Economists as Subjects: Toward a Psychology of Economists

Wilfred Dolfsma
Erasmus University & Institute of Infonomics

Abstract *Economists can reflect on their own field of research and themselves in a number of ways. The philosophy of science has long been the dominant way to reflect on the work of scientists, to be joined in more recent times by both the sociology of science and the rhetoric of science. In this paper I do not argue that these approaches are wrong, but I do argue that they should be complemented with a study of the individual scientist. A psychology of economists, in other words, is called for. One important theory in recent psychological literature (social learning/cognitive theory) is introduced as an instance to indicate what kind of suggestions concerning the reflective position of individual scientists might be derived. It would be preferable from this perspective that scientists set high standards for themselves, have an open mind to what happens in different disciplines, and set high standards by which to judge others. Then follows a discussion where some potential objections to the approach in general, or to the specific psychological theory in particular, are refuted.*

• • •

Methodologists of economic science and economists themselves have observed that economists are not reflective: many economists shut their eyes to the way in which they themselves tend to behave. Economists tend to be confronted with questions about their own position in society when their views and recommendations are not explicitly taken into account; (more) in policy making or by policy makers.[1] There are, of course, several ways in which a scientist can be reflective: the best known is by looking at the philosophical foundations of one's discipline. In addition, the sociology of science has emerged as an important source of reflectivity.[2] In this relation, two more possibilities for reflectivity need to be mentioned; one old and one new. An investigation into the history of a science creates the possibility for critical reflection. A relatively new possibility is the economics of science.[3] Such reflectivity might have implications for the kinds of theories that are developed in economics, for the behavior of economists, or for both.

Methodologists of economic science have traditionally been looking for ways to distinguish between true science and "metaphysics."[4] The reasons for this attempt are laudable. Knowledge is power, the train of thought seems to go, but to be able to discredit at least some kinds of knowledge would allow the philosopher to stand up against, for example, some of the despicable outgrowths of 20th century socio-political movements, such as national-socialism or communism. Scholars such as Rudolf Carnap and Sir Karl Popper—and those who have followed in their footsteps, sometimes by amending or developing their thought—

are in this line of reasoning. Economic methodologists have followed the great example of the natural sciences in their philosophy of science stance as well.[5]

There are, however, some problems in the methodological approach briefly described above, if it is to be viewed as the only or the most important way in which to be reflective, centering on the concept of knowledge. On the one hand there is the belief that people (scientists) will pick up any piece of information in a clear and straightforward way. Information needs no interpretation; it is passively incorporated into the system of knowledge they have already acquired. This, in fact, implies a conception of how people learn that is Bayesian in nature.[6] On the other hand there is the belief that by interpreting the many different pieces of information and judging them on their "scientificness," philosophers of science can exert an influence on what information will be used or held for true by people or by scientists. Information, thus, is interpreted by its receptors after all—interpreted in the light only of the judgements made by the authority of the philosophy of science (it is hoped at the least).

In this article I will argue that there is a need to develop the psychology of scientists much further than it has up to now.[7] Some first attempts have been made up to now, but these mainly fall into two categories. The first category presents a thorough descriptive analysis (most notably Earl's) of scientists in order to explain some of the phenomena in a particular field. Incentive structures are important foci of attention. The second group swiftly moves into a consultative mode by making recommendations to the reader of how to make use of the system.[8] The implied reader is typically seen as a novice scientist trying to have his or her work published in prestigious journals. In this article, I will take a more normative approach, showing what attainable goals might be formulated for scientists (economists) from the point of psychology. Before developing these arguments, I will briefly go into two more established ways of being reflective as an economist that might function as a starting point for a psychology of economists.

