In our second week, we considered the value-free ideal of science, and the way it plays into feminist critiques of science. We concluded that a lot of the arguments for a value-free picture of scientific inquiry revolve around some notion of impartiality—or a method which could somehow be totally extracted from its context and all the value-judgments in that context of human activity, to be derived strictly and exclusively in accordance with logic and observable evidence. This week, we are going to look at how this value-free ideal has faced challenges over the course of the twentieth century, and especially how it was challenged by feminist epistemologists and philosophers of science in the 1980s and 1990s. We'll come to see that these debates and challenges all revolved around the idea, which we saw in Reichenbach's early work in the 1930s, that there are internal and external workings of science—logical elements of scientific work that are clearly distinguishable from its sociological and psychological elements.

I begin with some ground we already covered in week 2: the logical empiricist tradition of the 1920s and 30s, comprising theorists such as Hans Reichenbach some of whose work you read. Logical empiricists believed there to be "an algorithm or set of algorithms which would permit any impartial observer to judge the degree to which a certain body of data rendered different explanations of those data true or false, probable or improbable" (Laudan, 1984, p. 5). If at any point, scientists held different and conflicting theories, all they would require was to consult the evidence, along with the algorithmic rules of inference from evidence to theory, in order to determine which theory was objectively better supported. In this way, scientific method represented an objective means for arriving at universal agreement on an accurate, factual representation of nature, free from the distortions of personal perspective. The tenability of this version of the value-free thesis depends on "whether the canons of scientific inference dictate assignments of minimum probabilities in such a way as to permit no differences in the assignments made by different investigators to the same set of alternative hypotheses" (Levi, 1960, p. 357). In other words, can two competent scientists rationally disagree on the theory best confirmed by the same evidence? As this section will show, ongoing debates on the topic of values in science can be seen as a sequence of moves and counter-moves in an attempt to challenge or salvage the notion of a scientific method that could serve as a universal formula for delivering any two competent inquirers to the same scientific theory on the basis of the same evidence.

The logical empiricist notion of science was a rule-governed, mechanical method inevitably bringing any given inquirer to the same theoretical position on the basis of the same evidence. Carnap (1959) attempted to specify a precise methodology for the derivation of scientific theory from evidence by analyzing language and placing restrictions on what counts as a meaningful statement. His theory of science attempted to show that valid scientific knowledge was derived from the rules of logic and mathematics, applied to foundational, value-free statements of evidential observations, or "protocol sentences." "Any statement," Carnap claimed, ". . . which does not fall within these categories becomes automatically meaningless" (1959, p. 76). Any statement (or corresponding belief) inflected with value-judgments or other forms of subjectivity was therefore definitionally excluded from the operations of scientific theory construction, guaranteeing a scientific method insulated from the influence of values. Reichenbach undertook a similar project with his "rational re- construction" of scientific inquiry. He charged epistemology with the task of separating the "context of discovery"—in which were found all the sociological elements of scientific process—from the "context of justification"—in which were found the "justifiable operations" of science "bound to factual knowledge," allowing his philosophy of science to "separate the arbitrary part of the system of knowledge from its substantial content, to distinguish the subjective and the objective part of science" (1938, pp. 6–7, 15). Scientific method, in these logical empiricist characterizations, involved clearly defined rules enabling scientists to agree on the truth, falsity, or likelihood of a given theory based purely on the evidence, without the incursion of individually variable, socially or politically influenced decisions. Logical empiricists accepted that scientists make value judgments in conducting their research, but they rejected the idea that such judgments enter into "the scientific method as such," relegating these judgments instead to the "sociological" elements of science as practiced. The respectability of the logical empiricist tradition in early- to mid-twentieth century philosophy established a strong basis for claims of the value-neutrality of scientific knowledge, based on its derivation using precise scientific method.

