

H. E. DOUGLAS, *Science, Policy, and the Value-Free Ideal*. Pittsburgh: University of Pittsburgh Press, 2009.

For centuries, philosophy of science, and epistemology in general, have been mainly concerned with the ways of pursuing and securing knowledge about the world (whatever this *world* can be). The scientist has been depicted basically as an agent concerned about how to get the maximum amount of knowledge, or knowledge of the maximum possible epistemic quality (with the definition of *epistemic quality* usually left to the preferences of the philosopher), though historians, sociologists and anthropologists of science have justly insisted in the fact that researchers do actually have other, non-cognitive goals, and the pursuit of these can and does influence in a rather strong way the actions of scientists. Traditionally, the difference between the philosopher's and the social scientist's view of the goals of researchers has been presented as an absolute tension, with (some prominent) historians, sociologists and anthropologists claiming that the presence of non-cognitive goals demonstrates that the results of scientific research are not epistemically *objective*, and (some prominent) philosophers arguing instead that, for this same reason, non-cognitive goals must be kept apart from the daily activities of scientists. This position, the idea that *extra-scientific* values or interests should play the smallest possible role in science, is the thesis Heather Douglas refers to as the *value-free ideal*, and the one she criticizes in her recent book *Science, Policy, and the Value-Free Ideal*.

The main interest of the book lies, first, in showing (chapters 1 and 3) that this ideal has not always corresponded to the mainstream view in philosophy of science, but has a relatively short history (as well, we must add, as a long *prehistory*, from Francis Bacon to Max Weber, though this does not mean that the authors in this *prehistory* adhered to something like the precise expression the ideal took in the North-American, post World War II tradition of analytic philosophy). On the contrary, the value-free ideal would be a *construction* (perhaps more an *intellectual* than a *social* one, according to Douglas' detailed narrative) of the Cold War times. Just for offering us this interesting story, and refreshing our memories of the discussion on the roles of values in science that took place around the 1950s within the growing community of American philosophers of science, this book deserves to be read.

The second reason that makes Douglas' book interesting is that, contrarily to the image of the scientist typical in philosophy and epistemology (i.e., the disinterested pursuer of knowledge), on the one hand, and, on the other hand, in history and sociology of science (i.e., basically the constructor of an academic career), she points out to the essential role scientists have in modern societies as *advisors* (a role that has been taken into account more often in the literature on *science, technology and society*; see chapters 2 and 7). In short, the idea is that scientists not only *create* (or discover) new bits of knowledge, but they are (in principle) responsible for *other people accepting* this new knowledge. These other people would have behaved differently, had other theories, hypotheses or laws been raised to the status of *scientific facts*. Scientists *shape* the society they live in. Of course, this shaping can be achieved in a rather general and abstract way: e.g., through the inclusion in textbooks of bits of information (laws, formulae...) that engineers will employ decades or centuries later. But it can be accomplished in a

much more direct and powerful way: e.g., when scientists, individually or collectively, act as advisors of political or entrepreneurial bodies. According to Douglas (see esp. chapter 4), it is the increasing engagement of science and scientists in the configuration of *public policies* what mainly justifies to reconsider the role of values in scientific activity.

The core of Douglas' argument appears in chapter 5, where she introduces the distinction between *direct and indirect roles* that values may play in science, a distinction—she argues—is more relevant than the more typical one about what types of values (e.g., cognitive vs. non-cognitive values) do affect scientific research:

The values can act as reasons in themselves to accept a claim, providing direct motivation for the adoption of a theory. Or, the values can act to weigh the importance of uncertainty about the claim, helping to decide what should count as *sufficient* evidence for the claim. In the first direct role, the values act much the same way as evidence normally does (...) In the second, indirect role, the values do not compete with or supplant evidence, but rather determine the importance of the inductive gaps left by the evidence. More evidence usually makes the values less important in this indirect role, as uncertainty reduces. Where uncertainty remains, the values help the scientist decide whether the uncertainty is acceptable, either by weighing the consequences of an erroneous choice or by estimating the likelihood that an erroneous choice would linger undetected (...) A direct role for values at this point in the scientific process is unacceptable, but an indirect role is legitimate. (p. 96)

So, in a nutshell, a value acts in a *direct* way when it serves to justify a step in the reasoning process of scientists. In order to guarantee the objectivity of scientific knowledge, Douglas insists (chapter 6), it is necessary that only *evidence* (which, as far as I understand, is *not* a value) can legitimately act in a direct role when the reasoning step we are considering is that consisting in accepting or rejecting a theory, hypothesis or fact. Values can legitimately act in this role only in the *first* stages of a research process, i.e., to help decide what problem to tackle, what methods to use, and so on. This does not mean that *all* intromission of values at this stage is legitimate, of course, but that particular discussion about values—for that purpose and at this stage—is scientifically legitimate, and corresponds to the social and ethical responsibility scientists have as members of a society. This part of Douglas' argument does not certainly depart too much from the traditional interpretation of the value-free thesis, which also allowed that non-cognitive values and interests played an important role in these aspects of the research process.

