Science, Technology and Society:
A Philosophical Perspective

Wenceslao J. Gonzalez (Editor)

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SCIENCE, TECHNOLOGY AND SOCIETY: A PHILOSOPHICAL PERSPECTIVE

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He has edited 20 volumes and published 70 papers. He is the editor of the monographic issues on *Philosophy and Methodology of Economics* (1998) and *Lakatos's Philosophy Today* (2001). His writings include "Economic Prediction and Human Activity. An Analysis of Prediction in Economics from Action Theory" (1994), "On the Theoretical Basis of Prediction in Economics" (1996), "Rationality in Economics and Scientific Predictions: A Critical Reconstruction of Bounded Rationality and its Role in Economic Predictions" (1997), "Lakatos's Approach on Prediction and Novel Facts" (2001), "Rationality in Experimental Economics: An Analysis of R. Selten's Approach" (2003), "From *Erklären-Verstehen* to *Prediction-Understanding*: The Methodological Framework in Economics (2003), and "The Many Faces of Popper's Methodological Approach to Prediction" (2004).

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Many of her books deal with ethical issues related to technological risk and their environmental consequences*: Nuclear Power and Public Policy* (1980); *Science Policy, Ethics, and Economic Methodology* (1984); *Risk and Rationality* (1991); *Ethics of Scientific Research* (1994); *Technology and Human Values* (1997); … Her works have been translated into Chinese, Czech, Dutch, French, German, Italian, Japanese, Korean, Norwegian, Russian, and Spanish. She is widely in demand as a lecturer by universities, governments, and industrial groups as well as National Academies of Science. She has been invited to prestigious colloquia such as the *Boston Colloquium for the Philosophy of Science* or the *Pittsburgh*

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Ramón Queraltó Moreno is professor of philosophy at the University of Seville. His realm of research is present tendencies in thought and especially philosophy of technology. He is a correspondent member of the *Académie Internationale de Philosophie des Sciences* and the editor of the journal *Argumentos de Razón técnica*. He is head of a research group on “Science, Technology and Society” of the High Council of Scientific Research (CSIC). He is the author of eight books, among them are *Mundo, Tecnología y Razón en el fin de la Modernidad* (1993), *K. Popper, de la Epistemología a la Metafísica* (1996), *Razionalità tecnica e mondo futuro. Una eredità per il terzo millennio* (2002), and *Ética, Tecnología y Valores en la sociedad global* (2003). He has published widely. Among his papers are “Does Technology ‘Construct’ Scientific Reality?” (1993), “Hypothèse, objectivité, et rationalité technique” (1996), “Since Indeterminacy: The New Picture of the Physical World at the End of Modernity” (1997), “Technology as a New Condition of the Possibility of Scientific Knowledge” (1998), “Scientific Realism, Objectivity and Technological Realism” (2000), and “Science as Technoscience: Values and Their Measurement” (2004).

THE RELEVANCE OF SCIENCE, TECHNOLOGY AND SOCIETY: THE "SOCIAL TURN"

Wenceslao J. Gonzalez

The emphasis on the realm of *Science, Technology and Society* or *Science and Technology Studies* may have the same degree of relevance that the "historical turn" had in the past. It is a "social turn" which affects philosophy of science as well as philosophy of technology. It includes a new vision of the aims, processes and results of scientific activities and technological doings, because the focus of attention is on several aspects of science and technology which used to be considered as secondary, or even irrelevant. This *turn* highlights science and technology as *social undertakings* rather than intellectual contents.

According to this new vision, there are several important changes as to *what* should be studied –the objects of research–, *how* it should be studied –the method– and what the *consequences* for those studies are. The new focus of attention can be seen in many changes, and among them are several of special interest: a) from what science and technology are in themselves (mainly, epistemic contents) to how science and technology *are made* (largely, social constructions); b) from the language and structure of basic science to the characteristics of *applied science* and the *applications of science*; c) from technology as a feature through which human beings control their natural surroundings (a step beyond "technics" due to the contribution of science) to technology as a *social practice* and an *instrument of power*; and d) from the role of internal values necessary for "mature science" and "innovative technology" to the role of *contextual* or *external values* (cultural, political, economic …) of science and technology.

This "social turn" is a move that covers a larger area and introduces a more radical scope than the preceding "historical turn", which was developed predominantly in the sixties and the seventies. On the one hand, *STS* enlarges the domain in comparison with the contributions made by Thomas Kuhn, Imre Lakatos, Larry Laudan … The role of historicity as a crucial element for the philosophical approach was analyzed mostly in the case of science. *De facto*, the major philosophers of that period paid little attention to technology. Furthermore, technology was customarily seen by them as an instrument that science uses for observation or experimentation. On the other hand, *STS* brings with it a more radical scope than the "historical turn," because that conception –including *The Structure of Scientific Revolutions*– still assumes that the internal contents of science have more weight than the external factors (social, cultural, political, economic …).

In addition, there is a further enlargement introduced by the "social turn" in comparison with the "historical turn." *STS* considers the contributions of several disciplines, among them practical ethics, policy analysis, legal studies, sociology of science and sociology of technology, economics of science and economics of technology … Thus, the "social turn" includes more scientific contributions than history of science and history of technology. But the main interest is not in the intellectual history, either of science (e.g., of scientific theories) or of technology (e.g., on the changes in the know how), but rather in contextual elements of the discoveries or improvements of science or technology (the search for fame, power relations, institutional traditions …).

Within the realm of *Science, Technology and Society* or *Science and Technology Studies*, this book focuses on the philosophical perspective. It attends to philosophy of science as well as to philosophy of technology. Thus, the papers analyze the sphere of scientific activity and the circle of technological doings, which includes the scientific-technological area of "technoscience." The volume takes the philosophical approach as complementary to the empirical studies on science and technology. From this angle, the analysis considers some aspects of the "social turn" and, as is usual in philosophy, it includes the component of critical attitude.

It is a book which belongs to *Gallaecia. Studies on the Present Philosophy and Methodology of Science*. This collection, published since 1997, seeks to analyze different issues of science and technology from a philosophical perspective. Two of the previous volumes are related to the topics of this book: *Scientific Progress and Technological Innovation* (1997) and *Science and Ethical Values* (1998). In addition, some relevant philosophers of science have also received attention in monographic publications: Karl Popper, Thomas Kuhn, Imre Lakatos and Larry Laudan. All the volumes are coordinated from the University of A Coruña (Spain) and until now they have been published in Spanish. The collection seeks to increase its presence in the international forum of ideas. This is the main reason for publishing this new book of *Gallaecia* in English.

Ferrol, 19 October 2004

Wenceslao J. Gonzalez
Professor of Logic and Philosophy of Science

I

Theoretical Framework

1. The Philosophical Approach to Science, Technology and Society
2. Objectivity and Professional Duties Regarding Science and Technology

THE PHILOSOPHICAL APPROACH TO SCIENCE, TECHNOLOGY AND SOCIETY

Wenceslao J. Gonzalez[1]

There is nowadays, through the "social turn" in philosophy of science and philosophy of technology, a *new panorama* in the philosophical approach to science and technology. For several decades, the previous ideas on scientific findings and technological contributions were frequently thought of as context-independent (mainly, as epistemic contents and instruments to control our surroundings), whereas the new vision presents a different picture where contextual values (cultural, political, economic, ecological …) have a central role in science and technology. Moreover, the landscape is now an interdisciplinary endeavor where the *empirical studies* on science and technology commonly accompany the *philosophical reflections* on scientific activity and technological doing.

Many features can be considered regarding this *philosophical approach* to science, technology and society connected to the "social turn." Looking forward to what this issue *is* and *ought to be*, the analysis will follow several steps: 1) to characterize the interdisciplinary endeavor on science and technology focus on the social setting; 2) to clarify the notions of "technoscience," "science" and "technology" because they –in one way or another– underlie all the discussions; 3) to make explicit the variations in the philosophical approach, which has moved from the "internal" constituents to the "external" factors both in philosophy of science and in philosophy of technology; 4) to specify the relation between science and society from a philosophical perspective, which includes the social dimension of science as well as the relevance of practice; and 5) to elucidate the nexus between technology and society from a philosophical approach, which requires us to take into account the social dimension of technology and the role of economic values in technology. Thereafter, there is a presentation of the structure and origin of this book and a posterior bibliography to complete the inquiry.

1. AN INTERDISCIPLINARY ENDEAVOR

Science, Technology and Society or *Science and Technology Studies* are two ways of referring to an interdisciplinary endeavor. *STS* combines the contributions of several disciplines and, accordingly, it uses different methodologies. Its object is not an isolated realm analyzed by a traditional kind of research, because it depends on views on science and technology developed in the last four decades.

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[1] I am grateful to Kristin Shrader-Frechette for her comments on this paper.

Indeed, *STS* has received increasing attention since the mid-1980',[2] when the discussion included explicitly a third term: "technoscience." It is also a period where philosophy of technology increased progressively its presence in the realm of *STS*,[3] connecting technology with new areas for philosophical research (issues related to bioethics, environmental concerns, social problems, policy discussions …).[4]

Since the constitution of *STS*, both philosophy of science and philosophy of technology have had a key role in this contemporary field. Their contributions are interconnected with contents of other disciplines. *De facto*, *STS* is a broad intellectual enterprise where several disciplines are involved: practical ethics, policy analysis, law, sociology, economics … The reason for this wide variety of contributions is clear: *STS* cannot be reduced to the theoretical study of science and technology, because it includes also a *practical dimension* as well as a *social concern*. In Europe the first aspect is still dominant, whereas in the United States the second facet has a central relevance.

Both names –*Science, Technology and Society* and *Science and Technology Studies*– are commonly used for the same subject matter. The *sense* of these expressions includes the assumption of science and technology as *human activities* in a social setting rather than two forms of mere knowledge. And the specific *reference* of these expressions goes beyond the intellectual outcomes or products of science and technology: it looks for those *concrete components* of science and technology which have repercussions in social life in different dimensions (ethical, political, sociological, economic…) Therefore, *STS* pays special attention to the empirical ingredients of both researches –scientific and technological–: it seeks their links to the lives of the citizens. Thus, the philosophical approach goes along with other aspects in *several contexts* (enviromental, political, legal, sociological, economic …) which should be considered as well.

In many ways philosophy of science and philosophy of technology are at the core of *STS*, because either the other disciplines are deeply embedded in the philosophical approach or they have at least a clear connection with some philosophical problems. Thus, insofar as there is this common ground –the philosophical roots– in this field, *Science, Technology and Society* or *Science*

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[2] Some of the most influential views on *STS* had already started before the mid-1980's, cf. BARNES, B., *Scientific Knowledge and Sociological Theory*, Routledge and K. Paul, London, 1974; LATOUR, B. and WOOLGAR, S., *Laboratory Life: The Social Construction of Scientific Facts*, Princeton University Press, Princeton (NJ), 1979; KNORR-CETINA, K., *The Manufacture of Knowledge. An Essay on the Constructivist and Contextual Nature of Science*, Pergamon Press, Oxford, 1981; and COLLINS, H. M., *Frames of Meaning: The Sociological Construction of Extraordinary Science*, Routledge and K. Paul, London, 1982.

[3] Cf. IHDE, D., "Has the Philosophy of Technology Arrived? A State-of-the-Art Review," *Philosophy of Science*, v. 71, n. 1, (2004), pp. 117-131.

[4] Cf. SCHARFF, R. C. and DUSEK, V. (eds.), *Philosophy and Technology: The Technological Condition*, Blackwell, Oxford, 2003.

and Technology Studies conforms an interdisciplinary endeavor rather than a multidisciplinary enterprise. The difference is neat: in the first case there is a base which –to some extent– is shared by the disciplines working on the field, whereas in the second case there is a collection of several disciplines dealing with the topics without a real connection between them.

But the kind of *philosophical approach* developed in *STS* is an enlarged vision of what it is usually understood as "philosophy." The philosophical traditions or schools which are very speculative have here some elbow room, because the reflection on the practical dimension of science and technology in the social setting requires close attention to the concrete phenomena. Moreover, the philosophical approach in *STS* is a richer view than previous ones: on the one hand, it is –to some extent– an expansion of philosophy of science and philosophy of technology through the emphasis in the external factors; and, on the other hand, *STS* deals with new problems which have appeared in contemporary society (ecological, ethical, political …). Consequently, the philosophical approach is open to new ideas, as we can see in many publications in this context (for example in problems such as risk and rationality).[5]

Besides philosophy of science and philosophy of technology, there are several disciplines –philosophical and scientific– involved in *STS*. Each one of these studies deals with an aspect which affects either the relation between *science and society* or the nexus between *technology and society*. Usually, these studies take into account their philosophical linkage insofar as they are included in *STS* rather than being developed on their own. These disciplines assume the *internal aspects* of science and technology (language, structure, knowledge, method …), but they put special emphasis on *external features* (social, historical, economic, political …). Among those studies of *STS* are practical ethics, policy analysis, legal studies, sociology of science and sociology of technology,[6] economics of science and economics of technological change … All of them have also, in one way or another, a bond to history of science or to history of technology.

STS includes a philosophical linkage in those studies. 1) *Practical ethics* was originally a philosophical study which has enlarged its realm to create new

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[5] Cf. Rescher, N., *Risk: A Philosophical Introduction to the Theory of Risk Evaluation and Management*, University Press of America, Lanham, 1983; Shrader-Frechette, K., *Risk Analysis and Scientific Method: Methodological and Ethical Problems with Evaluating Societal Hazards*, Reidel, Dordrecht, 1985; and Shrader-Frechette, K., *Risk and Rationality: Philosophical Foundations for Populist Reforms*, University of California Press, Berkeley, 1991.

[6] "Sociology of science" seems a better name than "sociology of scientific knowledge": on the one hand, sociology should take into account *more aspects* than knowledge, because science is also a human activity with aims, processes and results; and, on the other hand, "sociology of scientific knowledge" appears frequently as an expression of the *social constructivist* conception, which is a possible orientation of the sociology of science rather than the only one.

In addition, from a historical point of view, there is also an influential "sociology of knowledge" which has differences with present perspectives, cf. Mannheim, K., *Essays on the Sociology of Knowledge*, Routledge and K. Paul, London, 1952.

specialities, such as bioethics or environmental ethics.[7] 2) *Policy analysis* is also connected with philosophy of science and philosophy of technology insofar as they need several epistemological and methodological distinctions[8] (such as science and non-science, technics and technology, technoscience, basic science and applied science, oriented science and non-oriented science …). 3) *Legal studies* are usually interwoven with concerns about practical ethics and issues raised by policy analysis. This is the case in science (e.g., in the research about stem cells or in the case of human cloning) as well as in technology (e.g., in nuclear research). Laws on science and technology depend on philosophical assumptions by the members of parliaments or political assemblies. 4) *Sociology of science* and *sociology of technology* have clear philosophical roots in important conceptions developed in recent times (such as Kuhnian views in the Strong Program, neo-Kantian positions in the social contructivisms or postmodern conceptions in the ethnomethodology of science as well as in the approach of the Social Construction of Technology –SCOT–). 5) *Economics of science* and *economics of technological change* also have links with the philosophical approach.[9] Among those ties are specially visible the vinculum with the analysis of rationality in decision-making.[10]

Even though some of these disciplines clearly develop a *scientific study* of a science (e.g., sociology of science, economics of science …), where empirical information has a central role, this feature does not exclude a philosophical approach. Philosophy can pay attention to the aims, processes and results of scientific activities and technological doings. It analyzes what science and technology are, but it also considers what science and technology ought to do in order to have better standards. Thus, insofar as science and technology in *STS* are

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[7] Both philosophy of science and philosophy of technology have an *ethical dimension*, which will be pointed out later on in this paper. But bioethics and environmental ethics have received increasing attention from professionals related to health sciences (medicine, nursing, …) and sciences connected with the environment (ecology, forestry, …). Thus, they study more specific details (mainly in the sphere of the consequences of human actions) than philosophy of science and philosophy of technology.

[8] For Kristin Shrader-Frechette, the political analyses of technology are a central part of the philosophy of technology and she criticizes the attempt to reduce technology to epistemology, cf. SHRADER-FRECHETTE, K., "Reductionist Philosophy of Technology: Stones Thrown from Inside a Glass House," *Techné. Journal of the Society for Philosophy and Technology*, v. 5, n. 1, (1999), pp. 32-43.

[9] On the status and characteristics of economics of science, cf. GONZALEZ, W. J., "De la Ciencia de la Economía a la Economía de la Ciencia: Marco conceptual de la reflexión metodológica y axiológica," in AVILA, A., GONZALEZ, W. J. and MARQUES, G. (eds.), *Ciencia económica y Economía de la Ciencia: Reflexiones filosófico-metodológicas*, FCE, Madrid, 2001, pp. 11-37; especially, pp. 20-22.

For the economic views on technological change, cf. NELSON, R. R. and WINTER, S. G., *An Evolutionary Theory of Economic Change*, Belknap Press, Cambridge, 1982, and FREEMAN, C. and SOETE, L., *Economics of Industrial Innovation*, 3ª ed., The MIT Press, Cambridge, 1997. On the determinants and directions of technological change, cf. DOSI, G., "Technological Paradigms and Technological Trajectories," *Research Policy*, v. ll, (1982), pp. 147-162.

[10] Cf. GONZALEZ, W. J., "Racionalidad y Economía: De la racionalidad de la Economía como Ciencia a la racionalidad de los agentes económicos," in GONZALEZ, W. J. (ed.), *Racionalidad, historicidad y predicción en Herbert A. Simon*, Netbiblo, A Coruña, 2003, pp. 65-96.

open to metascientific and metatechnological reflections, *STS* can consider the normative aspects which are central to philosophy of science and philosophy of technology. Among them are problems related to scientific rationality and issues connected with technological rationality.[11]

In addition to the scientific studies already pointed out (a list that could be enlarged with other social sciences),[12] *STS* includes *history of science* and *history of technology* as well. Moreover, they are also under philosophy of science and philosophy of technology. This phenomenon is particularly clear after the "historical turn" taken in the sixties in philosophy of science. This turn was led by Thomas Kuhn and Imre Lakatos in the first decade, and continued thereafter by Larry Laudan.[13] They have highlighted the *historicity* in the case of scientific activity, but it can be held that technological doings also have a historical character, as can be seen particularly in the study of technological change.

To be sure, history of science and history of technology can be developed emphasizing either the epistemic content –the "internal" aspect– or the social dimension (the external element). But both aspects –internal and external– should be considered when philosophy of science and philosophy of technology are developed. The "social turn" of *STS* stresses the second scope: the contextual conditions. Moreover, that kind of research –the social dimension– is *tout court* a subject of *Science, Technology and Society* or *Science and Technology Studies* insofar as it insists on the *context* of science and technology (social, political, economic, professional …) instead of the intellectual content.

An overall view on *STS* should preserve the compatibility between the *philosophical approach* on science and technology and the *empirical research* on scientific activity and technological doing (i.e., history of science and history of technology, economics of science and economics of technological change, sociology of science and sociology of technology …). The autonomy of empirical research is compatible with the existence of philosophical roots. Furthermore, the harmony between the philosophical approach and the empirical research on science and technology underlies this interdisciplinary endeavor of *STS*. These

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[11] Shrader-Frechette insists on the idea of *rational* as a "highly normative term," SHRADER-FRECHETTE, K., *Risk and Rationality: Philosophical Foundations for Populist Reforms*, p. 7. For her, "The challenge, for any philosopher of science who holds some sort of middle position (between the relativists and the logical empiricists), is to show precisely how theory choice or theory evaluation can be rational, even though there are no universal, absolute rules of scientific method that can apply to every situation," SHRADER-FRECHETTE, K., *Risk and Rationality*, p. 8.

[12] A clear example is psychology of science. It works on scientists as individuals and as members of a scientific community. It studies several aspects, such as which are the characteristics that the scientists have *de facto* or should have in order to make scientific discoveries. Cf. MARTINEZ SELVA, J. M., "Psicología del descubrimiento científico," in GONZALEZ, W. J. (ed.), *Aspectos metodológicos de la investigación científica*, 2nd ed., Ediciones Universidad Autónoma de Madrid and Publicaciones Universidad de Murcia, Madrid-Murcia, 1990, pp. 305-315.

[13] Cf. GONZALEZ, W. J. (ed.), *Análisis de Thomas Kuhn: Las revoluciones científicas*, Trotta, Madrid, 2004; GONZALEZ, W. J. (ed.), *La Filosofía de Imre Lakatos: Evaluación de sus propuestas*, UNED, Madrid, 2001; and GONZALEZ, W. J. (ed.), *El Pensamiento de L. Laudan. Relaciones entre Historia de la Ciencia y Filosofía de la Ciencia*, Publicaciones Universidad de A Coruña, A Coruña, 1998.

studies will be more fruitful insofar as they consider the distinction between science and technology, since it is relevant not only in theoretical terms but also in practical terms (aims, processes and results).

2. TECHNOSCIENCE, SCIENCE AND TECHNOLOGY

Until now the emphasis has been here on *science* and *technology* as two different domains, each with its specificity and different studies. But in recent years –mainly in Europe– there has been a distinctive insistence on "technoscience" (and also on "scientific-technological" inquiry) rather than on the terminology of "science and technology."[14] Moreover, *technoscience* appears frequently in publications on *STS* and, sometimes, as the central topic.[15] Commonly, the term is used to reinforce the scientific-technological view and, in one way or another, technoscience seems to be in tune with an instrumentalist methodology (i.e., the theories are tools for problem-solving, and their values are measured according to their practical success).

Historically, the notion of "big science," proposed by Derek J. de Solla Price in 1963, might be considered as the first movement towards this new view.[16] But the consolidation of the term "technoscience" came later on with Bruno Latour,[17] who is interested in an ethnomethodological study of the activity developed in the laboratory. When he suggests this research on the social anthropology of science, he proposes *technoscience* to avoid an endless repetition of "science and technology." To a lesser extent, the term has also received the impulse of other authors.[18] Nowadays it is an expression used very frequently in some countries (among them Spain).

There are several reasons for proposing "technoscience." Some are theoretical and some practical. On the one hand, from a conceptual point of view, *technoscience* is the new attempt to blur the distinction between "science" and "technology" due to several options, such as the dismissal of criteria of demarcation or the integration of both domains (a task which could be performed in different ways). And, on the other hand, from a practical perspective, there is a tendency to consider i) that we have now *new phenomena* that belong to a new sphere –technoscientific– different from the previous scientific and technological ones, due to the new practices developed in laboratories, or ii) that the *interaction* between science and technology is so strong as to be almost indiscernible. This situation requires

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[14] Cf. ECHEVERRIA, J., *La revolución tecnocientífica*, FCE, Madrid, 2003.

[15] An interesting case is IHDE, D. and SELINGER, E. (eds.), *Chasing Technoscience: Matrix for Materiality*, Indiana University Press, Bloomington, 2003.

[16] Cf. PRICE, D. J. DE SOLLA, *Little Science, Big Science*, Columbia University Press, N. York, 1963.

[17] Cf. LATOUR, B., *Science in Action: How to Follow Scientists and Engineers Through Society*, Harvard University Press, Cambridge (MA), 1987.

[18] Cf. HOTTOIS, G., *Le paradigme bioéthique: une éthique pour la technoscience,* De Boeck-Wesmael, Brussels, 1990.

characterization of the three notions -technoscience, science, and technology –in order to establish what is the subject– matter studied by *STS*.

What is "technoscience"? The question about the sense and reference of *technoscience* can be answered in at least three different ways. a) It is a term that stands for the *identity* between science and technology: they have been increasing their ties to the point that there are no semantic differences between both of them in "technoscience," and they also have a common reference because there are no longer ontological differences between them. b) "Technoscience" is a term compatible with "science" and "technology" insofar as technoscience expresses the sense of a strong practical interaction between science and technology. The reference of technoscience is then twofold: there are two different aspects of reality which can have a *causal interaction* (or at least there is a relation which preserves the ontologies of science and technology). c) "Technoscience" is the term for a new reality, a kind of *blend* or *hybrid* of science and technology.[19] Consequently, the referent has properties which are different from science and technology.[20] In this case, the three of them can coexist and, therefore, *STS* can work on science and technology as well as technoscience.

Some people say that "technoscience" could be "techno"-"logos." This means that it is a subject which can be undestood as directly based in science. But this claim makes no sense when defining "technoscience," because the difference between "technics" and "technology" lies in this point: *technics* is practical knowledge of an accumulative kind, based on human experience but without the support of an explicit scientific knowledge; whereas *technology* is a human activity which transforms the reality (natural and social) in a creative way, and it does so on the basis of aims designed with the assistance of scientific knowledge as well as by means of specific technological knowledge. Thus, the "know how" in technology is accompanied by the "know that" given by science. Therefore, *technology* can only be "techno"-"logos" (i.e., a human activity which needs scientific knowledge and practice).

To reject the claim that technoscience is merely technology –based-on– science, because "technology" is *eo ipso* a human enterprise supported by science, requires us to clarify the relations between *science* and *technology*. There are several kinds of analysis (semantic, logical, epistemological, methodological, ontological, axiological, …) which can consider the theoretical as well as the practical aspects. In this regard, Ilkka Niiniluoto has focused on the ontological

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[19] Donna Haraway, "under her earlier figure of cyborg, sees technoscience as the full hybridization of science and technology," IHDE, D., "Has the Philosophy of Technology Arrived? A State-of-the-Art Review," p. 121. Cf. HARAWAY, D., *Simians, Cyborgs and Women: The Reinvention of Nature*, Routledge and Institute for Social Research and Education, N. York, 1991.

[20] Technoscience understood as hybridrization or symbiosis of science and technology suggests examples, such as the interaction of computer science and technology of information and communication, which lead to products popularly called "new technologies," where the patents are on properties different from those obtained by previous technologies. Cf. ECHEVERRIA, J., *La revolución tecnocientífica*, pp. 64-68 and 71-72.

approach. He has proposed *five different models* for consideration, which take into account the views that have been more influential in the relations between science and technology.[21]

1) Technology is reducible to science (i.e., technology depends ontologically on science), which means that either it is *applied science* or is *an application* of science.[22] 2) Science is reducible to technology (i.e., science depends ontologically on technology), which can be seen as an instrumentalist position insofar as science appears as *an instrument* to dominate nature through technology (a view held by some philosophies focused on praxis, such as different versions of pragmatism, Marxism … or even nihilism). 3) There is an *identity* of science and technology. This thesis is a way of understanding "technoscience," but is so strong that even its supporters –mainly constructivists–[23] try to emphasize the identity in *methodological terms* –as a common process– rather than in ontological terms (as being the same entity). 4) Science and technology are independent both ontologically and causally. It is a *parallelist* view: they move according to the same rhythm but without interaction.[24] 5) There is an ontological independence between science and technology, but they are in a causal interaction.

This last option of Niiniluoto's models –the *interactionist view*– is also a version of "technoscience." It is a sound conception because it respects the conceptual difference between "science" and "technology." On the one hand, commonly science and technology have different *aims*, *processes* and *results* (i.e., outcomes or products). Thus, they have theoretical as well as empirical differences. But, on the other hand, science and technology *are interconnected* in many ways, as history has shown us, at least since the XVII century (as can be seen in cases such as the construction of the telescope and the knowledge of satellites). In this regard, there is an interesting metaphor: they are like two legs of the same body.[25] Therefore, to accept *technoscience* in the second sense –as an interaction– could be compatible with notions both of "science" and "technology." Nevertheless, both of them should also be charactized in order to have a clear account of the triadic distinction among science, technology, and technoscience.

Defending the idea of the difference between the three of them requires us to insist on *science* as a complex reality which condenses a trajectory of centuries and it is open to improvement in the future. Thus, the *characteristics of a science*

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[21] Cf. Niiniluoto, I., "Ciencia frente a Tecnología: ¿Diferencia o identidad?," *Arbor*, v. 157, n. 620, (1997), pp. 285-299; especially, pp. 287-291.

[22] Cf. Bunge, M., "Technology as Applied Science," *Technology and Culture*, v. 7, (1966), pp. 329-347. For D. Ihde, "Bunge's take on technology and its relation to science, turns out to be nearly identical with Martin Heidegger's," Ihde, D., "Has the Philosophy of Technology Arrived? A State-of-the-Art Review," p. 118.

[23] It should be pointed out that constructivism can be developed in a large number of directions, cf. Hacking, I., *The Social Construction of* What*?*, Harvard University Press, Cambridge (MA), 1999.

[24] Cf. Price, D. J. De Solla, "Is Technology Historically Independent of Science? A Study in Statistical Historiography," *Technology and Culture*, v. 6, (1965), pp. 553-568.

[25] Cf. Rescher, N., *Razón y valores en la Era científico-tecnológica*, Paidós, Barcelona, 1999.

are not simple, but they can be enumerated basically in several elements: i) science possesses a specific language (with terms whose sense and reference are precise); ii) science is articulated in scientific theories with a well patterned internal structure, which is open to later changes; iii) science is a qualified knowledge (with more rigor –in principle– than any other knowledge); iv) it consists of an activity that follows a method (normally it is deductive, although some authors accept the inductive method)[26] and it appears as a dynamic activity (of a self-corrective kind, which seeks to increase the level of truthlikeness).

Apart from these characteristics, there are others which have been emphasized in recent times: v) the reality of science comes from social action, and it is an activity whose nature is different from other activities in its assumptions, contents and limits; vi) science has aims –generally, cognitive ones– for guiding its endeavor of researching (in the formal sphere and in the empirical realm); and vii) it can have ethical evaluations insofar as science is a free human activity: values which are related to the process itself of research (honesty, originality, reliability …) or to its nexus with other activities of human life (social, cultural …).[27]

These characteristics of science are connected to a kind of rationality which is different from technological rationality,[28] because the aims, processes and results that, in principle, science and technology seek are different. Thus, scientific rationality has several aims, mainly in the cognitive sphere, and they can be pursued in order to *increase our knowledge* (basic science) or to *resolve practical problems* in a concrete area (applied science).[29] Meanwhile technological rationality is oriented towards a *creative transformation of reality,* either natural or social, according to a design, which is followed by an activity and a posterior artifact (or final product).

Etymologically, "technology" is a kind of knowledge insofar as it is the *logos* (the doctrine or learning) of the *techné*[30] (either in the realm of "arts" –to create beautiful objects– or in the sphere of "technics"– to build useful items–). In addition, technology is a social activity which is developed in an intersubjective doing in order to transform the previous reality (natural or social), based on scientific knowledge as well as specific technological knowledge. As a consequence of this process, there is an expected product which should be tangible: a visible artifact or a new kind of social reality. This final product of

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[26] Cf. Niiniluoto, I. and Tuomela, R., *Theoretical Concepts and Hypothetico-Inductive Inference*, Reidel, Dordrecht, 1973.

[27] On these seven elements of science, cf. Gonzalez, W. J., "De la Ciencia de la Economía a la Economía de la Ciencia: Marco conceptual de la reflexión metodológica y axiológica," p. 15.

[28] Cf. Gonzalez, W. J., "Racionalidad científica y racionalidad tecnológica: La mediación de la racionalidad económica," *Agora*, v. 17, n. 2, (1998), pp. 95-115.

[29] Cf. Niiniluoto, I., "The Aim and Structure of Applied Research," *Erkenntnis*, v. 38, (1993), pp. 1-21.

[30] Cf. Mitcham, C., "Philosophy of Technology," in Durbin, P. (ed.), *A Guide to the Culture of Science, Technology and Medicine*, The Free Press, N. York, 1980, pp. 282-363.

technology ight be registered in a patent, which could hardly be the case in the final outcome of science (even in applied science).[31]

Yet *technology* is more than knowledge used in a transformative way to get a final product, because it includes a variety of components. 1) Technology has its own language, due to its attention to internal constituents of the process (design, effectiveness, efficiency …) and external factors (social, economic, political, cultural …). 2) The structure of technological systems is articulated on the basis of its operativity, because it should guide the creative activity of the human being that transforms nature (or the human and social reality). 3) The specific knowledge of the technological activity –know how– is instrumental and innovative: it seeks to intervene in an actual realm, to dominate it and to use it in order to serve human agents and society. 4) The method is based on an imperative-hypothetical argumentation. Thus, the aims are the key to making reasonable or to rejecting the means used by the technological process. 5) There are values accompanying that process, which could be internal (to realize the goal at the lowest possible cost) and external (ethical, social, political, ecological, etc). These values condition the viability of the possible technology and its alternatives. 6) The reality itself of the technological process is supported by social human actions which have an intentionality and are oriented towards the transformation of the surrounding reality.[32]

Therefore, technology can be seen as an attempt to direct a human activity to obtain a *creative and transformative domain* of that reality –natural or human and social– on which it is working. Primarily, it does not seek to describe or to explain reality, because there is already a discovered reality (i.e., known to some extent) which technology wants to change. This domain appears in new designs and in the effectiveness-efficiency pair, but it also requires us to consider a large number of aspects related to this activity (ethical, economic, ecological, political, cultural, etc). Thus, even though a technology may achieve its aims as such (i.e., effectiveness), it might not be acceptable from the point of view of other factors, such as economic criteria (e.g., the cost-benefit ratio), ethical values (e.g., consent, fairness), ecological effects (e.g., the contamination of rivers), political consequences (e.g., the decrease of civil liberties) or incompatibility with the dominant culture.

Central to this account about technoscience, science and technology is the need for a clear distinction between them. This goal seems necessary in order to clarify the contents of the studies on science and technology (and, hence, for

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[31] Usually, the outcomes of science are public and have free access to users, whereas the products of technology can have a patent and, therefore, they could be private and with no free access for users.

[32] Cf. GONZALEZ, W. J., “Progreso científico e innovación tecnológica: La ‘Tecnociencia’ y el problema de las relaciones entre Filosofía de la Ciencia y Filosofía de la Tecnología,” *Arbor*, v. 157, n. 620, (1997), p. 266.

the relations between science, technology and society). The relevance of the distinction is not merely of a conceptual kind because it has also a practical dimension. In effect, each one of them –technoscience, science and technology– is social from at least two points of view: i) all are developed by a community of researchers, and they should have a neat notion of their work (aims, processes and results); and ii) insofar as technoscience, science and technology are human undertakings, they belong to a social setting which can have repercussions at the different levels (aims, processes and results). The philosophical approach should consider the internal constituents as well as their external factors; and due to the characterization of "technoscience" as a type of interaction between science and technology, the focus here will be on these cases.

3. THE PHILOSOPHICAL APPROACH: FROM THE "INTERNAL" TO THE "EXTERNAL"

Virtually each one of the elements of science (language, structure, knowledge, method, activity, ends, and ethical values) and the components of technology (language, system, specific knowledge …) can be seen either from an *internal* point of view or from an *external* perspective. Nowadays it is generally accepted –mainly in discussions of realism in science and technology– that a philosophical approach should consider the "internal" constituents as well as the "external" factors. Obviously, *STS* emphasizes the latter, because it highlights the social dimension of science and technology. Furthermore, technoscience always seems connected with the social setting. Sometimes –especially in postmodern views– the emphasis on "external" factors is so strong that it becomes controversial insofar as "internal" constituents are diluted.[33]

"Internal" constituents are those which can be studied in themselves without a specific reference to the context, because it is understood that they belong to the entity itself (either science or technology). Thus, there is no real attention to the diverse frameworks (social, cultural, economic, political …) where either the science or the technology is developed. Then, the intellectual contents –language, structure, knowledge …–, practices or results of science or technology are considered as something autonomous. Consequently, it is assumed that the scientific or technological community has specific rules that have no real or relevant interference with the external factors. Moreover, possible interference is seen, in some cases, as disturbing the scientific activity or technological doing.

"External" factors are those related to the diverse contexts (social, cultural, economic, political …) which are the milieu of science and technology. The existence and relevance of the external factors is clear insofar as the key element of science or technology is the notion of "activity" rather than the concept of

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[33] Cf. HAACK, S., *Manifesto of a Passionate Moderate*, The University of Chicago Press, Chicago, 1998; KOERTGE, N. (ed.), *A House Built on Sand: Exposing Postmodern Myths about Science*, Oxford University Press, N. York, 1998; and SOKAL, A. and BRICMONT, J., *Intellectual Impostures. Postmodern Philosophers' Abuse of Science*, Profile Books, London, 1998.

"knowledge" or even the idea of "result" (outcome or product). A *human activity* is always developed within a context: historical, social, cultural, political … Within a context there are values. Some of them have more recognition or prestige than others. Decision making –an indispensable element in a human activity– follows from some kind of values, according to the milieu where the activity is going to be carried on.

3.1. The Approach in Philosophy of Science

Philosophy of science studies the *constitutive elements* of science: its language, structure, knowledge claims, activity, aims, values … Consequently, they are analyzed by the philosophical approach in the semantics of science, the logic of science, the epistemology, the ontology of science, the axiology of scientific research, the ethics of science … . Linked to these components of science is the scientific method. Its scrutiny corresponds to the *methodology of science*, which is bound up with research in philosophy of science. Thus, the inquiry into the existence and dynamics of scientific progress –methodological inquiry– is connected with scientific knowledge (epistemological examination).

Methodology of science can, of course, be studied by non philosophers –the specialists in each science– but it belongs initially to the philosophical sphere. In fact, methodological questions, which combine queries about what science *is* and what science *ought to be*, start from a philosophical stance, due to their metascientific character. However, within the wider philosophical realm, methodology of science has its *specific status* (in order to analyze and prescribe the scientific process), as has been pointed out in the section on the general scope of philosophy and methodology of science.[34]

For many years (from the mid-twenties to the mid-sixties), the emphasis in philosophy of science was on the "internal" factors (above all, science as a qualified knowledge which possesses a rigorous method based on logic). The focus either on logical empiricism or on critical rationalism used to be on the scientific elements themselves. It paid little attention to the "external" context (social, cultural, economic, political …). The objectivity of scientific knowledge and the methodological process of a logical character were two points widely shared during that period. In addition, the context of justification received more attention than the context of discovery. Thus, even the psychological factors were of little interest for the philosophical approach.

The "historical turn," starting in the sixties, changed the panorama completely. The new emphasis was the idea of science as a human activity and, therefore, as a social event. The change of the *turn* includes historicity as an internal constituent of science as well as an external factor. Both elements combined –human activity

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[34] Cf. Gonzalez, W. J., "Ámbito y características de la Filosofía y Metodología de la Ciencia," in Gonzalez, W. J. (ed.), *Aspectos metodológicos de la investigación científica*, 2nd ed., pp. 49-78.

and historicity– lead to another key element: the decision making of the scientific community requires us to take into account social and political factors, not only the internal constituents of science. Furthermore, the responsibility of the scientist goes beyond the endogenous aspects (honesty, originality, reliability …) of scientific activity to reach the exogenous elements. Thus, the aims, processes and results are not mere individual ingredients but rather social factors.

Posterior philosophical views, such as some versions of naturalism of sociological roots and most of the postmodern conceptions of science, have emphasized the sociological dimension of scientific activity. The existence in *STS* of three very influential positions within the sociology of science should be pointed out: a) the "Strong Program" of the Edinburgh school led by Barry Barnes[35] –now at the University of Exeter– and David Bloor,[36] based on some Kuhnian as well as Wittgensteinian ideas; b) the empirical program of relativism (EPOR) developed by Harry Collins,[37] which studies scientific controversies with an interpretative flexibility and analyzes their connections to the socio-cultural milieu; and c) the ethnomethodology –the study of the actors' network at the workplace– defended by the social constructivism of Bruno Latour[38] and Steve Woolgar.[39]

These schools of sociology of science assume post-Kuhnian views on the role of the *context of scientific knowledge* which affects the vision of the scientific objects of research. They highlight science from an external point of view: as a *social practice* where the "epistemic contents" are really "social factors" –the Strong Program; as a *self-referential project* within a constructivist framework – the empirical program of relativism; and as a social construction based on *individual*

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[35] Cf. Barnes, B., *Interests and the Growth of Knowledge*, Routledge and K. Paul, London, 1977; Barnes, B., *T. S. Kuhn and Social Science,* Macmillan, London, 1982 (Columbia University Press, N. York, 1982); Barnes, B., *The Elements of Social Theory*, Princeton University Press, Princeton, 1995; and Barnes, B., Bloor, D. and Henry, J., *Scientific Knowledge. A Sociological Analysis*, The University of Chicago Press, Chicago, 1996.

[36] Cf. Bloor, D., "Wittgenstein and Mannheim on the Sociology of Mathematics," *Studies in History and Philosophy of Science*, v. 4, (1973), pp. 173-191; Bloor, D., "Popper's Mystification of Objective Knowledge," *Science Studies*, v. 4, (1974), pp. 65-76; Bloor, D., *Knowledge and Social Imagery*, Routledge and K. Paul, London, 1976 (2nd ed., The University of Chicago Press, Chicago, 1991); Bloor, D., *Wittgenstein: A Social Theory of Knowledge*, Macmillan, London, 1983; and Bloor, D., *Wittgenstein, Rules and Institutions*, Routledge, London, 1997.

[37] Cf. Collins, H. M., "An Empirical Relativist Programme in the Sociology of Scientific Knowledge," in Knorr-Cetina, K. D. and Mulkay, M. (eds.), *Science Observed: Perspectives in the Social Study of Science*, Sage, London, 1983, pp. 85-100; and Collins, H. M. and Pinch, T., *The Golem: What Everyone Should Know About Science*, Cambridge University Press, Cambridge, 1993.

[38] Cf. Latour, B. and Woolgar, S., *Laboratory Life: The Social Construction of Scientific Facts*, 2nd ed., Princeton University Press, Princeton (NJ), 1986; Latour, B., *The Pasteurisation of France*, Harvard University Press, Cambridge, MA, 1988; and Latour, B., *We have Never been Modern,* Harvester, Brighton, 1993 (translated by C. Porter.)

[39] Cf. Woolgar, S., "Critique and Criticism: Two Readings of Ethnomethodology," *Social Studies of Science*, v. 11, n. 4, (1981), pp. 504-514; Woolgar, S., *Science: The Very Idea,* Tavistock, London, 1988; and Woolgar, S. (ed.), *Knowledge and Reflexivity: New Frontiers in the Sociology of Knowledge*, Sage, London, 1988.

actions –in the ethnomethodology of science– where there is no clear demarcation between "scientific actions" and "political actions.."[40] All of them have gone far beyond the initial expectations of Thomas Kuhn, when he insisted on the historical character of science within a social setting of scientific communities, because he still gives more relevance to the internal constituents of science than to the external factors.[41]

All these sociological conceptions of science present philosophical perspectives where the *external factors* of science have clearly more weight than the internal constituents.[42] They are usually more interested in *scientific practices* than in their theoretical contents. In those authors both the idea of truth, as an aim to scientific research, and the role of objectivity receive little attention. This is due to their ideas on the revisability of science, which they see through a strong sense of historicity or by means of the emphasis on social constructions (including the scientific object itself). Their accounts of scientific activity adopt different brands of relativism,[43] which are mostly semantic, epistemological and methodological, but sometimes they also reach the ontological level (as is the case in explicit versions of social constructivism).

They have received clear-cut criticisms in order to keep in mind the importance of the *epistemic content* of science,[44] which can be connected to the need for *objectivity* in science.[45] Furthermore, it seems reasonable to assume there is an inherent structure of the world to be discovered.[46] Therefore, science is social in a way: insofar as it is a human activity developed in a social setting, because science is one of our human endeavors as social beings. But there are

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[40] B. Barnes points out this feature of the ethnomethodology and considers that the conception of B. Latour goes too far, cf. BARNES, B., "Thomas Kuhn and the Problem of Social Order in Science," in NICKLES, TH. (ed.), *Thomas Kuhn*, Cambridge University Press, Cambridge, 2003, pp. 122-141; especially, pp. 134-135.

[41] "I am among those who have found the claims of the strong program absurd: an example of deconstruction gone mad," KUHN, TH. S., *The Road Since* Structure. *Philosophical Essays, 1970-1993, with an Autobiographical Interview*, edited by James Conant and John Haugeland, The University of Chicago Press, Chicago, 2000, p. 110.

[42] About the connections of those views with Kuhn's philosophical approach, cf. GONZALEZ, W. J., "Las revoluciones científicas y la evolución de Thomas S. Kuhn," in GONZALEZ, W. J. (ed.), *Análisis de Thomas Kuhn: Las revoluciones científicas*, pp. 15-103; especially, pp. 36-43.

[43] On the difference among the diverse versions of relativism, cf. HAACK, S., "Reflections on Relativism: From Momentous Tautology to Seductive Contradiction," in TOMBERLIN, J. E. (ed.), *Philosophical Perspectives, 10: Metaphysics*, B. Blackwell, Oxford, 1996, pp. 297-315.

[44] Cf. LAUDAN, L., *Science and Relativism: Some Key Controversies in the Philosophy of Science*, The University of Chicago Press, Chicago, 1990; and NIINILUOTO, I. (1991), "Realism, Relativism, and Constructivism," *Synthese*, v. 89, pp. 135-162; especially, pp. 150-155.

[45] Cf. NIINILUOTO, I., *Critical Scientific Realism*, Clarendon Press, Oxford, 1999, pp. 252-301.

[46] For Ian Hacking, "constructivists tend to maintain that classifications are not determined by how the world is, but are convenient ways in which to represent it. They maintain that the world does not come quietly wrapped up in facts. Facts are the consequences of ways in which we represent the world. The constructivist vision here is splendidly old-fashioned. It is a species of nominalism. It is countered by a strong sense that the world has an inherent structure that we discover," HACKING, I., *The Social Construction of* What*?*, p. 33.

a variety of elements of science (language, structure, knowledge, method, activity, ends, and ethical values) which cannot be reduced to their mere social interpretations. In this regard, it seems convenient to assess the need to preserve the *self-correcting character* of science as human activity within the sociological environment.

3.2. The Approach in Philosophy of Technology

It seems clear that *philosophy of technology* has followed in the past different routes than philosophy of science. In philosophy of technology there is no clear tradition similar to that which can be found in philosophy of science: an intellectual trajectory that goes back to logical positivism and reaches the present views (naturalism, social constructivism, feminist epistemology …).[47] The roots are different because "philosophy of technology has primarily drawn its philosophers from the *praxis* traditions, in North America from pragmatism, phenomenology, and the neo-Marxian critical theorists, with analytical strands in a minority role."[48]

This phenomenon can be connected with the fact that in philosophy of technology it is difficult to point to a major philosopher who has had (on the systematic treatment of its subject-matter) an influence similar to R. Carnap, H. Reichenbach, K. Popper or I. Lakatos in the realm of philosophy of science. In spite of the absence of a leading author of philosophical discussions of technology, one should recognize well-known philosophers who wrote either on "technics" or on "technology" in Europe in the middle of the twentieth century. Among them are J. Ortega y Gasset,[49] M. Heidegger,[50] K. Jaspers[51] and A. Gehlen.[52] They differ in their philosophical approaches to technology –some are very pessimistic– but they share an underlying anthropological view as well as an interest in the relations between technology and the social world (the *Lebenswelt*).

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[47] Cf. KOERTGE, N., "'New Age' Philosophies of Science: Constructivism, Feminism and Postmodernism," *The British Journal for the Philosophy of Science*, v. 51, (2000), pp. 667-683.

[48] IHDE, D., "Has the Philosophy of Technology Arrived? A State-of-the-Art Review," p. 118.

[49] Cf. ORTEGA Y GASSET, J., *Ensimismamiento y alteración. Meditación de la Técnica*, Espasa-Calpe, Buenos Aires, 1939 (originally written in 1933.) Reprinted in ORTEGA Y GASSET, J., *Meditación de la Técnica*, Santillana, Madrid, 1997.

[50] Cf. HEIDEGGER, M., "Die Frage nach der Technik," in HEIDEGGER, M., *Vorträge und Aufsätze*, Günther Neske, Pfullingen, 1954, pp. 13-44. Translated as HEIDEGGER, M., "The Question Concerning Technology," in SCHARFF, R. C. and DUSEK, V. (eds.), *Philosophy and Technology: The Technological Condition*, pp. 252-264.

[51] Cf. JASPERS, K., *Die Atom-bombe und die Zukunft der Menschen*, Piper, Munich, 1958.

[52] Cf. GEHLEN, A., "Anthropologische Ansicht der Technik," in FREYER, H., PAPALEKAS, J. CH. and WEIPPERT, G. (eds.), *Technik im technischen Zeitlater*, J. Schilling, Düsserdorf, 1965. Translated in abridged version as GEHLEN, A., "A Philosophical-Anthropological Perspective on Technology," in SCHARFF, R. C. and DUSEK, V. (eds.), *Philosophy and Technology: The Technological Condition*, pp. 213-220.

From a systematic point of view, philosophy of technology was less articulated than philosophy of science at the beginning of the seventies.[53] To some extent, it is still the case, especially regarding the internal constituents. For this there are chronological reasons: philosophy of technology is *posterior* to philosophy of science in a strict sense. In this regard, we can take into account some historical points: a) philosophy of science was consolidated by the Vienna Circle, because most of the elements of science (language, structure, knowledge, method, aims) were then under consideration; and b) there is somehow a continuity in many important topics of discussion (epistemological and methodological), even though there are also clear discrepancies in philosophy of science since then (e.g., on issues related to scientific realism). Thus, there are still controversies connected with the problems that they tackled, mainly in basic science and, to a lesser extent, in applied science.

Nowadays, after several decades of contributions to philosophy of technology, there is an important body of work in this realm.[54] But it is possible to find some authors, such as E. Ströker in the past, who maintain that the later interest in philosophical reflection on technological doing has repercussion at the level of philosophical studies on technology.[55] The situation has improved in the last twenty years, because philosophy of technology has received more attention than before. Nevertheless, D. Ihde has pointed out recently that "philosophy of technology has not, to date, generated recognizable and sustained internal arguments."[56] He argues that he cannot detect in philosophy of technology a debate like the 'realism versus antirealism' one in philosophy of science. Thus he claims that debates on 'appropriate technologies', 'deep ecology', 'sustainable environmental practices' or 'risk assessment' have not reached that level of discussion.

Nonetheless, in *STS* the presence of philosophy of technology is sometimes more intense than philosophy of science. On the one hand, this is due to the

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[53] Ronald Giere wrote then, when Kuhn and Lakatos were at a peak of their careers, that "the methodology of technology is philosophically nearly a virgin territory," GIERE, R. N., "The Structure, Growth and Application of Scientific Knowledge: Reflections on Relevance and Future of Philosophy of Science," in BUCK, R. C. AND COHEN, R. S. (eds.), *In Memory of R. Carnap*, Reidel, Dordrecht, 1971, p. 544.