A major challenge to this view arose in the 1950s in the works of C. West Churchman and Richard Rudner, who attempted to show that "scientists as scientists do make value judgments" (Rudner, 1953, p. 2; Churchman, 1956). Rudner argued that a constitutive part of the scientific method was the acceptance or rejection of a hypothesis. Since a scientific hypothesis is never completely verified by the evidence, a certain degree of doubt—or possibility of error—inevitably remains. The scientist must, therefore, determine whether the evidence supports the hypothesis to a sufficiently high degree of probability to warrant the acceptance of the hypothesis. Rudner claimed that the decision as to how high a degree of confirmation is appropriate or necessary is "a function of the importance, in the typically ethical sense, of making a mistake" (1953, p. 2). For example, "if the hypothesis under consideration were to the effect that a toxic ingredient of a drug was not present in lethal quantity, we would require a relatively high degree of confirmation or confidence before accepting the hypothesis—for the consequences of making a mistake here are exceedingly grave by our moral standards" (ibid., p. 2). If, on the other hand, we were testing a hypothesis concerning defective belt buckles, we would require a much lower degree of confirmation. Therefore, Rudner claims, if the scientific method intrinsically involves the acceptance and rejection of hypotheses, then it also intrinsically involves value-judgments of the ethical sort. This aspect of scientific methodology would lead different scientists to different decisions from the same evidence, depending on their ethical leanings, thereby introducing subjective bias and value-judgments into scientific methodology.

While Rudner convincingly argued that accepting a scientific hypothesis for use in a practical objective would require a value-judgment about the importance of the consequences of error, the Churchman-Rudner position did not quite succeed in overturning the value-free ideal of science as posited by the logical empiricists. Responders argued that value-judgments about the consequences of error actually occur at a point of contact between 'science per se' and the practical applications of science in society, rather than within scientific method itself. They argued that value judgments are made by social agents before the scientist begins or after the scientist has completed their procedural task of pursuing scientific confirmation. This argument was made in at least two different ways. One response agreed that accepting or rejecting hypotheses involves assessments of risk, but denied that accepting and rejecting hypotheses is intrinsic to scientific method. This involves distinguishing probabilities (or degrees of confirmation) from utilities, since a hypothesis "will be relevant in a great diversity of choice situations among which the cost of a mistake will vary greatly" (Jeffrey, 1956, p. 242). For example, a polio vaccine may be intended for human children or for pet monkeys, but nothing in the hypothesis, "This vaccine is free from active polio virus," includes information on its intended use. Therefore, the proper task of the scientist qua scientist is simply the assignment of probabilities to the hypothesis, while the subsequent decision whether to accept or reject it is left to a social agent, external to the scientific method, who will evaluate whether the degree of certainty reported by the scientist is appropriately high, given the importance of the consequences of error. Another response acknowledged that the scientist qua scientist accepts or rejects the hypothesis, but affirmed the possibility of accepting or rejecting a hypothesis in an open-ended situation where "practical objectives are difficult to specify" (Levi, 1960, p. 350). Accepting or rejecting a hypothesis would therefore be separable from the decision to act in a given situation on the basis of that hypothesis, removing the necessity of a value-judgment as to the consequences of error. Different scientists qua citizens may very well make different decisions evaluating practical risks—decisions inflected by their individual personalities and politics—but they will be forced to agree on the degree of confirmation of a given theory based on scientific method. This leaves the objective scientific method intact, and science appropriately insulated from subjective, social, and political values by the procedural nature of its method.

The Churchman-Rudner argument offered some evidence for a highly limited involvement of value-judgments in science. Everything in the scientific method remained universal and value-free, except the decision as to the appropriate degree of confirmation or strength of evidence necessary for the acceptance of a theory. However, as we've seen, this aspect of scientific inquiry was easily marginalized and assigned to external agents operating outside scientific methodology proper. A much more radical challenge to the value-free ideal was offered by Thomas Kuhn in his Structure of Scientific Revolutions, where he demonstrated the "insufficiency of methodological directives, by themselves, to dictate a unique substantive conclusion to many sorts of scientific questions" (2012, p. 4). An inquirer who is adequately following scientific methodological directives, Kuhn claimed, may nevertheless "legitimately reach any one of a number of incompatible conclusions" (ibid., p. 4). In this way, Kuhn challenged the procedural nature and universality of scientific method, by suggesting that the development of a theory, as well as its acceptance or rejection, was influenced by elements that were "apparently arbitrary" and, importantly, differed from one inquirer to another according to their "individual makeup" (2012, p. 4). This arbitrary element of scientific method not only affected individual scientists, but was responsible for the legitimate disagreements between different scientific communities across time. Such variance, then, was intrinsic to the scientific method; it was neither an illegitimate intrusion of subjective, social, or political values, nor localizable to points of contact between a logical, rule-governed method and a value-burdened society.

In later clarifications of his theory, Kuhn identified five criteria operative in scientific method, functioning "not as rules, which determine choice, but as values, which influence it" (1977, p. 331).

