was internal to the practice of science but based upon the reasoned application of shared epistemic values rather than upon the subjective whims, hopes, desires, expectations, etc. of individual scientists. The following passage captures the positive character of his account of implicit, internal scientific objectivity:

> I am suggesting...that the criteria of choice with which I began function not as [*explicit*] rules, which determine choice, but as [*implicit*] values, which influence it.... Values like accuracy, consistency, and scope may prove ambiguous in application, both individually and collectively; they may, that is, be an insufficient basis for a *shared* algorithm of choice. (1977, 331)

10. See also, Kuhn's (1970, 191–198) discussion of *Tacit Knowledge and Intuition.*

11. Kuhn (1970) follows this rhetorical question by arguing that "we may ... have to relinquish the notion, explicit or implicit, that changes of paradigm carry scientists and those who learn from them closer and closer to the truth" (170). In the pages that follow this quote Kuhn clearly rejects any notion of external objectivity.

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It is significant that the specific form of the dilemma against which Kuhn is defending his view of *internal* scientific objectivity has complete subjectivity or randomness of choice on one extreme and complete objectivity or determinateness—as in an algorithm for choice—on the other hand. Neither extreme invokes genuine human, reasoned judgment. This much of Kuhn seems right; any view that denies human reason a substantive role in science is evidently inadequate.[12]

But what seems lacking in Kuhn's account of scientific objectivity is an appreciation of the central role that algorithms or effective methods of decision making do play in all aspects of scientific research—both in the generation, reduction, and analysis of data and in the articulation, application, and testing of theories. 'Accuracy' as an epistemic value is ambiguous and in one of its senses—as correspondence to an independent reality—it has external objectivist connotations. However, 'accuracy' can also be associated with 'precision,' 'reliability,' and 'correlation'; and, as argued above, the latter values can be understood as specific features of coherence. All three of these ''coherence indicators'' come in degrees and have limiting values that represent generally unattainable ideals. Moreover, there is an intimate connection between these ideals of coherence and the functioning of algorithms or effective methods. For example, computers can fulfill the central role that they play in scientific research because of their remarkable degree of precision and reliability. The numbers that they produce are not guaranteed to be accurate—in a correspondence sense—but they are generally very precise and reliable and are subject to internal consistency (correlation) checks that would provide an indication of many sorts of failure.

Kuhn (1977, 328) seems prepared to grant that individual scientists may follow algorithms (perhaps some variant of Bayesian inference) in their theory choices, but claims that since different scientists follow different algorithms—and there is no objective (effective) way to decide among them—there is no algorithm for theory choice for the scientific group as a whole. It strikes me that in this passage Kuhn is granting both too much

12. In general, an ''internal realist'' faces this dilemma: since one cannot effectively invoke external reality as a standard for the evaluation of scientific theories and data, he/she must either admit that there are no objective standards or else identify some methodological standards in the practice of science. Algorithms and mechanical processes provide objectivity of a certain sort (cf. the discussion of objectivity as ''Escape from Perspective'' in Daston 1992). But at the same time, the automation of decision processes seems to eliminate the role of human reason in scientific inquiry. So the horns of the internal realist's methodological dilemma are: arbitrary subjectivity at one extreme and inhuman mechanism at the other extreme. Kuhn's concept of tacit knowledge, embodied *implicitly* in the practice (exemplars) of a community of experts, is his Witgensteinian response to that dilemma.

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and too little to his critics: too much, because it suggests that individual scientists, since they follow algorithms, do not reason or engage in discursive communication in arriving at their judgments about the characteristics of data and theories and the relations between them; and too little, because it suggests that algorithms play no significant role in influencing—indeed, in making possible—the collective judgments of the scientific group. In fact, algorithms play an essential and increasingly complex role in the "top down" application of scientific theory, in the "bottom up" production of scientific data, and especially in the interface between theory and data at multiple levels. It is through such algorithms that science attains the degree of external objectivity that it possesses—the mechanical objectivity of certain aspects of formal reasoning and the mechanical objectivity of "theory independent," artifactual data.