The Sociology and the Rhetoric of Science

Over the years dissatisfaction arose with the natural sciences as the idol for economic theory and the methodology of economics.[9] Some people have taken the road of studying what economists actually do, rather than telling them what to do. Klamer (1991, 29) uses the example of the so-called "pick-up" game of basketball played in American cities to make the point clearer, taking "the native's point of view." Two teams play a game of basketball, any onlooker willing to participate can join in. In this play there is no referee, but anybody who thinks a rule of the game was violated must say so and the play will be stopped. The role of the methodologist of economics following in the footsteps of the philosopher of the natural sciences in this metaphor can be described as attempting to enforce the rules from outside the playing field without playing the game itself—like the ref-

eree in an ordinary basketball game. The current situation is that there are economic methodologists who participate in the activity of "playing economics" at the "economics playfield," while they step away from the field every now and again to look at what happens and how the play proceeds. They will tend to stress the social or rhetorical aspects of the "game of economics." The difficulty of doing this is evident from Reder's work. Reder aims to undertake such an analysis but fails in his attempt.[10]

Two ways of looking at the "play of economists" have appeared at the stage of economic methodology in recent years: the sociology of science and the rhetoric of science. Both analyze what scientists are actually doing; the first by focusing on the social environment of the individual scientist or group of scientist. Scientists are influenced by their social environment in what they do and how they do it, but they themselves can sometimes influence this environment as well.[11]

Klamer's (1983, 1988) and McCloskey's (1983, 1985, 1988) rhetorical approach in economic methodology focuses on the discourse of economists: How they talk, what metaphors they use, which arguments are persuasive, how they persuade. In this approach knowledge is not acquired passively, but needs to be interpreted by those that incorporate it. Furthermore, any interpretation—judgement of the value or usefulness of information—not only depends on the extent to which the truth is approximated, or the intention of doing so, but will be influenced by what some would call "irrational" matters as well.

In a way both approaches deal with the social sphere of how people are influenced by their social environment. Depending on what will count as "discourse," the rhetorical approach can be seen as a subset of the sociological approach. Each science or sub-science has its own rhetoric, for instance. The process of learning or relearning the proper rhetoric of the (sub-) science you are in or want to be in takes time and effort.[12] Rhetoric is an important subset of the sociology of science, surely, but a subset nonetheless.

Thus far the individual scientist has not entered the picture of those people who study what scientists do or should do. Nevertheless, there always seems to be an implicit idea of the human psychology lurking in the texts representative of both approaches. In the philosophy of science approach there is, for instance, an assumption of how knowledge is being built. In the sociology and rhetoric of science there is an idea of how people are persuaded, and why.

In this paper on the psychology of economists I will not follow the trail of argumentation that tries to decide on the best way to study the sciences (the philosophical way or the sociological way) from the standpoint of what psychological theory and observations tell us. I will not look at the two approaches to inquire which one did best in incorporating the latest insights from psychology. In this paper I would like to focus on the desirable psychological characteristics of a scientist. It is argued that, if such criteria are attainable and take account of how peo-

ple actually behave and learn, progress in science is more likely. At the same time expectations about progress in science will be more modest—scientists are not likely to be more rational than others.[13] An explication of standards for individual economists allows a critical review of them, which is likely to be beneficial. But first I need to argue that a science that wants to be a (more) truly reflective science needs to have a perception of what an individual scientist should be like, what general characteristics he or she should have.

Why a (More) Fully Reflective Science Needs to Consider the Individual Scientist

Following the lead of the great philosophers of science is supposed to result in a better science, a science that can (hope to) strive for the truth. Applying the "golden rules" of the philosophy of science would thus be the rational thing to do. But the "golden rules" are not generally applied by those scientists working in and with the real world. There are at least two ways in which this neglect of the rules from the philosophy of science can be explained. First, scientists—ordinary men and women as they are—do not always or even usually act rationally: For instance, because they are concerned with what their social environment thinks about their behavior.[14] A second reason may be that the "golden rules" are ideal types that cannot be applied in the kind of nitty-gritty research with which a science is usually concerned.

To see scientists as being influenced by their social environment easily slides into what Granovetter (483) calls an "oversocialized" position. Similarly, from the rhetoric of science approach; scientists do use a particular kind of rhetoric (metaphors for instance) that unconsciously introduces a bias in their work. Nevertheless they can become aware of such biases, after which they may or may not change them.[15]

Allowing for a change in the convictions held by an individual implies an idea of the psychology of humans or scientists. When it is agreed that people are able to choose freely to some extent,[16] a psychology of scientists is called for.