The bibliography of initial stages of philosophy and methodology of technology can be found in MITCHAM, C. and MACKEY, R. (eds.), *Bibliography of the Philosophy of Technology*, The University of Chicago Press, Chicago, 1973.

[54] Among the important works in philosophy of technology, there are some which are interesting for the present purpose: SKOLIMOWSKI, H., "The Structure of Thinking in Technology," *Technology and Culture*, v. 7, (1966), pp. 371-383; and RAPP, F., *Analitische Technikphilosophie*, K. Alber, Munich, 1978.

In addition to the anthologies already pointed out, it should be mentioned here RAPP, F. (ed.), *Contributions to a Philosophy of Technology*, Reidel, Dordrecht, 1974; DURBIN, P. and RAPP, F. (eds.), *Philosophy and Technology*, Reidel, Dordrecht, 1983; and FELLOWS, R. (ed.), *Philosophy and Technology*, Cambridge University Press, Cambridge, 1995.

[55] Cf. STRÖKER, E., "Philosophy of Technology: Problems of a Philosophical Discipline," DURBIN, P. and RAPP, F. (eds.), *Philosophy and Technology*, pp. 323-336; specially, p. 323.

[56] "Has the Philosophy of Technology Arrived? A State-of-the-Art Review," p. 124.

importance of external factors in technology (economic, political, ecological …) which are more relevant than in philosophy of science. On the other hand, the social consequences of technology are frequently clearer to the public than scientific research (especially if it is in basic science instead of applied science). *De facto*, philosophy of technology has developed more intensively than philosophy of science its relations with human ends and its dimension as social practice.[57]

However, there are differences between them from the institutional point of view which affect the development of these branches of philosophy, because even now there is no "Philosophy of Technology Association" in the United States (and this is also the case in other countries). On the one hand, the biannual meetings of the Philosophy of Science Association include topics devoted to philosophy of technology. On the other hand, there is the Society for Philosophy and Technology, organized in 1983, where leading figures, such as Carl Mitcham[58] and Paul Durbin,[59] do not like a new subdiscipline of "philosophy of technology."[60] In addition, there are other societies that are also interested in issues related to philosophy of technology, mainly in the realm of the consequences of technological processes (nuclear plants, industrial contamination of water …). This is the case of the International Society for Environmental Ethics, whose president is Kristin Shrader-Frechette,[61] where the impact of technology on the environment is frequently analyzed.

Usually in philosophy of technology there are three different *foci of attention*, even though it is commonly accepted that technology includes all of them. 1) Technology as a *kind of knowledge* which combines scientific knowledge –know that– and specific technological knowledge –know how– and has a key role in design. 2) Technology as a *human activity* which implements a methodological process in order to transform the reality in a creative way. 3) Technology as a *product or artifact* of a human activity which has a physical presence in the social world and possesses a clear importance for macroeconomics and microeconomics. It seems clear that the first option considers the internal constituents more

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[57] Cf. Scharff, R. C. and Dusek, V. (eds.), *Philosophy and Technology: The Technological Condition*, parts V and VI, pp. 339-483 and 485-665.

[58] Carl Mitcham has a special interest in the historical aspects of the philosophy of technology and in different traditions on this subject, cf. Mitcham, C., *Thinking through Technology. The Path between Engineering and Philosophy*, The University of Chicago Press, Chicago, 1994, and Mitcham, C. (ed.), *Philosophy of Technology in Spanish Speaking Countries*, Kluwer, Dordrecht, 1993.

[59] Paul Durbin has work on central topics of the philosophy of technology, cf. Durbin, P. (ed.), *Philosophy of Technology. Practical, Historical and Other Dimensions*, Kluwer, Dordrecht, 1989; and Durbin, P. (ed.), *Broad and Narrow Interpretations of Philosophy of Technology*, Kluwer, Dordrecht, 1990.

[60] Cf. Ihde, D., "Has the Philosophy of Technology Arrived? A State-of-the-Art Review," pp. 118-119.

[61] Kristin Shrader-Frechette has emphasized the role of ethics in science and in technology, cf. Shrader-Frechette, K., *Ethics of Scientific Research*, Rowman and Littlefield, Savage (MD), 1994; Shrader-Frechette, K. and Westra, L. (eds.), *Technology and Values*, Rowman and Littlefield, Savage (MD), 1997; and Shrader-Frechette, K., *Environmental Justice: Creating Equality, Reclaiming Democracy*, Oxford University Press, N. York, 2002.

relevant than the external factors,[62] whereas the second and the third options tend to emphasize the external factors.

It may be fruitful to focus philosophy of technology explicitly on the elements already pointed out (language, system, knowledge, method, human activity, aims and values). If we develop the "internal" constituents in a philosophical study more similar to the philosophy of science, then it can be easier to understand the "external" factors. The philosophical reflection on technological doing can analyze the semantic, structural, epistemological, methodological, ontological, axiological (internal values) and evaluative (external values) aspects of technology. Generally, the attention goes to epistemological, methodological, ontological and evaluative considerations. It seems reasonable to think that to clarify the technological activity itself –the internal perspective– can be the initial step to pondering the social dimension of technology –the external view– which has many consequences and manifestations (technology as a crucial factor for social change, as an instrument for political power, as a means for the transformation of the ecosystem, etc).

4. THE RELATION BETWEEN SCIENCE AND SOCIETY FROM A PHILOSOPHICAL PERSPECTIVE

Although the distinction between "internal" constituents and "external" factors can suggest that the social elements belong only to the second sphere, it should be pointed out that this is not the case, because –in a sense– the social character of science is also *internal*. The reason is clear: each one of the constituents of science (language, structure, knowledge, method, activity, ends, and ethical values) is social insofar as science is *human-made* and the human being can only develop those elements *within society*. Understood in that way, the social constitution of science is unavoidable. To be sure, the conceptual framework of science belongs to us: science is "our" science.[63] There is no other being on earth able to construct and to use the elements characteristic of science.

Consequently, it should be assumed that there is an underlying *social dimension of science* which affects every constituent of science. In this sense, society is the necessary medium to conform those specific components of science. These constituents, due to their social origin and insofar as they are human-made products, are also finite as well as historical items. Accordingly, it might be assumed that those components (language, structure, knowledge, method, activity, ends, and ethical values) are neither absolute nor perfect. Furthermore,

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[62] A strong version of this conception maintains that "philosophical questions about technology are first and foremost about what we can know about a specific technology and its effects and in what that knowledge consists," PITT, J., *Thinking about Technology: Foundations of the Philosophy of Technology*, Seven Bridges Press, N. York, 2000, p. xiii.

[63] Cf. RESCHER, N., "Our Science as *our* Science," in RESCHER, N., *A System of Pragmatic Idealism. Vol. I: Human Knowledge in Idealistic Perspective*, Princeton University Press, Princeton, 1992, pp. 110-125.

if the self-correcting character of science is accepted, then those elements might be revisable within their social medium, which is the scientific community where they are developed.

4.1. The Social Dimension of Science

Ontologically, science is a *human activity* cultivated in scientific communities. In this regard, science is a social endeavor as such: it is not a mere individual activity in an isolated medium but rather the active venture due to individuals and groups working on some topics –basic research or applied research– either in a visible setting (a laboratory, an astronomical observatory …) or in "invisible communities" all around the world. From the point of view of the social origins of science, the idea of standing on "shoulders of giants," which is used to refer to great scientists of the modern age, only makes sense under the assumption of the existence of previous contributions of other scientists (as is the case of Galileo and the important scientists of that period). And the idea of "big science" and the increasing interaction between science and technology (e.g., in cases such as the Human Genome project) requires the cooperative action of social groups (i.e., diverse research groups) under "we-intentions" and the search for common goals.[64] In addition, the question could be raised of collective responsibility,[65] which goes beyond the personal responsibility of the scientist as an individual.

Scientific progress is then a social activity in "internal" terms, because only human society is able to develop this historical activity of science which includes an improvement regarding some aims.[66] Hence, the methodological idea of "scientific progress," when it is accepted, requires a historical social undertaking. Furthermore, scientific progress can be considered from the point of view of its consequences for the "external" medium, because it can have repercussions in different contexts (cultural, political, economic, ecological …). This is also the case regarding the central notions of science in different philosophical realms (semantic, logical, epistemological …), where there is an internal aspect of social origin –in the sense already pointed out– and an external factor, which can also receive the attention of science studies (sociology of science, economics of science …).

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[64] An analysis of this issue can be made on the basis of action theory. In this regard, cf. Tuomela, R., "The Social Dimension of Action Theory," *Daimon. Revista de Filosofía*, v. 3, (1991), pp. 145-158; and Tuomela, R., *The Importance of Us*, Stanford University Press, Stanford, 1995. In addition, it could be of interest to consider the social ontology, cf. Ruben, D. H., *The Metaphysics of the Social World*, Routledge and K. Paul, London, 1985.

[65] On this notion, cf. Rescher, N., "Collective Responsibility," in Rescher, N., *Sensible Decisions. Issues of Rational Decision in Personal Choice and Public Policy*, Rowman and Littlefield, Lanham, 2003, pp. 125-138.

[66] Cf. Gonzalez, W. J., "Progreso científico, Autonomía de la Ciencia y Realismo," *Arbor*, v. 135, n. 532, (1990), pp. 91-109.

Nevertheless, the social origin of science –a social activity– is compatible with the acceptance of the search for *objectivity* in science. The intersubjective undertaking of the scientific activity can be opened to grasp objective contents in the different realms (such as language, structure, knowledge …). On the one hand, the *critical attitude* of the scientific community towards the contents (linguistic, cognitive, procedural …) and the need for publication of the results of the scientific research for *public discussion* are oriented to disregard subjective elements in favor of objective ones. And, on the other hand, there is a *reality* (natural, social, or artificial) to be known by science in its actual properties. This includes the need for several distinctions, such as real world and possible world, ordinary experience and virtual experience …

Generally, social constructivism dismisses the difference between scientific knowledge and other kinds of human knowledge. Thus, for Trevor J. Pinch and Wiebe E. Bijker, "the treatment of scientific knowledge as a social construction implies that there is nothing epistemologically special about the nature of scientific knowledge: It is merely one in a whole series of knowledge cultures (including, for instance, the knowledge systems pertaining to 'primitive' tribes)."[67] This sociology of scientific knowledge goes beyond different versions of relativism –epistemological and methodological– because these views often assume some differences between scientific and non-scientific knowledge, whereas the position of social constructivism cannot see anything epistemologically relevant in the case of science.

Insofar as social constructivism is more radical than other versions of relativism (semantic, epistemological and methodological) and denies the relevance of the internal components of science,[68] it loses ground with its positive contributions on the external factors of science (cultural, social …). *De facto*, the social constructivist position moves in the opposite direction of logical positivism, where the primacy of the internal contents of science was almost complete. But both conceptions –social constructivism and logical positivism– go too far in their respective emphasis on the external factors and the internal components of science.

In my judgment, an adequate image of science requires us to take into account both aspects (internal and external). Thus, what science *is* and *ought to be* should pay attention to the contents of science: they are not mere elements for a "social

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[67] PINCH, T. J. and BIJKER, W. E., "The Social Construction of Facts and Artefacts: Or How the Sociology of Science and the Sociology of Technology might Benefit Each Other," *Social Studies of Science*, v. 14, (1984), pp. 399-441. Published, in a shortened and updated version, as PINCH, T. J. and BIJKER, W. E., "The Social Construction of Facts and Artefacts," in SCHARFF, R. C. and DUSEK, V. (eds.), *Philosophy and Technology: The Technological Condition*, p. 222.

[68] In this conception the aims, processes and outcomes of science are deprived of something that is not intrinsically mutable, due to the changes in the scientific communities and the different societies. Even the reality itself that is studied (the "objects" and its associated "facts") can be constructed and has not an ontological status on its own. This aspect is severely criticized by Ian Hacking in his book *The Social Construction of* What*?*

negotiation" regarding their relation to technology,[69] because the scientific contents include elements oriented towards a growth –intensive and extensive– of the available knowledge (basic science) or directed to an increment in the capacity of solving practical problems (applied science). In this regard, the existence of deep changes in science –"scientific revolutions"– might be under the influence of external factors (economic, cultural, political …) but, above all, they require conceptual changes.[70] If the sociology of scientific knowledge looks for a focus, it should take into account the existence of constituents of the scientific activity. Thus, the cooperative actions at the laboratories, the attitudes towards research priorities, the need for ethical values in the public domain of science … ask for an internal component of science.

Both kinds of knowledge –theoretical and practical– are commonly used in science. Normally, they seek objectivity and not merely an intersubjective agreement. On the one hand, it is true that the social activity of scientific research belongs to a cultural milieu and depends on the interaction of some agents in a social medium which can lead to agreements. But, on the other hand, this external context is not enough to grasp scientific activity, because science has something to do. It can make explicit features about the past, present or future reality (natural, social, or artificial) in order to give an explanation or to make a prediction; or it can use the research to offer a genuine contribution to solve real problems in concrete areas (medical, social …). Therefore, the intersubjective facet of science is not sufficient to understand the social phenomenon of science as a human activity in a social setting. Objectivity is then the crucial issue for the philosophical approach as regards science and society.

Objectivity is a feature that, in principle, can be connected with each one of the elements of science (language, structure, knowledge, method, activity, ends, and ethical values). It is habitually associated with the semantic, epistemological and ontological components of science, and it is also a central topic of discussion in the philosophical conceptions of scientific realism.[71] To accept the idea of *objectivity* in science means, on the one hand, to assume that there is an *independent reality* (natural, social, or artificial) to be known, and, on the other hand, to admit that the reality has *some properties* which do not depend on either the individual mind of the researcher or the construction of the scientific community working on that object (natural, social, or artificial). Consequently, those properties of the real object should be accessible to more than one mind or community.

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[69] Following B. Barnes, they state that "the boundary between science and technology is, in particular instances, a matter of social negotiation and represents no underlying distinction," PINCH, T. J. and BIJKER, W. E., "The Social Construction of Facts and Artefacts," in SCHARFF, R. C. and DUSEK, V. (eds.), *Philosophy and Technology: The Technological Condition*, p. 223.

[70] Cf. GONZALEZ, W. J., "Towards a New Framework for Revolutions in Science," *Studies in History and Philosophy of Science*, v. 27, n. 4, (1996), pp. 607-625.

[71] Cf. GONZALEZ, W. J., "El realismo y sus variedades: El debate actual sobre las bases filosóficas de la Ciencia," in CARRERAS, A. (ed), *Conocimiento, Ciencia y Realidad*, Seminario Interdisciplinar de la Universidad de Zaragoza-Ediciones Mira, Zaragoza, 1993, pp. 11-58.

Ilkka Niiniluoto explicitly links scientific character and objectivity: "In order to be scientific, inquiry has to be objective at least in two senses. First, the object of investigation has to be real in Peirce's sense, i.e., its characters should be 'independent of what anybody may think them to be' [Collected Papers, 5.405]. Secondly, the object should be allowed to influence the formation of the result of inquiry, and this influence should be intersubjectively recognizable."[72] In addition, if basic science cannot be objective, then it will be unable to follow on the road towards either truth or truthlikeness. And applied science, if it is not able to work on the basis of an objective representation of the world, will have difficulties in resolving concrete problems. Consequently, it seems a mistake of social constructivism to dismiss objectivity in the constitutive elements of science (language, structure, knowledge, method …).

According to these considerations, the relation between science and society from a philosophical approach needs "internal" constituents as well as "external" factors. Ethics of science is a good example of the necessity of both kinds of philosophical analysis of the scientific activity –the internal and the external–[73] which are better known in this case as "endogenous ethics" and "exogenous ethics." Both kinds of analyses are important and, to some extent, they are like two sides of the same coin, because the free human activity of basic science requires ethical values (honesty, responsibility, reliability …) and the social activity of applied science also needs ethical values (due to its relations with persons, social milieu and nature). Furthermore, ethics of science is also relevant in order to show the differences between basic science and applied science, because there are some problems which are specific to the second realm.[74] These varieties of analyses are relevant to the present discussions of bioethics (e.g., in the research on human cloning) and of environmental ethics (e.g., in the contamination of rivers or atmospheric pollution).

4.2. The Relevance of Practice

Another line of the "social turn" introduced by *Science, Technology and Society* or *Science and Technology Studies* is the *relevance of practice*. This is usually a view keen to the epistemology of pragmatists and the methodology of instrumentalists. Following that line, the contribution of the "historical turn" of science as a social activity with internal contents is accepted, but now increasing attention is added to the *practical elements* of science. Thus, there is more interest than before in different issues: a) the role of *instruments* in

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[72] Niiniluoto, I., *Is Science Progressive?*, Reidel, Dordrecht, 1984, p. 4.

[73] On the importance of ethics in scientific research as such and in its social dimension, cf. Agazzi, E., *Il bene, il male e la scienza. Le dimensioni etiche dell'impresa scientifico-tecnologica*, Rusconi, Milan, 1992; Resnik, D. B., *The Ethics of Science*, Routledge, London, 1998; and Shrader-Frechette, K., *Ethics of Scientific Research*, Rowman and Littlefield, Savage (MD), 1994.

[74] Cf. Gonzalez, W. J., "Ciencia y valores éticos: De la posibilidad de la Etica de la Ciencia al problema de la valoración ética de la Ciencia Básica," *Arbor*, v. 162, n. 638, (1999), pp. 139-171.

science, either for the discovery of new facts or for the justification of scientific statements; b) the characteristics of *applied science* as an issue that requires a specific focus, after decades of primacy of basic science for the philosophical approach; and c) the *applications of science* as a topic of special interest for philosophy insofar as science should solve practical problems in the social realm (economic, political, ecological …).

As to the importance of the instruments, especially their role in experiments, there have been interesting contributions over the last two decades.[75] The need for a material support –an artifact made technologically– for scientific discoveries and for the testability of scientific statements was in no way unknown before (at least since Galileo's times), but there are new views about the *character of the experiments* and the contribution of the artificial objects made by the social activity of technology. In addition, these reflections emphasize the "artificial character" of experimentation in the laboratory insofar as there is a dependence on instruments already thought of for some purposes. Again, we are faced with science as social action.

Where the practical utilities do have a key role is in applied science, which frequently includes an interaction between the scientific knowledge and the material support given by technology. There is a clear difference with basic science: the feature of a *practical orientation* of scientific knowledge. Thus, "besides epistemic utility, the knowledge provided by applied science is expected to have *instrumental value* for associated human activity. Applied science is thus governed by what Habermas calls the 'technical interest' of controlling the world."[76] Design sciences, which belong to the sciences of the artificial,[77] are a clear example of the interest in how the things *ought to be* to reach some goals.[78]

Other conceptions in favor of the insistence on science as a *practice* call attention to the applications of science. In this regard, "it is important to distinguish *applied* science from the *applications* of science. The former is a part of knowledge production, the latter is concerned with the use of scientific knowledge and methods for solving practical problems of action (e.g., in engineering or business), where may play the role of a consult."[79] These solutions

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[75] There are two books that have been very influential: Hacking, I., *Representing and Intervening*, Cambridge University Press, Cambridge (MA), 1983; and Galison, P., *How Experiments End*, The University of Chicago Press, Chicago, 1987.

[76] Niiniluoto, I., "The Aim and Structure of Applied Research," p. 6. Cf. Habermas, J., *Erkenntnis und Interesse*, Suhrkamp, Frankfurt, 1968.

[77] The "sciences of the artificial" can be understood in two different ways. On the one hand, the domain different from the natural sciences and the social sciences where design has a key role (e.g., library science, pharmacology, agricultural science …) and it is usually a "scientification" of a profession and, on the other hand, the scientific study of the properties of technology (i.e., the research on the physical, chemical … properties of technological artifacts) such as in the case of "engineering science." The focus is here on the first option.

[78] Cf. Simon, H., *The Sciences of the Artificial*, 3rd ed., The MIT Press, Cambridge (MA), 1996.

[79] "The Aim and Structure of Applied Research," p. 9.

to practical problems are more visible to the members of society than the research that has made the solutions possible. Thus, the applications of science in applied sciences (ecology, economics, medicine, pharmacology, nursing …) received more analysis in *STS* than other disciplines. Those applications, insofar as they are social actions of the scientists, can be analyzed at different levels (aims, means, results, consequences) by the empirical sciences included in *STS*.

From a philosophical point of view, there is again the need to consider the "internal" and "external" aspects. In this regard, one issue of interest is the relation between possible practical success and the cognitive content of the scientific theory used in applied science. To establish "practical success" is clearly more difficult in the case of social sciences than in natural sciences (as can be see frequently in the discussions of contributions of Nobel Prizes in Economics). Niiniluoto suggests using the case of ballistics. It is an applied science heavily linked to technology. He maintains that "practical success does not prove the truth of a theory. … But if Newton's theory were completely mistaken, it would be difficult to understand how it can achieve successful concretization. For this reason, the practical success of a theory is an indicator of its truthlikeness."[80] This aspect is not considered by social constructivism, and one it seems convenient to keep in mind in order to make decisions on social problems connected with science.

5. The Nexus between Technology and Society from a Philosophical Perspective

Following the previous analysis, it seems clear that science and technology have an *increasing practical interaction* (and that is the basis for "technoscience"), which is more visible in some projects related to many scientific groups (such as the Human Genome project or research into several diseases such as cancer). In addition, there is still a *conceptual difference* between "science" and "technology," according to the constitutive elements already pointed out, which has a neat range of repercussions at several levels: aims, processes and results (outcomes or products).[81] That conceptual difference, which also affects the social dimension, is diluted by an instrumentalist methodology that subordinates scientific activity to technological aims and considers that scientific theory is merely a tool for technological design.[82]

Moreover, besides the distinction between science and technology from the internal point of view (i.e., semantic, logical, epistemological, methodological, ontological and axiological), there are variances between scientific activity and

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[80] Niiniluoto, I., "Approximation in Applied Science," *Poznan Studies in the Philosophy of Science and Humanities*, v. 42, (1995), pp. 138-139.

[81] Among the differences in the results is the existence of *patents*, which follow from technological innovations. This phenomenon introduces an additional economic factor which increases the social dimension of technology.

[82] Cf. Lelas, S., "Science as Technology," *The British Journal for the Philosophy of Science*, v. 44, (1993), pp. 423-442; especially, p. 423.

technological doing from the external perspective. The differentiation comes ordinarily from the *complexity* and the *level of repercussion* of the values that intervene (ethical, social, cultural, political, ecological, aesthetic, economic …) Usually, these *external values* influence more deeply technology than science, whereas technology is generally more intelligible than science insofar as it is human-made[83] (i.e., design, process and product –an artifact– are made by human beings).

De facto, scientific progress and technological innovation are causally interdependent –as "technoscience" emphasizes–[84] but they are commonly different as human undertakings. Furthermore, the social milieu is often diverse, because technology has regularly more weight than science in private enterprises insofar as the technological products are more market oriented than the scientific outcomes. In this regard, there are still differences among the institutions or organizations –private or public– devoted to science and technology, even though in recent decades there is an increasing interactive position in favor of a joint venture (mainly in natural sciences and natural technologies).[85]

Those differences also have an incidence in the philosophical approach, because traditionally philosophy of technology has paid more attention to external factors than philosophy of science. "The dominance of the *praxis* traditions, plus the problem sets for philosophy of technology, which are situated more in the ethical-social-political arenas are divergent from the analytic and more dominantly epistemological concerns of most North American philosophers of science."[86] Although there are several cases where –as Don Ihde recognizes– the analytic minded philosophers have focused on the internal problems of technology (mainly, epistemological ones) and they have connected them with other topics, such as human-technology interfaces –internet and sensory devices– to think of the changes in human experience and the use of computer processes to produce models for highly complex phenomena in order to understand them.[87]

5.1. The Social Dimension of Technology

That technology has a more intense social dimension than science can be seen in different ways. The aims, processes and results of technology have *tangible*

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[83] I owe this idea to Herbert Simon.

[84] In addition to the causal interdependence of science and technology, there are other factors of convergence between scientific activity and technological doings. Among them should be emphasized economic rationality. Cf. GONZALEZ, W. J., "Racionalidad científica y racionalidad tecnológica: La mediación de la racionalidad económica," pp. 107-115.

[85] The interaction between social sciences and social technologies is more complex and it is less developed than the cases devoted to the nature.

[86] IHDE, D., "Has the Philosophy of Technology Arrived? A State-of-the-Art Review," p. 127. A clear example of that interest in ethical-social-political arenas can be see in SHRADER-FRECHETTE, K., *Burying Uncertainty: Risk and the Case Against Geological Disposal of Nuclear Waste*, University of California Press, Berkeley, 1993.

[87] Cf. "Has the Philosophy of Technology Arrived? A State-of-the-Art Review," pp. 127-128.

consequences for the citizens which are more visible than the enlargement of human knowledge (basic science) or even the solution to practical problems (applied science). The reason is clear: technology is oriented towards the creative transformation of the reality. Thus its design looks to *change existing reality* (natural, social, or artificial) to produce new results (a kind of human artifact: bridge, airplane, computer, cell phone …) which can affect directly the lives of the members of society. These changes might be in favor of social development or –as the present book points out in several chapters– they may be against the common good of citizens.[88]

Certainly the social dimension appears in the three main stages of the technological doing. 1) It intervenes in the *design*, because technology not only uses scientific knowledge (know that) and specific technological knowledge (know how) but also takes into account social and economic values in the design. This is clear in many technological innovations (new cell phones, faster computers, large airplanes …) that should consider the users of the product and the potential economic rentability of the new artifact. 2) The technological *process* is developed in enterprises –public or private– organized socially according to some values (economic, cultural, ergonomic, aesthetic …) and with an institutional structure (owners, administrators …) 3) The *final result* of technology is a human-made product –an artifact– to be used by society and it has an economic evaluation in the market. Hence, it can be said that technology is ontologically social as a *human doing*. In addition, its product is an item for society. Moreover, the criteria of society have a considerable influence in promoting some kind of innovations (with their patents) or an alternative technology (a new design, process and product).

Frequently, the social dimension of technology is viewed with concern, especially in the case of recent phenomena related to industrial plants (e.g., in accidents related to nuclear energy). But it is also an attitude that appears many times under the reflection on the *limits* of technology, when philosophy asks for the bounds (*Grenzen*) of technology. These terminal limits of technology should take into account the internal values as well as the external values (ethical, social, cultural, political, ecological, aesthetic, economic …). And philosophy of technology considers the external values in the context of a democratic society interested in the well-being of the citizens,[89] thinking that their members can

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[88] There are also reflections on present phenomena in comparison to the past, such as in the case of the Luddites, cf. GRAHAM, G., *The Internet: A Philosophical Inquiry*, Routledge, Londres, 1999, ch. 1; and KITCHER, PH., *Science, Truth, and Democracy*, Oxford University Press, Oxford, 2001, ch. 13, pp. 167-180.

[89] Cf. NIINILUOTO, I., "Límites de la Tecnología," *Arbor*, v. 157, n. 620, (1997), pp. 391-410. In this regard, on the relation about technological rationality and human happiness, cf. RESCHER, N., *Razón y valores en la Era científico-tecnológica*, ch. 8, pp. 169-190.

contribute to decision making (e.g., by means of associations or through the members of the parliament). The limits of technology include the *prediction of* what technology can achieve in the future, but also require the *prescription* of what should be done. And prescription is attached to an evaluation and an assessment of the good and bad for society of the decision (and that is a common practice in applied sciences such as economics).[90]

Sometimes the key is put on the social dimension of technology. Thus, in programs such as SCOT (the Social Construction of Technology) there are analogies with the relativists and social constructivist programs of science. Expressly, SCOT seeks in the sociology of technology similar bases that can be found in the sociology of science of EPOR (the Empirical Program of Relativism). "In SCOT the development process of a technological artifact is described as an alternation of variation and selection. This results in a 'multidirectional' model, in contrast with the linear models used explicitly in many innovation studies and implicitly in much history of technology. Such a multidirectional view is essential to any social constructivist account of technology."[91] But T. J. Pinch and W. E. Bijker, the proponents of the program, recognize that "with historical hindsight, it is possible to collapse the multidirectional model on to a simpler linear model."[92] Their solution is that the "successful" stages in technological development are not the only possible ones, although it is usually the case that the unsuccessful stages are not followed in new technologies.

A sound account of the social dimension of technology needs to be receptive to the internal constituents of technology, because –as Herbert A. Simon has pointed out– technological success requires one to be able to reach the aim (effectiveness)[93] or to do so with economy in the means (efficiency), otherwise it can hardly be taken seriously. That view offers a better solution than the multidirectional social constructivism of SCOT, even though it does not mean that technology is *eo ipso* linear, because it is a complex reality (language, system, knowledge …) which connects aims, processes and results. Thus, the emphasis on the external factors of technology should not dilute the relevance of the internal constituents of technology.

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[90] Cf. GONZALEZ, W. J., "Prediction and Prescription in Economics: A Philosophical and Methodological Approach," *Theoria*, v. 13, n. 32, (1998), pp. 321-345.

[91] PINCH, T. J. and BIJKER, W. E., "The Social Construction of Facts and Artifacts," in SCHARFF, R. C. and DUSEK, V. (eds.), *Philosophy and Technology: The Technological Condition*, p. 227.

[92] PINCH, T. J. and BIJKER, W. E., "The Social Construction of Facts and Artifacts," p. 227.

[93] "Going to the Moon (…) challenged our technological capabilities. (…) Although several potential side effects of the activity (notably its international political and military significance, and the possibility of technological spinoffs) played a major role in motivating the project, they did not have to enter much into the thoughts of the planners once the goal of placing human beings on the Moon had been set. Moreover these by-product benefits and costs are not what we mean when we say the project was a success. It was a success because people walked on the surface of the Moon," SIMON, H. A., *The Sciences of the Artificial*, pp. 139-140.

Both sides –internal and external– are needed in order to clarify the technological processes (in themselves as well as in their historical dynamics).[94] From the internal point of view, the methodology of technology has a central role. It is based on an imperative-hypothetical argumentation, where the aims are crucial to making reasonable or to rejecting the means used by the process of developing a technological artifact. And, from an external perspective, technology requires social values as human undertaking: the technological processes cannot be beyond social control, because society has the right to look for a reasonable ethics of technology and it can seek a rational technological policy for its citizens.

Two different philosophical orientations might be considered here about the process in technology: i) *technological determinism* assumes that the development of technology is uniquely determined by internal laws; and ii) *technological voluntarism* maintains that the change can be externally directed and regulated by the free choice of the members of the society. On the one hand, technological determinists can argue that the development of technology is *de facto* a complex system process where the imperatives have a role (at least, methodologically); but, on the other hand, technological voluntarists can point out that the citizens do not have to obey *eo ipso* those imperatives. Niiniluoto suggests a middle ground between "determinism" and "voluntarism:" the commands of technology are always *conditional*, because they are based on some *value premises*, and then it is correct that we do not need to obey technological imperatives. Therefore, the principle that "can implies ought" is not valid insofar as not all technological possibilities should be actualized.[95]

"Sustainable development" is an important notion in this regard, because it is related to multiple technological processes. Furthermore, it connects with the analysis of what kind of technological possibilities should be actualized. *Sustainable development* combines internal terms –as an epistemic concept– and external ones, due to the social consequences of linking human beings with technology and their being interwoven with the natural environment. It is a notion that includes empirical contents (some of them related to applied sciences) and value premises (social, cultural, political, economic …). But "sustainable development" raises the relevant question of the development of technological processes which can cause damage to the nature.

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[94] The historical dynamics of technology requires to consider the evolutionary changes (the improvements in off-shore platforms, aircrafts, automobiles, …) and the "technological revolutions" (such as the computers). An analysis of the second ones is in Simon, H. A., "The Steam Engine and the Computer: What makes Technology Revolutionary," *EDUCOM Bulletin*, v. 22, n. 1, (1987), pp. 2-5. Reprinted in Simon, H. A., *Models of Bounded Rationality.* Vol. 3: *Empirically Grounded Economic Reason*, The MIT Press, Cambridge (MA), 1997, pp. 163-172.

[95] Cf. Niiniluoto, I., "Should Technological Imperatives Be Obeyed?," *International Studies in the Philosophy of Science*, v. 4, (1990), pp. 181-187.

Philosophically, there is a twofold consideration of these technological processes in nature. On the one hand, technological innovations have produced a world of artifacts which have increased *positive freedom* of members of society. The dominion over nature has contributed to that purpose. But, on the other hand, the transformation of nature made by the social actions of technologists can have *negative consequences*, either intentionally or in an unforseen way (side effects). And sustainable development assumes the idea of a collective responsibility for the environment, because it is a natural reality that belongs to the whole society and to each one of its members.

Understood as a dynamic process of "meeting today's needs without compromising the ability of future generations to meet their own needs",[96] *sustainable development* has been critized as being modeled on a "Western paradigm" of linear growth and progress.[97] And it is well known that the notion is used in an international political program of calling all nations to joint efforts in favor of a secure sustainable development of the planet. As a social project, it includes a relation between means and ends of the kind of "technical norms" (in G. H. von Wright's sense): 'If you want A, and you believe you are in situation B, then you ought to do X'.[98] For Niiniluoto, "technical norms as conditional if-then statements can be objectively established results of science. Still, they are value-laden in two different ways."[99] Firstly, the goals should be *accessible* as well as *desirable*; and secondly, there is a *hierarchy of values* in place: free trade is commonly a supreme principle, and the drastic changes in industry, energy … are thought to be compatible with that value (which is economic as well as social and political).

Again, as in the case of science, a purely instrumental view –a *technocratic conception* of sustainable development, in the present case– is defective: we need to take into account the external values (social, cultural, aesthetic, ecological …). They should be considered to establish the ends (accessible and desirable) and, indirectly, they might have repercussions on the means. Understood in this way, the social dimension of technology can have a role not only in the ends but also in the means: if they intervene in the circle of the aims (and, consequently, in technological design), they can have an effect on technological processes (and, thereafter, in the products). With this philosophical approach, technological rationality is not purely instrumental (means to ends) because it should include the *evaluative rationality*

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[96] According to Niiniluoto, that is the first definition of "sustainable development," and it was used in the *report* "World Conservation Strategy," published by the International Union for the Conservation of Nature and Natural Resources, Gland (Switzerland), 1980.

[97] Cf. Niiniluoto, I., "Nature, Man, and Technology - Remarks on Sustainable Development," in Heininen, L. (ed.), *The Changing Circumpolar North: Opportunities for Academic Development*, Arctic Centre Publications 6, Rovaniemi, 1994, p. 76.

[98] Cf. von Wright, G. H., *Norm and Action*, Routledge and K. Paul, London, 1963.

[99] Niiniluoto, I., "Nature, Man, and Technology - Remarks on Sustainable Development," pp. 80-81.

on the ends.[100] Among the values to be considered are the social values and those ingredients are a guarantee of a better protection of the environment.

5.2. Technology and Economic Values

Economic values have a clear role in the case of technology, both in internal terms (in epistemological and methodological areas) and, in a more visible way, in external terms (in social and political spheres).[101] They have more influence than other values in technology insofar as economic values might be decisive when choosing a concrete design instead of other alternative designs which, from a strictly technological point of view, were more interesting (i.e., a technological innovation, a larger capacity in the artifact, a better operational device …). In addition, they affect the timing of the processes of production (short, middle and long run) as well as the social acceptance of products in the market.

Initially, there are *internal* economic values in the *epistemological component* of technology. Some economic values (such as profitability, competitiveness, productivity …) can affect directly the kind of design. They are based on economic rationality and on the contents of economic science itself, and they can contribute to resolving questions about which technological aims are preferable among those which are accessible. Thus, there is an evaluative rationality regarding the technological ends, which receives the influence of economic evaluations. Those values might affect decision making about available designs and, therefore, about the types of artifacts that should be made.

Also in the *methodological context* of technology there are economic values. The technological process should be oriented towards *efficiency* and not just effectiveness. Thus, economic values are crucial in order to achieve the end with fewer means (either in the same technological process or in comparison with an altervative technology). This “economy of means” (or efficiency) accompanies the *instrumental rationality* of technology, where the cost-benefit relation is a central criterium.[102] It leads to obtaining the chosen technological aim using the minimum possible of procedures. Thus, the economic values move on towards a suitable selection of resources in order to reach the designated aim.

Another sphere in technology is *external* economic values, where the social dimension of technology is manifested and can support empirical studies

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[100] On the notion of “evaluative rationality” in technology, cf. Rescher, N., *Razón y valores en la Era científico-tecnológica*, p. 172. About the distinction among epistemic rationality, practical rationality and evaluative rationality, cf. Gonzalez, W. J., “Rationality in Experimental Economics: An Analysis of R. Selten’s Approach,” in Galavotti, M. C. (ed), *Observation and Experiment in the Natural and Social Sciences*, Kluwer, Dordrecht, 2003, pp. 71-83, especially, pp. 74-76.

[101] Cf. Gonzalez, W. J., “Valores económicos en la configuración de la Tecnología,” *Argumentos de Razón técnica*, v. 2, (1999), pp. 69-96.

[102] Regarding instrumental rationality, “cost effectiveness –the proper coordination of costs and benefits in the pursuit of our ends– is an indispensable requisite of rationality,” Rescher, N., *Priceless Knowledge? Natural Science in Economic Perspective*, University Press of America, Savage (MD), 1996, p. 8.

(economics of technology, sociology of technological change …). External economic values have a role at two different levels: in the *technological activity* as a *social doing* (i.e., the process of devoloping a specific technology in a social setting) and in the product as an element involved in a *technological policy* (i.e., the final technological result as a factor of the policy, either of the public sector or the private organizations and corporations).

Technology is displayed as a *social action* in a historical setting. It has intentionality –in principle, to serve society– and is oriented towards a creative transformation of reality. This modification is guided by criteria of effectiveness and efficiency, which have a clear economic character in order to develop a specific technology (electrical, mechanical, chemical …). Thus, the technological activity itself requires us to take into account economic values –the cost of production– of making the artifact. In addition, the final technological result –the product– has a *tout court* economic value in the market. Thus, technology is affected by the rationality of economic agents (a bounded rationality) and the modification of the parameters of economy, due to changes in the conditions of the social milieu.[103]

Along with technology as a social doing, there is a *technology policy* of the public institutions and of the private enterprises, whose regulations can have a repercussion on the orientation of the technological development. The economic values appear clearly in the system of organizations and activities of research, development and innovation (R and D and I). This policy includes a significant percentage of the gross domestic product of countries (mainly in the US, European Union, Russian Federation and Japan). It has clear repercussions on technological change, especially in some economic sectors, such as energy (nuclear, solar, wind power …) and naval engineering (ship builders). This is due to the priorities of technological policy which include economic values (principally, the cost-benefit ratio). In addition, a sound technological policy should channel technology to protect nature and society, avoiding negative developments, because technology is not a merely economic phenomenon: its effects are relevant for culture and society as a whole.[104]

Undoubtedly, following these analyses on the nexus between technology and society, as well as the previous ones on the relation between science and society, it seems clear that the philosophical approach can contribute to studies on science and technology in different ways. Among them is the clarification of the *external factors* (social, economic, political …) of scientific activity (basic and applied) and technological doing. Insofar as the philosophical approach preserves the interest in the internal components of science and technology (language, knowledge …), the academic image of *Science and Technology Studies* and its

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[103] Cf. SIMON, H. A., “Economics as a Historical Science,” *Theoria*, v. 13, n. 32, (1998), pp. 241-260.

[104] Cf. NIINILUOTO, I., “Límites de la Tecnología,” p. 392.

intellectual contribution will be more balanced than the conceptions of relativists and social constructivists that have been influential in recent times.

Now, after the insistence on the need for internal constituents and external factors both in science and technology, it seems clear to me that scientific activity and technological doing can combine objectivity (in the sense and reference, in epistemic contents …) and intersubjective ingredients (social, cultural, economic, …). Thus, a realistic picture of *STS* is twofold. On the one hand, although science and technology are autonomous[105] (both are self-corrective for the revision of the results of their processes), they are not, in principle, context-independent insofar as their aims, processes and results receive the influence of social setting.[106] On the other hand, scientific activity and technological doing are not reducible to mere external factors (social, cultural, political, economic, ecological …) due to the relevance of their internal constituents. The "social turn" has emphasized contextual values but we also need the specificity of scientific activity and technological doing in order to have a complete image of the relations between science, technology and society.

6. The Structure and Origin of this Book

As the main goal of this book is to offer an updated analysis of the philosophical perspective on *Science, Technology and Society* or *Science and Technology Studies*, the structure of the volume follows four steps, which focus on different domains. Firstly, there is a theoretical framework about *STS* and the role of philosophy in it. This gives place to a consideration of the epistemic as well as the ethical attitude towards science and technology. Secondly, there is an analysis of the present situation in some important aspects (mainly in the sphere of regulatory science) and a vision of the future of *STS* as a practice rather than a "contemplative" research. Thirdly, the focus shifts to the relation between science and society in some key issues: design sciences and the characterization of experiments from a social point of view. Fourthly, attention goes to the nexus between technology and society, taking into account the patterns of rationality and technological change.

Each one of these domains, starting from the most general –the first– through to the most particular –the latest–, has a section in the book. 1) *Theoretical framework*: "The Philosophical Approach to Science, Technology and Society," Wenceslao J. Gonzalez (University of A Coruña), and "Objectivity and Professional Duties Regarding Science and Technology," Kristin Shrader-Frechette (University of Notre Dame). 2. *STS: From the present situation to the future projection*: "Metascientific analysis and methodological learning in

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[105] "The community of investigators ceases to be a scientific community if it gives up –or is forced to give up– this principle of autonomy," Niiniluoto, I., *Is Science Progressive?*, p. 6.

[106] "It would be wrong to maintain that the interests of scientists are always antagonistic to those of the broader public," Kitcher, Ph., *Science, Truth, and Democracy*, p. 127.

regulatory science," José Luis Luján (University of the Balearic Islands), and "How to Reform Science and Technology," Kristin Shrader-Frechette (University of Notre Dame). 3. *The Relation between Science and Society*: "Progress and Social Impact in Design Sciences," Anna Estany Profitos (Autonomous University of Barcelona), and "Experiments, Instruments and Society: Radioisotopes in Biomedical Research," María Jesús Santesmases (Higher Council of Scientific Research). 4. *The Nexus between Technology and Society*: "Philosophical Patterns of Rationality and Tecnological Change," Ramón Queraltó Moreno (University of Seville).

Originally, these papers were delivered at the *Jornadas sobre Ciencia, Tecnología y Sociedad: La perspectiva filosófica* (Conference on Science, Technology and Society: A Philosophical Perspective), organized by the University of A Coruña with the support of the Society of Logic, Methodology and Philosophy of Science in Spain. The meeting was held at the Campus of Ferrol on 11th and 12th of March 2004. The discussions were oriented towards the main goal: the philosophical stance on this interdisciplinary endeavor. As in the case of the previous *Jornadas sobre Filosofía y Metodología actual de la Ciencia* (Conferences on Present Philosophy and Methodology of Science), the ninth edition of these meetings has its central interest in the reflections developed nowadays.

Basically, every paper was focused in this direction, either in a clearly reflective way or in a more active tendency. The conference had a central figure: Kristin Shrader-Frechette, who tends towards the second disposition. She studied physics at *Xavier University* (1966) and was later awarded a *B. A.* in mathematics by *Edgecliff College* (1967). Thereafter, she prepared the dissertation in philosophy of science at the University of Notre Dame (1972). This was followed by postdoctoral work in several realms: community ecology (two years), economics (one year) and hydrogeology (two years). She has held senior professorships at the University of California and the University of Florida. Currently she is O'Neill Family Professor of Philosophy and Concurrent Professor of Biological Sciences at University of Notre Dame.

Kristin Shrader-Frechette has held fellowships in philosophy of science from important entities: the Woodrow Wilson Foundation, the National Science Foundation, the Carnegie Foundation … She was president of the committee for science and ethics of the *International Conference of Scientific Unions* (1990-96). Among her present activities is that of Editor –in– Chief of the Oxford University Press monographs series "Environmental Ethics and Science Policy," since 1988. In addition, she serves on the editorial board of *Business Ethics Quarterly*, *Encyclopedia of the Philosophy of Science*, *Humanities and Technology*, *Philosophy and Technology*, *Public Affairs Quarterly*, *Synthese* … She is article referee for *Economics and Philosophy*, *Philosophy of Science*, *Science*…[107] Before

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[107] About her academic career and her publications there is a detailed information in Kristin Shrader-Frechette's website: <*www.nd.edu/~kshrader*>.

her contribution to the ninth edition of conferences on present philosophy and methodology of science, a relevant number of important philosophers contributed to these meetings. Larry Laudan analyzed in 1996 the relation between history of science and philosophy of science; Ilkka Niiniluoto developed in 1997 his ideas on scientific progress and technological innovation; Evandro Agazzi emphasized in 1998 the relation between science and ethical values; Daniel Hausman contributed in 1999 to the reflection on philosophy and methodology of economics; John Worrall offered in 2000 new important insights on Lakatos's philosophy after 25 years; Wesley Salmon intervened in 2001 with new thoughts on scientific explanation; in 2002 Peter Machamer shed light on Kuhn's scientific revolutions; and Donald Gillies in 2003 presented new aspects on Karl Popper's views. All these topics have a volume in the collection *Gallaecia*.[108] Besides the papers of these authors, other relevant philosophers (Merrilee Salmon, James E. McGuire, Jarrett Leplin …) also have chapters in the books.

When a new edition of these conferences is published, it is time to give thanks to the sponsors. Among them are the City Council of Ferrol: the mayor, the deputy mayor and City councilor responsible for Campus, and the councilor regarding Culture. They have appreciated the relevance of the cooperation between university and society, which is especially important for this conference. My gratitude is also to the Santander Central Hispano bank, which contributes to the activities promoted by the vice-chancellor of cultural extension of the University of A Coruña. And, within the closest circle, my acknowledgement goes to the Faculty of Humanities in the person of its dean. In addition, regarding these *IX Jornadas*, my thanks go to all those –mainly students– who have aided the conference both in the material and organizative domain as well as in its administrative management. Furthermore, I would like to emphasize the role of the mass media of our area (press, radio and television channels), which have highlighted the interest of these conferences for the city.

Finally, I wish to express my warm gratitude to Kristin Shrader-Frechette for accepting this invitation to the conference and the papers as well as her interest in the details related to this activity. I would like also to thank my colleagues Anna Estany (Autonomous University of Barcelona), José Luis Luján (University of the Balearic Islands), Ramón Queraltó (University of Seville) and María Jesús Santesmases (Higher Council of Scientific Research) for their contribution to the

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[108] The collection *Gallaecia. Studies in Present Philosophy and Methodology of Science* includes the following volumes: GONZALEZ, W. J. (ed.), *Progreso científico e innovación tecnológica*, monographic issue of *Arbor,* v. 157, n. 620, (1997); GONZALEZ, W. J. (ed.), *El Pensamiento de L. Laudan. Relaciones entre Historia de la Ciencia y Filosofía de la Ciencia*, Publicaciones Universidad de A Coruña, A Coruña, 1998; GONZALEZ, W. J. (ed.), *Ciencia y valores éticos*, monographic issue of *Arbor*, v. 162, n. 638, (1999); GONZALEZ, W. J. (ed.), *Problemas filosóficos y metodológicos de la Economía en la Sociedad tecnológica actual*, monographic issue of *Argumentos de Razón técnica*, v. 3, (2000); GONZALEZ, W. J. (ed.), *La Filosofía de Imre Lakatos: Evaluación de sus propuestas*, UNED, Madrid, 2001; GONZALEZ, W. J. (ed.), *Diversidad de la explicación científica*, Ariel, Barcelona, 2002; GONZALEZ, W. J. (ed.), *Análisis de Thomas Kuhn: Las revoluciones científicas*, Trotta, Madrid, 2004; and GONZALEZ, W. J. (ed), *Karl Popper: Revisión de su legado*, Unión Editorial, Madrid, 2004.

conference and this volume. I also thank the authors of the contributed papers and the participants of the conference for their roles during those days. Last but not least, I am grateful to José Fco. Martínez Solano for his assistance in editing this volume.

7. BIBLIOGRAPHY

Within the vast literature on *Science, Technology and Society* or *Science and Technology Studies*, the present bibliographical selection seeks to offer those titles which might be useful as a road map of the field. Thus, it connects with the topics of this chapter and completes the information given in the previous pages, but it is not conceived as an exhaustive list of publications on *STS*, because the diversity of studies on science and technology presumably requires a monographic volume to display the bibliography. In this regard, and in tune with the characteristics of this book, which are to contribute to the *philosophical approach* on *STS*, there is a clear compatibility with other volumes on the different subject-matters of *Science and Technology Studies* (practical ethics, policy analysis, legal studies, sociology of science and sociology of technology, economics of science and economics of technological change …). Furthermore, each one of the papers in this book includes specific references to the topics with which they deal.

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OBJECTIVITY AND PROFESSIONAL DUTIES REGARDING SCIENCE AND TECHNOLOGY*

Kristin Shrader-Frechette

Royal Dutch Shell discovered oil in the Niger River delta in 1958, and soon after, it became the largest oil producer in Nigeria. The company received heavy criticism because it provided oil revenues to the Nigerian military government but not to the Ogoni tribe whose land and people have been destroyed by its oil drilling.[1] Even worse, Nigerian military officers said Shell put pressure on the Nigerian government to clamp down on Ogoni people who protested Shell's lax environmental behavior. Thousands of Ogoni have been killed for doing nothing more than engaging in nonviolent protests against the destruction of their farmland and streams by uncontrolled oil spills, uncontrolled oil leaks, and uncontrolled natural gas flaring.