*3.1. Formal versus Informal Reasoning.* One of the most remarkable features of the history of logic and mathematics is the recurring phenomenon of informal concepts, objects, and methods, inspired by models and analogies drawn from the everyday world, being replaced by more formal concepts, objects, and methods—with one effect being to transform reasoned judgments, often grounded in inarticulate intuition, into decisions that are systematically determined by formal methods and techniques. The culturally early development of the basic techniques of arithmetic (counting, adding, multiplying, etc.) was mentioned above, but more recent examples abound. Consider, for example, Descartes discovery of analytic geometry (which provided an objective ground in arithmetic for the intuitive geometric notions of points, lines, etc); Gauss's development of the concept of a limit (which superseded the intuitive conceptions of infinite operations in analysis); Cantor's investigations of mathematical analysis involving properties of the continuum (which led to transfinite number theory and the first explicit recognition of the implicit role played by the axiom of choice in mathematical reasoning); and Hilbert's demonstration of the incompleteness of the axioms of Euclidean geometry (which revealed the fact that for over two thousand years mathematicians had been implicitly using the intuitive properties of geometric diagrams in their explicit "formal proofs").

Similar developments, involving the transformation of intuitive processes of informal reasoning into more formal, algorithmic methods of calculation and argumentation, can be cited in probability theory and statistics, in applied mathematics (e.g., Fourier analysis), and in the hardware and software of computer technology. None of these developments eliminates the role of discursive human reasoning (*implicit* practice) in the formal and empirical sciences, but they do contribute to the *explicit objectivity* of that reasoning. The *explicit* formalization (mechanization) of

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lower level processes makes possible *implicit* discursive reasoning at a higher and more complex level.

*3.2. Exogenous versus Endogenous Facts.* As noted above, there are many different threads woven into the distinctive patterns that make up the pragmatic tradition. One of the most central is the notion that objective knowledge is grounded in effective action. Although he specifically rejects other elements of pragmatism, Hacking follows that tradition in building his realist account of science on the objectivity of intervention rather than on the objectivity of "static" representation—noting that "We shall count as real what we can use to intervene in the world to affect something else, or what the world can use to affect us" (1983, 146). An essential component of Hacking's realist account is the claim that the interventions of observers must be theory free because individuals who are completely ignorant of the relevant theories can become skilled observers. Thus, responding to a "doctrine that noticing is theory-loaded"—attributed to N. R. Hanson's account of the discovery of the positron—Hacking remarks that:

> . . . there is a tendency to infer from stories like that of the positron, that anyone who reports, on looking at a photographic plate, 'that's a positron', is thereby implying or asserting a lot of theory. I do not think that is so. An assistant can be trained to recognize those tracks without having a clue about the theory. (1983, 179)

And, again:

> It may seem that any statement about what is seen with a microscope is theory-loaded. I disagree. One needs theory to make a microscope. You do not need theory to use one. (Hacking 1983, 191)

In one respect Hacking's remarks seem correct and illuminating to me: as science advances, the collection of "raw data" depends less and less upon whether or not the "collection device" (be it human or inhuman) understands the abstract theory. However, there is an underlying ambiguity in his references to scientific observation, rendering his account highly misleading. In the first place, the claim—that scientific observations are not theory laden, because theoretically untutored individuals can acquire the skills to make them—is not sufficiently radical. The fact is that in advanced areas of science, no human observers of any sort—whether trained in theory or not—are required in order to produce and process the sorts of "observations" that Hacking is discussing; such "observations" can be generated and evaluated automatically—by computers interfaced with experimental apparatus at "the observational end" and with computational algorithms at "the theoretical end." The intermediate results in

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this process—and, indeed, the process as a whole—are *objective artifacts;* they are "objects made with skill with a view to their subsequent use." As will become apparent from the example discussed below, they can be hierarchically ordered along a continuum from the observational to the theoretical.

Viewed from the observational end of the continuum, we can think of these intermediate stages of the modern scientific process of relating observation to theory as *exogenous facts;* that is they are external to theory. Meter readings, photographic plates, and recorded clicks of a Geiger counter are objects in the everyday "life-world." However, although exogenous facts are externally objective entities, like any objects produced by skilled craftsmen or by an automated assembly line, their scientific relevance is not objective in the same sense. Whether or not a given artifact (or range of artifacts) counts as evidence that is relevant to a given scientific theory can only be decided—in general through a process of *implicit,* epistemically-value-laden discourse of the type described in Kuhn 1977—by trained, competent members of the appropriate scientific community. The resulting observations—which may either provide rational support for a scientific theory or rationally undermine it—are *endogenous facts;* they exist as facts only within the framework of the scientific practice (project) that provides their meaning.