These criteria included "accuracy, consistency, scope, simplicity, and fruitfulness" (ibid., p. 322). That is, the theory should be in agreement with experiments and observations; it should be consistent with itself and other currently accepted theories; its consequences should extend beyond the observations it was initially designed to explain; it should bring order to phenomena that might otherwise seem disparate and confused; and it should open up paths for further research. Individual scientists may legitimately and rationally differ as to whether and to what extent these criteria apply to concrete cases: for example, for a scientist choosing between the geocentric or heliocentric theories, simplicity may mean the amount of "computational labor required" to apply the theory to predict the location of the planets—which would favor geocentrism—or it may mean "the amount of mathematical apparatus" needed to explain the planets' qualitative features—which would favor heliocentrism (ibid., p. 324). Additionally, two of these criteria may come into conflict, and a scientist would have to decide which criterion to favor in terms of theory choice. Therefore, two scientists "fully committed to the same list of criteria for choice may nevertheless reach different conclusions" (ibid., p. 324). Moreover, different fields and periods of science had legitimately applied these criteria differently and attached different relative weights to them. While the tradition in philosophy of science had previously held that the rules of scientific method were unambiguous and fully determined, such that any differences of choice could be ironed out, Kuhn demonstrated that science was "a decision process which permits rational men to disagree" (1977, p. 332). Differences of theory choice and the in- fluence of subjective preferences were, for Kuhn, part of the "essential nature of science" (ibid., p. 330). In the absence of a universal, determinate method, science was now an institution that could produce a variety of different theories from the same evidence, introducing subjectivity into scientific knowledge.

Although philosophers of science largely came to accept Kuhn's characterization of a less deterministic scientific method, there has been a great deal of resistance to the description of its indeterminate criteria as 'values,' or to the equivocation of these 'values' with the kinds of social and political values that threaten the impartiality and authority of scientific knowledge. Some have sought to defend the value-free ideal of science by creating a categorical separation between epistemic values, which are conducive to the discovery of objective truth, and non-epistemic values, which include subjective preferences and biases. This is the kind of argument we saw in the McMullin reading. (Douglas, 2009; McMullin, 1982). Arguments for this distinction state that, although different scientists make different choices due to differences in their individual training, preferences, character, and background, there is still a clear dividing line between legitimate criteria for theory choice, which will leave intact the impartiality of science, and illegitimate criteria, which would sully the objectivity of science with social and political values. Epistemic values "promote the truth-like character of science"; non-epistemic values are those that promote any other kind of goal, including political, moral, social, and religious goals (McMullin, 1982, p. 18). In these accounts, Kuhn's criteria for scientific method—accuracy, consistency, scope, simplicity, and fruitfulness—are epistemic values oriented towards the ascertainment of truth, and do not challenge the objectivity of science. Non-epistemic values, such as the consistency of a scientific theory with a favored religious ideology, are argued to be clearly distinguishable from non-epistemic values. Therefore, whenever we see non-epistemic values operating in scientific inquiry, we can identify them as straying from proper scientific process, and identify the resulting knowledge as non-objective. Insofar as non-epistemic values operate in scientific inquiry, they are "gradually sifted out" by various processes such as replication of experiments, extension of theories, testing of theoretical moves, which are "designed to limit the effects. . . of fraud and carelessness, but also of ideology" (McMullin, 1982, p. 23). If we can distinguish epistemic values from non-epistemic values, and procedurally check and remove the latter's involvement in the production of scientific knowledge, then scientific method is still insulated from arbitrary value-judgments. In that case, even though scientific theories may legitimately differ, the social and political neutrality of scientific knowledge remains intact, and so does its impartiality and authority.

In the 1980s and 1990s, feminist philosophers of science challenged the distinction between epistemic and non-epistemic values in science. It is not clear, they argued, that any guarantee is possible that non-epistemic values will not enter into scientific methodology. In a detailed discussion of scientific studies of sex differences, Longino describes multi-faceted ways in which contextual (or non-epistemic) values and constitutive (or epistemic) values are "inextricably linked to each other" (Longino, 1990, p. 134).