The main contribution of Douglas' book is, of course, her thesis about the *indirect* role of values: values act in an indirect role when they serve to justify, not scientific claims directly, but the *criteria* to make decisions about what claims to accept or reject. These criteria are distributed all along the scientific process: what constitutes good evidence, what are the right experimental techniques, what are the good sampling methods, what are the right inferential norms, what margins of error are acceptable, when it is reasonable to suspend judgment, what is the weight that must be given to separate sources of evidence (each one possibly pointing in contradictory directions), and so on and so forth. The unescapable facts about science are, first, that acting according to a different configuration of these criteria can, and in most cases surely will, lead us to accept *different* scientific claims, theories, models or laws (or, if the case is extremely favourable to our epistemic goals, will lead us to accept more or

less the same claims but at a very different pace). And second, that the framework of methodological criteria or norms that is actually observed in a scientific community arise not like manna from heaven, but from the *choice* of its members (or at least, from their *collective* choice). So, scientists have the *responsibility* of deciding what framework of methodological norms are they going to choose, in order to conform their reasoning processes to them. This is reminiscent of what I have called in other papers the *constitutional approach* to scientific norms, though in my own work I have insisted more on the role that professional and cognitive interests may play in the constitutional choice of norms than in the role of ethical or political values. I think Douglas' book is particularly important, among other reasons, because it can help the community of philosophers of science and scholars in science studies to concentrate in this fundamental feature of the scientific process: its *normative* structuration in two levels: a *constitutional* one that refers to the criteria that help scientists determine which *moves in the scientific game* are *right* and which ones are *wrong* (with all the possible intermediate valuations in between), and an *ordinary* level that contains the much more numerous decisions governed by those criteria. Therefore, this book can be a very decisive step in persuading scholars that some of the most important questions they can ask about science are:

- 1) Which are the *norms* that actually govern each particular scientific discipline or community?
- 2) To what extent do these configurations of norms *overlap or conflict*?
- 3) *Why* are the accepted norms the ones they are?, and more importantly,
- 4) What norms do we, as *citizens*, should *prefer* that science in general or scientific research processes in particular were subjected to?

A philosophical study of science that —like *Science, Policy, and the Value-Free Ideal*— kept permanently in mind this type of questions would be of the greatest utility for the members of societies like ours in which scientific knowledge is having an increasingly stronger role and consuming more and more economic and human resources.

Having praised Douglas' book as an important contribution to the philosophy of science, I must, however, point out several shortcomings. First, it is not clear to me why Douglas exclusively concentrates on the effect that the choice of scientific norms may have on the production of cognitive *mistakes*. Obviously, scientists must be responsible for the errors they induce in others, but the implementation of a framework of methodological norms may have *other* effects on society as well. I do not see any reason why the valuation of these other effects is not important in the attribution of responsibility to scientists. Second, the book is not very illuminating in explaining how the choice of methodological norms will be exactly *derived*, at least in a partial way, from values (or, as I would like to add, interests).

Finally, Douglas concentrates, excessively from my point of view, on the North-American tradition of philosophy of science, and on the North-American history of interactions between science and politics. To non-American readers (and, I suspect, also for American ones not very familiar with the intricacies of the PSA history) this

seems somewhat parochial. For example, I find shocking that Jürgen Habermas is not quoted at all in a book on this topic, not to say Karl Popper, precisely the main advocate of the conventional nature of scientific norms.

Jesús Zamora Bonilla
UNED
jpzb@fsof.uned.es

JAVIER DE LORENZO, *Ciencia y artificio*. Oleiros (A Coruña): Netbiblo. 2009.

En este nuevo ensayo el matemático y filósofo Javier de Lorenzo expone su personal concepción de la ciencia moderna, defendiendo que se pueden explicar sus aspectos fundamentales a partir de lo que llama su “metáfora-raíz”. Una metáfora-raíz es en este libro una visión del mundo unitaria, exclusiva, no exenta de cierto carácter dogmático, de supuesta validez universal y que intenta imponerse sobre sus rivales. Y, según Javier de Lorenzo, es el mecanicismo la metáfora-raíz que ha hecho posibles tanto la ciencia que conocemos como la revolución sociopolítica y las actuales sociedades democráticas de los países occidentales.

¿Tan decisivo es el mecanicismo en la ciencia? No cabe duda de que la concepción mecanicista de la naturaleza propició el nacimiento de la nueva física en el siglo XVII y la acompañó durante siglos. Pero la relación no fue asimétrica. El atractivo del mecanicismo tuvo mucho que ver el éxito de la ciencia newtoniana, que es fundamentalmente una teoría del movimiento, una mecánica. El mecanicismo se vio reforzado con la tendencia a considerar la totalidad de los fenómenos naturales muy semejantes o, en el fondo, idénticos, a los fenómenos que tan bien conocía la física newtoniana. Por otra parte, resulta al menos muy discutible la idea de que las distintas ramas de la ciencia deban su origen y su existencia a una concepción mecanicista de la naturaleza. Baste recordar los duros golpes sufridos por el proyecto mecanicista laplaceano a lo largo del siglo XIX.

Sin embargo, el concepto de mecanismo del que parte Javier de Lorenzo es mucho más amplio que el habitual. Incluye, por supuesto, lo que llama “primado de la materia”, es decir, la hipótesis, según la cual, todo fenómeno natural es reducible a extensión y movimiento, y el “primado de la matemática”, que exige, entre otras cosas, la aceptación de un espacio definido geoméricamente y un tiempo continuo, lineal. Pero incluye también hipótesis de distintos tipos como, por ejemplo, las relativas a la independencia de la naturaleza respecto a nuestro conocimiento de ella, o a la capacidad humana para conocer de modo fiable las leyes de la naturaleza. Y, sobre todo, la metáfora mecanicista se convierte en el tipo de ideología que acabó venciendo a concepciones anteriores de la naturaleza que veían en ella cualidades ocultas o sobrenaturales y propiedades irremediabilmente cualitativas. Sólo teniendo en cuenta la amplia gama de creencias o hipótesis que caracterizan a este mecanicismo convertido en metáfora-raíz se entiende que pueda constituir “el marco necesario, imprescindible para la constitución del hacer científico” tal como hoy lo conocemos (p. 27).