So Hacking's reference to an "observation report" such as 'that's a positron' when looking at a photographic plate is doubly misleading: First, because it suggests that scientific observations typically involve perceptual reports of that type; and second, because it fails to distinguish between the exogenous fact—any theoretically naïve observer can be trained or conditioned (any sufficiently powerful and empirically connected computer can be programmed) to give such a report on the basis of the concrete, geometrical properties of the photograph—and the endogenous fact—only trained physicists operating within the framework of the theory and technology of particle physics are competent both to *make it true* and to *take it true* that this *specific* (*token*) *artifact* is an indicator of the *generic* (*type*) *event* "track of a positron." In this crucial sense, (endogenous) facts are not only theory laden; they are also value laden.[13] The increasingly technology-intensive scientific *projects* of the twentieth and twenty-first centuries are systematic attempts to *forecast* those (endogenous) facts—to produce or make true the exogenous artifacts that constitute the explicit, internal grounds for their acceptance—in the interest of values that extend

13. If theories are value laden—if the theories that scientists initially entertain depend upon broadly social values—and if facts are theory laden—if facts are *artifacts, made to be true* under the guidance of those presupposed theories—then it follows that facts are also laden with broadly social values.

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far beyond the epistemological values embodied in the narrowly conceived process of scientific inquiry, itself.

**4. An Example: Twentieth Century Particle Physics.** Galison's (1997) epic account of the technological evolution of particle physics in the twentieth century provides a compelling illustration of the increasing role of "effective procedures" in the theory and practice of the most advanced sciences. Galison distinguishes two traditions of instrumentation: an *image* tradition which had as its goal the "representation of natural processes in their fullness and complexity" through " . . . the production of images of such clarity that a single picture can serve as evidence for a new particle or effect," and a *logic* tradition which used "electronic counters coupled in electronic logic circuits" to " . . . aggregate masses of data to make statistical arguments for the existence of a particle or effect" (19–20) Noting that theoreticians, experimenters, and instrument makers speak different languages, Galison invokes an analogy of the development of first pidgen, and later creole, languages that enabled the various scientific and technological communities to communicate. One of the aims of *Image and Logic* is

> to show how two subcultures of instrumentation formed a wordless creole: On one side is the image tradition of photographic practices aiming at *noninterventionist objectivity*—producing homomorphic representations of nature. On the other side is the logic tradition of electronic practices aimed at *manipulative persuasion*—producing homologous representations of nature. (20, *emphasis added*).

However, it is evident from Galison's discussion of the forces that led to the merger of these instrumental approaches into a single tradition that the *noninterventionist objectivity* of the image tradition and the *manipulative persuasion* of the logic tradition were really "flip sides of the same coin." On the image side: The complex procedures of material (e.g., bubble chamber) and electronic (e.g., scanning and measuring) technology that were required in the production and reduction of the data represented by the physical images and in the testing of physical theories against those data also involved a great deal of *persuasive manipulation.* The noninterventionist objectivity of the images lay in the fact that, ultimately, they were scanned and measured by completely mechanical (i.e., explicit, computerized) procedures that avoided any possibility of subjective human influence.[14] On the logic side: the electronic counting and statistical

14. As a Ph D. student in Logic and Methodology of Science, I spent 15 months during 1961–2 as a scanner and measurer (and eventually programmer) for the Alvarez Group at the Berkeley Radiation Lab. That experience—which exposed me to the "frenetic" efforts

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manipulation of data points (e.g., clicks of a Geiger counter) was also noninterventionist and objective in the sense that it was performed by completely mechanical (i.e., explicit, computerized) procedures that avoided any possibility of subjective human influence. The "more intense" manipulative persuasion of that tradition lay in the fact that intervention in the setup and calibration of measuring instruments was much more straightforward when using electronic counters than when using bubble chambers—in part because the data reduction and theoretical feedback was so much more immediate. The point is that in both traditions the exogenous facts were generated by objective, noninterventionist processes—computers automatically recorded and processed the data. However, the endogenous facts—the facts only (implicitly or tacitly) identifiable by those specialists who could "speak the creole language"—where forecast (made to be true) by processes that were not only guided by theory, they were highly interventionist and involved considerable manipulative persuasion as well.