In her case studies, Longino argues that descriptions of empirical observations were influenced by sexist assumptions, including: caricatures of lesbian sexuality; assumptions of males' mathematical superiority; and most significantly, the assumption that "there are sex- appropriate and sex-inappropriate behaviors" (ibid., p. 131; Longino, 1987, p. 58). These value-laden descriptions would then serve as evidence in the assessment of theories of the hormonal-biological determination of gender differences. If descriptions of the data serving as evidence contained politically-biased assumptions, then even the 'epistemic' values—for example, in determining the hormonal-biological model as accurately accounting for the evidence—carry political bias or non-epistemic values in their application. Additionally, Longino found that areas of research often employ explanatory models which predetermine what sort of phenomena can figure in explanations, as well as the sort of relationships those items can bear to the explicandum. These explanatory models "serve as background assumptions against which data are ordered, in light of which data are given status as evidence for particular hypotheses and as a context within which individual studies gain significance" (Longino, 1990, p. 135). In the neuro-endocrinological studies she considered, a host of normative background assumptions were in place which "mediated the inferences from the alleged data" to the confirmation of the hypothesis that pre-natal and perinatal hormones are significant determinants of behavioral gender differences (Longino, 1987, p. 58). To adopt and extend the explanatory "linear-hormonal model," in which "there is a one-way causal relationship between pre- or post-natal hormone levels and later behavior or cognitive performance" (ibid., p. 58), a variety of assumptions needed to be taken. For example, in order to take the correlation between increased testosterone and increased mounting behavior in mice as confirmation of the hypothesis that athleticism and social dominance in male children is causally determined by their increased exposure to testosterone as infants, experimenters would need to assume: the general reliability of the inference from hormonal testing on animals to conclusions about human behaviors; the equivocation of animal behaviors such as mounting and lordosis with human child behaviors such as athleticism and social dominance; the negligability of interference of adults or cultural expectations in directing gendered behaviors in children; and the negligability or absence of the human capacities for "self-knowledge, self-reflection, self-determination," along with; the passivity and lack of self-awareness in human gendered behaviors (ibid., pp. 58–59; Longino, 1990). Any of these assumptions, Longino argued, could, or perhaps would, have been rejected if the scientists were not socially or politically predisposed to accept hormonal explanations of human gender-based behavioral differences.

The conclusion arising from Longino's extended critique of this research program is that, "[a]bsent any background theory or assumptions, there is not much one could project from this data" (Longino, 1990, p. 155); instead, "the confirmation of hypotheses and theories is relative to the assumptions relied upon in asserting the evidential connection" (Longino, 1987, p. 55). At times, "contextual features [or non-epistemic values] have facilitated the use of given data or observations as evidence for some hypothesis" (Longino, 1990, p. 82), making the separation of epistemic and non-epistemic values in the scientific method significantly more problematic than some theorists anticipated. Longino's point is not that research into hormonal determinants of human behavior is bad science. Instead, she claims that this use of contextual values is an ineliminable feature of many paradigmatic instances of scientific inquiry, without which "early modern science would not have gotten off the ground" (Longino, 1987, p. 56). If contextual considerations are ineliminable, as Longino claims, then so are the value-laden considerations that sometimes accompany them, and there is no formal basis on which to insulate scientific method from social values. With even the most conscientious researchers at work, scientific methods remain vulnerable to the intrusion of political ideologies, compromising the impartiality (as formerly understood) of scientific knowledge.

An even stronger critique of the epistemic/non-epistemic divide suggested that the distinction cannot sensibly be maintained. Extending Longino's critique, another feminist epistemologist, Phyllis Rooney, argued that "cultural and social values can in time. . . become encoded into constitutive features of the rationality and objectivity of particular scientific endeavors, into features that are genuinely epistemically compelling for given scientific communities" (1992, p. 21). In the case of research programs exploring the biological-hormonal determinants of behavioral gender differences, the social and political importance of gender differences meant that, instead of being a proper explicandum, "gender dimorphism became constitutive of the understanding of biological functioning, and thus in effect became constitutive also of the 'constitutive' value of the simplicity or fruitfulness of the linear-hormonal model itself insofar as it was constructed to 'explain' biological determinism" (ibid., p. 18). In other words, gender differences did not operate as a purely epistemic factor in accepting the linear-hormonal model on the basis of the latter theory's simplicity in bringing gendered behavior and biology un- der the same explanatory umbrella. Rather, gender differences were worked into theories of biological determinism "right from the start" as an issue of social and political interest, and that same social and political importance attributed to gender differences established the linear-hormonal model's ability to 'explain' them as a good epistemic reason for accepting the model. What we take as 'simple' or 'fruitful' in a theory, it seems, is impacted by the culturally-determined value of what features of the world that theory is, respectively, combining or opening up to future research.