1. KEN SARO WIWA

Nigerian writer Ken Saro-Wiwa –formerly a grocer, teacher, writer, and television producer– criticized "the collusion of commercial [Shell] and military [Abacha regime] force" responsible for destroying the Nigerian environment and dehumanizing the Ogoni people. Although he had enough money to settle comfortably and continue as a television producer and writer, Saro-Wiwa chose instead to be an advocate and activist. He founded the non-violent human-rights and environmental group, MOSOP (Movement for the Survival of the Ogoni People); organized peaceful Ogoni protests; condemned Shell's genocide; and argued for cleanup. For his efforts, Saro-Wiwa won numerous international civic and environmental awards. His son, a Nobel prize-winning author, Wole Soyinka, is continuing his father's human-rights efforts. But in spite of widespread protests from the international community, in November 1995 the Nigerian military government, dependent on Shell money, held a "kangaroo court," dominated by Shell lawyers, then hanged Saro-Wiwa and other nonviolent MOSOP advocates and activists.[2]

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[1] Cf. BIELSKI, V., "Shell's Game," *Sierra*, v. 81, no. 2 (1996), pp. 30-36; WHEELER, D., "Blood on British Business Hands," *New Statesman and Society*, v. 8, no. 379 (1995), p. 14. See also MCLUCKIE, C. W., *Ken Saro-Wiwa, Writer and Political Activist*, Lynne Rienner Publishers, Boulder, 1999. "Cruelty Under the Microscope," *Economist*, v. 357, no. 8197 (2000), p. 58; PEGG, S., "Ken Saro-Wiwa," *Third World Quarterly*, v. 21, no. 4, (2000), pp. 701-708; and DANIELS, A., "The Perils of Activism: Ken Saro-Wiwa," *New Criterion*, v. 18, no. 5, (2000), pp. 4-9.

[2] Cf. WHEELER, D., "Blood on British Business Hands," pp. 14-15; see BOYD, W., "Death of a Writer," *The New Yorker,* v. 71, no. 38, (1995), pp. 51-55. See "The Hidden Lives of Oil," *Chronicle of Higher Education,* v. 48, no. 30, (2002), pp. B7-B10.

Brian Anderson, head of Shell Nigeria, told Saro-Wiwa's brother that he could save his brother's life, provided Saro-Wiwa and MOSOP stopped nonviolent protests against Shell. Saro-Wiwa and MOSOP refused. As a result, the military government hanged the 9 nonviolent environmental activists.[3]

Shortly after the hangings, Shell had to hire 7 US public relations firms to handle global protests of Shell's and Nigeria's behavior. Members of Britain's Royal Geographical Society voted to expel Shell as one of its sponsors because of its Nigerian operations. And the 52-member British Commonwealth suspended Nigeria and said that, in order to avoid expulsion, Nigeria would have to show that it adhered to the human-rights principles of the group. Britain, the US, South Africa, Germany, and Austria recalled their ambassadors to Nigeria in response to the hangings. So did the 15 member nations of the European Union. The EU suspended its development aid to Nigeria, and the World Bank rejected a $100 million loan to Nigeria. Also in response to Saro-Wiwa's death, a huge coalition of government, labor, human-rights, and NGO groups boycotted the Nigerian military dictatorship. Shell Oil is still fighting in court to avoid paying damages for the human and environmental problems it has caused in Nigeria.[4] Ken Saro-Wiwa, however, has won. He has brought international attention to the unjust conditions Shell Oil imposed on his people.

Saro-Wiwa's actions were ethically uncontroversial insofar as they were nonviolent and insofar as virtually all western democracies in the world agreed with his tactics and his stance. From the point of view of universalizability, however, his actions are controversial because not everyone can be expected to follow them. Is there a less controversial case of a person attempting to reform the practice and use of science? A case that is more universalizable?

2. Ralph Nader

Called the "modern-day champion of the little person," a "male Jeanne d'Arc," and "the people's lawyer",[5] Ralph Nader has spent his life working to reform science and technology, to change the chronic violence of manufacturers against people and the environment. For three decades, he has fired the opening guns

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[3] Cf. Bielski, V., "Shell's Game," pp. 30-36. See Ainger, K., "Interview with Owens Wiwa," *New Internationalist*, no. 351, (2002), pp. 33-34.

[4] Cf. Mitchell, J. G., "Memorial to a Warrior for the Environment," *National Geographic*, v. 189, no. 4 (1996), p. xxiv; Mayall, J., "'Judicial Murder' Puts Democratic Values on Trial," *The World Today*, v. 51, no. 12, (1995), pp. 236-239; Kupfer, D., "Worldwide Shell Boycott," *The Progressive*, v. 60, no. 1 (1996), p. 13; Adams, P., "A State's Well-oiled Injustice," *World Press Review*, v. 43, no. 1 (1996), pp. 14-15; Pypke, D., "Partners in Crime," *World Press Review*, v. 43, no. 1, (1996), p. 16; Harington, H., "A Continent's New Pariah," *The Banker*, v. 145, no. 838 (1995), pp. 63-64; Boyd, W., "Death of a Writer," pp. 51-55; Knott, D., "Shell the Target After Nigerian Executions," *Oil and Gas Journal*, v. 93, no. 47, (1995), p. 37; and Anderson, A., "A Day in the Death of Ideals," *New Scientist*, v. 148, no. 2005 (1995), p. 3. See Larson, V., "Court Case Against Shell Can Proceed," *World Watch*, v. 15, no. 4 (2002), pp. 7-8.

[5] Cf. Gorey, H., *Nader and the Power of Everyman*, Grosset and Dunlap, New York, 1975, pp. 147 and 176. See Goldsmith, Z., "Mr. Nader Goes to Washington," *Ecologist*, v. 31, n. 1, (2001), pp. 26-27.

of campaigns that brought improved regulation of technologies associated with the automobile, chemical, gas, meat, nuclear, and textile industries. His crusades have led to dozens of laws, from the 1966 Freedom of Information Act and the 1966 National Traffic and Motor Vehicle Safety Act to the 1972 Consumer Product Safety Act. Nader also helped create the Environmental Protection Agency and the Occupational Safety and Health Administration. In countless ways, he has improved the health and welfare of ordinary people. He campaigned for mandatory air bags in cars, and they became standard equipment.[6]

Nader's commitments come in large part from his Lebanese immigrant parents who taught him to believe in the common people. Nader's greatest strength, according to a congressional aide, is his spiritual quality. "He moves around town like some fifteenth-century Franciscan, compelling men to act for the good."[7]

Born in 1934, Nader attended courtroom hearings as a five-year-old. While he was still in grammar school, he read *The Jungle*, Upton Sinclair's 1906 exposé of the federal meat-inspection system. As a Princeton undergraduate, he distinguished himself academically but could not understand campus indifference to threats like heavy spraying of DDT. After graduating from Harvard Law School and publishing his book on automobile safety, *Unsafe at Any Speed*, Ralph Nader became a national figure. Passionately attacking the American automobile industry, he argued that car manufacturers sacrifice consumer safety for the sake of high profits. His book showed that, over the previous 65 years, shoddy technology helped cause more than 1.5 million US auto deaths. The industry response was vicious. After several years and thousands of dollars spent on undercover detectives, General Motors was forced to give up its investigation of Nader. In Congressional hearings, the industry admitted that its private investigators could not find any compromising information on Nader's personal life. Suddenly Nader's name became a household word and his book, a best seller. These admissions enabled Nader to sue GM for invasion of his privacy, and the auto manufacturer was forced to pay him nearly half a million dollars in damages. He used the money to open his Washington-based Center for the Study of Responsive Law. Almost single-handedly, Nader launched the consumer-and environmental-protection movements. College students, doing public-interest research as "Nader's Raiders," worked with him, and "Public Interest Research Groups" (PIRGs) sprang up on hundreds of US campuses. Working to reform the use of science and technology, especially through his NGO "Public Citizen," Nader's coworkers see themselves not as radicals but as patriots.[8]

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[6] Cf. McCarrey, C., *Citizen Nader*, Saturday Review Press, New York, 1972, pp. 29, 115, and 138; Scarlott, J., "Ralph Nader," in DeLeon, D. (ed.), *Leaders from the 1960s*, Greenwood Press, Westport (CT), 1994, p. 330; Stewart, T., "The Resurrection of Ralph Nader," *Fortune*, May 22, (1989), p. 106.

[7] McCarrey, C., *Citizen Nader*, p. 129; see also *Citizen Nader*, pp. 13ff., and 319.

[8] Cf. McCarrey, C., *Citizen Nader*, pp. 28, 44, and 196; Scarlott, J., "Ralph Nader," in DeLeon, D. (ed.), *Leaders*, pp. 330-331; Stewart, T., "The Resurrection of Ralph Nader," p. 106.

Although Nader has been called “the single most effective antagonist of American business”,[9] he actually is a proponent of free enterprise. Nader argues that corporate abuses are possible only when market competition is not informed and open. US chemical manufacturers, for instance, are able to sacrifice consumer safety for higher profits only when the people have neither information about toxins nor alternatives to their use. Whenever consumers enjoy both full information and open competition, Nader says government regulation is unnecessary. Regulation, he claims, can promote monopolies that ultimately threaten consumer interests. Government regulation has given US utilities monopolistic control that has enabled them to avoid clean-energy technologies and to promote dirty ones, like nuclear power. As the antidote for such dominance of special interests, Nader promotes widespread citizen action and informed, open competition.[10]

In taking uncompromising public-interest positions, Nader admits he is not neutral. Washington, he says, doesn’t give one “the luxury of dealing in shades of gray.” He claims some issues are black and white because the stakes are high.[11]

Nader’s black-and-white approach raises an important question. Do informed, critical, attempts at reforming science and technology compromise objectivity? Nader says they do not. He claims that all citizens, and especially professionals, should nurture “conscience and competence.... an obligation to advance or protect the general interest.”[12] Do citizens, especially professionals, have a duty to be public-interest advocates, as Nader suggests? Or should they remain neutral, in the name of objectivity?

3. Objectivity and Neutrality

Several famous philosophers, like G. E. Moore, support the ideal of complete neutrality.[13] Richard Hare, for example, maintains that philosophy ought to involve no advocacy, only impartial training in mental skills.[14] Both Moore and Hare appear to define objectivity in terms of neutrality. Yet if Hare were correct, it would be impossible for philosophers to draw conclusions about which acts and ethical norms were more correct. Because philosophy includes normative ethics, it cannot always be purely neutral. Besides, if philosophers were always neutral, despite their expertise in ethics, then heinous consequences might be more likely. If professionals always remained neutral in the midst of controversies affecting

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[9] Gorey, H., *Nader and the Power of Everyman*, p. 29.

[10] Cf. Gorey, H., *Nader and the Power of Everyman*, pp. 43, 46, 64, and 76-77. See Nader, R. et al., *The Case Against Free Trade, GATT, NAFTA, and the Globalization of Corporate Power*, Earth Island Press and North Atlantic Books, San Francisco and Berkeley (CA), 1993.

[11] Cf. Gorey, H., *Nader and the Power of Everyman*, p. 199; McCarrey, C., *Citizen Nader*, pp. 1-29; and Thomas, R., “Safe at This Speed?,” *Newsweek*, n. 124, (August 22, 1994), p. 40.

[12] Gorey, H., *Nader and the Power of Everyman*, p. 29.

[13] See Midgley, M., *Wisdom, Information, and Wonder*, Routledge, London, 1989, p. 153, for criticism of Moore on this point.

[14] Cf. Midgley, M., *Wisdom, Information, and Wonder*, pp. 93, 99, and 106.

the common good, injustices might remain uncriticized. Plato spoke out against the civic ills of his time. He realized that participation in current ethical and political arguments promoted both personal growth and better public policies.[15] And John Locke criticized the alleged divine right of kings. When he argued instead for democracy based on the consent of the people he was hunted down for treason. In a letter to Norman Malcolm, Ludwig Wittgenstein made professional duties clear: "What is the use of studying philosophy if all that it does for you is to enable you to talk with some plausibility about some obtuse questions of logic, etc. and if it does not improve your thinking about the important questions of everyday life?."[16]

One of the reasons citizens, professionals, and scholars often fail to help reform science and technology, and therefore to act as advocates for the public interest, is that they accept an erroneous model of objectivity. According to this positivistic model, people are "objective" when they take no stances and remain completely neutral, as Moore and Hare advised. A corollary of this position is that whenever people's words or actions are not completely neutral, they are biased in a reprehensible way. According to this position, any sort of advocacy or activism, even in the name of the public interest, is evidence of prejudice. On the contrary, according to the model of objectivity this essay defends, all people, especially professionals, sometimes have duties to be advocates for the common good. They should not always remain merely passive observers of society, in part because genuine objectivity often requires advocacy or criticism.[17] Besides, if Quine, Kuhn, Kitcher, and hosts of others are correct, no claim can be neutral in the sense of being wholly free of evaluative inferences. And if not, then although some claims are more objective (less biased) than others, none are completely value-free. Some people erroneously believe there are neutral or value-free positions, perhaps because they fail to distinguish among different types of values, only some of which reflect bias. Because some kinds of value judgments underlie all claims, even in science, people have duties to avoid only the value judgments that are both biased and avoidable. But which are biased and avoidable?

On Longino's classification, there are three types of value judgments-bias, contextual, and constitutive-and they are neither mutually exclusive nor exhaustive. *Bias values* occur whenever people deliberately misinterpret or omit something so as to serve their own purposes. Obviously people always can and ought to avoid all bias values. *Contextual values* are more difficult to escape. They include personal, social, cultural, or philosophical emphases. Scientists employ contextual values if financial constraints force them to use particular methods

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[15] Cf. Midgley, M., *Wisdom, Information, and Wonder*, pp. 93, 99 and 106. See also Farrellaj, C., "Public Reason, Neutrality and Civic Virtues," *Ratio-Juris*, v. 12, no. 1, (1999), pp. 11-25.

[16] Midgley, M., *Wisdom, Information, and Wonder*, p. 239.

[17] Some of the analysis in this chapter relies on Shrader-Frechette, K., *Risk and Rationality*, University of California Press, Berkeley, 1991, ch. 4, pp. 53-65. See also Sher, G., *Beyond Neutrality: Perfectionism and Politics*, Cambridge University Press, New York, 1997.

or data rather than others. Contextual values might lead scientists to accept old data rather than to generate new information. Although in principle it sometimes is possible to avoid contextual values, in practice it is difficult to do so, because context influences everyone. Korenbrot, for instance, showed that the contextual value of limiting population growth has influenced many medical researchers who have overemphasized the benefits of oral contraceptives and underestimated the risks. Contextual values, such as the profit motive, heavily influence science, in part, because any research or belief is hampered by incomplete information. Facing an unavoidable data gap, people must use contextual value judgments to bridge the gap,[18] or avoid all judgments based on incomplete information or induction.

Constitutive or methodological values are even more difficult to avoid because they are necessary in choosing one method or rule of inference rather than another. Scientists collecting data must make value judgments about what data to gather, what to ignore, how to interpret observations, how to avoid erroneous interpretations. These constitutive value judgments are essential, even to pure science, because human perception does not provide people with pure facts. Instead, beliefs and values (that people already hold) play a key part in providing categories for interpreting observations. High-energy physicists, for example, do not count all the marks on their cloud-chamber photographs as observations of pions. They count only those streaks that their *beliefs* indicate are pions. Just as social, political, and economic contexts frequently frame beliefs, so also do scientific and logical methods. Methodological values unavoidably structure all knowing because there is no complete separation between facts and values, and all facts are laden (at least) with some methodological values.[19] If facts and values were separable, it would be impossible to develop theories or to explain causal connections among phenomena. Because methodological values influence what people see and how they see it, such values are not avoidable and, at best, people can make only better, rather than worse, methodological value judgments. Although people can avoid all bias values,[20] deliberate misinterpretations and omissions,[21] they cannot avoid methodological values.

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[18] Cf. Longino, H., *Science as Social Knowledge*, Princeton University Press, Princeton, 1990. See Scott, P., "Captives of Controversy: The Myth of the Neutral Social Researcher," *Science, Technology, and Human Values*, v. 15, no. 1, (1990), pp. 474-494, and Bornstein, R., "Objectivity and Subjectivity in Psychological Science," *Journal of Mind and Behavior*, v. 20, no. 1, (1999), pp. 1-16.

[19] For discussion of relevant examples from the history of science, see Brown, H. I., *Perception, Theory and Commitment*, The University of Chicago Press, Chicago, 1977, pp. 97-100 and 147, and Shrader-Frechette, K., "Recent Changes in the Concept of Matter: How Does 'Elementary Particle' Mean?," in Asquith, P. D., and Giere, R. N. (eds.), *Philosophy of Science Association 1980*, v. 1, Philosophy of Science Association, East Lansing, 1980, pp. 302-312.

[20] Cf. Midgley, M., *Wisdom, Information, and Wonder*, pp. 80-81.

[21] See Bevir, M., "Objectivity in History," *History and Theory*, v. 33, no. 3, (1994), pp. 328-344. See Longino, H., *Science as Social Knowledge: Values and Objectivity in Scientific Inquiry*, pp. 109-121. See also Shrader-Frechette, K., *Science Policy, Ethics, and Economic Methodology*, Reidel, Dordrecht, 1984, p. 73, and Shrader-Frechette, K., *Risk and Rationality*, pp. 40-44. See Ellis,

4. OBJECTIVITY

But if avoiding methodological values is impossible, then public-interest advocates have no duty to do so. But not all methodological value judgments are created equal. Some are more objective and defensible than others. Although all values are partially subjective in the sense that none can be empirically confirmed, not all are *subjective in a reprehensible way*, because not all values are biased or arbitrary. Conceptual and logical values, like explanatory or predictive power, may help guarantee objectivity. Just as there are good reasons, short of empirical confirmation, for accepting one belief over another, so also there are good reasons for accepting one value judgment over another.[22]

If not all value judgments are subjective in a reprehensible way, and if some values are better than others, then advocating better values, is defensible on epistemological and ethical grounds.[23] Yet many people wrongly assume that all advocacy entails bias. If it did, then any criticisms (whether of unjust ethical positions or faulty science) would be biased. But if any criticisms were biased, then one would have to avoid criticism of things such as heinous crimes or irrational inferences. Obviously it makes no sense to avoid such criticisms. And if not, criticism or advocacy need not involve bias.[24] In fact, criticism or advocacy may be the only way to avoid bias. If it is impossible to avoid some value judgments, even in science, then people who do not criticize poor judgments merely endorse whatever values are dominant. Such people –who believe that objectivity requires complete neutrality– also err through *inconsistency*; they implicitly *sanction* status-quo values if they are silent about questionable value judgments, yet they explicitly *condemn* work that sanctions value judgments.

Remaining neutral, in the face of flawed beliefs, also jeopardizes objectivity as well as consistency. Suppose a special-interest group uses largely political reasons for accepting a particular belief. A nuclear utility might employ unrealistic assumptions about future energy demand in order to argue for building breeder reactors. Not to criticize such unrealistic assumptions or value judgments is wrong, because not all assumptions about future electricity use are equally correct. It is more reasonable to assume higher energy costs will reduce, rather than increase, demand. And if so, the most *objective* thing to do, in the presence of questionable public-policy assumptions, is to be critical of them, not to remain neutral.

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[22] Cf. SHRADER-FRECHETTE, K., *Science Policy, Ethics, and Economic Methodology*, pp. 73-74; SHRADER-FRECHETTE, K., "Scientific Method and the Objectivity of Epistemic Value Judgments," in FENSTAD, J., HILPINEN, R. and FROLOV, I. (eds.), *Logic, Methodology, and the Philosophy of Science*, Elsevier, New York, 1989, pp. 373-389. See also GRUENDER, D., "Values and Philosophy of Science," *Protosociology*, v. 12, (1998), pp. 319-332.

[23] Cf. SHRADER-FRECHETTE, K., *Science Policy, Ethics, and Economic Methodology*, p. 183. See AUDI, R., *The Structure of Justification*, Cambridge University Press, New York, 1993.

[24] Cf. MARGOLIS, J., "On the Ethical Defense of Violence and Destruction," in HELD, V., NIELSEN, K., and PARSONS, C. (eds.), *Philosophy and Political Action*, Oxford University Press, New York, 1972, pp. 52-71. See also ENNIS, R. H., "Is Critical Thinking Culturally Biased?," *Teaching Philosophy*, v. 21, no. 1, (1998), pp. 15-33.

On one hand, many ethical relativists deny there is any objectivity, and they *overemphasize* the value judgments in knowledge. They reduce all knowledge claims merely to social constructs. On the other hand, many naive positivists –and other proponents of a sharp distinction between facts and values– *underemphasize* values. They reduce all knowledge claims to factual or logical truths. They ignore the evaluative aspects of knowing. A more plausible account of objectivity falls midway between the views of the cultural relativists and those of the naive positivists. According to the middle view defended here,[25] objectivity is not tied to freedom from all values but to freedom from *bias* values. It is tied to fair and even-handed representation of the situation.

If these arguments are correct, then objectivity, as freedom from bias, often requires advocacy for correct or less biased positions. Because people can be *blamed* for their failure to be objective, or *unbiased*, it must be possible for people to be more or less objective. But how might people recognize objectivity in a given situation? In the most minimal sense, beliefs or positions are objective and avoid bias if they survive the criticisms of those knowledgeable about them and potentially affected by them.[26]

It seems reasonable to define "objectivity" in terms of surviving such criticism because criticism need not be subjective. For example, when people make methodological value judgments about which of two environmental risk probabilities is more accurate, they are not talking merely autobiographically or subjectively. They are making claims about characteristics of *external* events that other people are capable of knowing. Moreover, the skills associated with making these judgments are a function of experience, education, and intelligence. But if so, at least three reasons suggest objectivity does not require having either an algorithm or empirical data guaranteeing the correctness of the resulting judgments. *First*, *empirical* factors (such as actual accident frequencies) could anchor objectivity and change the correctness of *judgments* about risk. *Second*, ethical factors could anchor objectivity, because people have duties to follow their contracts and to treat others consistently. *Third*, explanatory power could anchor objectivity, because reasonable people usually accept beliefs as objective if they are able to explain problems and

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[25] Cf. SHRADER-FRECHETTE, K., *Risk and Rationality*, ch. 4, pp. 53-65.

[26] See HEMPEL, C. G., "Scientific Rationality: Analytic vs. Pragmatic Perspectives," in GERAETS, T. S. (ed.), *Rationality To-Day*, University of Ottawa Press, Ottawa, 1979, p. 56; HEMPEL, C. G., "Valuation and Objectivity in Science," in COHEN, R. and LAUDAN, L. (eds.), *Physics, Philosophy, and Psychoanalysis*, Reidel, Dordrecht, 1983, p. 91; MCMULLIN, E., "Values in Science," in ASQUITH, P. (ed.), *Philosophy of Science Association 1982*, v. 2, Philosophy of Science Association, East Lansing, 1983; SELLARS, W., *Philosophical Perspectives*, C. Thomas, Springfield (IL), 1967, p. 410; and SMITH, M., "Objectivity and Moral Realism," in HALDANE, J. and WRIGHT, C. (eds.), *Reality, Representation, and Projection*, Oxford University Press, New York, 1993, pp. 235-256.

Moreover, according to the standard version of "discourse ethics," the objectivity of moral norms resides in their intersubjective acceptability under idealized conditions of discourse. See APEL, K.-O., *Towards a Transformation of Philosophy*, trans. G. Adey and D. Frisby, London, Routledge, 1980; and HABERMAS, J., "What is Universal Pragmatics?," in HABERMAS, J., *Communication and the Evolution of Society*, trans. T. McCarthy, Beacon, Boston, 1979, pp. 1-68, see note 24.

survive the criticisms of relevant communities. Reasonable people accumulate observations and inferences until the probability of their judgments is so great that they do not doubt them. As a result, they call their judgments "objective" when their evidence supports them. To secure objectivity, they do not rely solely on algorithms or on value-free empirical confirmation, as the naive positivist proponents of neutrality do. As argued earlier, because there is no value-free empirical evidence, it makes no sense to complain about well-supported judgments. Most scientists have never supported such an unrealistic notion of objectivity (as infallibility). Because they have not, reasonable people can secure objectivity by criteria such as external consisting surviving relevant criticism.[27]

But if social criteria help secure objectivity, then naive positivists are wrong to require value-free confirmability for the objectivity of empirical claims.[28] Cultural relativists also make too strict demands if they presuppose that objective judgments must be wholly value free and therefore infallible and universal. Many relativists believe that, because every judgment is value-laden, and because people often disagree about their judgments, therefore none is ever objective. Because of this disagreement, many relativists claim that no judgment ever is more objective than another, and that one is as good as another. Although such relativists are correct in recognizing the incompleteness of the positivists' model of objectivity, they go too far. They leap to the premature conclusion that, because people disagree about knowledge claims, therefore all value judgments are purely relative and none is superior to another.[29]

If those seeking to reform science and technology seek infallibility, certainty that transcends the possibility of error,[30] they falsely assume that, because

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[27] Cf. Scriven, M., "The Exact Role of Value Judgments in Science," in Klemke, E., Hollinger, R. and Kline, A. (eds.), *Introductory Readings in the Philosophy of Science*, Prometheus, Buffalo, 1982, pp. 269-297, and Nagel, T., *The View from Nowhere*, Oxford University Press, New York, 1986, pp. 143-153. For a related account of rationality and objectivity within the risk assessment context, see Rip, A., "Experts in Public Arenas," in Otway, H. and Peltu, M. (eds.), *Regulating Industrial Risks*, Butterworths, London, 1985, pp. 94-110. See also Lichtenberg, J., "Moral Certainty," *Philosophy*, v. 69, no. 268, (1994), pp. 181-204; and Hare, R. H., "Objective Prescriptions," in Griffiths, A. and Phillips, A. (eds.), *Ethics*, Cambridge University Press, New York, 1993, pp. 1-38.

[28] Cf. Popper, K. R., *The Open Society and Its Enemies*, Princeton University Press, Princeton, 1950, pp. 403-406; Popper, K. R., *Conjectures and Refutations*, Basic Books, New York, 1963 (3rd ed. revised, 1969), p. 63; and Popper, K. R., *The Logic of Scientific Discovery*, Harper, New York, 1965 (originally published in 1959 by Basic Books, New York), p. 56. See also Grattan-Guinness, I., "Truths and Contradictions about Karl Popper," *Annals of Science*, v. 59, no. 1, (2002), p. 89.

[29] For cultural relativists' claims, see Wildavsky, A. B. and Douglas, M., *Risk and Culture*, p. 188. See also Wildavsky, A. B., *Search for Safety*, Transaction Books, New Brunswick, 1988, p. 3. See Thompson, M., "To Hell with the Turkeys!," in MacLean, D. (ed.), *Values at Risk*, Rowman and Allanheld, Totowa (NJ), 1986, pp. 113-135. See Hankinson, R. J., "Values, Objectivity and Dialectic: the Skeptical Attack on Ethics," *Phronesis*, v. 39, no. 1, (1994), pp. 45-68; Wilburn, R., "Skepticism, Objectivity and the Aspirations of Immanence," *Dialectica*, v. 53, no. 4, (1998), pp. 291-318; and Vice, J. W., *The Reopening of the American Mind: On Skepticism and Constitutionalism*, Rodopi, Amsterdam, 1998. See also Van der Merwe, W. L., "Cultural Relativism and the Recognition of Cultural Differences," *South African Journal of Philosophy*, no. 3, (1998), p. 313; and Tilley, J. J., "Cultural Relativism," *Human Rights Quarterly*, v. 22, no. 2, (2000), p. 501.

[30] Cf. Feyerabend, P. K., "Changing Patterns of Reconstruction," *The British Journal for the Philosophy of Science*, v. 28, no. 4, (1977), p. 368.

there is no *perfect* judgment, therefore none is objective. Even if no judgment could escape falsification, it would not follow that all were equally unreliable.[31] Falsifications provide only necessary, not sufficient, conditions for claiming there is no objective knowledge. Both scientific inference and legal inference establish instead that something is *prima facie* true (reasonably probable, or because of a presumption in its favor) not that it is infallibly true. And if not, there is no reason that public-interest advocates need more than *prima facie* truth, in order to secure the objectivity of their judgments.[32] Disagreements over how to analyze knowledge claims mean neither that there are no informal rules of judgment nor that one rule is as good as another.

Those who reject public-interest advocacy, like attempts to reform science and technology, and who defend neutrality or complete ethical relativism appear to do so because they confuse three different questions. (1) Are there *general principles* (e.g., postulate environmental risk probabilities that are consistent with observed accident frequencies) that account for the objectivity of some knowledge claims? (2) Are there *particular procedures* (e.g., observe accident frequencies for a period of at least five years before concluding that they are consistent with postulated risk probabilities) that help guarantee the objectivity of judgments? (3) Does a specific knowledge claim, *in fact*, always illustrate either the general principles or the particular procedures? Complete ethical relativists often assume that, if one answers questions (2) and (3) in the negative, then the answer to (1) also is negative. This is false. Debate about question (2) does not jeopardize the objectivity of judgments, so long as people agree on (1). In fact, debate over question (2) must presuppose objectivity in the sense of question (1), or the discussion would be futile.[33] Therefore, if people can answer (1), then even if they cannot answer questions (2) and (3), it is possible to have objective knowledge.

Besides agreement on (1), another way to argue that the value-laden judgments of public-interest advocates can be objective is to incorporate insights from moral philosophy. As both ethicists from Aristotle to natural-law theorists, to contemporary analysts have recognized, ethics exhibits a hierarchy of methodological rules and value judgments. Different degrees of certainty are

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[31] See Kulka, T., "How Far Does Anything Go? Comments on Feyerabend's Epistemological Anarchism," *Philosophy of the Social Sciences*, v. 7, no. 3, (1977), pp. 279-280.

[32] See Hellman, G., "Against Bad Method," *Metaphilosophy*, v. 10, no. 2, (1979), p. 194; Quine, W. v. O. and Ullian, J., *Web of Belief*, Random House, New York, 2nd. ed., 1978; and Hempel, C. G., *Aspects of Scientific Explanation*, Free Press, New York, 1965, p. 463. See also Scriven, M., "The Exact Role of Value Judgments in Science," in Klemke, E., Hollinger, R. and Kline, A. (eds.), *Introductory Readings in the Philosophy of Science*, p. 277; and Weinberger, O., "Prima Facie Ought: A Logical and Methodological Enquiry," *Ratio-Juris*, v. 13, no. 3, (1999), pp. 239-251.

[33] See Siegel, H., "What is the Question Concerning the Rationality of Science?," *Philosophy of Science*, v. 52, no. 4, (1985), pp. 524-526; and Rudner, R., *Philosophy of Social Science*, Prentice Hall, Englewood Cliffs (NJ), 1966, pp. 4-5. See also Newton-Smith, W. H., "Popper, Science, and Rationality," *Philosophy*, vol. Suppl. 39, (1995), p. 13.

appropriate at different levels of generality, such that the most general rules are the most certain and the most universal (such as "postulate risk probabilities consistent with observed accident frequencies," or "do good and avoid evil"). The least general rules or value judgments are the least certain and the least universal (such as "person x errs in killing her attacker under circumstances y").[34] In order to apply the rules from the most universal, most general level, people must make a number of value judgments at lower levels. The fact that there is neither an algorithm nor empirical data (for these judgments) does not mean they are purely relative. Some are better than others. Some are better *means* to the *end* of explanatory power or predictive control.

The cultural relativists and positivists who oppose all public-interest advocacy miss both these points because they appear to presuppose that value judgments ought to be *infallible*, rather than *prima facie* true. Understanding objectivity in terms of *prima facie* truth requires (in part) understanding it in terms of some insights of Karl Popper, John Wisdom, and Ludwig Wittgenstein.[35] They anchor objectivity with actions, as well as with explanatory and predictive power. They do not define objectivity in terms of an impossible notion of justification. They secure objectivity, in part, by means of the criticisms made by the relevant knowledge communities. According to this scheme, a value judgment about the safety of some food additive, for example, is objective if it is able to survive and answer the criticisms of those informed about, and potentially affected by, the additive.[36] This social and critical account of knowing presupposes that objectivity, in its final stages, requires people to appeal to particular cases, just as legal justification requires. This account does not presuppose an appeal to *specific* rules of knowing, applicable to all situations. Nevertheless the *general* rules (such as surviving criticism) always are applicable. A *naturalistic* appeal to general rules, to cases, and to general values (such as consistency and predictive power), rather than to specific rules, is central to this social account of knowing. Instead of *specific* rules, applicable to all cases, the relevant community of knowers must evaluatively determine which judgments are objective.

As Mary Midgley recognized, what constitutes bias is not the acceptance of one's own scheme of values but the refusal to look at anyone else's.[37] Because knowing takes place within a varied community of knowers, having a multiplicity

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[34] See Hare, R. M., *Moral Thinking: Its Levels, Methods and Point*, Oxford University Press, Oxford, 1981.

[35] See Newell, R., *Objectivity, Empiricism, and Truth*, Routledge and K. Paul, New York, 1986, notes 82-84, 86 and 89; Stortland, F., "Wittgenstein: On Certainty and Truth," *Philosophical Investigations*, v. 21, no. 3, (1998), pp. 203-331; Hull, D. L., "The Use and Abuse of Karl Popper," *Biology and Philosophy*, v. 14, no. 4, (1999), pp. 481-504; and Zecha, G., *Critical Rationalism and Education Discourse*, Amsterdam, Rodopi, 1999.

[36] See Reale, M., "Axiological Invariants," *Journal of Value Inquiry*, v. 29, no. 1, (1995), pp. 65-75; and Kitcher, Ph., "The Division of Cognitive Labor," *The Journal of Philosophy*, v. 77, n. 1, (1990), pp. 5-22.

[37] Midgley, M., *Wisdom, Information, and Wonder*, p. 176.

of different values, public-interest advocacy should help emphasize the social and critical nature of knowledge.[38] It should help people recognize that an unbiased individual knower may be an inadequate focus for objective understanding.[39] Rather, knowledge and objectivity is achieved because members of the varied community of knowers interact and clarify issues. Each of the members' social contexts provides many categories and assumptions that enable people to interpret and correct understanding of phenomena.[40] Because any single observation is "always selective," the best way to be objective is to multiply standpoints, to "increase experience," to adopt a critical attitude, and to be ready to modify views on the basis of criticism and interaction.[41] But if so, people ought not to neglect the alternative standpoints of various members of the relevant knowledge community-including women, minorities, environmental stakeholders, or oppressed people. Otherwise knowers could fall victim to the dogmatism of a selective standpoint.

As John Stuart Mill recognized, the surest way of getting to the truth on any question is to examine all the important objections that can be brought against candidate opinions and alternative standpoints.[42] Such a multi-faceted and critical approach to knowing requires a community of knowers, each with somewhat different standpoints and advocacies. It requires a "free discussion" of views, giving assent only to those positions that survive critical evaluations from alternative standpoints. As Philip Kitcher put it, knowing requires a "division of cognitive labor" among knowers, a community whose existence suggests the inadequacy of privileging any particular observer as alone "objective."[43]

Part of what is wrong with those who reject attempts to reform science and technology is their failure to tie objectivity to evenhandedness and lack of bias. A standpoint can be classified as "objective" only when it meets at least two criteria.

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[38] Some of this discussion of the social nature of knowing is based on SHRADER-FRECHETTE, K., "Feminist Epistemology and its Consequences for Policy," *Public Affairs Quarterly*, v. 9, no. 2, (1995), pp. 155-174.

[39] Cf. LONGINO, H., "Multiplying Subjects and the Diffusion of Power," *The Journal of Philosophy*, v. 88, no. 11, (1991), pp. 666-674; and LONGINO, H., *Science as Social Knowledge*, pp. 109-121. See also TUANA, N., "The Radical Future of Feminist Empiricism," *Hypatia*, v. 7, no. 1 (1992), pp. 100-114.

[40] Cf. KUHN, TH. S., *The Structure of Scientific Revolutions*, The University of Chicago Press, Chicago, 2nd ed., 1970, pp. 91-204. See POLANYI, M., *Personal Knowledge*, Harper and Row, New York, 1964; and HANSON, N. R., *Patterns of Discovery*, Cambridge University Press, Cambridge, 1958.

[41] Cf. POPPER, K. R., "Science: Conjectures and Refutations," in FETZER, J. H. (ed.), *Foundations of Philosophy of Science*, Paragon House, New York, 1993, pp. 341-363, especially, pp. 350-352. See POPPER, K., *The Logic of Scientific Discovery*, p. 106; and POPPER, K. R., *Conjectures and Refutations*, especially ch. 11, pp. 253-292. See also HACOHEN, M. H., *Karl Popper*, Cambridge University Press, Cambridge (MA), 2000.

[42] Cf. MILL, J. S., *On Liberty*, Prometheus, Buffalo (NY), 1986, pp. 60-61.

[43] Cf. KITCHER, PH., "The Division of Cognitive Labor," *The Journal of Philosophy*, v. 77, no. 1, (1990), pp. 5-22. See POPPER, K. R., "Science: Conjectures and Refutations," in FETZER, J. H. (ed.), *Foundations of Philosophy in Science*, p. 354; and POPPER, K. R., *The Logic of Scientific Discovery*, p. 106.

(1) It survives criticism and testing by members of the relevant communities, and (2) it is consistent with democratic and procedural constraints such as fairness and evenhandedness.[44] Such a notion of objectivity and defensible objectivity in reforming science and technology must be procedural, open, and populist. On this account, *what* a pluralistic community of public-interest advocates and critics ought to *believe* is bootstrapped onto *how* they ought to *act*. People ought to act in ways that evenhandedly evaluate and predictively test all relevant perspectives, including those of women, children, minorities, environmentalists, industrialists, and so on. Such unbiased actions are necessary for objective knowing, and objective knowing (in the sense defined here) helps provide a reliable foundation for public-interest advocacy and criticism.

If this account of the social nature of knowing is correct, it provides important ethical reasons for public-interest advocates to consider the beliefs of all relevant members of the moral community. Because of its inclusiveness, this social account requires people to use the marketplace of ideas to analyze, defend, and criticize alternative positions. This is one of the surest ways to know as objectively as possible.[45] Professionals interested in reforming science and technology need to secure objectivity in part procedurally, by means of the interactions and criticisms of the relevant community of knowers and by tests for fairness and lack of bias. Those who ignore relevant criticisms are guilty of bias because objective knowing requires consideration of a variety of relevant standpoints and practices.

Failure to define "objectivity" accurately also may keep people from accepting their ethical responsibilities, including those to reform science and technology. Those who fail to behave as "public citizens" may fail, in part, because they miss the basic insight of Israel Scheffler: "objectivity requires simply the possibility of intelligible debate over the merits of rival paradigms."[46] If this is all objectivity requires, then it is time for citizens, scholars, and other professionals to enter public debates.[47]

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[44] See Adler, J., "Reasonableness, Bias, and the Untapped Power of Procedure," *Synthese*, v, 94, no. 1, (1993), pp. 105-125; and Williams, B., *Ethics and the Limits of Philosophy*, Harvard University Press, Cambridge, 1985, pp. 199-200. See Andrews, R. N., "Environmental Impact Assessment and Risk Assessment," in Wathern, P. (ed.), *Environmental Impact Assessment*, Unwin Hyman, London, 1988, pp. 85-97; and Cox, L. and Ricci, P., "Legal and Philosophical Aspects of Risk Analysis," in Paustenbach, D. J. (ed.), *The Risk Assessment of Environmental and Human Health Hazards*, J. Wiley, New York, 1989, pp. 1017-1046, for suggestions in this regard.

[45] See, for example, Popper, K. R., *The Open Society and Its Enemies*, pp. 403-406; Popper, K. R., *Conjectures and Refutations*, p. 63; and Maso, I. (ed.), *Openness in Research*, Van Gorcum, Assen, 1995.

[46] Scheffler, I., "Vision and Revolution: A Postscript on Kuhn," *Philosophy of Science*, v. 39, no. 3, (1972), p. 369.

[47] Some of this discussion of objectivity, speaking out, and ethical tests for objectivity relies on Shrader-Frechette, K., *Ethics of Scientific Research*, Rowman and Littlefield, Savage (MD), 1994, pp. 55-61. See Moore, A. W., "One or Two Dogmas of Objectivity," *Mind*, v. 108, no. 430, (1999), pp. 381-393. See also Alcoff, L. M., "Objectivity and Its Politics," *New Literary History*, v. 32, no. 4, (2001), p. 835.

5. OBJECTIONS

In response to this account of duties to reform science and technology, critics could object: (1) Reformers could err and thus contribute to faulty policy. (2) Without neutrality, mere politics could control science and policy.

As objection (1) suggests, not all reform attempts are ethically and practically defensible.[48] If scientists err when they speak out against some technological hazard, they could jeopardize both scientific credibility and sound policy.[49] Despite Nader's outstanding accomplishments, a congressional-committee chair claimed that he was "a bully and know-it-all, consumed by certainty and frequently in error."[50] Daniel Simberloff, a distinguished biologist, refers to this objection (1) when he worries that if scientists err in their advocacy, future policymakers might not listen to them.[51] Contrary to Simberloff, however, sometimes professionals ought to take the risk of attempting reform, in part because their making mistakes rarely leads to loss of credibility. When researchers disproved the scientific foundations of the Endangered Species Act, the diversity-stability thesis,[52] lawmakers did not repeal it. And when Dutch researchers showed that the US Rasmussen Report, WASH-1400,[53] was wrong, that *all* the accident failure –frequency values from operating experience fell *outside* the study's 90– percent confidence bands,[54] nations did not close their nuclear plants.

When Cal Tech founder and Nobel winner, Robert Millikan,[55] called belief in nuclear power a "myth," less than a decade before scientists confirmed the existence of fission energy, Millikan did not lose credibility. If the work of Kahneman, Tversky, and others is correct, experts chronically err, even in their own fields of expertise, when they reason probabilistically. In employing

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[48] See SHRADER-FRECHETTE, K., *Ethics of Scientific Research*, pp. 130-133.

[49] Cf. SIMBERLOFF, D., "Simplification, Danger, and Ethics in Conservation Biology," *Ecological Society of America Bulletin*, v. 68, (1987), pp. 156-157. See also VESILIND, P. A. and BARTLETT, L., "The Ethics and Science of Environmental Regulation," *Journal of Environmental Engineering*, v. 124, no. 8, (1998), p. 675.

[50] THOMAS, R., "Safe at This Speed?," p. 40.

[51] Cf. SIMBERLOFF, "Simplification, Danger, and Ethics in Conservation Biology," p. 157.

[52] See SHRADER-FRECHETTE, K. and MCCOY, E. D., *Method in Ecology*, Cambridge University Press, Cambridge, 1993, ch. 2. See, for example, US CONGRESS, *Congressional Record Senate*, 93rd Congress, First Session 119, US Government Printing Office, Washington, DC, 1973, p. 25668; COMMONER, B., *The Closing Circle*, Knopf, New York, 1971, p. 38; and MYERS, N., *A Wealth of Wild Species*, Westview Press, Boulder (CO), 1983.

See also REICHHARDT, T., "Academy Backs Science in Endangered Species Act," *Nature,* 375, no. 6530, (1995), p. 349; NOSS, R.F., *The Science of Conservation Planning, Habitat Conservation under the Endangered Species Act*, Island Press, Washington, 1997; NATIONAL RESEARCH COUNCIL, *Science & the Endangered Species Act*, National Academy Press, Washington, 1995; and REICHHARDT, T., "Inadequate Science in US Habitat Plans," *Nature*, v. 397, no. 6717, (1999), p. 287.

[53] US NUCLEAR REGULATORY COMMISSION, *Reactor Safety Study*, NUREG 75/014, WASH-1400, US Government Printing Office, Washington, DC, 1975.

[54] Cf. COOKE, R. M., "Problems with Empirical Bayes," *Risk Analysis*, v. 6, no. 3, (1986), pp. 269-272; see SHRADER-FRECHETTE, K., *Risk and Rationality*, pp. 109-111, 140-144 and 188-196.

[55] Cf. MILLIKAN, R. A., "Alleged Sins of Science," *Scribner's Magazine*, v. 87, no. 2, (1930), pp. 119-130.

necessary judgmental strategies (like the methodological value judgments discussed earlier) to make their problems easier to solve, experts fall victim to the same errors (such as the representativeness bias, as laypeople).[56] Scientists were wrong when they said that irradiating enlarged tonsils was harmless. They were wrong when they said that x-raying feet, to determine shoe size, was safe. They were wrong when they said that the Titanic would not sink. They were wrong when they said that irradiating women's breasts, to alleviate mastitis, was harmless. When government assessments of the risk of a serious accident at Three Mile Island (TMI) differed by two orders of magnitude,[57] the US Nuclear Regulatory Commission did not close TMI. It took an accident, not loss of scientific credibility, to do that. When 500,000 US GI's received harmful amounts of radiation during nuclear weapons tests in the Pacific and the United States in the 1940s, 1950s, and 1960s, government scientists called the exposures "safe." They did not lose credibility.[58] Even though many servicemen died of testing-induced leukemia, the US Atomic Energy Commission and the US Nuclear Regulatory Commission continued to seek the advice of the very researchers who had misled them in weapons-testing. Expert errors thus are nothing new. Despite Ralph Nader's having been accused of being wrong several times, the purity of his intentions is indisputable, and that is less than most can say of many scientific errors today. Nader's failure to gain personally from his activities is well known and helps make him credible.[59] Besides, if people refrained from taking partisan positions out of fear of error, they would never take important stands, even when it was

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[56] Cf. Kahneman, D. and Tversky, A., "Availability: A Heuristic for Judging Frequency and Probability," in Kahneman, D. H. et al. (eds.), *Judgment Under Uncertainty: Heuristics and Biases*, Cambridge University Press, Cambridge, UK, 1982, pp. 63-78; Kahneman, D. and Tversky, A., "Judgment Under Uncertainty," in Kahneman, D. H. et al. (eds.), *Judgment Under Uncertainty*, pp. 4-11.

[57] Cf. Rasmussen, N. C., "Methods of Hazard Analysis and Nuclear Safety Engineering," in Moss, T. and Sill, D. (eds.), *The Three Mile Island Nuclear Accident*, New York Academy of Science, New York, 1981, pp. 56-57.

[58] Cf. US Congress, *Government Liability for Atomic Weapons Testing Program*, Hearings before the Committee on the Judiciary, US Senate June 27, 1986, US Government Printing Office, Washington, DC, 1987. See also US Congress, *Cold War Human Subject Experimentation*, Hearing before the Legislation and National Subcommittee of the Committee on Government Operations, House of Representatives, One Hundred Third Congress, second session, 28 September, 1994, Government Printing Office, Washington, DC, 1994; US Department of Energy, *Human Radiation Experiments: The Department of Energy Roadmap to the Story and the Records*, National Technical Information Service, DOE/EH-0445, Springfield (VA), 1995; US Congress*, Human Subjects Research: Radiation Experimentation*, Hearings before the Committee on Labor and Human Resources, United States Senate, One Hundred and Third Congress, first session, 13 January, 1994, US Government Printing Office, Washington, DC, 1994; US Congress, *American Nuclear Guinea Pigs: Three Decades of Radiation Experiments on US Citizens*, A report prepared by the Subcommittee on Energy Conservation and Power of the Committee on Energy and Commerce, US House of Representatives, US Government Printing Office, Washington, DC, 1986.

[59] See, for example, McCarrey, C., *Citizen Nader*, pp. 12, 13, 139, 212 and 213; Gorey, H., *Nader and the Power of Everyman*, p. 23; Rowe, J., "Ralph Nader Reconsidered," in Scarpitti, F. and Cylke Jr., F. (eds.), *Social Problems: The Search for Solutions: An Anthology*, Roxbury Publishing Company, 1st ed., 1995, p. 65.

necessary to do so.[60] Moreover, the antidote for extremist advocacy or criticism is not to prohibit attempted reform but use democracy to counter extremism. The antidote is to create an enlightened societal framework, a marketplace of ideas, in which people can evaluate alternative public policies.[61] As already argued, humans do not make societal decisions in some neutral, individual environment, but by interaction among a plurality of partisans.[62] But if so, then avoiding reform, under the guise of neutrality, does not guarantee that others will be heard. It merely keeps everyone quiet, except those with enough money to be devious and manipulative. It limits the interactions necessary to educate citizens and to create democratic policy.

What if someone fears public-interest advocacy for reason (2), that it could cause politics or ideology to dominate public policy? As earlier sections argued, this objection is flawed because value-free inquiry is impossible, and because the best way to defeat questionable values is for citizens to recognize and criticize them.[63] Otherwise, people who avoid public-interest advocacy or criticism could use their neutrality as an excuse to remain silent while innocent people faced great risks or while powerful interests spread scientific disinformation. Camus made the same point: "We are guilty of treason in the eyes of history if we do not denounce what deserves to be denounced. The conspiracy of silence is our condemnation in the eyes of those who come after us."[64]

Remaining neutral and avoiding informed, public-interest attempts at reform of science and technology also might serve the interests of relativists and skeptics rather than social reformers, as happened during World War II. When citizens asked Columbia University anthropologists for their position on the actions of the Nazis, the scholars said they had "to take a professional stand of cultural relativity," to be "skeptics" with respect to all judgments of value.[65] But if citizens

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[60] See, for example, RESCHER, N., "Collective Responsibility," *Journal of Social Philosophy*, v. 29, no. 3, (1998), pp. 46-58.

[61] See POPPER, K. R., *Conjectures and Refutations*, passim; KITCHER, PH., "The Division of Cognitive Labor," pp. 5-22; MAYO, D. and HOLLANDER, R. (eds.), *Acceptable Evidence*, Oxford University Press, New York, 1991; and SHRADER-FRECHETTE, K., *Science Policy, Ethics, and Economic Methodology*, ch. 9, pp. 286-315.

[62] See ANDREWS, R. N., "Environmental Impact Assessment and Risk Assessment," in WATHERN, P. (ed.), *Environmental Impact Assessment*, pp. 85-97. For another discussion of these same points, see QUADE, E. S., *Analysis for Publication Decisions*, American Elsevier, New York, 1975, pp. 269ff.; WEIS, C. H., and BUCUVALAS, M. J., *Social Science Research and Decision Making,* Columbia, University Press, New York, 1980, p. 26; and LINDBLOM, C. E., and COHEN, D. K., *Usable Knowledge: Social Science and Social Problem Solving*, Yale University Press, New Haven, 1979, p. 64. See SEN, A., *Objectivity and Position*, University Press of Kansas, Lawrence, 1992. See previous four notes.

[63] Cf. PARSONS, C., "Introduction," in HELD, V., NIELSEN, K. and PARSONS, C. (eds.), *Philosophy and Political Action*, pp. 3-12.