In both the image tradition and the logic tradition the procedures for automated data generation and analysis produced objective artifacts of increasing levels of complexity. As Galison (1997) illustrates in great detail, data reduction proceeded simultaneously from the bottom up—through automated processing and recording of the "raw data"—and from the top down—through automatic evaluation and filtering of the data on the basis of background theoretical presuppositions. Throughout the twentieth century all aspects of the process of generating, reducing, and analyzing data and of the process of testing theories against data became increasingly mechanical and at the same time increasingly interactive and interventionist. In fact, there is an ambiguity in the concept of 'mechanical objectivity' that, from the present perspective, is highly significant. On the one hand, a 'mechanical process' is *noninterventionist,* in the sense that its performance does *not* require human judgment. Daston and Galison (1992) document the increasing importance that eighteenth and nineteenth century scientists attached to the objectivity achieved through mechanical drawing techniques and later through photographic techniques. They note that,

to mechanize the processes of data reduction and testing of theories and which had a lasting effect on my view of the increasing importance of *effective* (both material and formal) procedures in science—was the basis, in an earlier draft of this paper, for my taking the Alvarez Group as an exemplar of the trend toward mechanization in current science. Two anonymous referees recommended that I consult Galison (1997), and I was quite surprised to discover (1997, 371–384) that Alvarez, as a leading figure in the image tradition, had resisted the drive for *explicit objectivity* that was championed by Lew Kowarski, a leading spokesperson for the logic tradition, and that eventually dominated twentieth century particle physics.

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> One type of mechanical image, the photograph, became the emblem for all aspects of noninterventionist objectivity . . . because the camera apparently eliminated human agency. Nonintervention, not verisimilitude, lay at the heart of mechanical objectivity, and this is why mechanically produced images captured its message best. (Daston and Galison 1992, 120)

But, on the other hand, as Galison (1997) demonstrates in impressive detail, the history of twentieth century particle physics is a history of increasingly complex feats—complex both in an engineering sense and in a social sense—of human intervention and human agency. So the development and application of 'mechanical processes' is highly interventionist.

**5. Objectivity and Neutrality.** Lloyd (1995) has argued convincingly that once the illusory ideal of external objectivity is abandoned, the ideal of a value-neutral scientific method must also be rejected. The whole point of value neutrality is lost if there is no external reality that is independent of human hopes, fears, expectations, etc. to which scientific theories might converge. However, it seems undeniable that the artifacts (the explicit exogenous facts and the *explicit* measures of precision, reliability and correlation) that are associated with scientific experimentation and theory testing *really exist* and are *Really Real.* No doubt this was just as true when facts were recorded on the basis of subjective human perception and when verbal or written judgments of the adequacy of theories were arrived at by "seat of the pants" intuition. Protocol statements such as "I'm perceiving an electron" or "it seems to me that these data support this theory more than they support that theory" are just as real as automatic recordings of instruments or printouts of statistical tests. But instrumental data and formal methods of testing theories—and especially instrumentation and methods that are more objective in the sense of being more automatic, mechanical, or effective—have several advantages over "subjective" protocol judgments.

First, and perhaps most importantly, the end products of effective methods of data collection and theory testing are objective in all of the senses identified by Lloyd. They are (in a qualified sense) *detached and disinterested,* they are *public,* they *exist independently of us* and they are *Really Real.* Secondly, effective methods are generally more precise and reliable and are correlated to a greater degree among themselves than are less effective methods. Thirdly, effective methods make it possible for scientists to accomplish immensely complex tasks—both in the collection of data and in the testing of theories—that could never be attempted

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without automation.[15] This last point is related to the above argument that increasing automation raises the level and sophistication of human scientific discourse.

