

Reply to Giere*

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In his “A New Program for Philosophy of Science?”, Ronald Giere expresses qualms regarding the critical and political projects I advocate for philosophy of science—that the critical project assumes an underdetermination absent from actual science, and the political project takes us outside the professional pursuit of philosophy of science. In reply I contend that the underdetermination the critical project assumes does occur in actual science, and I provide a variety of examples to support this. And I contend that the political project requires no more than what other academic fields even in science studies are already providing.

In “A New Program for Philosophy of Science?” Ronald Giere sheds much light on the intended message of my “A Philosophy of Science for the Twenty-First Century.” But he also indicates where shadows need attending to. Allow me to oblige.

First, regarding the program for philosophy of science I am advocating, Giere notes that it includes both a naturalistic project and a critical project, but is unsure whether it also includes a political project. It does. If it does, he continues, then I am “envisioning a professional philosophy of science so different from that currently practiced that professional criticism seems pointless.” Perhaps so, but the different sort of philosophy of science I am envisioning is not so very different from other currently existing academic fields. Think of economics and political science, for example, whose members often play at least advisory roles in governments here and abroad, in organizations like Amnesty International, in labor unions, and in various civil rights organizations. The different sort of philosophy of science I am envisioning is not even so very different from other currently existing academic fields in science studies. Consider the history of science, for example. Beginning in the 1950s historians of science have testified before congressional committees on the desirability of creating a Department of

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Science (A. Hunter Dupree), on the tension between democratic and elite science (Daniel Kevles), on ethics in science (June Goodfield and Dorothy Nelkin), on the general value of outside experts trained in the social sciences in the formulation of science policy (Alex Roland), and on many other issues related to federal science policy, and they have also provided information and policy advice to the National Aeronautics and Space Administration and the National Science Foundation as well as private corporations. Reflecting on these activities and on current and future policy needs, historian of science John Heilbron has called for research and modes of disseminating the results of that research—from philosophers and sociologists of science as well as historians—that “contribute to the formation of a balanced and humane national science policy” (Heilbron 1987, 563). What I am proposing for philosophy of science fits right in with this goal.

Giere focuses most of his attention, however, not on this political project, but on the critical project I advocate for philosophy of science—that of appraising science in terms of an egalitarian ideal of human flourishing. Giere voices no complaint regarding parts of this project—for example, appraising research topics in terms of such an ideal, and funding them accordingly, or research assumptions, or methods of data collection, or epistemic values. He even observes that, with regard to the appraisal of research topics, I join “many others”—though presumably not many others in philosophy of science, the point I was pressing—in calling for such appraisal, and he also observes that we have strong reasons on our side. But Giere does complain about the appraisal of research results in terms of an egalitarian ideal—more specifically, the favoring of hypotheses that support egalitarian goals over those that do not in cases of underdetermination. To this point he notes that “there are many arguments that the extent of such underdetermination is not nearly so great as Kourany’s project assumes (see, e.g., Laudan and Leplin 1991).” But of course, there are also many arguments that challenge those arguments (see, e.g., Kukla 1998, in which Kukla spends much time systematically critiquing Laudan and Leplin 1991). Abstract arguments aside, the scientific and philosophical literatures contain many examples of underdetermination in “real scientific practice,” Giere’s ultimate concern. And though some of these examples are drawn from physics—for instance, Cushing (1994) on Quantum Mechanics—many are drawn from the social sciences and biology, the fields of interest to us at present. To cite some of the “many examples that could be used to demonstrate the Duhem-Quine problem at work in economics,” Cross (1982) has applied the Duhem/Quine thesis to macroeconomics, Smith (1989) to experimental economics, and Hands to demand theory (Hands 2001, 98). Glymour (1997, 1998) lays bare the Duhem/Quine problem in social scientific re-

search in general, and Herrnstein and Murray's *The Bell Curve* (1994) and IQ research in particular, and Wylie (1985, 1988) does the same for archaeology, though Glymour's and Wylie's immediate aims in these works lie elsewhere. And Giere himself in his response has pointed to one of the applications in biology—Longino's critique of the linear-hormonal research program and its comparison with the selectionist research program in her *Science as Social Knowledge* (1990). In this last case, in addition, Longino illustrates the kind of choice to which feminist principles lead.