[64] CAMUS, A., *Notebooks*, trans. J. O'Brien, Knopf, New York, 1974. See SAMUELS, S., "The Arrogance of Intellectual Power," in WOODHEAD, A., BENDER, M. and LEONARD, R. (eds.), *Phenotypic Variation in Populations*, Plenum, New York, 1988, pp. 113-120.

[65] Cf. SHRADER-FRECHETTE, K., *Science Policy, Ethics, and Economic Methodology*, p. 88. See DECEW, J. W., "Moral Conflicts and Ethical Relativism," *Ethics*, v. 101, no. 1, (1990), pp. 27-41.

fail to "get involved" by attempting to reform science and technology, there is less chance of avoiding injustice and resolving public controversies. As one scholar put it, "contemporary Pyrrhonism cannot sustain serious moral conflict."[66] In the face of serious evil, if people adopt positions of neutrality, then they are not neutral. Instead they contribute to evil by helping it to continue, while they claim to be neutral. In summary, at least 6 reasons show that objectivity is not neutrality and therefore that informed, balanced attempts to reform science and technology can be objective:

(1) Once people admit that value judgments are part of all knowing, then not to assess those value judgments is to become hostage to them.

(2) If not all positions are equally justifiable, then objectivity requires people to represent less justifiable positions as less justifiable.

(3) In the face of a great threat, people who represent objectivity as neutrality serve the interests of those responsible for the threat.

(4) People who represent objectivity as neutrality encourage lack of attention to evaluative assumptions and thus lack of public control over those assumptions.

(5) People who represent objectivity as neutrality presuppose that it is somehow delivered from "on high," rather than discovered socially, through the give-and-take of alternative points of view

(6) People who represent objectivity as neutrality sanction either ethical relativism or skepticism and thus encourages injustice.

Some members of at least three groups in contemporary society (post-modernists, positivists, and relativist social scientists) likely would support the Columbia University anthropologists who remained neutral toward Hitler. They would be undercut by the arguments I have made because members of all three groups confuse objectivity with silence or neutrality. They confuse tolerance with ethical relativism.

6. Constraints on Objectivity as Unbiased Advocacy for Reform

But I have argued merely *that* genuine objectivity may require duties to reform science and technology, not *when* objectivity may require it. When are such duties more defensible? At least three suggestions come to mind. The first is that citizens typically have greater responsibilities to curb bias to the degree that it is serious and they are able to do so. But if so, citizens should demand a higher standard of certainty in situations where policies are likely to be applied in ways

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[66] Tamas, G. M., "The Political Irresponsibility of Intellectuals," in Maclean, I., Montefiore, A. and Winch, P. (eds.), *The Political Responsibility of Intellectuals*, Cambridge University Press, Cambridge, 1990, pp. 247-256; especially, p. 256. See also Mosser, K., "Should the Skeptical Live His Skepticism?," *Manuscrito*, v. 21, no. 1, (1998), pp. 47-84.

that could threaten the common good. In many fields having consequences for the common good –for example, chemical research– profits often interfere with objective fact-finding. Because members of democratic or biotic communities are less able (than individual business clients) to give free informed consent to risky actions affecting them, citizens need to help assure these members of greater protection. One way to promote such protection would be for professionals to help improve standards of peer review for research that affects the public welfare.[67] They also could work to respond, especially in the popular press, to science that is biased and manipulated by vested interests and to eliminate the biases that often accompany professional work.

Asserting citizens' (and especially professionals') responsibilities –to promote unbiased information affecting the common good– is analogous to affirming similar duties regarding dangerous technologies. Just as there is a justifiable double standard (based on the gravity of the public threat) for speaking out against biased information, there also is a justifiable double standard (based on the severity of the public-health risk) for criticizing dangerous technologies. In both cases, human responsibility for counteracting the threat is proportional to its seriousness. This proportionality explains the reason that professionals typically ought to have a higher standard for assessing more hazardous technologies, like nuclear power. Because situations of greater threats require greater scrutiny, riskier technologies ought to have greater counterbalancing benefits.[68]

In other words, objectivity often is a matter of ethics as well as epistemology. *Epistemic* objectivity addresses *beliefs*. It requires citizens and professionals to assess *hypotheses* and their practical *consequences* in ways that avoid deliberate bias or misinterpretation. *Ethical objectivity* addresses *actions*. It requires more than merely avoiding deliberate bias or misinterpretation. Instead it demands that citizens take into account obligations to the common good when assessing their actions, omissions, and beliefs. For example, reformers might follow epistemic objectivity and assess whether an hypothesis (such as "this biotechnological experiment will not endanger ecosystems") is both probable and likely to lead to no undesirable consequences. Following a principle of ethical objectivity, they might evaluate whether epistemic objectivity alone provides an adequate test of the hypothesis or whether one also ought to consider factors like the public's rights to protection and its rights to know about potentially harmful acts. In cases that involve duties to stakeholders, objectivity requires not merely unbiased *epistemic* assessment of one's *beliefs*, but also unbiased *ethical* evaluation of the *actions*

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[67] See, for example, LLOYD, J., "On Watersheds and Peers, Publication, Pimps and Panache," *Florida Entomologist*, v. 68, (1985), pp. 134-139; and HOLLANDER, R., "Journals Have Obligations, Too," *Science, Technology, and Human Values*, v. 15, no. 1, (1990), pp. 46-49.

[68] See, for example, STARR, C., RUDMAN, R., and WHIPPLE, C., "Philosophical Basis for Risk Analysis," *Annual Review of Energy*, v. 1, (1976), p. 638. See also BAYLES, M., *Professional Ethics*, Wadsworth, Belmont, 1981, p. 116; TVERSKY, A. and FOX, C. R., "Weighing Risk and Uncertainty," *Psychological Review*, v. 102, no. 2, (1995), p. 269; and VAN RAAIJ, W. F., "The Life and Work of Amos Tversky," *Journal of Economic Psychology*, v. 19, no. 4, (1998), p. 515.

premised on those beliefs. Thus a second constraint, on unbiased advocacy for reform, is that one recognize both ethical as well as epistemic objectivity.

But how can people –especially scientific reformers– be *ethically* responsible for their beliefs? W. K. Clifford argued for an ethical obligation to seek the truth.[69] This presupposes that belief can be under at least some voluntary control; that people can, in part, *decide* to recognize bias or misinterpretation; that, in part, they can choose to be more or less objective. I shall not take time here to argue for an ethics of belief, but the main point is that, if facts are value laden, then accepting particular interpretations of scientific data is in part a matter of choice, and therefore, in part, a matter of voluntary control. And if so, science is therefore subject in part to ethical appraisal, an "ethics of belief",[70] in the sense that people have an ethical obligation continually to appraise and reform the practice and use of science and technology. A model for this ethics might be Oliver Cromwell's famous plea to the assembly of the Church of Scotland: "I beseech you... think it possible you may be mistaken." The goal is not to impose conclusions on the unwilling, but to promote informed, open, critical, public debate of one's own positions, as well as those of others. Neither truth nor sound science will survive long in a situation of neutrality or keeping silence so as to avoid error.

There are no necessary and sufficient conditions for morally justifying any complicated, situation-specific acts, such as attempting to reform science and technology or engaging in civil disobedience.[71] There are, however, a third sort of useful guidelines. In his famous letter from Birmingham jail, Martin Luther King implicitly provides at least three such guidelines: (1) collecting the facts to determine whether injustices actually exist; (2) negotiating to try to correct the injustices; and (3) purifying oneself. The point of purification is to avoid egoism and fanaticism, what T. S. Eliot called "the last temptation": "to do the right thing for the wrong reason."[72] A fourth consideration that helps clarify duties to reform science and technology, was formulated by John Locke. He warned that, in order

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[69] Cf. CLIFFORD, W. K., *Lectures and Essays*, Macmillan, London, 1886. See also TOULMIN, S., "Can Science and Ethics Be Reconnected?," *Hastings Center Report*, v. 9, (1979), 27-34; HAACK, S., "The Ethics of Belief Reconsidered," in HAHN, L. E. (ed.), *The Philosophy of Roderick M. Chisholm*, Open Court, La Salle (IL) 1997, pp. 129-144; VORSTENBOSCH, J., "W. K. Clifford's Belief Revisited," in MEIJERS, A. (ed.), *Belief, Cognition, and the Will*, University of Tilburg, Tilburg, 1999, pp. 99-111; FELDMAN, J., "The Ethics of Belief," *Philosophy & Phenomenological Research*, v. 60, no. 3, (2000), p. 667; and PRYOR, J., "Highlights of Recent Epistemology," *The British Journal for the Philosophy of Science*, v. 52, n. 1, (2001), pp. 95-124.

[70] See, for example, JAMES, W., *The Will to Believe and Other Essays in Popular Philosophy*, Dover, New York, 1956, pp. 17-30. See also OWENS, D., "John Locke and the Ethics of Belief," *Locke Newsletter*, v. 30, (1999), pp. 103-127; ADLER, J. E., "Ethics of Belief: Off the Wrong Track," *Midwest Studies in Philosophy*, v. 23, (1999), pp. 267-285; and MADIGAN, T. J., "The Virtues of Ethics of Belief," *Free Inquiry*, v. 17, no. 2, (1997), pp. 29-33.

[71] Cf. KING, M. L., "Letter from Birmingham Jail," in HARRIS, P. (ed.), *Civil Disobedience*, University Press of America, Lanham (MD), 1989, pp. 58 and 70; RAWLS, J., *A Theory of Justice*, Harvard University Press, Cambridge (MA), 1971, pp. 363-377.

[72] Quoted in FORTAS, A., "Concerning Dissent and Civil Disobedience," in HARRIS, P. (ed.), *Civil Disobedience*, p. 91 (see also pp. 91-105) and in KENNY, A., *Thomas Moore*, Oxford University Press, New York, 1983, p. 1.

to serve the ends for which the state exists, people sometimes must act against the alleged civil law *before* the most serious violations of their rights occur; otherwise, the violations might become impossible to remove.[73] The same could be said of violations of scientific objectivity. Other guidelines for reform, fifth, have been offered in the many discussions of conditions for justified whistleblowing. The important point, however, is not to give necessary and sufficient conditions, at this stage, but to argue instead that most philosophers and most scientists likely have some duties to secure objectivity, to reform science and technology. Most have not accepted this fundamental duty.

Not to attempt reform would amount to a self-fulfilling prophecy, a counsel for despair. More than a decade ago, Alasdair MacIntyre diagnosed grave "disorders of moral thought and practice" in society.[74] He made a misdiagnosis. Moral dissensus, as such, is not a problem. Dissensus may exist because a situation is unclear, because people disagree over how to interpret data, or because ethics consists of abstract principles that need to be interpreted and amended through democratic processes in concrete cases. Yet dissensus at the concrete level often is evidence of some consensus at the abstract or general level. Social knowing, requiring give-and-take, working through disagreement and criticism, is a necessary condition for objectivity, not a sign of its failure, as MacIntyre thought. Without this give and take, there is only apparent consensus, not objectivity, because people probably have not rationally agreed on a position. Apparent consensus probably signals that people are lazy, or live in fear, or are forced to agree, or lack the ability or intelligence to debate ethical issues. Dissensus then is not only a necessary condition for objectivity but can be evidence of an open, rather than a repressive, society; a result of increased moral autonomy; or a consequence of the freedom to develop a life that allows for alternative thoughts and actions. Dissensus is far superior to unthinking or coerced consensus, repression, or passivity that fears disagreements. Dissensus, or the conflict necessary to achieve reform of science and technology, is psychologically and politically discomforting. It means people must work out their differences, compromise in order to achieve a noble goal. Besides, from an ethical point of view, consensus is irrelevant. Actions are not right or wrong because people agree they are right or wrong, but because there is a rational justification for their rightness or wrongness.

7. CONCLUSION

Because he takes seriously the necessity of reform, even civil disobedience, black activist Martin Luther King goes so far as to say that the "white moderate," the proponent of neutrality who fears dissensus, is a greater threat to freedom, ethics, and justice than the Ku Klux Klanner who is racist and who lynches blacks.

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[73] Cf. LOCKE, J., *Second Treatise on Civil Government*, Prometheus Books, Buffalo (NY), 1986, Sections 159, 160, 220, 240 and 242.

[74] Cf. MACINTYRE, A., *After Virtue*, Duckworth, London, 1981, p. 6.

"Moderates" are dangerous precisely because people think they are balanced, objective, and therefore ethical. King worries about moderates because they are more devoted to *order* than to *justice*. He says they forget law and order are means to the end of justice, and not the reverse. Martin Luther King questioned whether religion was too bound up with the *status quo* to save the world.[75] Perhaps scientists and philosophers are too bound up with the *status quo* to help reform science and technology?

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[75] Cf. KING, M. L., "Letter from Birmingham Jail," in HARRIS, P. (ed.), *Civil Disobedience*, p. 69.

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II

STS: From the Present Situation to the Future Projection

1. Metascientific Analysis and Methodological Learning in Regulatory Science
2. How to Reform Science and Technology

METASCIENTIFIC ANALYSIS AND METHODOLOGICAL LEARNING IN REGULATORY SCIENCE

ON THE RELATIONSHIP BETWEEN ANALYSIS OF SCIENCE AND SCIENTIFIC PRACTICE[*]

Jose Luis Luján

In this work I deal with the relationship between epistemic and non-epistemic values and the methodological learning in a particular kind of scientific activity, namely regulatory science.[1] I will begin by discussing the main studies on the relationship between science and values, analyzing different authors' works on the change of epistemic values. It is in this analytical context that the topics of axiological and methodological learning in science appear.

Later, I will analyze some of the main controversies that have taken place in the last two decades in a particular type of regulatory science, that is risk assessment. This analysis shows that it is impossible to understand the transformations that have taken place in this activity (i.e., methodological change) without keeping in mind the relationship between epistemic and non-epistemic values in risk assessment. The conclusion is that only an approach that considers these non-epistemic values can offer a complete understanding of methodological change in regulatory science. The denial of the influence of these values first of all makes it difficult to analyze this kind of scientific research, but also cuts off methodological and social learning which could arise from such as understanding.

1. SCIENCE AND VALUES

The relationship between science and values has been the object of numerous works in recent times. Although there are many approaches to the study of this topic, two of these have centered most of the analyses:

a) The relationship between epistemic values and scientific change.
b) The relationship between epistemic (or cognitive) and non-epistemic values in scientific activity, especially in the applied sciences.

The first of these approaches appeared in philosophy of science with the antipositivist revolt. Most of the work done during the 1970's and 80's centered on the analysis of epistemic values, their role in scientific change and the change of the epistemic values itself (e.g., Kuhn, Shapere, Laudan and McMullin, among others). This line of research coincided in time with the development of the sociology of scientific knowledge, an approach that considers interests (of different type) more important in scientific change than epistemic values.

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[*] The author is grateful to the Spanish Ministry of Science and Technology for supporting this work (project: BFF2001-0377).

[1] Regulatory science is a particular kind of applied science: the policy-relevant science. See JASANOFF, S., *The Fifth Branch. Science Advisers as Policymakers*, Harvard University Press, Cambridge, 1990.

Recently, some authors have also considered the influence of non-epistemic values, characterized as contextual values.[2]

The second approach appeared more or less at the same time than the first one, but its articulation in philosophy of science occurred later. Here I will only analyze this problem in regulatory science. Authors like Shrader-Frechette, Mayo, Cranor and Douglas have pointed out that the methodological decisions that appeal to epistemic values can have important social and environmental consequences that affect people's lifes. They also emphasize the importance of ethical or practical values in this particular type of scientific practice.

2. Scientific Change and Epistemic Values

Ernan McMullin has pointed out that it is possible to conceptualize many of the deep changes which the theory of science has undergone as a consequence of the growing understanding of the role of value judgments in science.[3] This, for McMullin, constitutes the "Kuhnian revolution." The relationship between values and scientific change has been studied in two ways. The most immediate of these is related to the role of values in scientific change. The other is to analyze the change of the epistemic values itself. Here I will briefly discuss the main contributions of Shapere, McMullin and Laudan to this subject.

Shapere criticizes the image of scientific change as the alteration of substantial beliefs caused by new discoveries about the world. However, scientific change and innovation also occur in the methods, the rules of reasoning as well as in the concepts used in science and in the analysis of science. Even the criteria of what counts as a 'scientific theory' or as an 'explanation' change over time. Shapere rejects the idea according to which there are "high-level" criteria of rationality that are unalterable and that serve to judge the rationality of "low-level" changes (like changes of concepts, theories, etc.). On the other hand, Shapere also rejects the relativist answer which denies the possibility of asserting the rationality of scientific change or the existence of scientific progress.

Without presupposing universal and suprahistorical criteria of rationality, Shapere seeks to show the rationality of scientific change. The criteria of evaluation can change, and in fact they do. Some of these changes of criteria of evaluation are justified. There is frequently a chain of developments that connects the various groups of evaluation criteria, a chain that allows tracing a 'rational evolution' between them. If it is shown that the transition from EC_1 in t_1 to EC_2 in t_2 constitutes a rational evolution of the criteria, then, *ceteris paribus*, it is rational to apply EC_2, instead of EC_1, in t_2.[4]

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[2] See Longino, H. E., *Science as Social Knowledge: Values and Objectivity in Scientific Inquiry*, Princeton University Press, Princeton, 1990.

[3] Cf. McMullin, E., "Values in Science," in Klemke, E. D., Hollinger, R. and Rudge, D. W. (eds), *Introductory Readings in the Philosophy of Science*, Prometheus Books, Amherst, 1998, p. 515.

[4] Cf. Shapere, D., *Reason and the Search for Knowledge*, Reidel, Dordrecht, 1984, p. 212.

For Shapere, the changes in the aims of science or in the criteria of rationality themselves are intimately connected to changes in our substantial beliefs about the world. Aims and criteria are proposed and modified, as happens with low-level theories. We not only learn, but we learn to learn. The aims of science change over time, and the reasons for this change are determined by the content of science at a given time, by its rules, its methods, the substantial beliefs and the interaction among all these components, in such a way that what is considered a legitimate successor also changes. The ontological and methodological commitments that justify scientific change also change. And this change is justified by the previous commitments and the scientific results which these have brought about. Therefore, Shapere doesn't have to introduce changes of the high-level units (which he calls scientific domains) nor scientific revolutions. A gradual change, product of the interactions among ontological and methodological commitments, reasoning principles, scientific results, etc., is sufficient to explain the change of particular scientific theories as well as changes in the criteria of evaluation themselves.

In his book *Science and Values*, Laudan arrives at conclusions similar to those of Shapere. According to Laudan, most of the philosophical approaches concerning scientific change take for granted a common model of justification. This model has three levels: (i) laws and theories; (ii) methodological rules; and (iii) statements concerning aims, as well as basic cognitive or epistemic values. The controversies on an inferior level are solved applying the principles of the immediately superior level. Laudan states that this model doesn't agree with what one can observe from the history of science: there are cases in which, for example, aims or epistemic values are modified appealing to scientific methodology or scientific theories. Therefore, according to him, we should discard the hierarchical order implicit in the hierarchical model in favor of an egalitarian principle that stresses the patterns of mutual interdependence among the different levels.[5]

In contrast to the hierarchical model, Laudan suggests a *reticulated model of justification*: the cognitive aims justify the methodological principles, and these show the feasibility of those; the methodological principles justify the theories, but these limit those; and, lastly, there should exist a harmony between scientific theories and cognitive aims. Not only is there a change of scientific theories, but also methodological and axiological change. The two most important characteristics of the reticulated model are (a) the non-linear conception of justification and (b) the gradual character of scientific change (theoretical, methodological and axiological).

Laudan criticizes both the hierarchical point of view defended by authors like Hempel, Reichenbach or Newton-Smith, and the holistic one defended by Kuhn. Laudan proposes his reticulated model in order to show in which way axiological

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[5] Cf. LAUDAN, L., *Science and Values. The Aims of Science and their Role in Scientific Debate*, University of California Press, Berkeley, 1984, p. 63.

debate and the formation of consensus in relation to the aims of science are possible. The scientific controversies on one level can be solved by appealing to the consensus reached on a different level. The different levels of controversy and consensus constitute a framework of justification whose components maintain relationships of interdependence. This interdependence explains the gradual character of certain episodes of scientific change, which without further analysis could give the impression of being holistic.

Another philosopher that also considers the problem of changes in scientific rationality is McMullin. He defines scientific rationality as the relationship between methods and aims or values. Therefore, the question about changes in the patterns of scientific rationality is related to the question about changes in epistemic values. For McMullin this is a debate started by the work of Kuhn, and continued in the works of Shapere and Laudan.

McMullin exposes the possibility of changes in the cognitive values of science.[6] He illustrates his point of view with several examples. In one of these he compares the astronomy of pre-Hellenic Greece with that of Babylon. Because of its socio-cultural context, Babylonian astronomy was geared towards prediction, a goal in fact related to omens. Babylonian astronomy has an empirical character, including a rich observational base, accuracy in the obtained data and a great predictive capacity. It lacks, however, interest as to the causes of the trajectories of celestial bodies. The cultural context of the Greek cities gave rise, according to McMullin, to a different type of astronomy, based mainly on the intention of explaining the observed phenomena. To understand is, among other things, to know how an entity endowed with a certain nature behaves under normal circumstances. The prediction is here of secondary interest.

In this and other examples given by McMullin's we can see that the cognitive values change and that this change can be driven by external factors. McMullin's position is, nevertheless, that the very development of science gradually eliminates the influence of the non-epistemic values, and that changes in those epistemic values are caused by scientific development itself.

Before concluding this section I would like to make reference to the general implications of Laudan's approach for the philosophy of science. Laudan states that although the methodological rules are often expressed in the form of categorical imperatives, their very form is that of hypothetical imperatives. This means that the maxim 'lets reject *ad hoc* hypotheses' must be understood as 'if you want to get new fruitful theories, you must reject the *ad hoc* hypotheses'. This kind of

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[6] Cf. McMullin, E., "The Goals of Natural Science," in Hronzsky, I. et al. (eds.), *Scientific Knowledge Socialized*, Akadémiai Kiadó, Budapest, 1988, pp. 27-58; and McMullin, E., "The Shaping of Scientific Rationality: Construction and Constraint," in McMullin, E. (ed), *Construction and Constraint: The Shaping of Scientific Rationality*, University of Notre Dame Press, Notre Dame (IN), 1988, pp. 1-47.

interpretation of the methodological rules makes a normative naturalization of the philosophy of science possible.[7]

Methodological rules are statements about connections between the aims and the means proposed to reach them. Therefore, methodologies can be empirically evaluated. Then, Laudan says, the only relevant meta-methodological question is the following one: "given any proposed methodological rule (couched in appropriate conditional declarative form), do we have –or can we find– evidence that the means proposed in this rule promotes its associated cognitive end better than its extant rivals?."[8]

For Laudan there are methodological and axiological changes in science. The reticulated model of justification allows for methodological and axiological learning.[9] Laudan points out:

"My general statement is that all the principles and rules for the evaluation of scientific theories make some substantial presuppositions about the structure of the world which we live in and about us as researchers... As soon as we recognize this, it becomes clear that the truth of any rule depends, to a certain extend, on what we will learn about the world in the future. But this is simply to say that methodologies and theories of knowledge are, in fact, theories. They represent our best conjectures about how to ask questions to nature and about how to evaluate the answers. Like all theories, they are tentative, and they are open to changes, precisely due to what we learn."[10]

3. The Role of Values in Applied Science

The study of the function of values are especially important in applied science because its particular relation with social conflicts. Kristin Shrader-Frechette, in several of her works, has analyzed the role of epistemic and non-epistemic values in applied science, specifically in the science that has the mission of advising public policies. Her starting point is the judgments about methodological values (or methodological value judgments) that scientists are continuously forced to pass. Although scientists can avoid bias and cultural values, "methodological or epistemic values are never avoidable, in any research, because all scientists must

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[7] Cf. Laudan, L. "Progress or Rationality? The Prospect for Normative Naturalism," in Papineau, D. (ed), *The Philosophy of Science*, Oxford University Press, Oxford, 1996, pp. 194-214; Laudan, L., "La teoría de la investigación tomada en serio," in Velasco, A. (ed), *Racionalidad y cambio científico*, Paidós/UNAM, México D. F., 1997, pp. 25-41; and Laudan, L. "Naturalismo normativo y el progreso de la Filosofía," in Gonzalez, W., (ed), *El Pensamiento de L. Laudan. Relaciones entre Historia de la Ciencia y Filosofía de la Ciencia*, Publicaciones Universidad de A Coruña, A Coruña, 1998, pp. 105-116.

[8] Laudan, L. "Progress or Rationality? The Prospect for Normative Naturalism," p. 208.

[9] A similar point of view has been defended by D. Mayo. See Mayo, D., *Error and the Growth of Experimental Knowledge*, The University of Chicago Press, Chicago, 1996.

[10] Laudan, L., "Naturalismo normativo y el progreso de la Filosofía," p. 115.

use value judgments to deal with research situations involving incomplete data or methods."[11]

Methodological value judgments come into play whenever a scientist makes an inference on how to treat unknown cases, what statistical tests to use, how to determine sample size, establishing where the burden of proof lies, what theory or model to use, the acceptability of the interpolation of lost data, if it is correct to extrapolate the data from the laboratory to field trials, if the incomplete information on a phenomenon is enough to extract conclusions, etc. That scientists need to make methodological value judgments means that they must judge their own methods. Such judgments can be correct or erroneous.

One of Shrader-Frechette's numerous case studies is related to the scientific controversy surrounding the construction of an underground nuclear repository in Maxey Flats (Kentucky). Some of the studies completed previous to the construction of the nuclear repository calculated that the plutonium would be displaced by only half an inch in 24.000 years. When the installation was again opened after ten years, plutonium was discovered in a two-mile radius from its original location. The geological predictions were erroneous by six orders of magnitude.[12]

The construction of this nuclear repository was preceded by a controversy between two groups of scientists. The controversy started because of the lack of data regarding the suitability of the ground with respect to avoiding the possible displacement of radioactive material due to underground water currents. In this situation, both groups opted for different hierarchies of cognitive values. One of the groups gave priority to the scientific community's majority point of view regarding the capacity of plutonium to migrate in that kind of soil. In other words, this group adhered to the criterion of external consistency, which translated into a number of strategies to test the impermeability of the soil. For the other group, on the contrary, the internal coherence prevailed and it insisted on the porosity of the soil.

In this example the scientific controversy had important, and maybe dramatic, social consequences. In this type of situations, Shrader-Frechette argues, Laudan's reticulated model is insufficient. It would be necessary to add an additional level on which moral values like the protection of citizens from possible leaks of radioactive material were contemplated.[13] In applied science with important social consequences the criteria for the selection of hypotheses cannot be the

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[11] Shrader-Frechette, K., *Ethics of Scientific Research*, Rowman and Littlefield, Lanham, 1994, p. 53. On bias and cultural values in science see Longino, H. E., *Science as Social Knowledge: Values and Objectivity in Scientific Inquiry*, pp. 62-103.

[12] Cf. Shrader-Frechette, K., *Risk and Rationality: Philosophical Foundations for Populist Reforms*, University of California Press, Berkeley, 1991; and Shrader-Frechette, K., "Hydrogeology and Framing Questions Having Policy Consequences," *Philosophy of Science*, v. 64, (1997), pp. S149-S160.

[13] Shrader-Frechette, K., "Scientific Progress and Models of Justification," in Goldman, S., (ed), *Science, Technology, and Social Progress*, Lehigh University Press, Bethlehem, 1989, pp. 196-226.

same as in academic science. Rather, it is necessary to keep in mind the different social consequences of alternative hypotheses.[14]

Although Shrader-Frechette does not deny the influence of the non-epistemic values in scientific research, her argument proceeds in the inverse order. The methodological decisions that appeal to cognitive values possess consequences for peoples' lives, and for that reason it is also necessary to keep in mind moral values. This is clearly the case in applied science, which is the main object of Shrader-Frechette's analysis.

Heather Douglas has followed the same line of argument as Shrader-Frechette.[15] She analyzes the indeterminacies that appear in several studies on the carcinogenic potential of dioxins, and argues that due to the inductive risk, the risk of adopting the false hypothesis, it is necessary to keep in mind non-epistemic values whenever non-epistemic consequences associated with the errors exist. These non-epistemic consequences of the errors must be considered in the different stages of research: choice of methodology, characterization of data, and interpretation of the results.

As a general consideration of the role of values in applied science, I will refer to the work of Deborah Mayo on risk assessment. According to Mayo, it is possible to analyze risk assessment from two points of view: the sociological and the metascientific.[16] The sociological approach defends the idea that since all assessment of risk is influenced by social factors it is not possible to carry out any type of objective comparative evaluation of alternative assessments. The metascientific approach, on the contrary, strives to carry out such comparisons by showing which are the underlying assumptions of the different risk estimates, and their justification, and in what measure they are supported by the available evidence.[17]

Although both positions, the sociological and the metascientific, build on the idea of the influence of political and moral values in risk assessment, they arrive at radically different conclusions. In the words of Mayo:

"The metascientific view acknowledges the lack of value-free, universal, algorithmic methods for reaching and evaluating claims about the world (in our case, risk-assessment claims). But far from understanding this to preclude

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[14] Cf. Shrader-Frechette, K. and McCoy, E. D., *Method in Ecology. Strategies for Conservation*, Cambridge University Press, Cambridge, 1993; and Shrader-Frechette, K. "Hydrogeology and Framing Questions Having Policy Consequences," pp. S149-S160.

[15] Although Douglas doesn't refer explicitly to the work of Shrader-Frechette. See Douglas, H., "Inductive Risk and Values in Science," *Philosophy of Science*, v. 67, (2000), pp. 559-579.

[16] Cf. Mayo, D. G., "Sociological versus Metascientific Views of Risk Assessment," in Mayo D. G. and Hollander, R. D. (eds.), *Acceptable Evidence: Science and Values in Risk Management*, Oxford University Press, Oxford, 1991, pp. 249-279.

[17] On this distinction between sociological and metascientific analysis of risk assessment, see also Shrader-Frechette, K., "Radiobiological Hormesis, Methodological Value Judgments, and Metascience," *Perspectives on Science*, v. 8, (2001), pp. 367-379.

objectivity, an explicit recognition of how value judgments can influence the statistical risk assessments can –according to the metascientist– be used to interpret assessments more objectively. One way to recognize the policy influences and implications of risk assessment judgments is to evaluate their corresponding protectiveness for the case at hand. This requires critical metascientific tools."[18]

For Mayo, there are two conceptions of risk assessment with negative consequences. One of these affirms that the considerations related to risk management can (and must) be separated from risk assessment. The other affirms that due to the presence of these considerations it is not possible to carry out comparative analyses of different risk assessments. In the face of these two conceptions, Mayo defends the idea that an understanding of the interrelations between the content of knowledge statements, the methods and the assumptions or presuppositions facilitates the critical analysis of risk assessment. This is a legitimate and constructive task for philosophical analysis.

4. Methodological Learning in Risk Analysis

In what follows I will approach the interactions of epistemic and non-epistemic values in methodological change in a particular kind of regulatory science, namely risk assessment. In this analysis I will deal mainly with two issues. The first is related to methodological learning, the process by which scientists, as well as society, learn about the relationship between methodological judgements and the aims of scientific research, as well as the use of scientific knowledge in the policy-making process. The second issue concerns the influence of social dynamics (in this case the social conflicts related to technological risks) on this learning process. I will approach both issues in a combined way, showing that a fruitful relationship between philosophical and social studies is possible.

The analyses of risk assessment have been centered on the social and environmental consequences of inductive risk (Shrader-Frechette, Cranor, Douglas). Hempel characterizes inductive risk as the possibility of making an error in accepting or rejecting a scientific hypothesis.[19] In academic science the inductive risk affects only the aims of science, in risk assessment it can also affect public health and the environment. Traditionally it has been accepted that the non-cognitive consequences should not be contemplated by scientists as long as they are related to the use of scientific knowledge, not to the process of knowledge production. This general approach prevents methodological learning in relation to the question of how to generate scientific knowledge useful for the protection of public health and the environment. This is, in fact, an approach that was abandoned some time ago by several government agencies in charge of assessing risks.

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[18] Mayo, D. G., "Sociological versus Metascientific Views of Risk Assessment," p. 261.

[19] Cf. Hempel, C. G., "Science and Human Values," in Hempel, C. G., *Aspects of Scientific Explanation*, The Free Press, New York, 1965, pp. 81-96.

In its well-known 1983 report, the National Research Council defines risk assessment as a research process with the following four steps:

1. *Risk identification.* Characterizing the nature and scope of evidence indicating that a substance can increase the incidence of disease (cancer, birth defects, etc...) in humans, laboratory animals, or other test systems.
2. *Quantification of the dose-response relationship.* Calculating the incidence of an effect as a function of exposition in several populations, extrapolating from high doses to low doses, and/or laboratory animals to human beings.
3. *Exposition analysis.* Identifying the populations that are, or could be, exposed to a substance in certain circumstances.
4. *Risk characterization.* Estimating the incidence of effects on health under different conditions in each of the populations. In this step the information obtained in each of the previous steps is used.

Risk characterization, the fourth step of risk assessment, is generally conceived as a synthesis and translation of the information. Synthesis of the information obtained in the three previous steps and translation of that information in the sense of showing its meaning for health and environment protection in such a way that it is useful for policy makers. In this sense, in the 1983 report the necessity of specifying the possible consequences of current uncertainties in the previous steps is pointed out. In this conception of risk assessment the first three steps would be directly related to scientific knowledge, while the fourth one would include mainly meta-analysis and prediction.

The conclusions of risk characterization are used for risk management, which mainly consists of drawing up different types of regulations regarding the use of products and productive processes. The traditional conception of the relationship between risk assessment and risk management is that assessment is a scientific activity which provides evidence about the nature and scope of risks, while management is in charge of making regulations, taking into account this evidence and the socially established levels of protection of public health and the environment. This conceptualization of the relationship between risk assessment and risk management is related to the traditional distinction between facts and values.

In its 1983 report the National Research Council clearly defends the separability between assessment and management: if considerations related to management affect risk assessment, the credibility of the assessment can be compromised. A similar point of view was expressed again in the 1994 report: it defends the necessity for risk assessment to be independent, and for explicitly distinguishing between conclusions based on facts and judgments based on values. The 1996 report questions this point of view and emphasizes the interaction between assessment and management of risks.[20]

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[20] Cf. NATIONAL RESEARCH COUNCIL, *Understanding Risk. Informing Decisions in a Democratic Society*, National Academy of Sciences, Washington, DC, 1996.

Possibly due to its social relevance, risk assessment has suffered a wide-reaching process of methodological analysis. As we will see in the following, this analysis has lead to a learning process regarding the methodologies that can best fulfill the practical values of risk assessment.[21] These practical values are to provide scientific knowledge about risks which is useful for the protection of public health and the environment. The controversies that have surrounded this type of regulatory science in the last two decades can be classified in four areas: 1) the burden of proof, 2) risk definition and identification, 3) standards of proof, and 4) rules of inference.

4.1. The Burden of Proof

The debates about the burden of proof are of a political nature, and are mainly related to the prioritization of different values, like economic growth or the protection of public health and the environment. That the debate is of a political nature doesn't mean that knowledge about how to better reach the prioritized goals would not be important.

Let us suppose that a sufficient social agreement exists that gives priority to the protection of public health and the environment over economic growth. Then the question emerges of how to reach this objective in a satisfactory way. For some time now, some environmental groups have considered that it would be necessary to shift the burden of proof. That is, whoever promoted an innovation would have to demonstrate that it didn't involve important risks for public health and the environment. The promoters of this point of view use moral arguments as well as empirical ones.

A moral argument in this context is one that points out that whoever will obtain most benefits from the introduction of an innovation has the responsibility of demonstrating that he or she is not giving rise to an important risk. Empirical arguments are those that affirm that the current situation in which the burden of proof rests with the public administration –which before adopting a regulation must demonstrate the risk that a product or productive process involves– has not sufficiently protected public health and the environment. An example of the combination of moral and empirical arguments is provided by Joel Tickner when he maintains that:

"As government authorities never have sufficient resources to study every chemical, factory, or ecosystem, it becomes critical for those undertaking a potentially dangerous activity (and who will ultimately benefit most from that activity) to have to prove that their activities will not harm humans or the environment."[22]

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[21] I use the expression "practical values" following Rodríguez Alcázar. In regulatory science in general, and in risk assessment in particular, this expression is clearly appropriate. See Rodriguez Alcazar, J., *Ciencia, valores y relativismo. Una defensa de la Filosofía de la Ciencia*, Comares, Granada, 2000.

[22] Tickner, J.A., "A Map toward Precautionary Decision Making," in Raffensperger, C. and Tickner, J. A. (eds), *Protecting Public Health and the Environment. Implementing the Precautionary Principle*, Island Press, Washington, 1999, pp. 162-186.

Another empirical argument (in this case, a comparative one) points out that in the economic sectors in which a shift of the burden of proof has already taken place, for example the pharmaceutical sector, public health is better protected than if the burden of proof rested with the public administration. The correctness of these empirical arguments depends on, for example, the characteristics of contemporary society and its institutions, the level of innovation and the characteristics of chemical substances and, particularly, the risks these can involve for public health and the environment. Statements about these characteristics are subject to an empirical analysis regarding their correctness or incorrectness.

For some authors it is necessary to analyze the social consequences of regulation and their level of permissiveness. The cumulative experience of the last twenty years regarding the assessment and regulation of risks indicates that sometimes the control of a risk through regulation produces the emergence of other risks (risk tradeoffs). This means that regulating a risk can become harmful for the protection of health and the environment if the countervailing risk is bigger.[23] Therefore, the effectiveness of the regulation depends on numerous factors.

Cass Sunstein argues that the relationship between health, wealth and safety must be analyzed since the data indicates that a decrease of wealth increases the risks to public health. Sunstein says that an increasing number of research projects in the area indicate that lives are being lost as a consequence of the obligatory cost of regulation, and that there are reasons for the government to take this problem seriously.[24] However, Shrader-Frechette points to the utter lack of empirical evidence in terms of controlled experiments or statistical analysis for Sunstein's causal assumption that regulation increases risk because it increases poverty.[25]

These considerations in this section indicate that the effectiveness of a proposal of a political nature like the shift of the burden of proof depends on numerous factors like certain economic and administrative characteristics, the nature of risks, the relationship between regulated risks and countervailing risks, etc. The determination of these factors depends on social education about risks and its regulation. Scientific knowledge is one of the key factors that contribute to this learning process.

The shift of the burden of proof has also been relevant to the development of some lines of scientific research and technological innovation. This has been the case with the European regulation of biotechnology. This regulation, guided by the precautionary principle, shifts the burden of proof. It has decisively

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[23] Cf. Graham, J. D. and Wiener, J. B., "Confronting Risk Tradeoffs," in Graham, J. D. and Wiener, J. B. (eds), *Risk vs. Risk. Tradeoffs in Protecting Health and the Environment*, Harvard University Press, Cambridge, 1995, pp. 1-41.

[24] Cf. Sunstein, C. R., "Health-Health Trade-Offs," in Elster, J. (ed.), *Deliberative Democracy*, Cambridge University Press, Cambridge, 1998, pp. 232-259.

[25] Cf. Shrader-Frechette, K, "Review of *Risk and Reason* (Cass Sunstein, 2002, Cambridge University Press)," *Notre Dame Philosophical Reviews,* (2003.04.09).

influenced the modification of certain trajectories of technological innovation in biotechnology and given an impulse to research programs (like scientific post-marketing monitoring) on the long-term ecological impacts of Genetically Modified Organisms (GMOs).[26]

4.2. Risk Definition and Identification

Risk assessment and risk management are related to public concerns about the negative consequences of industrialization for public health and the environment. The conceptualization of something as a risk is a normative question subject to the influences of social dynamics. For Nicholas Rescher, risk assessment cannot be considered a value-free scientific activity, because to measure the magnitude of a risk demands a normative valuation, that is, to compare different types of harm and to value their "negativeness" for different social agents.[27] For Rescher, there are no objective facts that allow us to calculate the severity of damage or the best way of impersonally distributing harm.[28]

Health problems caused by pollution can be of different kinds. However, most of the risk assessments have traditionally focused on the carcinogenic potential of chemical substances. This is because the US EPA, from the beginning, considered that cancer represented a group of health problems caused by pollution, i.e., cancer was considered as an indicator for the health risks of many substances.[29] The centrality of cancer in risk assessment is due to the fact that environmentalists in the U.S. had focused their attention on the disease during the 1960s and 70s. The warnings outlined by Raquel Carson in her classic *Silent Spring* in 1962 were considered at that time anecdotic and non-scientific. The attempts of quantifying the carcinogenic potential of chemical substances were an answer to this type of warnings.[30]

Tesh has synthesized the environmentalists' main critiques of risk assessment.[31] These are:

1. Risk assessment has been centered on cancer, instead of considering all the diseases that the exposition to toxic products can cause.

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[26] Cf. Todt, O. and Lujan, J. L., "Spain: Commercialization Drives Public Debate and Precaution," *Journal of Risk Research*, v. 3, (2000), pp. 237-245; Lujan, J. L. and Todt, O., "Dinámica de la precaución. Sobre la influencia de los conflictos sociales en la regulación de los OGMs," in Iañez, E. (ed), *Plantas transgénicas: De la Ciencia al Derecho*, Comares, Granada, 2002, pp. 141-154; and Todt, O., "Regulating Agricultural Biotechnology under Uncertainty," *Safety Science*, v. 42, (2004), pp. 143-158.

[27] Cf. Rescher, N., *Risk: A Philosophical Introduction to the Theory of Risk Evaluation and Management*, University Press of America, Lanham, 1983.

[28] Against Laudan's affirmations, which use fatality as the measure of risk. See Laudan, L., *The Book of Risks*, John Wiley and Sons, New York, 1994.

[29] Cf. Tesh, S. N., *Uncertain Hazards. Environmental Activists and Scientific Proof*, Cornell University Press, Ithaca (NY), 2000.

[30] Cf. Cranor, C., *Regulating Toxic Substances. A Philosophy of Science and the Law*, Oxford University Press, New York, 1993, p. 110.

[31] Cf. Tesh, S. N., *Uncertain Hazards. Environmental Activists and Scientific Proof*, p. 66.

2. Current risk assessment is done under the supposition that risks affect everybody in the same way, and has not paid attention to biological and social diversity.
3. Disease has been considered as the expression of the damage caused by substances, other impacts have not been considered.
4. It has been supposed that substances act independently, instead of analyzing the synergies and interactions that can take place among them.
5. The standards of proof required for establishing the relation between the presence of a substance and a damage are too demanding.

These criticisms have forced changes in risk assessment and in general in scientific research devoted to health and environmental risks. The characterization of damage has been extended beyond cancer and disease. Problems related to sexual development, reproductive problems like infertility and cognitive dysfunctions have gotten the attention of researchers. The so-called hormonal disrupters are the best-known example. The effects of environmental pollution on wildlife and ecosystems has begun to be considered as indicators for human health.[32] Physiologic abnormalities that can be found in the blood or the adipose tissue are also used in this way, i.e. as biological markers for the effects of pollution on health. Also, guidelines have been written up for risk assessments to consider human diversity regarding age, genetic susceptibilities, etc.

The controversy on biotechnology in Europe has also led to a redefinition of the possible damage. In the beginning, the possibility of genetically modified plants generating resistance in insects to certain insecticides had been considered by European authorities as an agronomic problem. But beginning with the 1996 controversy over the registration of a variety of transgenic corn, this possibility began to be considered as an adverse effect. As a consequence, scientific methodologies were developed for its study.[33]

The conceptualization of certain effects of human actions as risks depends on social concerns. These concerns have influenced risk assessment in a decisive way. And these concerns are also related to learning about the effectiveness of risk assessment and risk management. The necessity to extend the characterization of harm has been argued on the grounds that the assessment and regulations centered on cancer and diseases were not leading to the desired level of protection of public health through regulation. This conclusion is also a consequence of the scientific and social learning about risk during the last decades.

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[32] Cf. TESH, S. N., *Uncertain Hazards. Environmental Activists and Scientific Proof,* p. 67, and KRIEBEL, D. ET AL., "The Precautionary Principle in Environmental Science," *Environmental Health Perspectives*, v. 109, n. 9, (2001), pp. 871-876.

[33] Cf. LUJAN, J. L. and TODT, O., "Dinámica de la precaución. Sobre la influencia de los conflictos sociales en la regulación de los OGMs," pp. 149-151.

4.3. Standards of Proof

There are two questions regarding standards of proof that have arisen recently. The better-known concerns the *level of rigor* of the standards of proof in the risk regulation processes. The second one is what *kind of evidence* must be considered as proof, for example, that a substance can cause negative effects for health or the environment.

It has been argued that the standards of proof in the current regulatory processes are extremely demanding. This means that numerous dangerous substances are possibly being used because their danger has not been demonstrated according to such standards of proof. Therefore, it has been proposed, with the purpose of protecting health and the environment, to relax these standards of proof. Those who defend this position see two advantages: that possibly many dangerous substances which at the present time are not regulated would be regulated, and –with the same amount of resources– many more substances could be analyzed. Both consequences are desirable given the aim of protecting public health and the environment.

A proposal in this sense has been advanced by Carl Cranor.[34] Cranor proposes using short-term tests as the basis for regulating chemical substances. This type of scientific tests concerns mutagenic properties, the relationship between chemical structure and biological activity, etc. Cranor's proposal is that in some cases this kind of test substitute bioassays and epidemiological studies, more intensive in time and resources.

Cranor's argument is typically methodological. The aim of risks assessment is the protection of public health and the environment, and Cranor's analysis consists of a comparative evaluation of different methodologies of risks assessment (in this case, of carcinogenicity), keeping in mind this aim. This evaluation is carried out comparing the social costs of the false positive and the false negative that are produced by the different methodologies. The conclusion Cranor arrives at is that short-term tests can be socially better in many cases than tests that make use of bioassays and/or epidemiological studies.

Cranor defends his proposal pointing out some of the relevant characteristics of toxic substances that give rise to problems for their identification and assessment. Toxic chemicals (carcinogens, hormonal disrupters, neurotoxins, etc) are substances difficult to detect, they possess long periods of latency, are bio-accumulated and are taken up by human beings through the alimentary chain. In addition, they don't show direct evidence of their effects or their effects are

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[34] Cf. CRANOR, C., *Regulating Toxic Substances. A Philosophy of Science and the Law*, Oxford University Press, New York, 1993; CRANOR, C., "The Normative Nature of Risk Assessment: Features and Possibilities," *Risk: Health, Safety and Environment*, v. 8, (1997), pp. 123-136; and CRANOR, C., "Conocimiento experto y políticas públicas en las sociedades tecnológicas. En busca del apoyo científico apropiado para la protección de la salud pública," in LUJAN, J. L. and ECHEVERRIA, J. (eds.), *Gobernar los riesgos. Ciencia y valores en la sociedad del riesgo*, Biblioteca Nueva, Madrid, 2004, pp. 99-141.

similar to diseases which are the result of other causes, they operate through unknown molecular or sub-molecular mechanisms, their consequences are catastrophic for the affected individual, although their probability is low. These characteristics are very well known today due to the knowledge accumulated by scientific research. The cumulative experience of the analysis of these substances is also important for other reasons: we know what we do not know, and we know about the difficulties of obtaining a deeper knowledge of them.

Moreover, Cranor analyzes the epistemic characteristics of scientific research regarding biochemical risks, focusing especially on toxicology. Cranor asserts that:

> "Scientific bodies and most scientists are typically quite demanding in minimizing or preventing factual false positives, that is, that their procedures show that a substance has a toxic property when in fact it does not. They want to ensure that they are not mistakenly claiming that a substance is toxic when it is not. They tend to be cautious in coming to such conclusions.
>
> One instance of this practice is that scientists guard against random statistical errors in their experiments from producing false positive results by demanding that support for their conclusions must be statistically significant. This is only one statistical measure that is used in scientific inquiry, but a particularly easy one to utilize and quantify. Moreover, a focus on preventing false positives in statistical tests will as a matter of the mathematics involved increase the number and rate of false negatives. And, at least in research, scientists appear to have a lesser concern to prevent false negatives."[35]

The greater concern for the false positive is a methodological translation of an epistemic value, that is, accuracy. The concern for the false positive leads to require certain kinds of evidence in order to reach conclusions concerning the toxicity of a substance. Cranor's argument points out the consequences of the interaction between the characteristics of chemical substances and the epistemic characteristics of the scientific research on risk. The characteristics of toxics make it very difficult to establish links and causal trajectories. And the epistemic characteristic of research on the risks of toxic products makes it necessary to acquire knowledge of those links and causal trajectories. The combination of both factors, characteristics of toxics and epistemic characteristics of research, leads to research which is intensive both in time and resources.

Cranor reaches the conclusion that this type of risk assessment has undesirable social consequences. His work is an analysis of the social consequences of using certain methodologies. He maintains that the scientific epistemology, at least in these extreme forms, is not normatively neutral when it is used for social applications. In the particular case of the risk assessment of toxic substances, a

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[35] Cranor, C. "Conocimiento experto y políticas públicas en las sociedades tecnológicas. En busca del apoyo científico apropiado para la protección de la salud pública," p. 110.

conflict arises between an epistemic value like accuracy and a practical value like the protection of public health.

There are other proposals similar to those of Cranor. Some authors defend that in order to protect the environment and public health an analysis based on the weight of evidence is better than trying to acquire an exact determination of the level of risk involved.

> "The weight-of-evidence approach to decision-making takes into account the cumulative weight of information from numerous sources that address the question of injury or the likelihood of injury to living organisms. Types of information that might be considered include observational studies, worker case histories, toxicological studies, exposure assessments, epidemiological studies, and monitoring results. Based on the weight of evidence, a determination is made as to whether an activity has caused or is likely to cause harm and the magnitude of that harm."[36]

In a way this approach also implies a weakening of the standards of proof, and considers in a generic way the evidence accumulated about the relationship between substances and health and ecological problems. But it doesn't only consist in relaxing the standards of proof, but in addition it calls for taking into account all the available information coming from different sources. It is possible that no type of information by itself would be enough to affirm a causal relationship, but the combination of available information can be enough for decision-making.[37] Of course, this approach has consequences for the development of scientific research programs. The arguments in defense of this approach are similar to those analyzed in the case of short-term tests: it is a better way of reaching the objectives of protecting the environment and public health.