However, the distinction between exogenous facts and endogenous facts (and the above mentioned qualification) is crucial here. Exogenous facts are externally objective and therefore in an important sense neutral (i.e., detached and disinterested), public, and Really Real. On the other hand, endogenous facts are none of these. They are accessible only to members of a scientific, technical elite and reflect the values of members of that community, and they are not Really Real in the sense that Lloyd understands that term. This crucial point could be illustrated with a broad range of examples reflecting a variety of value contexts, including the feminist context that was the primary focus of Lloyd's (1995) analysis of scientific objectivity. Kourany (2003) also emphasizes the "feminist project," noting that, "Science shapes our lives, and perhaps most important, science shapes our consciousness of ourselves" (10).[16]

**6. Conclusion.** In its most progressive form, modern science does not rest upon the traditional empiricist foundations of sense data or observation or protocol sentences.[17] Rather, the external, objective foundation of science is the hierarchy of artifacts that the community of competent scientists, utilizing complex (increasingly automated) experimental and computa-

15. This was certainly true of the vast quantity of data collected and analyzed during the *projects* of twentieth century particle physicists. As emphasized by an anonymous referee, this benefit of the increasing role of *effective methods* in science—the ability to tackle much more complex observational and computational tasks—is distinct from the contribution that *effective methods* make to the objectivity (neutrality and transparency) of science.

16. In my judgment, Giere's (2003) response to Kourany relies on an "unreliable" distinction between *belief* and *action*—a distinction that is very much like Brandom's (1994) distinction between *perception* and *action* and Hacking's (1983) distinction between *representing* and *intervening.* Giere writes,

> The concept of 'evaluating' theories can be ambiguous. One understanding is an *epistemic* evaluation as to suitability for *belief.* But another is a *practical* evaluation as to suitability as a basis for *action.* It may be scientifically unacceptable to *believe* in the truth of a theory because it conforms to a moral ideal, but it is not unacceptable to *decide* to rely on such a theory for purposes of practical action or policy. . .Thus, by distinguishing between belief and action, appraising theories by conformity to moral ideals need not be so controversial an idea after all. (2003, 20)

But if one recognizes that—though their *effectiveness* may be limited—scientific *research projects* are, to use Brandom's (1994, 233, 521) terminology, in the business of *making observations to be true* and not just in the business of *taking observations to be true,* then Giere's response can be seen as more directly supporting Kourany's call for a "New Program for Philosophy of Science."

17. I agree with the comment of an anonymous referee that scientists still do (and, no doubt, always will) rely upon sense perception, intuition, and other non-explicit and non-effective processes. That this is so is implicit in the analogy of "raising the level of communicative

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tional technologies, has been able to produce (forecast). The remarkable coherence—precision, reliability, and correlation—of the elements and levels of these complex hierarchies of artifacts is the ultimate basis for the judgment (of those who are competent to judge) that science is making progress. Thus, because they have been produced by effective procedures that are not influenced by subjective human input, the hierarchy of artifacts (bubble chamber photographs, recordings of measuring instruments, printouts of statistical tests.) produced by fundamental particle physicists is externally objective—the existence and properties of these (exogenous) artifacts are independent of their representation. However, the claim that a particular photograph contains the track of a mu-meson is only internally objective, because it depends upon the implicit (Kuhnian) judgment of the scientific community that the hierarchy of externally objective artifacts accurately exemplifies the current scientific theory.

The scope of science is determined by the complex hierarchies of observational and computational artifacts that the institutions of scientific research are able to produce (forecast), together with the broad array of applied technological methods and products to which these structures have contributed. Science is both more limited and less limited than the scientific realists, who *take* objective truth as the ideal toward which science aims, would have it.

On the one hand, science is more limited than *external* scientific realists claim, because the ultimate measure of progress is the coherence (precision, reliability, and correlation) of the resulting hierarchies of objective artifacts. As Lloyd (1995, 365–366) notes, even such a methodological formalist as Carnap denied that science was capable of discovering what is Really Real.[18] There is no objective, meaningful question of whether mu-mesons *really* exist or of whether—to use one of Kourany's (2003) examples—it *really is the case* that "women are inferior to men in mathematical ability."