But even if scientific decision making *is* sometimes genuinely underdetermined by the available data in real scientific practice, Giere is still loath to encourage scientists to favor those hypotheses that support egalitarian goals over those that do not. His reason is that hypothesis selection is "supposed" to be "strongly based" on empirical data—scientists are "supposed" in these cases simply to withhold judgment. But this is too quick. Never mind that a choice favoring the more egalitarian option *will be* a choice "strongly based" on empirical data, since all available data will have been taken into account, withholding judgment is frequently not feasible in real scientific practice, at least the real scientific practice that concerns us here, the scientific practice that bears on egalitarian goals. Take an example currently in the news—hormone replacement therapy (see, e.g., Kolata and Petersen 2002, and Dranginis 2002). Since the 1960s estrogen, or a combination of estrogen and progestin, has been touted as an effective preventive not only for menopausal discomforts such as hot flashes and night sweats but also for such diseases of postmenopausal women as osteoporosis and heart disease. However, for nearly forty years—up until a few weeks ago, in fact—the evidence for the latter (disease prevention) claim was equivocal: while there were dozens of studies of postmenopausal women, as well as dozens more of animals and cells, that appeared to support the claim, there were also problems with those studies (they were only observational, they furnished only indirect support, etc.), some study results were negative, and some studies suggested that hormone replacement therapy had unacceptable side effects (e.g., significantly raised the risk of other diseases such as breast cancer). Nevertheless, there was no possibility of "withholding judgment" here. For one thing, only with Bernadine Healy, the first woman head of the National Institutes of Health, and the huge, expensive Women's Health Initiative she fought to set in motion in 1993, was there the possibility of getting any more definitive kind of evidence. And in the meantime there was the need to act, to either prescribe hormones or not prescribe hormones, to either try medically to protect postmenopausal women from heart disease, osteoporosis, and other ills (colon cancer, Alzheimer's disease, etc.) or not to try. (Significantly, the principal investigator of the long-term, randomized, controlled study that Healy made possible, the study that was not to

yield results until at least 2004 but was stopped early only a few weeks ago, reported to *The New York Times* that when the study was being planned doctors and researchers criticized it as unethical because it would include a control group of women taking placebos who would thus be denied the benefits of hormone replacement therapy.) The upshot was that medical authorities embraced hormone replacement therapy as an effective disease preventive and recommended it at menopause for nearly all women, with the result that millions of healthy women were medicated for years, sometimes for life. At the same time feminist organizations such as the National Women's Health Network—a network of 300 organizations plus more than 8000 individual members—rejected hormone replacement therapy as an effective disease preventive for nearly all women.

Values were involved in both stands. Were these values, however, simply “ideological bias,” as Giere suggests (“In the end, it is the *explicit* appeal to ideology in the evaluation of hypotheses that makes this aspect of Kourany's critical project unacceptable . . . ”)? While it is hazardous to speculate regarding the values that influenced the medical establishment (though there is reason to believe that the pharmaceutical industry and its values played a significant role here), no speculation is needed regarding the values that influenced the feminists. Their values are clear enough in various publications (see, e.g., the National Women's Health Network 2000 and 2002 and their many prior publications, and Love and Lindsey 1998 and their publications before and after), and these values included items such as: menopause should be thought of, not as the onset of a deficiency disease requiring therapy, but as simply a normal transition to a new stage of life (notice that few have suggested that *men*, who also decrease their hormone production as they age, should be similarly thought of as diseased and hence in need of prolonged, possibly even life-long, hormone replacement therapy); and women should take charge of their own health and make life-style changes (exercise, diet, etc.) and seek out safe alternatives (e.g., plant estrogens from such foods as whole grains and beans) to drugs that have well-documented risks in order to protect themselves from heart disease, osteoporosis, and other diseases of old age. Since these values were fully justified in the works cited, they did not function as “ideology”—if by “ideology” Giere means (as he seems to mean) some kind of unjustified system of values.

But even if egalitarian values like the ones given above *were* fully justified, Giere is *still* reluctant to allow them to intervene in cases of underdetermination. The reason is that such intervention poses a “danger”: “In a democratic society, those duly elected or appointed to make the relevant decisions may not share [these] values. So decisions may well be made in conformity to [opposing values].” In short, if unjustified values can intervene in hypothesis selection, it is better—safer—not to have any values

intervene at all. But unjustified values can intervene as well in research topic appraisal and funding. Shall we then take the “safer course” and have no values intervene there either? Giere has found such an idea unacceptable. Then why not simply rule out the intervention of *unjustified values*—in both hypothesis selection and research topic appraisal and funding—not the intervention of values themselves? Actually we have no choice. Since values *do* intervene in science—in hypothesis selection and research topic choice as well as in assumptions and concepts and methods of data collection and in many other ways as well—and since no one has yet provided a workable strategy to screen these values out, we had better do all we can to make these values as justified as any other aspect of our scientific enterprise.

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