The American regulatory agencies have changed the standards of proof from the 70s until now. In the guidelines published by the EPA in 1976 for the assessment of carcinogenic substances it was considered that the evidence in humans (epidemiological studies) was fundamental to the identification of carcinogens and to the establishment of the dose-response relationship. The 1983 NRC report insisted on the relevance of human evidence, but it recognized the difficulty of obtaining and interpreting it, and recognized that in many cases it was necessary to appeal to data coming from bioassays. In 1985 the Office of Science and Technology Policy analyzed the problems presented by epidemiological studies for establishing causal relationships. In the guidelines of 1986, the EPA recommended carrying out global evaluations of the evidence coming from epidemic studies, bioassays and other information coming from

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[36] Tickner, J. A., "A Map toward Precautionary Decision Making," p. 169.

[37] Some analysts that defend this approach consider that the weight of evidence must be inversely proportional to the possible magnitude of the damage. That is, the higher the possible harm, the less information would be required to take a decision. Therefore, this proposal is based on the valuation of the social consequences of the application of a epistemic value like accuracy.

short-term tests and concerning the relationship between chemical structure and biological activity. In 1996, the EPA recommended considering all the available evidence: human, animal as well as supplementary evidence.

4.4. Inductive Risk and Rules of Inference

In risk assessment scientists must face numerous indeterminacies. These indeterminacies are solved by methodological decisions. An important part of these indeterminacies that show up in risk assessments are related to the fact that it is not possible to study experimentally the effects of many chemical substances on human beings. Therefore, scientists must appeal to epidemiological data as well as data coming from bioassays (i.e. using animal models). In other words, it becomes necessary to extrapolate from high dose to low dose and from animals to human beings. In the studies on the effects of radiation, for example, it is necessary to estimate the effects of low-dose radiation based on data of high levels of radiation (usually epidemiological data coming from nuclear accidents or from the atomic bombs dropped on Japan in World War II). To this end different extrapolation models (e.g., dose-response curves) are used. Using one or the other model leads to very different results that can differ by several orders of magnitude.

The indeterminacies can show up in the different stages of risk assessment. These indeterminacies can be solved through the selection of interpretations of data, alternative methodologies and/or rules of inference. Generically, we can call the selection among alternatives in each one of these categories methodological decisions.[38] These methodological decisions determine to an important part the final result of a process of risk assessment, and can be influenced by a different type of considerations.

These methodological decisions can be related, for example, to the weight conferred to different studies with different results, the level of statistical significance, the models of extrapolation, etc. Different alternatives will increase the number of false positives or false negatives. It is impossible to minimize both types of errors without modifying the size of the sample. To minimize one or another type of error is a normative decision with social consequences.

To increase the size of the sample is the other possibility. But then questions regarding the social (economical) costs of the research regarding that particular risk, and also relative to the opportunity costs related to the possibility of analyzing other, different risks enter into consideration. Therefore, as Carl Cranor has pointed out, "where statistical studies are needed to provide evidence of toxicity, decisions about sample size, cost of experiment, and the desired degree of accuracy all involve normative decisions in the very conception and design of such studies. Moreover, since risk assessment is an imperfect procedure,

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[38] Cf. LOPEZ CEREZO, J. A. and LUJAN, J. L., *Ciencia y Política del riesgo*, Alianza Editorial, Madrid, 2000, pp. 107-114.

there will be mistakes and which mistakes the process is designed to avoid is an important normative issue. One must make a policy decision (or decisions) about the degree of accuracy and the importance of the risks to be prevented. Explicit discussion of alternatives to conventional risk assessment and risk management practices of scientists should occur because many current practice may frustrate preventive goals."[39]

The first problem presented by the epidemiological studies is the identification of diseases. This first step depends on the quality of the gathered medical information as well as the quality of the information that can be obtained during the study. The characteristics of some diseases, like long periods of latency, hinder their identification. When a disease has been identified, it must be related causally to the presence of some substance. The problem that appears here is related to the inference of causality from statistical data. The effect of a substance can be a small variation of the normal rate of deaths from cancer that could be statistically not significant. In some cases a relationship cannot be established because the sensibility of the epidemiological study simply does not allow for this.[40]

Let us suppose that it has been possible to establish a relationship between the substance and the disease. Then it is necessary to estimate the dose-response relationship. As happened with the identification of disease, the problems begin with the compilation of information. Information is needed on the source of contamination, the way of exposition (air, land, water, etc.), the channels of transportation in each medium, physical and chemical transformations, the path of entrance into the organism, the intensity and frequency of exposition, and the patterns of spatial and temporal concentration.[41] Then there are other, more simple cases: the epidemiological data coming from accidents. In such cases, it is possible that the epidemiological data does not offer any doubt regarding the causal relationship between the exposition to high doses of a particular substance and a serious disease. The problem is to determine, from this epidemiological data, what happens in the case of low doses, i.e., to extrapolate from high dose to low dose. What happens here is that the data is normally compatible with different mathematical models of extrapolation. The theoretical models are underdetermined by the available evidence.[42]

Similar problems show up in studies with laboratory animals. Heather Douglas has analyzed research on the relationship between dioxins and liver cancer in laboratory rats. The study was carried out in 1978 and was used for the regulation of the dioxin levels in the environment. The samples taken from laboratory animals' livers were re-evaluated twice, in 1980 and in 1990. The classification of these

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[39] Cranor, C., "The Normative Nature of Risk Assessment: Features and Possibilities," p. 128.

[40] Cf. Mayo, D. G., "Sociological versus Metascientific Views of Risk Assessment," pp. 267-275.

[41] Cf. Tesh, S. N., *Uncertain Hazards. Environmental Activists and Scientific Proof*, pp. 35-36.

[42] Cf. Lopez Cerezo, J. A. and Lujan, J. L., *Ciencia y Política del riesgo*, pp. 107-114 and 119-130.

samples as cancerous lesions (benign or malign) was different each of the three times (the number of cancers found in 1978 was higher than in 1990.). As Douglas points out, "the judgment of whether a tissue sample has a cancerous lesion or not has proven to be more subtle than one might initially suppose."[43] Also, in the bioassay studies, problems show up related to the models of extrapolation, as in the case of the epidemiological studies. Other indeterminacies are related to the extrapolation from animals to humans.

An issue which has been studied in-depth by the analysts of risk assessment is the higher concern among scientists for false positives than for false negatives (Cranor, Shrader-Frechette, Douglas). This tendency is the methodological translation of the search for accuracy, and its goal is to avoid asserting a false hypothesis. As I have already pointed out, it is impossible to reduce false negatives and false positives at the same time. The higher concern for the false positives is translated into an increase of the false negatives. This concern is summed up in the studies' election of the levels of statistical significance, which reflects a decision about which kind of error a scientist is willing to tolerate more easily. These methodological decisions possess consequences for regulation, and therefore have social consequences. The false positives lead to over-regulation, while the false negatives to sub-regulation. In general, the social costs of sub-regulation are higher than those of over-regulation.[44] Several authors have defended the idea that scientists involved in risk assessment must bear in mind the social consequences of their methodological decisions. In other words, they should also pay attention to practical values.

The attention to practical values has been an explicit policy of regulatory agencies in their elaboration of guidelines for choosing among different dose-response models. In the studies on non-carcinogenic substances models with thresholds are used, below which effects are not observable. In the case of carcinogenic substances, however, models without thresholds are used, assuming that although the doses are small, they can cause alterations in the DNA that could lead to the emergence of tumors. In the debates that have taken place during the last few years regarding the new EPA guidelines for the assessment of carcinogenic substances, specific models of extrapolation have been explicitly advocated as being scientifically defensible as well as protective of public health.

5. ANALYSIS AND DISCUSSION

In the preceding section of this paper I have analyzed different changes and proposals for change in risk management (e.g., in connection with the burden of proof), the definition and identification of risks, the level and types of

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[43] DOUGLAS, H. "Inductive Risk and Values in Science," p. 571.

[44] In general, although not always. This depends on the possible use of the product and the magnitude of the risks. Other considerations would be relative to who benefits from the product and who suffers the risks.

evidence required for regulation (standard of proof) and the rules of inference (in the identification of risks as well as in the establishment of the dose-response relationship).

It is not the aim of this work to carry out an evaluation of such changes and proposals. My aim has been to show that such changes and proposals take place in a learning process relative to the best ways of reaching the goals of protecting public health and the environment from the risks introduced by products and productive processes. This learning process has taken place on the basis of the cumulative experience with respect to the characteristics of toxic substances and their interactions with human beings and ecosystems, the epistemic characteristics of scientific research on those toxins and their interactions, and the effects of the different regulatory systems regarding the protection of health and of the environment.

In all the cases analyzed, the dynamics has been similar to that proposed in Laudan's reticulated model: the learning process is the product of the interaction among substantive elements of scientific knowledge (in this case statements about the health and ecological consequences of products and productive processes), methodological rules and the goals of research. This model is appropriate if we keep in mind that the goals of risk assessment are to provide knowledge useful for protecting public health and the environment. That is, if we accept in some sense the modification of Laudan's model proposed by Shrader-Frechette, and the considerations about risk assessment spelled out by authors like Cranor and Douglas.

Laudan, however, would not agree with these modifications.[45] The only values and goals that he considers are the epistemic ones, and in that sense he doesn't distinguish between applied science and academic science. Science is mainly a matter of belief, and in this realm the relevant issue is to compare the different theoretical alternatives and to choose the best one according to certain epistemic values (e.g., preferring the theory best supported by empirical tests).

From a classic point of view, whenever scientific knowledge is used to advise actions (e.g., regulations or public policies in general), it is necessary to carry out calculations of the social costs of false positives and false negatives. Therefore, it is necessary to distinguish between knowledge and its applications, in this case between risk assessment and risk management. This is the classic point of view according to which the consideration of non-epistemic values in the research process distorts scientific knowledge.

Some authors have documented historically the influence of external factors or non-epistemic values on risk assessment.[46] But the classic point of view must

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[45] Cf. LAUDAN, L., *The Book of Risks*, pp. 2-42.

[46] Cf. JASANOFF, S., *The Fifth Branch. Science Advisers as Policymakers*, ch. 11, pp. 229-250; and TESH, S. N., *Uncertain Hazards. Environmental Activists and Scientific Proof*, ch. 2, pp. 24-39.

be understood as a proposal relative to the goals and values of scientific research, and to the relationship between scientific knowledge and its applications: it affirms that it is better for science to try to reach epistemic goals, letting itself be guided by the criterion of preferring the comparatively best theories. This is, of course, a reasonable proposal defended by many philosophers of science, scientists and also public policy makers.

Nevertheless, my valuation is that this proposal can impede an important part of the methodological learning in risk assessment. Not keeping in mind the context and the final goals of regulatory science limits our possibilities of methodological learning. It is in this sense in which I consider that it is important to keep in mind the non-epistemic values in this kind of scientific activity. It is more likely that methodological innovations be obtained in risk research if the practical goals of that kind of scientific knowledge are considered explicitly. To limit methodological learning relative to those goals can lead to a situation in which science doesn't perform the social functions that are expected of it.

From a long-established point of view the questions related to the burden of proof and the standards of proof belong to the realm of risk management. However, we have seen that decisions on the burden of proof and the standards of proof influence methodological learning in risk assessment. It is an empirical question if the short-term tests or the weight-of-evidence-based approaches are useful or not to protect public health and the environment from technological risks. The same is valid for the different models of extrapolation. But to be able to decide on these empirical questions, it is necessary to keep in mind the practical values of risk assessment.

As we have seen, Mayo argues that certain types of sociological analysis of regulatory science disable methodological learning.[47] This is because such studies do not facilitate the comparison among the different methods in order to reach the goals that are pursued with the research. But the approaches asserting the neutrality of science regarding practical values also disable methodological learning, at least in the realm of applied science and regulatory science.

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[47] MAYO, D. G., "Sociological versus Metascientific Views of Risk Assessment," pp. 275-276.

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HOW TO REFORM SCIENCE AND TECHNOLOGY[*]

Kristin Shrader-Frechette

Reasoning by analogy is a dangerous enterprise for an analytic philosopher. Yet several years ago, Oxford philosopher Jonathan Glover used a powerful analogy. Suppose each of 100 Sub-Saharan tribesmen has a bowl of 100 baked beans for lunch. Suppose also that a band of 100 bandits attacks the unarmed tribe, and each bandit takes one lunch from one of the tribesmen. Obviously this theft is morally wrong. But suppose instead the bandits have an ethicist among them. He directs the 100 tribesmen to line up with their bowls of baked beans. Next suppose that each bandit takes one bean from each of the 100 bowls.[1] The bandit ethicist might say this second action is not wrong, because no single bandit did significant harm to any one person; each bandit stole only a single bean from each tribesman. Yet the net effect of both thefts is the same.

1. ANALOGIES AND TECHNOLOGICAL RISK

If it is wrong for a single polluter directly and knowingly to kill an innocent, identifiable individual, is it likewise wrong, all things being equal, for a polluter to contribute equally to releases which, together with the equal emissions of 99 other polluters, also kill an innocent person prematurely? Is there something wrong with societies in which thousands of manufacturers each emit "acceptable" levels of pollutants, yet together these "acceptable" pollutants are the main contributors to a cancer rate that kills half of us prematurely (see notes 43-44)?

Consider a case closer to home, the heavily industrialized area of East Chicago. The community is 89-percent minority, extremely poor, and roughly 100 miles from the university where I teach. Suppose I see a child swimming in East Chicago's Calumet River, just downstream from the effluent pipe of a facility releasing toxic wastes. Suppose I know that, if the child swims in the contaminated water (near the pipe) for only several hours, he will have been exposed to a neuron-toxin that will cause him to die prematurely. Finally, suppose that I could prevent the child's death by spending only 2 hours of my time, to contact him, his parents, the facility's security guard, the polluting company, and officials (who might build a fence or post warning signs about

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[1] The Glover example is quoted by PARFIT, D., *Reasons and Persons*, Oxford University Press, New York, 1984, p. 511. See SHRADER-FRECHETTE, K., *Risk and Rationality*, University of California Press, Berkeley, 1991, pp. 70-71.

swimming in the river). If I did not spend these 2 hours to prevent the child's death, when I could do so, at no great sacrifice on my part, arguably I would be morally reprehensible. Now consider a second case. Suppose I know that the same river supplies drinking water for East Chicago; that nearby children (who drink the water), exhibit statistically significant increases of fatal neurological injuries; and that, by donating only 2 hours of my time to a local nongovernmental organization (NGO), dedicated to river clean up, I could help prevent one child's death from the neurotoxin. If I would be wrong for not trying to prevent the first child's death, wouldn't I also be wrong for not trying to prevent this second child's death? Each of faces cases like this, again and again, in many areas of our lives. Yet most of us do nothing about it.

One reason is that reality is rarely as clear as these examples. On the one hand, there are many uncertainties, including how to apportion collective responsibility, how many children would die because of the contaminated river, how many hours of work are necessary to save one child's life, how many other people would join the NGO, and so on. On the other hand, there are dose-response curves for many of the 80,000 industrial contaminants to which we are subjected, and many developed nations have Toxic Release Inventories (TRIs), in which each industry must reveal which, and what quantities of, toxins it releases. Only a minimal amount of research is necessary, especially for a university-educated person, to become aware of the magnitude of any given threat she faces. Thus, if we in democratic nations do *nothing at all*, either to become informed about technological problems, to help educate others about them, or to be active in NGOs seeking to avoid them, when we easily could do so, we err. *Prima facie*, we are as much at fault as the person who does nothing to protect the child swimming in the contaminated river, the person who does nothing to help reform the practice and use of science and technology. Why would we be at fault?

2. The Need Argument and the Case of the ICRP

Perhaps the major argument is that such reform is badly needed, both to secure the integrity of science and to protect the victims of its misuse. A recent US report underscored the bias in much science when it showed that, merely by reading the titles, authors, and financial supporters of scientific research, it was possible to predict the conclusions in 81 percent of all cases.[2] Other biases in the practice of science occur because commercially and industrially controlled research accounts for 75 percent of all scientific work. Publicly funded research, driven by

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[2] Cf. Erman, D. and Pister, E., "Ethics and the Environmental Biologist," *Fisheries*, v. 14, no. 2, (1989), p. 7. See also, for example, Turner, C. and Spilich, J., "Research into Smoking or Nicotine and Human Cognitive Performance: Does the Source of Funding Make a Difference?," *Addiction*, v. 92, (1997), pp. 1423-1426.

no particular profit agenda, accounts for only 25 percent of all science.[3] Given the private control of most scientific research, it is not surprising that most of us are ignorant about many effects of science and technology, like toxic chemicals. US government reports admit that "up to 90 percent of all cancers are environmentally induced and theoretically preventable."[4] If so, we need not so much a cure for cancer, as its prevention. We are literally killing ourselves, because of our misuse of science and technology. Among developing nations, need for reform is even greater. The World Health Organization (WHO) claims that pesticides annually kill 40,000 people in developing nations; they seriously injure another 450,000. Most of these pesticides are produced in developed countries, banned for use there, but instead shipped abroad.[5] Or consider infant formula, a misused food technology. In 2001, after the International Baby Food Action Network argued against aggressive and misleading advertising, marketing, and labeling of infant formula in developing nations –by companies like Nestlé– President Bush argued against WHO safeguards. The WHO urges exclusive breast-feeding as safer, healthier, and cheaper for at least the first 6 months. But the US argued fiercely against WHO curbs on misleading baby-formula advertising in poor nations. It said it was "very anxious not to inhibit commercial activity."[6] Bush's behavior, even in the baby-formula case, suggests why industry and government, alone, cannot do the job of reforming science and technology.[7] Without consumer and public pressure for reform, companies that are more responsible (than Nestlé, for example), that accept WHO norms, could financially destroy themselves. Their higher standards, as in the infant formula case, could make them unable to compete with less scrupulous companies. To expect firms to introduce safer science and technology, and thus risk being undercut financially by less scrupulous

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[3] Cf. Beder, S., *Global Spin: The Corporate Assault on Environmentalism*, Chelsea Green Books, White River Junction, 2002, pp. 17-49, 63-71, 141ff and 161ff. Information on lobbying statistics from Dick Armey, House of Representatives Majority Leader, is taken from Armey, D., "Washington's Lobbying Industry, Appendix: Measuring the Lobbying Industry," obtained at *www.flattax.gov*, January 5, 2002. See also *The Center for Responsive Politics* at *www.opensecrets.org/lobbyists/index.asp* for information on the lobbying industry and amounts spent by various industries on lobbyists. Legislative data on the effects of lobbyists, campaign contributions, and PACs, is from Box-Steffensmeier, J. and Grant, J., "All in a Day's Work: The Financial Rewards of Legislative Effectiveness," *Legislative Studies Quarterly*, v. 25, no. 4, (1999), pp. 511-524. See also Common Cause, "Why People Who Value Families Should Care About Campaign Finance Reform," obtained at *www.commoncause.org*, Jan. 2, 2003. Additional lobbying information is from Ansolabehere, S., Snyder Jr. J. and Tripathi, M., "Are PAC Contributions and Lobbying Linked? New Evidence from the 1995 Lobby Disclosure Act," *Business and Politics*, v. 4, no. 2, (2002), pp. 131-156.

[4] US Office of Technology Assessment, *Assessment of Technologies for Determining Cancer Risks from the Environment*, US OTA, Washington, DC, 1981.

[5] Cf. Mathews, J., *World Resources 1986*, Basic Books, New York, 1986, pp. 48-49. See also Repetto, R., *Paying the Price: Pesticide Subsidies in Developing Countries*, Research Report Number 2, December 1985, World Resources Institute, Washington DC, 1985, p. 3.

[6] Yamek, G., "Pop Musicians Boycott Promotion," *British Medical Journal*, v. 322, no. 7280, (2001), p. 191. See also Exeter, P. B., "Campaigners for Breast Feeding Claim Partial Victory," *British Medical Journal*, v. 322, no. 7280, (2001), p. 191.

[7] See Shue, H., "Exporting Hazards," in Brown, P., and Shue, H. (eds), *Boundaries: National Autonomy and Its Limits*, Rowman and Littlefield, Totowa, 1981, pp. 130ff, for a similar argument.

corporations, is unrealistic.[8] This is why citizens, especially professionals, must bear the burden.[9] Underdeveloped countries, in particular, may be unlikely to impose strict technological standards, for example on ftlinepesticides, because they are competing with other nations for foreign investment.[10] Thus, citizens themselves need to force reform, in much the same way as college students are forcing reform of sweatshops.

Consider a recent case in which I have been involved, a paradigmatic example of (1) how scientific research and recommendations often merely "follow the money," and (2) why we need to reform the practice and use of science and technology. The case concerns the International Commission on Radiological Protection, the agency which issues global radiation-pollution protections (which are then adopted as law by individual nations). In 2003 in Vienna, the ICRP issued its first environmental-protection recommendations.[11] Before 2003, there were only international radiation regulations for protecting humans. I was the US member of the 5-person international committee of scientists (one each from Canada, Norway, Russia, the UK, and the US).

Despite the need for radiological protections, the 2003 ICRP recommendations are scientifically flawed and illustrate the need to help reform science and technology. (1) They omit all radiological protection of the abiotic environment, such as air and water. (2) They take an incomplete, reductionist approach to ecological risk assessment by ignoring all ecosystem-level structures and functions and instead address risks only to a few reference species. (3) They focus only on modeled, not measured, doses to these organisms. (4) They define "reference species" in terms of no operational scientific criteria but instead characterize them pragmatically as those species chosen because the analysts know the most about them. (5) They make no recommendations to optimize radiological protection of the environment and keep exposure ALARA (as low as reasonably achievable), although optimization and ALARA are a key part of ICRP norms for protection of humans, and even though all amounts of ionizing radiation are risky.[12]

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[8] See Loomis, T., "Indigenous Populations and Sustainable Development," *World Development*, v. 28, no. 5, (2000), pp. 893-910; Schnieder, T., "Lighting the Path to Sustainability," *Forum for Applied Research and Public Policy*, v. 15, no. 2, (2000), pp. 94-100; and Elliot, D., "Renewable Energy and Sustainable Futures," *Futures*, v. 32, no. 3/4 (2000), pp. 261-274.

[9] Cf. Shrader-Frechette, K., *Risk and Rationality*, pp. 157-166; and Gewirth, A., *Human Rights*, The University of Chicago Press, Chicago, 1982, p. 186.

[10] See Shue, H., "Exporting Hazards," in Brown, P. and Shue, H. (eds), *Boundaries: National Autonomy and Its Limits*, pp. 131-133. See Goldberg, K., "Efforts to Prevent Misuse of Pesticides Exported to Developing Countries," *Ecology Law Quarterly*, v. 12, no. 4, (1985), pp. 1025-1051 and McGinn, A., "Phasing Out Persistent Organic Pollutants," in Brown, L., Flavin, C. and French, H. (eds), *State of the World 2000*, Norton, New York, 2000, p. 87.

[11] Cf. International Commission on Radiological Protection (ICRP), *A Framework for Assessing the Impact of Ionizing Radiation on Non-Human Species*, reference 02-305-02, ICRP, Vienna, 2003.

[12] Cf. International Commission on Radiological Protection (ICRP), *Recommendations, ICRP publication 60*, Pergamon, Oxford, 1991.

In omitting abiotic protection, the ICRP errs because it ignores what is most easily, reliably, and empirically measured, air and water, and what is the "early-warning signal" for high species doses. Its omitting ecosystem-level risks is problematic because state-of-the-art ecological risk assessment (ERA) includes two different levels of methods, the toxicological and the systems level. And its requiring modeled, not measured, doses to references species is scientifically flawed because model results would be almost totally dependent on extrapolations chosen by the modeler, a scientist usually employed by the radiological polluter. There are no empirical checks and balances; no replication of results; and no escape from subjective, nonempirical models because estimates will be only those the modeler judges "likely",[13] not those based on explicit confidence levels, with statistically measurable uncertainty bounds.

The ICRP's basing all its environmental protections on doses to some arbitrarily chosen "reference species" likewise is scientifically indefensible because it gives no scientific definition of "reference species"; they are simply species about which modelers have the most information. In using reference species, the ICRP arguably sanctions science that amounts to the drunk looking for his watch under the streetlight. Why does the drunk look for his watch under the streetlight? Not because he lost his watch there, but because that is the only place he can see. Why does the ICRP sanction use of reference species? Not because they are species that are important for radiation protection, but because they are species about which we know something.

Obviously the ICRP recommendations are flawed in the way they do science, but they also err ethically in the way they use science to defend regulations. There is a representativeness bias, because all members of the ICRP committee were chosen, not by independent experts, but by those industrially and governmentally responsible for radiation protection; because virtually all members of the committee had done research only on toxicological, not ecosystem, ERA; and because virtually all members had already written articles, usually for their nuclear-industry employers, in support of modeled, rather than measured dose. There also were violations of procedural justice, because the pro-nuclear chair of the committee, from Sweden, allowed no votes from the 5 committee-member scientists.

When the US member requested basing all recommendations on the best science available from top refereed journals, the chair instead defended using mainly nonrefereed "gray" literature (published by industrial and private groups). When the American committee member asked the committee to require uncertainty analysis of estimated doses, the chair simply removed (from the

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[13] Cf. International Commission on Radiological Protection (ICRP), *A Framework for Assessing the Impact of Ionising Radiation on Non-Human Species*, reference 02-305-02, ICRP, Vienna, 2003, paragraph 119.

report) the written admission that no uncertainty analysis was done. When the US member asked for peer-review of the document, the committee chair asked for comments on the draft, posted on the ICRP website, but only the committee chair had access to the comments. When the US member called for a vote on the document, both the chair and the ICRP told her the ICRP did not vote. The draft document, in essentially the same form as the original draft, was published in 2003. It was published in a deliberately misleading way, listing all committee names, but without acknowledging that some members did not support it.

What will happen when international scientific protections, like these, rely merely on models, not measurements? On gray literature, not the best scientific journals? On a largely nontransparent monitoring system controlled mainly by those who use (and profit from) nuclear pollution?[14] The most obvious effect is that it will be easier and cheaper to pollute and yet not violate the law. US nuclear weapons cleanup will cost a trillion dollars; throughout the world, hundreds of reactors must be expensively decommissioned; and throughout the world, millions of nuclear workers and atomic veterans are loudly demanding compensation. It will be cheaper for government and industry to address these problems, if they have the flawed, nonempirical, nontransparent ICRP norms.

3. Why Science and Technology Go Wrong

At least one reason, for the ethically and scientifically flawed ICRP recommendations, may be that those of us who could help prevent such scientific and technological problems do not do so. And often we do not do so because much contemporary scientific ethics amounts to “rearranging deck chairs on the Titanic.” One such wrong-headed ethical approach is individualism. What most scientists emphasize (when they give their grad students and post-docs the required course in research ethics) is individualistic ethics: Individuals should not falsify data. Individuals should not claim authorship when inappropriate, and so on. By fixating on the personal, individual issues that are *necessary* for good science, they ignore *institutional* issues that are sufficient. Focusing on the individual trees, they ignore institutional forest of scientific ethics.

What are some of these institutional issues? As a January 2001 editorial in *Nature* argued, one issue is whether the university-industrial complex is “out of control.”[15] The Novartis deal with Berkeley and the Hoecht deal with Harvard both give patent rights to industry donors, even for work they have not funded. They convert public educational resources to private profits.[16] Another institutional issue

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[14] Cf. Shrader-Frechette, K., “Science versus Educated Guessing,” *BioScience*, v. 46, (1996), pp. 488-489.

[15] Cf. Nathan, D. and Weatherall, D., “Academic Freedom in Clinical Research,” *New England Journal of Medicine*, v. 347, (2002), pp. 1368-1370.

[16] Cf. Shrader-Frechette, K., *Ethics of Scientific Research*, Rowman and Littlefield, Savage (MD), 1994.

is preventing harassment of scientists who speak truth to power. Hematologist Nancy Olivieri was sued for breach of contract, after she entered into a research contract with a drug company. Fearing for patient safety, she blew the whistle on damaging side-effects of the medication, side-effects that the company tried to keep quiet.[17] Yet another institutional issue is why most nations have given in to industry pressure, and are not tracking chronic diseases and their possible environmental causes. If there were a health tracking act, as physicians' groups have recommended, and as Nancy Pelosi has introduced into the US Congress, the causes of technologically-induced fatalities would be geographically evident. Given our ignoring all these institutional problems, most of our nations focus on individual human deaths from known causes, like the avoidable 30,000 US auto deaths each year from drunk drivers. At the same time, they ignore the same number of deaths, 30,000 in the US, mostly among children, caused by power-industry particulates.[18] Drunk drivers are not a powerful lobby that contributes to presidential campaigns. But utility industries are.

Still another institutional issue is why scientists and professionals neither keep informed nor speak up when politicians misinterpret and misuse scientific and professional conclusions, for political reasons. The Bush administration recently disbanded more than 200 independent, federal scientific advisory committees that came to scientific conclusions different from those of Bush's donors and industry supporters. For example, after years of study, one federal scientific committee concluded the public is at risk from the genetic-testing industry and worked with FDA to develop the first such regulations. But the genetics-testing industry protested, so Bush dissolved the scientific committee, and no regulations were recommended. In Latin-Americanese, Bush "disappeared" the committee. Paul Gelsinger's son Jesse died in a Pennsylvania gene-therapy experiment, and Gensinger commented: "money is running the research show."[19] Such cases suggest that often science is for sale to the highest bidder or highest campaign donor. Perhaps political science is replacing laboratory science. By ignoring such institutional issues of scientific ethics –the invisible elephant in the middle of the laboratory– and focusing largely on issues like authorship, scientific ethics falls into the same individualistic pitfalls as most medical ethics. In so doing, ethicists ignore problems that kill far, far more people.

Minimalism, another wrong approach to ethics, presupposes that, if we do not lie, cheat, or steal, we are ethical. It ignores the fact that we are all members of familial, national, civic, and scientific communities, in whose problems and

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[17] Cf. Drazen, J., "Institutions, Contracts, and Academic Freedom," *New England Journal of Medicine*, v. 347, (2002), pp. 1362-1363.

[18] Cf. Shaeffer, E., "Power Plants and Public Health," *Physicians for Social Responsibility Reports*, v. 34, (2002), p. 3.

[19] Musil, R., "Political Science on Federal Advisory Panels," *Physicians for Social Responsibility Reports*, v. 24/25, (2003), p. 3.

omissions, we are all complicit. Many scientists are minimalists because they are communally challenged and relationally challenged. Yet most of us would not say, in response to being called at work, after our child was seriously hurt at school, "I'm too busy to go to the hospital. I'm a scientist, and I don't have time for those 'outside' activities. I make my social contribution through my science." Such an answer would be appalling. But such an answer also is appalling in response to the need for each of us to engage in continually working to reform science and technology. It also sounds like the attitude of the Prussian Academy of Sciences, when it universally condemned Albert Einstein in 1933, for criticizing Hitler's violations of civil liberties. The academy said science required Einstein to remain neutral.[20] Ethics does not always dictate what side one should take, like Einstein's, but it does dictate that we all have a moral responsibility to investigate, to be critical. People don't have the right to enjoy benefits of membership in the scientific or philosophical community and, at the same time, claim the right to be apolitical when that community is misrepresented or fails to do its job.

4. JUANA GUTIERREZ AND COLLECTIVE RESPONSIBILITY

But if individualism and minimalism don't work in ethics and science, what does? I call the alternative "scientific citizenship," It consists of ongoing reform of science and technology by participating in "deliberative democracy",[21] in the ways we all learned in grammar-school government glass. Citizens, and especially professionals, engage in deliberative democracy by public speaking, popular writing, public-interest research, reporting, surveying, requesting government information, whistleblowing, boycotting, picketing, demonstrating, suing, using initiative and referendum, fundraising. They also can evaluate one of the many current draft environmental impact assessments (EIAs), technology assessments, and risk assessments and make their comments public. In the US, 2500 draft EIAs are written for public comment each year,[22] and presumably some lesser number also are written in nations like Spain. Today many of the most effective respondents to these draft EIAs are housewives and mothers, worried about their children. Educated scientists and philosophers ought to be able to do at least as much.

One person who shows the power of deliberative democracy, the power of attempts at reform, is Juana Gutierrez, a grandmother who lives in the poverty of East Los Angeles. The daughter of a Mexican farmer, Gutierrez came to the US when she was 15. Although her father told her not to get involved, she says she became a grassroots activist, because "I was worried about my kids." 18 different companies annually discharge more than 33 million pounds of toxic waste into

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[20] Cf. SHRADER-FRECHETTE, K., *Ethics of Scientific Research*, p. 30.

[21] Cf. YOUNG, I., *Inclusion and Democracy*, Oxford University Press, New York, 2000.

[22] Cf. ISAACS, K., *Civics for Democracy*, Essential Books, Washington DC, 1992.

the East LA air and water. The noxious chemicals explain why only poor Latinos, like Gutierrez, live in East LA. The median annual income is about one-third of the US average, and unemployment is 33 percent. Yet Gutierrez and other mothers and grandmothers were angry that, despite the disproportionate, life-threatening pollution in East LA, officials wanted to place an above-ground oil pipeline, another hazardous-waste storage site, and another toxic-waste incinerator in their neighborhood. The proposed incinerator was slated to burn 125,000 pounds of hazardous materials per day-including used motor oil and industrial sludge.[23]

In 1986 Gutierrez joined Aurora Castillo to co-found MELA, Mothers of East Los Angeles. To inform the Hispanic community about technological threats to the neighborhood, Gutierrez used her most available network: people streaming out of Sunday Mass. Through church leafleting, she and other Latina mothers advertised for protest marches, held every Monday. Pushing baby strollers and wearing white kerchiefs to symbolize nonviolence, MELA members became a formidable force.[24] Eventually the men began to help with the action. They carried signs calling themselves the “chauffeurs” of the mothers.

From their church, MELA protestors walked, every week, more than a mile to the gates of the $ 20 million incinerator project. As they marched, they chanted: “El pueblo parará el incinerador!” (The people will stop the incinerator!) “Pueblo que lucha triunfa!” (People who fight win.) The facility owners had sited it in East LA because they said residents would not fight. Yet Gutierrez and MELA fought –through 6 years of agitation, 4 lawsuits, 16 hearings, and 6 mile– long protests. Finally, in June 1991, the Mothers passed around cookies among their 400 members to celebrate cancellation of the incinerator. Soon after, MELA began a lead-poisoning education project that now employs 10 youths. Defying “a system that penalizes low-income communities,” Gutierrez and MELA have dispelled the myth that poor people do not care about technological threats to their health.[25]

Why was Gutierrez so successful? She recognized the importance of collective action. Duties to reform science, like duties to clean up the air in Gutierrez’s neighborhood, require massive cooperation and collective action. These obligations are not mainly owed by individuals to individuals, because individuals cannot act alone and be successful. Instead it is arguable that people have obligations to promote institutions and policies that aim for fair relations

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[23] Cf. Martinez, M., “Legacy of a Mother’s Dedication,” *Los Angeles Times*, Section B, (September 7, 1995), p. B3; see also p. B1.

[24] Cf. Schwab, J., *Deeper Shades of Green*, Random House, N. York, 1994, pp. 55-58. “Mothers’ Group Fights back in Los Angeles,” *New York Times*, Section A, (December 5, 1989), p. 32.

[25] Cf. Schwab, J., *Deeper Shades of Green*, pp. 44-45; Martinez, M., “Legacy of a Mother’s Dedication,” p. B1; “Mothers of Prevention,” *Time* 137, no. 23, (June 10, 1991), p. 25; Quintanilla, M., “The Earth Mother,” *Los Angeles Times*, Section E, (April 24, 1995), pp. E1, E5; and Martinez, M., “Legacy of a Mother’s Dedication,” p. B3. See Dellies, H., “Group Preaches Gospel of Water Conservation,” *Chicago Tribune*, Section 1, (March 20, 1995), p. 3. See also Gelobter, M., “Have Minorities Benefited? A Forum,” *EPA Journal*, v. 18, no. 1, (1992), pp. 32-36.

among people.[26] But people can always object that they, individually, bear little or no personal responsibility for collective problems like the practice and use of science. But as one Worldwatch researcher put it: "Everyone is aboard the same ship. The Plimsoll line carries the same meaning for all."[27] Although everyone contributes to planetary problems, no one –acting alone– can eliminate the most pressing civic and environmental harms. As a result, precise individual responsibilities are not clear.

Problems of collective responsibility are illustrated, in part, by the "tragedy of the commons." The tragedy is that each person enhances individual gain by misusing common resources, like scientific knowledge.[28] One individual may profit financially by driving a heavily polluting automobile or by keeping quiet in the face of misuse of science, but the tragedy occurs because everyone loses when someone misuses the commons, such as polluting the air we all breathe or allowing the misrepresentation of science. Because of the tragedy of the commons, people have a powerful incentive to be "free riders."[29] Free riders are those who gain benefits from everyone's contributing to collective goods, like clean air or scientific progress, even when they do not themselves contribute.

In the case of philosophers, scientists, and other professionals –all who enjoy special abilities, roles, and circumstances– the duty not to be a free rider and to practice collective responsibility is greater than that for people without such abilities and roles. This and the next paper argue that, provided people use the criteria for informed, inclusive, deliberative, and critical reform, outlined subsequently, their behavior will be more ethically defensible than would their neutrality. Also, given the power of vested interests, the world is like a giant soccer match, with one team representing the public interest, including science, and the other team representing private interests. Often the public-interest team has too few players. Often even government regulators and agencies are recruited to play on the team representing private interests. As a result, the public-interest team often has to run uphill to make a goal. Often the contest is not fair. Because the playing field of government, industry, and society often is tilted, and because particular individuals, working together, can help to make it level, all citizens share some collective responsibility to do so.

One difficulty with affirming duties to reform science, however, is that often these obligations are collective. Ultimate moral responsibility for advocacy

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[26] See notes 31-42.

[27] POSTEL, S., "Carrying Capacity: Earth's Bottom Line," in BROWN, L. ET AL. (eds), *State of the World 1994*, Norton, New York, 1994, p. 21.

[28] See HARDIN, G., "The Tragedy of the Commons," *Science*, v. 162, (1968), pp. 1243-1248; SWANSON, T. and SANTOPIETRO, G., "The Economics of Environmental Degradation: Tragedy for the Commons," *Journal of Economic Issues*, v. 32, no. 3, (1998), pp. 878-880.

[29] See SHRADER-FRECHETTE, K., *Environmental Ethics*, Boxwood Press, Pacific Grove (CA), 2n ed., 1991, pp. 165 and 185. See also CONDREAN, C., "Sidney Godolphin and the Free Rider," *Business and Professional Ethics Journal*, v. 17, no. 4 (1998), pp. 5-19; and STROUP, R. L., "Free Riders and Collective Action Revisited," *Independent Review*, v. 4, no. 4, (2000), pp. 285-300.

rarely belongs to a single person because an individual alone often is not causally responsible for avoiding threats to public welfare. One person usually cannot prevent misunderstanding of a dangerous pesticide or destruction of a wilderness.[30] How does one determine the level of each individual's "share" of collective responsibility for public-interest advocacy?

Rudyard Kipling's response to this question was that "the sin they do by two and two they must pay for one by one." But if one accepts Kipling's notion of collective responsibility, would all praise and all blame fall on everyone for all acts? Would no one be responsible, and would the concept of responsibility become vacuous?[31] On the one hand, people like H. D. Lewis think responsibility is like a pie, and the more pie-eaters there are, the less responsibility there is. His physicalistic view is analogous to the position that, the more people one loves, the less love there is to go around. On the other hand, Larry May, Michael Zimmerman, and others agree with Kipling. They argue that responsibility is not like a pie. It does not decrease by sharing. They say responsibility decreases only as a result of things like duress, incapacity to accomplish some end, or the honest belief that particular actions will do no good.

Larry May, however, argues that groups have responsibility and that each individual can evaluate her own responsibility by examining the group structures through which members are related to one another. Solidarity among members of a mob, for example, facilitates the joint action of the mob, according to May. Whenever the relational structures among members of a group help cause the actions and intentions of the members of the group, then one can attribute collective responsibility to the group.[32] Thus, in May's framework, there are at least two reasons for believing we have collective responsibility for reform of science. One is that we, as professors, are part of a relational structure, within society, that requires us to be public intellectuals, by virtue of our training, our roles, and our employment. The other reason is that, as experts in epistemology and ethics, we are responsible, as philosophers, for criticizing poor science, that is, poor epistemology, and poor ethics. More generally, simply as citizens and as humans, our interdependency creates a third reason, within May's "relational

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[30] See also LADD, J., "Philosophical Remarks on Professional Responsibility in Organizations," in BAUM, R. and FLORES, A. (eds.), *Ethical Problems in Engineering*, Center for the Study of the Human Dimensions of Science and Technology, Troy (NY), 1980, pp. 193-194. See MILLER, S., "Collective Responsibility," *Public Affairs Quarterly*, v. 15, no. 1, (2001), pp. 65-82.

[31] Cf. LEWIS, H., "The Non-Moral Notion of Collective Responsibility," in FRENCH, P. (ed), *Individual and Collective Responsibility*, Schenkman Books, Rochester, 1972, pp. 119-131.

[32] Cf. MAY, L., *The Morality of Groups*, University of Notre Dame Press, Notre Dame (IN), 1987, pp. 3-111; and ZIMMERMAN, M., "Sharing Responsibility," *American Philosophical Quarterly*, v. 22, (1985), pp. 115-122. For a typical questioning stance on collective responsibility, see RAIKKA, J., "On Disassociating Oneself from Collective Responsibility," *Social Theory and Practice*, v. 23, no. 1, (1997), pp. 93-108; and PAUL, E., MIKLLER, F., and PAUL, J., *The Welfare State*, Cambridge University Press, New York, 1997. See also WATSON, G., "Reasons and Responsibility," *Ethics*, v. 11, no. 2, (2001), pp. 374-94; and RESCHER, N., "Collective Responsibility," *Journal of Social Philosophy*, v. 29, no. 3, (1998), pp. 44-58.

structures," for reforming science. This third reason is that we all are members of communities, some of which are global, as the WTO so disastrously affirms. As Hannah Arendt put it, "this taking upon ourselves the consequences for things we are entirely innocent of, is the price we pay for the fact that we live our lives not by ourselves but...[within] a human community."[33] Her rationale for collective responsibility is that, "as citizens we must prevent wrong-doing since the world we all share, wrong-doer, wrong-sufferer and spectator, is at stake."[34] Gandhi echoed a similar theme. Community interconnectedness creates responsibility for other members of the community: "Whenever I live in a situation where others are in need... whether or not I am responsible for it, I have become a thief."[35]

Emphasizing that social groups enable individuals to do more harm and more good than they could otherwise do, Larry May says communities also create more responsibility for those whose lives are woven into the fabric of the group itself. Certainly, as public intellectuals, our lives are more woven into the fabric of society than the lives of those who are not public intellectuals. May argues, correctly, that the benefits of community membership accrue only at the cost of increased responsibility on the part of the members: group membership is a source of both benefits and responsibilities, and greater benefits are not possible without greater responsibilities. Group membership creates heightened moral duties, in part because groups often are able to transform individual values, and individuals often are able to transform group values. Psychological studies show, for example, that racism is in part the result of socialization and interaction within certain kinds of social groups. But if groups influence individuals, and individuals influence groups,[36] then individuals bear some responsibility for group actions and omissions. As Joel Feinberg warns:

> "No individual person can be blamed for not being a hero or a saint...but a whole people can be blamed for not producing a hero when the times require it, especially when the failure can be charged to some discernible element in the group's "way of life" that militates against heroism."[37]

Confronting the evils of Nazism, German and French philosophers, such as Jean-Paul Sartre, Karl Jaspers, and Hannah Arendt, also grounded collective responsibility in community and interdependence. Jaspers writes:

> "There exists a solidarity among men as human beings that makes each co-responsible for every wrong and every injustice in the world, especially for crimes committed in his presence or with his knowledge. If I fail to do

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[33] Quoted in May, L., *Sharing Responsibility*, The University of Chicago Press, Chicago, 1992, p. xi.

[34] Quoted in May, L., *Sharing Responsibility*, p. xi. See also Hart, J., "Hannah Arendt: The Care of the World and of the Self," in Drummond, J. (ed), *Phenomenological Approaches to Moral Philosophy,* Kluwer, Dordrecht, 2002.

[35] Cited in Goulet, D., *The Cruel Choice*, Atheneum, New York, 1971, p. 133.

[36] Cf. May, L., *Sharing Responsibility*, p. 152.

[37] Feinberg, J., *Doing and Deserving*, Princeton University Press, Princeton, 1970, p. 248.

> whatever I can to prevent them, I too am guilty. If I was present at the murder of others without risking my life to prevent it, I feel guilty in a way not adequately conceivable either legally, politically or morally."[38]

Jean-Paul Sartre echoes a similar theme. "If someone gives me this world with its injustices, it is not so that I may coolly contemplate them but so that I may animate them by my indignation, expose them and show their nature as injustices, that is, as abuses to be suppressed."[39]

In order to expose injustices for what they are, metaphysical guilt forces people to reassess who they are. To avoid moral guilt for great social harms people must sometimes change who they are. They must become more virtuous, more authentic, and create themselves in new ways. Authenticity consists, in part, of accepting responsibility for the harms committed by the group to which people belong. No matter how restricted people's options are, Sartre believes they can choose authenticity. At least they have the choice of what stance to adopt toward the injustices around them Jaspers says people choose in "individual solitude" to transform their approach to the world,[40] to transform their attitude, character, and perhaps their behavior. People cannot choose their parents or nationality, for example, but they can choose their attitudes toward them.

According to Jaspers' account of metaphysical guilt, people do not have responsibility merely for their conscious intentions and deliberations. Instead, as Aristotle noted, because they have partial control over their attitudes, virtues, and character, they also are responsible for who they are and become. One way for people to exercise control over their characters is to be sensitive to how their attitudes affect others. And attitudes toward science affect others in powerful ways, as contemporary public health and environmental problems reveal. As Aristotle notes: "While no one blames those who are ugly by nature, we blame those who are so owing to want of exercise and care."[41]

In short, following Aristotle and May, insofar as people share in the production of an attitudinal climate, May says they participate in some group that increases or decreases harm. Collective responsibility is not an important theme in contemporary postmodernism, which tends to be nihilistic. Nor is it an important theme in contemporary analytic philosophy, which tends to be

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[38] JASPERS, K., *The Question of German Guilt*, trans. E. Ashton, Capricorn Books, New York, 1961, p. 36.

[39] SARTRE, J. P., *What is Literature*, trans. Bernard Frechtman, Methuen, London, 1950, p. 45. See also FORREST, P., "Collective Responsibility and Restitution," *Philosophical Papers*, v. 27, no. 2, (1998), pp. 79-91.

[40] Cf. SARTRE, J. P., *Anti-Semite and Jew*, trans. George J. Becker, Schocken Books, New York, 1965, p. 90; MAY, L., *Sharing Responsibility*, pp. 146-151. See also JASPERS, K., *The Question of German Guilt*, p. 74; ADAMS, R., "Involuntary Sins," *Philosophical Review*, v. 94, (1985), pp. 3-27; and MANGIN, M., "Character and Well Being," *Philosophy and Social Criticism*, v. 26, no. 2 (2000), pp. 79-98.

[41] ARISTOTLE, *Nicomachean Ethics*, Edition by Terence Irwin, Hackett, Indianapolis (IN), 1985, Book III, Chapter 5.

quietest.[42] Contemporary virtue ethicists, more sensitive to the notion of collective responsibility, argue that traditional ethical thinkers often have placed too much emphasis on the will, on intention, and on a restricted notion of self control. They say traditional ethicists give too little emphasis to attitudes, character, habits-to more existential accounts of responsibility and control. Traditional deontological and utilitarian moral theorists tend to conceive of duties more narrowly, in part because they take less account of the communal and interdependent nature of human existence. They tend to view humans as isolated from each other, as alone in winning praise or blame. They tend to distinguish sharply commissions from omissions –being active versus being passive– in allowing a harmful act to occur.[43] However, James Rachels, Peter Singer, John Harris, and most of the virtue theorists argue against such distinctions. They argue that people have responsibilities to prevent harm wherever they can, regardless of whether they err through commission or omission, directly or indirectly, actively or passively.[44] Instead they show that failing to prevent harm may be as serious as performing it, because all acts occur within a web of human interdependence. Not choosing also is a choice for which people are responsible. They take to heart Edmund Burke's warning. All that is necessary for the triumph of evil is that good people do nothing.

For many of us, reform can be achieved, simply by doing good science and by acting as a watchdog on those who misuse science. Most of us are critical of the way corporate boards fail to act as watchdogs on corporate policies of corporations. We are critical of Enron and Parmalat. But if so, we also should be critical of the way scientists and ethicists fail to act as watchdogs on the misuse of science.

Juana Gutierrez took up the cause of reform, in part because she saw, first hand, injustice in her own Latino community. Many of us do not recognize the need for reform of science and technology because we are more insulated from their most serious threats. Often we are the whites who do not breathe the polluted air of black ghettos. Often we are the rich who do not risk our lives in the unsafe workplaces of the poor. Because we do not experience these abuses, first hand, we do not understand the need for reform.

5. The Case for Duties to Reform Science and Technology

Why is reform needed, and why are all professionals, especially scientists and philosophers, obliged to help reform science and technology? There are at

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[42] May, L., *Sharing Responsibility*, p. 3. For an exception, see Melleman, G., *Collective Responsibility*, Rodopi, Amsterdam, 1997.

[43] Cf. Stocker, M., "The Schizophrenia of Modern Moral Theories," *Journal of Philosophy*, v. 73, no. 14, (1976), pp. 453-466. See May, L., *Sharing Responsibility*, pp. 4 and 10.