A Psychology of Scientists

Perhaps disagreement among psychologists does not range over such a large area of topics as does disagreement among economists—nor is it perhaps expressed so vehemently. We now go into psychological theory and select one perspective that is one of the more important, but not the only theory, in psychological literature: social learning theory.[17] For the purpose of this paper, I have chosen this particular approach in psychology because it is corroborated in a large number of empirical studies. In addition, it is an approach that is realistic,[18] and it is an approach that sits well with (social and institutional) economics.[19] As laypersons

we think it is a plausible theory as well: it is persuasive.[20] It seems to be able to explain human behavior well. For these reasons, Davis (forthcoming) has argued for social psychology as a theoretical source of inspiration to reconceptualize the individual in economic theory.

Following the approach that is used here, Albert Bandura (1977, 1986) claims that patterns of action are established when they are rewarded at crucial points in their development. He identifies three different ways in which people *can* be rewarded. There are external, vicarious and self-rewards. When people are rewarded they tend to repeat the action for which they got a reward. If we finish a paper and it turns out to be a good paper, our external reward may be that it will be published or that we will get a lot of stimulating responses. Behavioral psychology in the line of B.F. Skinner (1954) would rest its case here. Social learning theory (in his later publications Bandura calls his theory social cognitive theory) suggests that if others write a good paper that is in some way similar to ours, and are rewarded for it, this would be an incentive for us as well. Their paper being published vicariously rewards us to write a similar paper, or to publish in the same journal, etc.[21] Finally, we can reward ourselves after we have finished a paper that is to our liking. After having worked on a paper for a considerable time, having made a plethora of versions and scrutinized every single one of them meticulously, we may decide that it is now right. To reward ourselves we may indulge in a visit to a football game, a few beers in the pub, or a day off to have a short holiday. Each of these three kinds of rewards does not play an (important) role in every period of an individual's life, nor does it play a large role at every moment in time. Different rewards may at times work in opposite directions as well. Empirical research needs to establish which rewards have played what role.

The picture of social learning theory presented here is a brief one, for sure. Only some of its features were introduced, and in a brief manner at that. It does, however, serve our purpose of giving a background from which we can derive some general characteristics a scientist should have—in my view—so that one might expect him or her to do a good job. Psychologists can predict the performance of students fairly accurately by looking at the extent to which vicarious and self-rewards influence their behavior.[22] Indicators showing the extent to which self and vicarious rewards motivate students predict their achievements most adequately. Remember that important aspects of a discipline's knowledge and attitudes are formed during undergraduate and graduate studies.[23]

One needs to be careful in trying to derive, in Humean terms, an "ought" from an "is." Although I am aware of the methodological questions related to it, I do wish to come to a few suggestions as to preferable characteristics of scientists. Since these will be derived from a psychological theory that has found much empirical support, the criteria derived will not be unattainable. There are a few characteristics, I would suggest, that good scientists should have. These are general characteristics that are based on the psychological theory from which I just

presented some of the central tenets. A study of a more thoroughgoing nature than this one, combining a larger or deeper review of psychological literature with empirical observations, would be most welcome. This paper merely amounts to a first attempt at formulating a normative position concerning the psychology of scientists. A first, and suggestive list of characteristics an economist should have as necessary but not sufficient conditions for being a *good* scientist could be:

- A scientist should set high standards for herself, standards which she will not easily lower (she should sparingly or at least not easily reward herself, but attaining the level at which such self rewards are released must be within reach).
- A scientist should have an open mind, both toward what other sciences have come up with in terms of theories and empirical findings, and toward the critiques or comments of others on her own work. (She should be vicariously rewarded in these ways. There is a possible conflict between this point and the previous one. I argue here that an appropriate balance should be struck, appropriate to the circumstances at hand).[24]
- Similarly, a scientist should not be rewarding others easily (scientists should not indulge in giving others rewards—external rewards to these other persons—for accomplishments meeting only dubious standards).[25]
- It may follow that scientists should be willing to enter into a discussion with others on a common field of expertise/interest to test one's position in the face of comments brought forward by others.