On the other hand, science is less limited than *external* scientific realists claim, because the observational and computational artifacts produced by science are technological achievements of modern human culture—they are made to be true. The images and logic events produced by the projects

discourse." As Kuhn has so convincingly argued, the perceptions and intuitions required in order to construct and calibrate modern scientific instruments and to deduce and test the empirical consequences of modern scientific theories are essential to the discipline, though they are restricted to those who have been "enculturated" in the scientific practice.

18. See, especially, Carnap (1950). His primary purpose in that article was to counter the sort of view that Lloyd (1995) refers to as *ontological tyranny,* but the same type of argument, based upon the distinction between *external* and *internal* questions, also counters the viewpoint that she refers to as *type/law convergent realism.*

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of twentieth century particle physics and the explicit correlations produced in measuring toxicity or identifying factors of human intelligence reflect human social values—they are products of modern material and social technology. Science is not just in the business of representing (of enabling us to *take true*) what is Really Real: science is also in the business of constructing a system of artifacts that will enable us to more effectively achieve (to *make true* or to forecast) our human aspirations.[19] That is why it is so important that we recognize and reject the viewpoint that Lloyd refers to as a double standard.

At the same time, it is important to recognize that modern science embodies an objective methodology that sharply differentiates it from other social institutions. Through the development and use of more effective concepts and methods science has increased the openness and transparency of many aspects of our social and political practice. To the extent that scientific data can be collected and analyzed and scientific

19. An anonymous referee, who has provided me with perceptive and constructive comments on earlier drafts of this paper, has argued—referring to this passage—that "... the author is perhaps implicitly denying his or her own thesis ... This sentence suggests that science is after all non-exclusively in the business of enabling us to take true what is Really Real (which seems to have been dismissed as nonsensical in the previous paragraph) . The sentence also makes use of a distinction between taking true and making true that has previously been denied." But this criticism is a variant of the "false dilemma" that has so often been directed against antirealists: either there is an objective, Really Real world that exists independently of the process of inquiry or else the world is simply a construct designed and created by that process. The map analogy is suggestive of the first horn—science is constructing a map that *represents* an independently existing Reality. But the blueprint analogy is not intended to suggest the second horn. Blueprints often fail: there is a scope and limit to what any individual, social institution, or biological or physical system in general can succeed in *forecasting.* To say that science is in the business of *making true* rather than simply *taking true* is not to say that "anything is possible." A principal objective of this paper has been to establish a middle ground between the extremes of external objectivism and internal subjectivism. The artifacts that are the *effective* products of scientific inquiry—including empirical and theoretical measures of precision, reliability and correlation—are Really Real: they are not merely reflections of any individual or group's hopes, fears or expectations. I chose the example of twentieth century particle physics to illustrate the increasing role of *effective methods* in science because of the deep influence that my early experiences at the Berkeley Radiation Lab had on my philosophical conception of science. However, particle physics is perhaps the least convincing context in which to illustrate the other thread of my argument: the contrast between *representing* and *forecasting.* I eliminated an example drawn from economics—the question of whether the Consumer Price Index (CPI) is a *valid measure* (cf. Section 2.2) of a Really Real construct or magnitude—from an earlier draft in response to a criticism that "... the use of the CPI illustration looks like an end-run around some difficult questions ." In fact, it seems to me that various techno-scientific developments in biology provide the most fertile ground for illustrating the claim that scientific inquiry, in limited but significant ways, constitutes the world that it investigates. And that is one reason for emphasizing the arguments of feminist philosophers of science, such as Lloyd and Kourany.

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theories tested without relying upon subjective human perception and judgment, to that extent there is a neutral scientific method.[20] Since this neutrality has a fundamental social and epistemic value, science progresses when "higher levels of communicative discourse" are reached by transforming subjective judgments regarding the generation and reduction of data or the testing of theories into objective decision procedures that are automatic or mechanical. Yet, there is no algorithm for constructing and evaluating algorithms—no effective methods for designing and calibrating measuring instruments or for creating and testing constructs or theories. So despite the expanding role of effective methods in scientific practice—the application of which enhances the explicit, internal objectivity (transparency and neutrality) of scientific accomplishments—the endogenous process of generating and selecting those effective methods is deeply imbued with human values, both epistemic and social.

20. As Brandom (1994, 402–403) points out, making claims *explicit* renders them subject to rational dispute in a way that they are not so long as they remain *implicit* in practice.

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