[44] See, for example, Singer, P. (ed), *Applied Ethics*, Oxford University Press, Oxford, 1986, and De Ruyter, D., "The Virtue of Taking Responsibility," *Educational Philosophy and Theory*, v. 34, no. 1, (2002), pp. 25-35.

least 5 reasons: ability, complicity, consistency, professional codes of ethics, and self-interest. We have this special duty, first, because we are able to do what very few others can. Special abilities and special knowledge create special obligations. Scientists and professionals have been very effective, for example, in informing the public about nuclear technology. Ecologists have warned about dangers of genetically engineered microorganisms and the laxness of federal regulations for biotechnology.[45] Hundreds of famous entertainers have acted as public-interest advocates, as the 2001 case of three major British pop bands reveals. Pulp, Dodgy, and Ian Brown all refused to take part in the V2001 music festival because it was sponsored by Nestlé. The pop bands said Nestlé is violating the international code on the marketing of infant formula, and thus causing the needless deaths of many babies in developing nations. Steve Lowe, manager for Ian Brown, said something that more university professors should be saying: As fathers, we know that "only by raising public awareness" can we "pressure companies to act in a socially responsible manner."[46] Philosophers, in particular, have duties to speak out on issues that affect public welfare because they have expertise in logic, ethics, and argumentation-all of which is relevant in social conflicts. As Larry May argues, it is time that philosophers recognize the public price they must pay for the privileges they have successfully sought.[47]

Besides abilities, we also have special duties to help reform science and technology because we are complicit in much harm done by science. Consider the research on passive smoking. Even when one uses multiple logistic regression analyses controlling for article quality, peer review status, topic, year, and so on, the only factor associated with scientific conclusions, that passive smoking is not harmful, is whether an author is affiliated with the tobacco industry.[48] Those who are able to criticize such science, and who do not do so, arguably are complicit in the harm caused by passive smoking. The same is true of most manufacturer-funded scientific studies on pharmaceuticals. They claim their products are superior to others, but in at least half of all cases, the statistics are either missing or inconclusive. Yet peer-reviewed journals publish the pharmaceutical studies anyway.[49] And most professionals say nothing.

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[45] Cf. Colwell, R., "Natural and Unnatural History," in Shea, W. and Sitter, B. (eds), *Scientists and Their Responsibility*, Watson, Canton, 1989, p. 17; and Von Hippel, F. and Primack, J., "Public Interest Science," *Science*, v. 117, no. 4055, (1972), p. 1169.

[46] Yamek, G., "Pop Musicians Boycott Promotion," *British Medical Journal*, v. 322, no. 7280, (2001), p. 191. See also Exeter, P., "Campaigners for Breast Feeding Claim Partial Victory," *British Medical Journal*, v. 322, no. 7280, (2001), p. 191.

[47] Cf. May, L., *Sharing Responsibility*, pp. 142-145.

[48] Cf. Barnes, D. and Bero, L., "Why Review Articles on the Health Effects of Passive Smoking Reach Different Conclusions," *Journal of American Medicine Association*, v. 279, (1998), pp. 1566-1570.

[49] Cf. Rochon, P., Gurwitz, J., Simms, R., Fortin, P., Felson, D., Minaker, K., and Chalmers, T., "A Study of Manufacturer-supported Trials of Nonsteroidal Anti-inflammatory Drugs in the Treatment ofArthritis," *Archives of Internal Medicine*, v. 157, (1994), pp. 157-163.

A more obvious case of complicity concerns what we buy. Most people in developed nations have benefited from products and manufacturing technologies whose lower prices are the direct result of unethical behavior and misuse of science and technology. Companies such as Sony, Motorola, Ericsson, Nike, General Motors, Mercedes-Benz, Ann Taylor, and Dockers are able to sell their products at lower prices because their contractors' and subcontractors' employ "debt bondage" or indentured servitude. To secure work at the Motorola subcontractor in Taiwan, for example, Philippino workers pay a "labor broker" about $2,400. Once in Taiwan, the workers have to pay an additional $3,900 each to a Taiwan labor broker. Although their monthly salaries in Taiwan are each about $460 (or 5 times what they could have made in the Philippines for the same job), they never see the money. Debt repayment to the brokers, room, and board eat up the entire salary. Typically such immigrants work 12-16 hours per day, 7 days a week. They labor in unventilated, locked rooms, and they include children as young as 11. They sleep in locked, dirty, sometimes rat-infected small rooms. Workplace traffic in human beings is the fastest-growing criminal market in the world. Sweatshops flourish in Latin America, Asia, Africa, and even the EU. Illegal immigration to the EU is 500,000 a year, mostly from China, and illegals pay between $5,000 and $25,000 for passage, which they must pay off from their meager wages. Global profits are about $9 billion in human trafficking, and they exceed global drug profits. Yet no manufacturers have so far been prosecuted for using sweatshops staffed by illegal immigrants. In Italy, illegals fuel 70 percent of the underground economy, while western consumers reap the benefits.[50] One way to compensate for our complicity in such harm is to commit ourselves to engage in ongoing reform.

In addition to duties based on great need, on our abilities, and on our complicity in the misuse of science, consistency also dictates duties to help reform science. It would be irrational to train students to do science or philosophy and not to train them to monitor the democratic and epistemic conditions necessary for good science. To seek the end, good science, and not pursue the means (public education, whistleblowing, watchdogging) necessary to achieve it, is irrational.

We also have duties to help reform science because the people, the taxpayers, often employ professors. Professors therefore have special obligations to protect the common interest,[51] and thus obligations to reform whatever threatens the

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[50] See ARLACCHI, P., *Slaves: The New Traffic in Human Beings*, Rizzoli, Milan, 1999; EDMONDSON, G. ET. AL., "Workers in Bondage," *Business Week*, no. 3709, (2000), pp. 146-155; and STEIN, N., "No Way Out," *Fortune*, v. 147, no. 1, (2003), pp. 102-107. See also HAMMOND, K., "Leaked Audit: Nike Factory Violated Worker Laws," *Mother Jones Online magazine* (sponsored by: Foundation for National Progress, San Francisco, (CA) 1997) and accessed February 10, 2005 at *http://www.motherjones.com/about/index.html* and *http://www.motherjones.com/news/feature/1977/11/nike.html*. See also *Sweatshop Watch*, a coalition to eliminate sweatshops in the garment industry, at *http://www.sweatshopwatch.org/.*

[51] See ABBARNO, J., "Role Responsibility and Values," *Journal of Value Inquiry*, v. 27, no. 3/4, (1993), pp. 305-316; MAY, L., *Sharing Responsibility*, passim; and BAYLES, M., *Professional Ethics*, Wadsworth, Belmont, 1981, pp. 92-109. See also MARTIN, M., *Meaningful Work: Rethinking Professional Ethics*, Oxford University Press, New York, 2000.

public interest, such as misuse of science and technology. One of the main reasons for professionals' duties to society is citizens' related rights to free informed consent to decisions affecting their welfare. To help ensure this consent, professionals must communicate openly with the public, especially regarding science and technology.[52] If they do not, industrial and political leaders can "get away with" whatever they wish. Scholars and other professionals also have duties to help reform science-related institutions, of which they are a part, because their economic, political, and intellectual power helps control much of what happens in society.[53] Given a sophisticated, technocratic society; given professionals' special knowledge; and given their near-monopoly over their intellectual services, professionals' great power "enlarges the significance of sins of omission."[54]

Virtually all codes of professional ethics also recognize a responsibility for the common good.[55] The American Institute of Biological Sciences, or AIBS code, for example, requires biologists to expose fraud, professional misconduct, conflicts of interest and to promote open exchange.[56] And in most codes, public responsibilities receive the highest priority, in part because they are enjoined by role responsibilities.[57] Just as parents, medical doctors, teachers, and so on, have certain responsibilities in society, by virtue of their roles, so also professionals have special responsibilities because of their roles and their corresponding trusteeship duties to society.

Even apart from such roles, duties to protect society (by helping to reform science and technology) are part of the Good Samaritanism required of all citizens. In the 1800s, Portugal, the Netherlands, and Italy had laws requiring citizens to undertake the "easy rescue" of others. After 1900, Norway, Russia, Turkey, Denmark, Poland, Germany, Romania, France, Hungary, Czechoslovakia, Belgium, Switzerland, and Finland also added similar statutes. In striking contrast, says Joel Feinberg, English-speaking countries have remained apart from the European consensus. They have not punished even harmful omissions of an unethical kind. He believes the Europeans are right, that people ought to be

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[52] Cf. American Association for the Advancement of Science (AAAS), *Principles of Scientific Freedom and Responsibility*, Revised Draft, AAAS, Washington DC, 1980, pp. 1 and 6. Some of this discussion of professional responsibility is based on Shrader-Frechette, K., *Ethics of Scientific Research*, pp. 64-67.

[53] Cf. Bayles, M., *Professional Ethics*, p. 4. See Wueste, D. (ed), *Professional Ethics and Social Responsibility*, Rowman and Littlefield, Lanham, 1994.

[54] Camenisch, P., "On Being a Professional, Morally Speaking," in Baumrin, B. and Freedman, B. (eds), *Moral Responsibility and the Professions*, Haven Press, New York, 1982, p. 43.

[55] Cf. Bayles, M., *Professional Ethics*, pp. 94, 109; See American Society of Biological Chemistry (ASBC), *Bylaws*, American Society of Biological Chemistry, 1977; National Society of Professional Engineers (NSPE), "Criticism of Engineering in Products, Board of Ethical Review, Case No. 67.10," in Baum, R. and Flores, A. (eds.), *Ethical Problems in Engineering*, pp. 64-72; and Office of Government Ethics, *Standards of Ethical Conduct for Employees of the Executive Branch: Executive Order 12674-Principles of Ethical Conduct for Government Officers and Employees*, Internal Revenue Service, Washington, DC, 1993, p. 35042.

[56] Cf. Shrader-Frechette, K., *Ethics of Scientific Research*, pp. 42-44, 72 and 78-84.

[57] Cf. Shrader-Frechette, K., *Ethics of Scientific Research*, pp. 63-80.

required to be Good Samaritans, at least in cases posing no great risk or cost to them. At least, he says, such omissions may be manifestations of character flaws such as lack of sensitivity or benevolence.[58] If he is right, then omitting what we can easily do to reform science and technology, especially in life-threatening situations, is arguably unethical.

Apart from all these ethical arguments for helping reform science and technology, there is a prudential or self-interested argument.[59] US Public Health Service data show that in 1900, 1 in 27 people died of cancer; in 1910, 1 in 19. In 1920, 1 in 16 people died of cancer. In 1930, 1 in 12 people. In 1940, 1 in 9 people died of cancer. In 1950, 1 in 7. In 1965, 1 in 6 people died of cancer.[60] By 1982, the National Cancer Institute (NCI) warned that one in every three would die of cancer. A special 1996 report published by the US National Academy of Sciences warned that, shortly after the year 2000, cancer would be the leading cause of mortality.[61] The major culprits include tobacco, diet, and environmental carcinogens such as industrial chemicals.

6. Practical Steps of Reformers

If these ethical and prudential arguments for reforming science and technology are plausible, how might one begin to reform scientific research and teaching? The reclamation will need to start with the recognition that although science can be objective, it is also, as Kitcher put it, unavoidably social. The first step in reform is recognizing the need for it. But if so, scientists must learn to practice what Kitcher calls "well-ordered science," science subject to informed, pluralistic, democratic constraints.[62] Well-ordered science requires researchers and educators to look out for ways that vested interests tilt the scientific playing field.

If Karl Popper is right, well-ordered science can be accomplished through what he calls "intelligent piecemeal social engineering." Popper condemns *ideology* and *utopianism*, yet praises *reformism*. He writes:

Work for the elimination of concrete evils rather than for the realization of abstract goods. Do not aim at establishing happiness by political means. Rather aim at the elimination of concrete miseries… Choose what you consider the most urgent evil of the society in which you live, and try patiently to convince people that we can get rid of it.[63]

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[58] See Feinberg, J., "The Moral and Legal Responsibility of the Bad Samaritan," *Criminal Justice Ethics*, v. 3, no. 1, (1984), pp. 55-66.

[59] See Margolis, J., "On the Ethical Defense of Violence Destruction," in Held, V., Nielsen, K., and Parsons, C. (eds.), *Philosophy and Political Action*, Oxford University Press, N. York, 1972, p. 70.

[60] Cf. Lilienfeld, A., Levin, M. and Kessler, I., *Cancer in the United States*, Harvard University Press, Cambridge, 1972, pp. 2-3; see the next four endnotes and Epstein, S., *The Politics of Cancer Revisited*, Easts Ridge Press, Fremont Center, 1998.

[61] Cf. National Research Council (NRC), *Carcinogens and Anticarcinogens in the Human Diet*, National Academy Press, Washington, DC, 1996, p. 355.

[62] Cf. Kitcher, Ph. *Science, Truth, and Democracy*, Oxford University Press, New York, 2001.

[63] Popper, K., *Conjectures and Refutations*, Routledge and K. Paul, London, 1963 (3rd ed. revised, 1969), p. 361.

If the previous arguments are correct, one of the most urgent evils in society is that, through our misuse of science and technology, we are both killing people and misleading them about the causes of these fatalities. Particulates from fossil fuels, alone, cause hundreds of premature global deaths, mostly among the most vulnerable, like children. Yet those of us, who do not read government and medical reports, do not realize the consequences of our burning fossil fuels.

Reforming science and technology, however, does not require us to espouse environmentalism or any other "ism," but simply to become informed, to teach our students to become informed, to help level the playing field of science, to challenge those who cover up or manipulate scientific findings. Once deliberative democracy corrects these procedural injustices in the performance and use of science, and once people become active in creating the democracy they deserve, democratic procedure will destroy many substantive problems, such as pollution-induced deaths. Ideologies, of any sort, cannot alone make them disappear.

One way to help level the scientific playing field, to help reform science and technology, is to expose the poor science of researchers who are merely well-funded "front groups." The Global Climate Coalition, like the Advancement of Sound Science Coalition, is a front group funded by the oil, automobile, chemical, and tobacco industry to oppose signing the Kyoto Accords. Responsible Industry for a Sound Environment is a pesticide-industry-funded group writing to discredit right-to-know provisions in pesticide regulations. The Forest Protection Society is funded by the logging industry to promote rainforest logging. The Wetlands Coalition, funded by the oil and gas industry, has a logo that shows a duck flying over a wetland, but it lobbies and writes in favor of wetlands oil and gas drilling. In 1991, for example, Dow Chemical contributed to 10 anti-regulatory front groups, including the American Council on Science and Health and Citizens for a Sound Economy. Chevron, Exxon, Mobil, DuPont, Amoco, Ford, Philip Morris, Pfizer, Monsanto, and Proctor and Gamble all contribute millions each year to anti-regulatory, corporate front groups that pose as populist movements and that manipulate science to make their case. If Burger King said that Whoppers were nutritious and helped prevent heart attacks, the public might not listen. But if the American Council on Science and Health, an industry-funded front group, claimed its experts made this point, people might believe it, especially if they did not know who funds the council. Using scientific-sounding names, such front groups publish books and pamphlets arguing that pesticides do not cause cancer, that global warming is a myth, and that saccharin is not dangerous.[64]

Of course, when citizens challenge biased or profit-driven science, wealthy private interests sue them. Such harassing lawsuits have become so common that they have a name, "SLAPPs," "Strategic Lawsuits Against Public Participation." When two Londoners, Dave Morris and Helen Steel, distributed pamphlets arguing

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[64] See note 3.

that McDonalds burgers were unhealthy and contributed to rainforest destruction (because of land used for cattle grazing), McDonalds sued the citizens. The two Brits were forced to spend thousands of pounds defending themselves.[65] But if a significant number of us joined with them, in their information campaign, it would be impossible to sue us all. We could create a situation like that in Sweden, when Hitler came to round up the Jews. So many non-Jewish Swedes put red Stars of David on their foreheads, in solidarity with their Jewish countrymen, that the Nazi roundup became impossible.

Ideological and ignorant environmentalists, of course, also get their science wrong, not just MacDonald's or Dow Chemical. Ernest Sternglass' misuse of statistics, in arguing against nuclear power, is a case in point.[66] But even misguided environmentalists typically do not have the millions of dollars to get their flawed messages across. That is why the scientific bias of corporate groups tends, by comparison, to be more massive. Consider one of thousands of similar examples, that of Dr. Robert Watson, an atmospheric scientist who chaired a prestigious international panel assessing climate change. When Dr. Watson pushed to limit greenhouse emissions, Bush forced his replacement, as leader, with an economist.[67] Not to recognize, and blow the whistle, on the way political science can control biological science, is naive.

Most scientists and philosophers of science know enough to warn their students about alleged scientific information published by those who do not believe in evolution, but they are less wary of other material, like *Ecoscam* , published by St Martin's Press, whose author was paid by the industrially-funded Competitive Enterprise Institute to write it.[68] And the industrially funded Cato Institute explicitly pays scientists to discredit university-funded scientific research that challenges the safety of food additives, environmental carcinogens, pesticides, paints, and solvents.[69] Of the four most-cited, scientific think tanks, which include Cato, Heritage, and American Enterprise Institute, students need to know that none is typically identified as industry-supported, when their "hire education" articles appear.[70] If we would not teach science without a lab or field work, then we ought not teach ethics or philosophy of science without also teaching students how to protect and encourage what is necessary to avoid misuse of science in a democracy. Expecting to do good scientific or philosophical education, but ignoring how to do science or philosophy in a democracy, would be like expecting to run good experiments, but not feeding the lab animals.

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[65] Cf. Beder, S., *Global Spin: The Corporate Assault on Environmentalism*, pp. 63-74.

[66] Cf. Beder, S., *Ibidem*, p. 215.

[67] Slater, D., "The big book of Bush," *Sierra*, v. 87, (2002), pp. 37-47.

[68] Bailey, R., *Ecoscam*, St. Martin's Press, New York, 1994.

[69] Cf. Moore, C., "Rethinking the Think Tanks," *Sierra*, v. 87, (2002), pp. 56-59 and 73.

[70] Cf. Moore, C., "Rethinking the Think Tanks," pp. 56-59 and 73.

One way to teach and do philosophical, ethical, and scientific research, in a way that promotes ongoing reform of science and technology, is to do more than merely autopsies on dead scientific theories. Also do vivisection on existing theories, contemporary scientific controversies. Send government agencies and elected officials your comments on proposed science policies and on draft EIAs, technology assessments (TAs), and risk assessments (QRAs). Write science-related "op ed" pieces for local newspapers Review science-related books for the popular media. Along with helping students learn to read critically and to use only the best refereed journals, scientists and philosophers can help reform science and technology in many other ways:

1. Make one class assignment requiring assessment of some proposed, science-related, government legislation, and have students write (to officials and newspapers) about how to improve it or what is questionable about it.
2. Teach a project-based EIA, TA, and QRA course where each student evaluates a chosen EIA, TA, and QRA, and then publicizes scientific and ethical aspects of policy based on it.
3. Begin class with 5 minutes of exposing "hire education" that dominates science-such as biased corporate or environmental think tanks or research.
4. Have students do synopses of science-related articles from the popular press.
5. Give students extra credit for reading/reporting on ethically-related nonfiction by scientists like Paul Ehrlich, Richard Feynman, or Devra Davis.
6. Give students extra-credit for work with NGOs, e.g., the National Wildlife Association.
7. Use books like Katherine Isaacs' 1992 *Civics for Democracy*, to show students how to use their scientific and ethical education, in daily life, to help reform science and technology.

7. CONCLUSION

If we cannot count on politicians, legislators, corporations, NGOs, and courts to achieve balance and objectivity in doing, reporting, and using science, then those of us who know science, logic, and ethics must do so. Reforming science -that is, separating it from complete control by moneyed, private interests, is difficult only because so few of us take on the task of reform. Ralph Nader defined a real democracy as "a society where less and less courage and risk are needed of more and more people to spread justice."[71] If everyone does the work of democracy, then any one of us needs less courage to face the risks that democracy demands.

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[71] NADER, R., "Introduction," in ISAACS, K., *Civics for Democracy*, p. vi.

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III

The Relation between Science and Society

5. Progress and Social Impact in Design Sciences
6. Experiments, Instruments and Society: Radioisotopes in Biomedical Research

PROGRESS AND SOCIAL IMPACT IN DESIGN SCIENCES

Anna Estany

The idea of progress implies an evaluation based on something which we hope to achieve. In general, progress, when applied to the sciences, is believed to have occurred in the transition from a given theory A (paradigm, research program) to another theory B, when B is an improvement on A. The pure sciences (the research of nature by means of the experimental method in an attempt to satisfy our need for knowledge) are concerned with cognoscitive values which allow us to solve problems, come close to the truth, find more coherent systems, etc.

In the design sciences other elements apart from the cognoscitive ones play a part, and as a result indicators of scientific progress based on the standard model of pure science do not allow us the possibilities to evaluate progress adequately in these sciences. This is very important given both the growth of these disciplines throughout the twentieth century and the interrelationship between basic or pure sciences (physics, astronomy, chemistry, biology, psychology and sociology) and design sciences, the latter being the result of the overlap between science and the application of its findings to practical ends.

The social impact of design sciences originates from the importance of practical ends and as a result, from the intervention of factors external to science in the very notion of progress. The aim of this paper is to analyze indicators of progress in such sciences.

1. DESIGN SCIENCE

Design sciences are the result of the "scientification" and mechanization of the arts, in relation to skills and practical activities. Herbert Simon in *The Sciences of the Artificial* (1969) points out that the traditional model of science gives a misleading picture of fields such as engineering, medicine, business, architecture, painting, planning, economics, education, etc. which are concerned with "design," understood as objective, proposal or aim to be achieved, that is to say, not with how things "are," but with "how things 'ought to be' in order to attain specific goals.

Engineers are not the only professional designers. The intellectual activity involved in producing material artifacts is not really that different from that of prescribing a cure for a patient, or that of drawing up a program of a new sales plan for a company, or that of a social welfare program. Constructed in this way, design is the nucleus of professional training; it is the main characteristic which distinguishes the professions from the sciences. The schools of engineering as well as the schools of law, architecture, education, medicine, etc. revolve around the process of design.

Simon maintains that not only is the science of design possible but that it emerged in the mid-seventies. (In 1975 The Carnegie Mellon University founded the "Design Research Center," whose name was changed to "Engineering Design Research Center" in 1985.) As a result, a substantial body of both theoretical and empirical knowledge, which deals with the components of the theory of design and their interrelationship, exists today.

Ilkka Niiniluoto has taken up Simon's idea in his analysis of the objectives and structure of design sciences.[1] He makes a distinction between descriptive sciences (which describe how the world is), design sciences (which transform the world) and technology (which constructs artifacts). The structure of formulations is one of the elements which distinguishes one science from another. In the descriptive sciences the formulations take the form of "A causes B" or, in the case of stochastic systems "A causes B with the probability of p." In the design sciences the formulations take the form of "If you want to achieve A and you are at B you have to perform C," that is to say, the formulations are practical norms also known as "praxiological statements."

2. The Contribution of Praxiology: T. Kotarbinski

Given the structure of the formulations in design sciences, the contribution of praxiology, the science of efficient action is important. The task of praxiology is to investigate the conditions upon which the maximization of efficiency depends.

As we have already seen, the scheme of a practical norm is as follows: "Under circumstances A it is necessary (or it is advisable, or it suffices) to perform B in order to cause C." All practical norms are praxiological statements since they are recommendations tending to increase the efficiency of actions.

Kotarbinski points out that there are three elements in a practical norm: the theoretical foundation, the technical base and the organization of operations. By the theoretical foundation of a simple practical norm we mean the causal dependence of C on B. If by performing B you do not achieve C then a change in the theoretical element needs to occur, for example, element B is replaced by element D in the composition of a medicine. As a result, a much more effective medicine is produced. No changes in the structure of the instruments nor in the operations were necessary and yet the recommended use of D in the production of the medicine in question was better than the norm that recommended the use of B to achieve the same end. Progress is understood here in terms of the use of a different chemical relation from that which has been previously used.

By the technical foundation we mean the instruments (in the most general sense of the term) which make it possible to achieve the desired end. In the case where the end is not achieved, a new norm based on a new instrument or artifact

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[1] Cf. Niiniluoto, I., "The Aim and Structure of Applied Research," *Erkenntnis*, v. 38, (1993), pp. 1-21.

through technological innovation may appear. For example, a narrow tube is replaced by a wider one so that the container may be filled up and emptied, using the same manipulation of taps and without recurring to a new mechanical relation, and this is achieved in less time than was previously the case.

By the organizational foundation we mean the selection and combination of actions brought about by agents. Here a new norm consists of a new choice and order of operations different from those that have been used in the past and which design a new kind of recommended action for this new practice, for example, a norm for playing a musical instrument, indicating which movements should be made and in what order, in accordance with the fixed relationship between events, so as to achieve a clear sequence of sounds in the most effective way possible. Here as well the norms which recommend the order and speed at which the members of a group should play need to be determined so as the whole can work uniformly and without interruption and that the former operations prepare the way for the latter ones.

Cognoscitive progress in science depends on the theoretical base; technological advance depends on the technical base and this in turn depends on those disciplines which are most directly linked to the artifacts; and changes in performance, organization, agents and as a result, theories of actions depend on the organizational base. How does the social factor manifest itself in these three elements? In the first it may play a part in the selection of prioritized lines of research. Even today different lines of research may develop within the research of pure sciences and there is no doubt that a particular field of research which forms part of (or may form part of) the theoretical base is a point in its favor when it comes to prioritizing and granting financial support. In the second element the social factor is of greater and more direct importance. Many possible technological strategies are possible in order to satisfy a need. It is clear that technology is not the answer to everything but, in most cases, there are various ways of solving a problem with benefits and harmful effects for distinct social groups. Therefore, both technical possibilities and prevailing sociopolitical and ethical values converge in this element. In the third element the social factor is the main one and that which determines progress. Everything revolves around human action (individual and collective), which means that the part played by social agents is crucial in this third element.

3. Models of Engineering Methodology

The scientific method is a key element in scientific research. The question is whether the methodology of design sciences should take the standard scientific method as it is or whether it needs to be reformulated so as to adapt it to the specific needs of the design process.

It should be pointed out that while the majority of methodological models of design refer to engineering design, all authors either explicitly or implicitly accept that this methodology is applicable to the design of pharmaceutical products or educational systems. This has lead many authors to use the term "technological sciences" rather than "design sciences." It may be said that engineering has played the same role in design methodology as physics played in the philosophy of science, as can be seen in the scientific models devised by the Vienna Circle, positivism and logical empiricism. Although a clear reference to the particular nature of engineering disciplines can be detected in methodological models, this does not mean that such models should be rejected. Rather some restructuring is necessary as occurred in the case of models of science originating from logical empiricism when the philosophy of biology, psychology and social sciences emerged.

Different methodological models of design exist as we can see in the case of M. Asimov, G. Nadler and A. D. Hall. The differences between these models reveal an emphasis on different themes rather than opposed theses on the part of their authors. I am going to take McCrory's model as an example of a design method,[2] given that his thesis is particularly representative and moreover draws a comparison between his model and the standard scientific model,[3] revealing similarities and differences between both models.

Using the above two schemes we may draw the following conclusions:

1. The aim of design is not to originate scientific knowledge but is rather to use it in order to create something useful. The designer might be compared to the artist in so far as he or she does not create the colors and the forms, but combines them to generate new creations, which sometimes lead to works of art.
2. Figure 1 and 2 are outlines of idealized versions of the scientific method and the design method respectively. In the same way as the scientific method is inherent in scientific research, the design method is inherent in the design process.
3. In contrast to basic scientific research, which is driven by curiosity, design is driven by need.
4. The social factor plays an important role in non –technical areas such as the economy, society and geopolitics, to which we may add questions of a cultural or ethical nature– in short, all those factors outside the realm of science. The social factor contains within it a recognition of need however, it does not simply limit itself to this rather it is present in the entire process, especially in phase 4, in which production and the market come into play. We may be lead to think that the market is guaranteed once the need has been recognized however, many other factors are involved, for example the price, trends, cultural habits, etc.

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[2] Cf. McCrory, R. J., "The Design Method-A Scientific Approach to Valid Design," in Rapp, F. (ed.), *Contributions to a Philosophy of Technology*, Reidel, Dordrecht, 1974, pp. 158-173.

[3] See figures 1 and 2

Figure 1. Graphic representation of scientific method

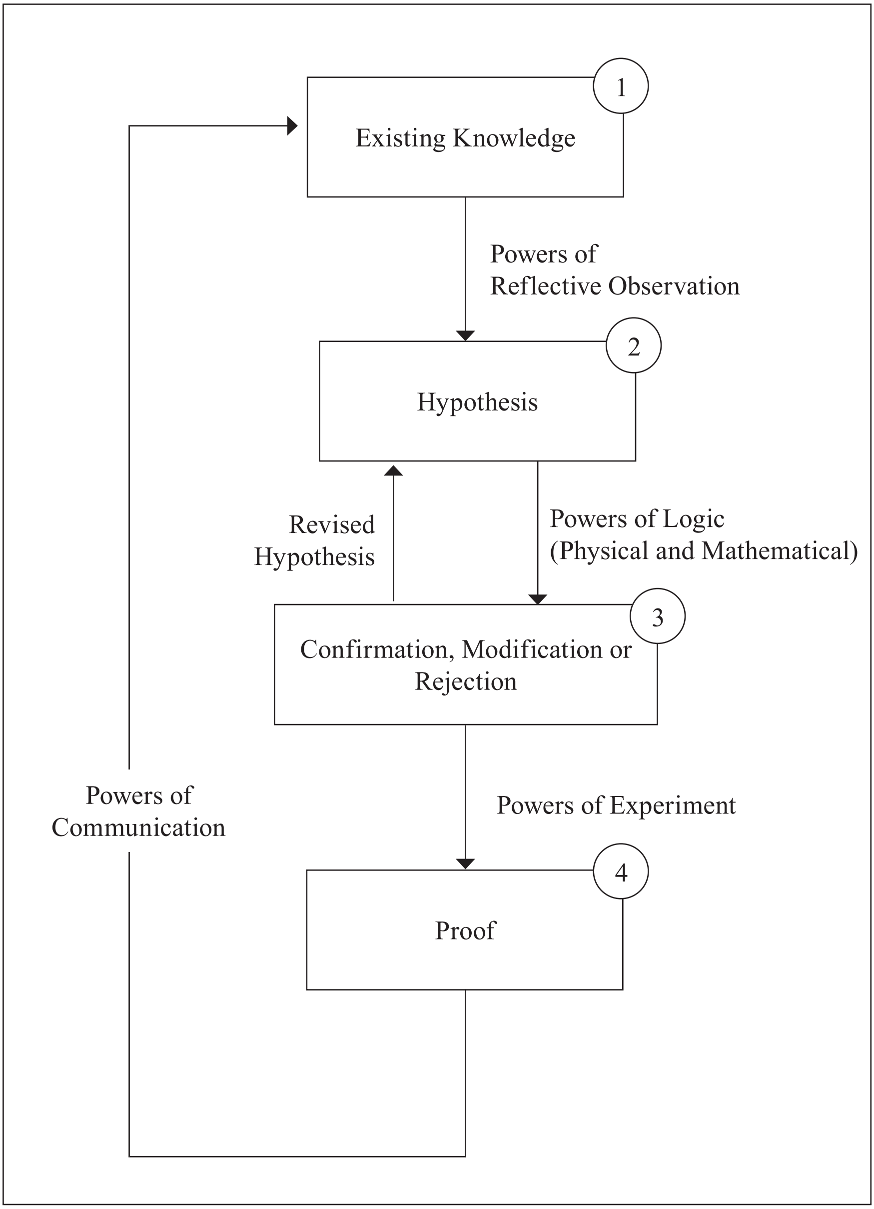
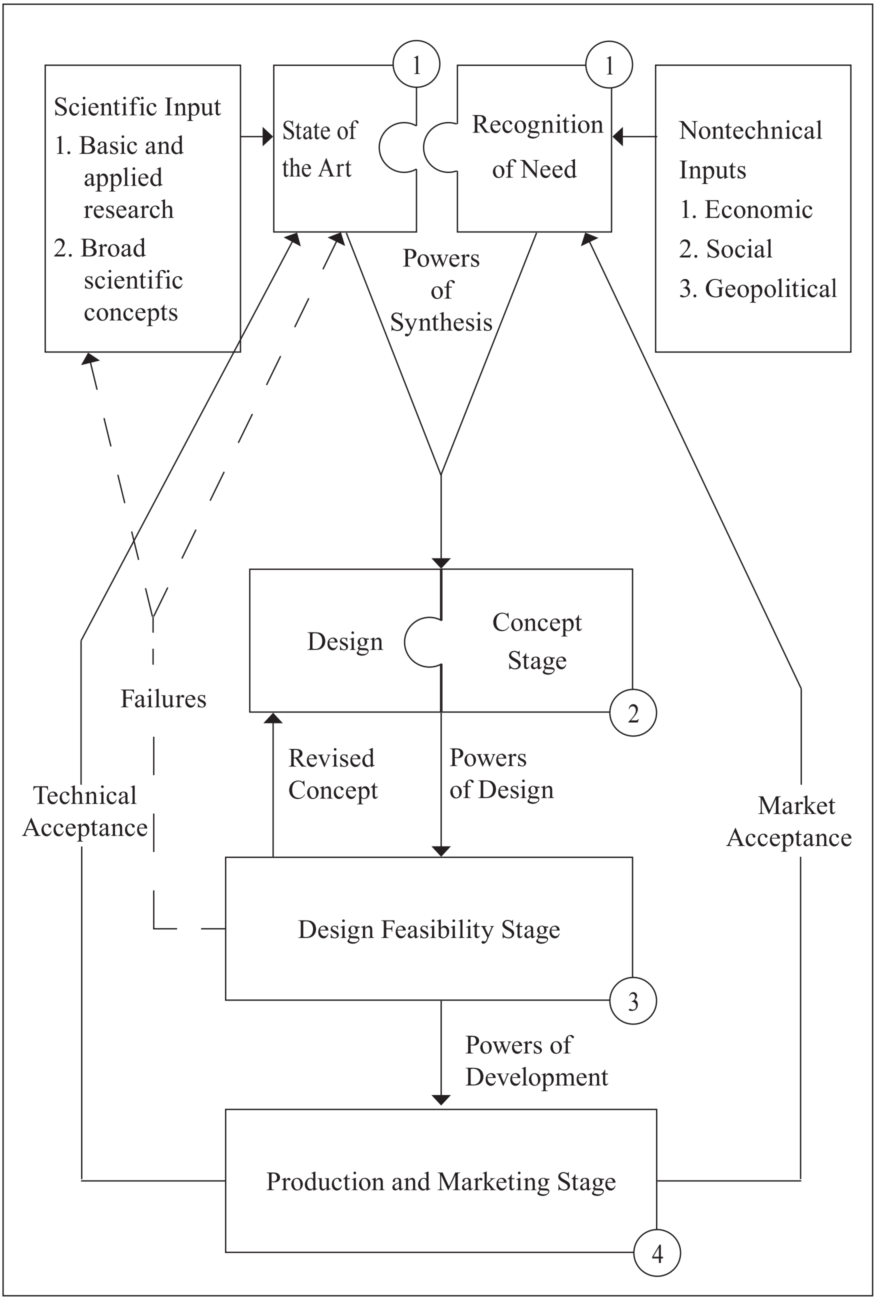


Figure 2. Graphic representation of design method



5. Besides the non-technical question, there is also the scientific question which conditions design and viability (Phases 2 and 3). Consequently, it cannot be concluded from the social impact of design sciences (the social being an integral part of these sciences) that design sciences can do without the contribution of pure sciences.
6. The social factor is a pillar of design sciences. Knowledge and need are inextricably linked. This means that design sciences depend on both and cannot do without either and, although this may appear to be a truism, it is not the case if we consider some of the sociological currents in the last few decades, which regard science (both pure and design) as a social construct.

McCrory's theories on design methodology may be summarized as follows: "The purpose of this discussion is to identify and describe a methodology which should be inherent in all design programs but which is seldom recognized and consciously utilized. Failures arising from attempts to avoid or shortcut the stages of the design process can be reduced if the controls and guidelines of the design method are followed. In practice, the design process as exemplified by the design method is often distorted, especially in the critical early stages. Need is seldom adequately defined and the socio-economic factors are often miscalculated. The route by which the designer may tap the state of technical art is long and hazardous, and the eagerness to accept and invest heavily in initial ideas sometimes causes designers to overlook the importance of the synthesis stage."[4]

4. REVOLUTIONS IN DESIGN SCIENCES

A lot has been written about scientific revolutions, however the question of whether the revolution occurred in pure or design sciences has not been dealt with. Nevertheless, philosophers have always referred to historical examples in relation to pure or descriptive sciences.

Most analyses of technological change or technological revolutions have fundamentally focused on invention (the conjunction of diverse technical elements which result in the emergence of a new technology) and innovation (the application of technology to a new field), at times making reference to the adoption and acceptance of the new practice on the part of the user.

If, however, we believe that the technical element is just one of the elements of practical norms and that any theoretical model of design sciences must cover not only engineering but also other scientific fields such as medicine, education, library science, etc. then we cannot equate revolutions in design sciences with technological revolutions (unless we refer to technology in its most general sense, for example, calling science education technological for the simple fact that they involve using computers).

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[4] McCRORY, R. J., "The Design Method-A Scientific Approach to Valid Design," p. 172.

I would like to make reference to the analysis of this problematic question by D. Wojick,[5] on the one hand, and by J. Echeverría in *La revolución tecnocientífica* (*The Technoscientific Revolution*),[6] on the other. Wojick's analysis centers on the processes through which new technology is accepted by the engineering community. Here the human factor is conceived in terms of the acceptance of innovations or revolutions by the engineering community, that is, the medical, educational fields, etc. rather than in terms of the consequences of technology for society. The idea here is that a technological revolution implies a change in the scale of values and as a result in the systems of evaluation.

Evaluation criteria are key concepts in the application of technology and constitute the nucleus of practical reasoning. The analysis of cost-benefit is a paradigmatic example of this type of practical evaluation, but the net value of a solution cannot however be reduced simply to its monetary value since social, aesthetic factors as well as other considerations such as utility, efficiency and safety need to be taken into account. Therefore, the procedures in any technological area taken as a whole imply the use of techniques from different fields. The evaluation of a food additive, for example, may require elements taken from fields outside the food industry such as medicine, chemistry, biology, industrial engineering, etc.

This leads to the question of how the systems of evaluation may be changed. In his analysis of these changes, Wojick draws on Kuhn´s analysis of scientific revolutions and applies this to changes in technological practices. In summary, the accepted system of evaluation plays a role in the organization of the use of technology in the same way as the scientific paradigm does in the organization of a scientific explanation. Wojick uses revolutions in the management of water resources, better nutrition and perception of nuclear energy as examples.

J. Echeverría analyzes technoscientific revolutions and puts forward a series of hypotheses which may be summarized as follows: megascience or technoscience came about in the twentieth century, above all after the Second World War; technoscience is a basic component of the information society; the technoscientific revolution is dealt with by "studies of technoscience," characterized by a profound transformation in cross-disciplinary studies of science and technology; the philosophy of science and technology should be based on the philosophical analysis of technoscientific activity rather than on scientific knowledge or technological artifacts; another question, and one which is perhaps even more important than the traditional philosophical problem of how to justify scientific knowledge, is the valorization of scientific practice; and finally the values of

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[5] Cf. Wojick, D., "Philosophy of Technology and the Structure of Technological Revolutions," in Bugliarello, G. and Doner, D. B. (eds.), *The History and Philosophy of Technology*, University of Illinois Press, Champaign (IL), 1979, pp. 238-261.

[6] Echeverria, J., *La revolución tecnocientífica*, FCE, Madrid, 2003.

technoscience is one of the areas in which the technoscientific revolution has a greater impact.[7]

If the evaluation systems are similar to paradigms, then a radical change in those systems would be similar to a change in scientific theory. Moreover, in the same way that a scientific theory is more than a combination of laws and equations, the technical engineering base is more than a combination of equations to be solved. Such techniques are included in the system of accepted procedures used to carry out a task correctly and these procedures, in turn, are included in a specific system of evaluation, that is, in a system of criteria used to determine what factors are relevant, how to measure them and how they may be evaluated. The aim of an evaluation system is to guide us so to transform and improve on the present situation to the best of our ability. The value to the evaluation system might be compared to that of truth to science.

Wojick, in keeping with Kuhn's thesis, points out that anomalies arise when the system of evaluation does not result in an improved state.[8] In the same way that failure of a paradigm to explain a phenomenon results in an anomaly so too does repeated failure of standard procedures to cure specific diseases result in an anomaly in the accepted system of evaluation used up to that point. Another type of anomaly arises when new scientific and technical knowledge make it clear that the procedures we use do not evaluate correctly specific factors. Examples of this kind of anomaly are to be found in the discovery of the long term effects of pesticides due to advances in biochemistry, the discovery of side effects of food additives due to advances in medicine or the discovery of adverse ecological effects of reservoirs due to advances in ecology. This type of anomaly may be difficult to detect as the knowledge which brings the anomaly to light may exist in another field, for example, engineers were unable to detect the anomaly until the mid-sixties because they did not have access to the necessary ecological findings. Moreover, in the same way that paradigms cannot be measured, so too may we find systems of evaluation that arise from points of view which cannot be measured.

All of this leads us to the need to reformulate the idea of progress. If we say that in descriptive sciences "a theory T1 is better than another theory T2, then we may say that in design sciences "a given design D1 is better than another design D2" and that moreover, it is better because "D1 better fulfills the recognized needs than D2" or because "D1 better fulfills the functions attributed to it than D2." In reply to the question, "What are the factors that make D1 fulfill the needs better than D2?," Kortarbinski provides us with an answer in his identification of the three elements which are involved in a practical norm: theoretical knowledge, technical factors and the conduct of individuals and groups.

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[7] Echeverria, J., *La revolución tecnocientífica*, pp. 175-181.

[8] Cf. Wojick, D., "Philosophy of Technology and the Structure of Technological Revolutions," p. 244.

There is a final question which should not be ignored and which is the rational basis of evaluation systems. Wojick compares evaluation systems to Kuhn's paradigms and also considers them to be incommensurable although it should be pointed out that it is not a good idea to consider them incommensurable, at least in terms of Kuhn's primitive meaning of paradigms. In fact, if we heed Wojick's comments, it does not follow that evaluation systems are incommensurable. Wojick makes reference to the knowledge afforded by ecology, that is, the environmental cost, as the reason for calling into question the construction of reservoirs. It is true that these environmental costs were not recognized by previous generations however, it may be said that this was so because the harmful consequences were unknown. The same may be said of food additives. This does not mean that there are universal spatial-temporal values however, neither does it mean that there are values which transcend systems of technological evaluation specific to a design science.

Echeverría advocates axiological pluralism and points to different kinds of values (basic, epistemological, technological, economic, military, political, legal, social, ecological, religious, aesthetic and moral) which play a role in technoscientific activity and which distinguish it from science of other historical periods. The influence exercised by all these factors is undeniable. The question lies in the weight of these factors in the whole of the scientific practice on the one hand, and in the newness of the phenomenon, that is, if it is really new or if it is simply a question of degree, on the other.

5. PROGRESS IN MEDICINE: THE CASE OF CANCER RESEARCH

Three elements of practical norms converge in cancer research and thus provide us with a good example of how to demonstrate progress in science and the interdisciplinary study of science. Its suitability is partly due to the study carried out by David Casacuberta and Anna Estany on the discovery of the "phenotype mutant" made by Manuel Perucho.[9] His discovery has made it possible to classify tumors according to their degree of genetic instability and specifically their microsatellites as well as to establish a series of relationships with other characteristics of value to the prognosis of patients. As Félix Bonilla Velasco (of the Medical Oncology Service in the University Hospital of Puerta de Hierro in Madrid) pointed out in the launch of Casacuberta's and Estany's book of 2003, the scientific contribution made by Manuel Perucho at the beginning of the 1990s has special importance for applied and clinical oncology.

In addition to the importance of the discovery and its relevance to the philosophy of science, we may add the special circumstances surrounding its publication, which turned into an authentic ordeal in that it was blocked by different branches

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[9] Cf. ESTANY, A. and CASACUBERTA, D., *¿EUREKA? El trasfondo de un descubrimiento sobre el cáncer y la Genética Molecular,* Tusquets, Barcelona, 2003.

of the scientific community from colleagues to journals representing the highest level of prestige in molecular biology. The difficulties encountered by Perucho and his team reveal the external face of science and the intervention of social, political and competitive factors in scientific research.

Perucho and his team's article entitled "Ubiquitous Somatic Mutations in Simple repeated Sequences reveal New Mechanisms for Colon Carcinogens" was published in *Nature* on 10 June, 1993. The history of the article, however, goes back to a conference held in Madrid in 1992 to celebrate the tenth anniversary of the discovery of oncogenes. Perucho presented his results at the conference for the first time and at the end of his presentation one of those present, Bert Vogelstein, asked for more details about Perucho's work to which Perucho fully complied. When Perucho finished his article he sent it to the prestigious *PNAS* (Proceedings of the National Academy of Sciences), which rejected it. Then he sent it to *Cell*, which also rejected it and finally to *Nature*, which accepted it after various revisions. However, the point is that while the paper was being revised at *Nature*, Vogelstein published similar results in *Science* (See outline 3). Perucho then began a long correspondence with the editor of *Nature*, John Maddox, who finally recognized that there were irregularities in the process and published an article entitled "Competition and the death of Science" in the editorial page of *Nature* on 25 June, 1993, in which Perucho's reputation was re-established and abusive practices which undermine progress in science were criticized.

The publication process is good proof of the existence of external factors which delay progress in science when it comes to publishing a discovery. Both non-specialized publications and scientific journals have voiced concern over the existence of different practices between prestigious and less prestigious laboratories and differences between the knowledge and language of the researcher. Articles coming from prestigious laboratories are subject to a much more stringent publication process than those coming from less well-known laboratories. Equally, it has been pointed out by the national press since the problems encountered by Perucho to publish his article in Nature that American and British researchers find it much easier to publish than their Spanish counterparts. It is probably not so much a question of whether one is American or British so much as whether one comes from a prestigious university or laboratory or not. The question of language is nonetheless very important. It is hardly surprising that American and British researchers have an advantage over Spanish researchers given that the international language of science is English, a language over which they have complete control, and that their works are easier to revise than that of a foreigner whose language is more limited and whose work often contains errors. However, in the case of Perucho it was not so much a case of institutional or linguistic difference as bias and abuse of position.

The speed at which Vogelstein was able to publish, in just under a week, can only be explained by surmising that he had direct contacts within the editorial team who facilitated publication, thus allowing him to publish his article with unusual speed. It would seem that for the great majority of scientists publication is a long and arduous process whereas for others the process might be compared to a rally driver on an unusually quiet four-lane motorway. Moreover, we are not just talking about favor; we are also talking about deliberate efforts to impede publication. Not content with accelerating the publication process, Vogelstein had both a direct and indirect part to play in the delay in publication of Perucho's article. Whether this was due to an explicit desire to block the article so as to gain time to compile his own results or whether it was due to a critical attitude on the part of Vogelstein towards a colleague, which in turn might be due either consciously or unconsciously to a competitive and territorial attitude, nevertheless questionable, is unclear as we do not have access to objective data.

The fact that such factors exist is a clear obstacle to progress within the scientific community. In science, where publication is the only means by which to climb the ladder, any control of access to publication and blocking of other researchers' publications converts it into an inherently unjust system, in which nationality or institution affiliation are only accentuated.

Above all, the existence of such impediments holds back progress in science. If a key article for the development of cancer research such as that of Perucho is in limbo for one year while another researcher takes all the credit then it is not just the researcher who is compromised but also the whole scientific community. The role of the mutant phenotype was present in literature but nobody was working on it. However, cancer research changed radically with the publication of Perucho's and Vogelstein's articles and in just a matter of weeks new articles, developing upon the above researchers' ideas, started to appear. Now these ideas are applied in all kinds of research, including practical applications of the discovery such as how to identify genes which have a predisposition to developing certain forms of cancer. Approximately a thousand articles have been published to date on mutant genes and genetic instability since the groundbreaking publication in 1993. This fact confirms the originality of the discovery, which has lead to the emergence of a new field with its own terminology (mutant genes, instability of microsatellites, etc.), and the enormous impact on cancer research in terms of the implications for the basic underlying mechanisms of certain tumors.

Here the idea of a deontological code of practice is fundamental, one which regulates the internal workings of scientific communities so as to eradicate falsification of data, plagiarism, etc. Many of the problems encountered by Perucho could have been avoided if the code of practice had been complied with.

6. Tension between Pure Sciences and Design Sciences in Cancer Research

The consequences of results in relation to tumors for millions of people around the world manifest the importance of scientific debate and highlight the central relationship between pure sciences and design sciences, in which external factors are brought to light in the debate over the place of cancer research. A study carried out by a group of German scientists and philosophers in the 1970s and 1980s on the relationship between basic and medical research in cancer research in Germany is of particular relevance to this debate. Hohlfeld interviewed 29 scientists working in the fields of molecular biology (basic research), experimental cancer research, clinical cancer research and cancer and epidemiological medicine from 1975 to 1976. The study highlights conflicts between theoretical, experimental and practical traditions in scientific research.

Basic research scientists, in this case molecular biology, hold that science cannot be driven by political aims. One scientist interviewed said:

"Basic research, in particular cell biology, must generate the necessary knowledge before any real breakthrough can be expected to occur. Impatience, no matter how justified and understandable on the part of millions of cancer patients, should not make either scientific organizations or politicians adopt measures which ultimately swallow up vast amounts of money without bringing any real success."[10]

This way of thinking reduces the problem of cancer to key events in biological processes which must be explained by molecular theories. As a consequence, progress here is conceived in terms of strictly epistemological values and not in terms of the cure or otherwise of cancer sufferers.

The first step towards the application of theories is "experimental cancer research," which is a type of research located behind the frontiers of "true" science and structured by still unsolved fundamental theoretical questions. It is not determined by the internal dynamics of scientific advance but by goal orientation or the solution of certain problems. Scientists working in this field share with basic scientists the idea that health problems must be solved by scientific instruments on the basis of clarification of underlying biological mechanisms and that this in turn requires "high technology." They also share the idea that experts are needed in the field. The fundamental difference between them is that experimental cancer researchers, unlike basic scientists, are motivated by the goal that their research must benefit humanity. Experimental cancer researchers expect to find a cure for the disease although they also explain strictly biological processes.