Some or all of the points mentioned may be obvious to some the readers. Nevertheless, it is important to present these points in a way that shows how they can be supported by a psychological theory. It further shows how the criteria that many scientists have implicitly adopted are interrelated. Commenting on the scientific work of others—or for that matter on one's own work—does not have to degenerate into brutal attacks that give the impression that there is no grain of empathic feeling in the person criticizing. Form and content are interrelated of any kind of communication, of course, but this does not mean that content determines the form in a strict way.

Some Objections Refuted

The advent of a new idea in a science seems always to be accompanied by heavy critique of its merits. In our psychology of science view this is a good thing. Scientists working within the extant "normal science" defend their positions; they do not give up easily. Indeed, as Popper (1976) has argued, they may well be right to be somewhat stubborn and cling to their theory, even though some empirical evidence seems to contest it. At the same time, the people coming with new or

different ideas are forced to explain and defend their views, which can be considered a test of their confidence; their willingness to stand up for the theory or view they have developed. To reiterate, such attempts to defend should not lapse into a dogmatic stance. One should not only be sensitive to self-rewards.

There are a number of objections to the psychology of scientists that I have argued thus far in this paper. Two of these objections would like to see the approach here advocated fail, and one is an objection that is at least somewhat sympathetic to the position brought to the fore and the views expressed in this paper.

First, introducing a psychology of science might be thought of as unfair, since it would entail (if properly used in, for instance, appointment procedures) that some people are denied access to positions as scientists. But is it unfair? I would think not. By making at least some of the criteria explicit that are now used in an implicit and rather intuitive way, a scientific debate and research will start to test the criteria for their validity. Certainly, some people will not be allowed to be scientists, but now the criteria are clear and well founded. What is more, people can change themselves in order to meet the stipulated criteria. My final argument is one of division of labor: The would-be scientist rejected for a career in science is, perhaps, in a better position to get another occupation. Here his qualities may be used in a better way—having clarity on this point better comes earlier than later.

"But where does this end?" some will ask, raising a second objection. "Will somebody else advocate a biology of science in the future?" "Will scientists in the future begin to breed scientists to meet the criteria stipulated by such a 'biology of science.'" Such a Brave New World of Science is not to come. It ends where people themselves have the opportunity/possibility to meet the criteria stipulated by psychology of science. Such criteria will have to be based on both theory and empirical findings. A question to counter the "where does it end" question of course is: "Why should scientists be absolved from a psychological assessment before they are hired?" Psychological tests are not used as a matter of course when scientists are appointed or granted tenure at a university—it would seem that they are one of the few exceptions on the labor market for highly educated professionals.

Now, thirdly, there might be doubt as to the consistency of a psychology of science presented here. Scientists should be sensitive to some extent to vicarious rewards, but this does not necessarily result in progress in science. The reason is that vicarious rewards are allotted within a community. Communities are sometimes small, particularly scientific communities. Within a specific science there are different research programs, research programs that may be incommensurable. Being sensitive to vicarious rewards does not entail that scientists from within a research program (in the Lakatosian sense of the word) will get into a discussion with scientists from different research programs, even of the same science. Hence there is no reason to hold that science makes any progress because of a psychol-

ogy of scientists. There is no guarantee that the criteria formulated, for instance, will push people to communicate with those others with whom the communication holds the best promises for progress of a field or science: for instance because they are in different research programs.

As the third objection is one that urges us to be more precise or specific, I devote more attention to it. First, this objection is an objection to my particular suggestion as to the way in which a psychology of science may be worked out. It does not undermine my prior and more important claim that a psychology of science is needed. Second, even if there should be no communication between different research programs, but only communication within a research program, the theory or view a scientist presents is likely to be tested, criticized, and commented upon more than otherwise. If so, this will result in improvements to be made to the scientific product. As a third point I would like to invoke that there always seems to be at least some communication between different research programs within a science or between different sciences. Even if there is little or no communication between research programs, at least people always seem to be aware of the objections that would be raised by their "adversaries." This is a reason why some research programs wane. On many occasions scholars have argued that different research programs or sciences can cross-fertilize each other. Such assertions have always depended on intuitive appeals. Here we give one powerful ground to support this assertion.