If science inherently involves not just epistemic, truth-oriented values, but also non- epistemic, socially-oriented values, with little basis on which to distinguish the two, then it can stake no claim to impartiality or neutrality on the basis of a methodological procedure. The methodological principles of scientific inquiry are instead relative to social and political values, and therefore debatable to a significant extent. Any criterion we utilize to evaluate a scientific hypothesis will involve a combination of truth-oriented and value-oriented criteria. If this is the case, it would be not only legitimate, but positively advisable, to scrutinize and reconsider the standards and procedures along which scientific inquiry is conducted to make scientific knowledge more conducive to politically desirable ends. This is precisely what Longino proposes in her later work, in which she describes alternative, feminist methodological criteria that we ought to apply in order to improve science by aligning it with progressive politics. She takes Kuhn's epistemic values—empirical adequacy, internal and external consistency, simplicity, breadth of scope, fruitfulness—and suggests that some of them could be substituted with more politically felicitous methodological values. Instead of consistency with other presently accepted theories, science could evaluate a hypothesis according to a standard of novelty, whereby a hypothesis is desirable for "postulating different entities and processes, adopting different principles of explanation, incorporating alternative metaphors, or. . . attempting to describe and explain phenomena that have not previously been the subject of scientific investigation" (Longino, 1996, p. 45). Novelty, Longino argues, would be a better methodological value, because "mainstream theoretical frameworks" are not adequate to solve human problems, being riddled with androcentrism, heterosexism, and other politically unacceptable biases. In place of simplicity and breadth of scope, science could adopt standards of ontological heterogeneity and mutuality of interaction. Rather than taking an ontological position in which "[d]ifference must be ordered, one type chosen as the standard, and all others seen as failed or incomplete versions" (Longino, 1996, p. 47), and treating differences as "eliminable through decomposition of entities into a single basic kind" (ibid., p. 46), a science conducted according to a standard of ontological heterogeneity would favor theories which approach heterogeneity as a resource in the natural world. This would build a natural ontology consistent with egalitarian politics, that "permits equal standing for different types" (ibid., p. 47). Rather than interpreting relationships between entities as unidirectional, a science conducted according to the standard of mutuality of interaction would build a natural ontology in which multiple parties to an interaction would be active rather than passive. Since "[a]symmetry of agency in the physiological context is used to naturalize asymmetry in the social," this would prevent justifications of sexism emerging from science (ibid., p. 47).

Longino also suggests pragmatic criteria in the evaluation of scientific hypotheses, including applicability to current human needs, which would bring about a scientific practice accountable to its effects on society, minimizing science's politically damaging effects; and diffusion of power, which would "give preference to research programs that do not. . . limit access to utilization and participation" but instead empower individuals to make decisions and locally implement scientific and technological innovations (ibid., pp. 48–49). Longino is clear that none of these alternative criteria are intended to give license to depart from the criterion of empirical adequacy, which she states should be common to any science.

As uncomfortable as it might sit with those accustomed to thinking that a value-independent method could "adjudicate scientific disputes" and avoid pollution from personal, social, or political values, it is reasonably clear from a century of failed attempts to delineate an insulated, autonomous, value-free scientific method that this hope must be abandoned. Not only has this narrative of value-free scientific methodology been manipulated to excuse highly politicized research programs, as an earlier section outlined, but theorists of science from Rudner to Kuhn to Longino have demonstrated the variety of ways in which this position is factually inaccurate and logically untenable. However, an acceptance of Longino's new scientific methodological values does not come easily to most people. Firstly, it is politically dangerous—as we've seen—to blithely disregard the impartiality or authority of scientific methods. Such approaches, while undoubtedly intended for progressive purposes, can be co-opted by those seeking to ignore or reject scientific evidence and it accompanying policy imperatives for their own personal power and profit. But from a more logical point of view, Longino's suggestions on a new improved scientific method leaves us with a disquieting doubt as to whether a science favoring novelty, ontological heterogeneity, and mutuality of interaction over simplicity and external consistency will be able to perform as well as a science conducted according to presently accepted criteria. It is difficult to accept that there is nothing special about the scientific method, or that any other method, chosen for its coherence with a progressive political ideology, will do just as well. We have not yet been offered an explanation as to the predictive power and technological success of science under the current methodological criteria, or a reason to believe these will not be compromised under a new, feminist science.

Can we have a theory of science which helps us to understand how current scientific methods can be considered authoritative on the basis of their broad and generalized success, at the same time as showing how we can hold scientific research accountable for its broader social and political success? This is a very serious question in feminist philosophy of science, and the philosophy of science and values, still to this day.

Next week, we'll look at some examples of how values have influenced the conclusions scientists have arrived at in their research.