When intrinsic scientific motivation combines with external goal orientation in a given field in which there are as yet unresolved theoretical questions this results in a kind of research known as applied research. Scientists doing this type of research try to apply the results of basic research to the clinical. This would be the case of cancer researchers whose loyalties are split between molecular biology

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[10] Hohlfeld, R., "Two Scientific Establishments which Shape the Pattern of Cancer Research in Germany: Basic Science and Medicine", in Elias, N., Martins, H. and Whitley, R. (eds.), *Scientific Establishments and Hierarchies. Sociology of Sciences*, vol. VI, Reidel, Dordrecht, 1982, p. 151.

and a clinical orientation. One of those interviewed said: "Cancer research is almost an invention. The cancer researcher is situated between two worlds. On the one hand, you have the clinical world and on the other, the prestigious basic research world, for example, the Max Planck Society. The people engaged in basic research claim that cancer research does not have a solid base, that such concern for cancer seriously limits basic research."[11]

The idea of design science fits with the characteristics of applied science. In this sense, pharmacology too shares some characteristics of applied science in that it includes responsibility for strategic planning of biomedical research with the clear aim of dealing with practical problems. One of the scientists interviewed described the rules of pharmacological research in the following way: "1. The scientist cannot publish the results of his work whenever he wants. 2. If his work evolves into an independent research problem which deviates from the given research goal, he must abandon it. 3. The work he does requires considerable flexibility and does not allow him to become a specialist in any particular field. 4. There is a lot of routine work. Forsaking scientific reputation is compensated by external recognition for his work and by financial reward."[12]

The above illustrates the possible difficulties encountered by design science researchers who are at the mercy of constrictions imposed by the industry. This leads us to a reflection on the complexity of clinical cancer research which is based on therapeutic schemes that are evaluated in clinical studies. This would imply applying the results obtained in animals to patients, although bridges would need to be constructed between molecular biologists and medical scientists in order to make these models relevant. Yet the training of these two is very different, and looking at Hohlfeld's findings from interviews conducted with the different scientists, it would seem that collaboration between basic and applied research scientists is not easy. First of all, doctors believe that molecular biology has not made any real advance nor even contribution to medical progress in the fight against cancer. Secondly, the medical model, theoretically adopts the model of empirical sciences, physics, chemistry, etc. while at the same time decontextualizing the sick individual. Thirdly, different medical specialists sometimes disagree about the treatment of cancer patients. Finally, doctors tend to distrust basic research scientists, as is confirmed by one of those doctors interviewed by Hohlfeld: "We cannot leave patients in the hands of the scientists... We won't be reduced to mere agents of scientists."

Finally, there are the epidemiological studies in the broad field of cancer research. The object of this type of research is man not rats. Their goal is to study what makes a healthy person sick, which is different from the doctor who studies

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[11] HOHLFELD, R., "Two Scientific Establishments which Shape the Pattern of Cancer Research in Germany: Basic Science and Medicine," p. 153.

[12] "Two Scientific Establishments which Shape the Pattern of Cancer Research in Germany: Basic Science and Medicine," p. 155.

people who are already sick. Difficulties arise in this field primarily because of the political and social consequences of the results of this type of study.

We might venture to say that the difficulties and tensions within cancer research mainly arise from the fact that different sciences, both pure and design, are involved. Amongst the design sciences involved in the treatment of cancer are medicine, pharmacology and nursing. Design consists of drugs, radiotherapy, chemotherapy and surgery. The theoretical base is made up by molecular biology, chemistry and physics. The technical base consists of scanning techniques, that is, the apparatus used to carry out analyses and nuclear medical processes. The organizational base consists of health politics, organization of health systems, new surgical methods and patient care, etc.

All of the above considerations enable us to identify the characteristics of progress in the different disciplines involved in cancer research. In keeping with Kotarbinski's ideas, advance in the cure of this disease will depend on advances in molecular biology, instruments and techniques employed such as scanners, chemotherapy, the health system which includes prevention programs for breast and skin cancer as well as health education in schools, etc.

At present many cancer research scientists are asking themselves the following questions: Why do we have the feeling that there have been more advances in the theoretical base than in the technical and organizational base? What might be the factors that make it impossible to cure the disease despite the fact that we know many of the mechanisms involved in the development of cancer? The rate of survival, which in some forms of cancer has increased considerably over the last few decades, is due more to techniques used to detect cancer in the early stages than to a real cure once the disease has been diagnosed.

It might be said that what is lacking here is a bridge between theoretical knowledge and the need which it is trying to satisfy. What is lacking is the design to meet this need. This is one of the reasons why medicine, a design science, is not just an applied science. Moreover, this confirms the idea that pure sciences, understood as descriptive sciences, should not be confused with basic research. The search for appropriate designs to cure cancer based on existing knowledge is an integral part of basic research in medicine.

An example of progress in cancer research which is based on a technical element is the development of a scalpel which is able to detect cancerous cells in a matter of seconds. According to an article which appeared in *El País* on 24 March, 2000, scientists working in the Sandia National Laboratories of the Department of Energy in the United States have developed a scalpel designed to detect the presence of cancerous cells while the surgeon is removing the tumor obscured by blood, muscle and fat. The instrument is called " biological microcavity laser" and has made it possible to distinguish between cell cultures, composed of normal brain cells called "astrocites" and its malignant form called "glioblastomas" in the laboratory. This may enable surgeons to eliminate malignant growths with greater precision and at the same time reduce to a minimum the quantity of healthy tissue which is removed.

The third base of progress is the organizational one. How might we interpret this base in relation to cancer research? At the present moment the circulation of knowledge on a worldwide scale (at least understood as a possibility) and the differences in life expectancy of cancer patients corresponds to differences in health politics according to economic, social and political factors. In other words, if the theoretical and technical bases remain stable then progress in cancer medicine will be due to changes in the organizational base and to possible action in the area of health politics.

In an indirect way, environmental programs (politics), which are to a large extent responsible for environmental factors in the development of cancer, would also play a part in the progress of cancer medicine. As a last resort, progress in cancer medicine would depend on the risk management of many of the practices of today's society. We may venture to say that the third point made by Kotarbinski contains all the philosophical issues pertinent to the risk factor even though he does not specifically refer to this.

7. Scientific Politics and Cancer Research

Investment in scientific research is one of the systems within the budgets of states and multinationals. This means that the scientific politics designed by states or companies will have important consequences for scientific progress. We shall refer to various issues within scientific politics. One of these issues is the financiation both of pure sciences as well as design sciences. In this sense, there may exist scientific political programs that prioritize lines of research according to pressure put on by social groups of a certain social class, race, religion, gender, etc. The question therefore is what do we invest in. For example, research in molecular biology might be directed towards curing certain diseases such as cancer, AIDS, coronary diseases or for resolving infertility, etc. We might say that we should invest in all those research programs whose aim is to satisfy a human need, but resources are limited however, and in any case there will always be the question of priority. We will therefore need to choose lines of research in which to invest, both in the public and private sector.

Another question pertinent to scientific politics is the scientific publication of results, and in this sphere academic journals are the most important publications amongst peers. Nevertheless, as we have seen earlier, this system is not without its problems. We may, for example, ask whether Perucho's nationality had any part to play in the difficulties encountered when he tried to publish his discovery despite the fact that we know that Perucho's and Vogelstein's results for molecular biology have nothing to do with their origin (the former being Spanish, the latter Jewish). This is one of the problems of most social constructivists who deduce epistemological consequences from sociological indicators. Individual and collective difference have a part to play in the likelihood of becoming a researcher, of getting more or less financial backing and may even influence the line of research, although the degree of reliability and corroboration is independent. On the other hand, the fact that Perucho's discovery and due merit were recognized in the end shows that the scientific community although exposed to fraud, unfair practice and lack of professional ethical conduct has at the same time mechanisms to correct these irregularities.

The importance of scientific politics may be explained in large part by what John Ziman calls "the collectivization of science," which emerged after the Second World War. Collectivization on the one hand means that academic science is at the service of industrial science and on the other, that science has to be carried out starting with collectives, in which the relationship between the members is not always hierarchical but equal. In other words, any line of research needs experts from different fields and this requires collaboration between them. In reference to the first question, cancer research is an example of an academic science at the service of industrial science, as conceived by Ziman for whom "industrial" has a broad meaning. In other words, molecular biology is at the service of medicine. In reference to the second question, Perucho's laboratory, where he made his discovery, is an example of collaboration and distribution of functions although an element of hierarchical organization also had a part to play.

8. CONCLUSION

The importance of science in today's society has lead to many studies on the relationship between science, technology and society, although most of these tend to question the rationality of science. Nevertheless, when it comes to identifying those factors which question scientific rationality very different questions such as disasters associated with the atomic bomb, transgenic products, bad relations between scientists in the laboratory, fraud, power structures, economic interest, suffering on the part of animals used in experiments and many more tend to be identified. All of these problems are very important and we may say that they have been caused, in most cases, by scientific development. However, we need to take stock of the beneficial gains, which are not a few both in terms of health and disease as well as quality of life when it comes to evaluating science at a practical level. Even if the evaluation were negative, which I do not believe is the case, we would need to distinguish between what knowledge of the natural and social world means and what we can do with this knowledge. This may appear self-evident however, social constructivists do not accept distinctions of this kind. Even in the case where for ethical reasons a certain line of research would have to be stopped, the distinction between knowledge and its use would still have to be maintained.

Although overlapping between descriptive or pure and design sciences occurs a great deal when faced with a given phenomena of the natural or social world, it is both possible and philosophically worthwhile to keep the conceptual distinction. The contrary might lead us to extrapolate characteristics from one or the other science, resulting only in confusion. The tension between the different collectives involved in cancer research is a good example of how all these disciplines cannot be brought together in a kind of megaconcept, as in the case of Echeverría's technoscience although it is not clear that he is in favor of a total fusion, or of the untenableness of the thesis that there is no difference between science and technology.

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EXPERIMENTS, INSTRUMENTS AND SOCIETY: RADIOISOTOPES IN BIOMEDICAL RESEARCH

María Jesús Santesmases

The purpose of this essay is to show an epistemic and cultural process that played a part in the invention and use of new instruments: that of the use of radioisotopes in biomedical research. Incorporating the values of the era in which the use of radioisotopes were designed and promoted, the production of these at an industrial scale and their "blackboxing" finally took place, and they have become useful utilities which are productive through the results they provide.

Studies on the invention and use of experiments show how the production of scientific knowledge, at the bench in the laboratory and by a community of experts, is socially embedded. While producing knowledge, experiments and instruments contribute at the same time to the construction of scientific expertise.

A given technique is part of the much broader context in which it is handled and of a wider culture than that of the person who becomes skilled in its design and tuning. It usually incorporates social and political practices by analogy. Thus it is not only that the instrument or the technique becomes part of the society in which it is used, not only that it shares with its environment norms and values, but that the social values of the environment in which the experiment is carried out penetrate into the design of a given device as such, and that these values intervene in the knowledge produced and reproduced by a given set of techniques in a so-called experiment.[1]

Norton Wise suggested considering the technical devices as mediating machines, mediating agents between scientific knowledge and its cultures, to show the mechanisms by which social and cultural values are embedded into knowledge, and the ways by which knowledge emerges in such cultures and values. Later methodological reconstructions have situated instruments and experiments as part of the knowledge they produce and disseminate, considering the instruments as scientific knowledge as such and not only mediating machines. This consideration leads to a blurred distinction between science and technology, as both would be part of the same box of learning. In this approach, techniques as an application would be meaningless, or at least an incomplete supposition, since the machine or the device of any size, no matter whether in design or by its "blackboxing" use, produces and reproduces phenomena that become

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[1] Cf. WISE, M. N., "Mediating Machines," *Science in Context*, v. 2, (1988), pp. 77-113; SHAPIN, S. and SHAFFER S., *Leviathan and the Air Pump: Hobbes, Boyle, and the Experimental Life*, Princeton University Press, Princeton, 1979; LATOUR, B. and WOOLGAR S., *Laboratory Life: The Social Construction of Scientific Facts*, Sage, Beverly Hills, 1979.

knowledge. It does not matter whether this knowledge is technical or scientific.[2] The concept of instrument has been considerably widened, including not only machines, devices, and experimental methods of any kind, but also living matter such as virus, microorganisms (bacteria), insects (i.e. *Drosphila melanogaster*), mammals (i.e. mice) and cell lines (i.e. HeLa cells) obtained by selective breeding and by cell culture.[3]

In addition to becoming scientific knowledge *per se*, instruments would also be considered agents by which the frontiers between the natural and the social order are constructed and by which the concept of the social and the natural are conceptualised and incorporated into knowledge.[4]

1. RADIOISOTOPES AND SOCIAL ORDER

The development of biomedical research since World War II has involved the design and use of many techniques and new policies for the promotion of the sciences as experimental processes. The outcome of all this influence, of both techniques and policies, has been instrumental for scientific production, clinical practice and biological thought.

These developments show how scientific production takes form, both influenced and constrained, stimulated and promoted by a set of cultures and policies. And during the twentieth century especially, by science and technology policies, as well as defence policies, and by a network of scientists and engineers in which instruments also took part as agents.

The consideration of instruments as agents evokes historical episodes of the sciences whose analysis is useful to find out about what that knowledge is and the circumstances which allowed the understanding of its production.

In this paper, the case of the use and promotion of radioisotopes in biological and biomedical research offers the opportunity to show some of the factors at play in the introduction of a new technique, its associated instruments and the knowledge produced. It is a useful case to study on how the so-called technosciences

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[2] References of historical and philosophical studies of instruments are very long already, beginning with pioneering works such as the compilation of GOODING, D., PINCH T. and SCHAFFER S. (eds), *The Uses of Experiment*, Cambridge University Press, Cambridge, 1989; for a philosophy of experimentation, see HACKING, I., *Representing and Intervening*, Cambridge University Press, Cambridge, 1983.

[3] Cf. RHEINBERGER H.-J., *Toward a History of Epistemic Things: Synthesizing Proteins in the Test Tube*, Stanford University Press, Stanford, 1997; KOHLER, R. E., *Lords of the Fly: Drosophila, Genetics and Experimental Life*, The University of Chicago Press, Chicago, 1994; LAUBICHLER, M., and CREAGER A. N. H., "How Constructive is Deconstruction?," *Studies in History and Philosophy of Biological and Biomedical Sciences*, v. 30, (1999), pp. 129-142; KAY, L. E., *Who Wrote the Book of Life? A History of the Genetic Code*, Stanford University Press, Stanford, 2000; CREAGER, A. N. H., *The Life of a Virus: Tobacco Mosaic Virus as an Experimental Model, 1930-1965*, The University of Chicago Press, Chicago, 2002; and RADER, K., *Making Mice*, Princeton University Press, Princeton, 2004.

[4] Cf. PALLADINO, P., *Plants, Patients and the Historian*, Rutgers University Press, New Brunswick, 2003; and CLARKE, A. E. and FUJIMURA, J. (eds), *The Right Tools for the Job: At Work in Twentieth-Century Life Sciences*, Princeton University Press, Princeton (NJ), 1992.

turned out to be a product of a wide variety of influences, laboratory cultures and policies, mutually shaped and combined at a given time.[5]

Escaping from a presumed concept of knowledge as an accumulative process, the analysis of the use of radioisotopes as instruments and techniques suggests the need of considering its emergence and development in a given time and a given locus as a product of specific circumstances. Following the paths taken by a given instrument, it is possible to show the features a particular set of circumstances imposed on the design and circulation of a technique, including its relationship with previous knowledge and the ways by which the instrument contribute to the dissemination of its use and the knowledge it provides.

When looking at instruments we look at the very core of the production of scientific knowledge, since science is linked to experimentation (no matter whether in hierarchical order or not). The knowledge obtained by using a given device or technique is later validated and is considered reliable by complex mechanisms. When any of us analyse these mechanisms as used for the social acceptation of truth, we do it embedded in our own time as well. Our time is that of growing expectations in sciences and techniques and of their promise. These expectations engender the posing of new questions about knowledge itself as well as the invention of new devices.

Asking ourselves basic, apparently innocent, questions about the usefulness of our own work while analysing instruments and experimental systems may lead us to understand the mechanisms through which data obtained at the bench of an experimenter, or at the table of a theoretician, is elaborated and becomes knowledge about the natural. This knowledge comes from experiments that are artificial indeed, but this seems to be irrelevant to the legitimation of this knowledge. It does not matter whether the knowledge from the natural comes from the artificial. Nonetheless, and more than that, the artificial experiment –as it is isolated from the natural world in which it was produced before it was tried as an isolated phenomena at the bench and in test tubes– becomes a useful method to provide knowledge about the natural. This method is precisely the scientific method itself, a long with the particular basis of what is considered scientific.

The natural and the artificial, the latter considered an accepted and reliable way to produce scientific truth, become so closely intertwined that it can be suggested that the natural and the artificial can hardly be separated.[6] And this

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[5] On the concept of technoscience as an outcome of the post-WWII era, see Echeverria, J., *La revolución tecnocientífica*, FCE, Madrid, 2003.

[6] On tecniques and the artificial, see Broncano F., *Mundos artificiales. Filosofía del cambio tecnológico*, Paidós, México D. F., 2000; and Ferreiros, J. and Ordoñez, J., "Sobre la no neutralidad de los instrumentos científicos," in Santesmases, M. J. and Romero, A. (eds), *La Física y las Ciencias de la Vida en el siglo XX: Radiactividad y Biología*, Ediciones Universidad Autónoma de Madrid, Madrid, 2003, pp. 13-22. On artifacts and nature in biological experimentation, Santesmases, M. J., "¿Artificio o naturaleza? Experimentos en Historia de la Biología," *Theoria*, v. 17, (2002), pp. 265-289.

blurred distinction between nature and experiments at the bench configures the contemporary social order. This social order is based on this blurred distinction and is at the basis of decision-making. This decision-making is based on the scientific expertise of different groups of people specialised in a given field, whose scientific authority is constructed precisely on their capacity to create and reproduce knowledge and to make it reliable through the usual dissemination (i.e. papers published in specialized journals, by peer-review and evaluation processes).

2. Provisionality and Historicity

The development of biological and biomedical research since World War II has been forged by a set of techniques, knowledge and policies whose influence has become instrumental in the paths taken by scientific production, biological thinking and clinical practices.

When analysing episodes of the historiography and philosophy of sciences distinctions between these two disciplines are blurred. The studies on experiments, experimental systems and techniques, and on procedures that became standard, show that our beliefs in what is nature are based on laboratory experiences. These experiences do not have much to do with the state of a given polymer or a living cell of any size in its original, *natural* context of growth and reproduction. For centuries, muscles, cells and polymers have been isolated from their original state. Since its very origin, the basis of successful experimentation has been the capacity of reproducing once and again the same phenomena at the bench. The interpretation of what we, philosophers and historians of science, may do are also embedded in our own time. This time is shaped by decisions made in public policy, and public and private life. These decisions are articulated around knowledge which is considered scientific; they are based on discussions between experts. The legitimacy of these experts is constrained by the way in which they perceive reality, by the values and cultures of our own society. Thus this circularity effects every step we take, every move we make in private and public life.

Our perception of reality includes the use of successful techniques and pharmaceuticals, their long-lasting effects on people's lives in contrast to the massive deaths of human beings, animals and plants far later than the atomic bombs at Hiroshima and Nagasaki and their annihilation capacity of any life form, not to mention other effects related to mutation genetics.[7] This is a time in which genetic differences among different living matter (i.e. genetic differences between human beings and mice) have come to be small at the genetic level and therefore can hardly account for the differences appreciated by the naked eye. There are successive reductions or approaches to natural phenomena by (artificial) experimentation and these circumstances of artificiality seem to be irrelevant:

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[7] Cf. Lindee, M. S., *Suffering Made Real: American Science and the Survivors at Hiroshima*, The University of Chicago Press, Chicago, 1994.

the more molecularised, the more the phenomena is reduced to reactions between molecules of known characteristics and known structure, the more reliable become scientific developments toward a better, more accurate knowledge about living things. Every form of life is reduced to a small-size phenomena, so small that a test tube provides enough room for what is considered life production.

From the disturbing success of the more recent techniques and their effects on everyday life, this paper reviews a recent case from the recent past, without any certainty, in order to look for a feature which is very overlooked forgotten: the provisionality of techniques.

Experiments with radioisotopes are an example of this provisionality. A review of the events that took place from the interwar period concerning their use in biological and biomedical investigation provides information that allows us to go into detail about the temporary value of a technique.[8] Some controversies shed light on the topic of the role played by the public in the process of making knowledge reliable. Social and intellectual discussion on atomic and nuclear energy, an issue which is at the core of science and technology studies, shows how public debates, and not only discussions between experts, construct knowledge while challenging formal scientific expertise.[9] Knowledge as well as social order and cultural values took part in the forging of a given technique. Currently almost disappeared in laboratories of molecular biology, radioactive isotopes were until the early 1980s a tool to invent experiments and pose questions, offering a wide capacity to answer them.

3. SECURITY AND SAFETY: DEFENCE AND RADIATION

Public opinion had an instrumental role in the process by which radioactive isotopes became useful in biological investigation and clinical therapy, and in the later substitution of these tools from the early 1980s. This substitution of other techniques in place of radioisotopes in the laboratories was based on the difficulty of the safe handling and removal of radioactivity from laboratory samples, although in clinical therapy and in endocrinology they remain central for diagnosis and treatment purposes.

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[8] And thus of knowledge as well –no matter whether matter is still falling down at gravitation acceleration described by Newton-. Some interpretations of experiments developed new techniques, and some techniques were kept in the laboratories while the theories were not, as these techniques contributed to new interpretations. The case of the electrophoresis apparatus and the colloid theory of proteins is a good case: see MORGAN, N., “The Strategy of Biological Research Programmes: Reassessing the ‘Dark Age’ of Biochemistry, 1910-1930,” *Annals of Science*, v. 47, (1990), pp. 139-150.

[9] On knowledge, experts and the public see, among others, SHRADER-FRECHETTE, K., “Objectivity and Professional Duties Regarding Science and Technology,” in GONZALEZ, W. J. (ed), *Science, Technology and Society: A Philosophical Perspective*, Netbiblo, A Coruña, 2005, pp. 51-79; and SHRADER-FRECHETTE, K., “How to Reform Science and Technology,” in GONZALEZ, W. J. (ed), *Science, Technology and Society: A Philosophical Perspective*, pp. 107-132. See also JASANOFF, S., *The Fifth Brach: Science Advisors as Policy-makers*, Harvard University Press, Cambridge (MA), 1990.

According to Rasmussen, in the aftermath of WWII, the growing public attitude against atomic energy and its devastating effects on Japan had an almost immediate effect on the promotion of biological research by US science policy authorities. "A large scale cultural force (…) propelled physics and physicists into biology" and together with trends towards biological explanations in terms of molecules contributed to increasing budgets for biological research and for the introduction of new techniques as well.[10] After the emergence of ecology movements against nuclear energy in the early 1970s, the growing safety measures imposed by prevailing norms of isotope use in fact promoted other techniques that eventually substituted isotopic tracing in the laboratories.

It is widely known that radioactive isotopes decompose emitting dangerous energy which is able to induce genetic mutation due to the features of this radiation, its wavelength and its frequency. Some of the more widely used radioisotopes have a long half-life and this makes their elimination difficult or even impossible. Norms for safely handling radioactive samples, samples in which radioactive isotopes were included for research, have been disseminated which included instructions not only concerning their handling but also how to manage residues once the sample is discarded after an experiment. These norms and the public attitude against nuclear energy, including public concern regarding radioactive residues, made the use of radioisotopes in the laboratory a nuisance. They should be stored in special warehouses built for this purpose and should be reliably collected when they became useless for further experiments. (Note, however, that despite the risk associated with their use and the difficulties of storing its residues, nuclear energy still is a main source of energy in the so-called developed world and the problem of nuclear residues is literally stored inside concrete warehouses, frequently underground: see the works of Kristin Shrader-Frechette on nuclear energy risks and her essays in this volume).

The US Atomic Energy Commission was the agency that provided early training for experts in the use and handling of radioisotopes, offering the first training course at the Oak Ridge Institute of Nuclear Studies in 1948. These training courses were part of the main strategy of the US AEC aimed at keeping a strong political support for atomic energy, its research and possible recycling as a peaceful source of energy: i.e. nuclear power plants. This strategy allowed physicists to keep their role as researchers supported by the government while maintaining atomic energy at the core of science-policy strategies of the post-war era.

At this time, growing US national security concerns manifested the tension between them and the aim of the Marshall Plan toward cooperating in European war recovery. According to Creager, the numerous radioisotopes requests that came from scientists outside the United States "sparked a debate about whether

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[10] Cf. Rasmussen, N., "The Mid-century Biophysics Bubble: Hiroshima and the Biological Revolution in America," *History of Science*, v. 35, (1997), pp. 245-293.

the Commission should or even could export radioisotopes." The possibility of isotope shipments to Europe was discussed at the US AEC during 1947. This worry showed a contradiction of US policy toward cooperating in the European war recovery and of the efforts of US diplomacy to tighten the bonds of friendship with her European allies by supporting scientific and technical cooperation. Finally AEC authorities made a decision in favour of radioisotope shipments to European biological laboratories and many were sent to laboratories in Europe, while "the issue of radioisotopes export from the US continued to draw political fire in the United States, even after the establishemnt of national energy facilities elsewhere."[11] Although having been trained in their courses was not a formal requirement for receiving radioisotopes from the US AEC, the agency recommended that researchers be qualified in their handling.[12]

In Spain, given the relative gap in the access to information on atomic energy, the issue of the safe handling of radioactive material, both atomic energy and radioisotopes, was among those included in the early plans toward the creation of the Junta de Energía Nuclear (JEN, National Board for Atomic Energy) in 1951.[13]

4. Radioisotopes for Biology and Medicine: The Atomic Era

The use of radioactive isotopes was one of the factors at play in the growing leading role of biological and biomedical research from the aftermath of WWII until the early 1980s. A given radioisotope of a given element was introduced into a molecule by synthesis. This allowed the researchers to obtain tracer molecules in which one of the atoms was not a stable (normal, usually found in nature) form but that of an isotope of the same element. The introduction of this element in a given molecule keeps its properties and apparently does not affect any of the chemical properties of the molecule.

The use of radioisotopes allowed researchers to detect the tracer molecule on the basis of its capacity to emit a certain type of energy, which could be detected by a device designed for this purpose (on the detectors, see below and references herein). Through this procedure, many molecules were traced, and the reaction on them "followed" by a detector, so as to obtain information about their transformation. This was used in research on carbohydrate metabolism, in photosynthesis, protein synthesis and in research on the role played by nucleic acids.[14]

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[11] Cf. Creager, A. N. H, "Tracing the Politics of Changing Postwar Research Practices: the Export of 'American' Radioisotopes to European Biologists," *Studies in History and Philosophy of Biological and Biomedical Sciences*, v. 33, (2002), p. 367.

[12] Cf. Creager, A. N. H., "The Industrialization of Radioisotopes by the US Atomic Energy Commission," in Garndin, K. and Woormbs, N. (eds), *Science and Industry in the 20th Century, Nobel Symposium 123*, Watson Publishing, Sagamore Beach (MA), forthcoming.

[13] Cf. Romero, A. and Sánchez Ron, J. M., *Energía nuclear en España. De la JEN al Ciemat*, CIEMAT, Madrid, 2001.

[14] Cf. Rheinberger, H.-J., *Toward a History of Epistemic Things: Synthesizing Proteins in the Test Tube*, Stanford University Press, Stanford, 1997.

More relevant was their eventual use in medical diagnosis and therapy, which were being among the early applications of the cyclotron's products. The possibility of contributing to medicine was envisaged quite early by Lawrence in his laboratory at the University of California, Berkeley. These applications reinforced the interest of cyclotrons as such and widened the possibilities of the funding of the laboratory.

From the mid-1930s on, the cyclotron and the promising use of its products in medical research and therapy captivated biologists, medical researchers and funding agencies. The British physiologist Archibald V. Hill compared its promising influence with that of the microscope: if with the latter cells were visible, with isotopes the atoms would be visible within the cells.[15]

The use of isotopes in biological research began in the 1920s in the US. Rudolf Schoenheimer used heavy water (in which the hydrogen atom was substituted by its heavy isotope deuterium) to do research on its effects on organisms and biological macromolecules.[16] During this decade cyclotrons began to be built and research on radioactive elements was developed and began to be disseminated following the success of the research done in Paris by Marie and Pierre Curie. A well-known case is that of the laboratory of Ernest O. Lawrence in Berkeley. According to Heilbron and Seidel, in the fall of 1933 the cyclotron began to produce so great a flux of neutrons from berilium through the effect of 3 MeV deuterons that they began to worry about its "physiological effects" on the researchers themselves. At this point Lawrence saw that this radiation could have some medical relevance in cancer therapy. By this time x-rays were no longer considered a promise in cancer therapy. With this idea in mind he applied for funds to the Macy Foundation with a project for doing research on them and succeeded in making them more intense than x-rays or radio rays.[17] In 1936 "[the] question [was] of more than theoretical interest, for it [bore] directly on the possibility of using very fast neutrons in the treatment of tumors."[18]

In 1935, neutron rays were investigated in the Department of Physiology at the University of California, Berkeley, and were shown to be considerably more biologically lethal than x-rays. The issue of the supposed danger of high-voltage rays had began to be discussed since 1929 but a level of tolerance was not yet established. Nevertheless, the possibility of harm from radiation introduced prudence in the Berkeley group, whose members took technical cautions with the aim of minimizing risks. Precisely these risks brought Lawrence back to his

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[15] Cf. Kohler, R. E., *Partners in Science: Foundations and Natural Scientists*, The University of Chicago Press, Chicago, 1991, p. 371.

[16] Cf. Kohler, R. E., "Rudolf Schoenheimer, Isotopic Tracers and Biochemistry," *Historical Studies on Physical Sciences,* v. 8, (1977), pp. 257-298.

[17] Cf. Heilbron, J. L. and Seidel, R. W., *Lawrence and his Laboratory: A History of the Lawrence Berkeley Laboratory*, University of California Press, Berkeley, 1989, p. 357.

[18] Heilbron, J. L. and Seidel, R. W., *Lawrence and his Laboratory: A History of the Lawrence Berkeley Laboratory*, p. 390.

earlier idea of the medical applications of neutron therapy. Supported at first by the Macy Foundation and then by the Rockefeller Foundation, he organised a group for medical research in the School of Medicine at Berkeley in 1937. In 1940, five doctors, a nurse and a technician, and four operators of the 60-inch cyclotron were members of the group and they were distributed in three sub-groups, one devoted to neutron therapy, one to leukaemia and one to biological markers, or tracers.[19]

The absorption of radioactive phosphorous in hematopoietic tissues (responsible for the elaboration of blood cells) and in cells of high multiplying capacity was also among the first research done.[20] Treatment of non-cancerous hyperthyroidism by radioiodine began at Berkeley and at the Massachusetts General Hospital in Boston about 1940. Later on, Glenn Seaborg and J.J. Livingood designed a method to use the cyclotron so as to produce longer half-life iodine isotopes, I-130 and I-131. A relationship between iodine and thyroid activity was found, and this led to using radioiodine in research and treatment of hypothyroidism. As early as 1941, some research groups were already using mixtures of I-130 and I-131 in patients with thyroid disorders. Effective treatment of cancer by I-131 dates from after the war.[21]

Neutron therapy began in the 37-inch cyclotron in September 1938. It would eventually become increasingly promising, with more advantages than those of x rays. Some results on the ingestion test of phosphorous-32 (P-32) were also obtained in chronic leukaemia patients and with radioactive iodine in thyroid patients. Isotopes from the 60-inch cyclotron proved better as it allowed the treatment of a greater number of patients.[22]

By 1936, Warren Weaver, the head of the Rockefeller Foundation Division of Life Sciences, was reluctant to give financial support to high-energy physics. His worries were related to the lack of support obtained for his program on the uses of spectroscopy in medicine. This project had been turned down by the group of experts which evaluated it. Therefore, by the mid-1930s he was, according to Kohler, "in no hurry to jump on the cyclotron bandwagon." Gradually Weaver began to accept the promises of the biological applications of cyclotron radiations. Between 1935 and 1945 the Rockefeller Foundation funded the construction of nine cyclotrons: three in Europe (one proposed by Niels Bohr in Copenhagen, another proposed by Irene Curie and Frederic Joliot at the Collége the France in Paris and a third one at the University of Stockholm) and six in the United States (first at the University of Minnesota, the University of

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[19] Cf. *Lawrence and his Laboratory: A History of the Lawrence Berkeley Laboratory*, pp. 359-360.

[20] Cf. Krige, J., "The Politics of Phosphorous-32: A Cold War Fable Based on Facts," in Doel, R., and Söderqvist, Th. (eds), *Writing Recent Science*, Routledge, London, forthcoming.

[21] Cf. Heilbron, J. L. and Seidel, R. W., *Lawrence and his Laboratory: A History of the Lawrence Berkeley Laboratory*, p. 398.

[22] Cf. *Lawrence and his Laboratory: A History of the Lawrence Berkeley Laboratory*, pp. 395-404.

Rochester, the Washington University and the University of California with two others including an E. Lawrence 184-inch machine and a Van de Graaf machine at MIT). In all these projects medical and biological applications were combined with "pure physics" objectives.[23]

5. BIOLOGY *VERSUS* PHYSICS

As the cyclotron size and power began to increase, so did the possibility of its application to biology and medicine. According to Kohler, the medium-size accelerators were the ones of more use for biological and medical research and also for radiotherapy. The use of the largest cyclotrons, which crossed the threshold of a new research front into the world of "mesotrons", for basic biochemical and biophysical work was "overshadowed" by high energy physics and its military applications. The bigger the cyclotron, the more likely it appeared to be attached to a hospital and thus the more Weaver worried about the clinicians getting funds intended for basic research, and not for clinical work.[24]

At the end of the 1930s it seemed clear that small-size cyclotrons produced isotopes for medical application and clinical use at small scale, at a time when 42-inch cyclotrons were already a standard model. Those of 60 inches had applications in what were already considered as "routine" radiotherapy.[25] The close relationship established between cyclotrons building-up and their medical applications allowed the training of physicists in the design and use of these machines while increasing their clinical capacity and promises for biological research. The power of these devices should increase as well and this contributed to the emergence of a new group of experts.[26] In their own interest, and with the aim of obtaining the economic support of foundations, physicists promoted the cooperation with medical researchers. Thus accelerators were a product of the cooperation between experts in both disciplines while allowing to obtain further support for the physicists' own work –the design of new accelerators, more powerful as well as more expensive– in a close circle of mutual benefits for both disciplines (physics and medical researchers and clinicians). Both groups of expertise and this beneficial relationship between them contributed as well to delineate the contents of biological and medical knowledge in the whole second half of the 20th century.

The cost of accelerators began to increase and private foundations were not allowed to pay even half of the increasing cost of deliver the huge budgetary needs for support. The cost of accelerators, on the one hand, and the scientific and technological policy of the United States during WWII, on the other, would

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[23] Cf. KOHLER, R. E., *Partners in Science: Foundations and Natural Scientists*, p. 372.

[24] Cf. *Partners in Science: Foundations and Natural Scientists*, p. 373-374.

[25] Cf. KOHLER, R. E., *Partners in Science: Foundations and Natural Scientists*, p. 373.

[26] Cf. HEILBRON, J. L. and SEIDEL, R. W., *Lawrence and his Laboratory: A History of the Lawrence Berkeley Laboratory*, chapter 8.

eventually leave the funding of the atomic energy and its applications under the responsibility of the US Atomic Energy Commission (AEC), created in the aftermath of the war.[27]

In the meanwhile, when the Manhattan project was already underway in 1943, there were already many successful developments in the use of radioisotopes in the United Sates. The interest of the heads of the Rockefeller Foundation in possible risks to researchers' health when handling radioactivity contributed to the creation of a Medical Division of the Manhattan Project. Laboratories at the universities of Chicago, Rochester (New York), California Berkeley, Columbia, Washington, Los Alamos and at Clinton Laboratories in Oak Ridge were some of those that carried out research in health-related risks as part of the Manhattan Project Medical Division. Among their research was that of the establishment of the acceptable radiation doses for experimentation. Given earlier results on leukaemia, research on the mechanisms by which radiation may affect hematopoietic tissues became a priority. Thus, public health promotion appeared to acquire strong links with the direct interest of the Manhattan project managers on radioactive security, while civil applications of atomic energy were perceived as enormous. Concerned for researchers' safety, the Manhattan project opened wide possibilities for the promotion of medicine as an experimental science, at a time when medical practice was based more on clinical knowledge than on instrumentation and experimentation, as it late would be.[28]

When the war ended, experts on research in isotopes applied to biology and medical therapy envisaged the possibilities that could be opened in this area of application of the Manhattan Project itself. For this purpose, it seemed necessary to keep on supporting scientists and technicians involved in this war-time project through training and research programs. This required the ability to keep the emergency climate that had featured medical research as related to the war effort, developed during the war itself.[29]

Therapeutic uses of radioactive sources, that had been an instrumental justification for the expenditures on cyclotron construction since the end of the 1930s, became in the aftermath of WWII an instrumental incentive in the promotion of scientific and technical contacts between physicists and biologists. At the end of the war the heads of the Manhattan project were strongly interested in putting radioisotopes at the disposition of medical research. The US Atomic Energy Commission assumed, among other responsibilities, and beyond those

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[27] See the works of A. N. H Creager in notes 11 and 12. On the history of the US AEC, Hewlett, R. G. and Anderson, O. E., *History of the United States Atomic Energy Commission. Vol I, The New World, 1939/1946; Vol. II, Atomic Shield 1947/1952*, The Pennsylvania State University Press, Pennsylvania, 1962.

[28] Cf. Lenoir, T. and Hays, M., "The Manhattan Project of Biomedicine," in Sloan, Ph. R. (ed), *Controlling our Destinies. Historical, Philosophical, Ethical and Theological Perspectives on the Human Genome Project*, University of Notre Dame Press, Notre Dame (IN), 2000, pp. 29-62.

[29] Cf. Lenoir, T. and Hays, M., "The Manhattan Project of Biomedicine," pp. 32-37.

related to the military and defence, that of research and development activities related to the use of the fission products, including radioisotopes for biological and medical purposes.[30]

6. The US Atomic Energy Commission as an Instrumental Agent

The Atomic Energy Commission approved a budget for contributing to biological and medical research in the mid-term. Rasmussen (1997) has convincingly argued that an intense "cultural force" promoted the biological applications of nuclear energy in the post-war period, as the US government was anxious to make atomic energy into a useful source for life. The atomic bombs exploded at Hiroshima and Nagasaki played a significant part in the construction of further strategies for the research community devoted to biological and medical research: the promotion of life sciences in contrast to death sciences (the bombs) was promoted so as to counteract the adverse public opinion to atomic energy.[31]

In this climate the social impact of the rhetoric promoting the uses for life of atomic energy was developed from source that most efficiently had contributed to suppress life itself. Robbley Evans, an MIT physic, stated in 1946 that the "pure truth is that only thanks to the medical advances, atomic energy has saved much more lives than it has extinguished at Hiroshima and Nagasaki."[32] This discourse proceeded from the interest to legitimate atomic bombs themselves, putting their victims and their survivors in a conceptual level that could be assimilated to that of instruments and experimental systems, as Susan Lindee argued.[33] The story of the first bomb launching, according to a detailed narrative of Everett Mendelsohn ended with the following statement:

> "As an analyst of the United States Army Institute of Pathology put it 'little boy' produced casualties including dead six thousand five hundred times more efficiently than ordinary high explosive bombs. The press release from the White House in Washington, at mid-day August 6, 1945 (local time) called the bombing "the greatest achievement of organized science in history."[34]

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[30] Cf. Creager, A. N. H, "Tracing the Politics of Changing Postwar Research Practices: The Export of 'American' Radioisotopes to European Biologists," pp. 367-388; Rheinberger, H.-J., "Putting Isotopes to Work: Liquid Scintillation Counters, 1950-1970," in Jörges, B., and Shinn, T. (eds), *Instrumentation Between Science, State, and Industry,* Sociology of the Sciences Yearbook, v. 22, Kluwer, Dordrecht, 2001, pp. 143-174. On the US Atomic Energy Commission, see Hewlett, R. G., and Anderson, O. E., *History of the United States Atomic Energy Commission. Vol I, The New World, 1939/1946; Vol. II, Atomic Shield 1947/1952*, passim.

[31] Cf. Rasmussen, N., "The Mid-century Biophysics Bubble: Hiroshima and the Biological Revolution in America," p. 246.

[32] Quoted in Rasmussen, N., "The Mid-century Biophysics Bubble: Hiroshima and the Biological Revolution in America," p. 246.

[33] Cf. Lindee, M. S., *Suffering Made Real: American Science and the Survivors at Hiroshima*, chapter 4.

[34] Mendelsohn, E., "Science, Scientists, and the Military," in Pestre, D. and Krige, J. (eds.) *Science in the Twentieth Century*, Harwood, Amsterdam, 1998, p. 175.

The transformation of this "the most relevant achievement in history" into an instrument for peace led US science policy strategies toward supporting biological and medical research, with an intensity comparable to that of the Western defence strategies. In addition to the expansion of atomic physics as a source of civilian nuclear energy through the building-up of currently instrumental power plants, the research promoted by the use of radioactive isotopes in biological research and medical therapy are yet to be evaluated in detail. Nevertheless recent studies suggest that this production of accelerators, of the development of atomic physics, became an instrument, both at the bench and at science policy level, essential to recycling atomic physics into the famous "Atoms for Peace" campaign.

Thyroid research and diagnosis, radiotherapy against cancer, and research on blood and carbohydrate mechanisms were the areas cited as top examples. Atomic bombs were not the only spectacular and powerful product of the increasingly wide political influence of the United States; also massive production of penicillin and sulfa-drugs had provided unprecedented success in therapeutics. US science policy during war time had shown its efficacy, curative in some cases and devastating in others.

The support that biological and medical research received from then on was enormous. Budgets increased from the immediate post-war period until the crisis of the late 1960s. This budget for biosciences grew earlier than that of the better known budgetary increase after the launching of the artificial satellite Sputnik I by the Soviet Union in 1957. Although the "golden era" for science and technology in the US and, slightly later, in the Western developed world was marked by a vast increase in the budgets for so-called R&D (research and development) from the late 1950s until the petroleum crisis, Rasmussen has called attention to the previous increase in budgets for research that took place as early as 1946. From all the US governmental agencies devoted to funding research, as disparate as the Office for Naval Research, the Atomic Energy Commission and the Department of Defence, biological and medical research began to receive growing budgetary appropriations from the aftermath of WWII until the crisis of the early 1970s. This crisis obliged a wide discussion on the criteria of budgetary distribution for research. However, for the years to come criteria and the objectives of the governmental programs for sciences and techniques would eventually be modified in the US and also in Europe.[35]

Radioactive isotopes did not work alone, however. Once methods of synthesis were developed, the presence of radioisotopes demanded skilful experimenters able to obtain molecules in which an atom of a given radioisotopes was introduce

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[35] Cf. DICKSON, D., *The New Politics of Science*, The University of Chicago Press, Chicago, 1988, 2nd ed.; STRICKLAND, S. P., *Politics, Science and Dread Disease: A Short History of United States Medical Research Policy*, Harvard University Press, Cambridge, Mass, 1972; APPLE, T., *Shaping Biology. The National Science Foundation and American Biological Research*, Johns Hopkins University Press, Baltimore, 2000; and KAY, L. E., *Who Wrote the Book of Life? A History of the Genetic Code*, Stanford University Press, Stanford, 2000, chapter 6.

so as to make possible to follow reactions and mechanisms. Isotopic tracers –as radioactive elements soon come to be known– do not alter significantly, or at least not appreciably, the properties of the compounds. The Isotope Distribution Program of the US AEC delivered shipments of radioisotopes to university laboratories and clinical departments from 1948 on. Its efficacy was such that 39 per cent of the papers published by the US *Journal of Biological Chemistry* (one of the most outstanding journals in biological research) in 1956 included uses of radioisotopes. The detectors could be designed and produced thanks to the funding available to support research in which these products of atomic energy were involved. At this time, funding from US AEC was almost unlimited for research projects of this kind in the United States.[36]

According to Rheinberger, scintillation counters –that was the name given to this detectors– became "highly-developed research-enabling technologies" that "require special product management." Contacts between producers of this instrument and its users made it possible to attend to scientists' needs, requirements and suggestions, and it finally took shape as a first prototype, followed by a series of modified machines, that were built up by Lyle E. Packard. Packard was a mechanical engineer who had worked for the Institute of Radiobiology and Biophysics at the University of Chicago. There he realized the possibility of dedicating himself full-time to design counters, since radioisotopes were being more and more widely used by scientists. He created his own firm in 1949. In 1953 Packard built the first commercial liquid scintillation counter for the University of Chicago. This became the prototype of a continuous production series that made possible a standard procedure for its handling. In this regard, Rheinberger argues that it opened new epistemological dimensions for radioactive experimentation in biology as well as in medicine.[37]

The AEC Radioisotopes Distribution Program and its relation to the need of the AEC itself to provide further legitimation of atomic energy for decades shaped the entire development of radioisotope dissemination and its uses in biological laboratories and hospitals.[38] As a product of this set of circumstances generated in the aftermath of WWII in the US, according to Rheinberger, the liquid scintillation counter effectively came to represent three key technologies of that century: i) mechanical automation, ii) electronics, and iii) radioactive tracing.[39]

As machines capable of posing questions and being used for research, liquid scintillation counters can be considered not only mediating machines, but

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[36] Cf. Rheinberger, H.-J., "Putting Isotopes to Work: Liquid Scintillation Counters, 1950-1970," in Jörges, B. and Shinn, T. (eds), *Instrumentation Between Science, State, and Industry,* pp. 143-174.

[37] Cf. Rheinberger, H.-J., "Putting Isotopes to Work: Liquid Scintillation Counters, 1950-1970," pp. 143-174.

[38] Cf. Creager, A. N. H, "Tracing the Politics of Changing Postwar Research Practices: the Export of 'American' Radioisotopes to European Biologists," pp. 367-388.

[39] Cf. Rheinberger, H.-J., "Putting Isotopes to Work: Liquid Scintillation Counters, 1950-1970," pp. 143-174.

research agents as well. The counting phenomena was an experiment in itself, since the energy emitted by a molecule was detected in specific conditions. Inventing an experiment, followed by its performance, including the measure of the radiation emitted throughout the process, constituted a new landscape not only for laboratory practices but for experimental thinking as well.

7. Concluding Remarks

The growing negative public opinion regarding nuclear energy and the greater knowledge of the risks of radiation contributed to the later substitution of radioisotopes by other tracer techniques, based not on radioactivity but on luminescence and fluorescence. However, radioisotopes created a way of performing experiments and of making them visible. This performance was, precisely, the experiment as such. But this technical requirements could hardly be separated from the designing and performance of the experiment itself. As it can be suggested that there is no knowledge without techniques, experiments –that is, exactly what science is considered to be about– and to the same extent knowledge, are about instruments.

Whether as radioisotopes themselves or as liquid scintillation counters, both became knowledge. And they contributed to the creation of social order and values as well. Because in them was embedded the whole ideology of the post-war era of recycling atomic energy for civilian life. The mutual benefits of this recycling for both scientists (physicists, biologists and clinicians) and science policy authorities were at the basis of the knowledge published on biology and biomedicine during the long second-half of the twentieth century.

This reality of policies, experiments and reliable knowledge constructed scientific expertise as well as further policy-making (further realities). Embedded in their post-war culture, radioisotopes, the counters and the US AEC strategies combined to produce knowledge, techniques and policies. Sciences and techniques could not be separated from the Cold War climate. During this period, international cooperation meant sharing tools as well as research problems, where the tools available were at the core of the process by which research questions were posed. In the realm of biology, producing life in test tubes that carried radioactivity transformed the test tube into the small scale operator within the social order of the radioactivity era.

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IV

The Nexus between Technology and Society

7. Philosophical Patterns of Rationality and Technological Change

PHILOSOPHICAL PATTERNS OF RATIONALITY AND TECHNOLOGICAL CHANGE

Ramón Queraltó

1. TECHNOLOGICAL CHANGE AND DEBATES OF RATIONALITY

The analysis of the general problems of all sort that originated from the global phenomenon of technology is, without a doubt, one of the most important themes of our time. It does not seem exaggerated to affirm that technology has become one of the backbones, for good and for evil, of the present globalized society; and that, just like *science* as such, *technology* requires a specific and differenciated attention to understand exactly the social-historical situation we are living in. The term "technoscience" is even being progressively established to refer as a single item to the mingling between science and technology, in order to comprehend the transforming power of the technological dimensions on their own.

The social perception of the technological "factum" is at least ambivalent. On the one side, technology is contemplated as an indisputable factor of social progress that becomes unavoidable in order to reach progressively the best possible welfare for man in his daily life. But, on the other hand, we are constantly struck by the fear that the social path originated by technological expansion and development is often leading us to results of doubtful anthropological qualification in many aspects, especially for example in the prevailing social ethics –and usually hardly noticeable– in the mentality of contemporary society and man. That is the reason why one of the most widely debated themes that have arisen has been the one dealing with technological change, its sense or nonsense, its rationality or irrationality parameters, the possibility or impossibility of altering the direction or directions of the social change induced from technological change, etc. "Technophobias" and "technophilias" are cultural attitudes that nowadays become apparent in a thousand different ways, and that constitute the radically opposite extremes of such as social perception.

For these reasons, it is not surprising that different approaches to technological change have been proposed, encouraged by the reasonable desire "to know what rules to abide by." Different conceptions of technological change have appeared which we could say, try to "define a model" of that change with the aim of being able to understand it better. Maybe one of the most influential of these is, for example, the so called technological determinism. Simplifying slight internal differences in the positions of this interpretative current, it could be said that technological determinism is characterized by the fact that it conceives the direction of technological change mainly in function of the specific factors and features of technology as such,[1] because it considers that technology possesses, as a historical phenomenon, more than enough internal power to impose its own guidelines. That is the reason why the possible explanation of technological change has to be specifically immanent to technological development itself. As a consequence, the human possibilities of action in regard to the possible social directions of the present technological change, are undoubtedly reduced, although not ruled out at certain levels. In this respect, the ethical impact of this change would be especially worrying. A condensed formulation of this position could be found in the well-known "technological imperative," namely, "anything that is technologically possible to be done will be done." We will refer to this statement as the "strong" technological imperative.[2]

But the problem has also been visualized from other points of view. It can be objected that technological development depends likewise on very diverse social factors that are not technological in a strict sense. Thus, for example, economic, political and cultural factors. It seems an unavoidable fact that economic costs of the investigation in technology condition the development of a research project or any alternative. At the same time, the fact of investment being public, private or mixed and in which proportions, will influence decisively the direction of the technological innovation since they respond to very different social aims. Political options also condition substantially the lines of technological development according to the standards used in the design of the scientific and technological policies. In the same way, the development of many specific technologies would depend on their expected level of social acceptation in function of the sociocultural mentality (moral, religious, professional, etc.) of the potential users who are intended to be involved once they are put into circulation.[3] Because of this, and due to other similar reasons that we are not going to discuss now in order to not seem repetitive, it is affirmed that technology is essentially a "social product" and that, as a consequence, its immanent power underlined by technological determinism is really very small.