Concluding Remarks

The philosophy of science has recently been supplemented with the sociology and rhetoric of science. Important contributions have been made in all these fields. This paper does not deny this, nor does it pass judgement on which approach is to be preferred, or which is to be valued highest. What this paper does is argue that a psychology of scientists (and economists) is needed. It is likely that such an approach—which I think is complementary to the other approaches—will have the effect that a science itself will improve. Here I do not open the Pandora's box of how improvement in science is to be ascertained. What will happen is that a psychology of scientists will make a science more reflective.

Psychology of science is a new field of enquiry. The strides that we have made here are tentative ones based on a far from perfect knowledge of the literature in psychology and a selection of one particular strand within it (social psychology) to develop some of the ideas and indicate what a psychology of scientists might develop. The normative criteria developed are examples of a coherent set of explicitly formulated criteria that one may hold to judge scientists. The mere exercise of explicating them and relating them to a well-established psychological theory is, I would like to suggest, worthwhile.

Notes

[1] See Cordes, et. al., 472.

[2] Mäki, 80.

[3] See Sent, 1999a.

[4] This is, of course, a simplification, but it suffices for present purposes.

[5] See Hausman; Blaug.

[6] See Dolfsma, 2001; Forthcoming.

[7] My main concern will be for economists, but the argument is, I believe, significant for practitioners of other sciences as well. I will, therefore, use the terms "scientists" and "economists" interchangeably.

[8] See, for example, Mahoney, 104.

[9] See Mirowski, and Klant, respectively.

[10] Reder's ambitious aim is to analyze the sociology of economics, but he ends up writing a more conventional methodological account of economics. See also, Sent, 1999b.

[11] See, for example, Latour; Law.

[12] Klamer, 1983; Klamer and Colander.

[13] I am aware of the intricacies related to discussions about "progress" in science. It is a topic that needs more discussion.

[14] For telling tales see Klamer, 1983.

[15] This is indeed what many of the rhetoricians in the economics field aim at. See Nelson; Klamer, 1991.

[16] After a long discussion, Taylor (184) concludes that one's conviction on whether of not people have a free will cannot be grounded on an empirical or a theoretical/philosophical basis. Frankfurt (1971, 6; 1978, 162), however, argues

that a person is characterized by his or her ability to choose the kind of desires he or she wants, and to guide his or her actions to accord with these kinds of desires.

[17] See Bandura, 1977, 1986.

[18] See Rottschaeffer.

[19] See Dolfsma, 2001; Forthcoming.

[20] The attentive reader will notice that the first argument is meant to persuade the "orthodox" philosopher of science, while the second may persuade the rhetorically inclined reader.

[21] Explaining an important phenomenon, such as imitation, could thus make good use of a social learning theory perspective.

[22] See Zimmerman, Bandura and Martinez-Pons, 674.

[23] See Klamer and Colander; Frank, et. al., 170.

[24] What I implicitly suggest here is that the proneness of many scientists—especially economists—not to go beyond their field of immediate expertise is largely due to psychological factors. See Phelps-Brown, 9.

[25] For the present purposes, it suffices to say that standards of excellence within a science are deemed to be set in the social community of that science. I will elaborate on that later.

References

Bandura, A. (1977). *Social Learning Theory.* Englewood Cliffs, NJ: Prentice-Hall.

_____. (1986). *Social Foundations of Thought and Action—A Social Cognitive Theory.* Englewood Cliffs, NJ: Prentice-Hall.

Blaug, M. (1992). *The Methodology of Economics.* Cambridge: Cambridge University Press.

Cordes, J.J., A. Klamer, and T.C. Leonard. (1993). "Academic Rhetoric in the Policy Arena: The Case of the Capital Gains Taxation." *Eastern Economic Journal.* 19, 4: 459-479.

Davis, J.B. (Forthcoming). *The Theory of the Individual in Economics.* London & New York: Routledge.

Dolfsma, W. (2001) "Metaphors of Knowledge in Economics." *Review of Social Economy,* 59, 1: 71-91.