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[1] Cf. HEILBRONER, R. L., "Do Machines Make History?," *Technology and Culture*, v. 8, (1967), pp. 333-345; MISA, TH. J., "Theories of Technological Change: Parameters and Purposes," *Science, Technology and Human Values*, v. 17, (1992), pp. 3-12; and SMITH, M. R. and MARX, L. (eds.), *Does Technology Drive History? The Dilemma of Technological Determinism*, The MIT Press, Cambridge (MA), 1994. This book is a collective work of special interest. An historical synthesis about the subject of technological determinism in the US is in M. R. Smith's article, "Technological Determinism in the Culture of USA."

[2] Later we will deal with the "weak technological imperative," whose exact definition will be propose at this moment.

[3] Think for example on an occidental mentality or on an islamic one. The rejection or acceptation of many technical artifacts depends on it. So, there are islamic countries in which the access to Internet is restricted and even punished by law. Obviously, the technological change will be very different in this case.

From here appear “evolutionary” models of technological change,[4] that is to say, explanatory constructions that emphasize an adaptive process of Technology to any imaginable sort of social situations.

Of course, there are also intermediate positions in which it is tried to combine harmoniously factors shown in the previous conceptions in some of their more significant internal lines, by elaborating new conceptual parameters that undoubtedly deserve attention in order to understand technological changes,[5] such as “technological tradition,” “technological style,” etc., as well as in order to evaluate their degree of penetration in the interior of technological work and of its main agents.

But, if it is a fact that a diversity of “models” of technological change exists beyond doubt, as it has been emphasized so far, although in a very simplified way, it is no less true that, looking globally at this scenery, it is generally characterized because the analytical instruments used in these models are more often taken from the social sciences supposedly more related to the sociotechnological phenomenon,[6] namely economy, sociology and political science. Undoubtedly, such an election, be it conscious or not, is suitable for many reasons. But this does not mean that from other perspectives it will not be possible to approach the matter with possibly fruitful results, particularly when the object of analysis, that is to say, technological change in a broad sense, is considered from the beginning as an object of extraordinary complexity.

One of the fields from which it could be possible to shed some clarifying light on the problem, and that certainly has not been developed as thoroughly as the above mentioned fields, is within the strictly philosophical field. Without a doubt, the contributions of the current philosophy of technology in regard to other aspects of the analysis of technological fact are remarkable, as is well-known. For this reason, by using this idea as a preceding basis, it is worth trying to study technological change from a mainly philosophical point of view. Because at the same time it is possible that from this point of view it would be preferable to define the reasons that could support the totality or a part of the horizons opened by the interpretative currents most centered on the different social sciences.

And that is not a vain desire. Especially since into the models of technological change there is built, sooner or later, a certain “canon” of rationality, that is to say, a working structure, supposedly rational, from which the jigsaw puzzle pieces

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[4] Cf. Dosi, G., “Perspectivas de la teoría evolucionista,” in Lopez Cerezo, J. A., Gonzalez, M. I. and Lujan, J. L. (eds.), *Ciencia, Tecnología y Sociedad,* Ariel, Barcelona, 1997, pp. 131-146. About social influence in technology, cf. Bijker, W. E., Huges, T. P. and Pinch, T., *The Social Contruction of Technological Systems: New Directions in the Sociology and History of Technology*, The MIT Press, Cambridge (MA), 1987; and Latour, B., *Science in Action: How to Follow Scientists and Engineers Through Society*, Harvard University Press, Cambridge (MA), 1987.

[5] Some of them are in Bijker, W. E., “The Social Construction of Bakelite: Towards a Theory of Invention,” in Bijker, W. E., Huges, T. P. and Pinch, T., *The Social Construction of Technological Systems*, pp. 159-187.

[6] An important exception could be L. Laudan. Cf. Laudan, R. (ed.), *The Nature of Technological Knowledge: Are Models of Scientific Change Relevant*?, Reidel, Dordrecht, 1984.

form an understandable intellectual figure, without excluding for this reason the limitations always existing in any reflexive task. However, traditionally the characteristic place for the analysis of rationality in a strict sense is the philosophical field, obviously without detriment to the consideration of other necessary and enriching parameters which must be taken into account by the philosophical analysis in order to be fruitful. For this reason, I think that it is not superfluous in any way to insert the philosophical point of view in the debate about technological change, since, in principle, there are historical reasons that justify this task by its potential productivity in relation to the problem posed.

This is intended to be our main perspective in this contribution. It is obviously not my intention to substitute previous perspectives or to argue formally with these, but rather it will be an attempt to organize the "battlefield," and will try to fulfil the objective of clearing a bit more the understanding of the problem in regard to one of its basic dimensions, that is to say, what has been called at the beginning a "theme of our time."

2. THE GLOBAL RATIONALITY OF TECHNOLOGY AND ITS FIRST CONSEQUENCES ON TECHNOLOGICAL CHANGE

The philosophical question, then, has to be referred to a general characterization about technological change that will try to clarify why it is produced and how to explain its direction on a global level. In other terms: to investigate if there is a generic guideline, some factor that, in one way or another, appears on a regular basis, and that, as a consequence, we have necessarily to take into account, in order to understand technological change as a whole. This is the main identifying factor of the philosophical perspective from a historical point of view. Could something similar be underlined with enough appearance of rationality?

It seems obvious to begin such an analysis trying first to indicate the possible spot on which the investigation has to be centered. Such a spot, if it is to match the level of generality mentioned above, has to be, then, a field which underlies the whole of the current technological "factum," that is to say, a field keeping within its own structure, from which it will be able to grasp some specific pattern or patterns of technological evolution. If it is a structural factor it means that it had to be an *internal* factor of Technology as a differenciated historical fact. From this viewpoint, the philosophical approach differs crucially from other approaches that come more directly from social sciences, in which the political, economic and social dimensions in a broad sense are especially predominant. These dimensions, in comparison, will insist more on external factors, that is to say, on the effects of technological achievements when they are applied to reality and on the different feedbacks that they can originate.

Of course, the external factors will be indispensable to complete correctly any "theory" about technological change and, as a consequence, anything will justify to disregard them without making the mistake of presenting an idealized model of technological development. This would be an explicative way uprooted from its enviroment of realization and, because of this, certainly obsolete in relation to the posed problem. So, the internal perspective –philosophical– will not only need the external perspective later but, as will be seen, it will introduce some relevant aspects in the theory to be obtained about technological change.

Due to all these reasons, we think it appropriate to answer the question from a philosophical perspective, bringing up the general question about whether there exists in general terms a model of rationality that impregnates technological work at a structural level. And it is not difficult to justify this question in this place, that is to say, that of technological rationality. Because, if it is possible to indicate its distinctive features, it would then be an internal factor, intrinsic if we want, to any technological work, which would have to be considered a common feature of any technological practice. So, to ask a direct question: What kind of rationality prevails in technological action due to the fact that it is precisely technological?

I have already described in detail this model of technological rationality in other contributions,[7] so we will only give here a summary which should be sufficient in order to reach my present objectives.

In the first place, the basic criterion of a technological rationality is not to answer in a simple way the question "What is this?," that is to say the ultimate essentialist question, but to answer to the pragmatic question "What is this for?" Any technological tasks interests specifically lie in the practical aspects of its products, so that a technological object is identified as such insofar as it exists in order to be operative with reality, being this whatever it is.[8] This is true to such an extent that the effectivity of its application is "conditio sine qua non" for it to be considered technological as such. This means that the constitutive criterion of technological rationality is the criterion of operational efficacy, that is to say, that of producing an immediate effect on reality according to the designed action (efficacy) and with the lowest general, temporal, or any other sort of cost (operational capacity). So, the technological dimension is basically the pragmatic dimension and not so much the theoretical dimension. As a consequence, this means that, at the level of the general structure of human knowledge, the investigation of the theoretical aims, that is to say, the answer to the question "What is this?," is subordinated to the investigation of the pragmatic aims, that is to say, "What is this for?."

But notice that we have used the word "subordination" and not "disappearance." Because, really, it is not that a technological rationality has no interest in the theoretical aims but that it makes them dependent on the pragmatic aims, namely,

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[7] Cf. QUERALTÓ, R., *Ética, Tecnología y Valores en la sociedad global. El Caballo de Troya al revés*, Madrid, Tecnos, 2003, chap. II, pp. 73-110; and also QUERALTÓ, R., "Cómo introducir vectores éticos eficaces en el sistema científico-tecnológico," *Arbor*, v. 162, no. 638, (1999), pp. 221-240.

[8] From an elementary particles accelerator to the so called technics for behaviour modification, also including an immense range of "intermediate" technological products, for example computer technology, cybernetic instruments, etc.

on its applicability to reality. Translated to more specific terms: the matter is not, for example, that technology is not interested in pure science, but that technological research will condition scientific research in terms of its efficacy in its application to reality. This is what is happening in the present process, very advanced today, of the transformation of *science in technoscience*.[9] It is evident that technology needs scientific knowledge, furthermore, technology is usually characterized as the technique derived from and inspired by science, but the main aim of technological investigation is not the description and possible explanation of the structures of reality, but rather its modification and transformation as a justificative requirement of this investigation. From here derives the fact that technological rationality subordinates necessarily the theoretical aim of human knowledge to its pragmatic aim. And all this by virtue of its criterion of constitution as a technological rationality, namely, the operational efficacy criterion.

From here, immediately, a second feature of technological rationality will be derived, which, as we will see later, will reach an extraordinary importance on the subject of technological change. The reason is that, because of its own internal structure, technological rationality is expansive on its own (autoexpansive) in an indefinite way. Indeed, if our first aim is the operational efficacy, what better efficacy can there be, at the epistemological level, than to cover as much as possible of the world from the point of view of such operational efficacy? In other words: if the criterion is that of operational efficacy, then a technological rationality will always tend to cover as much as possible of reality according to its forms of realization. It cannot act in another way if it has to comply with its constitutive criterion. Otherwise it will be no longer a technological rationality. For this reason, it will try to expand itself more and more everyday and to understand reality and all of its entities according to efficacy and operational capacity. In the limit, technological rationality will try to encompass the totality of the world and to understand it as "more technological." This is its intrinsic tendency, both at the cognitive level and at the operational level in a strict sense. For this reason, for example, there will be "more technology" every day, and not only because society and human beings will demand it. What Galileo said about science at his historical moment,[10] becomes true now in regard to technology too, because of more pressing reasons: technology cannot do any other thing but to increase. And this is due to internal and external reasons, namely social demand and its own internal structure.

Notice at this point how the result of philosophical analysis can be linked with other perspectives developed by different modalities of human knowledge, being able to cast some light on this phenomenon from its own point of view. From here

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[9] See the excellent book ECHEVERRIA, J., *La revolución tecnocientífica*, FCE, Madrid, 2003.

[10] Galileo said that science "could not do any other thing but to grow," cf. GALILEI, G., *Dialogo sui massimi sistemi ptolemaico e copernicano*, in GALILEI, G., *Opere*, ed. nazionale, a cura di A. Favaro, A. Garbasso, G. Abetti; Barbera, Firenze, 1929-39, 20 vols., vol. VII, p. 62.

it is possible to draw an important conclusion about technological change, and this is that it will always tend to increase the quantity and the quality of its effective efficacy on reality as a whole, man included. In other words: technological change, being inevitably inspired by technological rationality, will always tend to produce more and better control over reality, that is, not only a quantitative increase of the new technologies but especially a qualitative increase of them. This means that in turn technologies will be unavoidably more sophisticated and more effective. The social and anthropological consequences of all this can be really unpredictable both in their positive and negative aspects.[11] Thus it is possible to explain the crucial importance of giving to technological change a direction in accordance with the reasonable desires of the general anthropological welfare of today's human beings and societies, which involves directly ethical dimensions and problems of the first magnitude.[12]

However, it is precisely in respect to this matter about the direction of technological change where we can find another important consequence derived from the former discussions. If technology cannot do anything other than to grow and to expand itself, due to the structure of its rationality, then there is here a clear "philosophical" support in favour of the models of technological determinism in relation to technological change. Indeed, because if the constitution of the technological "factum" demands more and better operational efficacy, then the direction of technological change will not be able to be fulfilled except in that direction, despite the possible obstacles that it could find. It could seem as if at this point of the philosophical analysis an acceptable justification of the "technological imperative" formulated above would be discovered, namely "anything that is technologically possible to be done will be done." This situation undoubtedly increases the suspicion about technology and has provoked many pessimistic positions which are exaggerated, as we will fortunately see below.[13]

Notice how deterministic positions in relation to technological change are not emotive views; on the contrary they can be, at least in principle, firmly anchored on perfectly reasonable motives, regardless of whether they function explicitly or implicitly with better or worse effectivity. For this reason, I began with specific references to technological determinism, because its expansion is not only based on the verification of facts but on something deeper, which is

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[11] For example at the level of social welfare or at the level of other aspects that could be not included in it, such as an assault on privacy. All this is showed constantly nowadays. See for example: LOPEZ CEREZO, J. A. and LUJAN, J. L., *Ciencia y Política del riesgo*, Alianza Editorial, Madrid, 2000; GERGEN, K., *The Saturated Self. Dilemmas of Identity in Contemporary Life*, Basic Books, New York, 1991; SARTORI, G., *Homo Videns*, Laterza, Roma-Bari, 1997; and BUSTAMANTE, J., *Sociedad informatizada, ¿Sociedad deshumanizada?*, Gaia, Madrid, 1993.

[12] See QUERALTÓ, R., *Ética, Tecnología y Valores en la sociedad global*, second part, pp. 159-202.

[13] It is the case for example of ELLUL, J., *Le bluff technologique*, Hachette, Paris, 1987; and ELLUL, J., *Le système technicien*, Calman-Levym, Paris, 1977.

inherent in the internal structure of the rationality that characterizes Technology and technological change.

Nevertheless, my critical remarks on technological determinism will be based on the fact that, for a full understanding of the real and effective conditions of technological change, it is not enough to analyze the present situation and the subsequent inference of the internal power of technology as such. It will also be necessary to consider the analysis of the feed-back circuits between technology and Society, and to take into account the result of a philosophical reflection about the conditions of the realization of technological actions as such, that is, conditions without which there can be neither technological fact nor technoscience.

Before, however, it is advisable to finish the inquiry on the selected features of technological rationality that are significant regarding the problem of technological change. For this reason, finally, a third suitable feature of the subject will be emphasized. The fact is that technological rationality is obviously a rationality that transforms and modifies reality, and it cannot be otherwise. This is also derived from its criterion of operational efficacy, and from other reasons derived from the most immediate experience. Indeed, operational efficacy, to be itself, cannot "fall into the void" but produce its results on reality, in order to be identified as such. The opposite would be to deny its operational dimension, and so to certify that we are not in the presence of a phenomenon of technological rationality, but in a case of some other type of rationality.

Nevertheless, it could be argued, eventually, that any type of rationality also matches this feature, because, in the long or short term, this is what happens regularly with the achievements of human knowledge. Without entering now into too many theoretical discussions, because this would divert us from our main objective, we will answer this possible objection by underlining a fundamental difference in technological rationality. And this is that it transforms and modifies reality "prima facie," that is to say, it builds itself to be that, and it is this and no other its specific way of being. It tries to operate on reality because without this fact it will not be "born" as technological rationality. Certainly, this is not the case with other forms of rationality in which their possible factual applications are not indispensable requirements for their constitution, but on the contrary they arrive at this point afterwards, at much later phases of their development. Furthermore, this requirement does not work as a condition without which they cannot even be thought of as rationality. On the contrary, we have already repeated that operational efficacy, and also autoexpansiveness, are identifying factors of technological rationality.

In this line, it would not be an exaggeration to affirm that technological rationality tends to work, using a classical philosophical term, as a sort of "will of power" (*Wille zur Macht)* over the world. This is the reason to reinforce again the understanding of the deterministic positions on technological change and the apparent viability of the technological imperative in its strong version. A different matter will be that the world itself will not counter with some resistance and conditions, and that technological rationality will eventually be able to overcome all of them. This latter fact will have to be analysed carefully.

In conclusion, it could be said that technological rationality is a clear example of pragmatic rationality, in which the operational efficacy, the self-expansion and the intentionality of transforming and modifying the world, are features that characterize it perfectly with unavoidable identity marks. Furthermore, it would be a "strong" rationality and not weak, insofar as its *proved* results in efficacy and operational capacity are requirements that specifically distinguish it from other possible forms of rationality.

This is the state of affairs, looked at from the inside, that is to say, from the internal structure of the technological phenomenon and its intrinsic kind of rationality. It will be necessary to take this into account in order to follow up the analysis of technological change, without the risk of building castles in the air. However, these are not the only factors that are necessary to consider cautiously. The matter is more complex. Because now it is also necessary to "get out," let us use this expression, from the interior of the phenomenon. And its exterior is going to add a set of new elements that are unavoidable, and that, possibly, will bring us some surprises.

3. SOCIAL CONDITIONS AND TECHNOLOGICAL CHANGE

It could seem a banality to begin highlighting now that, to understand the phenomenon of technological change, it would be necessary to analyze the social dimension of technology, that is, the fact that technological development is realized for a given world and not for another and, as a consequence, it receives influences from it and is conditioned by it. In short, it becomes a feedback cycle. But, as it will be seen later, it is not in vain because it is indispensable to consider this issue if we want to avoid falling in a biased position into relation to the understanding of technological change.

It is true that there is an intrinsic tendency in Technology to magnify the operational efficacy of its action and its products. But it is not less true that the society to which it is directed also imposes determinations, not strictly technological, which will influence this intrinsic tendency and will provoke accelerations on many occasions and resistance on many others, with respect to technological change. And what we are trying to do is to calibrate the reciprocal influences by means of some reasonable conceptual instruments in order to arrive at some patterns of direction of the present technological change. It would not be appropriate to give preference, deliberately or not, to one of the factors involved, because in both cases we would fall into an idealistic vision of the phenomenon, arriving at either a strict determinism or at a "sociologism."

It is necessary to enumerate several social fields. The first of these is economic. To innovate in technology it is today necessary to make important investments, and this is a factor that conditions the matter from the beginning. The times of cheap technological investments are gone, especially because, to "sell the product," that is to say, to make it definitely effective at a social level, it is indispensable to develop not only processes of scientific-technological research that need large economic funds, but also a very sophisticated and sutile marketing. These are the conditions of the subsequent competitiveness. And this is expensive undoubtedly. This fact means that the significant technological development will be in the hands of the state or of the big private corporations, which are often transnationals. And so, the principal agents of the current technological change are the so called technoscientific companies.[14] A brief description of these will help us to visualize that circuit of feedback between society and technology pointed out at the beginning of this section.

A technoscientific company is a complex group of specialized agents who coordinate themselves in order to reach common objectives of technological production. This group consists of scientists, technologists, managers, economists, programmers, permanent evaluators of the process, etc. The most important point to underline here is that the production of *science* and *technology* is no longer the work of a homogeneous company –the group of scientists or technologists only, for example–, but it is a collective and heterogeneous practice, namely, a global action in which, in addition to pure researchers, there are other agents that organize the processes of innovation and production. The task of these agents is often to control, each one in its own field, the success of the whole system. The scientific-technological work is no longer the result which stems only from a research laboratory, but from a number of actions carried out by many specific agents, which are very different from each other.

The main pattern of rationality operating in this system is obviously to achieve its planned objectives, and this means, from the economic dimension, to obtain the best costs-profits rate with the technological product. Without a doubt, many will argue that this is another clear example of technological rationality, because here the criterion of operational efficacy is defined in terms of the maximization of such a rate, namely, to obtain the largest possible profits. Therefore, it seems that we are returning to determinism, given that the technological system taken as a whole would determine the direction of technological change, in virtue of the conditions of its socioeconomic praxis.

However, this would be a very superficial vision, because the purpose of any technological endeavour comes not only from within Technology itself, but also, if it is to be somehow beneficial, the technological demands coming from society must also be taken into account. It must be determined which of these demands

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[14] See the analysis in ECHEVERRIA, J., *La revolución tecnocientífica*, pp. 61-83.

is actually possible to develop. This should be considered at two levels: (1) Which demands are technologically practicable and (2) which ones stand out for having a presumed greater economic benefit when facing a specific social requirement. Therefore, technological innovation and change are not only produced by a potential which originates more or less from Technology as a phenomenon, but rather from the considerations that must be taken into account in technological praxis as a technoscientific system, in which this cycle of feedback, previously indicated, is produced.

One can see that these two levels become apparent from the very beginning. On the one hand, there is the intrinsic tendency within technology to maximize operative efficacy, which is a common theme throughout the whole of the technoscientific system. And, on the other hand, there are the demands of practicability on the part of society. So, from this point of view, i.e. from the analysis of the impact of economic dimensions on technology, the suggestion of a tempering force emerges in respect to the technological imperative mentioned above. Just because something is technologically possible does not mean that it is going to occur. Rather certain endeavours will be undertaken, or not, in function of social references and requirements, which are not strictly technological.

For this reason, given the complexity of actual technological praxis, it is perhaps convenient to propose another formulation of the initial technological imperative, which we shall call "the weak technological imperative." This would state that "anything that is technologically possible to be done will tend to be done." Yet this does not mean that it will be done unfallingly. Nuance is of radical importance here, because now the door to the possibility of social influence on technological change has been opened. It is not solely resigned to the mercy of its internal urgings. The weak statement, on the one hand, recognizes the previously highlighted intrinsic tendency of technology, and on the other hand alerts to the possibility of influencing such a tendency through other, not exclusively technological factors.

One reaches a similar conclusion when analyzing the political influence on technology. This influence is directly intertwined with the current economic situation, yet it adds some specific circumstances, especially in democratic states. This is due to the fact that, in effect, in democratic organizations, there is an element of social control on what the government does (even though, as everyone knows, there is still a lot of progress to be made in this area). If it is to be true to the pattern of specifically political rationality that operates today, then here the sensibility of the politician and of political action in relation to social demands, must be more pronounced. This means knowing how to keep power and possession of the government. Not heeding urgent or majority-held social demands could provoke the loss of this power. For this reason, it would be contrary to primordial political objectives.

Without a doubt, one could argue that such a vision of general political objectives is much too simplistic, and that these are more likely to be the achievement of greater social well-being in a generic sense, or of the common good in a classical sense. But let us not fool ourselves with respect to our times. It is true that greater and greater levels of social well-being are being achieved, and that the government depends on this progress for its own development and therefore obviously fosters it. Nevertheless, this is not its ultimate goal. The government's fundamental reason for sponsoring social well-being, plainly stated, is simply because if it did not do so, it would lose its political power in the next elections, or maybe even before then. And, in function of this basic motivation, naturally it seeks that the highest levels of social welfare be reached. The very survival of the goverment's political power depends on it, and this survival constitutes its first and last specific goal.

This poses a clear example of the interconnectedness between social elements and technological ones. Because on the one hand, preserving power as an ultimate end is an obvious example of technological rationality in politics: the operative efficacy, on the political level, demands holding onto power. If it were any other way, this would be a failure, techno-politically speaking (if I can be permitted to coin the term). Therefore, this obligation of techno-political efficacy requires that these social demands be met, and amongst these demands, technological action; precisely in order to satisfy its own pattern of rationality of contemporary political activity. Because, if it were not so, the result would be a total failure, both technologically and politically.

Of course to many people, this way of behaving and conceiving political power will seem morally insuficient because, in fact, the parameters of social well-being are manipulated with the previously mentioned ulterior motive, which is a clear feature of a technological rationality applied to politics. Yet, things are the way they are and not the way we wish them to be. Philosophically speaking this is the cruel divide between what is and what should be, and we have no choice but to bear with it. What one cannot do is to be unaware of the situation.[15]

But, such a situation is not as negative as some have believed. Because what is clear is that for better or for worse motives, the door to the possibility of taking action on technological change is opened after this brief description of the political extent on technology. And this is a matter of extending these possibilities to the maximum.

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[15] This is just the background of the book *Ética, Tecnología y Valores en la sociedad global,* in which it is shown that, despite of these obstacles, it is possible to introduce ethical values in scientific-technological systems and in technological society as a whole. In this regard, cf. also Queraltó, R., "Cómo introducir vectores éticos eficaces en el sistema científico-tecnológico," pp. 221-240. Furthermore, in order to envisage the important ethical changes provoked by technology, see Queraltó, R., "Ética y Sociedad tecnológica: pirámide y retícula," *Argumentos de Razón técnica*, v. 5, (2002), pp. pp. 39-83.

Moreover, the political example shows something that is perhaps unexpected. And this is that again and again in our times, technological rationality acts as social rationality. It is not as such an insensitive and monolithic block. But rather by being forcibly directed towards the transformation of reality, it must necessarily adapt to this reality in order to be successful in its own self-demand of efficiency, all which implies that technological rationality, so to speak, has to moderate its "natural" tendency, in order to be efficient as a strong pragmatic rationality. So, its operational efficacy requires a certain adaptability in turn. This means, for our purposes, that the very philosophical analysis of technological rationality is an unavoidable consequence of the influence of the implicated social factors as a whole. Thereby, the possible model of technological change must necessarilly include these factors and not rely solely upon an explanation in which the most important thing is a supposed immanent quality of technology in itself, as a definitive motor for technological change.

Because of this perhaps now we should introduce one more element in the formulation of the "technological imperative," which would add nuance to the last tested meaning: "Anything that is technogically possible to be done will tend to be done in function of social conditions and requirements."

In the same manner as before, this formulation refers to the intrinsic tendency of Technology, which we have recognized from the beginning and which has been philosophically justified, and it also respects the necessary placement of technological activity in concrete situations. These situations would influence technological development, shaping the whole into a permanent system of feed-back. This is where, in our opinion, the problem of the direction of technological change should be centered.

Two relevant consequences can now be pointed out as a general result of this analysis. The first refers to the possible model of global rationality. This model would encompass technological action as such according to its intrinsic tendency to impose its specific traits, and at the same time it would include the influence of other factors that are implicated but that are not technological in a strict sense.

Such a model should provide elements that make the direction of technological change understandable. With such premises, this model of rationality cannot perhaps be other than a systemic model, meaning a systemic rationality. This means that technological change can be conceived as a system, a group of elements in interaction, that make up a network of participants which mutually influence one another through the complicated relationships that are developed, with the final result of producing determined technological products. Two points deserve to be highlighted here. On the one hand, there is a system which is a dynamic entity. Put in other words, it is in continuous internal movement, which produces permanent mutations in its possible products-the output of the system. On the other hand, in order to carry out its funtions and systemic achievements, the system should include, arrange, and integrate into a whole, the globality of demands which come from its elements and reciprocal relationships. If this second condition is not satisfied, the system will simply not work. It would cease to be a system as such.[16]

However, when we find systemic elements that contradict one another, or that are at least not naturally integrable, as is usually the case in the event of technological change, the mutual influences will cut off the specific protagonism of such elements until a certain agreement is reached in order to somehow advance. It is theoretically possible that one of these elements, or a similar group that works as one element, imposes its decisions. But, it is unlikely that the influence of the remaining elements would be below a minimum threshold which would make them fall into absolute inefficiency. And this is precisely because the technologial action, designed by its own primordial nature, is a direct intervention on reality. As a consequence, this must be considered in order to justly fulfil the internal criteria of rationality, namely the operative efficacy on this or that reality. So, from a systemic rationality technological determinism is excluded, because such a conception would be incompatible with such a model of rationality, given that technological determinism would only highlight a single sector of the network of actors to be considered in the matter, that is, the technological sector in isolation. But the necessary relationships with other implicated sectors, such as political, social and others, would not be taken into account to a sufficient extent.

This last consideration leads to the second consequence which it is necessary to examine. In order to do this, and to remain faithful to the point of view adopted here, I will make use of a well-known philosophical aphorism, the Ortegian principle of "I am me and my circumstances." After the previously expounded, it is not hard to understand the principle's application to our problem.

In effect, technology and technological change are not only themselves as such but "they are themselves and they are their circumstances." In other words everything that surrounds them and influences them with greater or lesser intensity, in the measure of the previously mentioned feedback cycle as well as the model of systemic rationality. So to speak, technology is not technology, but rather, for its proper understanding, "technology is itself and its circumstances." Few doubts can arise in this respect if we accept the general results of the inquiry thus far.

But the Ortegian statement includes a second part whose undoubtable reach has unfortunately not been much considered. Because the Ortegian idea, formulated in its totality, states the following: "I am myself and I am my

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[16] Cf. Laszlo, E., *Introduction to Systems Philosophy*, Harper and Row, New York, 1973; Laszlo, E., *The Relevance of General Systems Theory*, Braziller, New York, 1972; Agazzi, E., *I sistemi fra scienza e filosofia*, SEI, Torino, 1978; and Agazzi, E., *Il bene, il male e la scienza. Le dimensioni etiche della impresa scientifico-tecnologica*, Rusconi, Milan, 1992. Of course, the well known bork by Bertalanffy, L. von, *General Systems Theory*, Braziller, New York, 1967.

circumstances, and if I do not save my circumstances I will not save myself".[17] For the issue at hand, the consequences are multiple. In the first place, the aphorism adapted to technology would read as following, "technology is technology and its circumstances, and if I do not save the circumstamces of technology, technology will not be saved". As a whole both statements mean that in order to save myself, the circumstance in which technology unavoidable operates has to be saved too, and consequently technology itself has to be saved in the measure that the set forms an integrated whole, all functioning together at the same time. Because, let us not fool ourselves with excessive pessimism or optimism: either technology is better or worse integrated with its circumstances, in other words with me and with the society that it addresses, or in the end technology would devour itself in a technological chaos with no detectable direction. And this, in reality, is the heart of the question, the manner in which the matter of technological change and its social reach are presented. Often repeated statements such as "humanizing technology" or "putting a human face" on technological development converge on one point, and that is the social control of the direction of technological change.

And now that we have arrived at this point, what can we say about it from a philosophical perspective, as adopted here, without getting away from any of the main factors that protagonize the current described scene?

4. TECHNOLOGICAL CHANGE AND THE ASSESSMENT OF VALUES

There can be no doubt that the plane chosen here from which to develop the philosophical analysis of technological change is the pragmatic one. Technological rationality, we concluded above is a strong pragmatic rationality, and the cycle of techno-social feedback, previously mentioned, logically comes into contact with the praxic dimension as a preferencial area for philosophical investigation. From here the question posed has to be resolved according to its pragmatic placement.

From a current philosophical perspective, one cannot be surprised by such a placement. In effect, for some time the so-called pragmatic turn in today's thought, has been noted. A worthy example of this is the transition from the received view in philosophy of science as logic and epistemology in general to the philosophy of scientific action as a central orientation of such philosophical branch. This contemporary situation has widely broadened the quality of the problems to be analyzed, no longer circumscribing them to only the traditional realm of a logical, methodological and epistemological nature, but rather admitting aspects that are ethical, political, economic, etc., which before were considered foreign to the philosophy of science.

Without a doubt, one of the most significant aspects that has manifested itself in this line has been the analysis of all sorts of values implicated in the work of

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[17] ORTEGA Y GASSET, J., *Meditaciones del Quijote*, in ORTEGA Y GASSET, J., *Obras Completas*, vol. I, Alianza Editorial, Madrid, 1983, p. 322.

today's science and technology. (It has reached the point where some distinguished scholars have proposed, as the main task of contemporary philosophy of science, the elaboration of an axiology that encompasses all of the influential aspects of the scientific-technological processes, from logical and epistemological dimensions to ethical and social ones in general).[18]

The justification of such a perspective at first sight might seem too unidirectional. The notion of value had been relegated preferentially to the philosophical realm of ethics and social and political philosophy. So, in a first approach, if one focuses the investigation on axiological issues, it could be argued that in reality we would be going to the other extreme of the situation. If before the constellation of problems was of a logical and epistemological order, with the exclusion of ethical, social and political questions, now the situation has been reversed and, as a consequence, the traditional analyses that have been so central to the reflection of the philosophy of science and technology, would be excluded from the philosophical problematic. In the end, it could be concluded that the deficient "law of the pendulum" is being fulfilled here, swinging from one pole to the other, thereby producing harmful exclusions on both sides.

Yet fortunately we do not believe this to be so. To be fair, the pragmatic perspective operates in this case from the beginning with a notion of value that is rather different from the corresponding traditional notion of philosophy. From this inherited position, without a doubt, the proposed critique would be correct, but precisely what happens is that in the pragmatic vision, this initial premise is very notably modified. It is here where the cards are definitively played, that is to say, in the meaning of the central axiological notion, namely, the notion of value.

Because the pragmatic notion of value is no longer to be considered in the classic sense, as something that should be done because of its intrinsic quality, which justifies itself for being held up by a more or less definitive transcendental level. But rather the pragmatic conception understands value as a guideline for solving problems.[19] In other words something has value as long as it constitutes a pattern or rule for overcoming problematic situations of very diverse natures. This way of understanding value is consistent with a pragmatic perspective, because the realm of praxis in general and its analysis take root as its own position in a wide field of evaluations of actions in general.

However, from this basic premise of the pragmatic notion of value, there is no difficulty in integrating all of the previously mentioned dimensions. Because, effectively, in current scientific technological activity, problems of all these types are considered: ethical, political, etc. So, a correct epistemological rule will have

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[18] This is the case, for example, of ECHEVERRIA, J., *Ciencia y Valores*, Destino, Barcelona, 2002, ch. I, pp. 29-116.

[19] On the relevance of this position, see LAUDAN, L., *Progress and Its Problems. Towards a Theory of Scientific Growth*, University of California Press, Berkeley, 1977. On Laudan's philosophy, cf. GONZALEZ, W. J. (ed), *El Pensamiento de L. Laudan. Relaciones entre Historia de la Ciencia y Filosofía de la Ciencia*, Publicaciones Universidad de A Coruña, A Coruña, 1998.

value in the measure that it solves, in determined aspects, the problem of truth or the epistemic validity of a statement, a law, or a scientific theory; a research procedure will have value in the measure that it solves methodological problems; a political decision in reference to a technological action will equally have value in the measure that it solves problems of innovation, development, and direction in a determined technological task. It is precisely the generality of the pragmatic notion of value which enables it to propose an encompassing aspect of all of the dimensions implicated in tecnological phenomena in general, without excluding any a priori, at least in the beginning.

For this reason it is not reductive to propose the general analysis to contemporary technoscience in terms of analysis of values, being sure to use the practical dimension in all of its amplitude and demands.

Now, this does bring up a problem of chief importance, whose analysis will provide the answer to the question raised at the end of the last paragraph, and which will be developed in the last part of this contribution. It is related to the general problem of the assessment of values in techno-scientific processes, and therefore in the direction of technological change and their possible patterns of understanding.

From this position, and for the purpose of grasping which way to direct oneself and how to carry out technological change, it is necessary to procede with an assessment of the implicated values. In other words, it is necessary to procede to a consideration of the implicated values which would make reasonable the taking of a decision for one or another technological objective. If before it was said that the rationality of technological change could be analyzed by means of a systemic model, then this evaluation could be carried out using, at least initially, the features of such a model. This immediately implies various important consequences.

In the first place, values should be selected in function of a concrete problem, with the objective of centering the evaluative analysis. Secondly, nothing which is identified as significant should be initially excluded, whatever its nature may be, whether it be epistemological, ethical, political, economic, etc. In formal terms, one could write that technological change, as it is composed of a system of technological actions, could conform to a system of values:

$$Sv_i$$

In the third place, it is necessary to somehow measure the set of values, that is to say, to quantify them to a certain extent. And this is where, without a doubt, the most serious problem arises. Because, is such a thing possible? Can one indeed measure a value? That depends on how a value is conceived. If one adopts a notion of value according to classic axiology, it will be rather difficult, because with this a justified scale of values is internally produced by the ultimate transcendental “crown” from which the corresponding architecture is established. This would logically involves a certain rigidity. All of this is internally coherent with such a point of departure.

In the case of a pragmatic vision of value, the guidelines for measurement will be to assess as to what measure and at what costs –of all kinds, not only in economic terms– the problem or problems are solved by using one or another system of values, **Sv** or **S'v'**. For example, it can be stated that **Sv** highly satisfies epistemological values, but perhaps it falls short in technological values (in other words, in specific operative efficacy) or these last values, in this specific technoscientific event, suppose an important decline of the presence of social-economic values (or viceversa). In sum, the possible situations are multiple. For this reason, it all comes down to assessing and deciding which values are to be pondered with more attention in each technological event that we consider.

This way, we can even write inequations of the kind "more than" or "less than" with respect to the values implicated in one direction or another,

$$Sv > S'v' \quad \text{or} \quad Sv < S'v'$$

Of course we cannot pretend with this calculation to reach an exact mathematic solution, as we could determine physical magnitudes such as the cinetic energy of a motion. But, one could establish a certain measurement that would suggest the global viability of one technological endeavour or another. And this is the ultimate purpose of the value assessment. Evidently these inequations would not have the mathematic exactitude required in other dimensions of techno-scientific action because its own object, the elucidation of the capacity to solve systemic problems in which values of diverse nature are implied, would impede this. Such a pretension would be suspicious even, because it would go outside of the framework of the problem itself. But, all the same, it would be neither adequate nor necessary. What the value assessment tries to do is to find reasonable guidelines for decision making. And we think that this analysis, necessarily brief for this occasion,[20] can suggest it conveniently.

One should not believe this procedure to be completely new, because Popper, for example, asserted that in order to allow non-scientific ideas (primarilly philosophical ones) into scientific research, the justifiable method should not be the usual empirical constrastibility that we find in science. But rather it should be an analysis that leads to establish whether or not the idea in question resolves, for better or worse, the scientific problem at hand. This is precisely because the nature of such ideas makes empirical contrastibility impossible, even though these ideas were always present in the elaboration of scientific theories.[21]

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[20] A more detailed elaboration can be found in QUERALTÓ, R., "Science as Technoscience: Values and Their Measurement," *Actes du Colloque de l'Académie Internationale de Philosophie des Sciences*, 2003, Lecce (Italy), forthcoming.

[21] POPPER, K. R., *Conjectures and Refutations. The Growth of Scientific Knowledge*, Routledge and Kegan Paul, London, 1963, chap. 8.2, pp. 193-200.

To conclude, it is a question of carrying out a dialogue of assessing in which the significant elements are weighed in terms of their mutual influences, reciprocal restrictions, social effects, and above all, the evaluation of problems and their levels of solution.

However, one can now ask, respecting technological change, which parameters, whose presence is regularly produced, can be outlined. Accordingly, these would have to be taken into account regularly. It is a matter of there being a hard nucleus of pragmatic values (if the expression be allowed), which in fact would serve to identify the question of technological change as such, and thereby to condition the analysis specifically.

We believe, effectively, that such values could be indicated without difficulties.[22] In the first place, undoubtedly, one may point out the technological values par excellence, that is to say, the operative efficacy and the self-expansion inherent in Technology. It should be clear from the beginning that these values as such would be neither rejectable nor acceptable in all of their potentiality through their intrinsic quality. At this point, is it reasonable to doubt that efficacy is desirable and indispensable for every technological endeavour and that self-expansion is concomitant with the technological phenomenon? For this reason, their presence and influence must be taken into account, but these will have to be counterweighed in function of the systemic whole in which they are found.

In the second place, the epistemological and methodological values stand out as well because of their regular importance for the attainment of technological achievements. This is the reason by which these are unavoidable for the adaptation to reality that any technological event requires. Yet, likewise, they cannot be considered in isolation because a particular methodological line of investigation could be preferred over another alternative line leading to the same result, in function of the lesser or greater economic impact in its concrete development, or in relation to political or ethical-social values.

In the third place, we find the ecological values. These are to be considered, given the extreme incidence of technological activity in the social and natural environment, whose harmful effects are amply demonstrated and reported. In this point, for example, a great deal of operative efficacy as a technological value would be rejectable if the value of the corresponding environmental impact is excesive or intolerable. As is well-known, often the presence of these values is acquiring greater weight in political decisions of medium or high reach, in good part as a justified reaction to the massive proliferation of harmful industrial waste, for example, which was almost completely uncontrolled until recently. Here it is shown, as a matter of fact, how technological values in a strict sense are not those which always determine the direction and actions of technological change.

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[22] A detailed analysis of this subject can be found in ECHEVERRIA, J., *Ciencia y Valores*, pp. 117-210.

In the fourth place, the political and economic values that are usually intertwined, which, as has been indicated, are going to be continually present given the growing transformation of science into technoscience and of the previous scientific-technological praxis into a new form of praxis characterized as techno-scientific enterprise. In this section, as an unavoidable value, one has the better cost-benefit relationship, which in the same way as before will be tempered by the influence of other systemic values.

And in the fifth place, last but not least, there are the social-ethical values. Globally the value of social well-being could be considered here as representative in its multiple dimensions, and of course, without reducing it to its material value. To the contrary, in this group, moral values would acquire a specific consideration, including civic and religious values. Because without including them, not only would there be an undesirable lack of control of technological potentiality in a strict sense, but sooner or later, there may be an excessive reaction against technology itself, as occurs in certain social phenomena such as technophobia, which perhaps has been provoked by the intent to impose technological initiatives and direction, overriding social-ethical values. This is not good for any of the implicated parties. Besides, in the case of technology, this would represent precisely a certification of operative inefficacy, which, from the philosophical perspective taken here, would be the worst thing that could happen.

Nonetheless, it is worthwhile to emphasize some final points which will make up the conclusion, provisional still, of this paper.

5. CONCLUSION: TECHNOLOGICAL CHANGE AND ITS SYSTEM OF MEDIATIONS

We believe that the set of preceeding philosophical analysis has proven that the technological phenomenon and the problem of technological change require the use of a model with a systemic character. This should be able to address all of the complexity of the current situation and its significant dimensions in each moment.

The main consequence of this perspective is that there are no directions of technological change which are justified in an apriori manner by themselves. This can be neither the option of an accused technologism nor that of a sociologism that ignore the very structure of technology and its implicit rationality. Undoubtedly, this broadens the possibility of sustaining that social intervention on technological change is real and viable.

However, quite a different question is raised by asking ourselves for the current situation if we try to visualize a general panorama of this intervention. Without undermining the previous conclusion, whose practical consequences are still in their initial phases of development, it must be admitted that, up to now, a systemic balance within the mentioned feedback loop has not be found. In other words, it seems that the intrinsic potentiality of technological rationality and technology as a historical phenomenon ocupy a predominant role and this, we think is an important reason behind technological determinism. From the historical-empirical point of view, this certainly does not lack support today. It is not an exageration to say that strong pragmatic technologically-inspired rationality, works efficiently on many relative social-cultural levels and acts perhaps, increasingly, as the social rationality of our era. This is not a small matter. Precisely from this fact we have departed in other writings from analyzing the ethical question in a strict sense.[23] On other levels, such as the political level, for example, it is evident that access to technological means is considered an indispensable factor in the global development of a country in the contemporary world, by which the technological transfer constitutes a fact that has been analyzed for decades in many of its possible problematic dimensions. Examples of this kind, which show the vigor of the technological factor in a strict sense, could be multiplied, almost to infinity.

For this reason, deterministic doctrines of technological change have supposed a first contribution to the general analysis of the phenomenon. It has fulfilled an important historical function, which is that of warning about the possible deviation from a more balanced direction in the favorable future of technological change. One should consider, after all that has been shown here, that the deterministic vision is in an initial phase of interpretation of the phenomenon whose base of maintenance is rooted in a detectable social-historical situation up to the present.

However, this does not mean that we must stay here, especially when so many uncontrolable negative effects of technological events have occurred, such as environmental disasters, radioactive leaks, attacks on privacy by abusive use of computer technology, etc. Consequently, it is necessary to react conveniently on all possible social fronts, and also on our specific point of view, namely, research in a strict sense. It is in this line that this contribution has to be considered.

In effect, it is important to show that the reflexive analysis of the problem uncovers the possibility of other points of view from which to consider it openly. The proposed interpretative model, as we have stated before, justifies the possibility of intervening with efficiency from the social context to imprint a certain specific direction on technological change, or at least to try to. Because (now we can sum up with a more philosophical conceptualization) technology always assumes a system of mediations, above all of a social nature. This signifies that these mediations are not “added” or “juxtaposed” to technological phenomenon as such, but rather to the contrary, it is something that forms an intrinsic part of technology itself as an historical fact. Let us look briefly at what this precision means.

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[23] For example, in Queraltó, R., *Ética, Tecnología y Valores en la sociedad global*, pp. 73-109 and 150-158.

Technology is not only, as is often said, an instrument to be used by man, but rather as something more relevant. It is a fundamental mediation between man and the world, and between man and life. The difference is decisive: an instrument is something that is there, outside, it is used in a determined moment or situation and when the action is considered finished, it is returned to the previous place. However, a mediation is something that is always there, whose presence is therefore continuous, and whose action sinks its roots into the intrinsic structure of man. Not in vain has it been repeated ad infinitum that man is a technical being.[24] It is convenient to never lose sight of this essential anthropological character of the technical and technological dimension,[25] because if this is not done, the specific reach of technology as a radical and concomitant fact to humanity will never be understood.[26]

For this reason, and in second place, in the measure that the technological is a mediation, it will always include the two poles, the strictly technical one and the human and social ones, combining them into a single element. From here it can be pointed out with all propriety that technology unavoidably involves a system of mediations that make it into this circuit of feedback that we have higlighted from the beginning. And this is not only because of a simple empirical observation of reality, that is to say, because technology is always applied to a concrete world, but also as a consequence of its very nature and as a result of its internal structure. This is what we have wanted to show with our philosophical inquiry.

Indeed, if the preceding arguments are correct, then the final question of this paper can be no other than the following: What conclusion is to be imposed in order to rebalance the direction of technological change which seems to be currently slanted by an excessive influence of strict technological parameters (operative efficacy, etc.)? The answer without a doubt is very simple, we must take maximum advantage of the character of mediation which is intrinsic in technology, especially in its social aspect. And how can this be settled at the present moment?

Obviously, by increasing the control of society on technological change and development. This would mean promoting opportunities for debate and decision-making in which all of the implicated actors are represented. In many cases, this

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[24] Only as an example: "Wir *haben* nicht die Technik, sondern wir *sind* sie!," in SACHSEE, H., *Technik und Verantwortung. Probleme der Technik im technischen Zeitalter*, Rombach, Freiburg i.B., 1972, p. 49. We refer to this quotation because it belongs to one of the first German authors that analysed this subject. It can be found in an old work, all which can add a special significance.

[25] A well accepted difference between "technology" and "technique" is the following: technology is technique derived from and inspired by scientific knowledge, but not viceversa, because science is an historical phenomenon starting from 16th century, but before there were other techniques in fact. Nevertheless, at present it is possible to assert that every technique is also technology due to the unavoidable interconnection between them. Thereby both terms are used in a similar way.

[26] An inquiry about this fact can be found in QUERALTÓ, R., *Mundo, Tecnología y Razón en el fin de la Modernidad. ¿Hacia el hombre "more technico"?*, PPU, Barcelona, 1993.

task is already being undertaken, especially in the institutional field,[27] yet there is still a long way to go. What is most important is to draw attention to the fact that technology and technological change are not only the work of scientists and technologists. These professionals are not the only ones who apply technology, rather this is made up of a complex productive system where agents of varied nature take action, from technoscientists to managers, social leaders, etc. For this reason, it is a matter of a collective effort in which society as a whole cannot remove itself nor be removed. Because to the contrary the entire technosocial system would suffer, and everyone would lose out, including *technology* in the strictest sense.

In the academic world, a reaction is also being produced along this line, which can be seen in the proliferation of studies about technological risk,[28] evaluation of technology, etc.[29] It is a question of proposing models for appraising the risk to be undertaken from one or another technology, and thereby of assuring reasonable guidelines for decision-making. Other studies follow the same line of inspiration, that is to say, the necessary connection between technology and society with all of its consequences. In general, the social control of technological phenomenon occupies the central place.

The indisputable difficulty of this task should not raise suspicions, even though, as we have recognized before, currently the intrinsic tendency of technology to impose and expand itself has the advantage, marking its own direction. Let us consider this situation almost "natural". In fact, when facing a new phenomenon, which is exactly what the appearance of the technological power is, there is an initial phase of expectation and social displacement in general, in which the new phenomenon advances, almost unstoppable. This is why positions of pessimism or impotence have arisen. This is where technological determinism has first seemed plausible.

However, things do not have to be this way. I have tried to demonstrate and justify this philosophically in these pages. Things are possible when they are dispassionately proven to be so. I have attempted to make this point evident

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[27] For example, science and technology policies, joint working groups in ministries and parliaments, joint research projects, etc.

[28] Cf. the work by Lopez Cerezo, J. A. and Lujan, J. L., *Ciencia y Política del riesgo*, Alianza Editorial, Madrid, 2000, as well as the following quotation and the selected references at the end of this paper.

[29] Cf. Winner, L., *Autonomous Technology: Technics-out-of-Control as a Theme in Political Thought*, The MIT Press, Cambridge (MA), 1977; and Winner, L., "Do Artifacts have Politics?," *Daedalus*, v. 109, (1980), pp. 121-136. In addition, Prof. Winner is promoting a "Center for Cultural Design" in Technology at the Rensselaer Polytechnic Institute Institute of New York. His aim is searching for the necessary balance between technology and society, which is just the main conceptual background in our text.

Cf. also Shrader-Frechette, K., *Science Policy, Ethics and Economic Methodology*, Boston, Reidel, 1984; Shrader-Frechette, K., *Risk Analysis and Scientific Method*, Dordrecht, Reidel, 1985; and Shrader-Frechette, K., *Risk and Rationality: Philosophical Foundations for Populist Reforms*, University of California Press, Berkeley, 1991.

from our own research field, that is to say, the philosophical field, because it constitutes, collectively conciously or unconsciously, another fundamental ingredient of our culture.

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