_____. (Forthcoming). "The Mountain of Experience: How People Learn in a Complex, Evolving Environment," *International Journal of Social Economics.*

Earl, P.E. (1983). "A Behavioral Theory of Economists' Behavior." A.S. Eichner, ed . *Why is Economics Not Yet a Science?* London: MacMillan & M.E. Sharpe, 90-125.

Frank, R.H., T. Gilovich, and D.T. Regan. (1993). "Does Studying Economics Inhibit Cooperation?" *Journal of Economic Perspectives,* 7, 2: 159-171.

Frankfurt, H.G. (1971) "Freedom of the Will and the Concept of a Person." *The Journal of Philosophy,* 68, 1: 5-20.

_____. (1978). "The Problem of Action." *American Philosophical Quarterly,* 15, 2: 157-162.

Granovetter, M. (1985). "Economic Action and Social Structure: The Problem of Embeddedness." *American Journal of Sociology,* 91, 3: 481-510.

Hausman, D.M. (1989). "Economic Methodology in a Nutshell." *Journal of Economic Perspectives,* 3, 2: 115-127.

Klamer, A. (1983). *Conversations With Economists.* Totowa, NJ: Rowman and Allanheld.

_____. (1991) "Towards the Native's Point of View—The Difficulty of Changing the Conversation." D. Lavoie, ed. *Economics and Hermeneutics.* London: Routledge, 19-33.

_____, and D. Colander. (1990). *The Making of an Economist.* Boulder: Westview Press.

_____, D. McCloskey, and R. Solow. (1988). *The Consequences of Economic Rhetoric.* New York: Cambridge University Press.

Klant, J.J. (1986). *What is Economic Methodology?* Amsterdam: Wigwam Press.

Latour, B. (1987). *Science in Action.* Cambridge: Harvard University Press.

Law, J., ed. (1986). *Power, Action and Belief—A New Sociology of Knowledge?* London: Routledge & Kegan Paul.

Mahoney, M.J. (1976). *Scientists as Subjects: The Psychological Imperitive.* Cambridge, Mass.: Ballinger.

Mäki, U. (1993). "Social Theories of Science and the Fate of Institutionalism in Economics." U. Mäki, B. Gustafsson , and C. Knudsen, eds. *Rationality, Institutions and Economic Methodology.* London & New York: Routledge, 76-109.

McCloskey, D. (June 1983). "The Rhetoric of Economics." *Journal of Economic Literature,* XXI: 481-517.

_____. (1985). *The Rhetoric of Economics.* Madison: University of Wisconsin Press.

Mirowski, P. (1989). *More Heat Than Light—Economics as Social Physics: Physics as Nature's Economics.* New York: Cambridge University Press.

Nelson, J.A. (1992). "Gender, Metaphor, and the Definition of Economics." *Economics and Philosophy,* 8: 103-125.

Phelps-Brown, E.H. (1972). "The Underdevelopment of Economics." *The Economic Journal,* 82, 325: 1-10.

Popper, K.R. ([1970] 1976). "Normal Science and its Dangers." I. Lakatos and A. Musgrave, eds. *Criticism and the Growth of Knowledge.* London: Cambridge University Press, 51-58.

Reder, M.W. (1999). *Economics: The Culture of a Controversial Science.* Chicago: The University of Chicago Press.

Rottschaeffer, W.A. (1992). "Social Learning Theories of Moral Agency." *Behavior and Philosophy,* 19, 1: 61-76.

Sent, Esther-Mirjam. (1999a). "Economics of Science: Survey and Suggestions," *Journal of Economic Methodology,* 6, 1: 95-124.

_____. (1999b) "The Armchair Culture of Armchair Economics: Review of Economics: The Culture of a Controversial Science by Melvin W. Reder." *The Review of Politics,* 61, 3: 561-562.

Skinner, B.F. (1954). *Science and Human Behavior.* New York: Free Press.

Taylor, R. (1966). *Action and Purpose.* Englewood Cliffs, NJ: Prentice-Hall.

Zimmerman, B.J., A. Bandura, and M. Martinez-Pons. (1992). "Self-Motivation for Academic Attainment: The Role of Self-Efficacy Beliefs and Personal Goal Setting." *American Educational Research Journal,* 29, 3: 663-676.